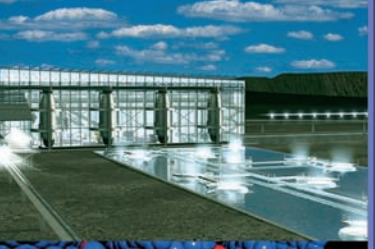
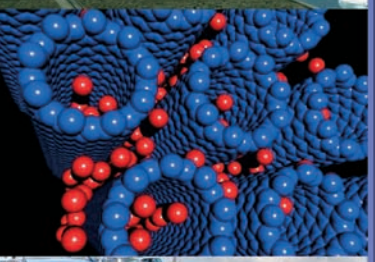
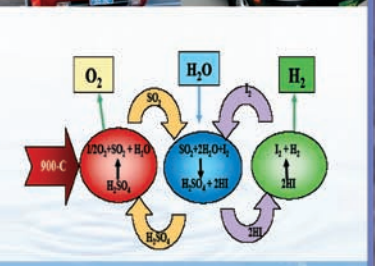
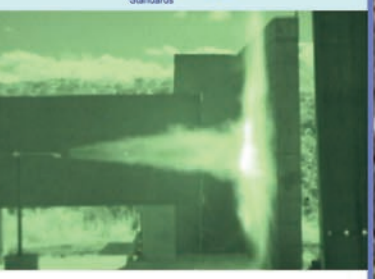




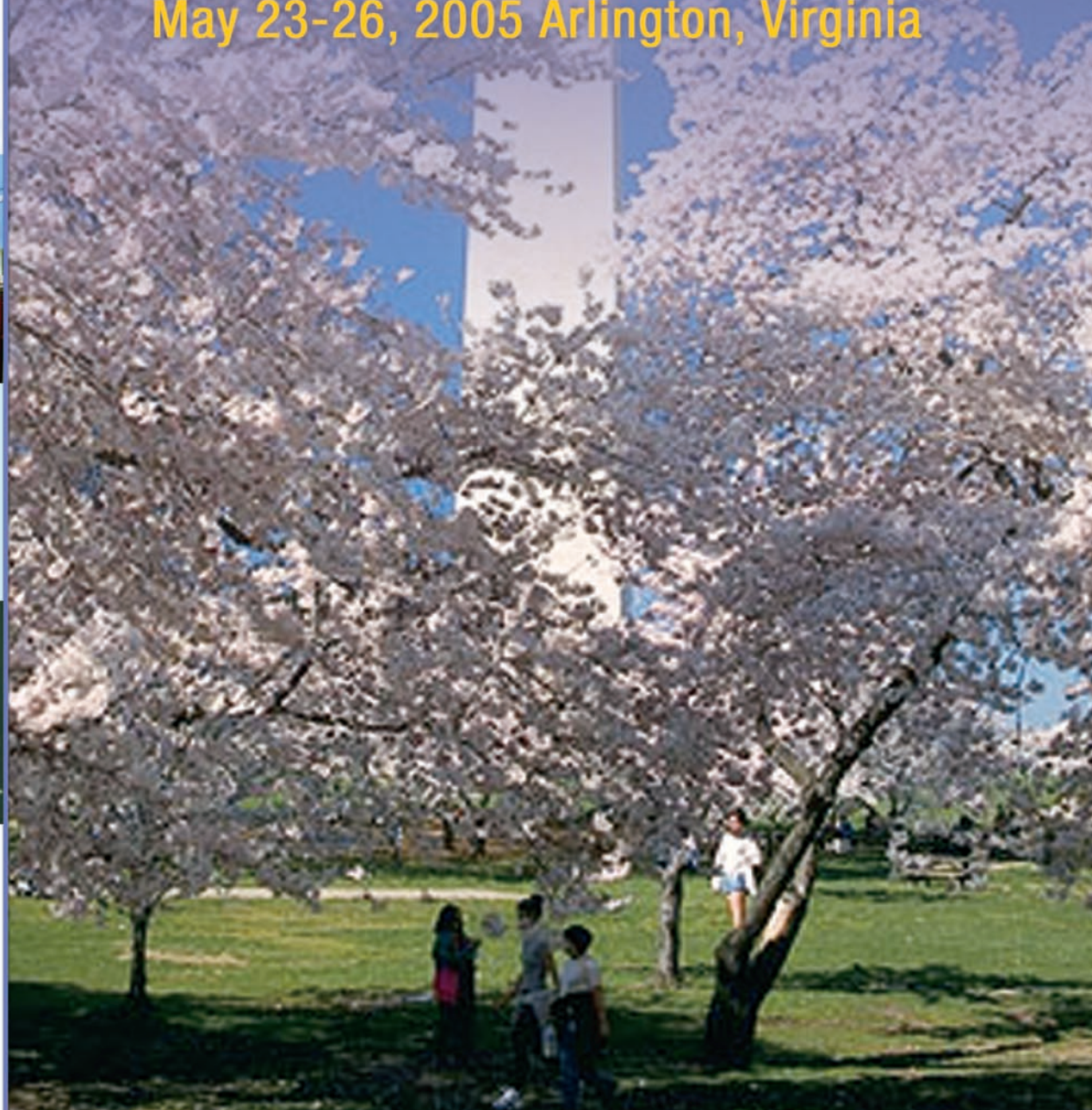




DOE Hydrogen Program

2005 Annual Merit Review and Peer Evaluation Report

May 23-26, 2005 Arlington, Virginia



U.S. Department of Energy

DOE/GO-102005-2187
September 2005

DOE Hydrogen Program

2005 Annual Merit Review

and

Peer Evaluation Report

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PROLOGUE

Dear Colleague:

This document summarizes the comments provided by the Peer Review Panel at the U.S. Department of Energy Hydrogen Program FY 2005 Annual Merit Review and Peer Evaluation meeting, held on May 23-26, 2005 in Washington, D.C. This was the second review of the entire DOE Hydrogen Program, in response to direction from the Under Secretary for Energy, Science, & Environment. All four Offices that support the President's Hydrogen Fuel Initiative — Energy Efficiency and Renewable Energy (EERE), Fossil Energy (FE), Nuclear Energy, Science, and Technology (NE), and Science (SC) — are participants in the DOE Hydrogen Program and supported this Review. This coordinated effort provides 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, for the first time projects from FE and NE were presented and peer reviewed. Subsequent to the announcement by Science of approximately 70 hydrogen and fuel cell-related awards in May 2005, we expect to include a significant number of Science projects in our FY 2006 meeting.

The recommendations of the Panel have been taken into consideration by DOE Technology Development Managers and Research Managers in the generation of work plans for FY 2006. The following table lists the projects presented at the Review and the major actions to be taken during the upcoming fiscal year (October 1, 2005 to September 30, 2006). These actions are, of course, subject to the appropriation process as well as Congressional and other priorities. The projects have been grouped according to which activity Program Element (production, delivery, storage, fuel cells, etc.) they support, and the weighted scores are based on a four-point scale involving five criteria. To furnish all principal investigators (PIs) with direct feedback, 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 2006 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 programmatic budget decisions for the new fiscal year. Thank you for participating in the FY 2005 Annual Merit Review and Peer Evaluation meeting.

We look forward to your participation in the FY 2006 Annual Merit Review and Peer Evaluation which is presently scheduled for May 16-19, 2006 in the Washington, D.C. area.

Steven G. Chalk
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	DOE Summary Comment
PD-02	*A Reversible Planar Solid Oxide Fuel-Fed Electrolysis Cell and Solid Oxide Fuel Cell for Hydrogen and Electricity Production Operating on Natural Gas/Biogas; MSRI; Greg Tao*	2.78		X			Congressionally-directed competitively awarded project which will continue in FY2006. Focused on DOE goals of improving efficiency and flexibility in hydrogen production from natural gas and renewable resources. A process analysis and well-to-electricity/hydrogen analysis is needed to determine overall efficiency and cost of hydrogen production compared to other pathways.
PD-03	*Autothermal Cyclic Reforming Based Hydrogen Generating & Dispensing System; GE Energy; Ravi Kumar*	3.06		X			Project is nearing completion in early FY2006. Process analysis is in progress to document distributed reforming targets.
PD-04	*Development of a Turnkey Hydrogen Fueling Station; Air Products; David Guro*	3.56		X			Nearing completion in early FY2006. Research on advanced hydrogen steam methane reformer for integration into the hydrogen generation system is complete.
PD-05	*Development of a Natural Gas-to-Hydrogen Fueling System; GTI; Bill Liss*	2.58		X			Behind schedule. Accomplishments unclear. Needs to sharpen focus on distributed generation RD&D targets. Better defined deliverables needed.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PD-06	*Production of Hydrogen by Biomass Reforming; PNNL; David King*	3.13		X			Expand outreach to industry. Quantified milestones and deliverables needed.
PD-07	*Hydrogen Generation from Biomass-Derived Carbohydrates via the Aqueous-Phase Reforming (APR) Process; Virent Energy Sys.; Randy Cortright*	3.21	X				Develop better defined milestones, and coordinate efforts with related projects.
PD-11	*Advanced Hydrogen Transport Membranes for Vision 21 Fossil Fuel Plants; Eltron Res. Inc.; Anthony Sammells*	3.18		X			Add a system engineering model to quantify the cost advantages of proposed work over conventional technologies and to provide R&D guidelines.
PD-12	*Scale-up of Microporous Inorganic Hydrogen-Separations Membranes; ORNL; Brian Bischoff*	3.15	X	X			Add a system engineering study to determine the economic viability of proposed work and establish the go/no-go decision points. Address hydrogen selectivity and clues to improve hydrogen selectivity.
PD-13	*Low Cost Hydrogen Production from Biomass Using Novel Membrane Gasification Reactor; GTI; Francis Lau*	3.07	X				Focus on membrane development and ensure good milestones and go/no-go decision points.
PD-14	*A Novel Slurry-Based Biomass Reforming Process; United Tech. Res.Ctr.; Tom Vanderspurt*	3.05	X				Focus on catalyst development, ensure good go/no-go decision points, and consider adding a catalyst development partner.
PD-15	*Maximizing Photosynthetic Efficiencies and Hydrogen Production in Microalgal Cultures; UC Berkeley; Tasios Melis*	3.48				X	Current project completed successfully (good progress) and follow-on project started. The project approach was thorough and well-thought out including gene identification and function.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PD-16	*Biological Systems for Hydrogen Photoproduction; NREL; Maria Ghirardi*	3.32		X			Good progress over the last year with plans for meaningful future work. Efforts are well-coordinated internally and with other groups.
PD-17	*Creation of Designer Alga for Efficient and Robust Production of H2; ORNL; James Lee*	2.92		X			Before significantly ramping up proton channel efforts, sufficient scientific basis needs to be established and validated by the peer community.
PD-18	*Fermentative Approaches to Hydrogen Production; NREL; PinChing Maness*	3.44		X			Great progress so far on this new project. Plans for the future are reasonable and promising.
PD-19	*Hydrogen Reactor Development and Design for Photofermentation and Photolytic Processes; NREL; Dan Blake*	2.97		X			Need to develop a strategy for gas permeability of polymers.
PD-20	*Photoelectrochemical Hydrogen Production: SHGR Program Subtask; U of Hawaii; Eric Miller*	3.25				X	Project successfully completed with satisfactory progress, potential significant long-term impact, and well established collaboration. Project will be continued under the UNLV High Temperature Solar Congressionally-directed project.
PD-21	*Photoelectrochemical Water Systems for H2 Production; NREL; John Turner*	3.36		X			The project is carefully thought-out and is quite feasible, though risky. In addition, there is a very good mix of experimental results and computational methods for bandgap tailoring.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PD-23	*Low-Cost, High-Pressure Hydrogen Generator; Giner Electrochemical; Cecelia Cropley*	3.26		X			Project making progress on reducing costs to meet DOE goals while addressing potential for high pressure electrochemical compression. Further cost analysis should be utilized to evaluate current performance against DOE goals. In addition project would benefit from additional partners.
PD-24	*High Temperature Solid Oxide Electrolyzer System; INEEL; Steve Herring*	2.88		X			Good progress to date. Unclear how high purity is obtained from 90/10 hydrogen/H_2O outlet stream. Identify future plans and link to DOE goals.
PD-25	*Renewable Electrolysis Integrated System Development and Testing; NREL; Ben Kroposki*	3.71		X			Project addresses system problems inherent in renewable hydrogen generation approaches. This work supports DOE's goals on the implementation of large-scale hydrogen generation from renewables using electrolysis. Project will continue funding in FY2006.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PD-26	*Alkaline, High Pressure Electrolysis; Teledyne; Samir Ibrahim*	2.81		X			Presentation provided comprehensive safety analysis of new alkaline electrolysis system design. Presentation did not identify major cost features and does not identify the critical component developments to improve technology. More extensive hardware results and data are expected in FY2006.
PD-27	*Sulfur-Iodine Thermochemical Cycle; INL/SNL; Paul Pickard*	3.64		X			Highly relevant to program goals and substantially different from other work. Good use of international collaboration. Materials identification critical.
PD-28	*Development of Solar-powered Thermochemical Production of Hydrogen from Water; Univ. of Nevada; Bob Perret*	2.94					This project will be continued as a multi-year Congressionally-directed effort. Emphasis is now being placed on research on the promising thermochemical cycles selected as recommended by the reviewers.
PD-29	*Alternative Thermochemical Cycle Evaluation; ANL; Michele Lewis*	3.06		X			Good complement to higher-risk NHI research. Use of and intended output to similar solar research very positive. Effort is needed to engage industry and universities.
PD-30	*High Temperature Heat Exchanger Development; UNLV; Tony Hechanova*	2.77		X			Project organization (multiple work packages) very confusing with opportunity for better alignment with program. Suggest breaking up into smaller pieces.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PD-31	*NHI System Interface and Support Systems; INL; Steve Sherman*	3.19		X			Link between nuclear reactor and hydrogen production system highly relevant to NE portion of DOE program. Need to clarify progress against stated barriers and industry links.
PD-32	*H2A Delivery Analysis; ANL; Marianne Mintz*	3.45		X			This work will be continued and become part of the expanded hydrogen delivery analysis effort as an industry and national lab collaborative project.
PD-33	*Hydrogen Permeability and Integrity of Hydrogen Delivery Pipelines; ORNL; Zhili Feng*	2.95		X			This project will be continued and the funding level will be increased to a more effective level. This project and the other pipeline research projects will be coordinated and leveraged for synergies as part of the hydrogen delivery pipeline working group.
PD-34	*Reversible Liquid Carriers for an Integrated Production, Storage & Delivery of Hydrogen; Air Products; Guido Pez*	3.25	X				This new unique liquid hydrogen carrier project will be continued with better designed go/no-go decision and direction points to ensure proper emphasis relative to on-board or off-board approaches and targets.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PD-35	*Materials Solutions for Hydrogen Delivery in Pipelines; Secat, Inc.; Subodh K. Das*	3.06	X				This new project will be continued. Clearer objectives, timeline, and milestones will be developed. This project and the other pipeline research projects will be coordinated and leveraged for synergies as part of the hydrogen delivery pipeline working group.
PDP-01	*Enhanced Hydrogen Production Integrated with CO2 Separation in a Single-Stage Reactor; Ohio State Un.; Liang Shih Fan*	2.99		X			Focus work beyond CO_2 sorption to include (a) hydrogen purity, (b) WGS catalyst life stability at high temperature, and c) issues related to the integration of the proposed concept with front end coal gasification step.
PDP-03	*Integrated Ceramic Membrane System for Hydrogen Production; Praxair; Joseph Schwartz*	3.01		X			Continue as novel separation system critical to development of next generation integrated distributed production system.
PDP-04	*Low Cost Hydrogen Production Platform; Praxair; Tim Aaron*	3.07		X			Continue to develop design for manufacturing and assembly techniques. Need to document progress against the DOE targets as the project proceeds.
PDP-05	*Solid Oxide Fuel Cell Carbon Sequestration; Nisource Energy Tech.; Norm Bessette*	2.58		X			Congressionally-directed multi-year project. Need more focus on addressing program technical targets in electrolysis.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PDP-07	*High Pressure Distributed Ethanol Reforming; ANL; Shabbir Ahmed*	2.62		X			Continue as the project is directly relevant to distributed production from renewable feedstocks. Focus on energy balance between reformer and water gas shift viewed as strength.
PDP-08	*Hydrogen Production for Fuel Cells via Reformation of Coal-Derived Methanol; U Cal., Davis; Paul Anders Erickson*	3.10		X			Focus work on the role of hydrocarbon impurities in reforming step and seeking means to reduce reforming catalyst deactivation rate, including the use of guard bed. Expand and seek more active industry participations in the project.
PDP-09	*Hydrogen Production via a Commercially Ready Inorganic Membrane Reactor; Media & Process Tech.; Paul KT Liu*	3.15		X			Address the material/preparation cost and low hydrogen flux associated with the proposed carbon molecular sieve/stainless steel support system. More experimental work to prove out the feasibility of the WGS membrane reactor.
PDP-10	*New York State Hi-Way Initiative; GE Global Research; Richard Bourgeois*	3.19		X			Congressionally-directed project has resulted in new electrolyzer design that shows potential for lower costs and mass production capability. Project will continue funding to advance alkaline electrolysis development.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PDP-14	*Development of Dense Ceramic Membranes for Hydrogen Separation; ANL; Balu Balachandran*	3.40		X			Expand industry participation and collaboration including cost-sharing. Address issues which can integrate proposed work with front-end coal gasification step with high system efficiency. Perform long-term membrane life testing.
PDP-15	*Ceramic Membrane Reactor Systems for Converting Natural Gas to Hydrogen and Synthesis Gas (ITM Syngas); Air Products; Christopher Chen*	3.60		X			DOE/EERE cost-share of commercial scale planar membrane complete. Scale-up to production of liquid fuels and phase III carbon sequestration to be continued in Fossil Energy Program.
PDP-16	*Cost-Effective Method for Producing Self-Supporting Pd Alloy Membrane for Use in the Efficient Production of Coal-derived Hydrogen; SwRI; Bruce Lanning*	3.23		X			More experimental work with better matrix planning to include long life membrane testing and testing with gas mixtures. Perform scoping economics study. Expand industry participation with cost-sharing.
PDP-17	*Defect-Free Thin Film Membranes for H2 Separation and Isolation; SNL; Tina Nenoff*	2.97		X			Continue as critical to lowering cost of hydrogen separation technologies. Focus on real-world experimental results with potential commercialization partners.
PDP-19	*Membrane Applications for Nuclear Hydrogen Production Processes; ORNL; Brian Bischoff*	3.09	X	X			Relevance to NE production technologies clear to some, unclear to others. Higher apparent need for thermochemical membranes than high-temperature electrolysis.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PDP-20	*Materials for High-Temperature Thermochemical Processes; ORNL; Dane Wilson*	3.21		X			Future plans should focus on new materials.
PDP-24	*A Novel Membrane Reactor for Hydrogen Production from Coal; GTI; Francis Lau*	3.21		X			Prove out the feasibility of membranes in the presence of contaminants found in coal gasifier. Improve membrane hydrogen flux. Add dual-phase membrane development work. Seek more inputs from industry.
PDP-25	*Hydrogen Separation; NETL; Richard Killmeyer*	3.26		X			Improve membrane fabrication capability to make thinner membrane to yield higher hydrogen flux. Perform experiments under conditions of commercial interests. Seek more industry inputs to the project.
PDP-27	*Startech Hydrogen Production; Startech Environmental; David Lynch*	2.88		X			Congressionally-directed multi-year project. Needs to focus more on materials and design, expand collaborations, perform thorough energy balance analysis including efficiency of electricity.
PDP-28	*EVermont Renewable Hydrogen Fueling System; Northern Power Sys.; Tom Maloney*	3.03				X	Congressionally-directed project (completed). Developed wind integrated electrolysis system.
PDP-33	*Discovery of Photocatalysts for Hydrogen Production; SRI International; Brent MacQueen*	2.70				X	This project will be completed this year with limited progress or results.
PDP-39	*Hydrogen Production – Increasing the Efficiency of Water Electrolysis; SNL; Donald Pile*	2.41			X		Project discontinued and work in this area will be funded as a partner with industry in a competitive award.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PDP-41	*Hydrogen Generation from Electrolysis; Proton Energy Systems; Stephen Porter*	2.64		X			Future technology development in FY2006 will need to show potential to achieve DOE program electrolysis goals. Primary focus should be on electrolyzer development with diminished emphasis on fueling station aspects of project.
PDP-42	*System Design and New Materials for Reversible Solid-Oxide, High-Temperature Steam Electrolysis; GE Global Research; James Ruud*	2.62	X				New project, will continue in FY2006. Project will be closely collaborated with other reversible solid oxide development at GE.
PDP-45	*Hybrid Sulfur Thermochemical Process Development; SRS; Bill Summers*	3.46		X			Highly relevant to program goals. Effective use of modeling. Large number of collaborators.
PDP-46	*Combined Reverse-Brayton Joule-Thompson Hydrogen Liquefaction Cycle; Gas Equip. Engr. Corp.; Martin Shimko*	2.43	X				This is a new project. The project will be continued. The program cost target will be clarified and a go/no-go decision on cost target feasibility will be made after one year of research.
PDP-47	*Active Magnetic Regenerative Liquefier (AMRL) Development; New Concepts Res. Corp.; Robert Thompson*	3.23	X				This is a new project. This unique approach to hydrogen liquefaction with breakthrough potential for cost reduction and efficiency improvement will be continued. It will be focused primarily on large scale liquefaction and early economics will be developed in detail.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
PDP-48	*Hydrogen Embrittlement of Pipeline Steels: Causes & Remediation; U. of Illinois; Petros Sofronis*	3.21	X				This is a new project providing more fundamental knowledge and insight into hydrogen embrittlement. This project and the other pipeline research projects will be coordinated and leveraged for synergies as part of the hydrogen delivery pipeline working group.
PDP-49	*Hydrogen Regional Infrastructure Program in Pennsylvania; Concurrent Tech. Corp; Eileen Schmura*	2.38		X			Congressionally-directed multi-year project. Need more focus on addressing program technical targets in Delivery Technologies.
PDP-55	*Distributed Bio-Oil Reforming; NREL; Bob Evans*	3.12	X				This project will be continued. Additional reforming catalyst expertise from industry and/or universities will be added to this effort.
PDP-57	*Developing Improved Materials to Support the Hydrogen Economy; Edison Materials Tech Center; Michael Martin*	1.92		X			Congressionally-directed multi-year project. Need more focus on addressing program technical targets.

Hydrogen Storage:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
ST-02	*Development of Metal Hydrides at Sandia National Laboratories; Sandia Nat. Labs ; Jim Wang*	3.05		X			This work is being incorporated into the Metal Hydride Center of Excellence. Increase systems analysis to help guide materials R&D. Consider building teams within the center and collaborate with relevant independent projects.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
ST-03	*Catalytically Enhanced Hydrogen Storage Systems; Univ. of Hawaii; Craig Jensen*	3.17				X	Project ends in FY2005. New materials work will continue as part of the Metal Hydride Center of Excellence. Need to continue the focus on materials with higher capacity than sodium alanate.
ST-04	*High Density Hydrogen Storage System Demonstration Using NaAlH4 Complex Compound Hydrides; UTRC; Don Anton*	2.89		X			Planned for completion in FY2007. Need to communicate system level "lessons learned" and consider collaboration with the Metal Hydride Center of Excellence, Storage Systems Analysis Working Group, and relevant independent projects. Develop new status and projection numbers for weight and volume of the second generation prototype.
ST-05	*Discovery of Novel Complex Metal Hydrides for Hydrogen Storage through Molecular Modeling and Combinatorial Methods; UOP; David Lesch, PI; Adriaan Sachtler, co-PI*	2.90		X			Transition from sodium alanate to higher capacity materials with potential to meet 2010 system targets. Consider collaboration with the Metal Hydride Center of Excellence and other relevant independent projects.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
ST-06	*Complex Hydride Compounds with Enhanced Hydrogen Storage Capacity; UTRC; Don Anton, PI; Susanne Opalka, co-PI*	3.17		X			Improvement since FY2004. Incorporate "lessons learned" from the UTRC system prototype project (ST-04) to guide material selection. The trend to move away from the sodium alanates is appropriate. Consider collaboration with the Metal Hydride Center of Excellence and relevant independent projects.
ST-07	*Sub-Nanostructured Non-Transition Metal Complex Grids for Hydrogen Storage; Cleveland State Univ.; Orhan Talu*	2.06		X			This project was awarded prior to the adoption of the current hydrogen storage performance targets. DOE will work with Cleveland State to prioritize work in FY2006 to stress the proof of concept of hydrogen adsorption/absorption in metal grids.
ST-09	*Clean Energy Research at the University of South Carolina; U. of S. Carolina; Ralph White*	2.18		X			FY2004 Congressionally-directed project (also received FY2005 Congressionally-directed funds). Work on well known materials ($NaAlH_4$, $NaBH_4$) is not likely to meet targets. Reduce duplication of other DOE projects.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
ST-10	*Fuel Cell and Hydrogen Research University of South Florida; U. of South Fl.; Lee Stefanakos*	1.53		X			FY2004 Congressionally-directed project (also received FY2005 Congressionally-directed funds). Diverse portfolio that needs coordination with the rest of the program. Terminate work on approaches that will not meet DOE targets (e.g. Mg_2FeH_6) and focus on novel approaches to meet 2010 targets and with potential to meet 2015 targets.
ST-12	*Process for the Regeneration of Sodium Borate to Sodium Borohydride for Use as a Hydrogen Storage Source; Millennium Cell; Ying Wu*	2.40		X			Collaborating with the Chemical Center of Excellence on a separate effort and will include Rohm and Hass for engineering expertise. Need to determine total energy required and economic feasibility for the system. Improve well-to-tank analysis with a good protocol/mode and evaluate data for go/no-go decision.
ST-13	*Chemical Hydride Slurry for Hydrogen Production and Storage; Safe Hydrogen; Andy McClaine*	2.56		X			Recheck assumptions in systems analyses. Need to build and test system to demonstrate system efficiency and cost. Assess potential hydrogen stream impurities from mineral oil. In FY2006, this project will test the solid-oxide oxygen-ion-conducting membrane (SOM) and carbothermic processes and update economic analyses.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
ST-14	*Development of New Carbon-Based Sorbent Systems for an Effective Containment of Hydrogen; Air Products; Alan Cooper*	3.29		X			Strong project. Conduct early systems analyses to establish minimum molecular target requirements. Continue dehydrogenation catalyst development and the search for materials with improved thermodynamics. Consider potential health effects of carriers. Need to confirm cycling stability.
ST-15	*Low Cost, High Efficiency, High Pressure Hydrogen Storage; Quantum; Jui Ko*	2.96					Expand collaborations (LLNL, ANL, ORNL, carbon manufacturers, etc.). Provide more detailed assessment of CoolFuel™ approach. Implement go/no-go decision points.
ST-16	*Advanced Concepts for Containment of Hydrogen and Hydrogen Storage Materials; LLNL; Salvador Aceves*	2.81					Need to narrow down number of approaches. Focus on second generation tank and test the system to provide experimental data to replace projected capacity values.
ST-17	*Advanced Manufacturing Technologies for Renewable Energy Applications; Natl Center for Manf. Sci.; Chuck Ryan*	1.53		X			FY2004 Congressionally-directed project (also received FY2005 Congressionally-directed funds with work scope still to be finalized). List of goals, milestones, schedule and deliverables should be provided.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
ST-18	*DOE Carbon-based Materials: NREL Activities and Overview; NREL; Mike Heben*	3.20		X			New scope was well received. Continue expansion beyond nanotubes as part of Center of Excellence on Carbon-based Materials. Add go/no-go or downselects for each approach and apply systems analysis to guide material property requirements.
ST-19	*Analyses of Hydrogen Storage Materials and On-Board Systems; TIAX LLC; Stephen Lasher*	2.90					Critical to the program. Coordinate with ANL and Center analysis activities. Work scope needs some parallel tasks, rather than all sequential. Resolve discrepancy between measured values (e.g. UTRC prototype) and system analysis projections. Need to document and communicate assumptions.
ST-21	*Standardized Testing Program for Chemical Hydride and Carbon Storage Technologies; SwRI; Richard Page*	3.04					Validation through round robin testing is critical. Incorporate feedback from Centers and others into protocols. Ensure reproducibility and communicate results to obtain acceptance from technical community.
STP-58	*Development and Characterization of Novel Complex Hydrides; Savannah River NL; Ragaiy Zidan*	2.81		X			This project is a CRADA with UTRC (ST-04), planned for completion in FY2007. Need to focus on materials with higher capacity than sodium alanate.

Fuel Cells:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-02	*Integrated Manufacturing for Advanced MEAs; De Nora; Emory DeCastro*	3.26		X			Reviewers concerned about "manufacturability" of Ion Beam Assisted Deposition coating process. Project has very broad scope. Should begin to focus and down select to allow optimization. Good collaboration and publishing.
FC-03	*Advanced MEAs for Enhanced Operating Conditions, Amenable to High Volume Manufacture; 3M; Mark Debe*	3.61		X			Pt loading meets 2015 target. Reviewers indicate that membrane performance at low RH needs improvement.
FC-04	*Development of High Temperature Membranes and Improved Cathode Catalysts for PEM Fuel Cells; UTC; Lesia Protsailo*	3.29		X			Well thought out and executed catalyst experiments, and down-selects for university work were made intelligently. Need to give some thought to bringing on an MEA manufacturer.
FC-05	*Electrocatalyst Supports and Electrode Structures; LANL; Mahlon Wilson*	2.92		X			Need to reconcile high activity of BNL catalyst with mediocre performance of the catalyst as tested in an MEA at LANL. Catalyst testing protocol needs to incorporate voltage cycling.
FC-06	*Development of New Polymer Electrolytes for Operation at High Temperature and Low Relative Humidity; Case West. Res. Uni.; Tom Zawodzinski*	2.86		X			Good focus on conductive mechanisms for new membrane approaches. Reviewers recommend the use of go/no-go decisions to down select approaches.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-07	*High-Temperature Polymer Electrolyte Membranes; ANL; Debbie Myers*	2.64		X			PI should work on issue of dimensional stability and establish collaborations with membrane suppliers/manufacturers.
FC-08	*Development of Polybenzimidazole-based, High Temperature Membrane and Electrode Assemblies for Stationary and Automotive Applications; Plug Power; Rhonda Staudt*	2.64		X			Project will focus on resolution of fundamental issues. Project should generate performance degradation data versus acid loss and creep of the membrane.
FC-09	*Enabling Commercial PEM Fuel Cells with Breakthrough Lifetime Improvements; Dupont; Gonzalo Escobedo*	3.48		X			Good combination of theory and experimentation. Comprehensive approach that addresses failure modes of the MEA in a systematic way. Mechanical tests may provide some guidance on stability, but they can't fully represent the cyclic stresses in a fuel cell. Good collaboration, but there is a lack of publications for this size project.
FC-10	*New Electrocatalysts for Fuel Cells; LBNL; Phil Ross*	3.40		X			Strength is blend of theory and experiments on model catalysts in the context of operation of real fuel cells. PI should conduct single cell tests with collaborators.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-11	*Non-Precious Metal Catalysts; LANL; Piotr Zelenay*	3.19		X			Quite significant accomplishments were achieved, but there is still a gap between the Pt and the non-precious metal catalyst performances. There needs to be more thought on catalyst cost and catalyst stability in acid.
FC-12	*MEA and Stack Durability for PEM Fuel Cells; 3M; Mike Hicks*	3.36		X			3M presented significant steps forward in membrane degradation and on mapping results of accelerated tests onto unaccelerated lifetimes. They should look at the system operation effect on membrane durability to improve understanding of decay mechanisms and solutions. 3M should perform additional characterization of physical properties and the effect of aging on these properties.
FC-13	*Development of transition metal/ chalcogen based cathode catalysts for PEM fuel cells; Ballard; Stephen Campbell*	2.69		X			Despite good research effort and analytical basis, the activity of the best catalyst for the oxygen reduction reaction was low. Fe and Cr selenides do not look promising, while Co selenide shows a little promise.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-14	*Novel Approach to Non-Precious Metal Catalysts; 3M; Radoslav Atanasoski*	3.09		X			A systematic, combinatorial approach incorporating synthesis of many catalyst samples in the Fe-N-C system. Catalyst activity is still low, despite improvements. Comprehensive approach and good team to cover not only catalyst discovery but also eventual manufacturability.
FC-15	*Novel Non-Precious Metals for PEMFC: Catalyst Selection through Molecular Modeling and Durability Studies; U of So. Carolina; Branko N. Popov*	2.91		X			Looking at Co-based alloys, C precursors, surface modifiers, and Ru alloys for making the non-precious metal catalysts. Good quality data generated. Good approach from a materials viewpoint. Objectives appear to include too many paths to be realistically accomplished with the resources and time allotted. Should include industry collaboration.
FC-16	*Low-Platinum Catalysts for Oxygen Reduction at PEMFC Cathodes; NRL; Karen Swider-Lyons*	3.06		X			Maintain collaborations and connections with industry and other organizations.
FC-17	*Low Pt Loading Fuel Cell Electrocatalysts; Brookhaven Nat. Lab.; Radoslav Adzic*	3.40		X			Solid approach and impressive results. Carry out more fuel cell testing (with cycling) of activity and stability of promising candidate catalysts.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-18	*Development of a Low-Cost, Durable Membrane and MEA for Stationary and Mobile Fuel Cell Applications; Arkema Chemicals; Michel Foure*	2.92		X			Innovative, novel approach to low cost membrane; reasonable polymer chemistry with beginning of life performance; good progress in learning. Inclusion of postmortem failure analysis on samples is good, but durability testing needs to include stress factors. Progress appears to be slow, should consider postponing scale-up. No discussion of membrane morphology. Publish more.
FC-19	*Development of High-Performance, Low-Pt Cathodes Containing New Catalysts and Layer Structures; Superior MicroPowders; Paolina Atanassova*	2.91		X			Well-focused relevant project; expertise in powder synthesis and the collaboration with fuel cell membrane suppliers for characterization activities are strong points; cycle testing is essential. Needs theoretical approach in addition to rapid screening to identify high performance catalyst. Opportunities to alter or modify the carbon support materials should be part of durability studies.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-20	*Platinum Recycling Technology Development; Ion Power, Inc.; Stephen Grot*	3.03		X			Viability of high efficiency recovery of platinum has been demonstrated. Development of a cost model is necessary, as cost should be a primary driver of this project to meet DOE Target. PGMs are a major cost factor in PEMFC, thus recycling is necessary to a sustainable PEMFC industry. Emissions from catalyst combustion needs to be addressed.
FC-21	*Platinum Group Metal Recycling Technology Development; Engelhard; Larry Shore*	2.91		X			Development of emissions free platinum recovery process is viable. Accelerate timeline to drive selection of best approach and focusing of resources. Backup option of pyrometallurgical approach should not be dismissed because of loss of polymer, which is only of marginal value.
FC-22	*Scale-Up of Carbon/Carbon Bipolar Plates; Porvair Corp.; David Haack*	3.02		X			Met DOE's technical targets, and estimate is that cost targets can be met at sufficient volumes. Demonstrated 20-cell stack with performance equal or better to machined graphite. Project will be completed in mid-FY2006 with process/quality control improvements developed and validated.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-23	*Cost-Effective Surface Modification for Metallic Bipolar Plates; ORNL; Karren More for Mike Brady*	3.31		X			Good progress on this project. Focus on cost-effective Fe-Cr alloys next year.
FC-24	*Water Gas Shift Catalysis; ANL; Theodore Krause*	2.65			X		Project discontinued due to shift in focus of fuel processing subprogram.
FC-25	*Catalysts for Autothermal Reforming; ANL; Theodore Krause*	2.68			X		Project discontinued due to shift in focus of fuel processing subprogram.
FC-26	*Selective Catalytic Oxidation of Hydrogen Sulfide; ORNL; Viviane Schwartz*	3.19		X			Good sulfur sorbents have been identified. Need to develop process to regenerate bed.
FC-27	*Cost and Performance Enhancements for a PEM Fuel Cell Turbocompressor; Honeywell; Mark K. Gee*	2.89		X			The incorporation of variable nozzle geometry to flatten efficiency and foil bearings for durability were important steps. Need to increase collaboration with the OEMs. Need to build and test a system. Project to be completed in FY2006.
FC-28	*Development and Testing of a Toroidal Intersecting Vane Machine (TIVM) Air Management System; Mechanology, LLC; Sterling Bailey*	3.11				X	Very novel compressor/expander design. However, the durability of the design is still uncertain since it remains to be analyzed and tested. Device has not been demonstrated that it is capable of high efficiencies promised. Project will be complete at the end of FY2005.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-29	*Development of a Thermal and Water Management (TWM) System for PEM Fuel Cells; Honeywell; Giullermo Pont*	3.03		X			Two humidification designs selected (enthalpy wheel, membrane humidifier) and design and evaluation is continuing. Aerospace and automotive heat exchangers evaluated – AL foam, microchannel and aerospace designs selected. Thermal and water management is essential to compact, reliable, durable fuel cell power systems.
FC-30	*Development of Sensors for Automotive PEM-based Fuel Cells; UTCFC; Brian Knight*	3.06				X	ATMI and NexTech are doing the developmental work. UTC and IIT are testing and evaluating sensors. Project will be completed by September 2005.
FC-31	*2005 DOE Hydrogen Program Sensor Development; Honeywell; Richard Gehman*	1.91			X		With the no-go decision on fuel processing, the chemical sensor work ended at the design stage. Of the remaining physical sensors, the main area of interest was humidity. When it became apparent that work would not meet program objectives and the company no longer had the expertise in-house to continue, research terminated.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-32	*DMFC Prototype Demonstration for Consumer Electronics Applications; MTI MicroFuel Cells; Bob Sievers*	2.51		X			Demonstration and real-world validation of a complete, integrated DMFC system for consumer electronics. High volume manufacturing techniques will be developed to reduce cost to levels appropriate for market entry. Project began in Fall 2004.
FC-33	*DMFC Power Supply for All-Day True-Wireless Mobile Computing; Polyfuel, Inc.; Brian Wells*	2.32		X			The heart of the project is the development of a next generation hydrocarbon membrane that will be engineered to meet the requirements of a DMFC system. Work will be done to miniaturize and integrate the remaining balance of plant components into a package small enough to fit into a laptop computer. Project began in Fall 2004.
FC-34	*Direct Methanol Fuel Cells; LANL; Piotr Zelenay*	3.23			X		Project discontinued to focus R&D efforts on direct hydrogen fuel cells.
FC-35	*Fuel Cell Systems Analysis; ANL; Rajesh Ahluwalia*	3.23		X			Excellent analyses. It would be extremely helpful for fuel cell manufacturers to supply experimental data to validate the models.
FC-36	*Bipolar Plate-Supported Solid Oxide Fuel Cell "Tuffcell"; ANL; J. David Carter*	2.63			X		Project discontinued to focus R&D efforts on direct hydrogen fuel cells.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-37	*Effect of Fuel and Air Impurities on Fuel Cell Performance; LANL; Fernando Garzon*	2.92		X			Consider internally generated impurities such as those from membrane decomposition or bipolar plate corrosion. Steer away from impurities found in reformate. Strengthen ties to the FreedomCAR Codes & Standards Technical Team.
FC-38	*Neutron Imaging Study of the Water Transport Mechanism in a Working Fuel Cell; NIST; Muhammad Arif*	3.52		X			Excellent collaboration with industry, national labs and universities but need to continue to make results of their work publicly available.
FC-39	*Microstructural Characterization of PEM Fuel Cell MEAs; ORNL; Karren More*	3.20		X			Intriguing results. More emphasis needed on post-mortem analysis (can results be quantified?) and characterization of decay mechanisms.
FC-40	*PEM Fuel Cell Durability; LANL; Rodney Borup*	3.85		X			Excellent work. Increased funding for FY2006.
FC-41	*Sub-Freezing Fuel Cell Effects; LANL; Bryan Pivovar*	3.49		X			Excellent start for this project. Increased funding in FY2006. Keep focused on the fundamentals.
FC-42	*Fiber Optic Temperature Sensors for PEM Fuel Cells; ORNL; Tim McIntyre*	3.11		X			Confirm that placement of probes in flow field does not impact measurements.
FC-43	*Research and Development for Off-road Fuel Cell Applications; Idatech; Erik Simpkins*	2.66		X			While the target application will not have a significant impact on reducing oil imports, the program does offer opportunities to make improvements to fuel cell systems that could translate to transportation applications.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FC-44	*50 kW Absorption Enhanced Natural Gas Reformer; ChevronTexaco; Jim Stevens*	2.93		X			Project should continue to improve materials and increase materials durability. Improving purification of the hydrogen stream well above 98% is needed. Sulfur issues should be addressed.
FC-45	*Cost-effective High-efficiency Advanced Reforming Module; Nuvera; Tom Holmes*	2.66		X			System Demonstration will be performed in FY2006. Nuvera should consider more publishing and presenting of their work.
FC-46	*150 kW PEM Fuel Cell Power Plant Verification; UTC; Tom Clark*	3.04		X			Project received good reviews. Future tests should be performed with reformate.
FC-47	*Back-up/Peak-Shaving Fuel Cells; Plug Power; John Vogel*	3.29		X			Excellent work. System will be sent to ANL for independent testing.
FC-48	*Economic Analysis of Stationary PEM Fuel Cell Systems; Battelle; Harry J. Stone*	2.63		X			Battelle will focus on a 50 kW natural gas system. Expand interactions with fuel cell OEMs on values used in the analysis.
FC-49	*Advanced Buildings PEM Fuel Cell System; IdaTech; Kyle Taylor*	2.68		X			Closely monitor alpha system operation, efficiency, and durability, with possible modifications to program based on these results.
FC-50	*Investigating Failure in Polymer-Electrolyte Fuel Cells; LBNL; John Newman*	2.88		X			Premier modeling group. Need to consider anisotropic temperature and gas flow distributions. Also consider other fuel cell designs besides UTC's.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FCP-01	*Montana PEM Membrane Degradation Study; Montana State Univ.; Lee Spangler*	2.78		X			Obtaining voltage signatures for MEA degradation is innovative and interesting. Excellent use of MRI. Good theoretical know-how in three different but supplementary areas. Enthusiastic researchers; should tie data to potential failure mechanisms; light on publications for a university. Lacks collaborations with outside researchers i.e. industry, and adding MEA supplier would be beneficial.
FCP-02	*Development of Higher Temperature Membrane and Electrode Assembly for Proton Exchange Membrane Fuel Cell Device; Oxford Perf. Matls.; Tony DeCarmine*	2.28		X			Initial concept was innovative; PEEKs is chemically and mechanically robust. PI is enthusiastic. Source of SPEKK material is no longer available; may have to look at crosslinking options as membrane has low conductivity and dissolves in water.
FCP-03	*Advanced Fuel Cell Membranes Based on Heteropolyacids; NREL; John Turner*	2.41		X			Next year need to show progress immobilizing the HPA in the ionomer and then demonstrating durability.
FCP-04	*Non-Nafion Membrane Electrode Assemblies; LANL; Bryan Pivovar*	2.99		X			Continue work on H_2/air fuel cells. Address cost benefits of non-Nafion membranes and electrodes.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FCP-05	*Hydrocarbon Membrane; SNL; Christopher Cornelius*	2.86		X			Very promising results at high temperature and moderately low RH. Measure conductivity of new membranes as a function of temperature and RH.
FCP-08	*Cathode Electrocatalysis: Platinum Stability and Non-Platinum Catalysts; ANL; Debbie Myers*	3.05		X			Good work on Pt dissolution. MEA testing of Au will be critical to determine whether to continue the work on Au.
FCP-11	*Modeling and Control of an SOFC APU; PNNL; Mo Khaleel*	3.10			X		Project will not be continued in FY2006 due to programmatic priorities.
FCP-12	*Corrosion Protection of Metallic Bipolar Plates for Fuel Cells; NREL; John Turner*	2.95		X			Check stampability of the stainless steel materials. Need to define timeline and long term plans.
FCP-14	*Fuel Processors for PEM Fuel Cells; U of Michigan; Levi Thompson*	2.18				X	Project is about to end.
FCP-15	*Plate-Based Fuel Processing System; Catalytica; David Yee*	2.73			X		Project terminated based on the DOE On-Board Fuel Processing No-Go Decision. Project will be completed by end of FY2005. Results may have applicability in military applications.
FCP-16	*Application of Advanced CAE Methods for Quality and Durability of Fuel Cell Components; NREL; Ken Kelly*	2.59		X			Focus on more generic research beneficial to all stakeholders. Must find more collaborators beyond Plug Power.
FCP-17	*Residential Fuel Cell Demonstration by the Delaware County Electric Cooperative, Inc.; Delaware Co. Electric Co-op; Mark Schneider*	3.06		X			Congressionally-directed multi-year project. The performance results should be made widely available to the fuel cell community. The energy storage specifics need to be identified.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FCP-18	*Smart Fuel Cell Operated Residential Micro Micro-Grid Community Grid Community; U of S. Alabama; Mohammad Alam*	2.34		X			Congressionally-directed multi-year project. The project is not consistent with the DOE Multi-Year Research, Development, and Demonstration Plan.
FCP-19	*Fuel Cells Vehicle Systems Analysis (Fuel Cell Freeze Investigation); NREL; Ahmad Pesaran*	2.54			X		Fuel Cell program has shifted away from systems to component work.
FCP-20	*Graphite-based Components for Thermal Management in Fuel Cell Systems; ORNL; Edgar Lara-Curzio*	2.74		X			Need to obtain cost projections for various combinations of graphite fibers and weaving and bonding (epoxy) methodologies.
FCP-25	*Fuel Processor R&D; ANL; Shabbir Ahmed*	2.37		X			Focus must be on meeting distributed generation technical targets. Useful to get input from stakeholders.
FCP-26	*Fore Court Fuel Processing; PNNL; Greg Whyatt*	3.03			X		Project discontinued to focus critical R&D efforts on fuel processing for stationary applications.
FCP-27	*Component Benchmarking; LANL; Tommy Rockward*	3.46		X			Outstanding collaborative effort. Expand test plan to include longevity and durability testing.
FCP-28	*Fundamental Science for Performance, Cost and Durability; LANL; Bryan Pivovar*	3.28		X			Continue exploratory work under high pH conditions. Consider transferring reference electrode work to other fuel cell researchers.
FCP-29	*Fuel Cell Testing; ANL; Ira Bloom*	2.56		X			Need to show more test data (which is difficult since companies want to keep info proprietary). At least include an overview table listing what kinds of stacks were tested.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
FCP-30	*Solid Oxide Fuel Cell Development for Auxiliary Power in Heavy Duty Vehicle Applications; Delphi; George Simopoulos*	2.93		X			The project only recently started in September 2004, therefore not much was accomplished prior to the review meeting. The team assembled by Delphi of Volvo and PACCAR will help provide real world perspective and requirements.
FCP-31	*Diesel Fueled SOFC for Class 7/Class 8 On-Highway Truck Auxiliary Power; Cummins; Dan Norrick*	2.99		X			The project only recently started in September 2004, therefore not much was accomplished prior to the review meeting. The team assembled by Cummins of SOFC-EFS and International Truck will provide the required expertise for the project and the ultimate integration into a truck will be a significant accomplishment.
FCP-32	*Fuel Cell APU; UC Davis; Christie-Joy Brodrick*	2.62				X	This project was completed in FY2005.

Technology Validation:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
TV-02	*DTE Energy Hydrogen Technology Park; DTE Energy; Rob Regan*	3.58		X			Project just beginning to produce data. Well-constructed project with very competent team. No additional funds but will be operated through FY2005 after which it will be picked up under a Learning Demonstration in FY2006.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
TV-03	*Hawaii Hydrogen Center for Development and Deployment of Distributed Energy Systems; Hawaii Natural Energy Inst.; Richard Rocheleau*	2.87		X			Project is composite of several different efforts that all include a power park application. New mayor cancelled old reciprocating engine deployment in Honolulu. Big Island of Hawaii is deploying large GE wind turbines. Proceed with biomass and wind projects on the Big Island of Hawaii.
TV-04	*Hydrogen Power Park - Business Opportunities Concept Project; Pinnacle; Raymond Hobbs*	3.52		X			Significant data presented from a myriad of projects. Very credible set of business plans in place. No additional funds required but will include wood chip gasifier and wind electrolysis projects in FY2005.
TV-05	*Validation of an Integrated System for a Hydrogen-Fueled Power Park; Air Products; Greg Keenan*	2.59		X			Project had range of reviews from 1) excellent project that investigates attractive co-production option to meet DOE targets to 2) a waste of tax payer dollars on existing patented technology. No additional funds required in FY2005 as it is part of APCI California Highway Initiative.
TV-06	*Novel Compression and Fueling Apparatus to Meet Hydrogen Vehicle Range Requirements; Air Products; Todd Carlson*	2.97				X	Project has identified an acceptable fluid for this application. APCI has made a Go decision to include at a refueling station.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
TV-07	*Controlled Hydrogen Fleet & Infrastructure Analysis; NREL; Keith Wipke*	3.41		X			Test protocols established and data center established to handle sensitive data. Supports Controlled Hydrogen Fleet & Infrastructure Demonstration effort.
TV-08	*California Hydrogen Infrastructure Project; Air Products; Mark Pedersen*	2.61			X		Congressionally-directed project. Reviews ranged from looking at many potential projects important to DOE to needs focus and introduction of cost effectiveness methodology to narrow the number of options being considered. No additional funds for earmark project in FY2006.
TV-09	*Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project; DaimlerChrysler; Klaus BonHoff*	3.47		X			Excellent team and important effort for DOE to get valuable data. Continue Learning Demonstration Project.
TV-10	*Hydrogen Fuel Cell Vehicle & Infrastructure Demonstration Program Review; Ford; Greg Frenette*	3.63		X			Excellent team and important effort for DOE to get valuable data. Continue Learning Demonstration Project.
TV-11	*Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project; Chevron Texaco; Rajesh Paulose*	3.34		X			Excellent team and important effort for DOE to get valuable data. Continue Learning Demonstration Project.
TV-12	*Hydrogen Vehicle and Infrastructure Demonstration and Validation; General Motors; Roz Sell*	3.54		X			Excellent team and important effort for DOE to get valuable data. Continue Learning Demonstration Project.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
TVP-01	*Hydrogen from Biomass for Urban Transportation; Clark Atlanta Univ.; Kofi Bota*	2.67		X			Project completed 200 hour tests but needs to operate for 1000 hours to complete validation of concept. $50,000 needed to complete 1000 hour validation test.
TVP-02	*Chattanooga Fuel Cell Demonstration Project; City of Chattanooga; Joe Ferguson*	2.32			X		Project suffers from assuming that there are no problems and the system will operate. Poor team research capability. No additional funds for earmark project in FY 2005.
TVP-04	*Power Parks System Simulation; SNL; Andy Lutz*	3.61		X			Excellent support for Power Park Projects with knowledgeable investigator.
TVP-05	*Technology Validation: Fuel Cell Bus Evaluations; NREL; Leslie Eudy*	3.28		X			Very good support analysis of bus projects and converting them to Learning Demo protocols.
TVP-06	*Auto-Thermal Reforming Based Refueling Station at SunLine; HyRadix/SunLine; John Harness*	2.96				X	Project completed and they are successfully marketing the technology.
TVP-08	*Hydrogen Filling Station; UNLV; Robert Boehm*	2.64			X		Congressionally-directed project. Need to make more progress on completing station and providing vehicles for use by the station.
TVP-11	*Hydrogen and Natural Gas Blends: Converting Light and Heavy Duty Vehicles; Collier Techs.; Neal Mulligan*	2.34				X	Las Vegas has converted 9 trucks to Hythane, and emissions are less than SULEV. Future conversions will be completed commercially and will add to the demand for hydrogen infrastructure. Recommend no further funding for this activity.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
TVP-12	*Fuel Cell Powered Underground Mine Loader Vehicle; Vehicle Projects LLC; David Barnes*	2.62			X		Congressionally-directed project. Good integration project in a marketplace where fuel cells might be competitive. Excellent collaboration. Not clear of project's significance to light duty vehicle program. Needs FY2005 funds from earmark to complete.
TVP-14	*Hydrogen Transition Infrastructure Analysis; NREL; Margo Melendez*	2.49				X	Analysis completed. Looks like government effort but needs industry input or effort will be ignored. GIS is effective tool and is an innovative approach. New case studies will be funded by Analysis to NREL with closer collaboration with industry. Important part of transition analysis.

Safety, Codes and Standards:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
SA-02	*Hydrogen Codes and Standards; NREL; Jim Ohi*	3.21		X			Project provides valuable input to the codes and standards process; will increase focus on hydrogen quality.
SA-03	*Research and Development for Hydrogen Safety, Codes and Standards; SNL; Jay Keller*	3.61		X			R&D role is critical as input to the codes and standards effort. Work will continue to shift toward experiments that examine real world risks.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
SA-04	*International Standards and Regulations; LANL; Cathy Padro*	2.69		X			International codes and standards activity and harmonization critical, but not clear the extent to which this activity helps this difficult process. Should increase coordination with DOT.
SA-05	*HAMMER Emergency Response Training for the Hydrogen Economy; PNNL; Bruce Kinzey*	2.96		X			Important activity, but will be difficult to implement training of very large number of people. In FY2006, training activity will be funded by the Education program element; this element will support development of training facilities.
SA-06	*Hydrogen Safety Review Panel; PNNL; Steven Weiner*	3.20		X			Overall Panel expertise is good, but the workload is high and impact on safety is hard to gauge.
SAP-01	*IEA Hydrogen Task 18: Evaluation of Integrated Demonstration Systems; Longitude 122 West, Inc.; Susan Schoenung*	3.41		X			Good to share this data and analysis – the overall focus is correct. However, the differing size and configurations of the projects makes it difficult to compare data.

Education:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
ED-02	*Hydrogen/Alternative Energy Center; Lansing Comm. College; Ruth Borger*	2.94		X			FY2004 Congressionally-directed project. Should be completed in FY2006 with no additional funding required.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
ED-03	*Shared Technology Transfer Project; Nicholls State Un.; John Griffin*	1.83		X			FY2004 Congressionally-directed project. Should be completed in FY2006 with no additional funding required.
ED-04	*Montana Hydrogen Futures Project; U. of Montana; Paul Williamson*	2.54		X			FY2004 Congressionally-directed project. Should be completed in FY2006 with no additional funding required.
ED-05	*Hydrogen Technology and Energy Curriculum (HyTEC); U of Cal. Berkeley; Barbara Nagle*	3.46		X			Continue funding in FY2006. Build on partnerships and use established network for widespread dissemination and to extend project reach.
ED-06	*H2 Educate!; NEED; Mary Spruill*	3.66		X			Continue funding in FY2006. Build on partnerships and use established network for widespread dissemination and to extend project reach.
EDP-01	*Baseline Knowledge Assessment; ORNL; Tim Armstrong*	3.30		X			Disseminate survey results as widely as possible. Plan for follow-on surveys in out years.
EDP-02	*Hydrogen Technology Overview Publication and Program Information Kit; Andersen Creative; Cindi Andersen*	2.14		X			Continue to support. Coordinate with other educational materials development projects to ensure comprehensive coverage and avoid duplication. Disseminate widely.
EDP-03	*Understanding the Hydrogen Economy; Energy & Envr. Analysis; Rick Tidball*	2.83		X			Continue to support. Coordinate with other educational materials development projects to ensure comprehensive coverage and avoid duplication. Disseminate widely.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
EDP-04	*Hydrogen Education and Outreach; NHA; Patrick Serfass*	3.58		X			Continue to support. Coordinate with other educational materials development projects to ensure comprehensive coverage and avoid duplication. Support from industry partners and leverage current product distribution plan.
EDP-05	*Hydrogen Technology Learning Centers; STAC; Bob Kripowicz*	2.71		X			Continue to support. Focus on collaboration among the three learning centers.

Analysis:

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
AN-02	*Moving Toward Consistent Analysis in the HFC&IT Program: H2A; NREL; Maggie Mann*	3.68		X			The first phase of the project was presented at the Merit Review and received a high approval rating. The second phase of the project is being funded in FY2005 and will continue to be funded in FY2006.
AN-03	*Hydrogen Transition Modeling and Analysis: HYTRANS v. 1.0; ORNL; David Greene*	3.27		X			This project is a co-funded project with PBA. The project should continue since it will be a useful model for transitional analysis. The model is planned to be linked to the Macro System Model which is under development.

Project No.	Project title, Performing Org	Final Score	New	Continued	Dis - continued	Project Completed	DOE Summary Comment
AN-04	*WinDS-H2 Model and Analysis; NREL; Walter Short*	2.91	X				This is a new project which uses the past work on the WinDS model for the new project basis. The project startup was delayed due to funding, which is reflected in the lower score. This project will provide an optimization tool for future hydrogen infrastructure and production analysis. The model will include renewable hydrogen production pathways in the model structure.
AN-05	*Technical and Economic Studies of Regional Transition Strategies toward Widespread Use of Hydrogen Energy; UC Davis; Joan Ogden*	3.27	X				This project is useful for the infrastructure development and builds on some of the past UC Davis projects. The project started late due to funding delays. Also, the funding for the project was reduced due to Congressional earmarks. The project is planned to continue in FY2006.
AN-06	*Fuel Choice for FCVs: Hydrogen Infrastructure Costs; TIAX LLC; Stephen Lasher*	3.16		X			This project provides valuable information about renewable resources and their values. This project is an extension of an existing project. The funding for this project was delayed resulting in a delay in startup.
ANP-01	*Hydrogen Production in a GHG-Constrained Situation: Major Results & Conclusions; Tellus; Bill Dougherty*	2.75				X	This project was funded by the Production Subprogram. The project is complete.

TABLE OF CONTENTS

INTRODUCTION ---------- **1**

HYDROGEN PRODUCTION AND DELIVERY ---------- **9**

PD-01: Hydrogen Production & Delivery ---------- 12
PD-02: A Reversible Planar Solid Oxide Fuel-Fed Electrolysis Cell and Solid Oxide Fuel Cell for Hydrogen and Electricity Production Operating on Natural Gas/Biogas ---------- 14
PD-03: Autothermal Cyclic Reforming Based Hydrogen Generating & Dispensing System ---------- 17
PD-04: Development of a Turnkey Hydrogen Fueling Station ---------- 20
PD-05: Development of a Natural Gas-to-Hydrogen Fueling System ---------- 23
PD-06: Production of Hydrogen by Biomass Reforming ---------- 25
PD-07: Hydrogen Generation from Biomass-Derived Carbohydrates via the Aqueous-Phase Reforming (APR) Process ---------- 27
PD-11: Advanced Hydrogen Transport Membranes for Vision 21 Fossil Fuel Plants ---------- 30
PD-12: Scale-up of Microporous Inorganic Hydrogen-Separations Membranes ---------- 34
PD-13: Low Cost Hydrogen Production from Biomass Using Novel Membrane Gasification Reactor ---------- 38
PD-14: A Novel Slurry-Based Biomass Reforming Process ---------- 41
PD-15: Maximizing Photosynthetic Efficiencies and Hydrogen Production in Microalgal Cultures ---------- 43
PD-16: Biological Systems for Hydrogen Photoproduction ---------- 45
PD-17: Creation of Designer Alga for Efficient and Robust Production of Hydrogen ---------- 48
PD-18: Fermentative Approaches to Hydrogen Production ---------- 51
PD-19: Hydrogen Reactor Development and Design for Photofermentation and Photolytic Processes ---------- 54
PD-20: Photoelectrochemical Hydrogen Production: SHGR Program Subtask ---------- 57
PD-21: Photoelectrochemical Water Systems for Hydrogen Production ---------- 59
PD-23: Low-Cost, High-Pressure Hydrogen Generator ---------- 62
PD-24: High Temperature Solid Oxide Electrolyzer System ---------- 65
PD-25: Renewable Electrolysis Integrated System Development and Testing ---------- 69
PD-26: Alkaline, High Pressure Electrolysis ---------- 72
PD-27: Sulfur-Iodine Thermochemical Cycle ---------- 74
PD-28: Development of Solar-Powered Thermochemical Production of Hydrogen from Water ---------- 77
PD-29: Alternative Thermochemical Cycle Evaluation ---------- 80
PD-30: High Temperature Heat Exchanger Development ---------- 83
PD-31: NHI System Interface and Support Systems ---------- 87
PD-32: H2A Delivery Analysis ---------- 89
PD-33: Hydrogen Permeability and Integrity of Hydrogen Delivery Pipelines ---------- 91
PD-34: Reversible Liquid Carriers for an Integrated Production, Storage and Delivery of Hydrogen ---------- 94
PD-35: Materials Solutions for Hydrogen Delivery in Pipelines ---------- 97
PDP-01: Enhanced Hydrogen Production Integrated with CO_2 Separation in a Single-Stage Reactor ---------- 99
PDP-03: Integrated Ceramic Membrane System for Hydrogen Production ---------- 103
PDP-04: Low Cost Hydrogen Production Platform ---------- 106
PDP-05: Solid Oxide Fuel Cell Carbon Sequestration ---------- 108
PDP-07: High Pressure Distributed Ethanol Reforming ---------- 111
PDP-08: Hydrogen Production for Fuel Cells via Reformation of Coal-Derived Methanol ---------- 113
PDP-09: Hydrogen Production via a Commercially Ready Inorganic Membrane Reactor ---------- 117
PDP-10: New York State Hi-Way Initiative ---------- 120
PDP-14: Development of Dense Ceramic Membranes for Hydrogen Separation ---------- 123
PDP-15: Ceramic Membrane Reactor Systems for Converting Natural Gas to Hydrogen and Synthesis Gas (ITM Syngas) ---------- 126
PDP-16: Cost-Effective Method for Producing Self-Supporting Pd Alloy Membrane for Use in the Efficient Production of Coal-derived Hydrogen ---------- 128
PDP-17: Defect-Free Thin Film Membranes for H_2 Separation and Isolation ---------- 131
PDP-19: Membrane Applications for Nuclear Hydrogen Production Processes ---------- 134
PDP-20: Materials for High-Temperature Thermochemical Processes ---------- 137

PDP-24: A Novel Membrane Reactor for Hydrogen Production from Coal ----------140
PDP-25: Hydrogen Separation ----------143
PDP-27: Startech Hydrogen Production ----------147
PDP-28: EVermont Renewable Hydrogen Fueling System ----------149
PDP-33: Discovery of Photocatalysts for Hydrogen Production----------151
PDP-39: Hydrogen Production - Increasing the Efficiency of Water Electrolysis----------154
PDP-41: Hydrogen Generation from Electrolysis ----------157
PDP-42: System Design and New Materials for Reversible Solid-Oxide, High-Temperature Steam Electrolysis----------160
PDP-45: Hybrid Sulfur Thermochemical Process Development----------162
PDP-46: Combined Reverse-Brayton Joule-Thompson Hydrogen Liquefaction Cycle ----------165
PDP-47: Active Magnetic Regenerative Liquefier (AMRL) Development ----------167
PDP-48: Hydrogen Embrittlement of Pipeline Steels: Causes & Remediation ----------170
PDP-49: Hydrogen Regional Infrastructure Program in Pennsylvania----------172
PDP-55: Distributed Bio-Oil Reforming----------174
PDP-57: Developing Improved Materials to Support the Hydrogen Economy ----------177

HYDROGEN STORAGE ----------181

ST-01: Hydrogen Storage ----------184
ST-02: Development of Metal Hydrides at Sandia National Laboratories----------187
ST-03: Catalytically Enhanced Hydrogen Storage Systems ----------193
ST-04: High Density Hydrogen Storage System Demonstration Using $NaAlH_4$ Complex Compound Hydrides----------199
ST-05: Discovery of Novel Complex Metal Hydrides for Hydrogen Storage through Molecular Modeling and Combinatorial Methods ----------204
ST-06: Complex Hydride Compounds with Enhanced Hydrogen Storage Capacity----------208
ST-07: Sub-Nanostructured Non-Transition Metal Complex Grids for Hydrogen Storage ----------212
ST-09: Clean Energy Research at the University of South Carolina ----------217
ST-10: Fuel Cell and Hydrogen Research University of South Florida----------222
ST-12: Process for the Regeneration of Sodium Borate to Sodium Borohydride for Use as a Hydrogen Storage Source----------226
ST-13: Chemical Hydride Slurry for Hydrogen Production and Storage ----------231
ST-14: Development of New Carbon-Based Sorbent Systems for an Effective Containment of Hydrogen -----235
ST-15: Low Cost, High Efficiency, High Pressure Hydrogen Storage----------240
ST-16: Advanced Concepts for Containment of Hydrogen and Hydrogen Storage Materials ----------244
ST-17: Advanced Manufacturing Technologies for Renewable Energy Applications ----------248
ST-18: DOE Carbon-based Materials Center of Excellence: NREL Activities and Overview ----------252
ST-19: Analyses of Hydrogen Storage Materials and On-Board Systems----------257
ST-21: Standardized Testing Program for Chemical Hydride and Carbon Storage Technologies ----------262
STP-58: Development and Characterization of Novel Complex Hydrides----------266

FUEL CELLS----------269

FC-01: Fuel Cell R&D ----------271
FC-02: Integrated Manufacturing for Advanced MEAs----------274
FC-03: Advanced MEAs for Enhanced Operating Conditions, Amenable to High Volume Manufacture -------277
FC-04: Development of High Temperature Membranes and Improved Cathode Catalysts for PEM Fuel Cells----------280
FC-05: Electrocatalyst Supports and Electrode Structures----------284
FC-06: Development of New Polymer Electrolytes for Operation at High Temperature and Low Relative Humidity ----------286
FC-07: High-Temperature Polymer Electrolyte Membranes ----------290
FC-08: Development of Polybenzimidazole-based, High Temperature Membrane and Electrode Assemblies for Stationary and Automotive Applications ----------293
FC-09: Enabling Commercial PEM Fuel Cells with Breakthrough Lifetime Improvements----------296

FC-10: New Electrocatalysts for Fuel Cells ----------299
FC-11: Non-Precious Metal Catalysts ----------303
FC-12: MEA and Stack Durability for PEM Fuel Cells ----------306
FC-13: Development of Transition Metal/Chalcogen Based Cathode Catalysts for PEM Fuel Cells ----------309
FC-14: Novel Approach to Non-Precious Metal Catalysts ----------312
FC-15: Novel Non-Precious Metals for PEMFC: Catalyst Selection through Molecular Modeling and Durability Studies ----------315
FC-16: Low-Platinum Catalysts for Oxygen Reduction at PEMFC Cathodes ----------318
FC-17: Low Pt Loading Fuel Cell Electrocatalysts ----------321
FC-18: Development of a Low-Cost, Durable Membrane and MEA for Stationary and Mobile Fuel Cell Applications ----------325
FC-19: Development of High-Performance, Low-Pt Cathodes Containing New Catalysts and Layer Structures ----------328
FC-20: Platinum Recycling Technology Development ----------331
FC-21: Platinum Group Metal Recycling Technology Development ----------334
FC-22: Scale-Up of Carbon/Carbon Bipolar Plates ----------337
FC-23: Cost-Effective Surface Modification for Metallic Bipolar Plates ----------340
FC-24: Water Gas Shift Catalysis ----------343
FC-25: Catalysts for Autothermal Reforming ----------346
FC-26: Selective Catalytic Oxidation of Hydrogen Sulfide ----------349
FC-27: Cost and Performance Enhancements for a PEM Fuel Cell Turbocompressor ----------352
FC-28: Development and Testing of a Toroidal Intersecting Vane Machine (TIVM) Air Management System ----------355
FC-29: Development of a Thermal and Water Management (TWM) System for PEM Fuel Cells ----------358
FC-30: Development of Sensors for Automotive PEM-based Fuel Cells ----------361
FC-31: 2005 DOE Hydrogen Program Sensor Development ----------364
FC-32: DMFC Prototype Demonstration for Consumer Electronics Applications ----------367
FC-33: DMFC Power Supply for All-Day True-Wireless Mobile Computing ----------371
FC-34: Direct Methanol Fuel Cells ----------374
FC-35: Fuel Cell Systems Analysis ----------376
FC-36: Bipolar Plate-Supported Solid Oxide Fuel Cell "Tuffcell" ----------379
FC-37: Effect of Fuel and Air Impurities on Fuel Cell Performance ----------382
FC-38: Neutron Imaging Study of the Water Transport Mechanism in a Working Fuel Cell ----------385
FC-39: Microstructural Characterization of PEM Fuel Cell MEAs ----------388
FC-40: PEM Fuel Cell Durability ----------391
FC-41: Sub-Freezing Fuel Cell Effects ----------394
FC-42: Fiber Optic Temperature Sensors for PEM Fuel Cells ----------397
FC-43: Research and Development for Off-road Fuel Cell Applications ----------401
FC-44: 50 kW Absorption Enhanced Natural Gas Reformer ----------404
FC-45: Cost-effective High-efficiency Advanced Reforming Module ----------407
FC-46: 150 kW PEM Fuel Cell Power Plant Verification ----------410
FC-47: Back-up/Peak-Shaving Fuel Cells ----------413
FC-48: Economic Analysis of Stationary PEM Fuel Cell Systems ----------415
FC-49: Advanced Buildings PEM Fuel Cell System ----------418
FC-50: Investigating Failure in Polymer-Electrolyte Fuel Cells ----------422
FCP-01: Montana PEM Membrane Degradation Study ----------425
FCP-02: Development of Higher Temperature Membrane and Electrode Assembly for Proton Exchange Membrane Fuel Cell Device ----------428
FCP-03: Advanced Fuel Cell Membranes Based on Heteropolyacids ----------431
FCP-04: Non-Nafion Membrane Electrode Assemblies ----------434
FCP-05: Hydrocarbon Membrane ----------437
FCP-08: Cathode Electrocatalysis: Platinum Stability and Non-Platinum Catalysts ----------440
FCP-11: Modeling and Control of an SOFC APU ----------443
FCP-12: Corrosion Protection of Metallic Bipolar Plates for Fuel Cells ----------447
FCP-14: Fuel Processors for PEM Fuel Cells ----------450
FCP-15: Plate-Based Fuel Processing System ----------453

FCP-16: Application of Advanced CAE Methods for Quality and Durability of Fuel Cell Components ----------455
FCP-17: Residential Fuel Cell Demonstration by the Delaware County Electric Cooperative, Inc. ----------459
FCP-18: Smart Fuel Cell Operated Residential Micro Micro-Grid Community Grid Community ----------462
FCP-19: Fuel Cells Vehicle Systems Analysis (Fuel Cell Freeze Investigation) ----------465
FCP-20: Graphite-based Components for Thermal Management in Fuel Cell Systems ----------467
FCP-25: Fuel Processor R&D ----------470
FCP-26: Fore Court Fuel Processing ----------473
FCP-27: Component Benchmarking----------475
FCP-28: Fundamental Science for Performance, Cost and Durability ----------477
FCP-29: Fuel Cell Testing ----------480
FCP-30: Solid Oxide Fuel Cell Development for Auxiliary Power in Heavy Duty Vehicle Applications ---------483
FCP-31: Diesel Fueled SOFC for Class 7/Class 8 On-Highway Truck Auxiliary Power ----------485
FCP-32: Fuel Cell APU ----------488

TECHNOLOGY VALIDATION ----------491

TV-01: Technology Validation Sub-Program----------493
TV-02: DTE Energy Hydrogen Technology Park----------494
TV-03: Hawaii Hydrogen Center for Development and Deployment of Distributed Energy Systems ----------496
TV-04: Hydrogen Power Park - Business Opportunities Concept Project ----------498
TV-05: Validation of an Integrated System for a Hydrogen-Fueled Power Park ----------501
TV-06: Novel Compression and Fueling Apparatus to Meet Hydrogen Vehicle Range Requirements ----------504
TV-07: Controlled Hydrogen Fleet & Infrastructure Analysis ----------507
TV-08: California Hydrogen Infrastructure Project----------510
TV-09: Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project----------513
TV-10: Hydrogen Fuel Cell Vehicle & Infrastructure Demonstration Program Review ----------515
TV-11: Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project----------517
TV-12: Hydrogen Vehicle and Infrastructure Demonstration and Validation ----------519
TVP-01: Hydrogen from Biomass for Urban Transportation----------521
TVP-02: Chattanooga Fuel Cell Demonstration Project ----------524
TVP-04: Power Parks System Simulation ----------527
TVP-05: Technology Validation: Fuel Cell Bus Evaluations ----------530
TVP-06: Auto-Thermal Reforming Based Refueling Station at SunLine----------532
TVP-08: Hydrogen Filling Station ----------535
TVP-11: Hydrogen and Natural Gas Blends: Converting Light and Heavy Duty Vehicles ----------538
TVP-12: Fuel Cell Powered Underground Mine Loader Vehicle----------540
TVP-14: Hydrogen Transition Infrastructure Analysis----------543

SAFETY, CODES AND STANDARDS ----------547

SA-01: Safety, Codes and Standards----------549
SA-02: Hydrogen Codes and Standards ----------551
SA-03: Research and Development for Hydrogen Safety, Codes and Standards ----------555
SA-04: International Standards and Regulations ----------558
SA-05: HAMMER Emergency Response Training for the Hydrogen Economy ----------561
SA-06: Hydrogen Safety Review Panel ----------564
SAP-01: IEA Hydrogen Task 18: Evaluation of Integrated Demonstration Systems ----------567

EDUCATION ----------571

ED-01: Hydrogen Education ----------573
ED-02: Hydrogen/Alternative Energy Center----------574
ED-03: Shared Technology Transfer Project ----------577
ED-04: Montana Hydrogen Futures Project----------580
ED-05: Hydrogen Technology and Energy Curriculum (HyTEC) ----------584
ED-06: H2 Educate!----------587

EDP-01: Baseline Knowledge Assessment ----------590
EDP-02: Hydrogen Technology Overview Publication and Program Information Kit----------592
EDP-03: Understanding the Hydrogen Economy ----------594
EDP-04: Hydrogen Education and Outreach ----------596
EDP-05: Hydrogen Technology Learning Centers----------598

SYSTEMS ANALYSIS ----------601

AN-01: Systems Analysis----------603
AN-02: Moving Toward Consistent Analysis in the HFC&IT Program: H2A----------605
AN-03: Hydrogen Transition Modeling and Analysis: HYTRANS v. 1.0 ----------608
AN-04: WinDS-H2 Model and Analysis ----------611
AN-05: Technical and Economic Studies of Regional Transition Strategies toward Widespread Use of Hydrogen Energy ----------614
AN-06: Fuel Choice for FCVs: Hydrogen Infrastructure Costs----------617
ANP-01: Hydrogen Production in a GHG-Constrained Situation: Major Results & Conclusions ----------620

APPENDIX A: ATTENDEE LIST ----------623

APPENDIX B: PROJECTS NOT REVIEWED ----------658

APPENDIX C: FEEDBACK AND RECOMMENDATIONS ----------664

APPENDIX D: EVALUATION FORMS----------677

INTRODUCTION

This report is a summary of comments from the Peer Review Panel at the FY 2005 DOE Hydrogen Program Annual Merit Review, held on May 23-26, 2005, 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 2005 accomplishments and FY 2006 plans for DOE laboratory programs and industry/university cooperative agreements and R&D that supports development.
- Provide an opportunity for program participants (hydrogen production manufacturers, hydrogen storage manufacturers, fuel cell manufacturers, etc.) 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.

During the plenary session on the first morning, in addition to remarks from the DOE Hydrogen Program and the programs of the European Commission and Japan, the Office of Science provided an overview of its current and future hydrogen and fuel cell efforts. This presentation was a prelude to the 2006 Annual Merit Review, which will expand in scope to cover Office of Science (SC) projects that were awarded in FY 2005. Projects from the Fossil Energy (FE) and Nuclear Energy (NE) program areas were also included in this FY 2005 Annual Merit Review and Peer Evaluation.

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, hydrogen production manufacturers, hydrogen storage manufacturers, fuel cell manufacturers, 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	Abdel-Baset, Tarek	Daimler-Chrysler Corporation
2	Adams, Jesse	Department of Energy - Golden Field Office
3	Adjemian, Kev	Arkema, Inc.
4	Adzic, Radoslav	Brookhaven National Laboratory
5	Anderson, Arlene	Department of Energy
6	Anderson, Michele	Office of Naval Research
7	Arbuckle, Sheral	Ford Motor Company
8	Archer, Douglas	Department of Energy

9	Armstrong, Tim	Oak Ridge National Laboratory
10	Atanasoski, Radaslov	3M
11	Atanassova, Paolina	Cabot Corporation
12	Bailey, Carol	SENTECH, Inc.
13	Bain, Addison	Consultant
14	Bakke, Paul	Department of Energy - Golden Field Office
15	Balachandran, Balu	Argonne National Laboratory
16	Bauer, David	Ford Motor Company
17	Bavarian, Farshad	Chevron Texaco
18	Benjamin, Thomas	Argonne National Laboratory
19	Blair, Larry	Consultant
20	Borup, Rod	Los Alamos National Laboratory
21	Bose, Arun	National Energy Technology Laboratory
22	Boyd, Lynnae	National Renewable Energy Laboratory
23	Cantão, Mauricio Pereira	LACTEC Instituto Tecnológico para Desenvolvimento
24	Carlson, Eric	TIAX
25	Carole, Tracy	Energetics
26	Carpenter, Joe	Department of Energy
27	Ceasar, Gerry	DOC/ National Institute of Standards and Technology
28	Chahine, Richard	Hydrogen Research Institute / UQTR
29	Chernicoff, Bill	U.S. Department of Transportation – RITA
30	Clark, Todd	Department of Energy
31	Contadini, J. Fernando	Petrobras/CENPES and Clamper Industria e Comercio S.A.
32	Conte, Mario	ENEA, Italy
33	Cross, James	Nuvera Fuel Cells, Inc.
34	Curry-Nkansah, Maria	BP
35	Debe, Mark	3M
36	DeCastro, Emory	De Nora N.A., Inc., E-TEK division
37	Dempsey, Roxanne	Department of Energy
38	Devlin, Pete	Department of Energy
39	DuBois, Jennifer	University of Notre Dame
40	Eisman, Glenn	Rennselaer Polytechnic Institute
41	Elam, Carolyn	Department of Energy - Golden Field Office
42	Ernst, Bill	Plug Power
43	Evans, Bob	National Renewable Energy Laboratory
44	Feinberg, Ed	Decision Support / Energy Consulting
45	Fiegenschuh, Karl	Ford Motor Company
46	Filiou, Constantina	European Commission
47	Fitzimmons, Tim	Department of Energy
48	Fletcher, Jim	University of North Florida
49	Frank, Maria Helena Troise	Petrobras/CENPES
50	Freeman, Scott	Daimler-Chrysler Corporation
51	Gabrielov, Alexei	Shell Chemical LP

52	Geyer, Bernadette	US Fuel Cell Council
53	Gross, Tom	Consultant
54	Gruber, Jill	Department of Energy - Golden Field Office
55	Haberman, Dave	Consultant
56	Hansel, Jim	Air Products and Chemicals
57	Hardis, Jonathan	DOC/ National Institute of Standards and Technology
58	Hennessey, Barbara	National Highway and Traffic Safety Administration
59	Hicks, Mike	3M Center
60	Hirano, Shinichi	Ford Motor Company
61	Hooker, Doug	Department of Energy - Golden Field Office
62	Johnson, Will	W L Gore
63	Jorgensen, Scott	General Motors Corporation
64	Kamat, Prashant	University of Notre Dame
65	Kerr, John	Lawrence Berkley National Laboratory
66	Kopasz, John	Argonne National Laboratory
67	Krause, Curt	ChevronTexaco
68	Kroposki, Benjamin	National Renewable Energy Laboratory
69	Kumar, Romesh	Argonne National Laboratory
70	Laine, Jerri	Tekes, The National Technology Agency, Embassy of Finland
71	Larkins, Jim	Georgetown Bus Program
72	Larsen, Robert	Argonne National Laboratory
73	Lasher, Stephen	TIAX
74	Laskin, Jay	Consultant
75	Lipp, Ludwig	FuelCell Energy
76	Lott, Melissa	QSS Group, Inc.
77	Lutz, Andy	Sandia National Laboratory
78	Mann, Maggie	National Renewable Energy Laboratory
79	Marianowski, Len	Consultant (retired from GTI)
80	Maroni, Victor	Argonne National Laboratory
81	Masten, David	General Motors Corporation
82	Maupin, Paul	Department of Energy Office of Science
83	Mazer, Jeff	Department of Energy
84	McGetrick, Jim	BP
85	McGrath, James	Virginia Tech
86	McQueen, Shawna	Energetics
87	Mettes, Jacques	Power and Energy
88	Moore, Graham	ChevronTexaco Technology Ventures
89	Moore, Tom	Consultant
90	Motyka, Ted	Savannah River National Laboratory
91	Myers, Debbie	Argonne National Laboratory
92	Neves, Jr., Newton Pimenta	CENEH
93	Noronha, Fabio Bellot	National Technology Institute
94	Ozokwelu, Dickson	Department of Energy

95	Padro, Cathy	Los Alamos National Laboratory
96	Parks, George	ConocoPhillips
97	Paul, Dilo	National Energy Technology Laboratory
98	Perry, Mike	UTC Fuel Cells, LLC
99	Peters, John	Montana State University
100	Peterson, Dave	Department of Energy - Golden Field Office
101	Pez, Guido	Air Products & Chemicals
102	Podolski, Walt	Argonne National Laboratory
103	Porter, Stephen	Proton Energy Systems
104	Powars, Charles	St. Croix Research
105	Quah, Michael	NextEnergy
106	Reinker, John	GE Global Research
107	Retureta, Stephanie	Department of Energy - Golden Field Office
108	Roan, Vernon	University of Florida
109	Robbins, John	ExxonMobil
110	Roelofs, Mark	DuPont
111	Rogers, Jerry	General Motors Corporation
112	Ross, Phil	Lawrence Berkley National Laboratory
113	Rubinstein, Leon	Shell Hydrogen LLC
114	Saeki, Margarida Júri	Universidade Estadual Paulista Júlio de Mesquita Filho
115	Sandrock, Gary	SunaTech, Inc.
116	Schlasner, Steve	ConocoPhillips
117	Schneider, Jesse	DaimlerChrysler RTNA
118	Serfass, Jeff	National Hydrogen Association
119	Shen, John	Department of Energy
120	Sims, Ron	Consultant
121	Sjoding, Dave	Washington State University
122	Smith, Brad	Shell Hydrogen LLC
123	Sofronis, Petros	University of Illinois
124	Steele, Mike	General Motors Corporation
125	Stevens, Jim	ChevronTexaco
126	Stone, Paul	Stone Team Technology (retired DowChemical)
127	Stroh, Ken	Los Alamos National Laboratory
128	Surdoval, Wayne	National Energy Technology Laboratory
129	Swartz, Scott	NexTech Materials, Ltd.
130	Taylor, Amy	Department of Energy
131	Thomas, George	Sandia National Laboratory (retired)
132	Ticianelli, Edson A.	Universidade de Sao Paulo
133	Titchen , John	Hydro Tasmania
134	Tran, Doanh	DaimlerChrysler Corporation
135	Tumas, Bill	Los Alamos National Laboratory
136	Tyler, Reginald	Department of Energy - Golden Field Office
137	Uihlein, James	BP
138	Vanderborgh, Nick	Consultant

139	Wagner, Fred	General Motors Corporation
140	Wagner, Fred	Energetics
141	Weimer, Alan	University of Colorado
142	Welch, Corey	National Renewable Energy Laboratory
143	Wells, Brian	PolyFuel
144	Wesson, Rose	National Science Foundation
145	Wheeler, Douglas	DJW Technology
146	Williams, Mark	National Energy Technology Laboratory
147	Wipke, Keith	National Renewable Energy Laboratory
148	Wolfe, Barbara	QSS
149	Wolverton, Chris	Ford Motor Company
150	Yancey, Lea	Department of Energy - Golden Field Office
151	Zalesky, Rick	ChevronTexaco
152	Zawodzinski, Tom	Case Western
153	Ziegler, Dick	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 in the Table 1 above a total of **153** panel members participated in the merit review process. A total of **191** projects were reviewed at the meeting and a total of **1208** evaluation forms were received from the Peer Review Panel (not every panel member reviewed every project). The 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 D to this report.

The five criteria and weights were:

- Criterion/Score 1: Relevance to overall DOE objectives (20%);
- Criterion/Score 2: Approach to performing the research and development (20%);
- Criterion/Score 3: Technical accomplishments and progress toward achieving the project and DOE goals (35%);
- Criterion/Score 4: Technology transfer and collaborations with industry, universities, and other laboratories (10%); and
- Criterion/Score 5: Approach to and relevance of proposed future research (15%).

The individual criterion scores from various reviewers were averaged together to obtain average scores for each of the five above-mentioned criteria for each project. All scores were used, i.e.

elimination of the highest/lowest scores or other score selection schemes were not employed. The resulting 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 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.

A few new projects were reviewed, where the third criterion (Technical Accomplishments) did not apply because of the projects' 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*

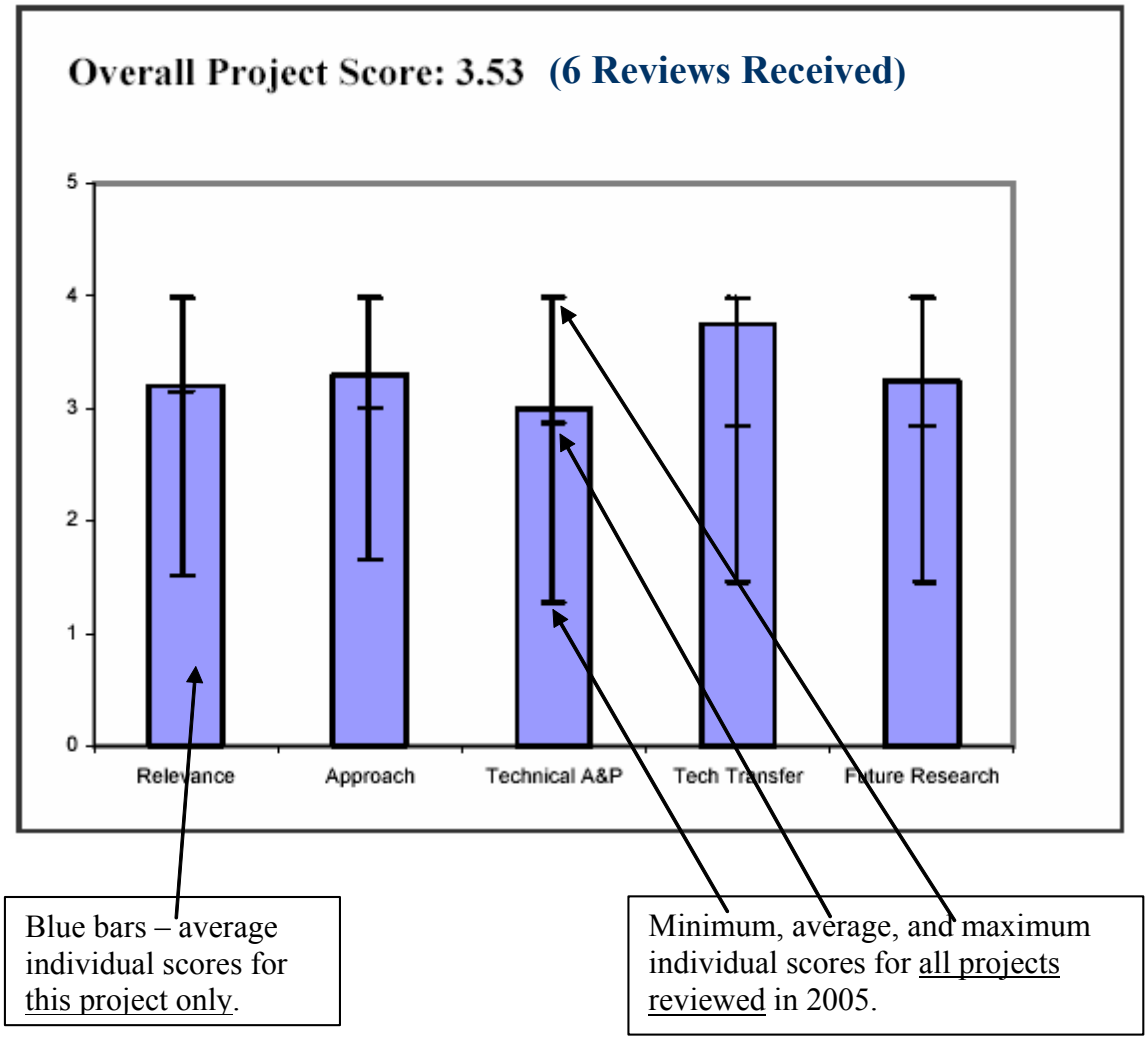
Appendix B lists the new projects for which Technical Accomplishment was not scored. It also provides a list of projects that were presented at the meeting, but were not reviewed.

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, are provided in the seven main 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 (subprograms) into which they fall in DOE Hydrogen Program planning. A brief summary written by the Team Lead for 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 project scores and how the particular project compared with all other projects presented. An example of a graph is provided below:



Using the formula mentioned above, the weighted final score is displayed as the "Overall Project Score" at the top of the chart. The text in parentheses, next to the "Overall Project Score," indicates the number of panel members who reviewed that particular project.

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 across all the reviewed projects (i.e., across all subprograms 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, suppose 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

As an example then, the "Relevance" bar/line on 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 with max, min and average values of 4, 1, and 2.3 respectively for the Relevance criteria of all three projects.

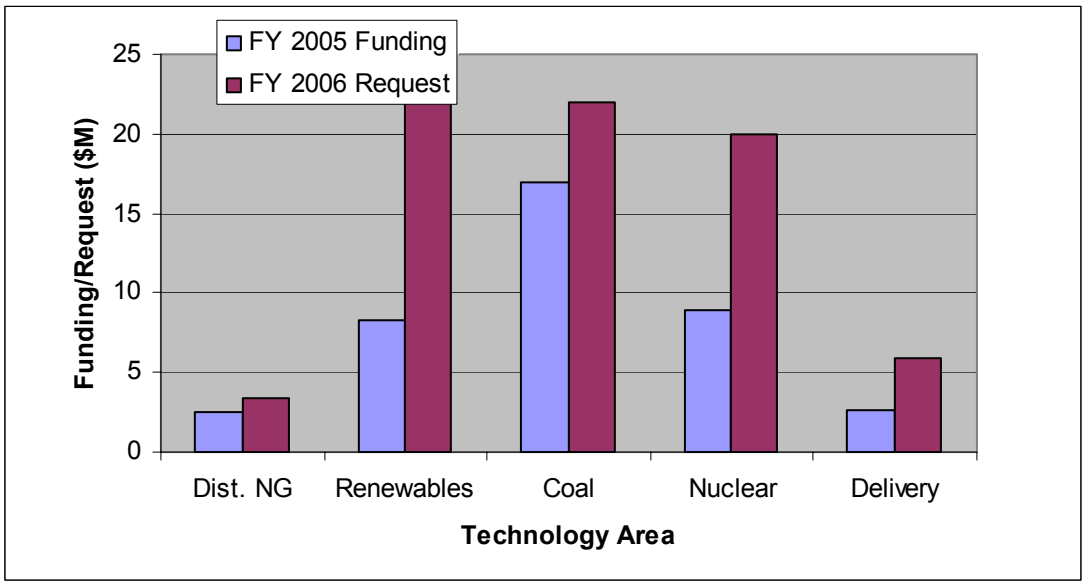
Hydrogen Production and Delivery
Summary of Annual Merit Review Hydrogen Production and Delivery Subprogram

Summary of Reviewer Comments on Hydrogen Production and Delivery Subprogram:

For the first time, this review session combined hydrogen production and delivery research from all DOE programs 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. Reviewers commented that production and delivery technology plans are comprehensive and clearly stated in terms of the Hydrogen Program mission and the President's Hydrogen Fuel Initiative. The production and delivery projects are considered to be appropriately diverse in terms of energy sources and related process technologies. Reviewers commented that the program's production and delivery plans described include concise goals and technical barriers, plans to overcome barriers, and timely milestones to overcome the barriers and meet targets. Another general review comment was that the recently developed hydrogen production analysis tool, i.e. the H2A Model, is making a major contribution by allowing research projects and technologies to be evaluated using common assumptions and parameters.

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 3.71, 3.04 and 1.92 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 Reforming: While the diverse project teams were viewed favorably, cost data presented was determined to be lacking. Also, some reviewers requested that partners broaden cooperative efforts beyond current customer base in light of public monies received for the research. The projects reviewed are thought to be well-aligned with the Program goals.

Electrolysis: Projects in electrolysis development were generally scored favorably. Most of the projects were viewed to be in-line with current program goals and objectives. The highest scoring projects included electrolysis development integrated with renewable energy sources and also fundamental electrolysis system development. One project will be discontinued based on reviewer scoring. Several reviews made comments regarding the standardization of analysis and cost modeling used in these projects. DOE is currently proceeding on these goals using both H2A modeling and feedback from industry on assumptions used in these models.

Biomass: This technology area has been re-focused since the 2004 Merit Review. The two projects in this area are new and were reviewed favorably. Both projects are high risk research aimed at potential cost breakthroughs using different approaches. The high risk nature of these efforts was noted by the reviewers. The reviewers' recommendations to focus on the key high risk element of each project with clear go/no-go decisions will be followed. Also, some projects are focused specifically on distributed reforming of bio-derived renewable liquids and are recognized as an important pathway for hydrogen production.

Solar-Driven High Temperature Thermochemical: There was only one project reviewed in this area, and it was reviewed favorably. The reviewers expressed concern that too much effort had been spent on screening potential thermochemical cycles. The screening has now been completed and laboratory research on the most attractive cycles has been initiated. Two new projects in this area will be getting under way shortly and will be reviewed in 2006.

Photoelectrochemical Hydrogen Production: The projects in this area received mostly high ratings from the reviewers. The projects were seen as generally well thought-out and logically organized with good scientific results and collaborations. This technology area should appropriately continue to focus on materials research and development.

Biological Hydrogen Production: The projects in this area received generally high ratings from the reviewers. There have been several significant advancements in the past year that were recognized by the reviewers. The projects that started this year, even with low levels of funding, have made progress. There are collaborations set up that are working well in this technology area.

General Separations: There was only one project reviewed in this area. Reviewers emphasized that the research is fully relevant to RD&D Plan objectives and that lowering the cost of hydrogen separation technologies is critical. Bridging the gap between modeling and real world experimental results with potential commercialization partners is key. Three new projects will be getting underway shortly and will be reviewed in 2006.

Hydrogen from Coal: The reviewers agreed that process intensification, including the combination of water-gas-shift reaction and hydrogen separation in a single membrane reactor would offer the best promise to reduce hydrogen production from coal in the downstream coal gasification step. Good progress was noted in the membrane development area. Recommendations included the use of a system engineering model to incorporate the proposed work with front end coal gasification to provide R&D guidelines. Also recommended was the need to obtain more long-term coal-derived gas separation technology performance data.

Hydrogen Production Using Nuclear Energy: In general, the projects reviews in this area, which included the development of high-temperature electrolysis, thermochemical cycles, and supporting systems, were favorable. Comments were positive on the experimental nature of the research and the progress to date. Most of the project reviews noted an interest in clearer communication of future research plans and milestones. Many also identified research areas (e.g., economic analyses) which are addressed by different parts of the nuclear program. The program will make adjustments in the future to clearly articulate links between projects, the planning documents, and other program activities.

Hydrogen Delivery: This area received generally high scores from the reviewers. The projects were viewed as well-aligned with Program goals with generally good approaches to the research and sound collaborations. Nearly all of these projects are just getting underway. There were concerns raised as to possible duplication of efforts and the need for coordination across the several hydrogen pipeline projects. DOE has just formed a hydrogen pipeline working group consisting of all of these pipeline projects that will meet periodically, will help to coordinate efforts, and will strive to create synergy across these projects.

Project # PD-01: Hydrogen Production & Delivery
Devlin, Pete; U.S. Department of Energy, EERE
Schmetz, Ed; U.S. Department of Energy, FE
Henderson, David; U.S. Department of Energy, NE
Mallock, Tom; U.S. Department of Energy, SC

Brief Summary of Sub-Program

The purpose of this Hydrogen Production and Delivery Sub-program Overview and introduction is to describe sub-program goals/objectives, budgets, barriers/targets, approach to R&D, technical accomplishments, interactions and collaborations, solicitations and awards, and future directions. As such, it sets the stage and puts into context the R&D and analysis projects which will be presented in this sub-program area during the Annual Merit Review.

Degree to which the Sub-program area was adequately covered and/or summarized

- Very broad range of objectives included in this sub-program. As these span basic biology to materials engineering, it is difficult to get a very good summary in one presentation. Still, major goals and achivements well articulated.
- Excellent summary of program.
- Coverage of the sub-program's technologies was comprehensive.
- Overview would be improved with more discussion of where funds are being spent and why.
- Adequately covered, but the presentation of information could be enhanced by using more comparitive diagrams rather than endless lists.
- Sub-program was well covered and summarized showing clear and concise goals, barriers, approach and plans to overcome barriers, milestones and timelines to overcome the barriers and meet targets.
- A good job in covering the many facets of this sub-program.
- Very articulate summary.
- Good overview of money and trends.
- Clear summary of technical scope.
- Unclear definition of going forward plans -- appears to be studying too many areas with few dollars.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- Several "target" numbers and strategies for achieving them were outlined.
- Issues with respect to target volumes were inadequate. Expectations should be set for volumes at various dates from major project areas (hydrogen from fossil, electrolysis by power source, etc.). Cost alone is not particularly useful, and projects should be aware of the overall project goal and address their parts.
- New cost basis of gge's on an FC/ICE efficiency ratio basis needs to also explore a range of fuel cell efficiencies.
- Identified, but not sufficiently discussed.
- Comprehensive presentation in terms of content but correlations between goals/time/readiness and challenges were not clear.
- Yes, important barriers, including cost targets, were identified and plans and timelines to address them were outlined.
- Issues/problems are being addressed in a well planned out manner. The program has decision points in place and has the flexibility built in to eliminate or go around obstacles along the way.
- Clear decision points laid out for near- to mid-term.
- Roadmaps and working groups discussed.
- Funding not clearly discussed.
- DOE Gen IV strategy not specified.

Does the Sub-program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Yes, it does appear to be focused. The H2A project is making a major contribution to putting projects on an equal footing.
- Updated plans, cost goals, and target guidance are indicators of active, fully engaged, effective management of the sub-program.
- Sub-program area is well focused.
- Course corrections have been made to address the economic needs of the real world deployment for hydrogen fuel cell vehicles in terms of technology required to satisfy them.
- Program appears to be running well.
- Clear hydrogen tecnology path articulated.
- Unclear nuclear roadmap -- key to selected hydrogen technology.
- Program clarity required to tie nuclear to hydrogen programs!

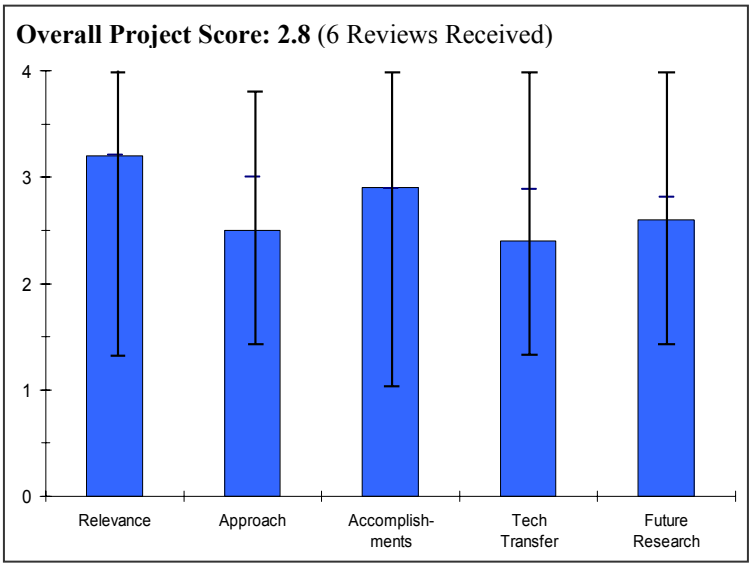
Other comments:

- Gge's are great for transportation. Another unit should be considered for power application, perhaps kW efficiency.
- Coal program should consider picking off easy hydrogen before passing on the remaining carbon and hydrocarbons to the power plant.
- Reviewers task for projects that have not yet started is unclear. These were already subject to solicitation assessments. What is the reviewer supposed to be adding?
- The large number of presentations in follow-on Production/Delivery sessions made discussion hard or impossible. Reduction or grouping (in panels?) might help.
- Having advanced copies of presentation was extremely helpful. Great progress here.
- Too little time allowed to cover the material for a sub-program with so many facets.
- EERE Team Lead briefed the group on the use of genetically modified algae at NREL to achieve higher efficiency without addressing the risks and implications of genetic engineering — this is an area of concern regarding NREL's competence and safety in this area.
- Funding allocations for 2004 and 2005 were shown in pre-charts. Would be nice if target funding ratios (or percentages) were shown on the same charts to show whether the funding targets are being met.
- Logic and structure of EERE Team Lead's efforts should be carried over to NE, which appears to be stuck on a GenIV reactor/ hydrogen roadmap decision.
- Unless quick action is taken, DOE dollars will be wasted if GenIV strategy is not established, communicated and then implemented.
- Most disorganized effort in entire session.
- Complete unwillingness or inability to time-manage the session.

Project # PD-02: A Reversible Planar Solid Oxide Fuel-Fed Electrolysis Cell and Solid Oxide Fuel Cell for Hydrogen and Electricity Production Operating on Natural Gas/Biogas
Tao, Greg; Materials and Systems Research, Inc. (MSRI)

Brief Summary of Project

This industry-academic joint project will develop 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 will focus on material research, stack design and fabrication, and experimental verification.



Question 1: Relevance to overall DOE objectives

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

- Good mix – a SOFC- Electrolyzer combination.
- This project is intended to address the need for production of low cost hydrogen.
- This project is well aligned with the DOE R&D plan
- Tightly focused on hydrogen program goal of improving efficiency and flexibility in hydrogen production from natural gas and renewable resources.
- This project provides a relatively near-term transition option — an interesting alternative to the standard water electrolysis.

Question 2: Approach to performing the research and development

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

- Appears feasible for composite Fuel Cell/Electrolyzer. No discussion of barriers and how they will get through them.
- The approach proposes the integration in a single system of Solid Oxide (SO) technology to generate electricity from fossil and renewable fuel to produce hydrogen through electrolysis.
- Integration of the combined functions, power generation and hydrogen production, in a single SO stack has, in similar projects, resulted in poor performance of both functions.
- Good technical approach, but need to develop targets for system/component cost and performance and then report progress against those.
- It is not clear that replacing electricity from the grid with electricity from an SOFC will lead to a more efficient process. An analysis to support that claim was not presented.
- It is not clear that reforming some natural gas to produce hydrogen to make electricity to electrolyze water to produce hydrogen is more efficient than the proposed method.
- Approach of using fuel-driven electrolysis has great potential to reduce voltage needed to electrolyze water to hydrogen.
- As an adjunct to this program it would be very helpful to see and overall system analysis and optimization study to see how the use of fuel to drive electrolysis compares with the use of the same fuel to make hydrogen via steam reforming. In other words, if hydrogen is the desired product, would it make more sense to make it from gas via reforming, instead of the scheme proposed here?

- The approach, as presented would use no precious metals and use no external electricity; however it was unclear specifically how the technology would be applied.

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

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

- 25% complete. Good scale up from button cell to 4 cell stack to 5 cell stack. Talked about future but did not show future slides.
- Good progress in button and short stack testing but performance is rather poor.
- Substantial effort will be required to improve performance to an attractive level.
- Improvement in fuel use efficiency must be realized for approach to be commercially viable.
- Good progress on SOFC materials in less than a year.
- Substantial progress made in relatively short period of time: button cells prepared and tested, temperature effects on I/V curves measured; cells tested in SOFC and SOFEC mode; proof of concept demonstrated for reducing voltage needed for electrolysis by putting fuel on cathode side of fuel cell; and a number of cathode materials developed and tested.

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

This project was rated **2.4** for technology transfer and collaboration.

- Did not discuss.
- Good team of university and industrial partners.
- Two companies and a university are involved. Project is relatively new and outreach should be encouraged.
- Don't know. Cannot recall the presenter delineating the roles and contributions of collaborators at the University of Missouri and Aker Industries.
- Both industry and University collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Did not show timeline to accomplish future work.
- Future plans should be focused on improving integrated performance of fuel cell and electrolysis cell in the combined configuration. Performance should exceed that of dual (separated SOFC and SOFEC) units.
- Good emphasis on durability testing and performance optimization.
- Could perform some cost analysis sooner rather than later to help set performance and cost targets.
- The system configuration and operating plan/duty cycle is not well defined. How much electricity will be available for export? Or will most of it be consumed internally because of high ASR?
- This program is clearly now in data generation mode, collecting data on new materials performance under a complex set of experimental variables. Plans are solid and well thought through. As noted above, plans should include an analysis of the trade-offs in using the fuel component for direct generation of hydrogen, instead of reducing voltage required for electrolysis.

Strengths and weaknesses

Strengths
- Good idea but needs work and more information/detail on slide presentation.
- Good team making strong progress toward achieving the project objectives.

Weaknesses

- No time scale on graphs.
- The project would benefit from defining a target-driven path for achieving cost competitive hydrogen/electricity production and then benchmark against those targets.
- The process is not well defined. The amount of hydrogen production relative to electricity generation and the thermal efficiency of the overall process are apparently not known or were not presented. The approach to lowering the ASR of the cathode material was not discussed other than to say that it would be improved. This is very important to system efficiency.

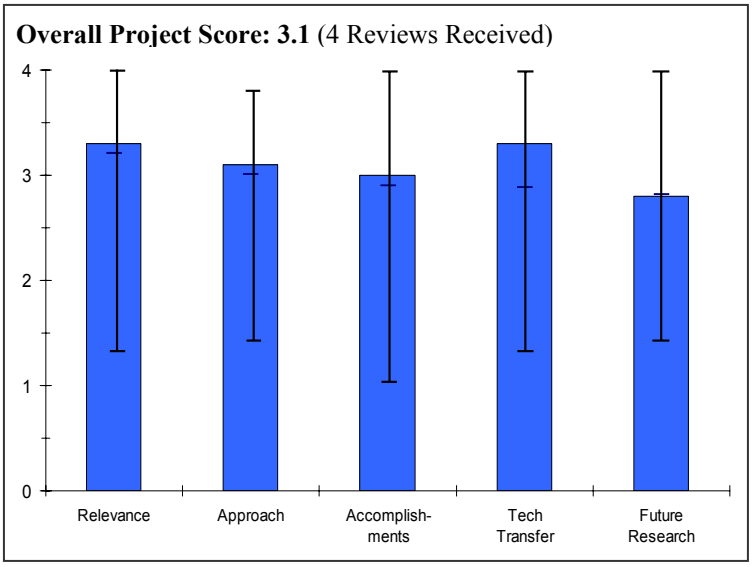
Specific recommendations and additions or deletions to the work scope

- Presentation needs work. Not enough detail to this project to justify. Need to see results against time.
- Focus on performance improvements.
- A process analysis and well-to-electricity/hydrogen analysis is definitely needed. The thermal efficiency of the process needs to be determined. DOE's system integration experts may be able to help in this regard. This analysis should be a high priority for the next year.
- Possibly explore deployment scenarios (in addition to balance of plant cost analysis).

Project # PD-03: Autothermal Cyclic Reforming Based Hydrogen Generating & Dispensing System
Kumar, Ravi; General Electric Global Research (GEGR)

Brief Summary of Project

GEGR designed fabricated and is demonstrating a hydrogen generating system designed for vehicle refueling. The system, based on novel autothermal cyclic reforming technology, can produce 40 kg/day of hydrogen, sufficient for the refueling of at least 1 bus or 8 cars daily. GEGR partner Praxair completed tests using simulated reformate at greater than 120 psig demonstrating that 99.99% pure hydrogen stream can be produced with greater than 75% recovery.



Question 1: Relevance to overall DOE objectives

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

- States they can come close to DOE target.
- The project addresses distributed production with a novel natural gas reformer process.
- Directly relevant to the DOE program objectives
- Project approach addresses a simplified lower potential cost of hydrogen production.

Question 2: Approach to performing the research and development

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

- Good slide, sufficient detail and information. Good explanation of ATR process. Good slides on targets and on safety.
- PI should describe the innovation involved with the PSA and compression and dispensing and which barriers they address.
- Need to comment on how barriers B, C and E are being addressed.
- Should provide more information on how the capital costs are being reduced.
- The approach is to develop an autothermal methane reformer that does not require the use of precious metal catalysts. Nickel catalysts are reduced to the metal state by recycling the PSA tailgas through the reformer, an interesting approach.
- GE should analyze where the thermal efficiency losses occur in their process and compare the analysis with a similar analysis for conventional SMR.
- GE should determine the impact on the process (cost and efficiency) of meeting the proposed higher quality hydrogen requirements that are in-line with the consensus quality requirements from the FreedomCAR Fuel Cell Tech Team and the North American consensus requirements that are being advanced to the ISO.
- Brought together an integrated team.
- Used the expertise of the partners well.

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

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

- Good task schedule slide. Catalyst change out 4 times per year seems like a lot.
- Need to describe the safety approach addressing the reformer cycling between fuel rich (reforming) and air rich (regeneration) steps without creating an unsafe mixture of fuel and air within the reformer.
- GE has made significant progress over the past year in bringing this project close to the demonstration phase. They appear to be on track with regard to permitting the facility and beginning operation this year.
- The project has made great progress on demonstrating the concept works.
- No data is presented on cost relative to the $3/gge. Clear that not all the information is available, but indications could be shared.

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

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

- Good slide from Praxair.
- Collaboration with Praxair on PSA and with UC-Irvine on-site leverages the partnership capabilities.
- Good collaboration between GE, Praxair, and UC.
- Well designed team.

Question 5: Approach to and relevance of proposed future research

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

- Future work seems to be on target. This project is off to a great start, and should contribute well to DOE program goals.
- Appears that the PSA and compression/dispensing add little new knowledge in light of other DOE funded projects.
- No mention of considered contingencies, optional paths, or off ramps.
- Project will end this calendar year. Operating data should determine whether GE met the DOE performance targets and cost target.
- Durability is modeled at this point, but the next phase needs to get real data.

Strengths and weaknesses

Strengths
- Good slide responses to previous year's comments.
- High pressure reformer saves parasitic load, i.e. no reformate compressor.
- Designed for a high number of starts and stops.
- Safety approach is inclusive of ASME, NFPA, and local fire marshal.
- Excellent project team. The project looks like it will result in a functional system and provide data which can be compared with the DOE cost and performance targets.
- Appears to be a path that drives out cost through simplification.
- Great to see the prototype using real world codes.

Weaknesses
- Catalyst life needs to be extended to reduce operation and maintenance cost.
- Efficiency appears to be low due to: 1) Methane conversion is low (~70% as inferred from slide 7); 2) PSA recovery of ~75%; and 3) Parasitic power required for air blower/compressor.
- There is little evidence of development in PSA and compression/dispenser.
- Catalyst lifetime is relatively short which could have an impact on the overall economics.

- Primary goal is to address the DOE cost goal for hydrogen production, but no data is presented. That seems to be a significant omission. How can we tell if we are on the right path? i.e. the program needs down-select measures if possible.

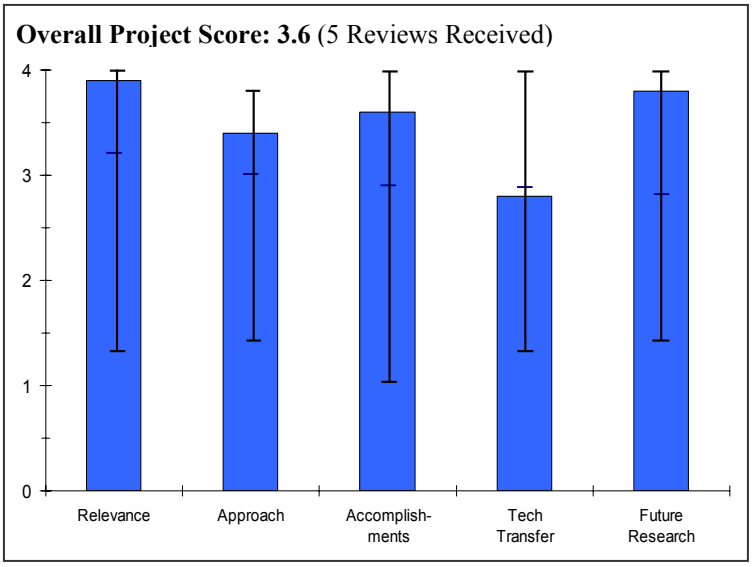
Specific recommendations and additions or deletions to the work scope

- Address how to overcome barriers.
- Develop and report the system efficiency and compare to the DOE target. Document progress against all DOE targets as the project proceeds.
- Test high pressure reformer and optimize methane conversion.
- Hydrogen quality is a moving target. GE should determine the impact of changes (both greater and less) in quality hydrogen requirements on the process and system design.
- Need to consider more robust catalyst system. Three month life is not a commercial life cycle. Not addressed is the cost; if the catalyst is very low cost, this might be ok. Can't evaluate further since that information is not presented.

Project # PD-04: Development of a Turnkey Hydrogen Fueling Station
Guro, David; Air Products and Chemicals, Inc.

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 Hydrogen Fueling Energy Station program, the 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.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.9** for its relevance to DOE objectives.

- Good budget slide. Good level of detail on slides.
- Hydrogen distribution infrastructure is one of the key points to bridge the gap to a hydrogen economy. This project supports the President's Hydrogen Fuel Initiative to accelerate the introduction of hydrogen as the primary energy carrier in the USA. It is also in agreement with the recent DOE on-board fuel processing no go decision.
- Clear relevance to near-term objectives.
- Working on the right issues.
- Important to validate current distributed reforming delivered cost of $3.00/gge and an overall efficiency of 65%.

Question 2: Approach to performing the research and development

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

- Good target slide. Good breakout/discussion on components.
- The project is very well designed.
- All the technical barriers of the hydrogen refueling system were addressed.
- Very structured, methodical approach to addressing technical issues.
- Not clear how the change between Las Vegas and Penn State is happening; they are projections, it appears. It might help to illustrate this in future presentations.
- Decision to use SMR over POX, AATR, and CPOX is a critical one and more information should be given about that decision.

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

This project was rated **3.6** based on accomplishments.

- Good slide on timeline. Good "forward looking" concepts. Good interfacing with NIST to write regulations.
- Hydrogen production and PSA system efficiencies as well as hydrogen cost meet the DOE 2005 targets.
- System approach very effective.
- Would rate a 4 if the cost data was presented along with quantifying what amount was driven by which technology enhancement (simply showed a list).
- Packaging (conceptual) very compact.
- The results of the project need to be more data driven. No real data given in the presentation. It is important to prove that the system works well and quantify reliability by showing line charts, statistics, and assumptions used for determining economics and efficiency numbers. Perhaps this is not the venue for so much detail disclosure, but it should be reported in other publications.
- The PSA results and efficiencies and cost reduction numbers are significant for distributed reforming.

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

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

- Good discussion regarding Penn State and their activities.
- Collaboration with the university or other laboratories is not clear. The project was basically developed by the industry.
- Nice interface with the University community.
- Learned from Las Vegas project regarding vehicles.
- Would rate 4 if the role of the University was more clear; is it beyond just being the site host?
- To have AP develop this system and commercialize it is a significant advancement for the Hydrogen program. Testing of fuel cell vehicles would show that in fact the purity is as stated.

Question 5: Approach to and relevance of proposed future research

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

- Good characterization of future work.
- The future work is focused on the integration of hydrogen generation step with purification/compression/ dispenser.
- Good plan.
- Great to see balance of plant items being aggressively addressed.
- Reformer based system needs to begin as soon as possible. LH_2 demo is of minimal value. CNG blends in vehicles are less desirable then FCV use since latter would demonstrate purity is being met.

Strengths and weaknesses

Strengths

- Good enough to lead to another proposal/ project on compression and CNG blend. Good description of future work.
- The project is very well designed, and it addressed all the key technical barriers of the hydrogen refueling station.
- Progress in packaging.
- APCI is a critical participant in this phase of the program and station is important for the distributed reforming work. Cost and efficiency numbers for the PSA and the overall process are significant findings.

Weaknesses

- Don't use white type on light background and black type on a blue background. Too hard to read.
- The time scheduled to integrate the hydrogen production sub-system and to operate and test the complete system is too short.
- Cost transparency.
- Results should be quantitative and data used to show how goals are being met. There is no way to validate that the quoted cost of $2.72/gge is accurate.

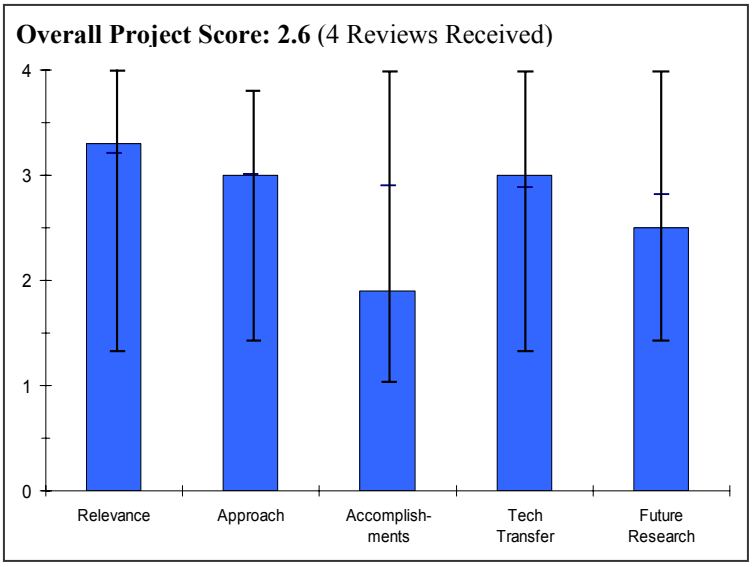
Specific recommendations and additions or deletions to the work scope

- There will not be available time to collect enough data of reforming sub-system coupled to overall system.
- System optimization is claimed in the presentation, but no details provided of process used to optimize and the specific results. Understandably, there is some rationale for a company to develop and protect future market advantages, but this will also limit dissemination of learning from this cost-shared project. If another entity wants to compare SMR over other process options and then optimize that system, then they should have access to the precompetitive fraction of the results from this project

Project # PD-05: Development of a Natural Gas-to-Hydrogen Fueling System
Liss, Bill; Gas Technology Institute

Brief Summary of Project

GTI is designing a competitive, fast-fill natural gas-to-hydrogen fueling system with 40-60 kg/day delivery capacity that strives to meet the DOE hydrogen cost goal of $2.50/kg or less. GTI will undertake system design and analysis to identify potential pathways, conduct development and lab testing to confirm subsystem operation, integrate the system and incorporate controls, and conduct lab and field testing to validate performance and reliability.



Question 1: Relevance to overall DOE objectives

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

- Difficult to determine which of the many accomplishments presented are relevant to the original scope of work.
- Project addresses most of the issues regarding on-site reforming. Some issues not addressed are hydrogen quality and reliability.

Question 2: Approach to performing the research and development

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

- Appears good approach.
- The project addressed all the technical barriers of the hydrogen refueling station based on reforming of natural gas.
- Why were Proton electrolyzers evaluated?
- How is it relevant to the natural gas hydrogen target?
- Extremely broad array of technologies tested. As a consequence, the project is too diffuse.
- Unclear what GTI did to optimize unit performance
- Highly diverse team.

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

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

- Good description of various generations, too much detail on accomplishments.
- Hydrogen production cost and overall efficiency still need to be improved.
- The performance of the purification process has to be improved.
- Accomplishments are unclear.
- Unclear if any of the units' performance was improved versus what is available from these unit producers.
- Unclear how the project contributed to cost reduction.
- No data presented on cost progress which is the primary goal.

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

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

- Good mention of partners.
- The project evaluates technologies for purification, compression, storage and dispensing of different companies.
- The plan for technology transfer is not described.
- Project appeared to rely on other companies as simple suppliers.
- Looks like the project is set up for success.

Question 5: Approach to and relevance of proposed future research

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

- Appears good, but needs to be focused.
- The future work consists of the construction of the complete system of hydrogen generation, and testing the integrated system.
- Focus has to be sharpened.
- Future work seems to be completing the laundry list of tests without conclusive improvements expected.
- Not clear that meeting the cost objective is driving the work. May be a presentation issue, but comes across as a list instead of cause and effect relative to the major program objectives.

Strengths and weaknesses

Strengths
- The approach of project address all the steps involved on the hydrogen delivery. The project also evaluated different technologies of several suppliers.
- Very diverse partners.

Weaknesses
- Are any of the accomplishments "milestones"?
- The integration and demonstration of the complete hydrogen refueling station still are not defined.
- Project budget and timeline not presented.
- Accomplishments in many fields (production, purification) not made clear. Statements about feasibility not backed up by any specifics.
- Not clear if any of the reformers actually produced on-spec hydrogen.
- Project seems to be very diffuse lacking clarity and focus.
- Unclear contribution of this project beyond the simple measurement of performance of existing technology
- Unclear deliverables.
- Short on data concerning costs.

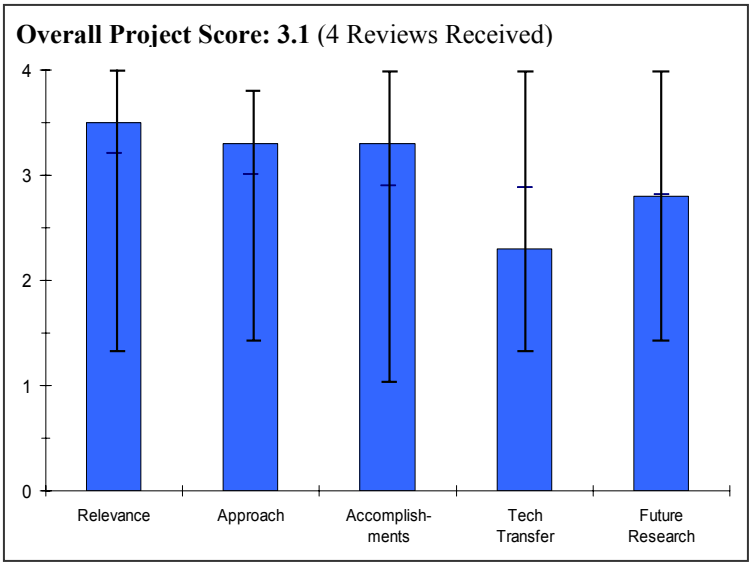
Specific recommendations and additions or deletions to the work scope

- There were a lot of elements to this presentation -- modeling, storage, economics, etc. Needs better organization of the actual accomplishments under this program.
- Eliminate electrolyzer from scope.
- Add measurement of hydrogen quality.

Project # PD-06: Production of Hydrogen by Biomass Reforming
King, David; Pacific Northwest National Laboratory

Brief Summary of Project

Pacific Northwest National Laboratory (PNNL) will develop cost-effective reforming technology for the distributed conversion of biomass feedstocks to hydrogen using the following potential feedstocks: ethanol, glycerol; sugars, sugar alcohols (xylitol, sorbitol, glucose); and less refined starting materials such as cellulose or hemicellulose. In addition, PNNL will provide technical and economic comparisons with alternate biomass conversion approaches.



Question 1: Relevance to overall DOE objectives

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

- Directly supports 2015 objective of reducing cost of distributed H_2 production from biomass-derived renewable liquids to $2.50/gge (delivered, untaxed) at the pump.
- Addresses distributed reforming of renewable liquids. Addresses cost issues, efficiencies, feedstock variety. As long as this project stays focused on reforming to hydrogen from "realistic" feedstocks, it is relevant.
- The project is fully relevant to the long-term goal of producing hydrogen from renewable feedstocks.
- A novel pathway!

Question 2: Approach to performing the research and development

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

- Project addresses feedstock impurities issue by exploring glucose and hemicellulose in the upcoming fiscal year.
- Safety issues with respect to lab-scale experiments are addressed.
- No information presented on reactor or process capital costs.
- The R&D approach is adequate.
- Use of combinatorial screening of catalysts was appropriate. Remaining approach addresses barriers. Would have liked to hear mote about the role of microchannel reactors in this project; are they a focus?
- There appears to be significant overlap with the Virent Aqueous Phase Reforming project (which was initiated in late 2005), but the collaboration activities are not specifically discussed.
- Well designed, but many years ahead before the pathway is commercial. Not clear how this might fit in from time scale.

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

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

- Project has made great progress towards the completion of each task to date.
- Catalyst performance is measured against the specificity/productivity/lifetime targets established by project.
- Sorbitol APR: what is the difference between the productivities on slide 13 (91.3 L/Lcat/hr) and slide 14 (~57 L/Lcat/hr)--what are the process conditions for these runs of different C3 feedstocks?

- No comparison of results for different feedstocks.
- Project is on track.
- At basic science level 4.0. At a cost trend for production cost, not much data yet.

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

This project was rated **2.3** for technology transfer and collaboration.

- Collaboration with Virent project and ethanol producers was mentioned.
- Details of the collaborations are lacking.
- Are there synergies with traditional reforming or lessons to learn from other reforming projects?
- Coordination with Virent was not defined -- there should be ongoing information exchange and coordination between the two projects to ensure efforts are leveraged and are not duplicated.
- No collaboration has been established with an ethanol producer for the ethanol steam reforming work.
- Expand further.

Question 5: Approach to and relevance of proposed future research

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

- Future work is investigating feasibility of using less pure renewable feedstocks--this is key to economic viability of technology since feedstock costs account for a significant fraction of the production cost.
- Ethanol reforming work focuses on meeting thermal efficiency and production cost targets.
- Are catalyst lifetime studies of 100 hours sufficient? (1,000-10,000 hours is often used by catalyst development companies).
- Economic analysis will be most important. Go/no-go decision points are appropriate and are scheduled for next year.
- Improved collaboration efforts are needed to focus future work.
- Better articulate the work planned and the results expected, instead of just a task list.

Strengths and weaknesses

Strengths
- The technologies being investigated have the potential to achieve DOE goal of economic, renewable-based hydrogen.
- Very novel pathway.

Weaknesses
- No indication of what the catalyst productivity/specificity and other technology targets correspond to relative to the cost of hydrogen.
- Need more feedstock comparisons.
- Provide more cause and effect data.

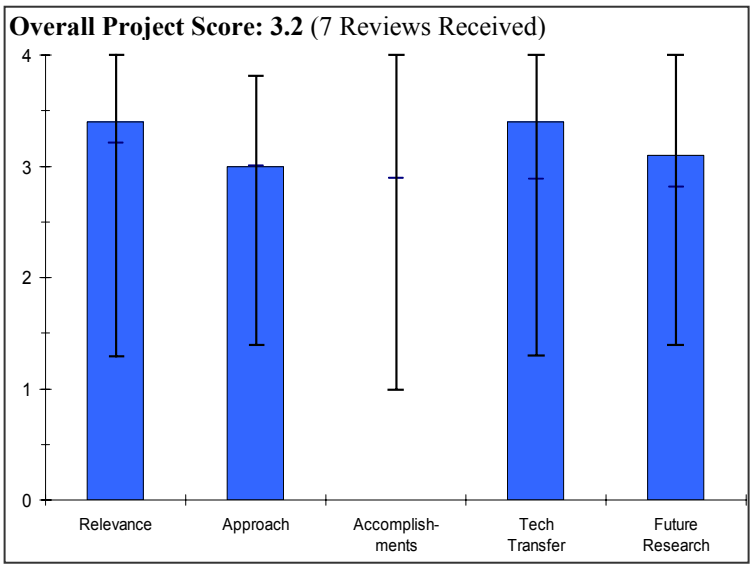
Specific recommendations and additions or deletions to the work scope

- Expand outreach to industry beyond what is currently in place.

Project # PD-07: Hydrogen Generation from Biomass-Derived Carbohydrates via the Aqueous-Phase Reforming (APR) Process
Cortright, Randy; Virent Energy Sys.

Brief Summary of Project

This project will combine the expertise of Virent Energy Systems (Virent), Universal Oil Products LLC (UOP), Archer Daniels Midland Company (ADM), and the University of Wisconsin to demonstrate the feasibility of generating high yields of hydrogen from corn. This proposed concept takes advantage of the fact that corn contains large amounts of starch which can be extracted and converted to glucose and sorbitol. Sugar based feedstock can also be produced from other biomass sources. The resulting aqueous solutions can be fed to the 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 a pressure swing adsorption.



Question 1: Relevance to overall DOE objectives

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

- Project directly supports the objective of generating H_2 from renewable bioliquids by 2015 at $2.50/gge.
- Technology is innovative and offers a pathway for renewable resource-based hydrogen.
- Clearly addresses technical objective: "By 2015, reduce the cost of hydrogen produced from biomass...by developing reforming technologies..."
- Also well-focused on goal of attaining "distributed hydrogen production technologies": an intermediate step toward long-term hydrogen production goals.
- Rather than focusing on natural gas or liquid fuels (the "low hanging fruit") attempts to access renewable carbon sources as hydrogen feedstock directly.
- Ultimately how much hydrogen could be expected from agriculture biomass in the U.S., without impacting food and industrial uses?
- Catalyst work is important to success of hydrogen from renewable liquids.
- Project is fully relevant to supporting the objective of developing low-cost methods for converting biomass to hydrogen.
- The project is aware of DOE hydrogen program objectives and the need to meet targets
- The project does support the hydrogen RD&D plan.

Question 2: Approach to performing the research and development

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

- The R&D approach makes sense.
- Project only looking at glucose and sorbitol as feedstocks--what about less clean feeds such as hydrolyzed lignocellulosics?
- The rationale behind APR of oxygenated hydrocarbons, as opposed to alkanes, is well articulated and reasonable.
- APR avoids high temperatures and pressures while, at the same time, using biologically derived feedstocks rather than petroleum products -- APR approach is therefore particularly mission-relevant.

- Little discussion of reactor design or scale-up possibilities.
- Project is a combination of integration and component R&D.
- Early focus on glucose and catalysts is appropriate. Go/no-go decisions points appear to be appropriate, but it is not clear if work will continue with a different feedstock if glucose is a no-go.
- Little detail presented on the actual project approach (too much detail presented on reaction pathways, etc.).
- Project timelines provide a reasonable sequence of events.
- Good to see that the thermal efficiency and economics of the baseline APR will be calculated in the first year, prior to the first go/no-go decision.
- The proposed plan seems reasonable.
- The PI should assess whether low system efficiency due to high water content is a concern.
- How likely is project to meet DOE cost targets?
- The approach appears to be sound for a new project.
- Increased emphasis should be placed on catalyst development, testing and evaluation early on in the project. This will impact the overall cost and efficiency of the project.

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

This project has not started and therefore was not scored on accomplishments.

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

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

- Project team appears qualified for performing the work proposed.
- Team includes a player in the biomass feedstock industry.
- Collaborating with PNNL project for reactor configuration work.
- Well integrated-university, national lab, and industry collaboration.
- Overlap with PNNL is apparent; the separate roles of PNNL and Virent (and University of Wisconsin) are not well defined.
- Facilities, staff, and project infrastructure is very impressive.
- Partners are appropriate. PNNL work in this area should be coordinated to eliminate duplication and improve chances of success.
- Unclear what the specific role of UOP will be.
- Good mention of similarity with PD-6 presentation.
- This program appears to have a good diverse team.

Question 5: Approach to and relevance of proposed future research

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

- Project recognizes the impact of feedstock cost on hydrogen production cost, but is centered on use of glucose.
- PIs may need to consider lignocellulosic feeds in order to meet DOE cost targets.
- A good plan is articulated.
- "Identify candidate sugar streams" is listed as a first year goal, but only glucose is discussed as a target for reactor design. Is there a plan for considering other sugars or sugar alcohols?
- Choice and optimization of catalyst is discussed. Reactor design and potential pitfalls therein are not discussed.
- Slides 3 and 4 not same as Future Work Gantt chart on pages 18 and 19. Off ramp step-up after catalyst development in Gantt is much better.
- Project has not yet started, but decision points are good and work plan seems reasonable.
- Project timelines provide a reasonable sequence of events.
- It is considered that the timeline is optimistic.
- The plans go beyond go/no-go decision point, which makes sense as this can help to indicate a future path on meeting the goals at the decision point and give confidence for the future achievement of results.
- This project appears to have a good overall plan.

Strengths and weaknesses

Strengths
- Technology addresses the production of renewable resource-based hydrogen.
- Technology is novel.
- An excellent first step toward practical reforming of renewable carbon sources to produce hydrogen.
- Involvement of ADM integrates agricultural production industry into an ongoing university/national lab/industry collaboration.
- Significant potential contribution to hydrogen production.
- Clear understanding of issues. Strong collaboration areas.
- The PI has a good background and experience in this field.
- The project appears to have a very competent and experienced team.
- The initial technical approach appears to be sound.

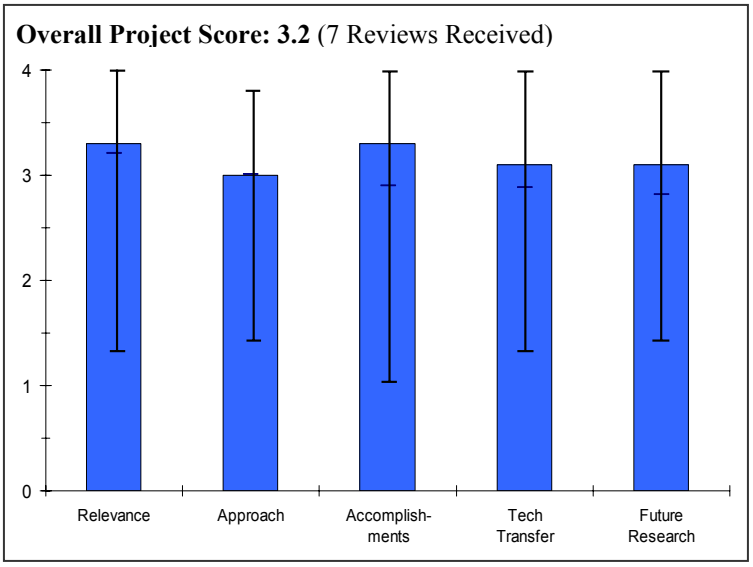
Weaknesses
- Quantifiable milestones and go/no-go decision points were not presented.
- Technology performance targets were not given.
- Project uses glucose as feedstock rather than lignocellulosic biomass.
- Issues involved in reactor design and fate of waste CO_2 are not addressed, though both are part of the upcoming 3-year project.
- Dependent on success of catalyst development.
- Possible conflict with other uses of agriculture product. Is this in addition to food and other uses? What is the potential for surplus agricultural and or new agriculture?
- Potential duplication with PNNL.
- Project should use H2A economic assumptions in the future for cost analyses.

Specific recommendations and additions or deletions to the work scope

- As acknowledged by the authors, using sorbitol/sugars rather than methane as feedstock will result in a proportionally much larger amount of waste CO_2 per molecule of H_2 produced. (This concern will apply to any plan for converting biomass to hydrogen). The recovery and fate of the waste CO_2 should be accounted for, in reactor design and total hydrogen production costs. However, the CO_2 released is recycled back onto the growth of new biomass so this pathway is a near-zero net GHG emitting pathway.
- Biomass program would benefit from a combined approach to a wider array of biomass.
- Competitive uses for agricultural feeds should be evaluated.
- Program should identify potential contribution to overall U.S. hydrogen production. Project should be asked to relate activity to this ultimate goal.
- Focus on catalysts is the first decision point -- knowledge from catalyst work may be beneficial even beyond this project.
- Combining this project with PD-6 should be considered as there is much similarity and potential overlap.
- Value only comes with scale up -- scalability should be established at an early stage.
- The project should show progression toward reforming of base corn waste, stover, etc., soon, not 'refined' sugar products.
- Emphasize catalyst development and testing early on in the project.

Project # PD-11: Advanced Hydrogen Transport Membranes for Vision 21 Fossil Fuel Plants
Sammells, Anthony; 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 during industrial processes, such as coal gasification. Currently, this project is focusing on four basic categories of dense membranes: mixed conducting ceramic/ceramic composites, mixed conducting ceramic/metal (cermet) composites, cermets with hydrogen permeable metals, and layered composites containing hydrogen permeable alloys. Eltron's layered composite hydrogen separation membranes have an order of magnitude higher hydrogen permeability than current palladium based membranes and are 200 times cheaper. In addition, these membranes operate at 320-440°C, which is consistent with water-gas-shift temperatures in gasified coal feed streams.

Question 1: Relevance to overall DOE objectives

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

- Would be good to specifically address this in the presentation.
- What advantages does this technology offer over the conventional technologies?
- Barriers listed in overview were not adequately addressed in presentation.
- This project supports the President's Hydrogen Fuel Initiative to accelerate the introduction of hydrogen as the primary energy carrier in the USA.
- The project shows close bearing to DOE's RD&D Plan. If successfully implemented, it is essential for the realization of the President's Hydrogen Fuel Initiative.
- Relates to a separation of hydrogen.
- Close attention paid to the membrane integration into a coal gasification plant to produce hydrogen.

Question 2: Approach to performing the research and development

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

- The project has a lack of focus on durability testing prior to scale up. The effects of impurities other than CO need evaluated for Fossil Energy application.
- Have any experiments on hydrogen purity been performed? Or conversion of CO? None were shown in the presentation. Permeation and flux are not the only parameters that need to be studied.
- 100% selectivity and recovery will not be achieved.
- Researchers should be congratulated for trying to incorporate systems thinking, but they are not taking into account actual conditions at SMR, coal gasification and biomass gasification plants. Into the WGS reactors, there is unconverted CH4 and impurities. Also, the off-gas from conventional PSA units is used to fuel the reforming reaction. Without this gas being available, the plant will have to burn additional fuel in the reformer. This will result in CO_2 emissions that cannot cheaply be sequestered. Slide 20 is misleading and incorrect.
- What is the conversion of CO in the WGS reactor?

- Modeling experiments should be conducted to determine the total conversion of CO in the water gas shift reactor. Even though the temperature is higher than conventional low-temperature WGS, the removal of product hydrogen will shift the equilibrium to the right. But the researchers must determine to what extent.
- Not using the retentate to fuel the reformer is a big mistake. You're losing the energy in the unconverted CO, CH4, and unrecovered hydrogen. It would be very unfortunate to sequester these energy-containing compounds with the CO_2. The researchers should investigate systems designs that would solve this problem.
- The project addresses the cost reduction of the hydrogen production from fossil fuels through the development of membranes with high hydrogen flux at high temperatures.
- The approach is sound and logical.
- Some consideration should be given towards addressing challenges in the commercial manufacture of the applicant's (Eltron) hydrogen membrane.
- Comparisons with other membrane types should be done on an equal basis (i.e., similar physical conditions).
- The membrane development work here would benefit from having a well-defined total system engineering model.
- A good systematic approach was proposed and implemented.

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

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

- A project that began in 2000 should have performed a full economic analysis by now. Recommend no further funding before establishing economic viability.
- Verbal statement about mass transfer limitations in the catalytic membrane reactor was very interesting. Good lesson learned. Should elaborate on this. Does this mean that you are planning on using this membrane only for separation? Have you considered installing static mixing impellers inside the membrane to increase the contact probability between the hydrogen and the membrane?
- Why did the membrane need to be regenerated when you were running at 80% hydrogen and 20% helium? This is obviously not due to adsorption of CO on the membrane surface.
- Adsorption of CO on membrane surface is an important finding. To combat this, will you flush with pure hydrogen? How often will depend on economics, but please realize that experimental data are required. How long were experiments run in Slide 13?
- Very good that experiments were conducted without sweep gas.
- Scale of Y-axis (same measured results) is significantly different between many of the slides, e.g., 10, 11, 12, 15. The different parameters that lead to the different results should be shown on the graph. Should perform parametric experiments to obtain results that can be compared across experiments and applied to further analysis.
- The performance of the purification process has to be improved since the hydrogen recovery is around 95%. Unfortunately, this can not be achieved without additional purification step which increases the cost of the process.
- The performance of the water gas shift catalyst and membrane in the presence of sulfur was not evaluated.
- The project appears to be progressing well on schedule and has identified barriers to meeting objectives together with strategies to address these barriers.
- The tolerance of the membranes to impurities was not sufficiently discussed.
- Outstanding membrane discovery and development work, as reflected by reported hydrogen performance data.
- Good process made in several key aspects including hydrogen flux improvements and membrane material improvement to lower the operating temperature.

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

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

- The companies that are listed certainly form a good network. Presentation should give details on how much funding goes to each partner, what the role of each partner is, and how the project team is coordinated.

- The project lists three industrial partners and one national lab. It is unclear what work was performed by each participant.
- Collaboration with engineering firms is cited but there seemed to be a reluctance to report on the real life performance application parameters.
- More integration with end-users of the engineering firms would be desired to seek R&D inputs.

Question 5: Approach to and relevance of proposed future research

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

- Future work lacks reliability/durability testing. Fossil applications are unpredictable if there is a need for thermal mechanical testing to predict long-term reliability.
- If the project is scheduled to end Sept 30, 2005, are the plans shown for FY06 proposed? Is there any funding for this?
- Not enough information on remainder of FY05 work was given. What is meant by "performance verification?" Verify what? Will hydrogen purity be verified, as stated in the barriers that are being addressed? How about conversion? Total hydrogen recovery? There are a lot of key pieces of information missing from this five-year old project.
- Need to determine why membrane loses functionality over time. Chemical changes to membrane or clogging?
- Need to study permeability as a function of distance along membrane. Economic analysis is not possible without these data. Good that you realize that there is a trade-off, but you're not even doing the experiments to begin to understand that trade-off.
- The future work plans to integrate the coal gasification system with WGSR and membrane. But, it does not address the increase of the efficiency of hydrogen recovery and catalyst and membrane poisoning by sulfur.
- Future plan is well focused on the construction and operation of bench (1.3.lb/day) and pilot (5 lb/day) units.
- The reported on very high quality membrane research/development work should continue.
- There will be a new five year plan to continue development work.

Strengths and weaknesses

Strengths
- Interesting materials.
- Good laboratory facilities.
- Good potential for new high flux membrane.
- The development of a membrane with high hydrogen flux under the reaction conditions.
- The project appears to have developed a hydrogen separation membrane that warrants further development and investigation. The characteristics of the membrane appear to supersede other types of membranes.
- Highly innovative, successful membrane development.
- Long term membrane performance data were presented. Membrane performance data at high pressure were presented. Progressive improvements in hydrogen flux of membranes were presented.

Weaknesses
- Team needs someone who can do analysis work. This research has gone on blindly too long. Cannot appropriately design experiments if you don't know what you're trying to achieve.
- Too many figures of merit have not been studied: 1) hydrogen purity, 2) CO conversion, 3) hydrogen flux as a function of feed throughput, without additional hydrogen added in, 4) total hydrogen recovery.
- The hydrogen recovery obtained by the membrane decreases the efficiency of the process, which inhibits a higher reduction of the cost of hydrogen production.
- The performance of the applicant's membrane should be more accurately quantified with "apples to apples" comparisons with other membranes.
- The PI states that the project is 85% complete, with plans to build and operate a bench and pilot scale test unit as part of its future work. It is unclear if the construction and operation of these two test units will be completed with the funds provided for the original project ($2.8 million).
- Project costs are high in comparison to other projects that have comparable accomplishments.

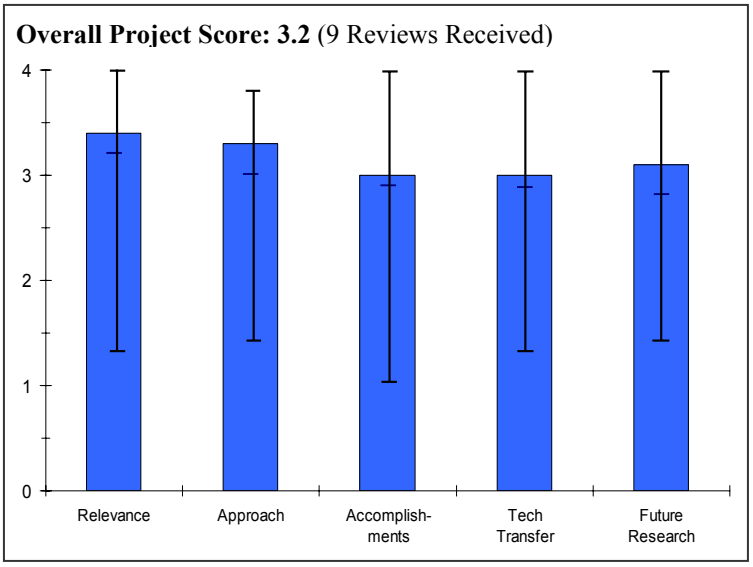
- An apparent lack of a total system engineering model to serve as a context within which guiding economics could be derived.
- The "guard bed" used in the experimental work and future deployment was not discussed adequately. No conclusions and milestone were presented. Module assembling and its associated engineering issues were not adequately discussed (i.e., capacity per stacked module).

Specific recommendations and additions or deletions to the work scope

- More emphasis on long term testing with thermal cycling.
- Need to present results on hydrogen purity.
- Need to perform total plant CO_2 balance. This should take into account the emissions from the reformer combustor. Slide 20 does not show the full emissions.
- Need to do some cost analysis. References to cost studies are presented out of context (i.e., not necessarily representative of the current work) and not based on Hydrogen Program standards.
- Need to present results with true syngas (includes not only CO but also methane and other impurities). A guard bed won't solve all impurity problems.
- Continue the project but now progressing to an engineering 'unit process' development mode. Needs total system design with economics.
- Give quantitative estimates on the membrane cost estimates. Expand work on the sulfur tolerance of membranes. Expand work on membrane performance under cycling conditions. More work on the CO effects on membrane performance. More fundamental study on the phases of palladium in membrane under testing conditions.

Project # PD-12: Scale-up of Microporous Inorganic Hydrogen-Separations Membranes
Bischoff, Brian; Oak Ridge National Laboratory

Brief Summary of Project



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 **3.4** for its relevance to DOE objectives.

- A cost-effective coal gas derived hydrogen production technology responds to Hydrogen Fuel Initiative RD&D needs and the proposed work is consistent with DOE program goals.
- The project technical strategy does not include any discussion of a development path leading to scale-up and commercial maturity or any success criterion such as how to achieve DOE hydrogen cost target.
- Considering the 50 yr experience base, a list of risks and barriers and planned accomplishments to overcome key technical obstacles is appropriate.
- It is not clear what differentiates the proposed technology, i.e., how it is techno-economically superior to, existing and proven commercial separation technologies, such as PSA.
- This project is funded by FE and relevant not only to coal, but also biomass gasification as well as for CNG distributed reforming.
- Would be good to specifically address this in presentation.
- What benefits does this technology offer over conventional separations technology?
- Barriers not given in slides.
- The project, if successful, supports the critical goal for improved, lower cost separations.
- This project supports the President's Hydrogen Fuel Initiative to accelerate the introduction of hydrogen as the primary energy carrier in the USA.
- The project clearly supports the President's Hydrogen Fuel Initiative and fits in well with the RD&D Plan objectives.
- Consistent with the object of generating hydrogen from fossil fuel gasification stream.
- Offers a promising alternative technology to carry out hydrogen membrane separation. Will not be able to achieve high purity hydrogen.

Question 2: Approach to performing the research and development

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

- Data indicates high operating temperature favors both flux and hydrogen selectivity, higher than ideal warm gas clean up temperature of approximately 300 deg C.
- The material exhibits the expected temperature dependence for its performance beyond the ideal warm gas clean up conditions; the PI needs to look at this issue (on how to achieve commercially relevant flux at warm gas clean up conditions).
- The delivery condition (T, P) of the PSDF coal gas slip stream is not discussed, nor the clean up procedures.
- The PI clarified during the review that the 0.4-5 nm pore size is a generic scheme; actual pore size will be tuned to allow selective diffusion of hydrogen (KE ~0.3 nm) while retaining CO_2 on the feed side.
- The project just began but is based on significant background capabilities.
- The project should conform to the HFCIT defined barriers and targets which are quantitative with milestones if there are no similar metrics in the FE program.
- Economic analysis should be done throughout project to provide research direction.
- Application of the existing technology developed at ORNL to this particular need is a very good way to start.
- Proposed larger (1 meter) scale is appropriate and encouraged.
- Very good that the project will screening materials using compatibility tests to determine effects of different gasifier environments.
- PI seemed a bit unconcerned about leaks.
- The project addresses the cost reduction of the hydrogen production from coal through tests in pilot plant scale of hydrogen separation membranes.
- The approach is technically sound and logically focused toward reaching the final goal.
- It is very hard to see how practical hydrogen, CO, H_2S, etc. selectivities might be realized using microporous membranes.

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

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

- Tasks have been initiated. Planned tasks are conducive to achieving project goals. The work and schedule are consistent with DOE Program requirements. A major accomplishment is making spherical SS powders of uniform size and narrow size distribution. Sulfur tolerant materials have been identified as well as high temperature (> 500 deg C) material. Powder sintered SS tubes fabricated and module design in progress.
- Better separation at high temperature is important and needs to be incorporated in the gas conditioning plan. For example, will wet scrubbing need to be incorporated in the process prior to separation to remove contaminants and, if so, will the gas be heated back up to the optimum temperature?
- The gas purity is 95% which will presumably not be adequate for fuel cell applications. What is the technical approach for increasing purity?
- Kudos for acknowledging that you will get less than 100% selectivity. All membranes will suffer from this, but your willingness to discuss this will mean that you are more likely to achieve success and design the best "system." This might include some further hydrogen purification; please make sure to take this into account in design.
- Good use of results from previous research.
- The project was recently initiated and has made good progress; seems to be a little behind schedule.
- The project just started and then this criterion was not evaluated.
- The project only started in Dec 2004, but several preparatory project plan tasks have been initiated.
- Project has just started. Initial data on hydrogen: CO_2 selectivities of ~2:1 is not very encouraging.
- Contract just started.

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

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

- ORNL is working with NETL and Ames to successfully execute the tasks. PSDF facility will be used for long term tests with candidate materials. Meetings among technology partners have been held and continuing.
- The Partnerships being established with Southern Company, Pall, and NETL are promising but as yet undefined. The technical interaction with Pall on metal interfaces needs to be directed towards the eventual commercial scale of operation due to the importance of cracking in large scale systems.
- Good ideas on collaborators. This should be the very first thing that you accomplish. Seems that it should have been done before the project plan was finished, as different partners could bring unexpected capabilities. Organizing collaborators is not on the project schedule.
- Firm commitments from Pall, Southern Company and the commercial gasifier operator need to be established.
- The project is establishing partnership with industries.
- The project lists all its partners at the outset of the presentation and clearly identified their roles.
- Collaboration with Pall and NETL.
- Track records on prior experience of ORNL in membrane technology transfer area were not addressed.

Question 5: Approach to and relevance of proposed future research

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

- Future work plan is consistent with achieving proposed task objectives.
- The project will benefit from an experimental plan with explicit performance criteria. What will be the impact of trace contaminants besides sulfur species on the surface, such as phospine and arsine?
- The commercial scale of the system should be known and used to determine tests of physical stability at target operating temperature.
- Will you suffer from adsorption of CO, CO_2, etc. on the membrane?
- Plans presented on slides 17, 18, and 19 show excellent foresight.
- Would be rated a 4 if there were firm commitments in place from the commercial partners for fabrication and testing plans.
- The applicants future work plan is well thought-out and focused toward the construction and testing of membrane modules at a commercial gasifier facility.
- The project schedule should include some flexibility to accommodate any unforeseen failures or challenges encountered during the implementation of this project.
- No specific input, ideas were provided for realizing the needed >>2:1 hydrogen: CO_2 selectivity for the membrane.

Strengths and weaknesses

Strengths

- The project goals are consistent with DOE Program requirements of technology R&D for planned transition to hydrogen economy. The project aims to develop a technology to cost-effectively separate hydrogen from industrial gases, such as from a coal gasifier. The proposed effort builds on significant prior knowledge base available to the PI, and successful project outcome is highly likely.
- This new project rests on strong capabilities in membrane development.
- ORNL expertise in this area is a significant strength. Capability in this area will significantly help this project achieve success.
- Use or previous success in this area should be commended.
- The development in pilot scale of a hydrogen separation membrane with mechanical, thermal and chemical stability.

- The project only began in Dec 2004, so milestones and accomplishments are not addressed. The work effort is a logical continuation of work performed in the past under a different project, where the current effort seeks to eventually demonstrate at the pilot scale, the efficacy of a membrane system to separate hydrogen from a coal-based syngas
- The strength of the program lies in exploiting the very considerable technology base at Oak Ridge on microporous membranes. Hopefully some (perhaps recently declassified) new knowledge could be applied to address the hydrogen:other gases high selectivity challenge.
- Progress in membrane fabrication areas was presented.

Weaknesses

- The work is routine data gathering using known techniques. The work lacks innovative approaches; differences from current state-of-the-art are not explained, nor clear.
- It is not clear how the completion of the future tasks will expand the knowledge base to advance the hydrogen production technology beyond current state-of-the-art.
- It is not clear why the title and purpose of the project is scale-up using prior expertise and technical know-how, because hydrogen separation materials are not yet selected (or available) for scale-up.
- Recognizing that this is funded by FE, the EERE cost and performance targets should be addressed in the work and in future presentations.
- Is 95% purity good enough, given competing technologies that are being developed?
- Project plan needs to be revisited once collaborators are in place.
- Project should bring analyst in early on to help identify economic viability and areas that should be focused on in the design of the system.
- The extent of hydrogen recovery and the purity of hydrogen obtained by using these inorganic membranes can reduce the efficiency of the process and the cost of hydrogen production.
- A participation agreement between all concerned parties is unclear at this point, i.e., CRADA or other contract vehicle with Pall. All agreements should be quickly established to avoid any project delays or show-stoppers.
- The weakness is the inherent limited selectivity of microporous membranes, which the PI clearly recognizes.
- The background data discussion on membrane performance was inadequate. Inadequate discussions on follow-on step needed to achieve high hydrogen purity.

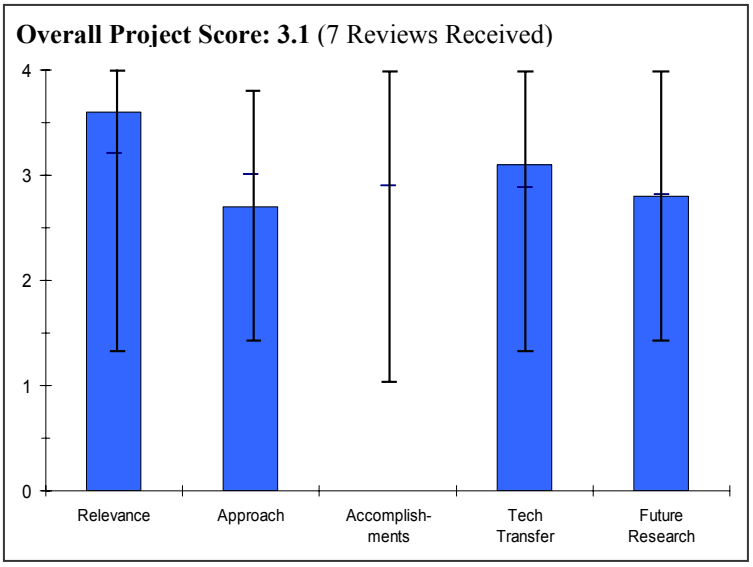
Specific recommendations and additions or deletions to the work scope

- None.
- It is recommended to improve the hydrogen selectivity of the membranes before scale-up.
- A suggestion: Could known totally selective dense hydrogen transport membrane materials be somehow utilized in conjunction with the present microporous membranes to achieve the required (not necessarily infinite) hydrogen, CO_2, H_2S, etc. selectivities?
- Need to develop quantitative membrane performance criteria which must be met in order to advance the testing from bench to larger scale units. Long term life membrane testing is needed. Need to obtain membrane performance data in the temperature and pressure ranges which match the feed stream from coal gasification step. Investigate the effects of membrane thickness/membrane pore ratio on membrane performance.

Project # PD-13: Low Cost Hydrogen Production from Biomass Using Novel Membrane Gasification Reactor
Lau, Francis; Gas Technology Institute (GTI)

Brief Summary of Project

The Gas Technology Institute project team (Arizona State University, National Energy Technology Laboratory, Schott Glass and Wah-Chang) will evaluate a novel concept of direct hydrogen production from biomass using a membrane reactor in a gasification process. The integrated gasifier and membrane reactor system is expected to meet or exceed the DOE's cost target of \$2.50/Kg H_2 for hydrogen production from biomass. The project will identify preferred candidate membranes (metal, ceramic, and glass), synthesize the materials, fabricate the membranes, test membrane performance, assemble a membrane module and demonstrate hydrogen production in a bench scale biomass gasification unit.



Question 1: Relevance to overall DOE objectives

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

- The project goal is to produce pure renewable hydrogen using a separation membrane integral with a biomass gasifier. This addresses renewable hydrogen production.
- No timeframe was presented.
- Does not specify candidate biomass type; nevertheless, highly relevant to a large class of non-agricultural biomass.
- Addresses DOE barriers as well as cost.
- The project, if successful, supports the critical goal of improved separations and a one-step shift reactor would lower costs of hydrogen production from biomass.
- The project does support the hydrogen RD&D plan.
- This project supports the President's Hydrogen Fuel Initiative to accelerate the introduction of hydrogen as the primary energy carrier in the USA.

Question 2: Approach to performing the research and development

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

- Although the approach is sound, gasifier technology is difficult.
- Membrane selection, development, and demonstration in only 2 years seem overly ambitious.
- Novel approach simplifies process, reducing capital costs.
- Seeking hydrogen production efficiency gain over conventional gasification.
- Cyclone System - is it low cost? Won't recycling of cyclone rejects build up in gasification chamber?
- Attempts to provide pure CO_2 output stream (for sequestration).
- Multiple fronts for membrane work. Need some decision points -- what are the show stoppers?
- The PI stated that the "types of membranes we have selected are 100% selective for hydrogen" -- on what basis is this assertion made?
- The scope of task 1, "to synthesize and test ceramic, metallic and multi-phase membranes," is far too ambitious given that numerous organizations have been working for years to develop successful membranes. What is the plan for coordinating with or building on these efforts?

- The approach appears to be sound for a new project.
- Increased emphasis should be placed on membrane development, testing and evaluation early on in the project which will impact the overall cost and efficiency of the project.
- The project addresses the cost reduction of the hydrogen production from biomass by developing a membrane reactor which combines gasification, with reforming and shift in one step.

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

This project has not started and therefore was not scored on accomplishments.

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

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

- The Team seems to cover the necessary expertise. However, the capabilities/businesses of the partners are not fully described. What is Wah Chang's core business?
- Partnerships appear effective. Should consider picking up a partner to validate sequestration potential.
- Are there any other DOE projects that may contribute to or benefit from the membrane work?
- Rated a 3 because it is unclear to me that this team is connected well enough to the organizations/expertise who have been leading work in the membrane field (such as Pall, Eltron, IdaTech, ORNL, SNL, etc.)
- This program appears to have a good diverse team.
- The collaboration with industry/university/laboratory exists and the tasks of each partner are clearly defined.

Question 5: Approach to and relevance of proposed future research

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

- The program has not officially started but the plan is logical.
- No timeline provided.
- Where are the go/no-go decision points?
- Material Synthesis & Testing Development should be highest priority; otherwise, well detailed with discussion of steps and methods.
- Project not yet started. Tasks are appropriate.
- There needs to be a major go/no-go decision before any work beyond Task 2.
- No proposed schedule was presented.
- This project appears to have a good overall plan.

Strengths and weaknesses

Strengths
- Promising process for hydrogen production from wood, waste, and other cellulose type biomass.
- Development of membrane/catalysts that can operate at gasifier temperatures would enable this and other projects.
- Reduction of capital costs worthy of effort.
- Strongly focused on DOE hydrogen from biomass targets. Work may prove to be applicable to other feedstocks/projects.
- The project appears to have a very competent and experienced team.
- The initial technical approach appears to be sound.
- Cost reduction of hydrogen production from biomass due to the elimination of steps.

Weaknesses

- Schedules, decision points, lacking in material presented.
- Extensive lab work would be necessary to prove feasibility of operations.
- Need decision points. Membrane work will be challenging -- need to define criteria for success, hydrogen purity, etc.
- Performance critically related to availability of robust membrane.
- The critical importance of the membrane in this project puts its development and evaluation on a critical pathway.
- The membrane that is selected will need to be demonstrated in a very difficult operating environment.
- Coke and sulfur resistance of membrane and catalyst under the reaction conditions.

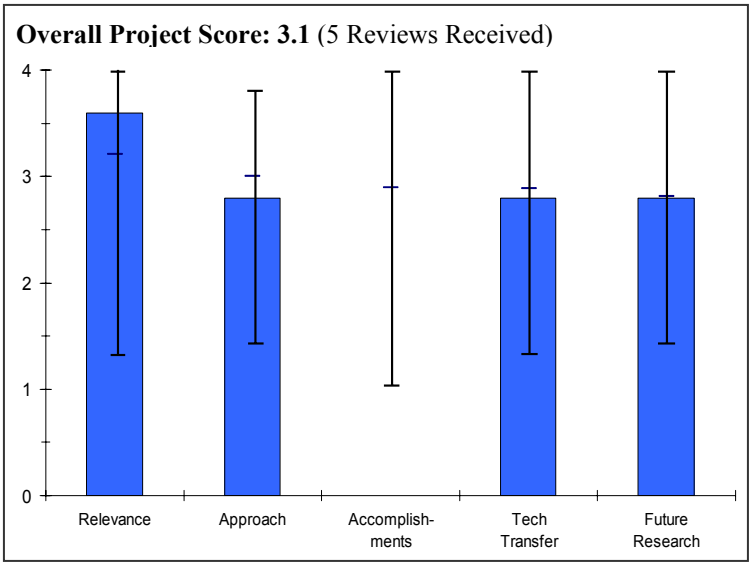
Specific recommendations and additions or deletions to the work scope

- Prioritize membrane/catalyst development with go/no-go at some point before proceeding.
- Detailing of project plans with timeline and decision points.
- Consider taking on a partner to demonstrate sequestration potential.
- Make sure this project takes advantage of any other DOE membrane work that is applicable.
- There needs to be a major go/no-go decision before any work beyond Task 2. No proposed schedule is presented.
- Consider further focusing scope of Task 1 to target on particular membrane materials that show promise.
- Emphasize membrane development and testing early in the project.

Project # PD-14: A Novel Slurry-Based Biomass Reforming Process
Vanderspurt, Tom; United Technologies Research Center

Brief Summary of Project

This United Technologies Research Center/University of North Dakota Environmental Energy Research Center project will provide a route to low cost hydrogen from biomass. This is a hydrogen permeable membrane enabled process where the cellulose and hemicellulose in slurred biomass are hydrolyzed to soluble sugars that then undergo solution phase reforming to hydrogen and CO_2 over a proprietary catalyst resistant to catalyst poisons.



Question 1: Relevance to overall DOE objectives

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

- Hydrogen from renewable (biomass) would advance DOE's goals and objectives.
- Good alignment with DOE program.
- Capable of cellulose and sugar operation
- Focused on DOE costs and efficiency goals for hydrogen. Different approach to hydrogen from biomass compared to other DOE projects.
- Project is fully relevant to the goal of developing options for producing hydrogen from biomass.
- The project does support the hydrogen RD&D plan.

Question 2: Approach to performing the research and development

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

- This seems to be primarily a catalyst development program culminating in a bench-scale reactor demonstration.
- The system efficiency go/no-go decision in FY2006 is appropriate but may not be valid without experimental data on catalyst behavior.
- Should modeling and catalyst development run in parallel?
- Potential for success enhanced with molecular/atomic catalyst design tools.
- Both wood and sugar stream should be tested as feeds.
- Barriers seem to be understood. Possible integration with other projects should be examined.
- The initial feasibility analysis should be carefully and thoroughly conducted so that a go/no-go decision can be made on the basis of realistic expectations for success.
- The approach appears to be sound for a new project.
- The first go/no-go decision appears to occur prior to the completion of the catalyst development stage.
- The development of an efficient, stable and durable catalyst is the key to both the economics and practicality of this overall project.

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

This project has not started and therefore was not scored on accomplishments.

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

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

- Involvement of a (energy) company with deep experience in fuel processing/reforming to assist in reactor design and/or a catalyst company would strengthen the program.
- Great synergy potential through collaboration.
- Unclear what relationships exist with other catalyst developers -- a lot depends on finding the right catalyst.
- Get a chemical or fuel company involved in the project -- who would be operating such a plant if successful?
- This program appears to have an adequate team.

Question 5: Approach to and relevance of proposed future research

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

- The program has not officially started. The vast majority of the presentation focused on catalyst discovery.
- Could use additional go/no-go points, especially during the catalyst development.
- The schedule for the initial analysis seems to be rather lengthy -- can't this time be shortened? Research on catalyst and hydrolysis should be performed concurrently with initial analysis if funding permits, as the results may feed into the analysis.
- More go/no-go decision points are needed (in years 2 and 3).
- This project appears to have a good overall plan but may have underestimated the difficulty in developing an efficient, long-term catalyst.

Strengths and weaknesses

Strengths
- Potential for feeds from cellulose or sugar intensive biomass.
- Good fit with DOE program.
- Employment of advanced molecular/atomic catalyst design.
- PI has good background and experience in catalyst development for the water gas shift reaction.

Weaknesses
- UTRC should involve more partners with specific expertise in fuel processing and reactor design.
- Catalyst development should precede plant and process design.
- Project plan could use additional go/no-go points, particular during with catalyst development phase.
- Not clear if potential challenges with pumping the slurry are addressed.
- The catalyst development will be a major undertaking given the difficult operating environment and it is key to the overall economics and practicality of this proposed process.

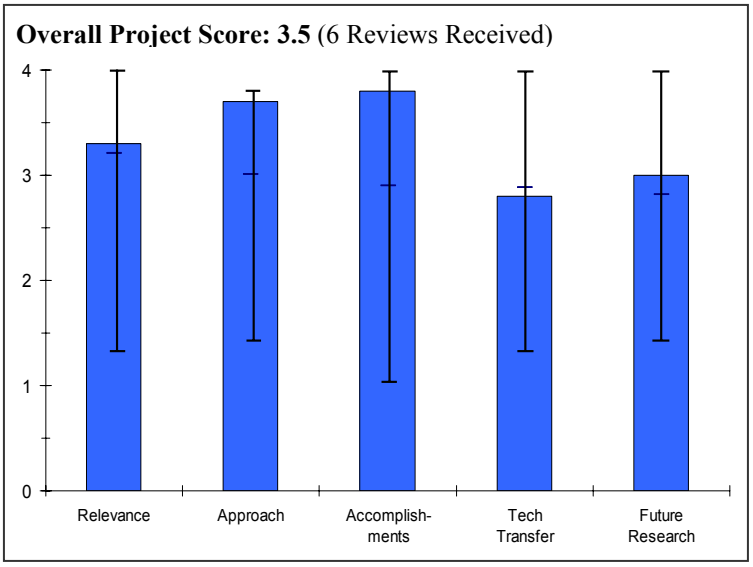
Specific recommendations and additions or deletions to the work scope

- Add a direct front-end agricultural biomass feed.
- Add go/no-go points in initial catalyst development work.
- Consider delaying design phase to during or after initial catalyst development.
- Another go/no-go decision point might be appropriate to address success or lack thereof on catalyst work if critical to success of project.
- More go/no-go decision points are needed (especially in years 2 and 3).
- Emphasize catalyst development and testing early on in the project and move ahead of go/no-go decision.

Project # PD-15: Maximizing Photosynthetic Efficiencies and Hydrogen Production in Microalgal Cultures
Melis, Tasios; University of California, Berkeley

Brief Summary of Project

In order to maximize photosynthetic efficiencies and hydrogen production in microalgal cultures, the University of California, Berkeley, is using molecular engineering to enhance photosynthetic solar conversion efficiencies and biomass/hydrogen production capabilities under mass culture conditions. The adopted approach applies DNA insertional mutagenesis and screening to isolate truncated chlorophyl antenna relevant transformants. Biochemical, genetic and molecular analyses of the transformant cells is then accomplished, followed by DNA sequencing to identify genes that confer a truncated chlorophyll antenna size (enabling the entire culture to better utilize available light).



Question 1: Relevance to overall DOE objectives

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

- Addresses only one barrier. There is not enough information on how this work impacts other projects and DOE's objectives.
- Very long term research, with good potential if a large number of biological and engineering unknowns are solved.
- The aspect of light efficiency in the context of the overall impact on hydrogen production could have been elaborated to further highlight the importance of this problem and the overall significance of the team's findings.

Question 2: Approach to performing the research and development

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

- The project has a clear focus on reducing the chlorophyll antenna size.
- The approach is thorough with not only identifying gene but also the functional characterization.
- Fundamental research project that is a long way from determination of technical feasibility.
- The approach concerning the identification of mutants and their characterization is well thought out and outstanding. The proposed path concerning the over-expression of the Tla gene product could provide some really significant results. With respect to the work proposed for the characterization of gene product itself, albeit rational, it is not clear it will provide any insights into the function. It may be very useful at this stage to develop a working hypothesis for Tla function that could be approached experimentally.

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

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

- Good progress in reaching overall project goal; however, results are not shown in terms of hydrogen production amounts achieved.

- The program has made consistent and significant progress from year-to-year towards meeting the goals.
- Fundamental understanding of the behavior of the microorganism is progressing.
- Clear cut results have been shown that directly impact barriers that are being addressed.

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

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

- More detail on collaboration with partners would be helpful to evaluate the potential impact of the project.
- Using NREL and ORNL to leverage their systems analysis and engineering capabilities.
- Is it possible to collaborate with other institutions to accelerate program?
- Little real collaboration exists.
- The coordination with the NREL group exists, but a greater focus on the overall implications of lowering the efficiency of light harvesting in cells to the overall potential impact on hydrogen production in bulk cultures would make this coordination more clear.

Question 5: Approach to and relevance of proposed future research

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

- Quantitative goal is not defined on future work slide. Is it the 30% long-term goal specified earlier in the presentation?
- The plan for future work is aimed at providing data which will aid in future engineering of the Chlamydomonas reinhardtii.
- Focus is on incremental elucidation of fundamental information.
- The proposed path concerning the over-expression of the Tla gene product could provide some really significant results. With respect to the work proposed for the characterization of the gene product itself, albeit rational, it is not clear it will provide any insights into the function. It may be very useful at this stage to develop a working hypothesis for Tla function that could be approached experimentally.

Strengths and weaknesses

Strengths
- Good progress in meeting project objective.
- Systematic approach to addressing the barrier of light utilization.
- The successful identification and characterization of a mutant strain that produces a lower complement of antenna molecules.
- The proposed experiments concerning the over expression of Tla with the potential for even lower production of chlorophyll molecules.
- The PI helped draft the DOE HFCIT roadmap and is working directly to overcome the technical barriers as defined by DOE.

Weaknesses
- Progress towards DOE goals is uncertain since actual hydrogen production from algae is not established.
- The lack of defined goals or hypotheses in experiments aimed at the characterization of Tla.
- The lack of a clearly articulated set of implications concerning the impact of hydrogen production.
- It is not clear how their partners fit in and how they actually work together.

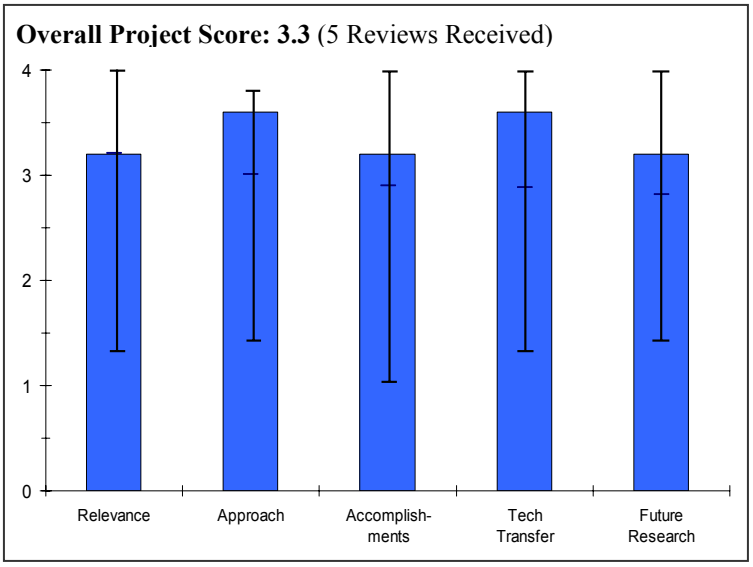
Specific recommendations and additions or deletions to the work scope

- Document progress against the DOE targets as the project proceeds.
- Develop a working hypothesis for Tla function.

Project # PD-16: Biological Systems for Hydrogen Photoproduction
Ghirardi, Maria; National Renewable Energy Laboratory

Brief Summary of Project

The goal of this National Renewable Energy Laboratory project is to develop hydrogen photoproduction technologies based on microbial water-splitting processes. The project is organized into three tasks: engineer an algal hydrogenase that is resistant to oxygen inactivation; develop and optimize a physiological method to produce culture anaerobiosis and subsequent hydrogen production activity in algae; and introduce bacterial hydrogenase with increased oxygen resistance into a water-splitting photosynthetic cyanobacterial system.



Question 1: Relevance to overall DOE objectives

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

- Aims first at making known biological hydrogen generating systems more robust, and in coupling a more robust system to photo oxidation of water. In these ways, addresses Barrier Z: continuity of hydrogen photoproduction.
- Authors argue that their approach addresses long-term hydrogen production goals; not clear why a biological means of hydrogen production should be a better or cheaper long term alternative to other means.
- Discussion of feasible target yields of hydrogen, even theoretical limits, would be helpful.
- Addresses only one barrier.
- The engineering of an oxygen insensitive hydrogenase enzyme could make the use of algae for hydrogen production more practical. The structural characterization of the algal hydrogenase could provide some interesting insights.
- Project addresses the challenge of continuous hydrogen production for biological system.

Question 2: Approach to performing the research and development

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

- Producing an O_2-stable hydrogenase is a high priority and rightly central to the approach.
- Current focus is on O_2 channeling to the active site, the hypothesized mechanism of O_2-instability. Some comments:
  a. Proposed double-mutant studies will resolve issue of extent O_2 channeling contribution to active site stability.
  b. Unclear whether pursuing further modeling of O_2 access channels will yield new information.
  c. More comprehensive study of other possible stability determinants may be in order if closing both predicted O_2 channels does not greatly enhance stability.
  d. Using more O_2-tolerant and crystallographically characterized FeFe-H2ase from C. pasteurianum as target for mutagenesis is very reasonable. Is there a plan to study and "improve" O_2 tolerance of semi-O_2-stable NiFe hydrogenase?
- Introducing an O_2-tolerant hydrogenase into a photosynthetic organism is one of the project's ultimate aims. This is an ambitious goal. A clearer set of incremental goals and plans would be helpful.
- PI has clear understanding of technical approach and pathways necessary to achieve goals.

- The long term possibilities of utilization growth under sulfur deprivation are difficult to predict. Developing strategies for metabolic engineering could be more fruitful.
- While the three different approaches were seen as "disjointed" in previous annual review, the multiple approach has a higher likelihood for success.

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

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

- Heterologous (E. coli) expression of several Fe-only H_2 cases has been accomplished. This is an excellent advancement.
- A cyanobacterial hydrogenase knockout has been prepared: also a significant and necessary preliminary achievement.
- Coupling hydrogen production to photosynthetic water oxidation using native cyanobacterial electron carriers represents the ideal situation; however, current work shows this may not be very efficient. Is there a practical plan for overcoming this barrier? This could be an especially difficult issue, as reduced mediators such as methylviologen are themselves unstable in O_2.
- Algal immobilization has been demonstrated to enhance hydrogen production. This is a conceptually simple yet important result.
- Progress is shown for all subtasks; however, improvements from previous year results are not specified in the presentation so it is difficult to assess results.
- The identification of the gene products involved in hydrogenase expression is a highly significant achievement and will provide for significant advancements in the context of manipulating the enzyme to enhance the process.
- Identified O_2 pathways that deactivate the hydrogenase. Algae immobilized and hydrogen production increased relative to previous production rate in suspended cultures.

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

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

- Several, mostly academic, collaborators are listed.
- Enlisting participation from a crystallographer and molecular modeling group is appropriate. Participation from a microbiologist with expertise in the appropriate metabolic systems could also be useful.
- Excellent use of collaborations and partners to further research.
- The project is well integrated with Berkeley and Oak Ridge groups.
- Collaboration occurring on all subtasks. Collaboration occurring among DOE Program's biological hydrogen production projects. An integrated system is planned with UC Berkeley and ORNL.

Question 5: Approach to and relevance of proposed future research

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

- The work is very exploratory and, possibly as a consequence, not much in the way of contingency planning is articulated.
- Long-term aspect of work means that progress towards DOE goals is slow.
- The proposed studies involving the mutagenesis of the hydrogenase enzymes, the expansion into the possibility of using other hydrogenases with desirable properties, and studies aimed at probing the effectiveness of cyanobacteria for hydrogen production will directly explore methods to advance the overall goals.
- Milestone is well defined in the upcoming year for each subtask. PI should have further-out-year plans as well as a "percent completed" grasp on the project.

Strengths and weaknesses

Strengths

- Plan for directly coupling sunlight to hydrogen production, without production of waste CO_2, goes to the heart of the aims of the Hydrogen Initiative.
- Work will push forward basic studies of hydrogenases, an area that has traditionally been characterized by slow progress and difficult-to-handle molecules/systems.
- Basic issues addressed -- mediators of O_2 tolerance in hydrogenases, species and strain variability of hydrogenases, coupling of hydrogenases to electron-carriers -- are of broad relevance, and likely common to any serious effort toward microbial hydrogen production.
- Innovative idea of fiberglass support produced good results. Demonstration of actual hydrogen production is significant in ability to relate the project to overall DOE objectives.
- Significant progress concerning the ability to express hydrogenase heterologously for mutagenesis studies on the hydrogenase enzyme.
- The established team is making good progress.

Weaknesses

- Work in places appears to aim at solving difficult engineering goals before the basic science is firmly in place.
- The unclear justification of the future efficacy of sulfur deprivation as a growth variation for hydrogen production.
- The responses to previous reviewer's comments could have been addressed in a more specific and substantive manner.

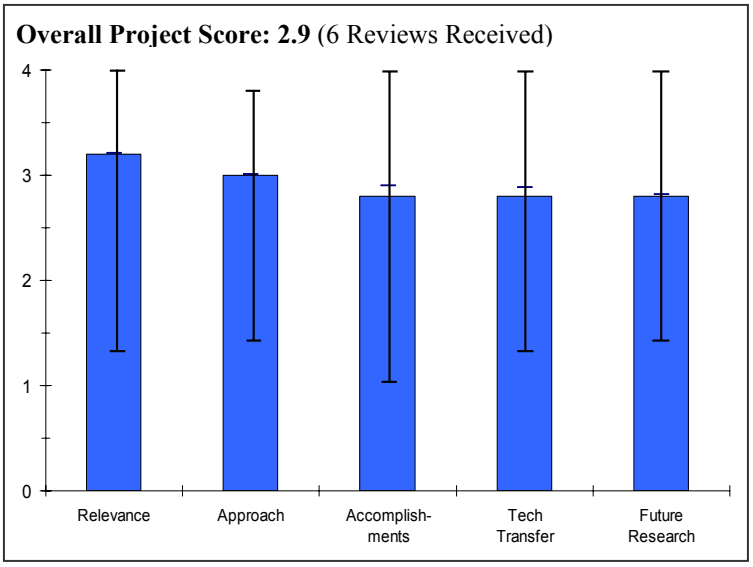
Specific recommendations and additions or deletions to the work scope

- None provided.

Project # PD-17: Creation of Designer Alga for Efficient and Robust Production of Hydrogen
Lee, James; Oak Ridge National Laboratory

Brief Summary of Project

In this project, Oak Ridge National Laboratory will create a designer alga by inserting a proton channel into the algal thylakoid membrane. This work will move toward overcoming the low rate of hydrogen production in photobiological systems. Success of this work will have a significant impact (a 10-fold improvement) on technology development in the field of renewable hydrogen production from water.



Question 1: Relevance to overall DOE objectives

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

- Clean production of hydrogen by biologically coupled water oxidation/proton reduction is certainly relevant to DOE's objectives.
- The proposed mode of hydrogen generation may not be efficient enough to be useful in the short term.
- This project addresses a key barrier to photobiological hydrogen production.
- Very long term fundamental effort.
- Addresses rate of hydrogen production technical barrier.
- The overall goals of this work interfaces well with the overall goals and work being conducted in the NREL and UC Berkeley groups.

Question 2: Approach to performing the research and development

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

- Whether the proton channel will correctly assemble needs to be tested before progressing to new channel gene designs.
- A proton channel seems to have a strong possibility of toxicity. Even if the algae survive, stably maintaining the channel-encoding plasmid may be difficult. Fairly strong selection pressure may have to be exerted to keep the plasmid in the host.
- The natural level of expression from the hydrogenase promoter may or may not be well-matched with the optimal number of proton channels.
- A thorough investigation of inhibitor studies should be performed to document the scientific basis for the issue and the technical approach. Is it really necessary to design, from scratch, a proton channel and its corresponding gene, or are there other, simpler solutions already available in other organisms?
- Fundamental nature of this effort makes assessment of technical feasibility unrealistic at this stage.
- Approach utilizes modeling and experimental methods. Approach to insert a proton gradient channel addresses four issues affecting the hydrogen production rate.
- The approach is creative, but the implications of the expression of the channel and the recovery of cells during the switch between aerobic and anaerobic metabolism may provide a significant barrier. There is a need to attempt to address this in a more substantive manner.

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

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

- The goals for the past year have been met. A channel has been designed, based on known channels. Two gene constructs have been made. These are very modest goals relative to the overall goals of the project.
- Seems to have gotten somewhat sidetracked from the major objective.
- Need to refocus on the proton channel effort and ensure that sufficient basis is developed to understand the potential impact on the full system, including oxygen generation.
- The results to date provide the groundwork for the experimental work proposed.

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

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

- Collaboration with investigators at NREL and UC Berkeley should be helpful for work with photosynthesis and algae, respectively. How active a role these investigators play is not clear at this stage.
- Publications in peer-reviewed literature will be essential for validating approach and results.
- Good cooperation with partners and coordination with other labs working in this area.
- The project is well integrated to the overall goals of utilizing algae for hydrogen production and complements and clearly complements the work at NREL and UC Berkeley very well.

Question 5: Approach to and relevance of proposed future research

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

- The path ahead is clear: the designed constructs are to be inserted into appropriate hosts and tested for channel production (and enhanced anaerobic hydrogen production).
- If the present set of constructs does not work, contingency plans are not clear. Project is described much more like a pilot project than an ongoing investigation.
- Need to work closely with photobiological teams to ensure timeline and activities align with overall approach.
- Should establish quantitative targets for out years.
- The future research direction is sound and the tools are in place to evaluate outcomes.

Strengths and weaknesses

Strengths
- A reasonable rationale for building a channel for dissipating protons in photosynthetic systems has been articulated.
- The materials for carrying out the primary proposed experiment are designed, assembled, and ready. All that remains is to test them.
- Proton channel addresses four issues pertaining to the rate of hydrogen production.
- This is a creative idea that could make a significant impact.
- Well integrated in the overall goals and work at other labs.

Weaknesses
- Project is very limited in scope: more of a proof-of-principle experiment. Not clear how the approach can/should be revised if the first efforts fail.
- More consideration should be given to the "switch" of the direction of proton flow through the channel with regards to meeting the DOE target.
- The lack of a clear idea concerning the practicality and implications of switching to anaerobic metabolism and the recovery rate of cells.

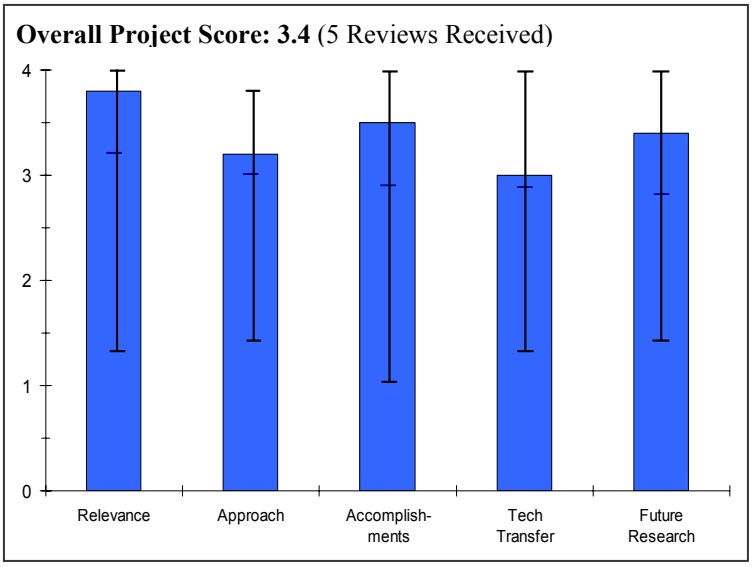
Specific recommendations and additions or deletions to the work scope

- Before significantly ramping up proton channel efforts, sufficient basis needs to be established and validated by the peer community.

Project # PD-18: Fermentative Approaches to Hydrogen Production
Maness, PinChing; National Renewable Energy Laboratory

Brief Summary of Project

This National Renewable Energy Laboratory project will develop fermentation technologies to convert renewable biomass resources to hydrogen. This project will screen and select for microbes that can efficiently convert cellulose and the various sugars in hemicellulose directly to hydrogen thus lowering the feedstock cost. Metabolic engineering of a selected model microbe is being performed to improve its hydrogen molar yield. Addressing both the feedstock cost and the hydrogen molar yield will enable fermentation technology to be competitive in the long term.



Question 1: Relevance to overall DOE objectives

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

- Strongly addresses issues of hydrogen production yield and use of lower cost feedstocks.
- Begins to address direct and efficient use of agricultural/waste feedstocks.
- Fermentation is articulated as an intermediate strategy, for use as costs of glucose production from biomass gradually lower and may also be useful as a flexible and low-tech option for fuel production from low value feedstocks.
- The project is fully relevant to the goal of biological production of hydrogen from biomass.
- There is a clear need to explore the production of hydrogen from biomass.

Question 2: Approach to performing the research and development

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

- Screening available cellulolytic strains for cellulose degradation capacity and ease of growth is a straightforward strategy.
- The technology for efficient screening (for cellulose degradation and hydrogen production) has been developed and is in place.
- Given the increasing rapidity of microbial whole-genome sequencing, may not be necessary to restrict screening to sequenced organisms.
- Approach to metabolic engineering of chosen strain is not described.
- It is not clear how the microbes are being selected for screening or how extensive the screening effort will be.
- Focus on cellulose feedstock versus glucose will improve the chances that the project will achieve cost H_2 production cost goals.
- Project is building on work with C. Thermocellum, which shows promise for direct fermentation of cellulose.
- For metabolic engineering perhaps a concern is not only the presence of a genome sequence but also that the organism be amenable to genetic manipulation.

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

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

- Progress described since Oct 2004, is impressive.
- Bioreactors for studying cellulose degradation and hydrogen production are well developed.
- Nine strains have been screened, but the anticipated scope of the screening effort is not clear. How were these nine chosen, how well do they represent the diversity of cellulolytic organisms, and how many more (if any) will be screened?
- Not clear what barriers to metabolic engineering strategies are anticipated, or how these will be handled. Presentation more focused on progress to date. Next phase may be significantly more difficult.
- Ability to reclaim solid lignocelluloses products from corn stover is encouraging; approach seems well suited for reclaiming "waste" solids produced from biomass processing to glucose.
- New project.
- Good progress on screening tasks
- New project -- in light of this, very good progress.
- Really nice groundwork has been laid in the early stages of the project. The overall concept is sound but there may be barriers in metabolic engineering.
- Project started in FY05 so design and planned work is good.

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

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

- Limited collaboration is mentioned.
- Consultation with a microbiologist or biochemist with (other groups with) expertise with the relevant microbes and/or metabolic pathways may be useful for the next stage of targeted mutagenesis.
- There was little information on the extent of collaboration with other institutions/projects.
- Good collaboration with other university researchers.
- No collaboration with industry.
- New project -- collaborations just beginning to develop; very good start.
- Looks like partners and sources for target organisms have been identified. Not clear the role of all partners.
- Described intra-project collaboration as well as collaboration among other DOE- funded projects.

Question 5: Approach to and relevance of proposed future research

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

- Details of the strategy for metabolic engineering of the selected strain were not described in the (brief) presentation.
- Lacks quantitative goals for optimization of fermentation parameters and molar yield of hydrogen.
- The future research direction is sound and the tools are in place to evaluate outcomes.
- The future work proposed is sound; perhaps a higher premium should be placed on the practical features of the organism and not the presence of a genome sequence since presumably a sequencing effort for a highly amenable organism could be justified. The team may want to rethink the emphasis on an organism that can utilize the more recalcitrant forms of biomass since it is not necessarily true that these organisms will be most effective for the less recalcitrant forms. The use of a thermopile may be impractical if there are options due to energy required for heating.
- Excellent specification of milestones in all future years of the project.

Strengths and weaknesses

Strengths
- Important problem of direct and efficient conversion of biomass to hydrogen is addressed.
- Impressive and rapid progress has been made so far.
- A logical schematic approach is outlined.
- Baseline level of hydrogen production (from a non-engineered, natural cellulolytic strain) is already very good.
- This is a creative idea that could make a significant impact.
- Very good value given the small size of the DOE investment.
- Solid groundwork has been laid and good preliminary progress has been made.

Weaknesses
- More discussion of the metabolic engineering strategy would be useful, as this could present significant difficulties
- Perhaps some additional considerations need to be made concerning the priorities of the organism selected as discussed in comments on proposed research.
- The implications of carbon dioxide production in this process were not discussed.

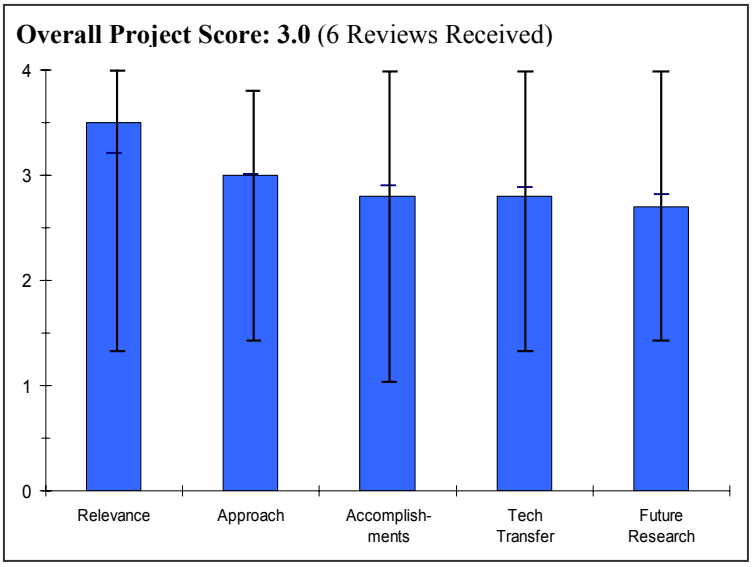
Specific recommendations and additions or deletions to the work scope

- None provided.

Project # PD-19: Hydrogen Reactor Development and Design for Photofermentation and Photolytic Processes
Blake, Dan; National Renewable Energy Laboratory

Brief Summary of Project

The objective of this project at the National Renewable Energy Laboratory is to identify materials of construction for reactors that can be used for solar production of hydrogen from water by photobiological or photoelectrochemical processes. This application will likely require large area reactors with demanding specifications for the performance of materials in the outdoor environment. Most of the potential designs will require transparent coverings that have low hydrogen permeability. Accelerated and outdoor weathering testing of materials is underway and key optical and physical properties for photolytic reactor applications are being evaluated.



Question 1: Relevance to overall DOE objectives

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

- Addresses long term goal of harnessing solar energy for hydrogen production.
- The project aims at assisting DOE sponsored hydrogen initiative with the identification of solar reactor concepts and developing reactors for photobiological and photoelectrochemical hydrogen production.
- This is clearly a project that must be undertaken in the context of the overall goal of using photosynthetic organisms to produce hydrogen.
- Addresses some key issues on materials of construction and design of reactors for low cost hydrogen via photofermentationn and photoelectrochemistry.

Question 2: Approach to performing the research and development

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

- Current work focused on selection and testing of reactor construction materials. This is a natural first step. The technical approach taken is very straightforward and has been effectively executed.
- Possible reactor designs described only schematically; not clear how well developed these plans are.
- The approach of material selection, assembling database on reactor design, and weathering tests are based on sound concepts.
- The approach is well balanced and is integrated with the ongoing research efforts.
- At this point it appears that the groundwork has been laid in terms of cataloging the available information concerning materials potentially available and their properties. The development of prototype reactors compatible with the growth of algae is complementary to the development of growth methods and is justified to be conducted in parallel to this work. The value of doing the work in house was fairly well justified.
- Very good approach.
- Starting with comprehensive data gathering and cheap weathering tests is a good cost-effective approach.
- Most elements of the approach are sound, but the low level of funding only permits a modest amount of work to be done. New work needed in reactor design and modeling as well as significant experimental testing of materials.

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

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

- Technical accomplishments are adequate but overall progress seems slow. Timeline for project is quite long.
- The project started in 2003 and only 1% of the goal has been accomplished.
- Selection of polymer material has been attempted by conducting weathering tests and hydrogen and oxygen permeability.
- These preliminary studies should set the course for achieving the goal of designing solar reactors.
- It appears that the footwork and cataloging of potential materials has been accomplished and basic engineering considerations have been addressed.
- Accomplishments are in line with the funding and resources provided. A substantial increase in both will be required to address the critical questions.

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

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

- Collaborations perhaps not necessary or appropriate at this stage, but will be important to establish for next phase.
- Nuclear Filter Technology is serving as an industrial partner.
- Fits well and is a necessary component of the overall concept of harnessing hydrogen producing photosynthetic organisms.
- Excellent job collecting and analyzing info from a variety of sources.
- Insufficient resources.

Question 5: Approach to and relevance of proposed future research

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

- Presentation slides and/or FY2004 Progress Report do not give much information about proposed future work. Therefore, difficult to evaluate future plans.
- The proposed research of identifying reactor concepts and design is important, but lack of details makes it difficult to assess the merit of future research.
- No specific strategy for minimizing the gas permeability of polymers.
- A reasonable and rational plan for future work is proposed.
- Issue of interaction of material of construction with microorganisms is not analyzed.
- Could bugs lower solar transmittance?
- Plans are modest compared with scale of task. DOE should seriously evaluate it's commitment to this effort at this time.
- The PEC and photofermentation approaches for hydrogen are longer term options, but validating the possibility of an efficient, effective reactor built out of affordable materials is a question that should be addressed sooner rather than later.

Strengths and weaknesses

Strengths

- Very reasonable first steps being taken toward project goal of photoreactor development.
- Project goal well within the defined goals of the hydrogen initiative.
- Carried out weathering and permeability tests for a range of polymers and identified possible candidates.
- Fits well into the overall plan and is a necessary element for using photosynthetic organism to produce hydrogen.
- Breadth of overview.
- Focus on key issues with respect to bioreactor performance

Weaknesses

- Pace and timeline of project seems somewhat slow.
- No detailed outline for designing reactors or conceptual design to address any of the problems highlighted in the presentation.
- The considerations of the heat generated and the thermal conductivity of materials was not addressed very well. It is not clear how temperature will be maintained and how maximizing light intensity and maintaining growth temperatures that are compatible was not adequately addressed.

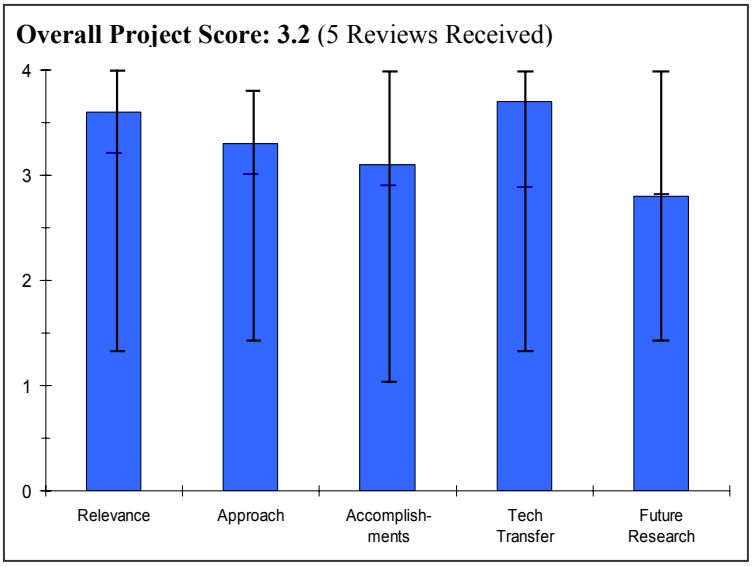
Specific recommendations and additions or deletions to the work scope

- The research efforts should also include the problems associated with temperature control and influence of electrolytes and catalysts on the degradation of polymer materials.
- Add biocompatibility of various materials.

Project # PD-20: Photoelectrochemical Hydrogen Production: SHGR Program Subtask
Miller, Eric; University of Hawaii

Brief Summary of Project

In this project, University of Hawaii 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 that incorporate low-cost thin-film materials and are suitable for use in commercial-scale systems.



Question 1: Relevance to overall DOE objectives

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

- The proposed research on photocatalytic hydrogen production using metal oxide (tungsten trioxide) based semiconductors is quite relevant to DOE Hydrogen Fuel Initiative.
- This is a very well thought-out project that seeks to develop a thin-film oxide photoelectrochemical device for electrolysis of water. The project is highly relevant to the DOE goals, and represents one of the more promising approaches to inexpensive electrolysis.
- Long term effort that could have significant impact.

Question 2: Approach to performing the research and development

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

- The approach of extending the photo response with nitrogen doping is interesting.
- Issues related to charge trapping and charge recombination within the hybrid electrode need to be addressed.
- Minimizing the loss of photogenerated charge carriers at the interface is important towards achieving the goal of 2% efficiency.
- Band energy and redox potential considerations need to be made while evaluating the photocatalysts.
- The approach is straightforward, and shows careful planning with respect to the logical development of large-scale WO_3 films.
- Good thinking about future manufacturability.

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

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

- The project has shown satisfactory progress by demonstrating the feasibility concepts.
- "Hybrid Photoelectrode" based on low temperature reactively sputtered films has been demonstrated. The authors have also succeeded in extending the response of the oxide semiconductor into visible by nitrogen doping.
- It is not clear if achievement of 4 to 7 % solar-to-hydrogen efficiency, with large-scale WO_3 films, is within reach. The present performance is only 0.7% -- not very remarkable.
- Efficiencies are still low, but progress is being made.

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

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

- The researcher has a well established collaboration with the research groups at UNLV, University of Colorado, and other institutions.
- Partnership with MVSystems and Intematix has also been identified.
- Good interaction with NREL and several universities. In particular, the research team has developed a strong collaboration with MVSystems, a major developer of thin-film deposition equipment.
- Good list of collaborators.
- Past related effort has a history of strong collaboration.
- Strong collaborations now in place with MVSystems, Intematix, and NREL.

Question 5: Approach to and relevance of proposed future research

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

- The PI plans to implement a materials discovery and development plan to reach solar-to-hydrogen efficiency of 4 to 7%. Although bandgap engineering might help this effort, the PI should address some of the basic issues that lead to the loss of photogenerated charge carriers. Effort in this direction is lacking.
- Future research plans call for a 4% to 7% solar-to-hydrogen efficiency device. However, there is little discussion of other aspects of the proposed device, e.g., suitability for large-scale manufacture. Also, there is an inadequate cost analysis for large-scale systems.
- Unless the efficiency can be increased significantly soon, there seems to be no end to the "tweaking."
- Emphasis should be on new materials discovery. Current set of materials being examined seem to be hitting performance plateaus.

Strengths and weaknesses

Strengths
- Ability to assemble a photocatalyst assembly and effort to extend the photo response into the visible via bandgap engineering.
- A good collaborative research partnership.
- Methodical work with a systematic approach to development of the WO_3 large-scale films.
- Good combination of skill sets in thin film fabrication, evaluation of devices as photocells and photoelectrodes, and characterization.

Weaknesses
- A better understanding of the energetic considerations for hydrogen and oxygen production (band energies and water redox potentials) is needed while exploring new catalytic systems.
- The value of solar-to-hydrogen efficiency is based on current measurements. Actual hydrogen evolution data is necessary to justify the claims. (Define acronyms next time -- the term STH is not well defined in the presentation).
- Progress to date is not particularly remarkable. More discussion of this status would be interesting.

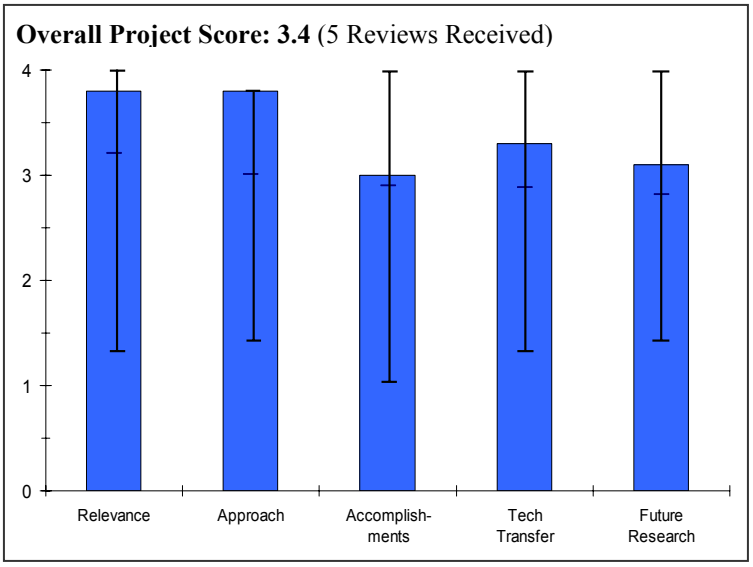
Specific recommendations and additions or deletions to the work scope

- A colloidal approach of synthesizing WO_3 particles and deposition as thin films can provide a low temperature film casting method. Similar method has been employed in casting TiO_2 films for solar cell applications.
- Spectroscopic investigation to identify charge recombination/separation process at different stages of modification can provide insight into the loss of photogenerated charge carriers.
- None suitable at this point in the project.

Project # PD-21: Photoelectrochemical Water Systems for Hydrogen Production
Turner, John; National Renewable Energy Laboratory

Brief Summary of Project

The goal of the National Renewable Energy Laboratory's research is to develop a stable, cost effective, photoelectrochemical-based system to split water using sunlight as the only energy input. The work during this year focused on identifying and characterizing new semi-conductor materials that have appropriate band gaps and are stable in aqueous solutions.



Question 1: Relevance to overall DOE objectives

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

- The development of photocatalytic systems using low bandgap semiconductor materials for the production of hydrogen supports the DOE hydrogen R&D initiative.
- Innovative and technically risky approach to achieving high solar-to-hydrogen efficiency through the use of III-V multijunction. This project builds on the very high efficiency and, in particular, the very high voltages of multijunction III-V devices. In addition, the approach is interesting because multijunction devices allow tailoring of the bandgap -- a quality that is well-suited to electrolysis.
- Project may provide a long term source for hydrogen, but it is not likely to impact production in the next two decades.

Question 2: Approach to performing the research and development

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

- PI is knowledgeable of the basic scientific issues and is developing a database resource to support future work.
- The PI has employed a multifaceted approach to improve the photocatalytic efficiency of III-V semiconductors. Both the bandgap engineering and computational modeling approaches are sharply focused towards achieving higher efficiency for photocatalytic hydrogen production.
- Technical barriers for the development of high potential difference (voltage) devices are aggressively pursued. The project is carefully thought-out and is quite feasible, though risky. In addition, there is a very good mix of experimental results and computational methods for bandgap tailoring.
- Adding materials modeling and simulation (in addition to the NREL computational activity) would help understand what is happening (such as corrosion rates) and provide direction to the research approach.

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

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

- Nitrogen doping has been employed to achieve bandgap control of GaP semiconductor catalyst and extend the response into the visible.
- The photocorrosion issue has been addressed using GaPN semiconductor systems.
- GaPN/Si multijunction devices have been demonstrated and characterized. The potential for these devices to achieve high STH efficiency is quite high. NREL has a remarkable record of development of multijunction III-V devices, which clearly benefits the present work.

- Much scientific progress, but little progress measured against technical program goals.
- Very good progress, but industrial partner involvement and more emphasis on computational techniques might have resulted in more progress.

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

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

- Collaboration with partners is apparent.
- The researcher has a well established collaboration with the research groups at Colorado School of Mines and University of Colorado.
- Partnership with MVSystems and Midwest Optoelectronics has also been identified, though their roles were not described.
- There is good collaboration between NREL, Colorado School of Mines, and University of Colorado.
- Seeing more cooperation in workers from other institutions pursuing similar goals.
- There is mention at the beginning of the presentation that two industrial partners have been brought in, but there is no evidence that they participated in the project.

Question 5: Approach to and relevance of proposed future research

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

- The approach of developing new catalyst materials and multijunction semiconductor systems with energy control provides a sound basis for continuing the proposed study.
- Matching of bandgap energetics remains a problem in matching the redox potentials of water splitting reaction. The proposed approach of bandgap grading and CIGSS type materials are new efforts in this direction.
- Future research will attempt to improve the GaPN/Si multijunction device and also the CIGS device. The research appears to be moving in a useful direction. Plans for future work appear to follow a logical path.
- This area needs a fresh set of approaches to new materials identification (see below).
- Future plans for materials modeling is good, but need to look outside of NREL for additional expertise to supplement the strengths of NREL.

Strengths and weaknesses

Strengths
- Good understanding of the research problem and unique strategy to tackle the problems.
- Highly competent research team with experience in high efficiency devices, several different device configurations, and good use of combinatorial approach to the design of improved devices.
- Great understanding of what the research results should be.

Weaknesses
- Too many slides in too little time.
- Fundamental understanding of the dynamics of charge separation and interfacial charge transfer in new catalyst is necessary to understand the efficiency limiting factors.
- It is not clear in the viewgraphs why presently existing high efficiency multijunctions, and for that matter high efficiency silicon devices, are not considered in this research. A cost/performance comparison of these semiconductor device technologies would be interesting.
- Many of the proposed paths to develop efficient and stable PEC materials have been deeply mined. This is an area that needs a fresh approach to identify new classes of candidate materials.
- Limited internal computational activity continues to be a leading weakness of this project.

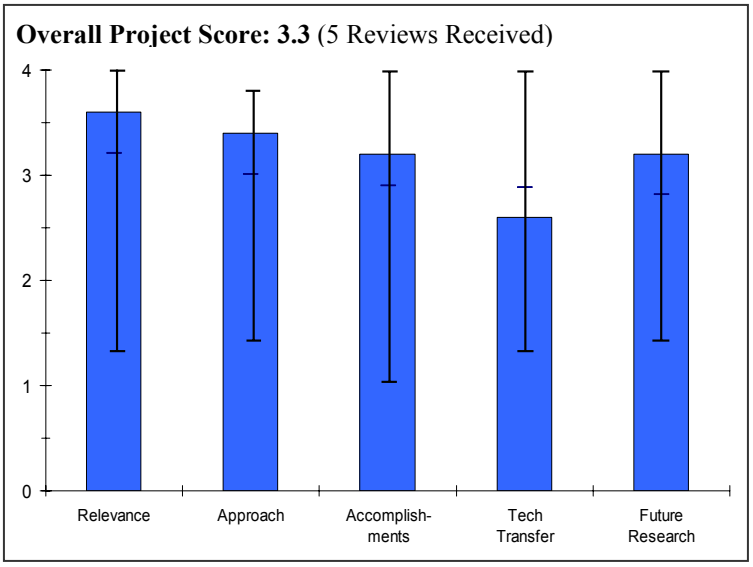
Specific recommendations and additions or deletions to the work scope

- Energetic considerations should be made while designing new catalysts.
- The researchers should make an effort to collect hydrogen evolution data.
- Spectroscopic investigation through collaborative effort is needed to elucidate the charge transfer dynamics in newly developed materials. (Collaboration with the spectroscopic team within NREL is a possibility).
- Impressive list of presentations at the scientific meetings. Should also make an active effort to communicate the results via scientific publications.
- No changes appear necessary at this point.
- Team with another organization to strengthen the materials computational efforts.

Project # PD-23: Low-Cost, High-Pressure Hydrogen Generator
Cropley, Cecelia; Giner Electrochemical Systems LLC

Brief Summary of Project

Giner Electrochemical Systems, LLC has an overall project goal of developing and demonstrating a low cost, high-pressure water electrolyzer system, which will eliminate the need for a mechanical hydrogen compressor, increase electrolyzer hydrogen discharge pressure to 5,000 psig, and demonstrate a 3,300 scfd high pressure electrolyzer operating on a renewable energy source. Past year (Jan 03-Mar 04) tasks included the development of lower cost materials and fabrication processes for stack components, fabricating and demonstrating an electrolyzer stack and system producing hydrogen at 2,000 psig, and designing and fabricating a test stand for 5,000 psig operation.



Question 1: Relevance to overall DOE objectives

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

- This project fits well with DOE's goals for production of hydrogen.
- Hydrogen by low-cost, high-pressure electrolysis fully supports DOE goals and objectives.
- Clearly tied to the objectives of high-pressure operation and a lower system cost.
- Definitely targeted at meeting goals and objectives of HFCIT program.
- Cost model still in the "big picture" stage -- needs detailing.
- Good progress in cost reduction efforts.
- Analysis of cost factors clearly identifies areas where improvements can be made.
- Program addresses cost reduction of capital equipment.
- Program attempting to reduce operating cost.
- All of the above are positive, but may not be enough to achieve target unless all benefits obtained.

Question 2: Approach to performing the research and development

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

- The approach is sound to start with an existing project and re-engineer to optimize cost and output pressure.
- Objectives are highly focused on those areas that have the greatest opportunity for quick success.
- One of the stated project objectives is to demonstrate an electrolyzer operating on a renewable energy source but the presentation addresses only electrolyzer issues which are all independent of the source of the electricity.
- Demonstrating improvements and innovative designs is key to achieving the objectives and understanding system trade-offs.
- Utilizes a multi-faceted approach to lower component costs and improving system performance.
- Demonstrating long-term durability and operability will be essential.
- Emphasis is on development and demonstration of a high pressure water electrolyzer.
- Focus of current work is on making incremental improvements to a prototype electrolysis system that reduce cost and complexity and improve performance.
- Program approach consistent with driving down cost.
- There may not be enough margins in cell stack cost reduction to hit cost targets, margin appears very small.

- Key to success appears to be new supported membrane approach to drop operating cost., i.e. cost of electricity.
- Data suggest operating cost may be the same, i.e., may not get reduction in operating cost.

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

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

- Good progress is being made in reducing unit costs after first arriving at an understanding of the overall economics involved.
- Good leveraging of past work and other existing work in other programs to obtain some synergy.
- Progress is steady.
- This project continues to show strong progress in achieving objectives.
- Clearly focused on achieving the technical targets and on understanding system trade-offs.
- Demonstrated cost reductions for some components and improved performance in some areas.
- Economic model developed that ties cost of hydrogen to cost of electricity.
- Seeking to improve dryer system efficiency and operability.
- The project does not report operating cost saving by going to advanced membrane equivalent to 2 mil Nafion.
- Project dependent on reducing cell stack cost, reducing part count may help, but only fractionally since individual cells will be larger and expensive materials elimination benefit not completely achieved.
- Claim to reduce area in approach, but data says they are increasing area of cell to reduce part count. Need to resolve approach.

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

This project was rated **2.6** for technology transfer and collaboration.

- Collaboration appears to not be very practical in what is largely an engineering effort.
- GM and the Center for Technology Commercialization are involved in the project.
- There is no obvious involvement of stack component developers or suppliers.
- Good partnership on component supply; could use help on the renewable integration aspects of the project.
- Not clear what the role of the two partners are; nothing was revealed in the presentation. GES seems to be a one organization shown on this project.
- Did not report what interactions were with partners. Assume there were none.

Question 5: Approach to and relevance of proposed future research

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

- Future plans are consistent with the objectives of the project toward resulting in a successful conclusion.
- Next steps are logical and consistent with the overall project objectives. Contingencies (dryer) are addressed.
- Need to benchmark the out year milestones to the DOE technical targets.
- Performance and durability testing of the total system will be necessary to evaluate success.
- More of the same -- reduce costs/complexity, increase efficiencies and performance (e.g., higher outlet pressure).
- Future work should resolve if part count is reduced by increasing cell area. This could be easily graphed to take into account additional materials of larger area cell, but reduce number of parts.

Strengths and weaknesses

Strengths
- Well thought out approach showing strong progress.
- A seemingly well-organized industrial development/demonstration project that is making respectable progress, albeit at an incremental pace.
- Good understanding by team of electrolyzer properties and design.

Weaknesses

- May be relying too much on low-cost electricity to achieve the targets. May need to push limits on system capital and operating costs to be competitive.
- Little apparent collaboration; nothing much said about exploring tie in to renewable power supply.
- Cost of electricity dominates the cost of producing hydrogen. It may be difficult to get < $0.036/kWh electricity except at specific locations

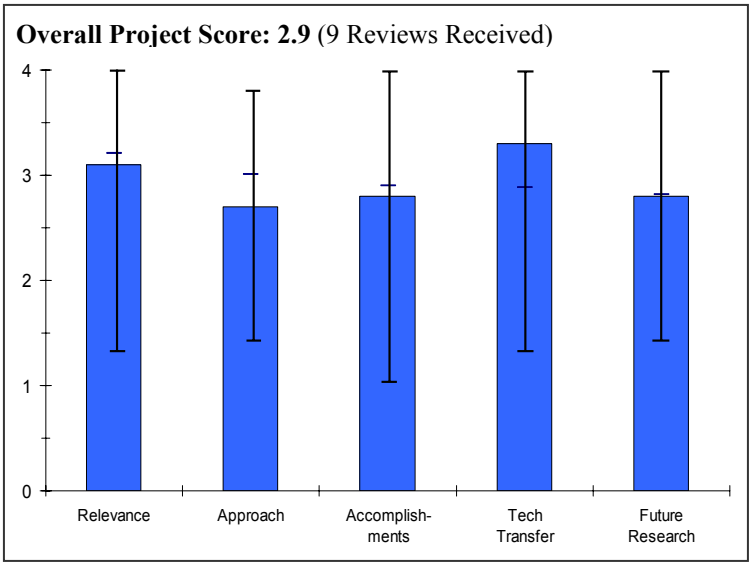
Specific recommendations and additions or deletions to the work scope

- Find a partner that can help with the dryer issues.
- For future reviews prepare a prior year to current year progress comparison to help gauge performance in the budget year.
- Program should also consider reducing cost of inefficiencies.

Project # PD-24: High Temperature Solid Oxide Electrolyzer System
Herring, Steve; Idaho National Laboratory

Brief Summary of Project

Idaho National Laboratory is currently researching and developing high and ultra-high temperature processes to produce hydrogen through chemical cycle water-splitting technology or other non-carbon-emitting technology utilizing heat from nuclear or solar sources. The project is seeking to develop energy efficient, high-temperature, regenerative solid-oxide electrolyzer cells (SOECs) for hydrogen production from steam; 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 **3.1** for its relevance to DOE objectives.

- Good mention of technical barriers.
- This project addresses the requirement for high volume production of low-cost hydrogen. A major DOE program goal.
- This could yield low-emission hydrogen production from domestic resources.
- Project aims at the development of energy-efficient, high-temperature, solid oxide electrolysis cells (SOECs) for hydrogen production from steam.
- Project will allow a higher efficiency production of hydrogen from electrolysis.
- Most aspects of the project align with the President's hydrogen vision the RD&D plan objectives. This project address issues with reducing capital costs and improving system efficiency for high temperature electrolyzers.
- A development and demonstration project focused on cost and efficiency improvements to the solid-oxide electrolyzer cell (SOEC
- The program is not addressing the quality/usefulness of the hydrogen being produced; stated purity of 90% is not suitable for fuel cells

Question 2: Approach to performing the research and development

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

- Appears well thought through. Good follow-on from 2004 to 2005. Good schematic description.
- The approach involves using nuclear reactor generated low-cost electricity to produce high volumes of hydrogen by high temperature solid-oxide electrolysis.
- The approach appropriately focuses on materials selection for critical component to achieve long-life time durability.
- Targets need to be defined for making hydrogen production via this route a competitive alternative. What cost and performance targets need to be met to make it more economical to choose hydrogen production over selling electricity to the grid?
- Project well designed, technically feasible, promoting technological integration with nuclear energy.

- The presentation does not spell out how the stated barriers are being addressed, i.e. capital cost, efficiency, electricity emissions, and electricity cost.
- The approach is generally well thought out and effective, but it was difficult to determine how the results would help achieve the DOE cost or efficiency targets.
- Development and testing of small scale SOEC stacks supported by paralleling engineering design and analysis.
- Plan is for staged development of progressively larger systems leading to a megawatt scale demonstration.
- The PI did not mention any plans or methods of overcoming the barrier of capital cost although it was listed as an area to be addressed.
- The PI stated that this is a program spanning several years into the future, yet no details of a plan to get there are apparent.

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

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

- Good progress made.
- Technical progress has been good thus far with the demonstration of a 10-cell electrolyzer stack operating at 900°C for a "short" time.
- Good modeling effort and progress in materials testing.
- Project started in early 2004, with already good achievements.
- The project appears to be well below the DOE target efficiency and there is no assessment of cost.
- The project has shown significant progress toward its objectives and to overcoming one or more technical barriers.
- Making good progress with small cell stacks; supporting some paralleling materials development.
- Since no plan/milestones have been presented as to how technical progress is being measured, it is difficult to judge the level of accomplishment
- No mention of the stated accomplishments as they pertain to the goals of the DOE program.

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

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

- Good description and acknowledgement of partners.
- Strong national laboratory team with appropriate industrial collaboration.
- Good collaboration, including industry partner.
- Good publication record.
- Project puts together an impressive number of institutions and researchers.
- Good collaboration between the partners.
- Excellent collaboration with partners.
- Clear description of the role of partners and their contributions.
- A good level of publication and data transfer.

Question 5: Approach to and relevance of proposed future research

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

- Good timeline and integration with nuclear program.
- Future plans must focus on demonstrating long-term durability at the short stack level before moving to scale-up of experimental systems.
- A more robust plan for durability testing of materials, including the effects of thermal cycling is necessary.
- Needs to undergo a technoeconomic feasibility study.
- Needs firm, quantitative out-year targets.
- Project shows a well defined timeline.

- The project is well mapped out for developing into the 200kW and 1MW demonstrations. However, there is a need for periodic assessments against goals for go/no-go on the demonstrations.
- The future plans look reasonable, but should describe how the results will help achieve cost or efficiency targets.
- Plans are consistent with project progression towards stated demonstration goals; the project appears to be on schedule
- No effective research plan was presented addressing possible decision points or contingencies, only a timeline.
- More experiments on system stability and scalability are needed.

Strengths and weaknesses

Strengths
- Well thought out and planned. Good results from work done so far.
- Strong complimentary team.
- Project objectives.
- Project approach.
- Project achievements in the short term.
- Collaborations with other institutions.
- The SOEC development is well planned.
- Approach is straight forward.
- Strong engineering design/implementation team.

Weaknesses
- Must develop a direct measurement method to monitor and verify hydrogen production in place of the "indirect" measurement currently in use.
- Given the increasing demand for low-cost, low-emission electricity, and the high cost of hydrogen delivery, it seems unlikely that hydrogen production would prove to be a competitive option over straight electricity generation and/or distributed electrolysis. A scenario and targets need to be defined where this type of centralized hydrogen production would be a viable option.
- Need to document approach to address the stated barriers.
- The devices are tested in a furnace to achieve 850 degrees. Are there Generation IV nuclear plants available to actually install this device for test today? Are there any plans to build any of these reactors?
- Need to describe how these experiments and tests will improve the technology and help the industry reach the DOE cost targets.
- Not enough information presented to gauge fully developed SOEC capital and operating costs, anticipated efficiencies, and cost of hydrogen.
- There is no detailed overall plan apparent from the presentation.
- The quality of the hydrogen being produced is not being addressed or checked.
- There is no mention of how the hydrogen will be captured at the output of the process so that it can be utilized.
- Unclear what were specific advances; how do achievements of the project compare with the state of the art
- No data on longer term performance on the system.

Specific recommendations and additions or deletions to the work scope

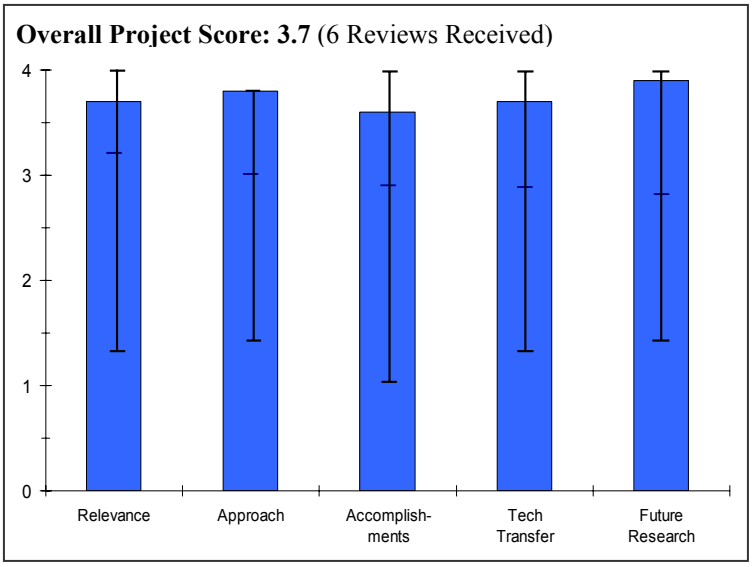
- PI could consider the possibility of a direct measurement of the hydrogen production rate, with a suitable analytical method for high temperature; the method used so far is indirect and combine electrical and dew point measurements.
- Another suggestion is to change the colors related to the electrolyte/insulator temperature contours, so that the red reflects the higher temperature zones.
- Document progress against the DOE targets as the project proceeds.
- Are there any effects of radiation from the nuclear energy or is the electrolysis sufficiently de-coupled from the nuclear energy system?
- Recommend more attention to studies that address the concern expressed above regarding cost and efficiency projections.

- For future reviews prepare a prior year to current year progress comparison to help gauge performance in the budget year.
- An analysis of the gas stream exiting the bench top process should be performed and any separation or cleanup addressed prior to any scaling up of the process.
- Add: materials stability under high temperature electrolysis conditions (corrosion, etc.).
- Add: economic and efficiency comparison of HT SOEC with IGCC (natural gas power plant) - high efficiency conventional electrolysis combo.
- Add: analysis of materials performance under the process conditions.

Project # PD-25: Renewable Electrolysis Integrated System Development and Testing
Kroposki, Ben; National Renewable Energy Laboratory

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).



Question 1: Relevance to overall DOE objectives

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

- Highly relevant to the DOE HFCIT Program.
- Good focus on integrating renewable electricity sources.
- Renewable electrolysis to produce hydrogen supports DOE's Hydrogen Program goals and objectives.
- Project goal is to develop and test a renewable electrolysis integrated system for power production and to use hydrogen as an energy storage alternative.
- The development of wind and photovoltaic (PV) combined with electrolysis systems is significant towards a hydrogen economy.
- Focused on meeting HFCIT RD&D cost and production rate goals with connection to renewable driver.
- This project is highly relevant to DOE objectives. Unlike many other projects, it is strongly engineering oriented, and addresses system problems inherent in a variety of hydrogen generation approaches. This is necessary work for the successful implementation of large-scale hydrogen generation from renewable.

Question 2: Approach to performing the research and development

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

- Very solid and comprehensive approach to tackle some difficult issues.
- The project includes analysis of integration of renewable (wind, solar) power with an electrolyzer, including assessment of when the power produced should be fed to the grid and when it should be used to generate hydrogen and system testing with PV arrays and wind turbines.
- The full system tests will help power electronics assessment and design.
- Project well planned and designed.
- Project integrates hydrogen production and the storage of that hydrogen for energy use.
- Addressing the renewable integration and the power electronics design/cost is a significant step towards making these systems practical.
- Developing roadmap for renewable-based hydrogen production

- Includes systems engineering, modeling, analysis, system integration and component development, and characterization testing and analysis.
- The approach is a reasonable mixture of modeling and small-scale demonstration. The empirical data from the demonstrations serve to both guide and validate system theory.

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

This project was rated **3.6** based on accomplishments.

- Progress is significant in each of the tasks.
- Testing of the electrolyzer with a PV array has been completed. Testing with a wind turbine has been started.
- Longer test runs should be considered to evaluate controls over wider power ranges.
- The project appears to be close to the 2005 DOE target for efficiency, but there is no cost data presented.
- Numerous encouraging technical accomplishments, particularly with respect to integration tests.
- This project is very productive for the size of its budget.
- Technical accomplishments include a variety of systems oriented demonstration-scale (several kW) experiments and a variety of modeling exercises. These impressive results suggest the robustness of hydrogen generation systems to fluctuations in current. Technical barriers to systems problems are being identified and aggressively addressed by this research.

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

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

- The project is making excellent use of many relevant resources among industry partners.
- The Team includes an electrolysis manufacturer, systems analysts, and power companies.
- Project assisted in organizing a Utility Hydrogen Workshop (Sep 2004)
- Project shows close cooperation with other institutions.
- Several well chosen collaborations; the roles of the collaborators were clearly spelled out.
- Considerable research coordination has been demonstrated with both industry and academia.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.9** for proposed future work.

- Future plans are consistent with the goals of the project and are responsive to past reviewer comments.
- Continued and expanded testing of complete systems will follow systems engineering, modeling, and analysis.
- Project well planned, the key technical barriers shall be addressed in the proper time.
- Good approach at looking at AC to AC power electronics.
- Good approach to add to data on PEM electrolyzer by testing alkaline electrolyzers in the future.
- Future plans follow logically from what has been accomplished thus far
- An expanded program with increased funding seems justified for this team -- they are getting the kind of results that can be used to build public confidence in the hydrogen economy.
- More aggressive interfacing and testing is planned for the duration of the subcontract. Future work clearly builds on past progress.

Strengths and weaknesses

Strengths
- Project goal: integration of electrolysis and wind energy.
- The approach to systems integration for making hydrogen from renewable electricity and addressing the appropriate barriers.
- Development of the power electronics.

- Well focused project; performing very well, especially considering the funding level; one of the best presentations.
- Well thought-out train of work; very straightforward sequence of research efforts; impressive empirical results; close coordination with modeling efforts; clear project schedule chart.

Weaknesses

- Technology transfer: few publications or presentations so far.
- Increased funding of this project should pay large dividends
- While there is considerable interfacing with wind machines, there appears to be only very limited interfacing with photovoltaic arrays. This is unfortunate because photovoltaics are a likely candidate for future large-scale electric supply for hydrogen generation.

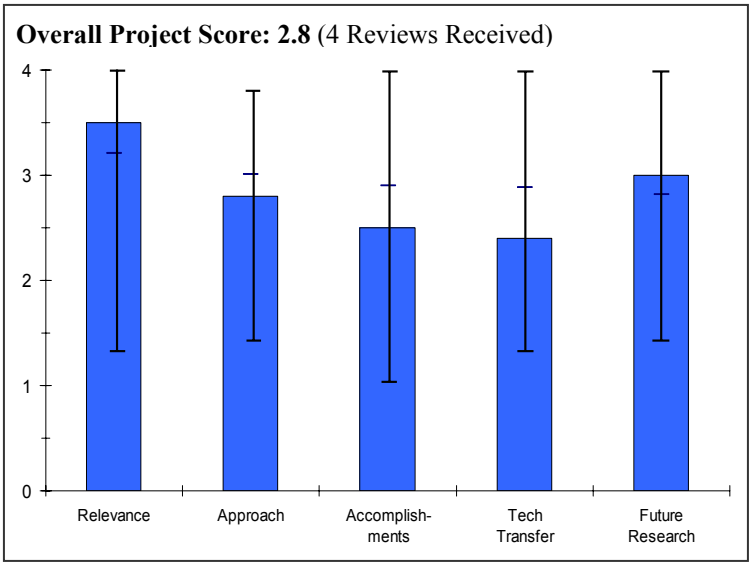
Specific recommendations and additions or deletions to the work scope

- It is suggested that the PI includes in the project scope issues related to the storage (and use) of hydrogen, at the moment the hydrogen produced is being vented.
- Focus on system costs.
- Document progress against the DOE targets as the project proceeds.
- Fund this project to the fullest, this is the kind of R&D that will get us to a deployed hydrogen economy.
- For future reviews prepare a prior year to current year progress comparison to help gauge performance in the budget year.
- In future work, increase the emphasis on photovoltaic array-based systems.

Project # PD-26: Alkaline, High Pressure Electrolysis
Ibrahim, Samir; Teledyne Energy Systems Inc.

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.



Question 1: Relevance to overall DOE objectives

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

- Water electrolysis to produce hydrogen supports DOE goals and objectives.
- Electrolysis is a key technology for the transition and long-term viability of the hydrogen economy. Reducing cost and improving efficiency will be key to market competitiveness.
- Supports HFCIT RD&D goals related to hydrogen compression, storage, dispensing, and cost.
- Emphasis on safety, manufacturing economics, delivery goals, durability.

Question 2: Approach to performing the research and development

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

- Safety analysis is important.
- Use of electrolysis to 1,500 psi followed by compression to 5,000 psi is conservative.
- The approach is sound, but it is difficult to judge likelihood for success without more information on cost and performance. Should be benchmarking progress towards the DOE technical targets.
- Parts reduction will be important to manufacturability of components.
- Trade-off studies, safety analysis, modeling
- Eventually fabricate and test a deliverable high pressure, alkaline electrolysis unit.
- Approach identifies top level problems but does not give details of how program will proceed.
- Approach does not identify major cost features and does not identify what are the critical components to improve technology.

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

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

- Progress made in areas of system design and analysis and safety analysis. Reporting of progress was overweighed in the safety area and underweighted in system analysis.
- Need to tie trade off in high pressure operation back to total system cost, including operating and maintenance. How much does elimination of one-stage of compressions save?
- Need to revisit the conclusion to keep operating pressures < 1000 psi. Rupture disks, relief valves, etc., can enable safe operation. Decision should be tied to cost and operability.

- Not clear that any "out of box" approaches are being employed. Small, incremental improvements will not achieve the targets.
- Safety is being addressed in great detail.
- Design related trade-off studies are in progress.
- It seems that thus far close to $1M has been spent but all of it has been on paper studies -- no test results were reported.
- Systematic safety analysis is beneficial.
- Programs appears slow to develop experimental data and is emphasizing pressure control system; does not appear to be main objective.

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

This project was rated **2.4** for technology transfer and collaboration.

- Although there is teaming, the roles of the various team members play and/or their expertise are not clear.
- There are three partnering organizations, but it's not clear how involved they have been to date.
- Partners identified but no interaction with other projects reported.
- Hardware trade study not reported.

Question 5: Approach to and relevance of proposed future research

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

- Planned work follows logically.
- Plans are good, but need to focus on achieving the system cost targets.
- Testing, component optimization, and more design work in the remainder of FY 2005
- Fabrication and delivery of a working prototype in FY 2006 -- they have much to do in the next 18 months.
- Program does not build on past progress because trade studies not done or not reported.

Strengths and weaknesses

Strengths
- This is a team with a long history in electrolysis and a strong understanding of the technology.
- Employing strong approaches to reviewing safety and performance.
- This particular project has put considerable effort into the analysis of safety/system control issues--the results/findings have broad implication and application in the HFCIT production arena of projects.
- Good effort on safety analysis.

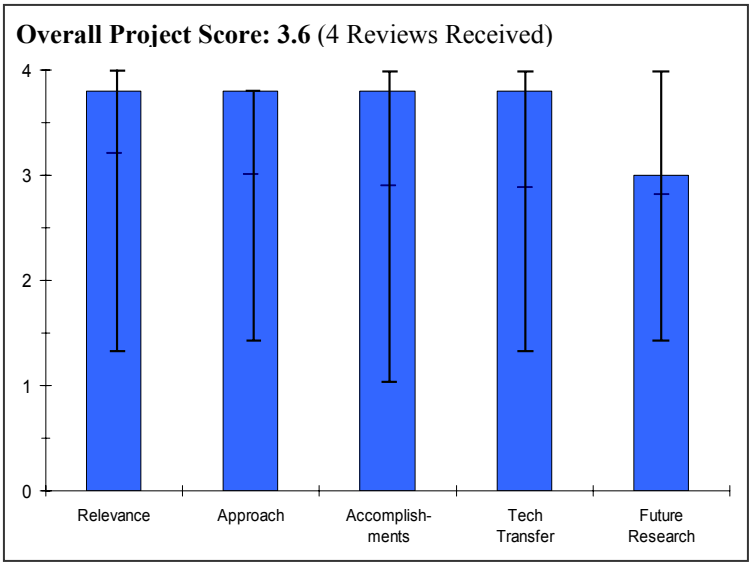
Weaknesses
- Need to pay more attention to the total system costs and trade-offs, including balance of plant.
- They don't seem to be off to a fast start, other than the safety analyses, with little to get enthused about.
- Program not reporting information.

Specific recommendations and additions or deletions to the work scope

- The pace of actual testing, component optimization and manufacture/assembly needs to be stepped up.
- For future reviews prepare a prior year to current year progress comparison to help gauge performance in the budget year.
- Program should be required to report more data.

Project # PD-27: Sulfur-Iodine Thermochemical Cycle
Pickard, Paul; Idaho National Laboratory/Sandia National Laboratories

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 design, construct and test the three major reaction sections: the prime (Bunsen) reaction, the HI decomposition section and the H_2SO_4 decomposition section. Key issues being addressed include materials of construction for the high temperature corrosive environment, thermochemistry of the HI distillation steps and heat exchanger decomposer design for efficient heat transfer. The three reaction sections are being designed with the goal of performing an integrated lab scale experiment on the full S-I cycle.

Question 1: Relevance to overall DOE objectives

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

- Key element of Nuclear Hydrogen Initiative.
- Nuclear power production is potentially a very important source of heat for hydrogen production.
- High relevance project.
- Critical to understanding viability of "nuclear hydrogen" option.
- This is definitely a relevant project and different from others that is being done.

Question 2: Approach to performing the research and development

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

- Flow sheet analysis and lab experiments on individual process sections, then separate processes will be integrated.
- Challenges and issues are addressed in the approach.
- Approach is well understood and articulated by the PI.
- The project addresses key issues of the thermochemical cycle: materials suitability, thermodynamics, heat integration, and process modeling.
- It is nice to see experiments started and additional experiments being planned for S-I – its about time!
- The three reactions of the cycle appear to be equally focused.
- One uncertain issue is how design heuristics are handled in the PFD and simulations. For example, proper design protocol calls for a heat exchanger approach temperature of 10 to 20 deg C and a pressure drop through a heat exchanger of 1.5 to 2 psi. This results in a decreased efficiency and could be significant for complicated processes such as this that have a large number of unit operations.

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

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

- Flow sheet analysis done.
- Analyzed options, selected best ones (more efficient, lower cost) for section construction.
- Have completed tests of some sections, e.g., sulfuric acid.
- Good progress on process selection; process simulation.
- Innovative approaches to designing the key process modules.
- Evaluating catalysts and how they actually behave is important for the H_2SO_4 dissociation reaction.
- Appears to be on track with accomplishments.

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

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

- Matching effort from French CEA.
- Work of other projects was mentioned during PI's presentation.
- PI referred to later presentations, conveying impression that activities across projects are coordinated.
- Outstanding level of collaboration amongst multiple institutions.
- Excellent division of labor between partners; effort seems to be well integrated.
- Closer collaboration with materials fabricators recommended.
- Appears to be well aligned with partners.

Question 5: Approach to and relevance of proposed future research

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

- General future work plans were presented.
- Would be improved with more specific information and indication of funding resources required to do the work.
- On-going economic analysis not included (based on materials and design selections).
- Did not specifically mention iodine recycle rates and the impact of the kg iodine/kg hydrogen. At one time, this ratio was 6,000 kg iodine/kg hydrogen. Supposedly work by the French partner indicates a 5 fold reduction in iodine flow. Is 1,000 kg iodine handling per 1 kg hydrogen really sufficient? This needs to be a major initiative – continuing to find ways to reduce iodine handling.
- Continuing on experimentation is good.

Strengths and weaknesses

Strengths
- Excellent technical approach.
- Work is being accomplished by a well-qualified team with significant expertise.
- The team is tracking related and synergistic work at other organizations -- national labs, universities, industry.
- Completeness of the analysis.
- Thorough selection of technologies for process steps.
- Addressing materials compatibility issue early.
- Experimentally based approach targeting specific concerns.

Weaknesses
- Ideally, results of work on other projects -- e.g., materials analysis, characterization and selection (from UNLV, INL) -- would be completed and fed into this project before major funds are spent on building and testing an integrated system.
- Corrosion work should be rapidly moving to detailed studies of corrosion mechanism and understanding of corrosion protection mechanisms.

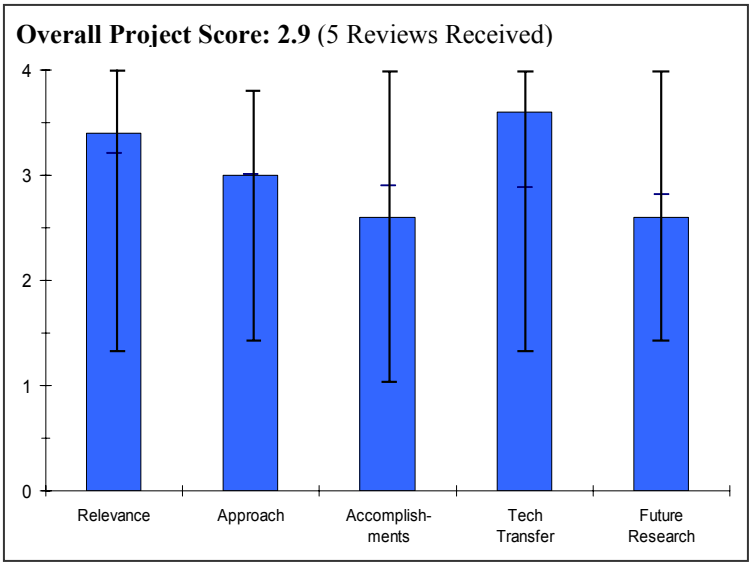
- Use of ceramics in large scale hydrogen reactors is questionable (risk of shattering; difficulty manufacturing).
- Lack of go/no-go point in the project timeline in case if suitable materials cannot be found.
- Identify iodine recycle and handling as an identified issue to be addressed.

Specific recommendations and additions or deletions to the work scope

- Be sure that PFD simulations and economics are based on real heuristics that include 1.5 to 2 psi pressure drops on heat exchangers and approach temperatures of 10 to 20 deg C.
- Be sure significant efforts are directed towards reducing the iodine handling and recycle issues relative to hydrogen.

Project # PD-28: Development of Solar-Powered Thermochemical Production of Hydrogen from Water
Perret, Bob; University of Nevada

Brief Summary of Project



The goal of this solar hydrogen generation research (SHGR) team project, led by the University of Nevada Las Vegas Research Foundation, is to define economically feasible concepts for solar-powered production of hydrogen from water. Task I objectives are to screen and select thermochemical cycles and systems; establish a thermochemical water-splitting cycle database; develop solar receiver/reactor design concepts for top cycles; and analyze and select the best systems for development. In Task II, the team will build on earlier CU/NREL work to study metal oxide reduction cycles, design an improved ultra-high temperature solar thermal receiver/reactor, and conduct fundamental kinetics and feasibility studies using the CU porous wall tube reactor coupled to the CU electric furnace and the NREL high-flux solar furnace. Task III objectives are to develop advanced multi-band gap materials and advanced materials for photoelectrodes immersed in an electrolyte to achieve high current density in a durable hybrid photoelectrode configuration conceived and designed by the University of Hawaii.

Question 1: Relevance to overall DOE objectives

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

- Renewable hydrogen is consistent with DOE goals and objectives.
- This project supports the large scale production of hydrogen using a renewable source (solar).
- The program can have a big impact on future hydrogen production but actual processes and overall efficiencies have not yet been determined.
- Project very relevant to hydrogen fuel initiative. Proposes an alternative to PV-electrolysis for solar hydrogen.

Question 2: Approach to performing the research and development

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

- Although very little experimental data has been generated, the approach of analyzing and ranking possible cycles provides insight into viable approaches and could reduce consideration of "wrong" cycles.
- The approach reviews and evaluates, based on the reported literature, existing solar cycles. Approximately two years has been spent on this activity.
- Is there a faster way to accomplish the screening of candidate cycles? The step-wise process of determining cycle efficiency potential and then determining whether the process can actually produce hydrogen seems time-consuming.
- Need to establish a cut-off for the screening of candidate cycles and focus in on conducting experimental studies of the cycle classes that show the highest potential.
- Do not see any economic analysis as part of the approach.
- Too much of the study has been spent on "paper study" as reported by previous reviewers.
- Excellent integration of detailed analysis of previous work with selective experiments to test theoretical premises.

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

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

- Possible cycles have been selected and detailed analysis and minor experimentation have been initiated.
- Progress appears to be very slow, after two years experimental assessments are just beginning on a number of cycles to identify the best solar cycle.
- The project has evaluated a large number of candidate cycles and has developed an extensive database.
- Little experimental knowledge has been generated.
- Process down-select procedures should have been expedited.
- Very good narrow-down of all potential thermochemical cycles.
- Experimental results limited so far.

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

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

- The team's expertise is very broad.
- A large team of academic, national laboratory, and industry partners is working on this project.
- Industry partners are listed, but it is unclear what collaboration is actually occurring
- The project has a good team.
- Excellent collaborative effort between multiple groups. Tasks are well split, and work is well coordinated.

Question 5: Approach to and relevance of proposed future research

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

- Future work should be accelerated to produce new experimental results to determine viability of this technical approach as soon as possible, rather than continued evaluation of multiple cycles. Focus should be concentrated on a single down-selected cycle.
- Need to establish go/no-go decisions and dates for down-selects.
- Future plan appears sound.
- Issue of materials selection for the reactor not addressed.
- Appropriate attention paid to the subject of system efficiency.
- An intermediate cost estimate based on current knowledge would be helpful.

Strengths and weaknesses

Strengths
- A strong, well qualified team to address the proposed concept.
- Good experienced team facing a large and difficult problem.
- Excellent integration of theoretical and experimental efforts.

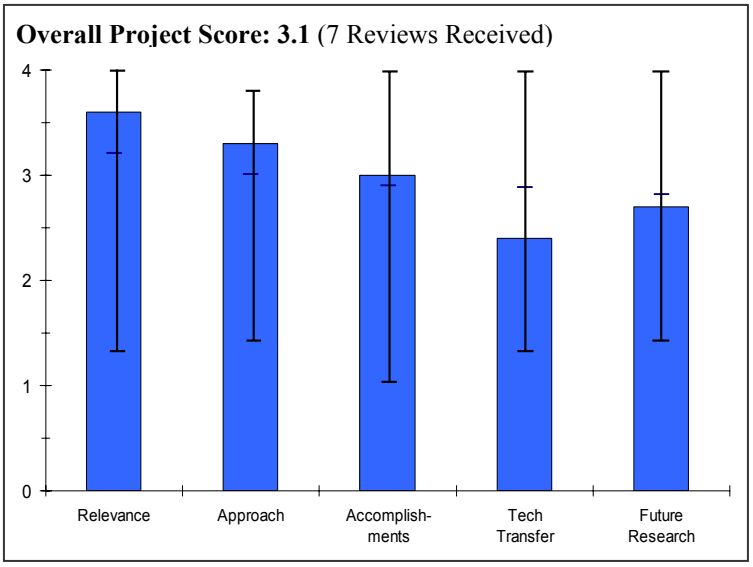
Weaknesses
- Slow progress.
- Too much effort being expended on evaluation.
- Issue of materials of construction not adequately addressed.

Specific recommendations and additions or deletions to the work scope

- Accelerate schedule.
- Recommend completion of paper study and downselect of best candidate processes so that economic comparison can be made to see if the proposed processes have any potential for producing large quantities of hydrogen meeting the DOE targets.
- Additional economic analysis would be helpful at this point.
- More in-depth study of materials suitable for reactor construction.

Project # PD-29: Alternative Thermochemical Cycle Evaluation
Lewis, Michele; Argonne National Laboratory

Brief Summary of Project



This Argonne National Laboratory project is concerned with identifying and evaluating potentially promising thermochemical cycles for producing hydrogen with efficiencies near that of the Nuclear Hydrogen Initiative (NHI) baseline cycles. The project consists of two phases: identification of cycles with particularly promising features such as lower temperature requirements, potentially high efficiencies, compatibility with current materials, and/or the promise of low capital costs; and evaluation using a to-be-developed scoping methodology based primarily on thermodynamic data and, to a limited extent, current engineering methods. The results of this updated study will be used to select the most promising alternative thermochemical cycles for the NHI program.

Question 1: Relevance to overall DOE objectives

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

- The project studies the nuclear production of hydrogen.
- A valuable low cost complement to high cost projects that comprise the Nuclear Hydrogen Initiative.
- Knowledge gained in this project will be applicable to not only nuclear-based hydrogen production but also solar-based hydrogen production.
- Directly addresses DOE program goal of H_2 production from non-fossil fuel sources.
- Good relevance - it is important to find less chemically aggressive cycles than S-I and Ca-Br.
- An identification of thermochemical cycles to be further investigated is important.

Question 2: Approach to performing the research and development

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

- A comprehensive survey was done, which indicated 4 major cycles to be studied.
- Thermochemical cycles other than sulfur-iodine and sulfur-bromine are being considered and analyzed with this project.
- Literature search and paper study are reasonable for the resources devoted to this project.
- Flow sheet analyses will be done on most promising cycles.
- Clear documentation and communication of methodology should be given sufficient (more) attention.
- Technical barriers are recognized and addressed.
- Excellent approach in starting with literature review and not reinventing the wheel.
- Great that researcher has access to proprietary information that can be "included" in assessment. Shows that this is not just a literature review of past work.
- Identification of inconsistencies in efficiency calculations should cause researchers to re-evaluate cycles identified in literature as promising.
- Proposed standard efficiency calculation is a valuable addition to this research area.
- Key objective of this work should be the one listed near the end on slide 5, i.e., identify critical R&D needs for selected cycles.

- Not clear when future work items are planned. First slide shows that this project ends at the end of this fiscal year.
- Approach is literature search to identify potential cycles quantify energetic where possible, and develop objective criteria for evaluating and ranking the cycles.
- Considering the white paper nature of the project the approach is adequate -- potentially leading to identification of new thermochemical cycles.
- Critical view of previous literature reports is commendable.
- There seemed to be some overlap with the solar thermal cycle evaluation, although this is a nuclear application. Nonetheless, the chemical side of the cycles being evaluated is the same.

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

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

- Project well documented and well described.
- Cycles of greatest interest, based on reasonable factors such as process efficiencies, have been selected.
- Completeness of information is limited by non-availability of proprietary data (presumably not in PI control).
- Appropriate caution in interpretation of data, such as idealized efficiencies, discussed by PI.
- New project.
- Excellent literature search.
- Excellent finding that efficiencies of cycles are determined using inconsistent approaches.
- Part of progress should be guidelines for R&D (not just paper assessment of chemical viability). Is this the extent to which you plan on making R&D recommendations?
- Identifying four cycles for further study is good.
- It is unclear what kind of statistical analysis was used, if any, to say one process is better than another with a certain degree of confidence.

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

This project was rated **2.4** for technology transfer and collaboration.

- Project presents good interaction with other institutions.
- Two International Nuclear Energy Research Initiative partnerships.
- Some collaboration with French CEA.
- Project could be improved with more robust coordination, which would require more funding and staff resources.
- Collaboration with industry and universities is lacking. The proprietary nature of the work is a barrier to open collaboration.
- Don't know what CEA or INERI are – not explained well. To an audience unfamiliar with your acronyms, recommend detailing partners' capabilities.
- Who will you be providing R&D guidance to?
- Who will you be providing data from this study to?
- Some collaboration in place with CEA. However, it was apparent from the session that several other groups in U.S. are doing similar work and it would make a lot of sense for these groups to pool their efforts.
- Seemed okay, however, the suggestion at the end that this work be combined and included with the big solar data base analysis is good.

Question 5: Approach to and relevance of proposed future research

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

- Details on future work plans and resource requirements were not covered.
- Excellent and needed start.
- A timeline for proposed work would be nice.
- What is meant by "Integrated Demonstration" on slide 24? This seems like a very ambitious addition to the existing project.
- Insufficient discussion of potential future developments.

Strengths and weaknesses

Strengths
- Project goals: studies on the hydrogen production from nuclear energy.
- Technology transfer: collaboration with other institutions.
- Project approach.
- Low cost.
- Type of work that's important as a check on high cost technology R&D.
- Reasonable figures of merit for identifying promising cycles.
- Good critical survey of reported results.
- Useful tool for future work on thermochemical cycle development.

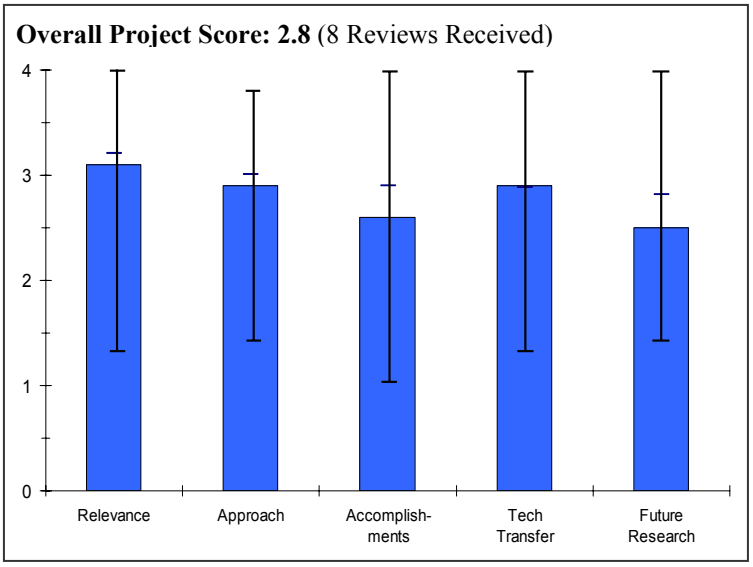
Weaknesses
- Insufficient collaboration, communication of methodology and comparing results with others doing similar work.
- Too many acronyms not familiar to the Hydrogen Program.
- Possible duplicative efforts in this area include Sandia (see PD-27), University of Colorado (PDP-44), and UNLV (PD-28).
- Issue of materials compatibility not addressed.
- Economic aspects are not addressed.
- It seems like a lot of what was done was repetitive with the solar program lower temperature cycles.

Specific recommendations and additions or deletions to the work scope

- If this work is continued, should add specific activity to seek out other organizations that have accomplished similar analyses and developed priority lists for thermochemical cycles, e.g., NREL.
- Make recommendations on future developments. Recommend the select cycles for experimental verification and identify key characteristics to be studies.
- Combine data base with that of solar project instead of a separate Excel file for this.

Project # PD-30: High Temperature Heat Exchanger Development
Hechanova, Tony; University of Nevada, Las Vegas

Brief Summary of Project



This University of Nevada, Las Vegas Research Foundation project will perform research and development of high temperature components involved in thermochemical separation and high temperature electrolysis of water to provide technical information on candidate materials, working fluids, and components to inform decision-makers in their selection of concepts to demonstrate. Candidate working fluids (gases and liquid salts) and materials will be identified and tested for their performance at high temperatures (up to 1,000ºC) and resistance to corrosive environments (such as sulfuric acid and hydrogen iodide) encountered in sulfur-iodine and other cycles. Efforts will also be conducted in component design, fabrication, and performance of metallic alloys, ceramics, membranes, and catalysts.

Question 1: Relevance to overall DOE objectives

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

- The project should be put in perspective with the quantitative metrics in the HFCIT R&D plan targets so that progress can be evaluated.
- The project consists of a set of sub-projects, whose objectives is the development of high temperature heat exchangers, crucial to the production of hydrogen from nuclear energy.
- Important technical information may be provided by the project.
- Contributions will be indirect, i.e., technical results will benefit other projects that directly address/achieve DOE's goals.
- Project not structured in a way to maximize potential for relevant results.
- The program can have a big impact on future hydrogen production and supports the Nuclear Hydrogen Initiative in NE.
- Presenter discussed the barriers from the Nuclear Hydrogen Initiative R&D Plan.
- The project supports a relatively narrow segment of the President's Initiative. Considering the funding level, a broader scope would be expected.
- The project is indeed extremely relevant.
- Presentation did not demonstrate relevance.
- Presentation did not identify the specific problems the group is facing in heat exchanger development; rather presentation reported group activities.
- Very relevant assuming S-I is relevant.

Question 2: Approach to performing the research and development

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

- This is a large project with 6 activities and many materials. It is not clear how the budget is distributed among the partners. Barriers for each element should be addressed.
- Impressive and ambitious set of projects, focused on the key technical barriers of nuclear hydrogen.

- Seven distinct work packages, for the most part, are independent of the others.
- Materials testing, test designs, coupon tests, standard test equipment are routine, not leading edge or advanced.
- This project has many partners and very broad scope from analysis to design to prototype fabrication. It may need to be reviewed by smaller subsets.
- Very empirical approach. More focused experimental program is needed.
- Much of the basic work could be performed by private labs at lower costs and with higher quality of results.
- Low temperature testing does not provide much useful info, tests should be conducted under conditions similar to reaction conditions.
- Most tests are very short term.
- Approach is unfocused.
- Approach did not identify the specific corrosion related problems.
- Approach involves unfocused tests, e.g. why does measurement of strength of SiC composites matters in heat exchangers? Creep is the problem and not strength.
- What is the criterion by which SiC composites have been chosen for such high temperature application? Do not assume that internal material interfaces in such a composite shall remain intact during operation.
- Numerical simulations for stress analysis were performed but what was the purpose? Stress analysis alone does not provide failure assessment and predictions. There was a stress of 10 MPa listed as an upper bound, but no justification was given. Why does such a number has any importance whatsoever?
- There are a large number of materials considered-- super alloys, ceramics etc. The question is what the criteria are. Material selection seems to be random, without using any operational relevance information, such as toughness or creep resistance requirements.
- It was not clear that an ASTM standard was used in the coupon tests or not and if a control coupon was used. If such standards were used, they need to be identified so the audience buys into the corrosion research.
- Fabricated SiC coupons will perform based on their method of manufacture. Purity and density will play a role. Instead of using these types of coupons, recommend using pieces of actual product (i.e. sections cut from SiC tubes) that meets product quality specs by a manufacturer. This would help to eliminate variability in the experiments.

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

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

- UNLV-Roy: Seven materials are to be evaluated but no results are presented from the analyses that are to be used in the project. The ties to the GA work should be reported in detail.
- GA: Only pictures on the coupons are presented and conditions of the test and relevance to eventual application are not given.
- MIT: No results are shown. No rationale for approach is given, especially cost analysis.
- UCB: Goals and results are well presented.
- Ceramatec: Goals and results are will presented.
- UNLV- Chen: important program element, but needs to be integrated with experimental results and targets.
- The individual projects present relevant achievements.
- Listings of technical accomplishments provided.
- Little or no linkage of accomplishments with potential affects on hydrogen production using nuclear thermochemical cycles, e.g., how might results of this project affect process designs?
- Good progress appears to be made by many collaborators.
- Results of materials testing for ID05SS12 unimpressive.
- Relevant temperature range not tested.
- Test time not sufficient (100 hrs vs. 40,000 hrs needed) (SS12 and SS15).
- Little practical use from adding 30 wt% Pt to structural materials (SS15).
- Very interesting proposed solution from Ceramatec.
- Overall progress is minor, much greater progress would be expected considering project's 18 months duration and funding level.

- Unfortunately, from the presentation, one cannot infer whether there was even a single result reflecting a systematic study toward material selection, operation, and failure mechanisms. Perhaps this is due to the fact that project is new.
- So far, the project's approach seems to be rather empirical.
- Corrosion results are based on the color of the specimen rather than on failure of the specimens. This is rather primitive. It was mentioned that components will be sent to GA for testing, but no details were mentioned on what the objectives are, e.g. whether the plan is to look for fracture of aestivation films, or look for creep strains ahead of a crack, or determine such parameters as threshold fracture resistance, etc.
- Good for a project still in the beginning state, for the most part.

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

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

- This is a large team and obviously working together, which should be increased in the future.
- Project itself has a number of participants.
- Interaction with Oak Ridge National Lab was mentioned -- a plus.
- PIs working on related projects evidently coordinate with those working on this project, and are counting on its results.
- The project has a good team.
- Quarterly meetings and weekly communication.
- Good student interactions.
- Need to work more closely (and sooner) to determine why results from different partners are not consistent or contradict.
- Even though many parties are involved, work does not seem to be divided logically between many of them.
- Some of the duplication of skills and testing could be eliminated.
- It seems that the group is uncoordinated and each section proceeds independently of the others.
- Seems like there was good collaboration among the parties.

Question 5: Approach to and relevance of proposed future research

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

- UNLV- Roy: No discussion of next steps.
- GA: Not clear that testing plans approach reaction conditions. Will accelerated exposure testing be done, as well as ASTM tests?
- MIT: Not clear how this work is to be integrated with other partners.
- UCB: Moving toward practical system development.
- Ceramatec: Moving toward practical system development.
- UNLV-Chen: Good plans.
- Continued funding is expected, but evidence of specific milestones, directions was not included.
- In general, future seems to be more of the same.
- The overall plan shows the program continuing to FY08 but most of the individual future plans only show work through FY06.
- Individual work packages seem to be well-planned.
- The overall integration/linkage is less clear.
- Some of the work seems to be simply testing lots of materials without clear direction.
- I believe the project needs major reconsideration. The PIs need to get together and lay out a plan of objectives, problem identification, plan of approach, and communication program.
- They are on the right track.
- It is unclear why a standard shell and SiC tube heat exchanger will not work, recognizing that the MEMS type will have improved heat exchange.

Strengths and weaknesses

Strengths

- Work at UC Berkeley and Ceramatec is targeted and showing good results. CFD work at UNLV is showing good results.
- Project relevance to the DOE Hydrogen Program.
- The PI presents adequate experience for the success of the project.
- Good experienced team facing a large and difficult problem.
- No strengths in this project, as was presented, apart from its extreme relevance to the President's Initiative.
- It seems to be a well integrated group that involves industry, academia, and government labs.

Weaknesses

- The presentation had to cover too much work in too few slides and time, so it may be that the UNLV and GA experimental work is under reported. Also, budget for the individual activities were not given so it is difficult to evaluate the level of effort.
- Suggest that the UNLV and GA work be performed and reported in the context of the target process conditions and that accelerated aging tests are done.
- The HFCIT program metrics should be used as guidelines since they are quantitative and have a time line for implementation.
- No information on how much funding is accounted for by each major project participants.
- Funding resources for project PD-31 seem to cover some of the same work as this project. Clarification of the relative responsibilities between these two projects would be helpful.
- Too broad of a program for this type of review.
- Project is too empirical.
- Distribution of work between different labs does not seem thought out.
- Relatively little payback considering cost and project length.
- Focus on S-I process is high risk. What if another process is chosen by DOE?
- As is mentioned above, the project lacks coordination. Materials are chosen randomly and there are no criteria to suggest candidate materials of relevance? For instance, the PIs need to ask questions of the sort: is creep an important mechanism for failure? If yes, they need to suggest why chosen materials x, y, or z are appropriate, and then proceed with testing and modeling. Strength should not be a choice criterion.
- Modeling, e.g. stress analysis, reflects no failure mechanism approach. The way stress analysis was presented was just a numerical exercise with no specific purpose whatsoever.

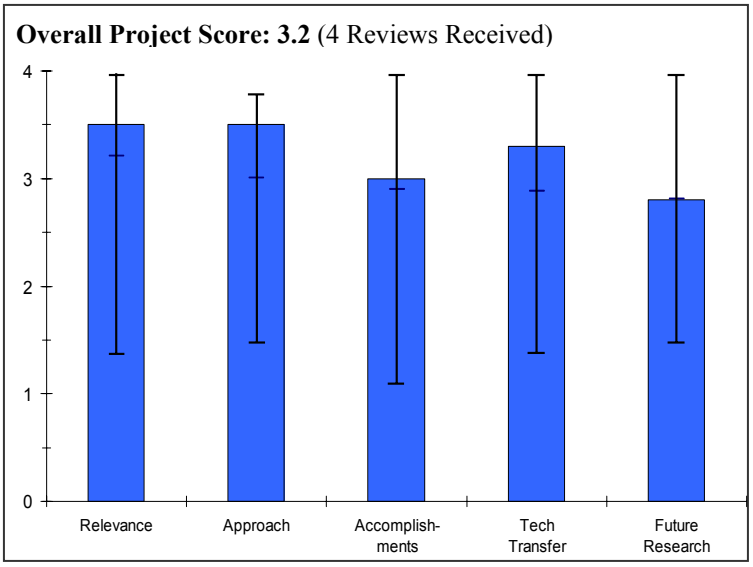
Specific recommendations and additions or deletions to the work scope

- High project costs demand good budget management.
- Recommend program undergo separate reviews on each separate task, and that this be done at NE.
- Much of the materials testing work (especially the lower temperature part) could be outsourced to a contractor with experience in materials testing. This would most likely save time and money.
- More emphasis on testing under reaction conditions.
- Look at processes other than S-I.
- Project needs refocusing. The PIs need to state clearly their approach, objectives, and plan of action. It seems that everything so far is taking place in a very empirical way. There is no road map toward problem identification and proposed problem solutions. There are no criteria of operation and related material failure concerns, apart from corrosion issues. It should also be remembered that corrosion is related to how a component is loaded. Will proposed corrosion tests to be done at GA reflect the specific heat exchanger operation conditions?
- Verify use of ASTM standardized test methods.
- Contact Saint Gobain about their off the shelf SiC heat exchangers and test specific materials (SiC) in this program.
- Look at lifetime studies, particularly for any CVD SiC films which tend to wear off.

Project # PD-31: NHI System Interface and Support Systems
Sherman, Steve; Idaho National Laboratory

Brief Summary of Project

The Interface with Nuclear Reactor project, otherwise known as the Systems Interface and Support Systems area within the DOE's 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.



Question 1: Relevance to overall DOE objectives

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

- Does not describe how goals and milestones in the Hydrogen Posture Plan will be accomplished.
- Project aims to develop a high-temperature heat transfer network to enable the linkage of a high temperature nuclear reactor to a nuclear hydrogen production plant, and thus is critical to the Hydrogen Fuel Initiative.
- A key contribution to future hydrogen production at nuclear plants is provided by this project.
- Appropriate integration of Nuclear Hydrogen Initiative activities is enabled by this project.
- Applicable to nuclear-based hydrogen production. While this area could be very large, it's unfortunate that researchers are not trying to find links to other hydrogen processes and technologies.

Question 2: Approach to performing the research and development

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

- No description on the options and approaches to overcome stated barrier on high temperature materials.
- Project consists of a set of sub-projects, to accomplish the linkage of a nuclear reactor to a nuclear hydrogen plant.
- Definition of other projects' activities are guided and helped by this work.
- Focus is on "system interface."
- Currently is mostly lab-scale work.
- Interesting study of process design where hydrogen plant is not directly on-site at nuclear plant.
- Excellently coordinated project between partners.
- Have not seen much mention of safety. Impacts on nuclear plant and impacts due to near-contact with nuclear materials should be mentioned.

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

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

- Doesn't address progress against stated barriers -- only analysis activity in those areas but not what was found/concluded.
- This is a very costly project. There has been significant technical accomplishments and progress so far.
- Initial study of nuclear plant/hydrogen plant spacing has been done.
- Accomplishments based on using advanced analytical and assessment tools.
- Accomplishments of UNLV (PD-30) included in presentation by this PI.
- Too many items to track. Recommend linking accomplishments to listed barriers. A timeline showing what has been accomplished would be useful.

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

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

- No indication that nuclear power industry has been contacted, consulted or otherwise involved.
- An interface between DOE and other nuclear thermochemical cycle project teams provided by INL.
- Excellent team, with good coordination.

Question 5: Approach to and relevance of proposed future research

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

- Milestones to complete design in the barriers area provides little insight/confidence that they will be overcome.
- Description of future work (FY06 and later) includes types of work being done by UNLV and associates, described in project PD-30.
- Moving to designs, balance of plant, permitting for pilot scale operations.
- Projecting large resource requirements.
- Unrealistic to talk about work to 2017 for one single project. Need more details on timeline if plan is to make significant accomplishments each year.
- Suggest a link to other aspects of hydrogen future development.
- Need better estimate of costs.

Strengths and weaknesses

Strengths
- Project relevance and approach.
- Project is vital to insuring a well-structured, comprehensive, properly integrated program to assess and develop technologies needed for eventually producing hydrogen at nuclear plants.

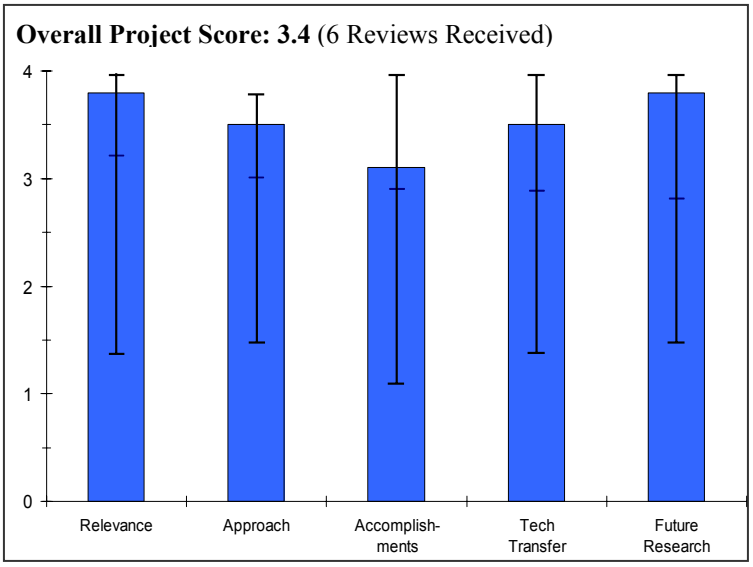
Weaknesses
- No industry involvement or even notification was indicated. Approach and future research describing milestones for completing testing, analysis, design are not an effective way to describe if barriers are going to be overcome. No discussion on scope and technical trade-offs and options. No description of the reactor/ plant interface concept.
- High project costs and long time frame make project management difficult.
- Funding resources for project PD-30 seem to cover some of the same work as this project. Clarification of the relative responsibilities between these two projects would be helpful.

Specific recommendations and additions or deletions to the work scope

- Keep good control on tasks and budged.

Project # PD-32: H2A Delivery Analysis
Mintz, Marianne; Argonne National Laboratory

Brief Summary of Project



This project, a joint collaboration of Argonne National Laboratory, National Renewable Energy Laboratory, Pacific Northwest National Laboratory and the University of California at Davis, is developing a set of Excel-based tools to examine alternative options for delivering hydrogen from centralized production facilities to stations or 'forecourts' for dispensing into hydrogen-fueled vehicles. Two tools, the Hydrogen Delivery Components Model and the Hydrogen Scenarios Model, are being developed. The Hydrogen Delivery Components Model estimates the contribution of individual major components (e.g., compressed gas tube trailers, liquid hydrogen trucks, small- and large-capacity compressors and storage tanks, pipelines, liquefiers, bulk liquid storage tanks, compressed gas and liquid terminals, and geologic storage facilities) to the delivered cost of hydrogen based on various design and scenario inputs. The Hydrogen Scenarios Model, which includes the Components model, estimates the contribution of an entire delivery pathway infrastructure to the cost of hydrogen under a particular scenario and calculates the discounted cash flow associated with that scenario.

Question 1: Relevance to overall DOE objectives

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

- DOE and others need a consistent program to normalize/validate vendors' information.
- It is important, but it is only a limited tool. Extrapolation on the results should be done (if) very carefully.
- Old studies have already show that liquid hydrogen is not the way to go. It should be removed from the beginning and use the resources in other more promising pathways.
- Project is developing analytical capabilities that are critical to assessing hydrogen production and delivery options.
- Provides needed public tool for assessment of delivery costs.
- This is the only public tool for delivery costs.

Question 2: Approach to performing the research and development

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

- Too great an emphasis on truck transport. Volumetric considerations alone demonstrate that gaseous truck transport makes no sense based on energy equivalent to gasoline. Similarly, liquid hydrogen truck transport has energy density and boil off concerns.
- Well thought out. Good progress made.
- Component model: There are technical correlations among the system components that are not addressed; for example, the production pressure with the plant storage pressure followed by the pipeline pressure and the fuel station pressure. The "generic contribution" can not be extrapolated.
- These models only cover current technology costs at the present time. Projections/analysis for future delivery technology costs need to be developed.
- Uncertainties about the scenarios and data should be quantified.

- Building on production H2A simplifies process and user training.
- Solid basis for calculations.
- Good efforts to validate and confirm.
- Scenario model useful for transition modeling

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

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

- Lots of good learning capability.
- Based on previous comments, would hope by now that both parts of the model would be available for public use. But clearly understand the delays.
- Scenario model is behind schedule.
- Components model well done.
- Scenario progress good.

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

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

- Turn over to other PIs to use -- good work here.
- Other interested parties should be involved, especially from industries.
- I think that a couple of bullets on how this fits into the bigger H2A program would have been helpful. Unfortunate that H2A questions efforts did not prompt a broader perspective response during the Q&A.
- Wide involvement of national labs, industry, and academia.
- Good effort to get buy-in from technical experts.

Question 5: Approach to and relevance of proposed future research

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

- Very good path forward planned.
- What is the relationship between H2A and Hytrans? Are there linkages?
- See comments on other topics to review the current plan.
- Is a formal process for changing default values needed?
- Tracking of changes once initial model is published?

Strengths and weaknesses

Strengths
- Good model to follow -- lots of work put into it. Good tool to use to "standardize" results of other projects.
- Sound approach.
- Transparency and simplicity.
- Large community of experts consulted.

Weaknesses
- None provided.

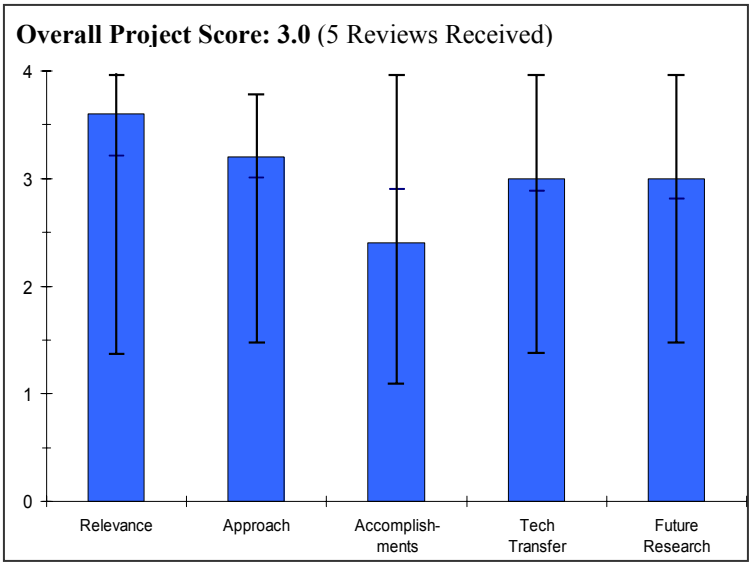
Specific recommendations and additions or deletions to the work scope

- Publish work/program, and begin to collect data.
- Formulate plan for model maintenance/modification.
- Consider carrier tab for components model.
- Consider fixed price in scenario models.

Project # PD-33: Hydrogen Permeability and Integrity of Hydrogen Delivery Pipelines
Feng, Zhili; Oak Ridge National Laboratory

Brief Summary of Project

This Oak Ridge National Laboratory project will provide a technical basis for the selection of high-strength steel pipelines for transfer of high-pressure hydrogen. The first task focuses on quantification of high- and low-pressure hydrogen permeation through base metal and weld metal regions of existing and new pipeline steels. The second task focuses on the effect of hydrogen permeation on the hydrogen embrittlement of the base metal and weld metal. In the third task, the optimization of the base metal and weld metal will be performed using the knowledge gained by the previous two tasks.



Question 1: Relevance to overall DOE objectives

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

- Good alignment of goal to provide hydrogen for high volume transportation potential.
- Investigates hydrogen embrittlement of steel pipe and welds..
- Unclear how "technical basis and guidelines to ensure integrity and safety" can be accomplished without the inclusion of a pipeline operator or regulator.
- The relevance to a hydrogen economy is clear but probably well in the future.
- The presentation leaned heavily on test chamber/safety, giving less opportunity for addressing the overall project objectives, accomplishments and prognosis.
- Pipelines cheapest delivery option.
- Hydrogen permeation is poorly understood. This project will provide insight into this.
- Understanding of relative importance of factors impacting embrittlement very important to program.
- Useful information for potential pipelining of hydrogen. Will support embrittlement work, but the embrittlement work itself will be even more useful.

Question 2: Approach to performing the research and development

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

- The information on structural integrity of hydrogen pipelines is certainly important to achieving a national network. However, given that hydrogen pipelines already exist, wouldn't much of this information already exist in some accessible data base or from the Energy and gas companies?
- Need more details on what types of mechanical tests will be performed.
- In looking ahead can the IHPV be designed to do natural gas/hydrogen mixtures?
- Addressing welding, steel impact, new steel tests with and without coatings, other materials (metals, composites, plastics?), and database of pipeline steels.
- Risk assessment -- why here, without a pipeline operator?
- The project objectives are relatively narrowly focused, which is good.
- There appears to be considerable duplication with the University of Illinois program (PDP-48).
- Determination of welding effects needed.
- Are high temperature measurements relevant to this problem?

- Need to tie permeability to embrittlement.
- The approach is consistent with the scope, but would be even better with broader scope.

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

This project was rated **2.4** based on accomplishments.

- Initial results look interesting. I look forward to getting details on why the XL65 steel performed better than the XL52. Assuming that these pieces of steel were already in use (dug up), was it simply a matter of aging as opposed to one being better for hydrogen permeability?
- How will you factor in the fact that some of the steel will be in different corrosive environments (like near the seacoast)?
- Established test units, GRNL IHPU Unit, SRNL low pressure, and identified steels for testing.
- Given future work, it doesn't seem like 60% done.
- No progress or future work on risk section.
- The work so far has validated some prior results -- not much new findings have occurred yet.
- As most buried pipelines do not suffer a wide temperature range in normal use, the value of high temperature testing is not clear.
- Design and construction progress reasonable considering the funding level.
- Work to date addresses only one of the three barriers to be addressed -- arguably the least significant barrier.

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

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

- Unclear how knowledge is being shared and received by this team with a broad spectra of interested parties. There is list of utilities that are providing materials, but will they also use the data?
- How about gas and energy companies -- will they review and comment on the information (beyond those involved in the DOE review)?
- Test equipment at labs excellent.
- Good slate of collaborators.
- Needs a pipeline operator for risk task.
- Could use refinery of ammonia partner for hydrogen management and risk assessment.
- There is an impressive list of project partners that can offer complimentary skills/knowledge -- steel, pipeline companies, welding institute
- Good selection of industrial and academic partners.
- Have an excellent collection of key collaborators.

Question 5: Approach to and relevance of proposed future research

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

- Recommend that consideration be given to gas mixtures since utilities will likely use existing and new pipelines for both if possible.
- No future work on risk? Or the steel database?
- Is the schedule too optimistic?
- Future work consists of a lot of highly technical activity of questionable value in the real world.
- Good plans for steels.
- Weld investigations important.
- Should adequately cover remaining barriers to be addressed.

Strengths and weaknesses

Strengths
- Addresses an important area that needs to be improved, be it for transportation, stationary power, or movement within plants.
- The test equipment is an excellent addition to the capacity to test hydrogen interactions with materials.
- Experimental capabilities.

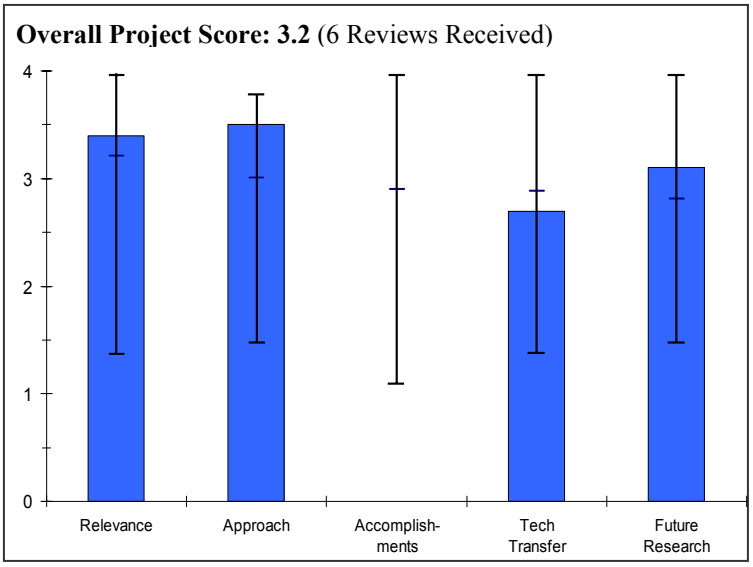
Weaknesses
- Seems behind schedule -- 60% seems optimistic.
- No status on database, management, or risk.
- How much value is there in investigating weld materials since welded areas are a relatively small part of pipeline surface area and weld metal becomes part of pipeline material anyway, i.e., no longer virgin material?
- The project should show why it has value, e.g., quantifying permeability with leakage volumes for a pipeline system and therefore degree of what is acceptable and what not.
- All this work benefits new pipeline construction but not necessarily using existing pipelines with hydrogen, that were formerly in use with another gas, e.g., natural gas.
- Need ties to other groups to measure effects of permeation.

Specific recommendations and additions or deletions to the work scope

- Need assurances that this information is not readily available from others.
- Clarify status of database, management and risk efforts. Drop if necessary.
- Needs additional collaborators for management and risk assessment, including a pipeline operator and a hydrogen intensive plant operator (petroleum refinery or ammonia plant).
- Future needs to get out of lab into real world, i.e., get to application not research.
- Do steel/pipeline manufacturers have some or a lot of this data already?
- Coordinate with other groups for embrittlement effects.
- Would be good if methods could be developed to resolve surface effects vs. bulk diffusion.
- Limit high temperature work. Focus on temperatures relevant for pipelines.

Project # PD-34: Reversible Liquid Carriers for an Integrated Production, Storage and Delivery of Hydrogen
Pez, Guido; Air Products

Brief Summary of Project



This is an Air Products-led, 3-year delivery project aimed at developing the engineering concepts to bring selected novel, reversible, liquid-phase hydrogen carrier systems into operative reality. These reversible liquid-phase carriers can be used to transport hydrogen from production sources to distribution systems where they can release hydrogen by dehydrogenation in stationary power applications or on-board for storage and vehicle fueling. The major effort will be to develop the conceptual design and initial prototype of an appropriate dehydrogenation reactor/heat exchange system for delivering hydrogen from the carrier. Additional efforts will be made to identify low-cost raw material sources and processing methods to enable economical synthesis of the carrier; to examine the requirements for incorporating a liquid carrier-based hydrogen storage device into a fuel cell power plant, and to develop a detailed conceptual design and economics for a liquid-carrier hydrogen storage and delivery infrastructure.

Question 1: Relevance to overall DOE objectives

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

- Innovative storage and delivery concept.
- This project has the potential to integrate delivery with on board storage with 6 wt% potential. However, the goals of hydrogen delivery optimization and onboard conversion are not necessarily identical. The project should have two sets of criteria for the two cases and specify the distribution of resources. This project should focus on dehydrogenation systems for fore court delivery and not for on board reforming. In that regard, the results should be benchmarked against a hydrogen delivery carbon carrier such as methanol or urea which has a significant weight fraction of hydrogen delivery potential. The technical and economic advantages over these alternative carriers should be specified.
- The fit with the Hydrogen Program unclear since on board reforming has been de-selected.
- Liquid carriers are the only delivery option that would leverage existing infrastructure.
- Potential paradigm-changing technology.
- Potential for meeting the hydrogen storage target while avoiding many infrastructure barriers.

Question 2: Approach to performing the research and development

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

- The approach is sound in identifying a potential hydrogen carrier that meets the 6 wt% requirement.
- This is a good time to get feedback from auto companies on plausibility (are there weight or material corrosivity issues under dehydrogenation temperature conditions?).
- The approach is on target but technical criteria were not given explicitly, although it is obvious that the PI has the criteria in mind. For example, vapor pressure is a key variable and was mentioned in the talk but not in the slides. The physical properties of the two forms should also be reasonably close (e.g., both liquids of similar viscosity) seems an important criteria but was not discussed. Recommend that the key variables of the system

be defined and candidate compounds screened by specific chemical and physical properties. It is not clear if there is a population of ten or a thousand compounds that should qualify for the system and be screened.
- The objectives are clear and well defined and linked with ST-14 program (material).
- How is recycle addressed in the approach activities?
- Very good mix of theoretical and experiments.
- Excellent use of theory to guide experiment.
- Approach appears sound, focusing on the fundamentals at this early stage.

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

This project just started and therefore was not scored on accomplishments, but the following reviewer observations were provided.

- If the 6 wt % has been reached (needs to be independently verified), then this is significant.
- Need more details on the properties of the carbazoles (will they add to the environment toxicity profile at a forecourt from leakage during filling/defilling? will it create waste disposal problems?).
- This is a new start but the PI presented some preliminary results which prove the concept. Suggest that the data presented in slide 10 on the packed bed results be unfolded to show the online variability or include estimates of variability such as confidence intervals. Is generation rate more steady at higher temperatures? It seems that the only reason to use a lower temperature would be for heat integration on board. This should be de-emphasized in favor of optimization of the forecourt dehydrogenation deployment scheme.
- The project has shown reasonable hydrogen production rates, at relatively reasonable temps/pressures, at least for production, but probably still insufficient for onboard use.
- Data on the recycling was thin.
- Proposed materials do not meet Program on-board storage wt% targets even as a raw material. How will this ever exceed the targets for a system?
- Good leverage off of storage project.
- Good candidates presented.
- Tailoring of dehydrogenation enthalpy is a significant achievement!
- Still very early to have any actual results at this time -- actually quite an accomplishment.

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

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

- It appears that in the future more details will be forth coming on economic and design feasibility which will be included in the H2A model. Additionally a sister project is being funded under storage so there appears to be integration and knowledge sharing.
- Would like to see more details on other stakeholders that AP will systematically engage in assessing the feasibility of their approach (auto companies, and energy companies who run forecourts).
- The collaboration is good including a university, a national lab, and industry. Appears that the partners are more focused on the on board application. This is the role of UTC. The microchannel rector seems to be emphasized because of the on board application. Is this the reactor of choice for the forecourt option?
- There is very little outside collaboration indicated.
- Good partners.
- Have potentially useful collaborators, but not clear what their contributions have been.

Question 5: Approach to and relevance of proposed future research

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

- Future work lacks details on path forward on carrier development.
- The project is off to a very good start.

- The project should show preliminary economics now, not in 2007/8 -- this might suggest a go/no-go decision need.
- Co-production of power from hydrogenation (recycle) reaction a stretch unless this is a very large scale operation.
- There is very little connection between a packed bed reactor and a microchannel type. Is the project not effectively starting again?
- Advanced reactor program right direction and good partners.
- Prototype reactor vital to define system and make decisions on technology implementation.
- Plan appears sound -- the results will tell the story.

Strengths and weaknesses

Strengths
- Sound approach with a good plan.
- This system may be more applicable to forecourt production than onboard.
- Good capabilities.
- Scheme utilizes existing infrastructure.
- Comprehensive approach.
- Interesting concept.

Weaknesses
- One of the reasons for de-selecting on board reforming was the notion of a high temp/high pressure chemical plant under the hood -- this is no different. The temperature range for this device is closer to being reasonable but still higher than existing PEM fuel cells.
- The need for two tanks is very cumbersome for an onboard application.
- Need to focus on finding carriers that meet DOE on-board storage targets.

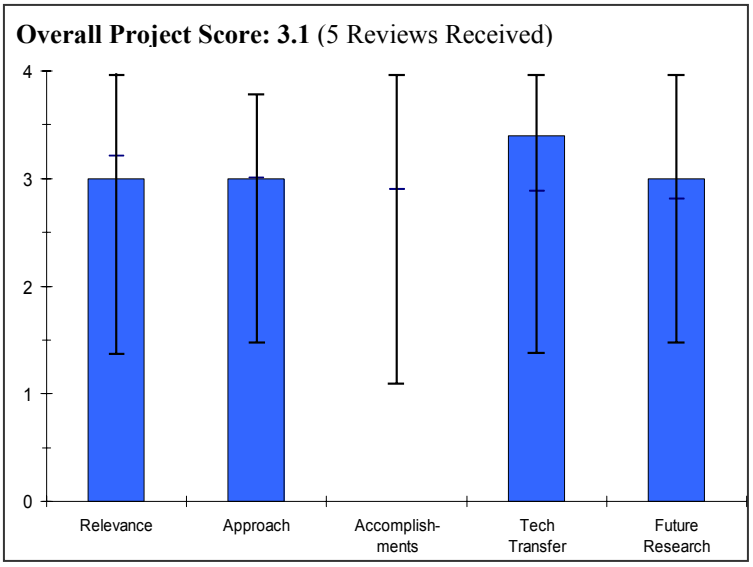
Specific recommendations and additions or deletions to the work scope

- The onboard option seems to be the favored path but the needs of the delivery area are better served by focusing on the optimization of that scenario.
- Is this practical at the scale required for a vehicle?
- How is variability of demand on a vehicle going to be addressed?
- What is the likely catalyst life/durability in reactor?
- Is the density/pumpability of liquid carrier in microchannel reactor going to be a problem?
- Does the carrier liquid present toxicity/flammability concerns?
- The liquid carrier only shows 3%, max 6.9% wt for original material -- this does not meet hydrogen onboard storage targets.
- If this project continues, there should be more emphasis on forecourt/bulk storage not onboard development.

Project # PD-35: Materials Solutions for Hydrogen Delivery in Pipelines
Das, Subodh K.; Secat, Inc.

Brief Summary of Project

This project focuses on an integrated approach to developing and testing new materials solutions to enable steel pipeline delivery of hydrogen at high pressures. The following two potential solutions to mitigating the hydrogen embrittlement of steels are being investigated: development of barrier coatings and in-situ deposition processes suitable for the protection of existing gas pipelines and newly added pipelines, and development of new alloys that have a better resistance to hydrogen embrittlement.



Question 1: Relevance to overall DOE objectives

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

- Addresses capital cost and embrittlement
- Minimizes embrittlement with coatings and pipe material and welding improvements
- Establishes baseline cost model
- Need to compare with existing H_2 pipelines
- Needs to be coordinated with PDP-48.
- Long term for new pipelines
- Good chance for impact
- Useful work to enable pipelines as a potential delivery pathway.

Question 2: Approach to performing the research and development

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

- Evaluates existing pipe under high pressure
- Finds or develops alternate alloys
- Develops coatings to minimize dissolution and penetration
- Evaluates selected coatings/alloy combinations
- Includes financial analysis
- Logical approach, tasks very well detailed.
- Why is a cost model needed when others exist already?
- The aspect of knowledge transfer to codes and standards may be valuable but probably is adequately covered by ASME
- Not sure how much existing data will be used vs. generation of new data
- Is 5000 psi testing relevant for 1000 psi pipelines?
- Coating processes must be simple/automated/cheap
- Is ABI test known to correlate well with embrittlement and other phenomena?
- Consistent with the objectives.

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

This project just started and therefore was not scored on accomplishments.

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

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

- A good mix of collaborators already established.
- Excellent group of collaborators.
- Pipeline operator, labs, materials specialists and manufacturers, and standards group.
- Hydrogen pipeline operator (industrial gas company) would be a logical additional partner here.
- Considerable partnering/cooperation is indicated but serves to illustrate overlap of efforts with other programs as most of the participants are the same entities.
- Good set of partners with applied and fundamental expertise.
- Plans call for appropriate interactions.

Question 5: Approach to and relevance of proposed future research

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

- Future research work presented as a list subject to funds availability
- These lists need prioritization. What will happen if funds are short?
- Need a Gantt chart and specific decision points.
- The project does not show a clear correlation between metallurgy, coatings, etc., and cost analysis for pipeline construction.
- Many of the future program items are very generic and self-justifying.
- Generally good. Difficult to assess at this stage.
- Consistent with objectives.

Strengths and weaknesses

Strengths
- Great collaboration potential.
- Work is essential for proliferation of hydrogen pipelines, and they would need to meet same standards as natural gas lines.
- Technical capabilities.
- Attacking relevant problem.
- Valuable work to enhance pipelines as an option.

Weaknesses
- May be under funded, and plan needs to address impact of under funding.
- Future research work presented as a list to fund based on funding availability.
- Need more details on timeline and decision points.
- All this information (except possibly the coatings aspect) is already available.
- Coating technology difficult to implement on real pipelines.
- Need to be sure experiments are relevant to real pipelines.

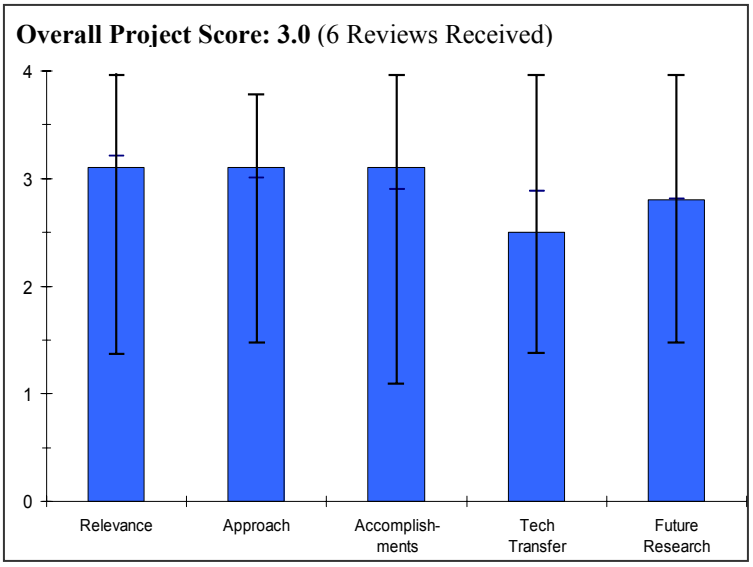
Specific recommendations and additions or deletions to the work scope

- Include industrial hydrogen pipeline operator.
- Prioritize and Gantt chart the tasks.
- This project should be stopped, or at least merged with PDP-48 and PD-33.
- The project should address how the integrity of a mill-applied coating will be maintained during construction and welding? Will it be significant if not retained?
- The project should demonstrate much more relevance and application to the 'real world' so as to justify the importance of this work.

Project # PDP-01: Enhanced Hydrogen Production Integrated with CO_2 Separation in a Single-Stage Reactor
Fan, Liang Shih; Ohio State University

Brief Summary of Project

This Ohio State University project aims at developing an integrated process to produce a pure hydrogen stream from fuel gas mixtures via coal gasification and to generate a sequestration ready CO_2 stream. The high pressure, high temperature and high purity hydrogen is obtained by incessantly driving the equilibrium-limited water gas shift reaction forward using a metal carbonate to remove the CO_2 by-product. This process uses a patented high reactivity nanoporous calcium based sorbent, whose physical properties are tailored by using surface modifiers, for concomitant carbonation reaction of the calcium oxide which reacts with CO_2.



Question 1: Relevance to overall DOE objectives

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

- Hydrogen is produced through process intensification and enhancement of the water gas shift reactor -- a key. Enhanced hydrogen production and the integration of a carbon dioxide separation membrane into a single-stage reactor are important focus areas in FE's draft "Hydrogen from Coal RD&D Plan."
- The carbon dioxide stream would be ready to be sequestered and is a key focus area of FE's draft "Hydrogen from Coal RD&D Plan."
- FE has a goal of achieving $3 per gge by 2005; however, the researchers do not know if the costs will help FE meet the goal.
- Lowers the cost of hydrogen.
- Reduces greenhouse gases (CO_2 sequestration ready).
- A cost-effective coal based hydrogen production technology is responsive to the Hydrogen Fuel Initiative and the proposed work scope is consistent with DOE Hydrogen program goals.
- The project, novel in approach, has completed 50% of the work in the development cycle. However, the results presented so far do not indicate techno-economic potential for advancing hydrogen production state-of-the-art. During review the PI informed the reviewer that he will ask for a no-cost time extension for completing the tasks, and the remaining tasks are consistent with DOE program goals.
- The project objective does not include any discussion of a development cycle leading to scale-up and commercial maturity or any success criterion and how the R&D efforts can accomplish such a goal. A list of risks and barriers and planned accomplishments to overcome key technology barriers is appropriate.
- Without additional cost information it is uncertain how the project will result in the use of hydrogen as an energy carrier in the US.
- The project describes a novel approach for enhanced hydrogen production through the in-situ removal of CO_2 from the water gas reaction products. It supports the President's Hydrogen Fuel Initiative and fits in well with the RD&D Plan objectives. Work conducted so far has focused only on maximizing CO_2 removal -- hydrogen production is a consequential (and not the primary) focus of this project.
- Work was focused on the CO_2 sorbent chemistry including carbonation and calcinations.

Question 2: Approach to performing the research and development

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

- Using calcium carbonate solvent to react with carbon dioxide appears to be an effective way to "capture" carbon dioxide from the gas stream.
- Heat generated during the shift reaction is used during carbonation to improve system efficiencies.
- The limestone industry could supply the raw material from which the solvent is manufactured potentially lowering the costs that might accrue through other methods of CO_2 capture.
- Innovative WGS and carbonation.
- Systems integration needed (H_2S cleanup, calcinations heat transfer).
- The project aims to develop a novel approach to tie up the CO_2 using CaO sorbent and thus generate hydrogen from shifted synthesis gas. The $CaCO_3$ can be decomposed, CO_2 captured, and CaO recycled to the system.
- The technology is a high temperature operation, and not compatible with coal gasifier warm gas clean-up conditions, i.e., energy is needed. WGS reaction is also proposed at high T (600ºC).
- An overall process scheme has been presented and the WGS-CO_2 capture experimental set-up described.
- The de-coupled WGS reaction CO_2 sorption approach seems novel, but its scale-up and engineering design may be very challenging.
- The main weakness of the approach is the deactivation of the WGS catalyst in presence of H_2S produced during the gasification. This can affect the overall efficiency and then it does not reduce the cost of hydrogen production.
- The approach is technically sound and logically focused toward reaching the final goal.
- No data was provided to determine if the concept of in-situ CO_2 removal from the WGS reaction does improve hydrogen production.
- Proposed work scope is narrowly focused on sorbent chemistry.

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

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

- It has been determined which temperatures carbon dioxide will react with the calcium carbonate solvent and is generally 600 to 700 degrees Celsius.
- The researchers are able to achieve equilibrium.
- Good progress on sorbent effectiveness, catalyst performance, and testing plan.
- Delays in pressure testing due to vendor equipment delay (no-cost extension to FY06).
- Sorbent surface optimization and CO conversion data using known catalysts and two sorbents have been presented. The data present CO conversion with CO_2 product removal at only early time scales and 80% CO conversion is observed for the short duration tests. The comparison of data with prediction is very insightful. Product (CO_2) removal does appear to favor improved production of hydrogen.
- As of now, the data base generated is not complete but, a stated before, the project PI will apply for a no-cost time extension.
- The extent of the deactivation of WGS catalyst was not studied yet. This can strongly affect the efficiency of the hydrogen production process;
- The cost of hydrogen production was not provided since the authors still did not measure the hydrogen purity obtained; therefore, the main technical barriers were not overcome yet.
- The project's major focus so far has been on the evaluation of sorbent characteristics to maximize CO_2 removal. Some work should have been done at the very outset to determine if hydrogen production is enhanced by this effort.
- Careful experimental work was conducted to prove several key points of the sorbent applications.

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

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

- The researchers are working with others to compare their product. For example, the researcher conducted lengthy negotiations with Toshiba to obtain data on their proprietary solvent. Boeing and GE are also potential collaborators.
- Collaboration indicated with B&W, Boeing, GE.
- All of the work thus far is done at Ohio State. During the review, the PI informed the reviewer that GE and Boeing have shown interest in the technology and Boeing is talking with Ohio State intellectual property office.
- There is no collaboration with industries or other universities. Technology transfer mechanisms were not described.
- Most of the work is done at the Ohio State University. There appear to be no outside collaborators at this point.
- Needs more interaction with industry to seek R&D inputs.

Question 5: Approach to and relevance of proposed future research

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

- The PI plans to study sintering effects and to use better equipment to test high pressure streams and hydrogen purity. The equipment that was scheduled for delivery from Germany had to be delayed due to the increase in value of the euro compared to the dollar.
- Significant future work planned for multicyclical testing (carbonation, calcinations, H_2S effects).
- The future research proposal slides were clearly explained by the PI. A test rig has been procured for high pressure tests and to determine the effect of H_2S. The description of future efforts contributing to expanding knowledge base or technical know-how was adequate.
- Study of the effect of H_2S on the catalyst activity and carbonation; hydrogen production, measuring the amount of H_2 in the exit stream; and multicyclical testing (carbonation, calcinations).
- Some effort should be devoted to determining the quality and quantity of hydrogen produced when using (and not using) the proposed concept.
- Good approach for further advancing sorbent chemistry/applications.

Strengths and weaknesses

Strengths
- The solvent developed appears to be much more reactive than other comparable solvents.
- Ambitious goals, steady progress.
- Removing a product from the WGS reactor by sorbent is an interesting approach although the product removal rate may not match the WGS reaction kinetics. The PI needs to address this issue of transport-kinetic coupling.
- The integration of water gas shift reaction and carbonation at the same reaction is an interesting approach for CO_2 sequestration.
- The project is a novel approach to remove CO_2 in situ from the WGS reaction. If successful, this would become a valuable process step to enhance hydrogen production. Future work is a logical continuation of work performed in the past.
- Good sorbent chemistry data were presented, including PCC (precipitated calcium carbonate) applications.

Weaknesses
- A major weakness is that this research does not have an economic basis. Ohio State University is requesting a one year extension to evaluate the economics of the project. FE has a goal of achieving $3 per gge by 2005 (according to the poster); however, the PIs do not know if the costs will help FE meet the goal.
- More time may be required to accomplish objectives.
- The exothermic WGS reaction system is operating at high temperature; the effect of high temperature on conversion is not discussed adequately. In some cases, high temperature-WGS is used to shift only moderately cleaned coal gas, but almost always followed by a low temperature-WGS to maximize conversion. This project does not address this issue.

- The PI needs to address that product removal is overcoming the high temperature shift operation, i.e., the lower conversion is countered by the favored eqm towards more hydrogen formation.
- Hydrogen purification strategy.
- Unclear if this additional process step improves hydrogen production or not. Bulk of the work has focused on CO_2 removal. If future work proves that hydrogen production is not improved, the project may still be continued under a carbon sequestration program.
- Discussions on the effectiveness of combines sorbent/water gas shift catalyst system were inadequate, e.g., can the system handle a CO-rich syngas from coal gasifies? Acronyms (e.g. MSB, PCC) were not explained. Proposed coal-to-hydrogen technology is complex with the deployment of sorbent.

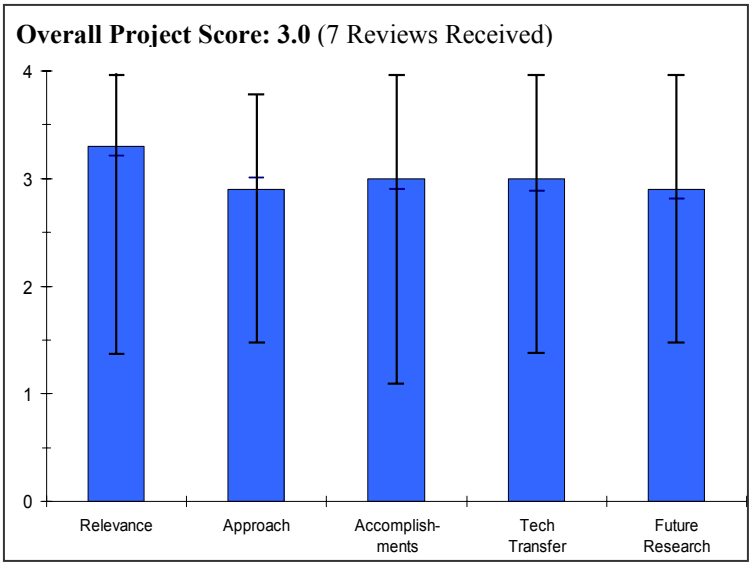
Specific recommendations and additions or deletions to the work scope

- FE should request that Ohio State promote the expensive test equipment (once it is received) to encourage other researchers to use the equipment for evaluating hydrogen quality.
- The PI should attempt to use the H_2A analysis tool during the present budget period.
- Possibly use the Aspen modeling for systems integration.
- Investigate sulfur tolerant WGS catalyst.
- The project can not be completed during the time scheduled (September 2005). There is a large amount of experiments that still have to be done. Furthermore, it is not sure that the project objectives will be reached after that. The authors attributed the delay to the difficult to acquire some of the equipments.
- Collection of data to determine if CO_2 removal during the WGS reaction does improve hydrogen production kinetics and hydrogen purity.
- Expand work on proving out the feasibility of sorbent carbonation/water gas shift (WGS) reaction system. Perform scoping economics study for the prose technology. Work on effectiveness of WGS catalyst in the presence of particulates from local gasifier.

Project # PDP-03: Integrated Ceramic Membrane System for Hydrogen Production
Schwartz, Joseph; Praxair, Inc.

Brief Summary of Project

In the current phase of this project, Praxair is focused on simplifying hydrogen production by combining the reaction and purification stages of hydrogen production into a single step in a single vessel which could significantly reduce the capital cost of producing hydrogen, Phase I analyzed and compared several different processes and, based on the likelihood of success, a two-stage process wherein each stage was comprised of a membrane reactor was selected.



Question 1: Relevance to overall DOE objectives

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

- Good concept to develop membrane. Good explanation.
- Success on this project will significantly benefit small-scale hydrogen production.
- This project supports the President's Hydrogen Fuel Initiative to accelerate the introduction of hydrogen as the primary energy carrier in the USA.
- Use of membranes for distributive hydrogen production is very essential for the success of the Hydrogen Program.
- The project is focused on small scale distributed hydrogen production.

Question 2: Approach to performing the research and development

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

- The project has a lack of focus on durability testing prior to scale up. Appears to be materials limited due to sensitivity to impurities. Metallic supports are more robust and should be evaluated as supports.
- Appears to be well designed with good progress.
- Use of previously developed support structures with application-specific membrane materials is an excellent approach.
- Excellent study of CO conversion as a function of hydrogen recovery.
- Matched thermal expansion of materials very insightful.
- In addition to effect of H_2S, suggest study of unconverted methane. Is there any chance of coking on the membrane surface, due to dissociation? CH_4 is relative stable, but with Pd at these temps, it would be worth addressing.
- Is there adsorption of CO on membrane surface?
- The project addresses the cost reduction of the hydrogen production through the development of hydrogen separation membranes integrated with WGSR.
- The three phase approach is a good plan. Phase I deals with technical and economic feasibility and therefore provides a good guide for Phase II development before system design and testing in Phase III.
- A sound step; by step approach was proposed.

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

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

- Is thermal cycling a potential issue for supports?
- More than halfway through – good that they have already incorporated lessons learned.
- Study of Pd-Ag membrane very useful to progress.
- Good experiments on hydrogen flux as a function of substrate manufacturing date. Shows keen understanding of variables that can affect performance.
- Good leveraging with other research efforts (membrane support, reactor).
- Excellent understanding of system design in last conclusion point on slide 24
- The membrane did not exhibit resistance to sulfur contamination.
- The team has a long list of barriers that have been successfully addressed: durability, impurities, flux, defects, selectivity, cost, operating conditions, etc.
- Good progress was made in substrate development and hydrogen flux.

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

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

- Have looked at DFMA partners.
- Not a large team, but seem to be making appropriate progress.
- Should discuss partnerships within Praxair that are already benefiting from the project as these are collaborations, too.
- The project involves collaboration between industry and research institute.
- Praxair is an engineering company with a good record of commercialization of similar technologies, including PSA for large hydrogen production and therefore is committed to commercializing this technology.
- Have plans to commercialize this technology upon successful completion from small-to-large scale applications.

Question 5: Approach to and relevance of proposed future research

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

- Future work lack focus on durability/reliability testing and thermal mechanical issue possibly due to ceramic support.
- Will incorporate with other DOE funded project. Good leverage with other projects.
- All plans shown on slide 23 are reasonable and necessary to achieve project success.
- Timeline given in slide 2 shows good project planning.
- The team has a good list of future activities, including continued performance improvement and integration.
- Integrated WGS/hydrogen separation operation will be conducted.

Strengths and weaknesses

Strengths
- Good progress made. Good list of future work and identification of last year's issues. Should continue work.
- Excellent project.
- The development in pilot scale of a hydrogen separation membrane with mechanical, thermal and chemical stability.
- Has pursued a sound step-by-step approach by identifying key problems and then finding solution (e.g. low cost ceramic substrate preparation).

Weaknesses

- Scale is fairly small. Good for transition, but economic pressures will be most intense during this phase of hydrogen economy development. Need study of applicable markets. Because DOE is helping to fund this work, applicable markets cannot be limited to company's current customer base.
- Approach gives little outlook on robust membrane.
- The extent of hydrogen recovery obtained by using these metallic membranes can reduce the efficiency of the process and the cost of hydrogen production.
- Quantitative experimental data presented were scarce.

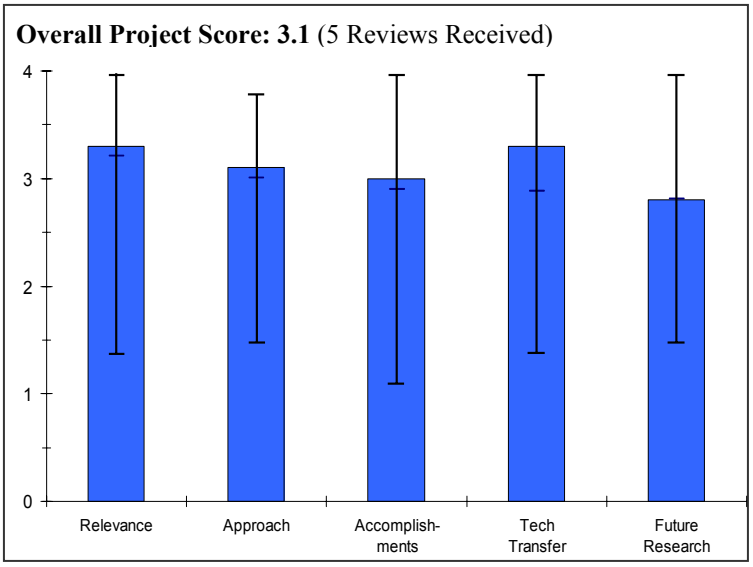
Specific recommendations and additions or deletions to the work scope

- Need a system and market study.
- The integration of membrane and WGSR has to be implemented taking into account specific catalysts. The performance of the integrated system has to be demonstrated in the presence of a real feed.
- Expand work to study the effects of H_2S on membrane performance, since biomass and coal will produce syngas with H_2S contaminant. Expand work on industrial partnership with industry on water gas shift catalyst incorporation into membrane reactor. Expand work on engineering considerations associated with membrane scale-up (e.g. seal).

Project # PDP-04: Low Cost Hydrogen Production Platform
Aaron, Tim; Praxair, Inc.

Brief Summary of Project

In this project, Praxair, Inc. is developing a low cost hydrogen production platform for the onsite generation of hydrogen. Their goal is to commercialize a cost effective, small scale, hydrogen production system using high volume packaging and design techniques. The system is based on traditional steam methane reforming (SMR) and pressure swing adsorption (PSA) process technology. The approach involves applying Design For Manufacturing and Assembly (DFMA®) techniques, as well as a high level of system design/integration, to lower the cost to produce hydrogen from small on-site plants. The program includes the construction and testing of component and complete system prototypes.



Question 1: Relevance to overall DOE objectives

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

- Good scope of work. Work linked to membrane development. Good discussion of poster.
- Project is fully relevant to achieving goals of developing low-cost hydrogen production capabilities for the transition.
- Important to validate current distributed reforming delivered cost of $3.00/gge and an overall efficiency of 65%.

Question 2: Approach to performing the research and development

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

- Good list of barriers identified.
- Sharp focus on more than one barrier.
- Component testing is a good approach.
- Is hydrogen compression and storage included in the project scope/economics? Suspect that 100-120 psi hydrogen will not be useful at a refueling station.
- Is 4.8 kg hydrogen per hour enough capacity for a typical refueling station?
- The approach relies too much on conventional technology.
- Apparent efficiency of the design appears to be low; this issue is not addressed explicitly.
- Insufficient attention paid to the issue of reliability.
- Turndown ratio of the prototype is too high for a real world refueling station.
- Start-up and shutdown robustness not explicitly addressed.
- Design decisions criteria and comparative data should be given when possible.

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

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

- Good progress made so far.
- Significant activity on addressing the DOE targets.
- Good cost analysis with documented assumptions.

- Good progress overall.
- The project needs to become data driven. It is important to document the specifics of system performance and quantify reliability by showing line charts for key variables, statistics and impact of variance of these variables on economics and efficiency numbers. Perhaps this is not the venue for so much detail, but it should be stated what the meta data needs to be to make such decisions i.e. describe the data base that will result from the testing program.
- The estimated cost for the system is a breakthrough for distributed production.

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

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

- Good list of partners identified.
- Good use of Boothroyd-Dewhurst and Diversified Manufacturing to assist in manufacturing analysis.
- Work with manufacturing companies for DFMA improvements is impressive.
- Praxair has included DFMA partners to develop the system and commercialize it. This is a significant aspect of the project.

Question 5: Approach to and relevance of proposed future research

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

- Good efforts so far. Future work looks feasible.
- System development and operation is the logical next step.
- Need to show how hydrogen compression and storage will be integrated into the system's design.
- Future plans inconclusive.
- Longer term testing should be included in the test plan as funding allows.

Strengths and weaknesses

Strengths
- Good poster explanation with nice layout.
- Manufacturing focus and addressing several barriers.
- Solid compact reformer development program.
- Sound conservative design and resulting low cost estimates for reaching HFCIT distributed production goals.

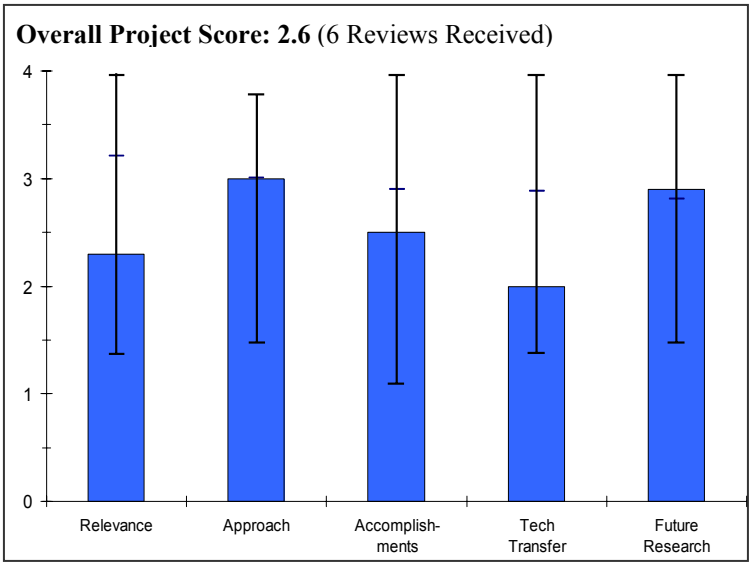
Weaknesses
- While safety has been addressed, it appears to be less focus on safety than other technical aspects of the project. One example is that the Hazop has not been completed prior to manufacturing study was done. Results of Hazop may alter the mechanical design.
- Unclear how much it adds to what we already know from the work of others (APCI, H_2Gen, GE, etc.).
- Need more information to support conclusions in economic analysis.
- Data analysis plan should be included in the project and linked to performance validation objectives.

Specific recommendations and additions or deletions to the work scope

- Consider further safety assessment including adding a third-party safety partner to the project team.
- Need to document progress against the DOE targets as the project proceeds.
- Add improved PSA performance.
- More data on reliability and start-up/shutdown testing.
- Component testing and integrated system run times should be extended to times consistent with an appropriate experimental design for reaching decision about performance.

Project # PDP-05: Solid Oxide Fuel Cell Carbon Sequestration
Bessette, Norm; NiSource Energy Technologies, Inc.

Brief Summary of Project



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 enables 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.

Question 1: Relevance to overall DOE objectives

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

- Creating affordable hydrogen fuel cells.
- Reduce greenhouse gas emissions (CO_2 sequestration ready).
- Good concept to sequester CO_2 with SOFC.
- Carbon containment is certainly desirable, but is it necessary at this point of development of SOFCs?
- SOFCs have significant materials problems -- this project just adds additional materials challenge to the mix.
- The project includes technology development that supports aspects of the hydrogen vision, but is not directly tied to achieving the objectives or overcoming the barriers outlined in the RD&D plan.
- Not clear how this links to the Hydrogen Fuel Initiative as this approach does not make hydrogen. This seems more relevant to the Future Gen Clean Coal initiative.
- Sequestration of CO_2 with a small SOFC system does not seem to be a feasible proposition.

Question 2: Approach to performing the research and development

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

- Fuel cell technical barriers addressed (stack design, materials development).
- Acumentrics has to develop their own membrane, which has not been tested/demonstrated.
- The proposed approach seems reasonable, but since the CO_2 is going to be collected why not just collect the unreacted fuel cell effluent, contain it and dispose (and/or) use it for some other purpose?
- Preventing back pressure buildup that would adversely impact SOFC performance could be a problem.
- This is a good approach for accomplishing the project objectives.
- Clearly a qualified team.
- Approach of using oxygen transfer membrane to oxidize residual fuel in SOFC anode waste gas has high likelihood of achieving technical success. However, one wonders if a different approach that captures some of the significant energy available in this stream would be of greater value. It would be helpful to see an evaluation of this option against use of an oxygen transfer membrane to produce a pure oxygen stream that

could be combined with the fuel cell anode gas and combusted in an engine where one could extract some energy from the fuel stream, and still retain the feature of producing a sequesterable CO_2 stream.
- Main issues addressed well.
- Reliability of the system (considering not a good track record of SOFCs) needs more attention.
- The system is very complex, combining a lot of complex processes. This increases reliability risk.

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

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

- Project recently started (11/2004).
- Pre-reformer used so far.
- Developed closed end tubes and double-chamber manifold.
- So far only 5% progress but appears to be on track.
- This project has just started and is only 5% complete -- this criteria should probably not be evaluated. The major challenges associated with this project have not been addressed yet.
- Slow start; only 5% complete.
- Technical progress in redesign of flow scheme, manifolding, and components of SOFC to capture and isolate the anode waste gas stream. Work has not yet started on selection and testing of the mixed oxygen ion/electron conductors needed to make this concept work.
- Would be helpful to get better understanding of the tools that will be used to characterize the oxygen transfer membranes before and after use in this application.
- Project just commenced.

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

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

- Not demonstrated.
- There was no indication of collaborations within this project. It appears to be a totally self-contained project. Apparently any resulting technology would be integrated into the presenter's existing SOFC product line.
- Appears to be some collaboration, but PI did not provide details.
- NiSource is sole collaborator.
- More in-depth collaboration with other (OTM) developers needed

Question 5: Approach to and relevance of proposed future research

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

- Electrochemical oxidizer development.
- Demonstrate full-scale and a 2000 hour endurance test.
- Given the stated objectives of the project, the plans for future work appear to be reasonable and adequate.
- Would benefit from enhancing partnership development to implement the technology into a practical system.
- Next steps in the work are clear -- design, synthesize and test oxygen membranes in this application.
- Provided that the unit can be stable in operations, the direction is good.

Strengths and weaknesses

Strengths
- Good mechanical design.
- Good concept and approach.
- Existing SOFC technology and hardware jump starts this project.
- Industry partner has strong SOFC capability.
- Potentially very high efficiency combined heat and power.

Weaknesses

- Materials development pending.
- Partners to develop components have not been identified.
- Relevance of this project is questionable given the current SOFC state-of-the-art.
- Geared towards power generation and carbon capture, not on hydrogen production for transportation applications.
- The idea of purifying and sequestering CO_2 on such a small scale.
- Poor track record of SOFC creates risks when a combination with another unproven technology (OTM) is planned.
- Unclear how this unit would be better that just a stand-alone SOFC combined heat and power.
- Unclear end use.

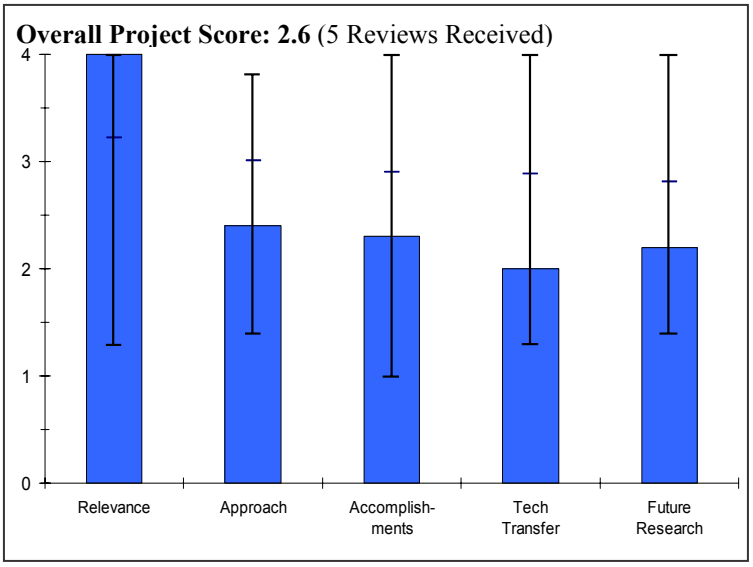
Specific recommendations and additions or deletions to the work scope

- Additional collaboration with SOFC materials developers may be beneficial.
- Eliminate CO_2 sequestration from the scope or increase the unit scale.
- Be more specific on where the technology could be applied.

Project # PDP-07: High Pressure Distributed Ethanol Reforming
Ahmed , Shabbir; Argonne National Laboratory

Brief Summary of Project

This Argonne National Laboratory project will study the feasibility of high pressure steam reforming of bio-derived liquid fuels, such as ethanol, with simultaneous removal of carbon oxides. The high operating pressure allows hydrogen purification by a variety of techniques to produce pressurized pure hydrogen for ease in storage, distribution, and dispensing. The study includes theoretical modeling and efficiency analysis of alternative process designs, experimental reaction studies to determine operating parameters and kinetic data, and investigation of potential separation/purification membranes and other approaches.



Question 1: Relevance to overall DOE objectives

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

- Good synergy with PNNL low pressure ethanol project.
- The operation of the process is quite difficult for distributed production scenarios.
- No measures of progress in the objectives.
- This is a study that could have been worked into another project perhaps.

Question 2: Approach to performing the research and development

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

- PI has looked at potential barriers of soot formation.
- Better treatment on the catalyst aspects is necessary (20% methane formation is too high).
- It should finish the basic development (to find the right catalyst) before developing the kinetics and the reactor.
- It is certain that they can develop a reformer that will efficiently convert ethanol to hydrogen, the applicability of such a process to the needs of the hydrogen future is difficult to identify.
- The fact that ethanol is a bio-derived liquid does not justify this project. Ethanol can benefit hydrogen but only at a small scale. Whole biomass is more appropriate (and less costly and more efficient) at mid and large scales.
- PI does not intend to study the economics of this process within a system context.
- Excellent idea to condense CO_2 out after WGS.
- Excellent understanding of how to balance reformer fuel requirements and ethanol conversion. PI should be funded to apply this to other reforming operations
- Needs to be integrated into other programs.

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

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

- So far, they are on track.
- Too much methane formation.
- Efficiency is far from the simulated possibility.

- While this is a newer project, literature evaluation and thermodynamics modeling should be commended. Excellent way to start a project.
- More work should be done to evaluate applicability of this process to mid and large central operations.
- High pressure process cannot be operated intermittently; PI does not understand importance of including storage.
- Not clear how this research will forward distributed hydrogen production from renewable feedstocks.

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

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

- Good synergy with PNNL.
- More cooperation with other organizations may help.
- Buying catalyst from Sud Chemie does not make them collaborators.
- Suggest contacting existing ethanol reforming companies.
- PNNL listed as a collaborator on slide 2 but not on slide 14.
- This is all government-to-government.

Question 5: Approach to and relevance of proposed future research

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

- Appears to be on track.
- It should concentrate efforts on the reformation process prior to the separation membrane and the reactor itself.
- High pressure means larger, continuous, and monitored operations. Ethanol is best suited to small-scale operations because it is easier to transport and store. Ethanol is more expensive and less energy efficient to produce and deliver than whole biomass, and since you cannot take advantage of lower hydrogen delivery costs at the larger scale, is not justified.

Strengths and weaknesses

Strengths
- Good concept -- appears to have looked at potential barriers in addition to cost and efficiency.
- Researcher has strong capabilities in process mass-optimized and energy-optimized design. Excellent understanding of energy balance between reformer and water gas shift.

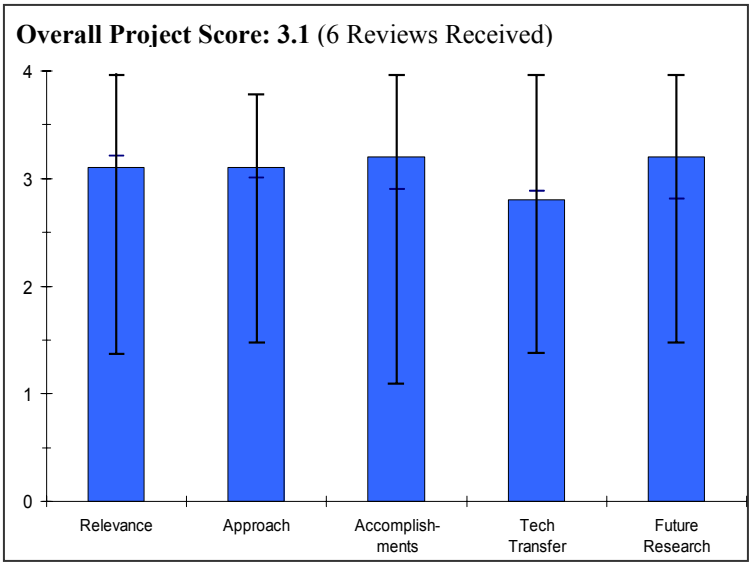
Weaknesses
- Additional emphasis on the catalyst and in the reforming process is necessary.
- Large scale process does not match up with chosen feedstock.

Specific recommendations and additions or deletions to the work scope

- Concentrate more on the reforming process (catalyst and operational aspects) tests before design the reactor
- DOE should perform a parallel and more detailed review on this project.
- Suggest keeping capability but applying it to a different process.

Project # PDP-08: Hydrogen Production for Fuel Cells via Reformation of Coal-Derived Methanol
Erickson, Paul Anders; University of California, Davis

Brief Summary of Project



Hydrogen can be produced from many feedstocks, including coal. The objectives of this project are to establish and prove a hydrogen production pathway from coal-derived methanol for fuel cell applications. The project has used internal and external analyses to quantify the differences in composition between coal-derived and a baseline fuel-cell-grade methanol. Degradation rates in a steam reforming system have been observed with both fuels, and enhancement methods have been studied. Both autothermal reformation and steam reformation have been proven with the coal-derived methanol. Presently, clean-up methods are being investigated and operation within a PEM fuel cell system will shortly take place with the coal-derived methanol.

Question 1: Relevance to overall DOE objectives

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

- Lowers the cost of hydrogen ($1.77/gge) via methanol reforming.
- Addresses hydrogen storage (via methanol).
- The project objectives and the work scope are partially consistent with the Hydrogen Initiative and the DOE program goals. The major objective is to compare the properties and performances of coal-derived vs. fuel cell grade methanol.
- The presentation does not address key technology issues related to production and delivery of hydrogen, does not list risks and barriers, nor pro-active approaches to overcoming key technology barriers. "Develop reforming technologies for gasification and pyrolysis processes" is more of an objective (intended result) statement, rather than a statement of barriers.
- A slide on the relevance and contribution of the work to DOE and Hydrogen Initiative program goals (i.e., why do it), is needed, but not included in the presentation (for the reviewer's understanding of the major issues and the current technology deficiencies).
- Methanol as a transportable intermediate from coal and biomass is a viable option that does not attract as much attention as ethanol. The project addresses two important factors: methanol impurities and reactor temperature inhomogeneity in leading to degradation. These are important barriers although not specifically called out in the MYPP barriers.
- The project consists on the hydrogen production through on board reforming of methanol. It is in disagreement with the recent DOE on-board fuel processing no-go decision.
- The project is well aligned with the President's Hydrogen Fuel Initiative and shows close bearing to DOE's RD&D Plan.
- If successful, it will provide a method to transport and store hydrogen using the existing infrastructure for liquid fuels.
- DOE has terminated onboard reformer program.
- Methanol has no existing infrastructure.

Question 2: Approach to performing the research and development

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

- Overcome non-chemical grade methanol reforming limitations.
- The major driver of the work is the three times petroleum hydrocarbon content in coal-derived methanol compared to fuel cell grade methanol.
- The approach to some extent involves innovative technical strategies and solutions to advancing knowledge base as well as routine data collection using proven techniques to achieve project goals.
- The approach will contribute to achieving the project goals.
- This is a sound academic approach to an engineering study. The reforming and autothermal should be compared on an efficiency basis as well as by conversion rates.
- The goal of the project was to evaluate the effect of the impurities of coal-derived methanol on the hydrogen production. In spite of the deactivation observed, the project did not propose solutions to overcome this problem.
- The approach is sound and logical.
- Both steam reforming and autothermal reforming were included.

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

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

- Demonstrated geometry improved reformer operation/life.
- Tested monolith ATR catalyst.
- The findings that (1) proper flow distribution can improve reactor capacity via heat transfer enhancement and (2) reactor geometry has the most influence on catalyst performance are novel and insightful.
- The project has developed a novel static flow distributor to prevent misdistribution and significantly minimize radial thermal gradient in the catalytic reactor.
- The novel static flow distributor is potentially patentable and the PI may consider applying for a patent.
- The slides present data without sufficient data interpretation and analysis and without adequately discussing the relevance of findings.
- It is not always clear how the new info relates to program goals and contributes to solving problems in order to advance hydrogen production technology state-of-the-art.
- During the review the PI explained the work scope and results very well.
- The conclusion from a 30 hr test that reactor geometry has the profound influence on catalyst degradation has been made prematurely.
- Limited SR performance data presented; does not appear to be of significance for scale-up studies.
- The most important accomplishments, bullets 1 and 2 of the Progress Results slide, are least explained.
- Temperature profiling of reforming reactor is excellent work. The presence of hydrocarbon impurities is an important factor to be considered. The results should be consistently labeled with operating conditions, including temperature, weight hourly space velocity, and steam to carbon ratio.
- The project did not demonstrate ways to overcome one of the technical barriers (deactivation) in order to improve the performance of the process.
- The project appears to be progressing well on schedule and has identified barriers to meeting objectives together with strategies to address these barriers.
- Some interesting data on methanol reforming were presented, including the installment of flow distributors in reformer (fixed bed).

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

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

- Collaboration with Eastman Chemical.
- Eastman and Methanax are reported as technology partners, but their contributions to achieving the project goals are not documented and therefore clear.
- Most of the work seems to have been conducted at the PI's facility.
- The PI said that Eastman has helped in the analysis and characterization of the coal-derived methanol fuel.
- Worked with Eastman and Methanex.
- The support of one the industrial partners ended recently (Nov 2004).
- The project only lists one passive collaborator (Eastman Chemical) who provides the PI with plant-produced methanol. It is unclear if Eastman's role will grow beyond this, or if Methanex will re-start its support that ended in Nov 2004.
- Needs more interactions with industries to seek R&D inputs.

Question 5: Approach to and relevance of proposed future research

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

- Need to finish ATR degradation/transient tests, review clean-up technology, integrate reformer with PEM hydrogen fuel cell, and compare performance.
- The proposed future work including quantifying fuel cell performance with two grades of methanol is consistent with project objectives.
- While completing autothermal data collection phase should perform parametric studies with mass balances and determine energy efficiencies.
- Before the integration of the reformer and cleanup to fuel cell, the project should focus on overcoming the barrier. The purchase of the complete system also does not address the problem of the impurities of the feed.
- Future plan is well focused with milestone tasks culminating on the final quantification of fuel cell performance using coal-derived methanol.
- What is the purpose of this study? Will this be answered in the future work?

Strengths and weaknesses

Strengths
- Good approach and progress.
- The project has progressed well consistent with the technical baselines listed in the Gantt chart.
- Good experimental studies of an important option for coal-derived methanol.
- The project builds on an existing commercial method of producing methanol from coal and using this methanol to produce hydrogen through steam and autothermal reforming technologies. If successful, it will produce hydrogen at around $1.77/gge (better than DOE's goal). It could potentially alleviate the need for hydrogen storage and distribution in transport applications if reforming can be conducted on-board a vehicle.
- Reforming data with coal based methanol were presented.

Weaknesses
- The PI agreed that the three times concentration of petroleum hydrocarbons in coal-derived methanol can be easily removed by using a guard bed packed with commercially available getter materials (the concentration is only in mg/liter range): therefore, a major project objective to conduct extensive work on how to use coal-derived methanol as the hydrogen carrier for fuel cells may not be that important.
- Results are not fully discussed in the presentation, e.g., why did conversion get worse at higher temperatures?
- The PI should solicit more involvement by the private sector at this stage to ensure on-going funding for a commercial demonstration.
- Discussions on the significance of catalyst deactivation data were inadequate.
- Inadequate discussions on the reasons leading to the improvement in methanol reforming with flow distributors.

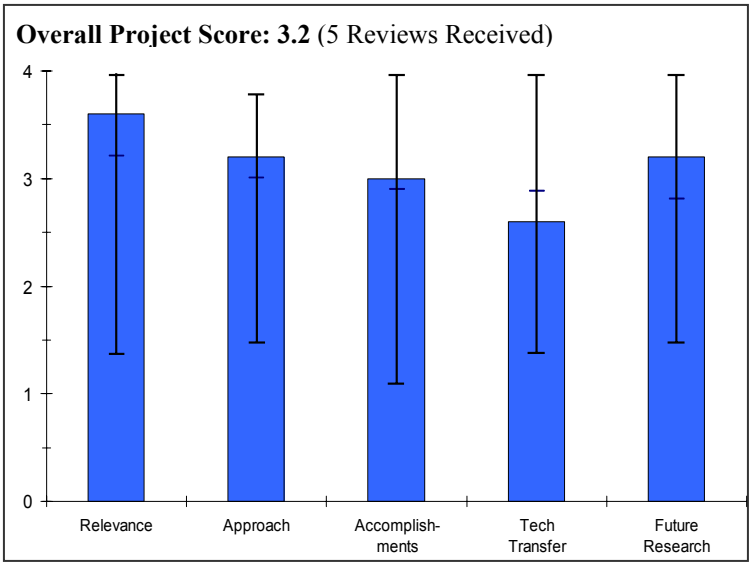
Specific recommendations and additions or deletions to the work scope

- Stationary hydrogen storage and production studies/tests are recommended.
- Expand to bio-derived alcohol feedstocks.
- The future work should include, as stated, the quantification of the coal derived vs. fuel cell grade methanol performance, and add a third test involving a guard bed with getter materials when using the coal-derived methanol.
- The project should focus on eliminating the barrier identified (deactivation due to the presence of impurities). The relevance of the project should be evaluated due to the recent decision of DOE to stop the support to studies of on-board hydrogen production.
- A rapid determination of the preferred reforming technique (steam reformation with Cu catalysts and optimized reactor geometry) versus autothermal reforming.
- Greater effort on quantifying fuel cell performance with coal-derived methanol. This approach, if successful, can radically alter DOE's R&D funding focus on hydrogen production from coal, hydrogen storage, and distribution.
- Quantify the differences in petroleum hydrocarbon composition between fuel grade methanol (from natural gas) and coal-derived methanol.
- Needs a good plan to wrap up the data in a package to show whether the coal-derived methanol is as good as or interchangeable with fuel grade methanol from natural gas.

Project # PDP-09: Hydrogen Production via a Commercially Ready Inorganic Membrane Reactor
Liu, Paul; Media & Process Technology Inc.

Brief Summary of Project

A carbon molecular sieve based hydrogen selective membrane has been developed as a reactor for water gas shift reaction. In addition to combining the reaction and separation in a single step, we will investigate field-implementation related issues, such as the development of full-scale membrane and modules, evaluation of the long term performance and material stability of the membrane, and a means to accommodate cooling requirement in a membrane reactor environment.



Question 1: Relevance to overall DOE objectives

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

- Lowers the cost of hydrogen.
- The project objectives are consistent with the Hydrogen Initiative and the work scope is consistent with the DOE hydrogen production program goals.
- The project results will expand knowledge base and advance the hydrogen production current state-of-the-art.
- The project is well aligned with the President's Hydrogen Fuel Initiative and shows close bearing to DOE's RD&D Plan.
- If successful, it will provide a method to produce hydrogen efficiently through a single stage WGS and gas clean up.
- Combining water gas shift (WGS) and hydrogen separation in one step will help program.

Question 2: Approach to performing the research and development

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

- Includes process intensification, membrane development, and pilot tests .
- The technical strategy and process development scope show potential for maturing to technical readiness for engineering scale-up and demo scale tests.
- Innovative approaches and solutions have been used to expand knowledge and develop a novel technical know-how.
- The approach is sound and logical.
- A scattered focus in the approach that seems a little too ambitious for the work effort and funding level. The work described ranges from fundamental mathematical modeling to constructing and testing a pilot-scale unit.
- A sound approach was proposed.

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

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

- High purity hydrogen and flux rate.
- Low cost ceramic membrane.
- A novel heat transfer mechanism (based on the moisture permeability of the membrane reactor material) could result in the development of a one-stage high temperature-Low temperature shift process without interstage cooling.
- The approach allows improved efficiency, process intensification, and potentially lowering hydrogen production cost.
- Developed a predictive mathematical model and verified the model with experimental data.
- The project is progressing well along established technical baselines and has achieved significant milestones.
- Many of the claims made use subjective terms, i.e., excellent, proved, verified, etc., without providing sufficient data to support these claims.
- No reference is made to potential and well-known barriers to combining WGS reactions with hydrogen separation (i.e., kinetics, catalyst poisoning by excess CO).
- Good progress made in several areas including carbon molecular sieve (CMS) membrane fabrication and membrane performance modeling.

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

This project was rated **2.6** for technology transfer and collaboration.

- Collaboration with USC (Professor Tsotsis), Pall Corp., and Dr. Kleiner.
- Pall Corp, Prof. Tsotsis of USC, and Dr. Kleiner, a consultant is involved in the project.
- No commitment from Pall Corp is included in the presentation.
- Also, Pall Accuse SS substrate may not be suitable because of low flux an higher cost compared to ceramic substrate.
- The project only lists consultants and suppliers. No clear indication of other institutions or efforts of coordination among other institutions.
- Needs more interaction with industry to seek R&D inputs.

Question 5: Approach to and relevance of proposed future research

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

- Includes simulation, pilot tests, economic analysis, and field test site identification.
- Legitimate future work, can be easily related to technology development goals, and will move the project forward to meaningful, successful outcome.
- A field test is planned, but not funded.
- Future plan is focused on the construction and testing of pilot-scale reactor and the refining of the mathematical model with pilot-scale reactor data. Based on the progress reported to date since the project's inception on 10/1/03, it is somewhat questionable whether these tasks can be completed on time without additional personal. If needed, this should be done at no additional cost to DOE.
- CMS membrane scale up plan was presented.

Strengths and weaknesses

Strengths

- Excellent development.
- The LT-WGS-MR technology is most adaptable to warm gas clean-up technology schemes and amenable to further scale-up.
- The high hydrogen serration flux at LT-WGS conditions compared to Pd-Cu membranes is very encouraging.
- The project is an effort in process intensification to develop a membrane reactor that can combine the WGS reaction and hydrogen separation simultaneously.
- Systematic approaches were used to pursue parallel approaches using ceramic and stainless steel substrates.

Weaknesses

- The PI correctly said that industry is moving away from hot gas clean up. At warm gas clean-up conditions, the HT-WGS technology is no longer applicable. PI agrees that the HTWGS-LTWGS approach is no longer applicable.
- Lacks academic approach and guidance, and has membrane sealing and defects issues.
- The terminology "Commercially Ready" used in the title is misleading -- the PI has yet to prove that his membrane reactor is ready for commercialization.
- The PI has not addressed any potential barriers or show-stoppers in his process intensification work effort that can delay or halt progress.
- Efforts on stainless steel/CMS membrane R&D were inadequate. Discussions on CMS membrane scale up issues were not adequately discussed.

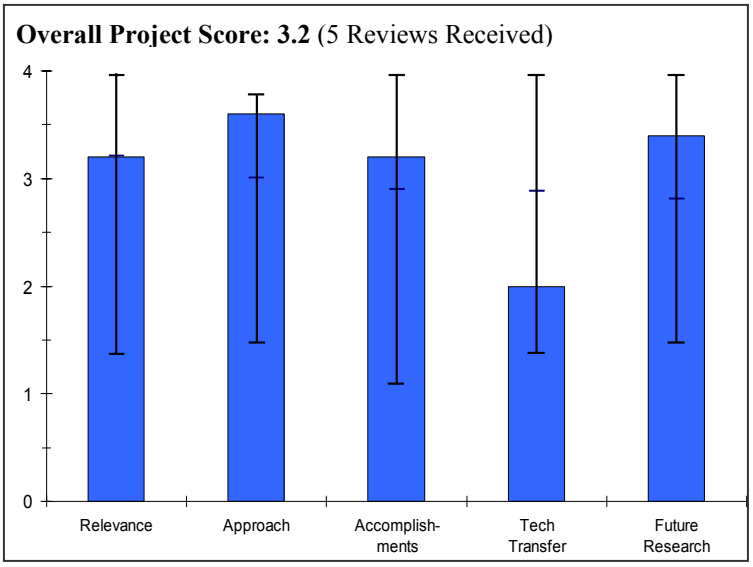
Specific recommendations and additions or deletions to the work scope

- Although the hydrogen flux at low temperature conditions is higher than achievable by Pd alloy membranes, it is not clear if the flux meets the DOE commercial target; the project needs to at this issue.
- Low flux will not achieve the process intensification claim.
- An awareness of potential barriers during process intensification (i.e., catalyst poisoning, kinetics) that might arise together with strategies to address them.
- Focus work on improving the effectiveness of stainless steel/CMS membrane development. More experimental work is needed to substantiate the modeling results. More long term life testing of membranes.

Project # PDP-10: New York State Hi-Way Initiative
Bourgeois, Richard; GE Global Research

Brief Summary of Project

Under this DOE contract, 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.



Question 1: Relevance to overall DOE objectives

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

- Electrolysis hydrogen generation is important to DOE goals and objectives.
- Emphasis on New York State barriers or hydrogen infrastructure development is too narrow although probably translatable to some other states.
- Most aspects of the project align with the President's Hydrogen Initiative and the RD&D plan objectives.
- This project addresses lower cost electrolyzers, which is a key element in the HFCIT MYPP.
- Supports a very important task in the distributed electrolysis portion of the hydrogen production component -- the development of electrolyzers with much lower cost than today's devices.
- The work to reduce cost for electrolysis is aligned.

Question 2: Approach to performing the research and development

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

- The approach is logical.
- Leverages multiple aspects of GE expertise.
- Economic potential may be too tied to low cost electricity; need to benchmark using standard programmatic assumptions for electricity cost, storage, handling, etc.
- The approach is generally well thought out and effective but could be improved in a development of larger units. Most aspects of the project will contribute to progress in overcoming the barriers of high cost electrolyzers.
- Program takes approach of revisiting the components of an alkaline electrolyzer and trying to find ways to maintain their key functionalities, but with goal in mind of low cost manufacture using low cost materials and manufacturing processes.
- Would score it 4 on the electrolysis work, but 1 on other aspects of the scope.

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

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

- A summary of 1-page overview/summary of progress would have been helpful in assessing status toward objectives. The words "accomplished", "completed", "initiated", "in progress" would have helped.
- Good progress in design and performance in a relatively short period of time.

- The project has shown significant progress toward against its objectives and to overcoming the barrier of higher cost electrolyzers.
- Impressive work in designing a modular five cell stack with plastic housing and simple connections for water, power, and products. Wire arc method for forming electrodes seems amenable to low cost manufacture. Excellent modeling (kinetic and CFD) to understand factors affecting homogeneity of reactions in working cells and stacks.
- Advanced manufacturing techniques look extremely encouraging.
- Can't see what the cost improvement is towards the $/gge target.

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

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

- It is unclear what the role of each team member is. Therefore, it is hard to determine if all bases are adequately covered.
- Competitive interests limits collaboration.
- Third-party validation of system would benefit the project.
- Little coordination exists; full coordination would take significant time and effort to initiate.
- This project would benefit if there were commercial sites that could use the developed technology. Currently the developed technology is too small in size, but any scale up should have a demonstration site included for field verification.
- Most of the work seems to have been done in house at GE, but this does not seem to be an issue as GE has many of the requisite skills in house.
- From data presented appears to be internal to GE.

Question 5: Approach to and relevance of proposed future research

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

- Reasonable but ambitious for the remaining 6 months.
- Long-term performance testing at a practical scale will be essential.
- Further improvement in electro catalyst, membrane, etc, not discussed. Is this the best design or is further optimization planned?
- Although future funding may not exist, this project shows excellent capacity for meeting DOE cost goals in the future and addresses key technical barriers.
- Next steps durability testing and scale up appear appropriate at this stage. Durability testing is needed to find which components are vulnerable to degradation, and this will lead to further work to solve those problems.
- Consider funding additional development. It appears the program ends this year.

Strengths and weaknesses

Strengths
- Leverages multiple capabilities in molding, system design, catalysis, materials development, manufacturing, and power generation and utilization.
- Innovative new electrolyzer design that shows potential for lower costs and mass production capability.
- Good integrated team at GE that seems to have the skills to make this real. PI appears to have access to many of those in-house skills and facilities.
- Very innovative manufacturing approach.

Weaknesses

- Needs to show path to achieving HFCIT targets for stack/system cost, using standard programmatic assumptions.
- Current demonstration is too small (only 500W size).
- Economic analysis seemed very limited. This project needs to have H_2A economics applied, especially with respect to electricity cost in order to get a more realistic view of the net hydrogen production costs. Those costs will have to include sitting, maintenance, labor, etc. A cold-eyes look at the estimated manufacturing cost would also be helpful in order to add credibility to the overall economics.
- Could improve with more collaboration outside.

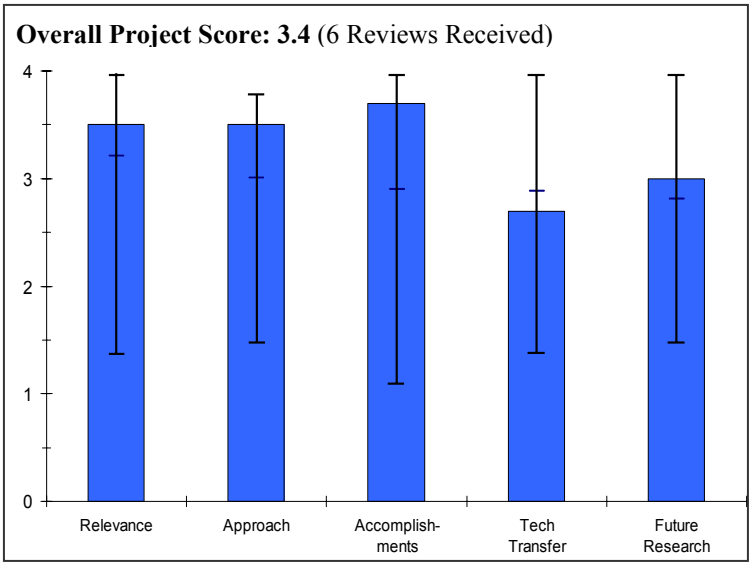
Specific recommendations and additions or deletions to the work scope

- Scale up to larger size -- 50kW minimum.

Project # PDP-14: Development of Dense Ceramic Membranes for Hydrogen Separation
Balachandran, Balu; Argonne National Laboratory

Brief Summary of Project

Argonne National Laboratory is developing dense membranes for separating hydrogen from mixed gases, particularly product streams generated during coal gasification and methane reforming, at commercially significant fluxes under industrial operating conditions without using electrodes or electrical circuitry. These membranes are to be ceramic-based, highly selective for hydrogen, chemically stable in corrosive environments at operating temperatures up to ≈900 C, and possess the physical integrity to withstand high operating pressures. Single-phase mixed proton-electron conductor and dual-phase cermet (ceramic/metal composite) membranes are being investigated in this project.



Question 1: Relevance to overall DOE objectives

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

- This project provides outstanding support of the goals of DOE Fossil Energy draft "Hydrogen from Coal RD&D Plan" in that Dr. Balachandran's objective in performing the research is to develop dense ceramic membranes for separating hydrogen from mixed gases at commercially significant fluxes under industrially relevant operating conditions. However, the project provides only a fair amount of overall support to the Department's Hydrogen program since the product streams of interest are exclusively coal gasification and (large-scale) methane reforming, limiting the overall relevance to the DOE Hydrogen Program.
- Lowers cost of hydrogen.
- Addresses six barriers under "Separations and Other Cross-Cutting H_2 Production" in the Multi-Year Research, Development and Demonstration Plan.
- Addresses barriers in FE's hydrogen from Coal RD&D Plan. This work is funded by DOE FE.
- Extremely relevant to the Hydrogen Initiative.
- The project is well aligned with the President's Hydrogen Fuel Initiative and shows close bearing to DOE's RD&D Plan in the development of hydrogen transport membranes.
- Consistent with generation of pure hydrogen from fossil fuel gasification streams.

Question 2: Approach to performing the research and development

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

- The approach is focused on achieving a flux rate of 100 at a low cost while. This is better than the 2010 goal for microporous membranes stated in FE's RD&D plan.
- ANL's focus on reproducibility and stability is critical
- The poster demonstrates a very good understanding of safety considerations.
- Systematic approach to select/fabricate, test, optimize and commercialize dense ceramic membranes.
- Approaches are right on target to meet the goals of the DOE Hydrogen Fuel Initiative and hydrogen from coal R&D plan.
- Perform high pressure flux measurements at Argonne.

- The three-pronged approach to develop the membranes is very good. All options are explored/covered by this approach.
- Sealing, defect issues major stumbling block for industrial application.
- The approach is technically sound and logical.
- Excellent approach of developing mixed proton, electron conducting membranes which permit a total selectivity for hydrogen separation.

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

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

- The Hydrogen from Coal RD&D Plan, however, expects to identify advanced hydrogen separations technology including membranes tolerant to trace contaminants by 2006 before the project concludes. Since 1998, high temperature membrane development at ANL has shown steady progress and should be a candidate for downselection.
- Developed dual-phase dense membranes that nongalvanically separate hydrogen.
- Steady progress since 1999 in H_2 flux improvement (2 to 66 scfh/ft^2) (400 scfh/ft^2 potential).
- Short-term measurements show stable flux in CO, CO_2, CH4, H_2O, and H_2S feed streams.
- Recognized as one of 2004's 100 most significant technological developments in R&D 100.
- Outstanding progress has been achieved in all areas of research related to several barriers.
- Hydrogen flux numbers obtained here are higher than DOE target numbers.
- Outstanding progress is seen from their slide #21.
- The project has overcome many of the past reviewers comments to increase flux, enhance performance, improve chemical stability, and reduce operating temperature.
- Outstanding membrane material development work from a PI with a substantial record of accomplishments in this field of high temperature membranes.

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

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

- The potential for technology transfer is not well established. Poster indicates that under guidance from NETL program managers, membrane technology transfer will occur through industrial collaborations.
- Collaboration with NETL.
- Potential collaboration with Eltron, Praxair, and Air Products.
- Collaborates with NETL in the high pressure permeation measurements.
- Have CRADA with Eltron Research Inc., but that project was not reviewed here.
- This project is funded under the National Lab base research program.
- The project only lists the P.I.'s organization (a national lab) and one other national lab as its partner.
- Private organizations should be marketed to recognize and commercialize this development effort of hydrogen separation membranes.
- It appears that more 'pull' from technology developers is needed to render it industrially practical.

Question 5: Approach to and relevance of proposed future research

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

- Proposed future work such as testing the long-term chemical stability of selected HTMs in atmospheres typical of coal gasifiers builds on past progress.
- Acquire analytical equipment (i.e., GC-MS) for additional testing.
- Continue developing new high temperature membranes to reduce cost, increase flux, and improve mechanical/chemical stability.
- Technology transfer through industrial collaborations.
- Excellent future plan.

- Include high pressure hydrogen permeation measurement.
- Can ANL do long-term permeation experiments (say 6-12 months)?
- Future work is focused on the further validation of past findings and the eventual transfer of the hydrogen separation membrane technology through industrial collaborations.
- The future research should be more guided by an engineering and scale-up model for its implementation. A development path should have been delineated.

Strengths and weaknesses

Strengths
- The dense ceramic membrane meets FE's flux goals for 2010 that were established specifically for microporous membranes, which may have lower material costs than dense ceramic membranes but lower flux rates.
- Systematic approach.
- Good progress.
- Research team with considerable experience in the area of gas transport membranes.
- Detailed approach of research.
- Experimental safety analysis, risk mitigation, and structural and procedural controls.
- Research recognized by peers -- R&D-100 Award in 2004. Experience/expertise in ceramic science and engineering.
- The project has developed dual-phase dense membranes that show potential of meeting DOE goals for hydrogen separation flux, stability, and operating conditions.
- Very impressive high temperature membrane synthesis and discovery capabilities. Excellent results of hydrogen selective permeation from acid gas containing source streams.

Weaknesses
- While the chosen material reduces membrane cost, the cost of development is very high and it is unclear how much if any cost sharing is provided.
- Long-term tests needed (future).
- Catalysis/catalyst area.
- Membrane sealing, defect issue. Stumbling block for industrial application.
- Private sector collaboration should be aggressively sought out to ensure that the PI's useful findings are moved toward commercialization.
- An apparent lack of a total system model where this membrane technology would be incorporated.

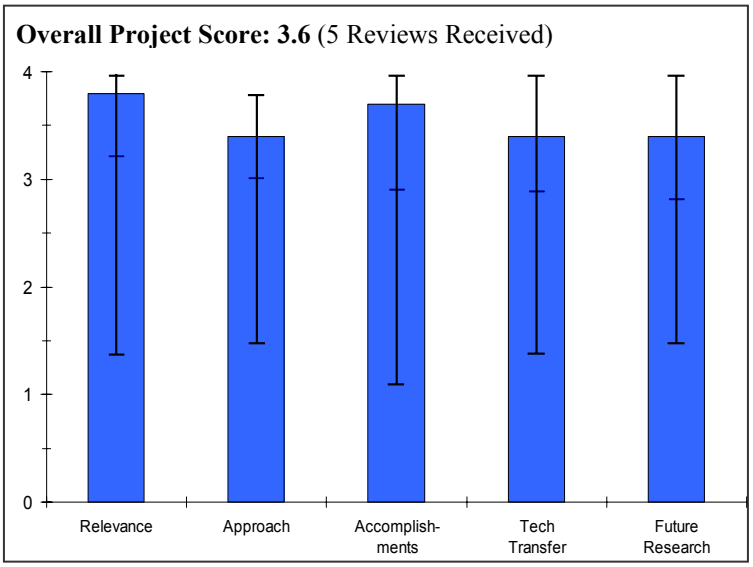
Specific recommendations and additions or deletions to the work scope

- ANL and FE might consider partnering with the renewable fuels and coal companies to test both biomass and coal feedstocks using the prototype dense ceramic membrane.
- Letters of support from industrial partners should be solicited rather than including the technology transfer aspects under "future work."
- Field testing in conditions of actual use.
- Additions:
    - High-pressure & high-temperature permeation test set-up at Argonne National Laboratory.
    - Study effect of catalysts on membrane surface.
    - Perform detailed microstructural analysis of the membranes & interfaces.
- Include the construction of a membrane reactor utilizing the ANL-3 (or improved) membranes to evaluate optimum membrane configuration, seals, and fabrication challenges (if any).
- Proceed with work but in a closer context with the requirements on the implementation development next stage.

Project # PDP-15: Ceramic Membrane Reactor Systems for Converting Natural Gas to Hydrogen and Synthesis Gas (ITM Syngas)
Chen, Christopher; Air Products and Chemicals, Inc.

Brief Summary of Project

Since 1998, Air Products and Chemicals, Inc. directed this research team comprised of industry and university participants to research develop and demonstrate an Ion Transport Membrane reactor system for the low-cost conversion of natural gas to hydrogen and synthesis gas. Technical, engineering, operational and economic data necessary for full commercialization is being collected and analyzed.



Question 1: Relevance to overall DOE objectives

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

- Low cost ceramic membrane reactor is relevant to the success of the hydrogen program.
- The project is well aligned with the President's Hydrogen Fuel Initiative and shows close bearing to DOE's RD&D Plan in the development of hydrogen production methods from natural gas.
- Proposed work offers advantages in improving the economics of hydrogen production from natural gas, with a CO_2 stream ready for sequestration.

Question 2: Approach to performing the research and development

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

- Development of potential material with unique composition and properties to ensure thermodynamics stability, mechanically reliable and ensure oxygen ion transport to enhance conversion of national gas to syngas is great.
- The approach is technically sound and logical.
- A good step-by-step approach was proposed to advance the development from bench unit to current process development unit PDU scale demonstration.

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

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

- Needs to address long term durability issues regarding the seal technology.
- Is there an issue with flux aging (degradation) with time?
- Great accomplishments to date on material development and the method of stacking planar membranes are novel accomplishments.
- The project has addressed many of the challenges associated with ion transport membranes (ITM) for this application.
- Clear cut results have been shown that directly impact barriers that are being addressed.
- Good progress made in all key aspects including membrane flux improvement and module assembling.

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

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

- Strong team with broad expertise
- Air Products as an engineering and chemical company has good track record in commercializing technologies.
- The project only lists its 9 partners. Their role is not clear, i.e., financial sponsors, in-kind providers of goods and services, or fee-based service or equipment providers.
- The industrial participants were well represented from researchers to end user.

Question 5: Approach to and relevance of proposed future research

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

- Scale up and testing is now appropriate for this technology.
- Future work builds on past progress and is focused on the testing of full size planar membranes at a 1 million scfd subscale engineering prototype.
- Future advancement from PDU to large scale units should help further advance this technology.

Strengths and weaknesses

Strengths
- Their innovative ideas and how they are being implemented is their strength.
- The project has made extensive progress in the development of planar membranes to meet the physical and material demands for producing syngas from natural gas.
- Great strides made in the areas of seals and module assembling. Plan was made in the water gas shift (WGS) catalyst accommodation in the membranes reactor.

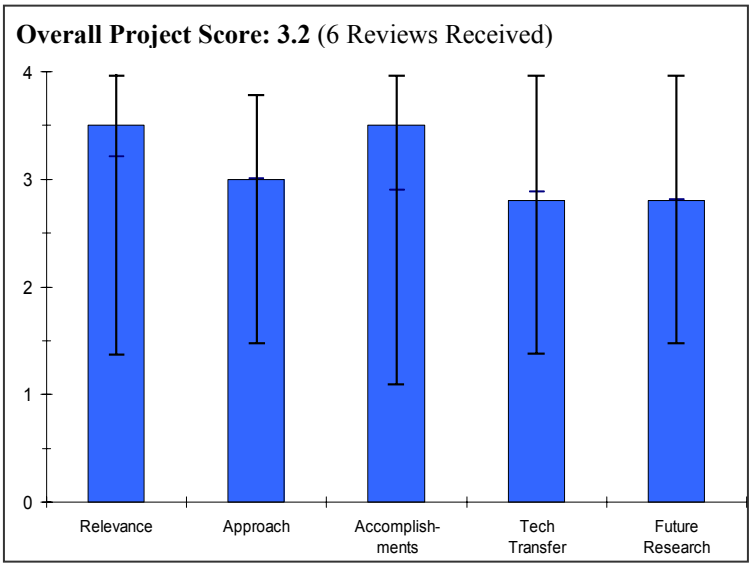
Weaknesses
- The outcome of the project may remain commercially unattractive until carbon emission taxes and other similar constraints favor the economics for the conversion of natural gas to hydrogen.
- The lack of a clearly articulated set of implications concerning the impact of hydrogen production.
- Inadequate quantitative experimental data were presented on membrane performance. No descriptions of WGS catalyst development.

Specific recommendations and additions or deletions to the work scope

- Expand work on WGS catalyst development, which needs to be incorporated into the membranes with high activity and long life. Continue to increase the through put per membrane module. Obtain operating data of membrane performance with natural gas feed and reforming catalysts incorporation into the membrane.

Project # PDP-16: Cost-Effective Method for Producing Self-Supporting Pd Alloy Membrane for Use in the Efficient Production of Coal-derived Hydrogen
Lanning, Bruce; Southwest Research Institute

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 will develop a process methodology for the cost-effective manufacturing of 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 continuous roll-to-roll 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 **3.5** for its relevance to DOE objectives.

- It is relevant to DOE Coal to Hydrogen Effort, however the degree of relevance to the overall DOE hydrogen program may be limited.
- Lowers the cost of producing hydrogen.
- Success should lead to reduced cost of production for coal, natural gas, and biomass.
- The project is well aligned with the President's Hydrogen Fuel Initiative and shows close bearing to DOE's RD&D Plan in the development of hydrogen production methods.
- Has offered to develop a self supported Pd membrane for hydrogen separation.

Question 2: Approach to performing the research and development

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

- It is relevant to DOE Coal to Hydrogen Effort, however the degree of relevance to the overall DOE hydrogen program may be limited.
- Formation of dense Pd alloy films on supporting substrates using ion/plasma-based vacuum processing.
- Optimization of release processes, characterization/testing, demonstrate membrane optimization.
- Approach shown on slide 4 was nicely described in results slides.
- Should perform matrix-designed experiments to characterize materials and manufacturing methods. From what is presented, only a few isolated approaches are researched.
- Good that PI is studying flux as well as hydrogen purity.
- The approach is technically sound and logical to develop a process to deposit a substrate on a thin-film metallic membrane.
- Has proposed a good work plan.

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

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

- Best performance data @ 400 C shown for a 5 µm Pd-Cu alloy foil, area = 2.6 cm^2:
  - pure hydrogen permeability = 2 •10-4 cm^3•cm/cm^2•s•cmHg0.5
  - hydrogen flux = 124 cm^3/cm^2•min = 242 SCF/ft^2•h;
  - feed pressure =20 psig
  - Exceeds DOE Hydrogen Program and 2010 DOE Fossil Energy Technical Targets.
- Developed innovative fabrication methods.
- Demonstrated high purity hydrogen (> 99.95%).
- Increased hydrogen permeation (thin film, surface pre-treatment) exceeded DOE targets demonstrating hydrogen @ flux of 242 scfh/ft^2.
- Nice results.
- Need more matrix studies to draw generalized conclusions.
- The project has made significant progress in developing methods for membrane fabrication, using flexible and rigid substrates.
- Results include exploratory study and membrane fabrication.

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

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

- Low cost sharing. Not enough information on how technology transfer is to occur, however excellent partners are at least identified.
- Collaboration with Colorado School of Mines and IdaTech.
- No mention of what partners are responsible for.
- CSM and IdaTech are good partners, but what do they do?
- The project lists 2 partners, of which only one (Ida Tech)has provided some financial cost-share for the project along with DOE funding.
- Discussions on technology transfer needs to be expanded.

Question 5: Approach to and relevance of proposed future research

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

- Future RD&D in enhanced permeation seems reasonable, however demonstrating at 99.95% pure hydrogen has limited application in terms of direct use in hydrogen fuel cell vehicles.
- Enhance hydrogen permeation (alloys, defect minimization).
- Module demonstration (performance and characterization @ 75 sqin).
- Prototype module to industrial partner IdaTech for testing at targeted ~100 scfh/sqft.
- With better matrix experiment design, approach should lead to significant results.
- Future work builds on past progress and is focused on improving hydrogen permeation, alloy compositions, and minimizing defects.
- Future work should include some lifetime tests to evaluate the stability of the membrane over time.
- No tests planned with gas mixtures to evaluate selectivity.
- Proposed work scope is broad. Priority of work order was discussed adequately.

Strengths and weaknesses

Strengths

- Low temperature membrane to complement existing high temperature ceramic membranes is reasonable way to diversity the FE portfolio.
- Fabrication development.
- Innovative.
- The project has made extensive progress in the development of Pd-alloy membranes that can be fabricated with substrates, providing control over hydrogen permeability.
- Good advances in membrane fabrication technique. Preliminary exploratory study conducted to improve membrane performance was presented.

Weaknesses

- Hydrogen purity levels derived in recent tests (99.95%) would be insufficient for direct use in fuel cell vehicles.
- Need full-scale development and long-term testing (future).
- PI was not available at poster on three separate visits. Couldn't answer questions that might have allayed concerns.
- Inherent limitations of planar design. Sealing and packing density limited.
- No tests conducted to determine membrane and substrate robustness or to test selectivity in gas mixtures.
- Inadequate discussions on the pros and cons using rigid substrate backing for Pd membranes compared to other alternatives. No conclusions. No milestones.

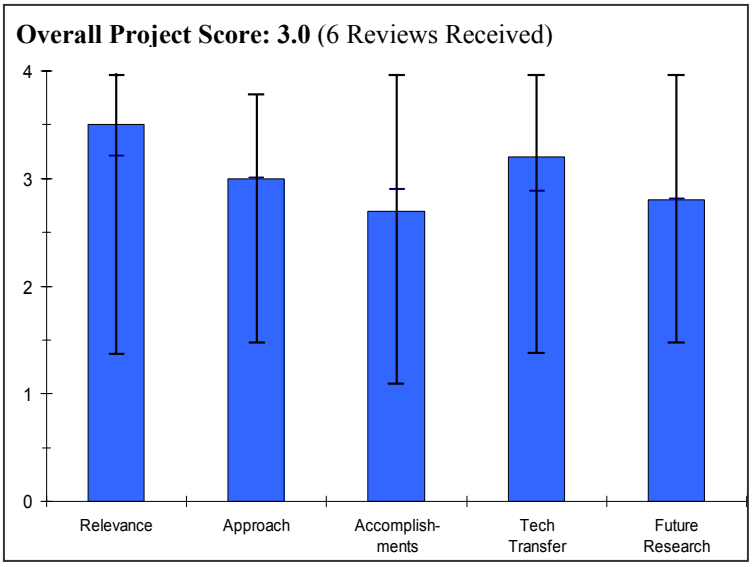
Specific recommendations and additions or deletions to the work scope

- Include testing of renewable feedstocks as well as coal.
- Full-scale development.
- Need more matrix experiments.
- Broaden test program to include membrane and substrate lifetime tests as well as tests with gas mixtures.
- The term "Cost-Effective" used in the title implies some cost information and economic analyses was or will be performed to support this claim. No such information has been provided in the work performed so far.
- Expand work to address issues which must be overcome to advance the membranes for testing by IdaTech. Prove out the reproductability of thin-film Pd membrane fabrication and its performance. Fine tune the membrane preparation technique. Needs long term life testing of membranes.

Project # PDP-17: Defect-Free Thin Film Membranes for H_2 Separation and Isolation
Nenoff, Tina; Sandia National Laboratories

Brief Summary of Project

In this project, Sandia National Laboratories (SNL) is working towards synthesizing defect-free thin film membranes for hydrogen separation and isolation which can replace existing expensive and fragile Pt catalysts. This work includes demonstrating effective light gas separations and commercialization potential of zeolite membranes. SNL will model the permeation of light gases through various frameworks/pores for optimized performance and validate them with actual permeation data obtained through testing on pure and mixed gases, including industrially relevant reforming streams. Collaborations for modeling and gas stream testing include New Mexico State University and New Mexico Tech University.



Question 1: Relevance to overall DOE objectives

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

- Good topic. PI is very knowledgeable on subject. Good use of zeolines with thin film base.
- Project is fully relevant to RD&D plan objectives-- lower cost hydrogen separation technologies have been identified as a critical need
- Relevant for low cost hydrogen production.
- The project is well aligned with the President's Hydrogen Fuel Initiative and shows close bearing to DOE's RD&D Plan in the development of hydrogen production and separation methods.
- Proposed work offers potential for improved H_2 separation if key issues can be overcome.

Question 2: Approach to performing the research and development

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

- Appears good. Good list of accomplishments and seems to be on track.
- Are effects of constituents besides H_2O and H_2S being evaluated?
- Limited solution provided by double-sided membrane for basic defect and sealing problems.
- Synthesis, testing and optimization of the performance of silicalite membranes.
- The approach is technically sound and logical.
- Unclear how "Defect-free" membranes are obtained through the proposed approach.
- Approach is research rather than development oriented.

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

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

- The team has made a lot of progress and has incorporated lessons learned in their work.
- Need to show progress against stated DOE targets.
- Little progress toward real world environment.
- Experimental results do not collaborate modeling results.
- The project has made significant progress in developing 1- and 2-sided membranes, testing these membranes, and determining that 1-sided membranes provide superior performance.
- Good progress made on thin film zeolite fabrication.

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

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

- Good activity with partners.
- Modest cost sharing contributions from industry.
- Encourage more collaboration for testing in "real world" conditions.
- No sure what the commercialization plan is.
- The project lists three industrial partners who have provided in-kind funding toward the project.
- More interactions are needed with industry to seek inputs to R&D work.

Question 5: Approach to and relevance of proposed future research

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

- Funding has been cut. Future work looks reasonable.
- What are the plans/prospects for integrating this membrane into a module that would/could be tested in an operating environment?
- Project appears not to have any future since funding has been cut.
- The applicant has identified several areas of interest that appear worthy of further investigation, including the deleterious effects of steam and CO2 as well as the increase in hydrogen selectivity with temperature.
- Proposed future work is broad in scope. Needs a priority order.

Strengths and weaknesses

Strengths
- Team seems to have found several other issues of interest while conducting work. Good to follow up with if funding is available.
- The project has made extensive progress in the development of zeolite membranes that have been tested with pure gas mixtures at temperatures up to 300ºC.
- Thin zeolite film (1-7 micron thickness) was successfully fabricated.
- Inadverse effects of steam on zeolite membrane performance were studied.

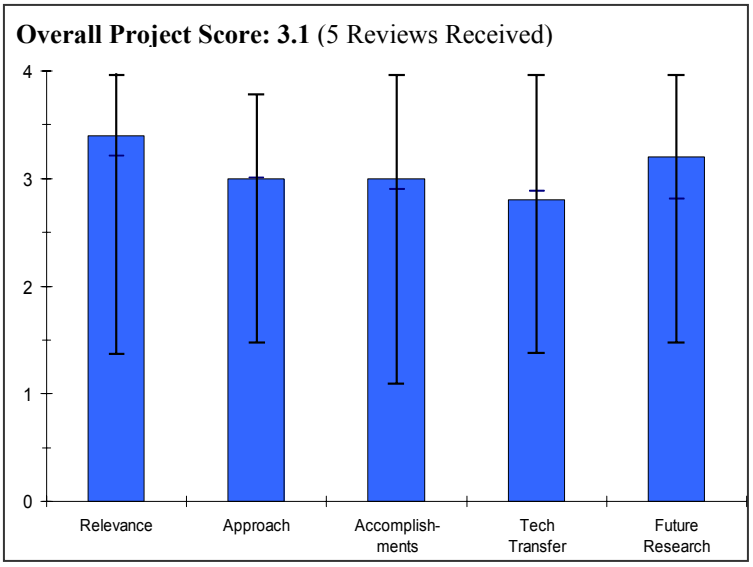
Weaknesses
- Little progress toward real world environment.
- The membrane manufacturing process described is unclear on how a "defect-free" membrane is obtained, other than by trial and error.
- Lifetime tests should be conducted to determine the performance of the zeolite membranes after an extended period of time.
- Inadequate discussions on the membrane performance data
- Did not list the target separation factors.

Specific recommendations and additions or deletions to the work scope

- With additional funding, the project should investigate the effect of H_2S on the membrane; i.e., if H_2S "plates out" at higher temperatures.
- Broaden test program to include lifetime tests.
- Additional tests with increasing levels of H_2S in the feed to confirm the absence of H_2S in the product as well as no damaging effect on the membrane.
- Highlight the pros and cons of zeolite membranes for H_2 separation when compared to competing technologies.
- Long term life testing is needed with promising membranes.
- Needs to test membranes at temperatures beyond 300C (note simulation data at 500C).

Project # PDP-19: Membrane Applications for Nuclear Hydrogen Production Processes
Bischoff, Brian; Oak Ridge National Laboratory

Brief Summary of Project



This Oak Ridge National Laboratory (ORNL) project will investigate the applicability of inorganic membranes for the production of hydrogen via the sulfur-iodine thermochemical cycle using nuclear energy as the source of heat to drive the relevant reactions. Inorganic membranes are candidates for facilitating the shift of the equilibrium in the decomposition of sulfuric acid, which is a critical step in this cycle. ORNL's inorganic membranes are being evaluated for the separation of hydrogen from steam and oxygen in high temperature electrolysis systems. The dramatically different separations in the sulfur-iodine cycle and in high temperature electrolysis dictate the need for a versatile and flexible fabrication process using materials compatible with the environments of interest.

Question 1: Relevance to overall DOE objectives

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

- It is relevant to DOE Nuclear-to-Hydrogen effort, however the degree of relevance to the overall DOE hydrogen program may be limited unless the scope is expanded to include separations applicable to renewable electrolysis applications for PEM fuel cells.
- Membranes for the S-I process support the President's Hydrogen Fuel Initiative.
- It is not clear how the membranes will lower the temperature of S-I process.
- Membranes for high temperature electrolysis only partially support the Initiative. Why do you need a membrane here?
- The project is well aligned with the President's Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

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

- Tests involving sulfur cycles and high temperature electrolysis seem reasonable.
- Why do we need a membrane to separate hydrogen from steam (high temperature electrolysis)? Water can be easily condensed.
- ORNL claims that their membrane fabrication is already versatile -- then why to put too much emphasis on fabrication of membranes and support tubes?
- Sulfur provides harsh environment for membrane.
- The approach is technically sound and integrates well with work conducted earlier by the applicant (on another project) to modify the outcome from these prior efforts into a new application.

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

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

- Preliminary work is encouraging where first-generation membranes show low-temperature separation of oxygen and sulfur dioxide from sulfur trioxide allowing high conversion at lower reaction temperatures.
- Data relevant to this project is not given.
- Separation factors are not high enough for practical applications.
- Almost identical accomplishments were presented in another presentation at this review meeting.
- The project has made significant progress in modifying its membrane design and materials of construction to adapt it for higher temperature operation for the sulfur cycle and high temperature electrolysis.

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

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

- Zero cost sharing. Not enough information on how technology transfer is to occur; however, excellent partners are at least identified.
- Collaborators (GA & INL) role is not defined. Are they real collaborators?
- The project lists one industrial partner and one national laboratory. It is unclear whether their involvement will continue in the absence of DOE (or other government agency) funding.

Question 5: Approach to and relevance of proposed future research

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

- Short-term plans for the remaining fiscal year seem plausible where samples will be exposed at high temperatures and characterized to determine the effect on support materials.
- Too much emphasis on the support tubes and too little on actual active membranes.
- No future plans for the high temperature electrolysis membranes and it is a good thing because there is no need for such a membrane.
- The PI has identified several areas of interest that appear worthy of further investigation, based on the work conducted to date.

Strengths and weaknesses

Strengths
- Inorganic membranes (if proven to work in high temperature conditions) may cost less to manufacture than microporous membranes used for S-I processes.
- Versatile process for fabrication of membranes.
- Safe operation procedure.
- Good experience in the area of porous membranes.
- Potential high payoff.
- The project has made extensive progress in the modification of the applicant's inorganic membranes in adapting them to thermochemical cycles requiring high temperature (850ºC) operation.

Weaknesses
- It doesn't appear as though ultra-pure hydrogen will result even after a second process step.
- Results that are relevant to this particular project were not given. The results presented here are old data and were presented in another oral presentation at this review meeting.
- Why should permeance increase with temperature in the porous membranes?
- Water molecules are smaller than hydrogen molecules -- how will the hydrogen be removed from water?
- Future plan lacks detail-- very vague. Be very specific.
- Sulfur provides harsh environment for membrane.

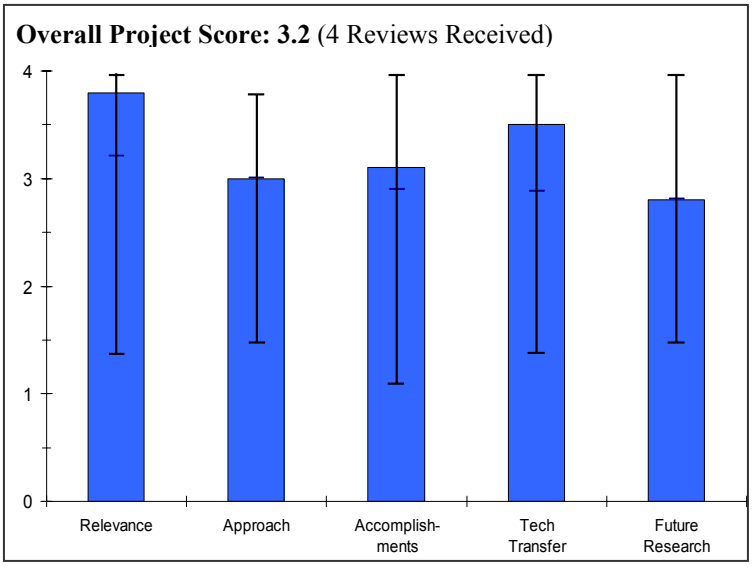
- The challenge in this effort is finding suitable materials to perform as membranes with the ability to withstand the rigorous conditions of high temperatures and acid gas environments.

Specific recommendations and additions or deletions to the work scope

- Delete the membranes development work for high temperature electrolysis.
- Add tasks to investigate the active membrane materials for the S-I process instead of concentrating just on the support tubes.
- Delete the tasks on fabrication of membrane modules; it is already claimed that they have a very versatile fabrication process.
- Add tasks to increase the separation factors for the S-I process.
- Some modeling studies may help reduce the vast number of options available for the selection of suitable membrane materials for this harsh environment application. This should precede the selection of materials as coupon samples for tests.

Project # PDP-20: Materials for High-Temperature Thermochemical Processes
Wilson, Dane; Oak Ridge National Laboratory

Brief Summary of Project



High temperature thermochemical processes are one of the promising approaches being investigated in the DOE Nuclear Hydrogen Initiative (NHI) for the production of hydrogen using nuclear energy. The thermochemical cycles currently being investigated include the sulfur-based cycles (sulfur-iodine and hybrid-sulfur) and calcium-bromine. These cycles generally involve very corrosive environments at elevated temperatures, up to 900ºC, which pose significant materials challenges. Identifying and demonstrating suitable materials of construction are critical viability issues for these cycles. The NHI is developing a long-term materials testing program to identify materials options and requirements. The ongoing reviews allow for an understanding of the material challenges of these processes, development of an "evolving" research and develop path forward, and identification of prioritized materials issues.

Question 1: Relevance to overall DOE objectives

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

- Project is 10% complete and appears to be a good project relevant to NE.
- Promote the development of processes, materials, and test methods for thermochemical hydrogen production at very high-temperatures up to 900ºC.
- Reduce costs of hydrogen production by using high temperature heat to accelerate reactions.
- Enable the use of waste heat from existing and/or future nuclear sources.
- Successful operation at 900ºC promising for use in new nuclear plants.
- Addresses volume and cost objectives for hydrogen production.
- This program can have a big impact on large-scale hydrogen production.
- The project is extremely relevant and the PI did an excellent job of demonstrating this relevance.

Question 2: Approach to performing the research and development

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

- Good characterization of project. Appears to be well thought out. Good partners are identified.
- Provide coordination and developing alternatives and testing to: (1) understand chemistry and temperature of various heat exchange mechanisms and production processes; (2) identify candidate materials; (3) establish test program to evaluate the materials and processes for operation at 900ºC or 600ºC; and (4) should focus on one temperature range rather than another.
- The approach to separate the S-I thermochemical cycle is a good step. However, material selection seems to be taking place in an empirical way and only through a literature search. It ought to happen more systematically on the basis of failure mechanisms, such as creep, creep-related damage, stress corrosion cracking, stress design requirements, microstructure stability, reliability, etc.

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

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

- Good progress so far. Identified two sets of materials for S-I/Ca-Br.
- 2004 start, 10% complete.
- Assessment of NHI thermochemical cycle service conditions and candidate materials.
- Reported on materials requirements for nuclear hydrogen generation.
- Isolated chemicals and temperatures for H_2SO_4, HI and CaBr reactions.
- Materials testing for NHI cycles initiated; refractory metals showing best corrosion performance, so far.
- Good progress was made including several publications.
- PI knows the area well, so expect significant accomplishments as the project matures.

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

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

- Good collaboration with GA, French, UNLV and other partners.
- Project enables transfer and collaborations among partners in industry, university, and labs.
- Cooperation and benchmarking with similar efforts internationally like Japan's pilot process.
- Very long timeline for commercialization at scale benefits greatly from coordination function as well as transfers and collaborations.
- The project has a good coordinated team.
- Even though the project is well thought out and its relevance well documented, coordination with other project was not that transparent.

Question 5: Approach to and relevance of proposed future research

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

- Good proposed research. PI will continue to work with partners.
- Revise and update material selection document.
- Develop a prioritized, integrated, materials evaluation program.
- Clear project plan with decision points in lab scale stage.
- Might benefit from separate tracks for high and medium temperature environments.
- Future plan is sound but not much detail identified.
- Extremely vague.

Strengths and weaknesses

Strengths
- Addresses a promising path for high volume, climate neutral, inexpensive, centrally-produced hydrogen.
- Could address either high temperature (900ºC), or medium temperature (600ºC). Project work is heat source independent -- higher temperatures targeting new nuclear plant waste heat.
- Medium temperatures have potential for, or are capable of, retrofitting to existing nuclear plants.
- Provides coordination and direction for the first stage of a long term multi-player project
- Work applicable to other heat sources.
- Good experienced team facing a large and difficult problem.
- The strength of the project is the PI. He is extremely knowledgeable of the overall engineering aspects of the project, but he needs to take charge and address issues of mechanism-based approach versus empirical approach on issues such as material selection, etc.

Weaknesses

- Numerous safety issues need to be addressed
- A very broad range of materials will need to be evaluated which will be a challenge for this project.
- Pressure differentials are important as far as the mechanical response of components is concerned, and need to be reported in the process of material selection.
- The project needs to go beyond the empirical selection of materials through a literature search. It is difficult to believe that SiC composites are materials suitable for heat exchangers.

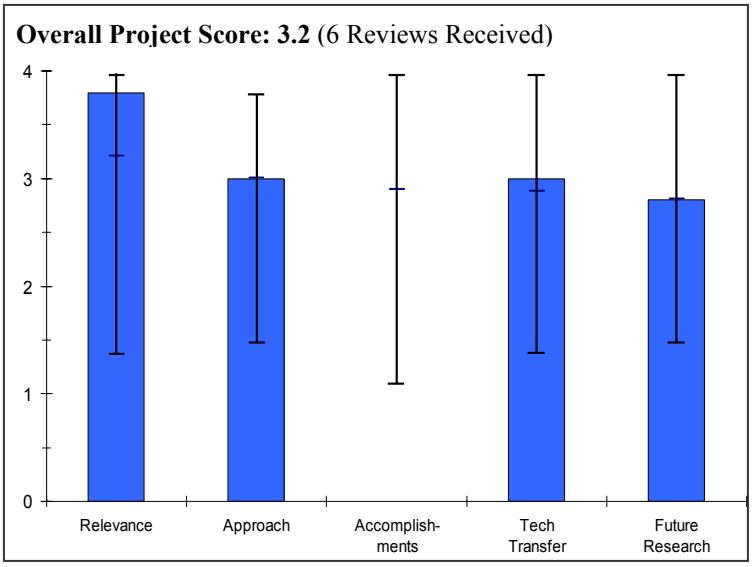
Specific recommendations and additions or deletions to the work scope

- Continue with research to narrow down potential materials.
- Project should make middle temperatures the priority to allow more heat sources, including existing nuclear heat exchange systems
- Depending on Japanese pilot experience, glass should be considered as a possible vessel material.
- Safety must consider interactions with nuclear or other plant components.
- List of selected materials re-written on the basis of operation requirements. For instance, classify materials based on their creep resistance, corrosion resistance, etc. Then weigh the importance of creep resistance versus corrosion resistance and look for optimized solutions. In other words, the project needs a systematic plan of stated material requirements, failure mechanisms, reliable operation condition requirements, and then an optimization of all these parameters based on appropriate and well thought out weight factors.

Project # PDP-24: A Novel Membrane Reactor for Hydrogen Production from Coal
Lau, Francis; Gas Technology Institute

Brief Summary of Project

In partnership with the National Energy Technology Laboratory, Illinois Clean Coal Institute and American Electric Power, Gas Technology Institute is conducting research on the technical and economic feasibility of a membrane reactor coupled with a coal gasifier for clean, efficient, and low cost production of hydrogen from coal. The project will screen, test, and identify potential candidate membranes under high temperature and high pressure coal gasification conditions. The best performing membranes will be selected for preliminary reactor design and cost estimates.



Question 1: Relevance to overall DOE objectives

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

- Lowers cost of hydrogen.
- Relevant to hydrogen-from-coal program.
- Aims to reduce the cost of hydrogen production.
- Success should lead to reduced cost of production from coal, natural gas, and biomass.
- The project is well aligned with the President's Hydrogen Fuel Initiative.
- The proposal work, if successful, should lead to significant improvement of the current coal to hydrogen production technologies.

Question 2: Approach to performing the research and development

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

- Permeation apparatus design and construction.
- Membrane materials development, screening and testing at high temperatures and pressures.
- Conceptual modeling and design of membrane reactor.
- Process evaluation and economic evaluation.
- Putting the membranes inside the gasifier is not a good approach -- extremely difficult for the membranes to survive.
- Feasibility studies an excellent way to start research project. Provided a more focused effort.
- Studying several good candidate membranes.
- Partners being able to provide membranes lets PI focus on areas of institutional strength.
- Testing apparatus makes research go more smoothly.
- Excellent series of experiments on temperature dependency and stability.
- Should also study purity as a function of temperature and stability.
- Planning on conducting studies of flux as a function of material area. Excellent that this was first modeled.
- Improvements critically related to availability of robust membranes.
- The approach is technically sound and adequate for this level of effort.
- Approach has included both experimental and modeling work.

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

This project is just starting and therefore was not scored on technical accomplishments, although reviewers provided the following preliminary observations:

- Constructed and commissioned high temperature/high pressure (1100ºC /1000 psi) membrane permeation unit.
- Developed membrane fabrication techniques.
- Demonstrated reasonable hydrogen flux for proton-conducting parasite membranes.
- Began chemical stability evaluation (stable in 250 hr pure H_2 feed, Zr doping shows good CO_2 stability, Zr doped BCN disk shows short term H_2S stability).
- Developed reactor model.
- Project is not kicked off yet.
- Work done using their internal funds was presented.
- Very low flux numbers obtained.
- Modeling work has been done for this concept.
- Set of very informative results.
- No data on purity.
- Very informative modeling results.
- Modeling provided good data to guide experiments.
- The project has made significant progress in developing suitable membranes and testing them in a high pressure and temperature device.
- Developed a model to evaluate the process.
- Results on experimental and modeling work were obtained.

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

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

- Partners with Arizona State University, University of Florida, and NETL.
- Collaborations with two universities.
- The two professors are well-known for their work in the development of membranes.
- Good coordination and role assignments between collaborators.
- In speaking to PI, learned that they will also work with Wah Chang.
- The project lists two partners from academia. It is unclear whether these partners can continue the project in the absence of DOE funding.
- There are no industrial partners for this project.
- More interactions are needed to seek inputs from industry (e.g. in areas of coal gasification and syngas cleanup and conversion to hydrogen).

Question 5: Approach to and relevance of proposed future research

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

- Flow sheet simulation.
- Chemical stability improvement.
- Hydrogen flux improvement (reduce thickness from 200 to 20 micron, dual-phase membranes, from 1 to 100 scfh/sqft).
- Reforming catalyst development.
- Simulated syngas testing scale-up.
- Mixed-conducting ceramic membranes show very low flux values -- why continue work in this area?
- Preliminary data shows the flux actually decrease with pressure over 4-5 atm.
- A timeline for achieving plans should be included.
- Which syngas will be tested?
- PI should make plans to test membrane with contaminants (S, alkali, ash, etc.).

- The only portion remaining under future work within this project is the completion of the technical and economic assessment of the membrane gasifier. All other items described will require new funding.
- Much more work is needed to prove out the feasibility of this challenging technology.

Strengths and weaknesses

Strengths
- Steady progress.
- Modeling work.
- Membrane test facility.
- Good team of researchers.
- A novel technical concept of accomplishing hydrogen separation within the gasifier itself, thus reducing the number of processing steps.
- Developed parasite membranes that can withstand high temperatures and pressures while producing sufficient flux.
- Has prepared ITM membranes and conducted hydrogen flux measurement at high hydrogen pressures at high temperature. Conceptual coal gasification configuration was presented to include the membranes. Preliminary membrane thermal stability data were obtained under simulated conditions.

Weaknesses
- Need long-term flux, chemical and mechanical stability testing in gasifier conditions (future).
- Working with mixed-conducting ceramic membranes with very low flux values.
- Trying to put the membranes inside the gasifier? Difficult for the membranes to survive.
- One of the project objectives to determine the technical and economic feasibility of a membrane reactor coupled with a coal gasifier was accomplished through theoretical modeling. The conclusions drawn from this modeling effort were already known in the literature (Hydrogen from Coal, David Gray, Mitretek, 2002), so it is unclear why a similar effort was undertaken by the PI.
- Mechanistic details of the model and assumptions made should be provided to understand the algorithms used in the model.
- It is also unclear whether kinetics can become rate limiting due to catalyst poisoning in the WGS reaction by the build up of non-permeates.
- Plot of hydrogen flux dependence vs. hydrogen pressure showed a peak in the mid- hydrogen pressure range. Inadequate discussions in the importance of these data , if any.

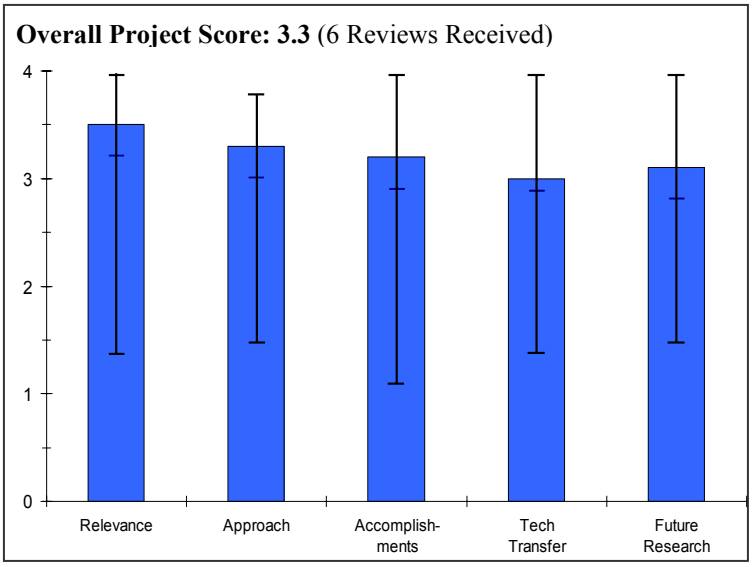
Specific recommendations and additions or deletions to the work scope

- Investigate fluid dynamics of conceptual membrane gasifier.
- Delete the effort on developing mixed-conducting ceramic membranes.
- Add efforts on dual-phase membranes that show promising flux numbers.
- An experimental verification of the model's findings, i.e., does the process intensification concept proposed here matches the theoretical findings of improving hydrogen production by 30-50%? Or are there unknown factors not included in the theoretical model that can negate this finding?
- Need to define the target hydrogen flux for membrane based on PI's know-how on local gasification. Need to prove out that membrane performance is not affected by the presence of particulates present in coal-based syngas.

Project # PDP-25: Hydrogen Separation
Killmeyer, Richard; National Energy Technology Laboratory

Brief Summary of Project

The overall goal of this project is to investigate, develop, and evaluate separation membranes to enhance hydrogen production from fossil fuels. Of interest are robust membranes suitable for the rapid, selective removal of hydrogen or carbon dioxide from various gas streams, while remaining resistant to impurities.



Question 1: Relevance to overall DOE objectives

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

- Lowers cost of hydrogen.
- Addresses barriers in Fossil Energy's Hydrogen from Coal RD&D Plan.
- A cost-effective coal gas derived hydrogen production technology, such as the project R&D aims to develop, responds to the Hydrogen Fuel Initiative mission, and the work scope of the project is consistent with DOE Hydrogen Program goals.
- The work scope is adequately planned and successful results for scale-up and technology demo is likely.
- The barrier issues are listed and the research efforts are focused on overcoming key technology barriers.
- Some definition of a success criterion such as, the cost of hydrogen production or meeting the DOE target, and how the R&D team plans to accomplish this milestone, would have been appropriate.
- Relevance critically related to availability of robust membrane.
- The project is well aligned with the President's Hydrogen Fuel Initiative and has identified the barriers that it is striving to overcome.
- Work proposed should help advance the hydrogen separation technologies.

Question 2: Approach to performing the research and development

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

- Water Gas Shift (WGS) membrane reactor development.
- Pd-Cu membrane testing of sulfur resistance.
- Novel hydrogen production and separation membrane development.
- Experimental approach is geared toward achieving the project milestones.
- NETL is focusing on developing Pd-based membranes.
- The researchers are using innovative approaches to provide solutions, explains advantages and differences from the current knowledge base, and generating a data base en route to a novel technical know-how for hydrogen production.
- The PIs recognize the technology R&D needs and has proposed work to overcome the technology barriers for scale-up leading to commercial maturity of the Pd alloy membrane system for hydrogen production.
- The approach is technically sound for a preliminary evaluation of the concept of incorporating a Pd-Cu based membrane reactor to perform the WGS reaction and hydrogen separation.
- Has proposed an approach including fundamental studies.

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

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

- Demonstrated steam and CO effects with temperature on Pd-Cu permeability and morphology.
- Demonstrated high permeability area (bcc) of Pd-Cu alloy phase diagram is subject to H_2S poisoning but the fact phase is resistant.
- Pd-based ternary alloy tested.
- Completed performance tests for ANL, ORNL, Eltron, and Synkera collaboration partners.
- Recent publication in *Science*.
- Steam was shown to have negligible effect on hydrogen permeation.
- Membrane surfaces were pitted after exposure to steam and CO; reasons not known.
- Good number of publications.
- CO slightly reduces the hydrogen permeation.
- Planned tasks and current accomplishments are conducive to achieving project goals.
- The schedule is consistent with DOE Hydrogen Program requirements.
- The project is progressing successfully along technical baselines towards a meaningful outcome.
- The project has made stepwise progress in the determination of the feasibility of membrane reactors in gasifiers and has preformed studies addressing key questions (e.g. effects of trace impurities on pd member separation efficiency)..

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

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

- Collaboration with ANL, ORNL, Eltron, and Synkera (performance testing).
- Collaborates with ANL, ORNL, GTI, and REB, Inc. (Muxbaum).
- The project is collaborating with other National Labs, research institutes, and a consultant.
- Most of the work is done at the project site.
- The project lists three National laboratories and one industrial company as partners. While technical input is provided by these partners, it is uncertain if these partners can financially sustain the project in the absence of DOE funding.
- More interactions with industries are needed to seek inputs to R&D work.

Question 5: Approach to and relevance of proposed future research

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

- Membrane pitting tests
- Test CO_2, NH_3, and HCl contaminant effects on Pd-Cu membranes.
- Conduct WGS membrane reactor tests in fabricated Pd-Cu helical coils.
- Conduct WGS experiments in presence of H_2S.
- Determine mechanism of S poisoning and resistance of Pd-Cu alloys.
- Continuing to do what they have done already (for most part).
- Trying ternary alloy is new work for the future.
- Legitimate future work has been proposed and can be related to the original objectives and intended outcomes of the work.
- Performance improvements using different membrane reactor configuration will be studied.
- Effects of other contaminants present in coal gas will be studied.
- Insightful mechanistic studies on why Pd-Cu alloys are sulfur tolerant will be conducted.
- Find and test other (better) membranes.
- The future work is a logical extension of past findings and focuses on further investigation of observations such as the influence of contaminants, time dependence of membrane pitting, and reactor configurations.

- Future work should investigate hydrogen production with and without the integrated WGS membrane reactor to experimentally verify theoretical projections of hydrogen production cost savings.
- Has proposed future research to assist industries to evaluate membrane effectiveness for hydrogen separation.

Strengths and weaknesses

Strengths
- Developing excellent membrane testing capabilities.
- Membrane test facility.
- Experience with fossil resources.
- Excellent team and& collaboration with top-notch institutions.
- Good number of publications.
- The researchers have recognized that pitting is a failure mode and will investigate further.
- The development of the Pd-Cu phase diagram will add value to membrane science information base.
- This R&D has made very interesting observations and reported new findings.
- The researchers have developed an fcc phase with sulfur resistance.
- A novel technical concept of accomplishing the WGS reaction and hydrogen separation within the gasifier itself, thus reducing the number of processing steps.
- Membrane permeation tests show sufficient flux at gasification temperatures. Other useful findings regarding the effect of factors such as steam and H_2S have also been obtained.
- Quantitative data were presented to show the effects of CO, H_2S and steam on H_2 separation using Pd-Cu alloys. Efforts have begun to study membrane morphology before and after hydrogen separation study.

Weaknesses
- Look at additional membrane thicknesses and types.
- Why not look into other alloys? Why just Pd-Cu?
- Membrane fabrication.
- The PIs are conducting experiments outside the desired or anticipated commercial operating ranges.
- The PIs recognize the barriers and may wish to refocus their efforts more towards technical maturity issues en route engineering scale-up and technology demonstration.
- Some of the project results, although obtained by advanced characterization techniques and approaches, do not necessarily advance the status of science; instead re-confirms known info.
- The PIs seem to be conducting experiments outside the desired or anticipated commercial operating range targets.
- Flux data at lower temperatures (~ compatible with warm gas clean up temperature) in the presence of CO and H_2O (or the sulfur-tolerant fcc phase) does not appear to be commercially significant, but the researchers have not adequately analyzed this observations or address this performance issue.
- Figure titles would make the information in the figures more readily comprehensible to the reader.
- One of the project objectives to determine the technical and economic feasibility of a membrane reactor coupled with the investigation of different Pd-Cu alloy compositions (0-100% Pd) at three different temperatures to determine their sulfur tolerance may take up an extended period of time due to the infinite number of binary and ternary alloy combinations.
- It is also unclear whether kinetics can become rate limiting due to catalyst poisoning in the WGS reaction by the build up of non-permeates.
- Pd membranes used were thick and gave low hydrogen permeability. No separation factor data were reported. Duration of membrane testing was not discussed.

Specific recommendations and additions or deletions to the work scope

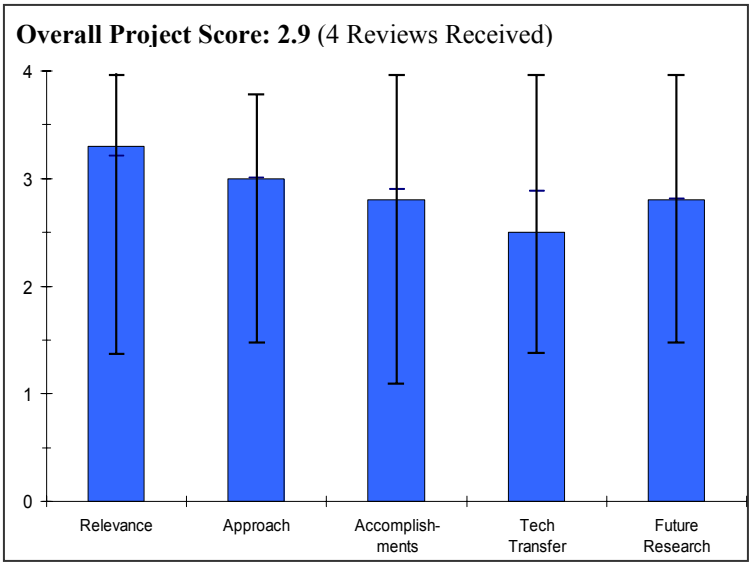
- Potential to expand membrane types and partners.
- Search for other alloys.
- Understand the reasons for pitting.
- Computational analysis to develop new membranes.

- Utilize mathematical modeling to predict potential alloy combinations that can be subsequently investigated in the laboratory.
- Perform experiments to verify if the process intensification concept of combining the WGS reaction and hydrogen separation reduces cost by 30%.
- Work with Pd membranes which yield higher hydrogen permeability and become competitive with competing technologies. Conduct long-term life testing with Pd membranes exposed to impurities. Prioritize future work which is broad in scope. Expand capabilities to serve as a site to test hydrogen separation membranes submitted by industries and to meet the testing needs of industries.

Project # PDP-27: Startech Hydrogen Production
Lynch, David; Startech Environmental Corp.

Brief Summary of Project

Startech Environmental Corp. will field test integrated hydrogen production on a pilot scale using plasma gasification and ceramic membrane hydrogen separation, and evaluate commercial viability and scalability through extended operation under representative conditions.



Question 1: Relevance to overall DOE objectives

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

- The project involves plasma processing of municipal solid waste (MSW) streams into a hydrogen-containing stream which is then passed through a separator vessel to purify the hydrogen. The process could be applied to other renewable streams.
- The project does support a limited but important portion of the hydrogen RD&D plan.
- MSW is cheap feedstock for hydrogen.
- Innovative. Could be part of a novel pathway.

Question 2: Approach to performing the research and development

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

- The plasma equipment was previously developed. This project integrates a hydrogen separation membrane to produce high-quality hydrogen.
- It is not clear that compatibility of the membrane with expected process streams is being sufficiently established.
- Some concern due to complexity of the project and multiple new technologies (membrane, plasma, etc.)
- Good approach for MSW since no waste sorting required and high destruction rate.
- Good progress.

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

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

- The first attempt at making the separation unit failed because of materials issues. It seems that insufficient design and materials analysis was performed to choose an appropriate material.
- Problems with membranes has caused a delay in the overall programs objectives
- Reasonable progress. Need clear timeline.

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

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

- Startech makes plasma equipment. Their partner makes ceramic hydrogen separation equipment.
- This project could benefit from additional university and national lab coordination.
- Good with membrane manufacturer. May need more interaction with other entities.
- Not overly broad yet. Perhaps due to the early proprietary work?

Question 5: Approach to and relevance of proposed future research

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

- More materials and design analysis should be performed.
- Having a backup membrane provider might have kept the project on track
- Should include process economics & energy balances very early. Need to compare with other hydrogen technologies. Electricity cost should be included.
- Need to address materials handling and solid by products as well.

Strengths and weaknesses

Strengths
- The system hardware appears to be completed now and ready for testing.
- Unique idea that may have applications to other areas such as medical waste treatment.
- Interesting approach. Low cost of MSW makes it a good potential feedstock.
- Novel approach.

Weaknesses
- Very little progress was made on the project.
- No experimental results to date.
- Electrical energy input carries generation inefficiency with it.
- Need heat and material balances to understand how process will work, i.e., efficiency of waste heat recovery & full utilization of process heat.
- Still early in the program, so hard to judge

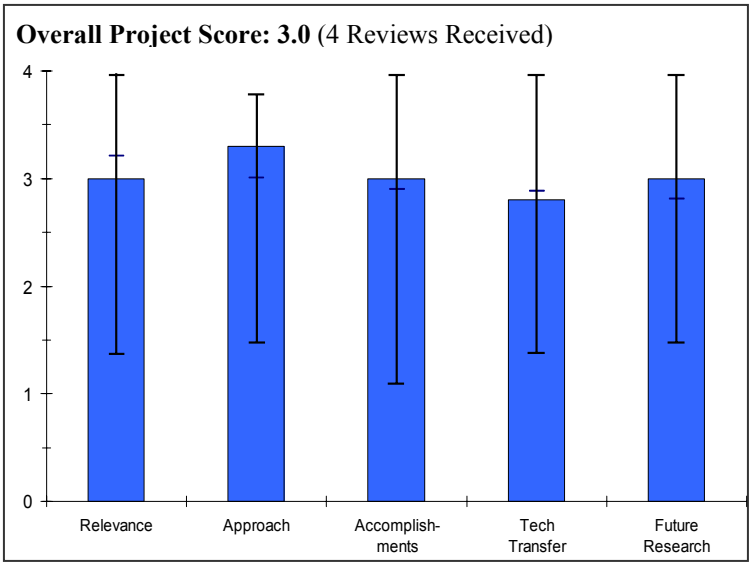
Specific recommendations and additions or deletions to the work scope

- The project needs to demonstrate results soon to show if this overall approach has any merit.
- Economics and energy balance need to be done ASAP.

Project # PDP-28: EVermont Renewable Hydrogen Fueling System
Maloney, Tom; Northern Power Systems

Brief Summary of Project

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; and show viability of distributed production pathway.



Question 1: Relevance to overall DOE objectives

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

- Most aspects of the project align with the President's hydrogen vision and program objectives.
- This project addresses improved electrolyzer efficiencies and reduced cost for power electronics, which area key elements in the HFCIT MYPP.
- Project is fairly well aligned with overall program goals

Question 2: Approach to performing the research and development

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

- Addressing the renewable integration and the power electronics design/cost is a significant step towards making these systems practical.
- The approach is generally well thought out and effective but could be improved by integration with other renewable electrolyzer projects. Most aspects of the project will contribute to progress in reducing the capital costs of electrolyzers.
- Process configuration options have been considered

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

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

- Project is making significant progress on cost and efficiency of cell and power electronics.
- The project has shown significant progress toward against its objectives and to overcoming the barrier of higher cost electrolyzers. Further research and funding is needed in the area of reducing electrolyzer capital costs.
- Potential for using different operating configurations is interesting and may provide some of the most interesting results.

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

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

- Collaborations appear to be compartmentalized around system components with little technology transfer between organizations.
- A little coordination exists; full and needed coordination would take significant time and effort to initiate. This project should collaborate with other renewable-electrolyzer projects.
- Should further develop a strong public relations and education component.

Question 5: Approach to and relevance of proposed future research

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

- Demonstration of cell design and power electronics in fueling station application to prove technology is significant next step.
- Development of power electronics has potential for further cost and efficiency improvements.
- Although future funding may not exist, this project shows excellent capacity for meeting DOE cost goals in the future and addresses key technical barriers.
- Should continue to pursue additional vehicles to increase the hydrogen use.
- This is a one-year earmark, and they appear to have very little planning for future development.

Strengths and weaknesses

Strengths
- Development of cell and power electronics to improve cost and efficiency.
- Demonstration in fueling application.
- Excellent improvement in electrolyzer efficiency.
- Good improvements in reducing power electronics costs.
- This company should examine the power requirements for specific applications and integrate designs for the power electronics to those markets.

Weaknesses
- Look at opportunities to lessen the dependency on the grid for power when wind is unavailable.
- This project should focus more on the improvements of the electrolyzers and not demonstrating the technology until ready.

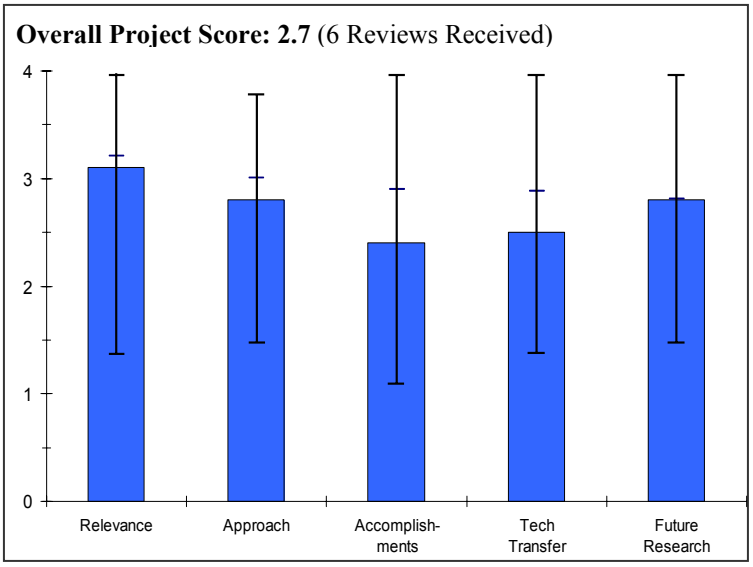
Specific recommendations and additions or deletions to the work scope

- Document progress against the DOE targets as the project proceeds.

Project # PDP-33: Discovery of Photocatalysts for Hydrogen Production
MacQueen, Brent; SRI International

Brief Summary of Project

This SRI International and NanoGram Corporation project addresses efficiency, durability and cost of photocatalyst materials for use in direct water-splitting systems for the production of hydrogen. The materials discovery required to meet the technical targets will be expedited by the use of high throughput screening tools being developed in this project. The inclusion of a partner with the means to produce commercially relevant amounts of materials will hasten the development required to make photoelectrochemical hydrogen viable.



Question 1: Relevance to overall DOE objectives

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

- Materials discovery is at the core of this research area. High throughput screening of bulk materials is an important area and should compliment the combinatorial and directed research approaches.
- The approach of using photocatalysts for the production of hydrogen is based on well established concept of 70's. The PI's proposal to improve the efficiency of water splitting reaction by the discovery of low cost materials with improved efficiency is relevant to the program
- Semiconductor nanorods are an innovative approach to single bandgap photoelectrochemical converters. The approach makes clever use of the novel quantum properties of nanostructure inorganic structures. In principle, this approach has the potential for high STH conversion efficiency. These devices will display low current -- a trait shared by triple junction III-V photovoltaic cells. However, an advantage with respect to triple junction cells is the much lower cost of materials. The project is highly focused on the DOE goals.
- Goal for discovering materials enabling high efficiency photoelectrolysis of water is fully aligned with DOE goal of finding efficient, cost effective paths to hydrogen using renewable energy sources. However, the development of high volume synthesis approaches is premature until promising classes of effective materials can be identified.

Question 2: Approach to performing the research and development

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

- It is unclear if this project is employing a sufficiently targeted approach to yield any kind of progress towards the technical targets.
- High-throughput screening can be useful. However, this project seems to be driven solely by industrial partner's area of interest, rather than on screening a wide array of materials to yield valuable information on the chemistry and bulk properties that could help guide future research.
- The approach of large volume nanoparticle synthesis and high though put screening has good scientific merit and this approach has helped researchers to identify useful photocatalysts. However, failure to implement additional characterization and analysis (see comments from past reviews) has limited the progress of this investigation.

- The approach is a good mixture of experiment and theory. The research is both high risk and high payoff in nature. However, present results appear to be quite limited. The project does not appear to be sufficiently thought out with respect to a logical train of experimental results.
- The approach is effective for identifying materials for testing.
- Somewhat hampered by partner's ability to produce samples.
- The approach employed involves the attempt to tune electronic properties of solids by creating nanoparticles, doping oxides with nitrogen, and investigating new candidate materials such as AgInZnSe. Theory is nicely used to justify the new materials approaches, and high throughput methods are effectively used to improve efficiency of screening many materials at the same time.

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

This project was rated **2.4** based on accomplishments.

- Does not appear to be benchmarking materials selection against what others in the field are doing or reporting. Still not making a strong effort to understand the properties that lead to success/failure.
- The project has shown modest progress with no major outcome in tackling low photoconversion efficiency.
- TiOxNy and TiOxSy materials developed in this study exhibit improved efficiency relative to TiO_2; however, the efficiency of these catalysts remains low. It is still a factor of ~10 lower than 2008 target.
- Demonstrated the ability to tune the bandgap of GaxIn1-xP.
- Photostability issues as well as the development of a reactor for water splitting reactions have not been addressed fully.
- The team has made good progress in characterizing the nanorods and determining the relationship between the geometry of the rods and their performance. Technical barriers are identified and there are well-defined approaches to their solution.
- Project has identified some interesting materials
- The researchers did a lot of work in studying many materials, but the results with the classes of materials tested proved that significant improvements in efficiency are needed to begin to approach efficiency targets. Near term stability improvements are promising, with longer term tests in progress on TiOS.

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

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

- PI needs to coordinate much more closely with others working in this field. Seems to be more focused on marketing the capability rather than leveraging the technical success of the project.
- The researcher has cited Nanogram as their partner.
- Academic collaboration would have helped these researchers to further understand the problems associated with the improving photocatalytic efficiencies.
- The researchers represent the industrial community. There is accordingly good transfer of technology.
- Project would benefit from additional collaboration or communication with other related projects
- Better coordination and sharing with groups at Santa Barbara, Hawaii, NREL possible.

Question 5: Approach to and relevance of proposed future research

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

- Again, materials selection should be guided by known properties and insight into possible success. Future work seems to be guided more by the next funding opportunity than on achieving the goals of the project.
- No effort has been made to make use of the catalysts developed in the current phase of work.
- The core shell structures involving metal chalcogenides suffer from long-term stability issues. (Lack of oxygen production in such systems has been demonstrated in earlier work. The PIs should refer to literature published on the water-splitting reaction using metal chalcogenides)

- Future research is focused on development and characterization of mixed phase materials. This clearly builds on past progress.
- Proposed work is an important next step
- Promising lead (mixed phase nanorods) identified as new class of materials for future investigations. Need to better illustrate how these materials will be characterized.

Strengths and weaknesses

Strengths
- Good materials development and screening capabilities.
- Developed a high through put screening procedure for photocatalyst evaluation.
- Identified N- and S-doped TiO_2 as the possible photocatalysts with improved performance.
- Reasonably focused approach, with a good mixture of modeling and laboratory data.
- Good mix of modeling and experimental work in the overall efforts.

Weaknesses
- Project focus seems to be more on developing capabilities than on materials discovery. Little progress has been made on advancing the science. In many aspects, the project appears to be duplicative of what others are doing, without contributing to any unique understanding on its own.
- Limited success to achieve the set goal of achieving higher photocatalytic efficiency and failure to understand the problems associated with S- and N-doped TiO_2 photocatalysts.
- The proposed core-shell structures involving chalcogenide semiconductors are susceptible to photocorrosion and needs better understanding of the photoinduced processes in such core-shell semiconductor systems.
- The quantum mechanical basis for the high potential conversion efficiency of the nanorods does not appear in the slides. Such development would add to the cogency of the approach. Additionally, the slides do not indicate a clear definition of problems that need to be solved.
- This area is in need of additional materials concepts to test for photoelectrochemical water splitting. This is a comment that applies to all the projects in this area.

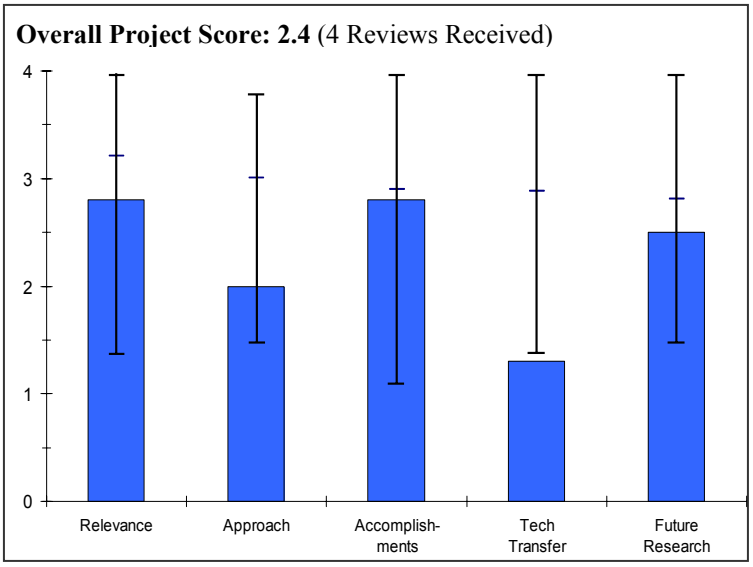
Specific recommendations and additions or deletions to the work scope

- The researchers should not abandon the efforts of previous investigation. They need to continue to build composite structures based on such oxides. Switching over to chalcogenide based core-shell structure may look scientifically sound, but is likely to suffer from photostability issues and lack of oxygen production. Fundamental understanding of the differences between oxide and chalcogenide photocatalysts in water splitting reaction is important in pursuing the research goals.
- It would be interesting if the research team developed a discussion of how the nanorod approach compares with that of quantum dot sensitized TiO_2 photoelectrochemical systems?
- It does not appear reasonable that SRI will come close to meeting their target efficiency based on progress to date and the short amount of time left before project completion.

Project # PDP-39: Hydrogen Production - Increasing the Efficiency of Water Electrolysis
Pile, Donald; Sandia National Laboratories

Brief Summary of Project

Sandia National Laboratories intends to improve the efficiency and consequently lower the cost of the electrolytic production of hydrogen through higher activity catalysts and more conductive membranes. Specific objectives of the project are to prepare polymer thin films as low resistance, hydroxyl ion conducting membranes and evaluate their electrochemical performance as electrolyte/separator in alkaline-based water electrolysis cells; and prepare and electrochemically evaluate transition metal (e.g., Mo) macrocycle complex-based electrocatalysts as low-cost, high catalytic materials for hydrogen evolution.



Question 1: Relevance to overall DOE objectives

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

- The project is relevant but not critical.
- Lower cost materials are needed to achieve the stack cost targets for electrolysis.
- The project does support a limited but important portion of the hydrogen RD&D plan. This program supports alkaline electrolysis material development.
- Program proposed replaces low cost electrolyte (KOH) and asbestos diaphragm with higher cost membrane.
- Benefits of membrane over KOH not identified.
- Cost analysis not reported.
- Catalyst not tested at operating temperature of electrolysis.

Question 2: Approach to performing the research and development

This project was rated **2.0** on its approach.

- Uses alkaline electrolyte -- a different approach.
- Technical feasibility is uncertain because of the novelty of the approach and the proposed materials.
- Would benefit from a strong experimental design for the materials synthesis and characterization activities.
- The PI has a good background in polymer membrane chemistry and has identified some possible new membrane materials.
- The methodology or selection process for determining new candidate membrane materials has not been clearly demonstrated.
- Not clear how stability of membrane will be improved using amine linkage. Does not address C-H bonds
- Did not compare conductivity to KOH. Majority of commercial electrolysis systems use KOH.
- Cost benefit for developing a membrane was not identified in presentation. Approach should identify the benefits of developing the membrane.
- Program should conduct testing at true operating temperatures of electrolysis

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

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

- Progress has been made but results are disappointing and the possibility of future success is not encouraging.
- Given the limited funding there are some interesting results in the membrane development task.
- Need to improve materials synthesis to lead to more reproducible results.
- The future work for this project should concentrate on membrane material development.
- Work on macrocycle catalyst should be discontinued in favor of membrane material development.
- Progress on catalyst was good. Researchers recognized the limitation of the catalyst and changed direction of program.
- New membrane offers promise but needs to have conductivity improved.

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

This project was rated **1.3** for technology transfer and collaboration.

- The poster did not contain any information about collaborations
- The team would benefit strongly from industrial partners, particularly in the membrane development area.
- Need to publish results and submit to a robust peer review.
- The PI discussed some collaboration but more is needed for this project to be successful.
- None identified.

Question 5: Approach to and relevance of proposed future research

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

- The membrane future work is logical and fairly specific.
- The catalyst future work involves changing both the materials and synthesis routes. This is an indication that the initial idea was not well-conceived and that the proposed changes are not ensured of success.
- There does not seem to be any go/no-go decision point or criteria.
- Need to develop a detailed plan for membrane synthesis and set targets for performance and durability testing.
- Future plan for membrane material development appear to be adequate.
- Future program does not address replacement of KOH but compares to commercial membranes. Need to compare to commercial competition which has liquid KOH.
- Future program should address the solubility of transition metal catalyst at high temperatures when in contact with hydroxyl membrane

Strengths and weaknesses

Strengths
- PI has a good electrochemical background
- The PI has shown some progress in creating materials with high ionic conductivity.

Weaknesses
- The project could benefit from additional collaborations with others in this field.
- The program does not identify the cost benefit of developing a membrane compared to KOH/asbestos diaphragm technology that is the real competitor in commercial electrolysis.
- Program does not identify industrial partners

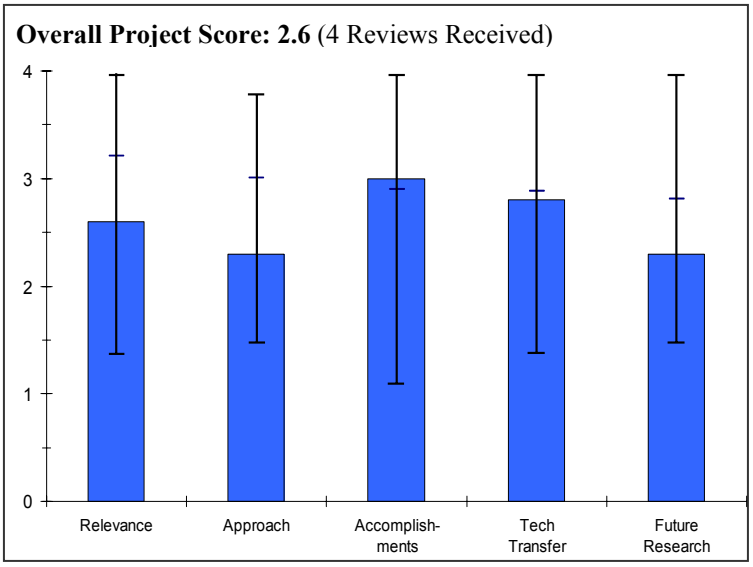
Specific recommendations and additions or deletions to the work scope

- Need to conduct a technoeconomic comparison sooner rather than later to determine the potential impact on hydrogen production cost; benchmark against current membrane and catalyst cost and performance.
- Stop catalyst development portion of project.
- Conduct solubility test of proposed catalyst prior to testing with membrane.
- Conduct cost analysis to determine if membrane replacement improves the overall cost of electrolysis equipment.
- Cost to manufacture macrocycle materials should be evaluated as they may be expensive.

Project # PDP-41: Hydrogen Generation from Electrolysis
Porter, Stephen; Proton Energy Systems

Brief Summary of Project

Proton Energy Systems and its team will determine pathways to optimum electrolysis-based hydrogen fueling through conceptual system design and component/system development. They will develop the requirements for the fueling system, optimize fueling system designs through systems analyses, and conduct R&D to improve component performance, cost, and/or durability.



Question 1: Relevance to overall DOE objectives

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

- Focus of work is clearly tied to the programmatic goals; however, need to do a better job of benchmarking against RD&D technical targets.
- Will the potential improvements identified achieve the HFCIT technical targets for electrolysis?
- This project is a straightforward attempt to improve the efficiency, durability, and cost of a membrane-based water electrolysis cell that delivers hydrogen at high pressure.
- It is not deep science or leap frog technology, but it does align with some near term visions for hydrogen utilization.
- The project does support a limited but important portion of the hydrogen RD&D plan.
- Researchers did not identify the original catalyst loading they were replacing.
- Report relative performance improvements but do not report absolute improvement values
- Not clear if they are truly approaching beneficial cost reduction.

Question 2: Approach to performing the research and development

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

- Appear to be targeting the improvements that will make the biggest difference in cost and efficiency.
- Need to focus more on improving the electrolyzer, rather than vehicle fueling.
- Need to ensure that a strong experimental design is used for studying durability and performance as a function of the multiple parameters being tested (temperature, catalyst loading, MEA).
- Need to define what the targets should be for meeting and exceeding conventional compression. Can a separate stack for electrochemical compression ever compete on a cost and performance basis?
- The project seeks to make improvements in overall MEA performance through optimization of core operating parameters, e.g., temperature, pressure, catalyst loading, MEA fabrication approach, etc.
- The approach is sensible and intuitive but not overly creative.
- The overall scope of the project is too broad.
- The project should emphasize electrolyzer efficiency improvements and electrochemical compressor development..
- Concept of using hydrogen pump to purify and pressurize the hydrogen needs to be experimentally proven.
- Research needs to include energy cost of drying the hydrogen. Comparison with diaphragm pump compares dry hydrogen to wet hydrogen; numbers not valid.
- Program needs to have cost analysis to demonstrate the benefits of hydrogen pumping compared to other pressurization/ purification methods.

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

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

- Considering the limited funding to this project, an impressive amount of revealing test data has been gathered since the start of the project.
- It is hard to appreciate the progress in terms of the pressing advances required to advance hydrogen utilization; however, from an incremental viewpoint, it seems that the project staff is doing a respectable job.
- Good progress was made during the past year on improving the efficiency of PEM electrolyzers on several different fronts.
- The project also made a good start on examining electrochemical compression.
- Comparison and test results offer promise that hydrogen pump concept will lead to purification process that can also pressurize the hydrogen.
- Data showing increase voltage with pressure confirms thermodynamic predictions.

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

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

- Good partnership development efforts.
- Two collaborating organizations are mentioned in the slides; it's not obvious how they contribute to the project just from looking at the slides of the poster.
- This project could benefit from additional university and national lab coordination.
- Program is operated by industry and transfer would appear automatic.
- Program could benefit from independent cost analysis.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Need to focus more on improving the electrolyzer system and on achieving the cost and performance targets.
- More R&D needs to be done to optimize operating conditions, catalyst type/loading, and membrane performance. Durability needs to be demonstrated over a wide range of conditions.
- Attention needs to be paid to balance of plant and the impact that will have on system cost and efficiency.
- Considering the scope and the goals of this project, the proposed future plans seem appropriate
- Future work should concentrate on electrochemical and electrolyzer portions of the project.
- Program needs to set hard targets for benefits obtained from this approach and set future programs to reach those targets.

Strengths and weaknesses

Strengths
- Strong team and a reasonable approach. Lots of operational experience that should benefit the project.
- The PI/presenter is clearly very knowledgeable about the technical details of the project; the presentation at the poster was very well done.
- The project organization has the means to do all that needs to be done to meet the project goals.
- The PI has a good background and experience in this field.
- Good progress was made on electrochemical portions of the project.

Weaknesses

- Need to benchmark cost reductions to achieving the technical targets. More work needs to be done on understanding the various trade-offs on cost, performance, and lifetime.
- It wasn't clear what the listed collaborations with Air Products or UC Irvine bring to the project.
- The most positive of outcomes for this project won't move hydrogen production capability forward by enough of an increment to make the most demanding production issues go away.
- Very little progress was made on the other portions of the project.
- Program reports no "hard" targets and only speaks of optimization. No measuring stick available to determine if program is really making progress toward its goals.

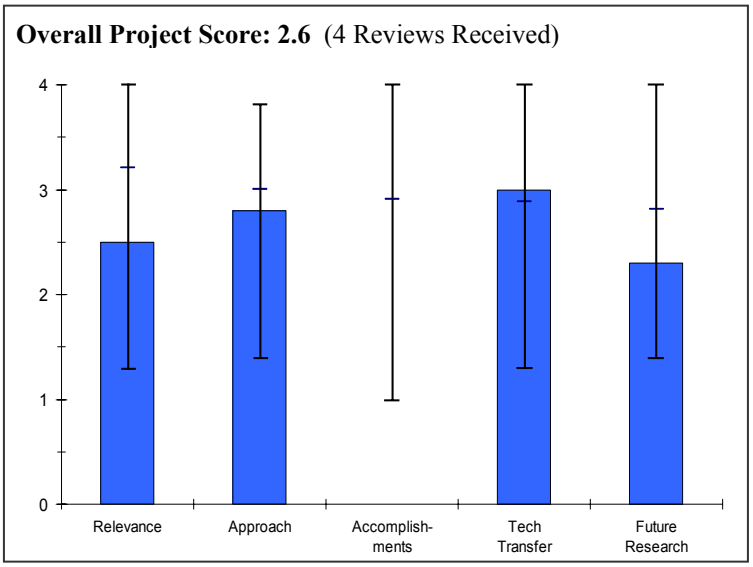
Specific recommendations and additions or deletions to the work scope

- Others are looking at fueling station designs. This project should focus on improving the electrolyzer system only.
- Give serious consideration to finding additional collaborators that can help move the potential positive benefits of electrolysis-based hydrogen production to a new level.
- For future reviews prepare a prior year to current year progress comparison to help gauge performance in the budget year.
- The project should concentrate on electrochemical compressor development, evaluation and testing.
- Add cost analysis and cost comparison to justify that hydrogen pumping for pressurization and purifications offers promise to meet DOE goals.
- Program should define limits to pressurization by electrochemical means. Can pressurization as high as 5,000 psi be reached using this method?

Project # PDP-42: System Design and New Materials for Reversible Solid-Oxide, High-Temperature Steam Electrolysis
Ruud, James; GE Global Research

Brief Summary of Project

GE Global Research, Northwestern University, and Functional Coating Technology, LLC will develop a pilot-scale, reversible SOEC system design capable of 1,000 kg/day hydrogen production at $2/kg based on new, low-cost, reversible solid-oxide electrodes. A system model detailed at the component level will define the plant configuration and operating parameters required to meet the cost targets. The materials will be developed and demonstrated in a multi-generational approach. The most promising materials system will be down-selected for extensive performance and durability mapping through the pilot-plant design operating space, and system performance will be predicted.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- Production of low cost hydrogen is a major DOE program goal.
- Elements of the scope of this project may overlap with other SOFC/SOEC work.
- The program is a start up effort without any experimental data yet.
- The operating temperature proposed will be as high as 800ºC and oxidation of nickel catalyst (at the electrolysis cathode) could result and should be avoided since this would result in a mole volume change every time the system is reversed (fuel cell to electrolysis to fuel cell) degradation of the catalyst would result.
- Program does not appear to address some fundamental properties of the electrodes

Question 2: Approach to performing the research and development

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

- Project approach involves modeling and system design focused on materials selection and electrode configuration for reversible SO operation.
- Demonstration and verification of design concepts should be included as a part of the technical approach.
- The approach builds on the SOFC anode support of the electrolyte concept. This may be a flawed approach because recent results from the SECA program indicate difficulties with this design.
- The program does not address sealing of the SOEC and depends on seals being developed in a separate SOFC program. This a weakness in the program.
- At high operating temperatures, migration of interconnect materials, such as chromium, can poison the cell. The program does not address this fundamental issue and appears to depend on the SECA SOFC program to solve all of the issues.

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

This project is just starting and therefore was not scored on technical accomplishments.

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

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

- Broad team strengths unknown.
- Good team proposed with university and industrial coating partners.
- University and industry collaboration has been proposed.
- Program presentation identified collaborators.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Future plans for design and modeling efforts are adequate, but inclusion of experimental program to verify design concepts and materials selection is needed.
- Project has not begun, and therefore cannot build on past progress.
- A schedule with milestones should be established for the future research.
- A go/no-go decision after the cost analysis should be in place.

Strengths and weaknesses

Strengths
- Strong, complimentary team.
- The program appears to be able to build on the activities in SOFC of the U.S. DOE SECA program and a synergistic benefit should be obtained.
- The cost analysis needs to be very rigorous so that direct comparison can be made to other methods of generating hydrogen.

Weaknesses
- Experimental verification component needed to demonstrate design concept and materials selection.
- The program has not established a clear timeline with staged/gate decision points to mark its progress. The program has a heavy dependence on the SECA SOFC program. If the SOFC program does not resolve the issue of seals, should this program be put on hold until resolution? What types of stabilities are anticipated for the interconnect materials used in the SOEC and does this problem depend upon the SOFC program?

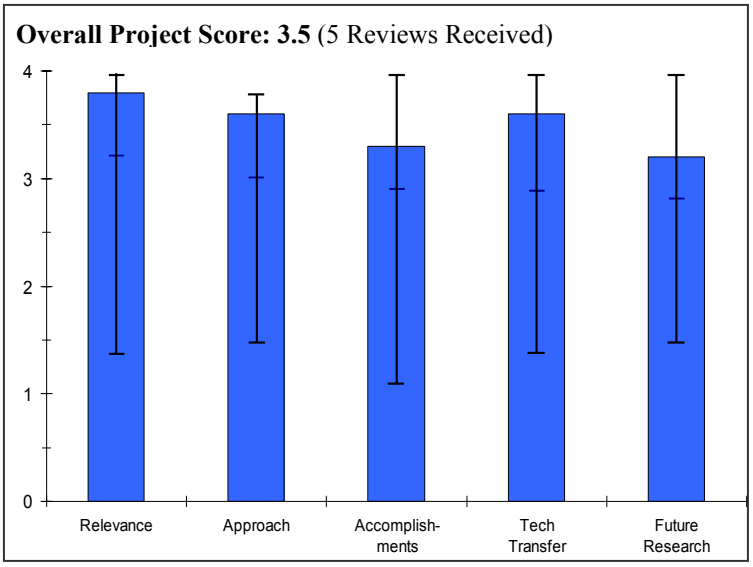
Specific recommendations and additions or deletions to the work scope

- Respond to weaknesses describe above.

Project # PDP-45: Hybrid Sulfur Thermochemical Process Development
Summers, Bill; Savannah River National Laboratory

Brief Summary of Project

This Savannah River National Laboratory project will address the key technology issues involved in the development of the hybrid sulfur (HyS) hydrogen production process. The HyS process is a highly efficient thermochemical water-splitting cycle that utilizes high temperature heat from advanced nuclear reactors or solar receivers to drive a two-step cycle involving only H-O-S chemistries. The key component, an SO_2-depolarized electrolyzer, will be designed and tested. The theoretical voltage to electrolyze water is reduced by 85% versus conventional electrolysis through the use of SO_2 depolarization. The electrolyzer development will focus on the adaptation of PEM technology for this new application.



Question 1: Relevance to overall DOE objectives

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

- Determining practicality/feasibility of thermochemical cycle hydrogen production at nuclear plants is an important initiative.
- Preliminary economic analysis indicates that the appropriate cost goals can be met.
- Fully supports the program goals of finding efficient ways to produce hydrogen from non-fossil fuel energy sources.
- This project has excellent alignment. In the end, hydrogen from water is the only option.

Question 2: Approach to performing the research and development

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

- Solid plan for the project, utilizing baseline flowsheets, AspenPlus software, advanced electrolyzer and PEM technologies, and small-scale testing.
- The project incorporates both past work and parallel on-going efforts, resulting in a high value-added for a relatively small investment.
- Process design flowsheeting and optimization is a key early in moving this technology option forward. Next natural step is to experimentally determine performance parameters of electrolyzer, as is planned.
- Focus includes experimental aspects of electrolysis applied to this process -- this is good and borrows from PEM development.
- Focus also includes modification of flowsheet to improve process efficiency.
- A key aspect is the decomposition of H_2SO_4 and the amount of SO_3 produced vs. SO_2. Even at 950ºC, 5 to 8 % SO_3 is formed based on thermodynamics. More emphasis should be placed on developing an understanding of this unit operation.

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

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

- DOE/NE getting excellent value for relatively small project investment.
- To overcome barriers, this work will have to be followed by more expensive testing and development work.
- Preliminary analysis indicates that the hybrid process has significant economic advantages.
- Process optimization is largely complete, and electrolyzer testing is just getting ready to start. Technical progress seems in line with project funding to date.
- Simulations completed for process, including various process options.
- Experiments started for electrolysis unit.
- Updated calculations made for efficiency and cost.

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

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

- System in place to share/exchange information with other projects using PEM technology.
- Collaborating with University of South Carolina, which is funded from sources outside the project?
- There are multiple collaborators that are helping to accelerate progress.
- Well-integrated with related work
- PI has appropriate expertise and experience to integrate past work and related on-going efforts into this project.
- Good mix of University, Industrial, and National Lab collaborators. Major collaboration with Proton Energy Systems on electrolyzer design and construction. Not clear what role of University of South Carolina has (but reviewer did not probe this area).
- Appeared to have good collaboration with partners, e.g. interacted with GA on S-I base and with partner to incorporate PEM knowledge with electrolyzer.

Question 5: Approach to and relevance of proposed future research

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

- Next step will require $2.5 million from industry and $1 million from DOE.
- Will be at least a brief break between completion of this work and beginning of follow-on phase.
- The proposed plan is appropriate.
- The suggested budget (increase of a factor of >10) seems optimistic.
- Path forward in electrolyzer testing is clear and relevant to program goals. Would be nice to see more detail on how researchers plan to characterize electrolyzer degradation modes in testing program.
- Future research is to experiment with the electrolyzer which is the right thing to do.
- Knowledge on efficiency and cost needs updated as progress is made.

Strengths and weaknesses

Strengths
- Effectively using models, experiments to perform sensitivity analyses and trade-offs.
- Economic analyses have benefited from involvement of multiple organizations -- General Atomics, Entergy, Bechtel, PEM cell manufacturers.
- Experimental setup for electrolysis using latest from PEM knowledge.
- Updated flow sheets with improved efficiency and economics.

Weaknesses

- Project utilizing a large number of contributors from Savannah River, each contributing a portion of his or her time. Increases the management challenge.
- Need to pay more attention to the H_2SO_4 dissociation and developments being made in the catalysis area. This aspect of the process seems to be neglected, or at least assumed solved or a non-issue.

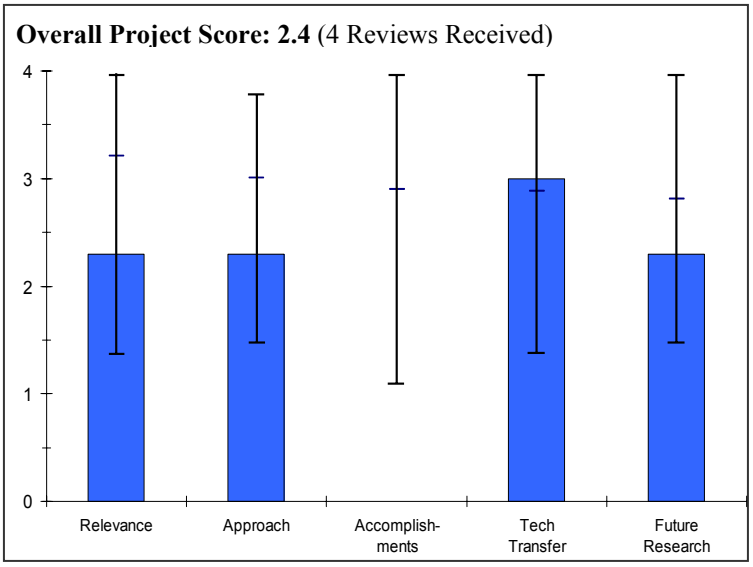
Specific recommendations and additions or deletions to the work scope

- Consider partnering with someone doing work with the catalytic H_2SO_4 decomposition step.

Project # PDP-46: Combined Reverse-Brayton Joule-Thompson Hydrogen Liquefaction Cycle
Shimko, Martin; Gas Equipment Engineering Corporation

Brief Summary of Project

In this project, Gas Equipment Engineering Corporation will design and demonstrate a novel hydrogen liquefaction cycle using multiple turbo-expander stages that combine the reverse brayton and joule Thompson cycles in a unique configuration. A small-scale pilot plant (several hundred kg/day) will be built that will be both a hardware demonstration and a model for scaling to larger plant sizes (>50,000 kg/day). A key component that will be developed in this project is a custom-designed foil-bearing turbo-expander.



Question 1: Relevance to overall DOE objectives

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

- Reducing cost of distributed hydrogen liquefaction.
- Pilot for medium sized fuel station, with scalability.
- No cost target, only minimization, and no reference costs with existing technology.
- This project is directed toward overcoming barriers to economic hydrogen liquefaction.
- The poster did not clearly address relevance to DOE program goals, although more efficient liquefaction technology would clearly be advantageous.
- Technology to increase liquefaction efficiencies important to delivery.

Question 2: Approach to performing the research and development

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

- Twin turbo expander-compressors.
- Build small scale pilot plant at 200-500kg/day of hydrogen.
- Scalable to >50,000kg/day of hydrogen.
- Targeting for capital and operating cost and power requirement reductions.
- The approach was not adequately defined, but this is likely because the project is not yet funded. The advantages of this approach are not clear.
- The project proposes starting with a small scale to match initial forecourt refueling station size but this presents cost and scale down efficiency challenges. It is not clear that small scale is the correct application.

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

This project has not started and therefore was not scored on technical accomplishments.

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

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

- Good transfer of technology from existing air separation technology.
- Would benefit with a hydrogen production collaborator, to match characteristic for effective operation at a pilot scale.
- Collaborations are appropriate.
- The existing partners are complementary.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Simple project schedule provided is adequate, but could provide more detail.
- Pressure of input stream not specified.
- Unclear on how the decision points will work shown with an asterisk on the project schedule.
- All work is future research. Work plan is reasonable.
- A more complete description of expected outcomes and future plans needs to be put in place to judge progress.

Strengths and weaknesses

Strengths
- Addresses costs of liquefaction with an existing technology.
- Could provide storage alternative for storing distributively produced hydrogen at fuel stations.
- Team appears to have adequate experience in this area.

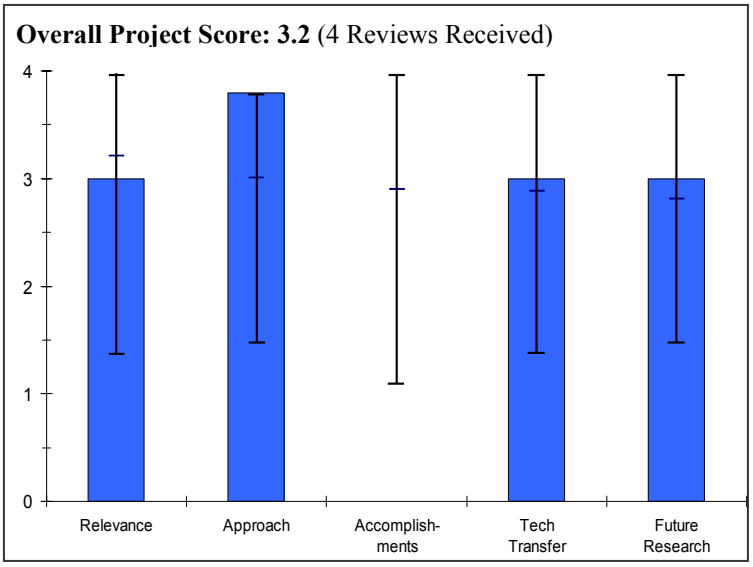
Weaknesses
- Poster does provide enough project detail to evaluate.
- Logic for large scale liquefaction not well understood.
- No cost targets or quantification of cost reduction.
- It is not clear that this project will result in any liquefaction breakthroughs.

Specific recommendations and additions or deletions to the work scope

- Provide additional justification with quantification of targets and detailing of plans.
- Add a hydrogen consumer as a project partner.
- Specify target pressure for hydrogen input stream.
- Need well defined criteria for decision points and go/no-go milestones.
- Given that turbo-expanders are not new, it is not clear that this is a 'research' project, more of a development/new application for an existing technology.
- It would be helpful to have included a technology/literature survey for existing uses and applications of turbo expanders (especially) to ensure no duplication of effort and a good knowledge of existing technology.
- It was not clear how long this program is expected to last and there was no indication of likely economics.

Project # PDP-47: Active Magnetic Regenerative Liquefier (AMRL) Development
Thompson, Robert; New Concepts Research Corp.

Brief Summary of Project



This project uses a novel liquefier that has the potential to simultaneously increase thermodynamic efficiency and reduce capital costs. The active magnetic regenerative liquefier uses an array of discrete magnetic refrigerants in a periodic heat exchanger to accomplish extremely efficient liquefaction of hydrogen. When such an array of solid refrigerants is combined into a highly effective regenerator with excellent heat transfer, low-pressure drop, and low longitudinal conduction, the potential for an extremely efficient regenerative refrigerator and/or liquefier is created. The capability to directly couple the magnetization of a working magnetic material within a regenerator unit with the simultaneous demagnetization of a working magnetic material within the same active magnetic regenerative refrigerator allows distributed work input and recovery from near-ambient temperature to cryogenic temperatures as low as ~20ºK. By using these coupled magnetization-demagnetization refrigerants, the net of work input is reduced substantially to that required for a very efficient refrigeration cycle, regardless of the temperature span of the liquefier.

Question 1: Relevance to overall DOE objectives

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

- Addressing technology, efficiency and capital.
- Needs to address capital and operating costs.
- Volume target 30 MT of hydrogen/day scale up not discussed.
- 25-35% improvement in electrical efficiency.
- A cost target for the liquefaction alone should have been included.
- Addresses liquefaction for distributed hydrogen system.
- Focused on reducing cost and increasing efficiency.
- The project addresses one component of the DOE Hydrogen Program delivery objectives.
- Liquefaction critical technology for delivery.

Question 2: Approach to performing the research and development

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

- Analyze design lab scale prototype (10-20 kg/day).
- Build and test first stage.
- Complete prototype.
- Use oscillating magnetic field to alternately stimulate molecules and remove heat; multiple stages to achieve liquefaction.
- Approach is radically different from the other liquefaction project.
- Barriers are understood.
- Project is designed to address the critical-path barriers first.

- The program delineates the tasks to be completed in a thorough manner.
- Status of the work and activities completed to date are listed and suggest a good start.
- Novel, high risk approach.
- Potentially breakthrough technology.

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

This project has not started and therefore was not scored on technical accomplishments.

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

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

- Predominantly technology companies.
- Canadian and American Involvement.
- Partnership with a liquid hydrogen distributor or consumer would be useful.
- Project could explore the possibility of partnering with a university with complementary (magnetic or liquefaction) experience.
- The project partners appear to be sufficient for the work proposed.
- Some industrial. Might benefit from some academic partners.

Question 5: Approach to and relevance of proposed future research

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

- Project is well thought out.
- Stop point after validation of single stage.
- A Gantt summary of the referenced MS Project plan would have been a useful addition.
- Specific discussion of price and scale-up progress evaluation should be included in the plan.
- Future work is appropriate.
- Milestones were not presented -- go/no-go decision points and criteria for making these decisions should be part of the planning.
- It is too early to say whether the proposed work will be appropriate.
- First stage in year one is a good goal. Ambitious schedule to complete liquefaction system within project scope.

Strengths and weaknesses

Strengths
- Key technology regarding liquid hydrogen.
- Focused approach and project plan.
- Off to a good start.
- Unique approach.
- Investigators seem to have a solid understanding of what is needed for the project to be successful.
- The application of magnetic refrigeration builds off existing natural gas liquefaction work.
- Novel approach. Good team.

Weaknesses
- Poster did not address breakdown of costs.
- Volume target unclear and no discussion of scale-up.
- The future work presented did not contain adequate detail.
- The project should prepare some early economics to guide decision making.

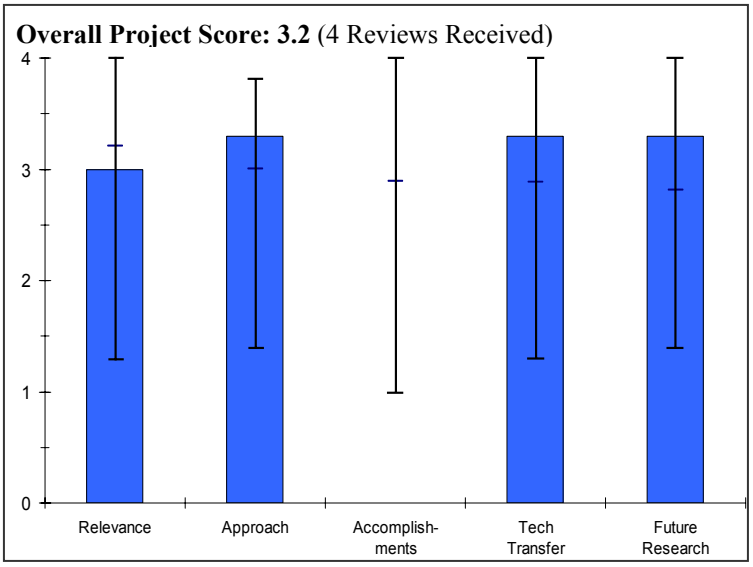
Specific recommendations and additions or deletions to the work scope

- Run project through H2A to verify target hydrogen price.
- Verify volume target volume: 30 MT hydrogen /day
- Since magnetic liquefaction is already a proven technology for liquid natural gas production, it might be asked why this project is being supported as it is essentially a development project to extend the technology to another related application.
- On the other hand a cheaper and more efficient liquefaction technology could be very valuable to the hydrogen economy.

Project # PDP-48: Hydrogen Embrittlement of Pipeline Steels: Causes & Remediation
Sofronis, Petros; University of Illinois

Brief Summary of Project

Hydrogen is a ubiquitous element that enters materials from many different sources. It almost always has a deleterious effect on material properties. The goal of the proposed program 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. Our approach integrates mechanical property testing at microscale, microstructural analyses, and 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 **3.0** for its relevance to DOE objectives.

- This project is aligned with the President's vision for a hydrogen economy.
- Supports an aspect of the delivery program -- metal issues and potential for use of steel pipelines for hydrogen duty.
- Pipelines critical to delivery effort.
- Embitterment problem needs to be understood.
- Critical for delivery using pipelines.
- Developing necessary understanding for hydrogen embrittlement in pipelines along with the potential to develop methods for mitigation through the future planned coatings work.

Question 2: Approach to performing the research and development

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

- The approach is well-thought out and addresses a key barrier in the use of the existing natural gas infrastructure for hydrogen delivery.
- The project objectives are thorough although many must be well understood already through prior work.
- Great expertise in fundamentals of hydrogen in steel. Project will build on researcher's expertise.
- Inclusion of fundamental simulations will aide project.
- The bulk of the work focused on microscopic investigations of embrittlement.
- A mechanistic model has been formulated.
- Includes finite element modeling.

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

This project is just starting and therefore was not scored on technical accomplishments.

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

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

- Have a good team assembled including end users.
- The participants bring a good mix of skills and knowledge.
- Good relations with steel/coating companies.
- The partner base appears to be strong and should facilitate technology transfer.
- Because this project just started, there was little objective evidence for extensive collaboration to date.
- The future work appears to involve significant collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Proposed research is on target.
- The proposed plan appears to be a logical extension of the program tasks.
- Good plans for extending knowledge to pipeline steels and broadening knowledge base for hydrogen steel interactions.
- Future work to include permeability studies on coatings.

Strengths and weaknesses

Strengths
- Collaborative team.
- Technical expertise.
- Fundamental experimental capability.
- Understanding embrittlement in pipelines is critical for hydrogen delivery and infrastructure. This work is a new start (4/30/05). Excellent progress has already been made. A mechanistic model has been formulated. The PI is well qualified for the work and a strong team that should lead to excellent collaborations has been established. Future work plans include studies to identify coatings with low/minimal hydrogen permeability.

Weaknesses
- None identified. Project is a new start.

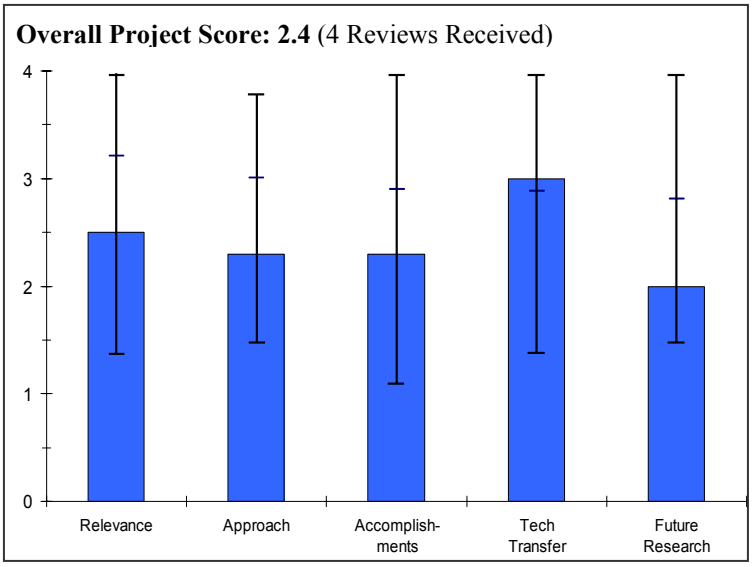
Specific recommendations and additions or deletions to the work scope

- There are many similarities to two other projects reviewed, PD-33 and PD-35, and the projects should be coordinated.
- Much of this knowledge must exist already in the steel and pipeline industries since hydrogen pipelines already exist without apparent problems. Use should be made of that knowledge in this program.
- Coatings seem to have a fundamental practical problem inasmuch as they have to be applied at the mill and retain their integrity during field installation. An assessment should be made of the value of this work through quantification of the size of the problem in the field to see if anything beyond current technology is needed.
- Needs to stay close to applied researchers and make sure that learning's are applied
- None identified. This project just started in 4/30/05

Project # PDP-49: Hydrogen Regional Infrastructure Program in Pennsylvania
Schumer , Eileen; Concurrent Technology Corp.

Brief Summary of Project

This Concurrent Technology Corporation program will undertake 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 will be conducted to determine the most attractive approach(es) for delivering hydrogen fuel, using Pennsylvania as a case study.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- Hydrogen delivery and transport are important aspects of the hydrogen infrastructure.
- The project appears to be trying to meet too many objectives -- pipelines, storage tanks, demand forecasting - yet focuses on Pennsylvania, which may not be a realistic proxy for a U.S. rollout such that findings may not be transferable.
- Understanding the potential of the current infrastructure and materials issues associated with hydrogen delivery important.

Question 2: Approach to performing the research and development

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

- Mixing hydrogen/natural gas in a pipelines will add costs of separation to the hydrogen costs which are too high already.
- Advantages relative to distributed reforming from natural gas are not apparent.
- Hydrogen sensors for hydrogen-in-air and hydrogen-in-methane are already available.
- Again, the project has too many activities that do not appear to have clear relevance to achieving the program objectives, e.g., regulatory/utility issues, demand scenarios, hydrogen recovery from mixed natural gas/hydrogen pipelines, and operational effects of mixed gases for customer use in appliances is not addressed at all.

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

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

- Have made progress identifying current pipeline materials and leak sources.
- Results seem to suggest that 50% hydrogen losses could still be cost effective; this is improbable since it would double the cost of delivered hydrogen above that for pure hydrogen delivered by pipeline.

- Program has only been in place a short time
- Considerable work has been accomplished, but it is difficult to see that much is of specific use toward addressing the stated objectives.
- Started in November 2004. Only 10% complete
- Work to date on data gathering and assessment.

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

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

- Involvement of Air Products brings a lot of hydrogen experience to the table.
- An extensive array of stakeholders/collaborators are shown but it is hard to understand what input they have provided, leading one to suspect that most are there in name only.
- Team has incorporated good partners.
- The key collaboration is with Air Products which has a large R&D (and presumably assessment) portfolio for hydrogen science and technology.

Question 5: Approach to and relevance of proposed future research

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

- Addressing issues such as hydrogen-in-air sensors which have already been developed.
- The project should focus on a smaller set of achievable deliverables, e.g., hydrogen/natural gas mixtures and effects in existing systems, not steels and storage tanks.
- Few details of the proposed future experimental work were provided.

Strengths and weaknesses

Strengths
- Collaboration with Air Products.
- Understanding the potential and limitations of current pipeline infrastructure for hydrogen delivery along with methods to improve it are important. The collaboration with Air Products which has a large R&D portfolio in hydrogen science and technology could be a strength to this program as the experimental program evolves.

Weaknesses
- Hydrogen costs should increase substantially due to additional separations costs.
- The project seems to be trying to be all things to all people without clear focus on a more manageable set of deliverables.
- The exact experimental program to be established was not presented in sufficient detail to fully assess. The project funding is considerable, so it will be important that this program is focused on specific critical barriers to delivery.

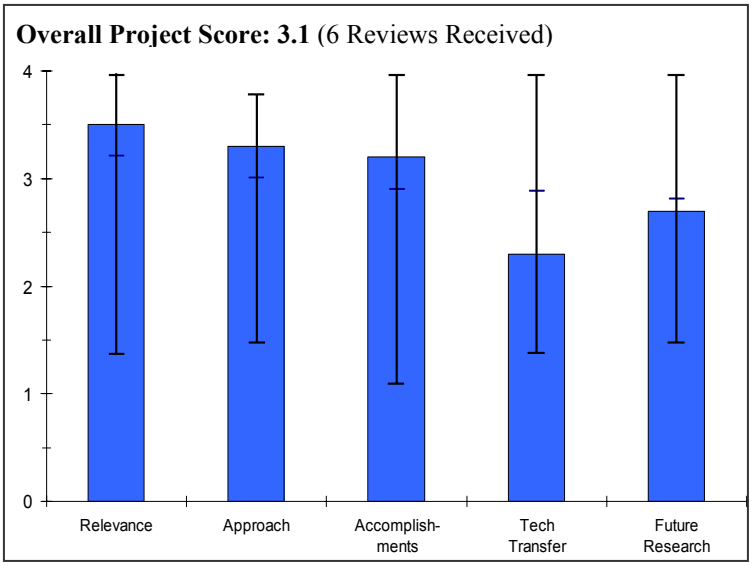
Specific recommendations and additions or deletions to the work scope

- Look at potential for using pipelines for pure hydrogen and time separated flows (with "pigs" in between), these would be preferable to mixing natural gas and hydrogen. (This is possible with gasoline, may not be possible in gas pipelines). May get some mixing, but should be more cost effective than total mixing.
- The presenter was not able to explain a direct path of transferability of the findings from this project to the rest of the USA; in fact, indicating that there are quirks to the Pennsylvania situation that make it less transferable than might be thought.
- In answer to a question as to why the utility companies were apparently not interested in having hydrogen in their natural gas pipes, the response was that they are interested, but the distribution system cannot take the higher pressures needed to maintain the energy flow with the lower density hydrogen in the system without expensive system modifications to update facilities and continue to meet applicable codes. Even so, the project seems to be ignoring this information and carrying on assessing hydrogen/natural gas mixtures regardless!

Project # PDP-55: Distributed Bio-Oil Reforming
Evans, Bob; National Renewable Energy Laboratory

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 and deactivation and regeneration strategy as a basis for process definition and assessment for automated distributed reforming of whole bio-oil.



Question 1: Relevance to overall DOE objectives

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

- Fuel Processor improvements: capital, manufacturing, operation.
- Addresses feedstock and catalyst issues.
- Target $3.60/gge.
- Target volumes not addressed.
- What range of bio-oils is addressed?
- Project aligns with President's hydrogen fuel initiative and offers a renewable hydrogen source.
- Project applies to the distributed reforming of renewable liquids goals in the RD&D plan.
- The project objectives are clearly stated and fit with program goals.
- Enabling the fueling of 300 cars per day is a great means to support the President's initiative.
- Plausible pathway for renewable hydrogen generation.

Question 2: Approach to performing the research and development

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

- Add methanol to pyrolysis generated bio-oil and steam reform resulting vapor.
- Identify candidate bio-oils.
- Identify candidate catalysts.
- Fluidized bed reactor adds complexity and cost to the system which may not be needed.
- Autothermal reforming in a fixed bed should address coking problems with proper conditions now that the feed is bio-oil rather than bio-solids.
- Apparently competitive with other renewable liquids that can be reformed, such as ethanol. Distributed reforming barriers are addressed.
- The approach is concise and focused and includes taking note of existing work already compiled.
- Future plans through volatilization, etc. seem to be in the right direction toward a successful project extension.
- Logical.

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

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

- Recently started.
- Initial work completed.
- Past work reviewed.
- Successful short (6 hour) runs with NREL catalysts.
- Preliminary statistical analysis of short runs provides guide to sensitivity. Sensitivity chart on the poster was very confusing needed more labeling, and better alignment of data with the tornado chart.
- Statistical method does not correlate factors affecting performance need to include interactions.
- Catalysts selectivity to hydrogen is low, with very high methane production after short periods on-line, indicating catalyst deactivation.
- New project with new concept, but it draws on prior work done on bio-oil reforming for central production.
- Catalyst characterization appears effective in identifying important variables.
- Catalyst behavior needs to be studied for a longer period of time than the period of the order of 10 hours addressed.
- Sensitivity analysis on system parameters was very convincing.
- Getting biomass into a transportable friendly state such as a liquid seems a key to any large scale use of bio-derived hydrogen.

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

This project was rated **2.3** for technology transfer and collaboration.

- Adequate collaboration.
- Would benefit from a commercial partner.
- Would benefit from university collaboration.
- Does not appear to be any collaboration at this point.
- Could coordinate with projects doing biomass feedstock analysis to address any potential feedstock issues.
- A number of pyrolysis, biomass processing companies/technologies exist. The project does not appear to have made any effort to collaborate with other knowledge sources.
- Coordination of activities needs to be spelled out more specifically.
- Look for industry participants.

Question 5: Approach to and relevance of proposed future research

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

- Develop low-temperature system and improve catalysts.
- Study factors affecting costs, volumes, and by-products.
- New configuration with reaction engineering.
- Long term bench testing.
- Good project plan.
- Pilot will need to improve catalyst regeneration strategy. Bench process uses discrete manual swaps of the catalyst, not supportive of long runs.
- Homogeneous catalytic partial oxidation of these oils should be more difficult than heterogeneous. Suggest focus on heterogeneous catalysis and optimizing conditions of temperature, H_2O:C and O_2:C to minimize carbon formation.
- Builds on prior work. Need a go/no-go upon determination of ability of technology to meet DOE goals.
- The FY05 budget is only $100K and there is no mention of succeeding years, yet the forward plan goes out to 2010 -- is there a mismatch here?
- The plan is good, but specific details need to be discussed.
- Logical path.

Strengths and weaknesses

Strengths
- Establishes a process for combined gasification and reforming of bio-oil.
- Initial short run work encouraging.
- Methanol can be biomass sourced for an all bio, emissions neutral system.
- Provides an alternative renewable liquids option besides ethanol reforming.
- Factor analysis comparison for catalyst operation.
- New approach through volatilization and oxidative cracking seems to be promising.
- Pragmatic.

Weaknesses
- Uses a narrow range of feedstock sensitivity to boil-oil variety issues such as feed source, hydrocarbon composition, and contaminants.
- Need to develop catalyst regeneration strategy.
- Lack of collaborations is a major weakness.
- Effect of bio-oil composition and characteristics on catalyst behavior needs further investigation.
- A more detailed plan is needed on issues such as oxidative cracking kinetics, catalytic oxidation mechanisms, etc.
- Needs funding support to continue the pace.

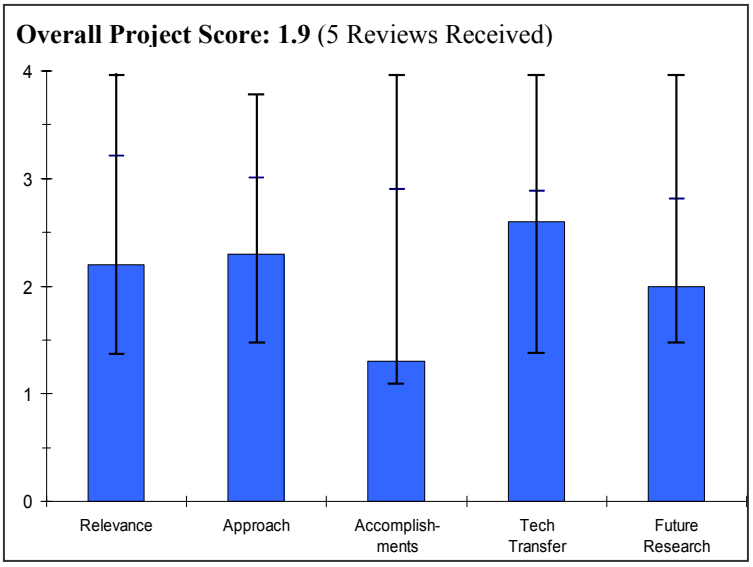
Specific recommendations and additions or deletions to the work scope

- Add a project stage to determine sensitivity of bench unit to variations in bio-feed.
- Improve the statistical analysis by including interactions between factors.
- Collaboration with a catalyst company dealing with reforming catalysts such as Sud Chemie, Johnson Mathey, etc. would help catalyst development work. Development with model compounds may help improve understanding.
- Would like to see discussion of how bio-oil would be delivered for hydrogen production on a distributed basis, and the associated cost.
- The project does not seem to have any unique elements that make it specific to NREL. Catalyst/reformer companies are already out there that could do this work.
- Address the weaknesses mentioned above and design the collaborative research plan (e.g. university involvement) toward addressing these weaknesses.

Project # PDP-57: Developing Improved Materials to Support the Hydrogen Economy
Martin, Michael; Edison Materials Technology Center

Brief Summary of Project

The Edison Materials Technology Center will solicit and fund 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 will be 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 **2.2** for its relevance to DOE objectives.

- The project proposes to identify and initiate a series of small projects addressing hydrogen and fuel cells technical needs.
- Earmark to Ohio based non-profit Energy Materials Technology Center.
- Orientation is too near-term and for commercialization and job creation.
- Only a part of activity need be hydrogen related.
- While this process has its merits, it does not align with DOE goals, which are longer range and more likely to result in a significant improvement to energy system.
- Materials progress might be supportive of DOE objectives.
- Most of the projects funded via EMTEC do not appear focused on significant challenges to the industry, but rather near-commercial technology.
- Project marginally relevant. Subprojects funded within it seem of little importance to the overall DOE Hydrogen Initiative.

Question 2: Approach to performing the research and development

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

- Solicit and evaluate projects, then manage technology projects – this duplicates process already accomplished by a number for federal agencies.
- Project puts out its own solicitations, does own reviews, has awarded five projects so far, and will do another RFP soon.
- Awards are made on the basis of near term commercialization and job creation potential.
- EMTC is strictly a managing function with no major assets.
- Access to facilities through diverse membership.
- The collection of projects makes little impact on the DOE barriers.
- Appears to be duplicating DOE functions.

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

This project was rated **1.3** based on accomplishments.

- Slow progress toward identifying and selecting progress. Only seven projects funded after over a year.
- Five project awards to-date: nanocatalyst development, spiral stackable reactor, hydrogen sensor, integrated photovoltaic/electrolysis, solid state welding with hydrogen storage potential.
- This is not really a technical accomplishment. Is hard to rate versus DOE goals, as DOE metrics are not included in the poster.
- Need to identify how the project is moving towards meeting DOE targets.
- Technical achievements interesting by themselves but not clear how they fit into the overall hydrogen program.

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

This project was rated **2.6** for technology transfer and collaboration.

- Interactions/coordination primarily with in-state (Ohio) support organizations. Seven projects negotiated and funded.
- Potential for collaboration is impressive, based on the poster, including State of Ohio, USAF, AFRL and a membership with over 100 industry, university, and government members.
- These provide access to $2B in state-of-the-art facilities.
- Collaboration experience in ceramics, metals, polymers, and many material processes.
- There needs to be more focus on transferring technology from the EMTEC projects into other DOE projects.
- Edison Materials Technology Center is doing a good job of driving projects to results. This is something lacking in academic and national lab environments where the trend is so much research for research sake.
- Not much technology collaboration can be noted. It appears that different parties are working on totally unrelated subprojects within this project.

Question 5: Approach to and relevance of proposed future research

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

- Continuation of identification and project selection process, primarily of projects as they are funded.
- Specific research is not being proposed here. Rather, RFPs call for projects meeting the general criteria and a review process to award contracts.
- Difficult to evaluate the RFP/award process, though projects may ultimately be supportive of the Program.
- Unclear directions for future research. There seems to be no strategic research direction. Subprojects are not connected to each other. Despite proclaimed focus on biomass and coal, no subprojects actually address these areas.

Strengths and weaknesses

Strengths
- Orientation toward near term goals and job creation.
- Organization has a track record of energy and materials related projects.
- A different approach toward awards may provide lessons for all.
- Variety of technologies funded.
- The strength of the center is in collaborating with different groups to focus their research efforts on results.

Weaknesses
- Is potentially another layer through which researchers and developers must navigate.
- Little focus on DOE barriers and targets.
- No guiding logic behind project choices.
- Disjointed effort.

Specific recommendations and additions or deletions to the work scope

- Consider funding fewer projects at higher funding levels. Present funding levels may be too low to expect meaningful technical results capable of commercialization with marketable products.
- Alignment of the DOE goals of the awarded projects should be required.
- EMTC should coordinate awards with DOE to avoid duplication and conflict.
- Align funded projects to solve problems which are encountered in other hydrogen programs and transfer technology to those programs.
- More work on biofuels and coal-to-hydrogen.

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 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, despite the newness of the effort, the level of funding and the difficulty in the task. The general opinion was that the Centers of Excellence will enhance integration of efforts, collaboration, cooperation, and overall effectiveness of the subprogram. In addition, the interface and collaboration with DOE's Office of Science was noted as desirable and appropriate. The proposed annual solicitation process was considered critical and a good aspect of program management.

The reviewers also highlighted a number of challenges and recommendations, summarized as follows:

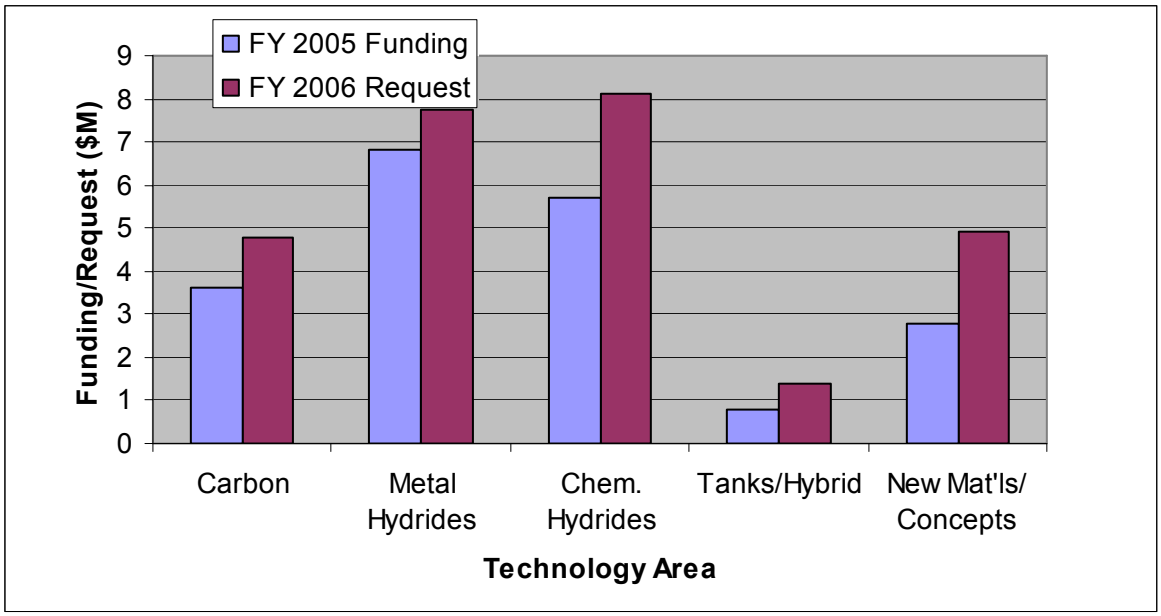
- Considering that hydrogen storage is a key hurdle and given that the effort is newer than others, such as fuel cell activities, DOE's management approach from a funding perspective was considered suboptimal.
- In addition, as projects move into more advanced stages, there was concern that more resources would be required to adequately monitor and manage projects.
- The Centers of Excellence were considered to be a challenge to manage to ensure the full value of broad collaboration and synergy of the efforts.
- The redirection of funds to congressionally-directed projects and its impact on the program continues to be a concern.

It was also noted that the hydrogen storage subprogram has substantially more university participants compared to prior years. The differences between Office of Science and EERE funding approaches was mentioned, highlighting that while basic science projects may last several years, effective management of applied programs may require termination of projects. Research that is clearly not helping to reach the technical goals should not be funded for many years. In some cases, reviewers stated that approaches that do not have potential to meet 2015 targets should be deemphasized.

In general, comments discussed how the incorporation and accomplishment of go/no-go decisions are critical and a "real test for management." Some reviewers believed that the storage subprogram still has at least a few projects that do not deserve funding, based on the fact that they either duplicate higher caliber work in other projects or are not very likely to produce meaningful results in a timely manner. A recurring recommendation was to continue to address system level issues to help direct materials R&D.

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 FY2006 funding profile, subject to congressional appropriations, and the Centers of Excellence 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 funding, 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. A major milestone in FY2006 will be the go/no-go decision on R&D for single-walled carbon nanotubes with a metric of reproducibly achieving 6 wt.% (material basis) hydrogen storage capacity.



Majority of Reviewer Comments and Recommendations:

In general, the reviewer scores for the storage projects were average to low, with scores of 3.3, 2.7 and 1.5 for the highest, average and lowest scores respectively. All projects reviewed were into at least their second year of effort. The three Centers of Excellence were presented but not reviewed, because of their late start due to funding. The scores are indicative of discriminating reviewers, perceived scope of work, and perceived insufficient technical progress in view of the challenging task (and in some cases, relative to funds received). In most cases, scores were lower than implied by written comments. Key recommendations are summarized below. DOE will act on reviewer recommendations as appropriate for the scope and coherency of the overall hydrogen storage research effort.

- **Carbon:** Implement go/no-go decision points. While some reviewers showed concern for the level of investment in this higher-risk area of materials research, most reviewers were very supportive of the broadened scope of work, beyond single walled nanotubes, and the added risk mitigation provided by complementing more traditional research areas, such as metal hydrides. Continue to shift emphasis to address the broader area of carbon-based materials and other high surface area adsorbents. Emphasize go/no-go decision points in the project research plans.
- **Metal Hydrides:** Continue to refocus efforts on new materials with potential to meet the long term storage targets. Sodium alanate work should be phased out. There is perceived overlap, particularly in the independent projects. DOE is urged to facilitate collaboration across the program to leverage efforts with the metal hydride center of excellence and apply lessons learned from mature projects. Thermal management, another system-level issue, should receive greater emphasis. Emphasize go/no-go decision points in the project research plans.
- **Chemical Hydrogen Storage:** Continue to address life-cycle efficiency, particularly for off-board regeneration of spent fuel, early in the program. Emphasize go/no-go decision points in the project research plans.
- **New Materials & Concepts:** The bulk of these Grand Challenge projects were not formally reviewed because they had just started. However, informal feedback indicated that there exists overlap with existing independent projects and center work. DOE needs to help facilitate more focus and coordination. Greater emphasis on new approaches to meet long-term targets is positive in the DOE plan.
- **Tanks:** Continue to focus on cost reduction and conformability. More fully justify the benefits of cryo-compressed approaches to increase capacity.

- **Testing and Analysis:** Testing and analysis are viewed as important cross-cutting enabling activities. In particular, it is important to develop tools that allow for rapid system-level storage capacity estimation tools using material-based properties. For essentially all areas, defining a list of system components and their respective weight, volume, and cost, is recommended. Additional recommendations include analyses of possible alternative options and rapid translation of results from a materials perspective into system performance and vice versa. More emphasis should be given to testing and analysis, which is a cross-cutting enabling issue.

One overall recommendation was for DOE to advise presenters for next year's review to state the status of the performance of the material (e.g. wt.% hydrogen, kWh/L, and refill times) compared to previous year levels to help gauge progress. This recommendation may be generally applied across all subprograms to more clearly distinguish progress from year to year.

Project # ST-01: Hydrogen Storage
Satyapal, Sunita; U.S. Department of Energy

Brief Summary of Sub-Program

The purpose of the Storage Overview is to describe goals/objectives, budget, barriers/targets, approach to R&D, technical accomplishments, interactions and collaborations, solicitations and awards, and future directions. As such, it sets the stage and puts into context the R&D and analysis projects which will be presented in this sub-program area during the Annual Merit Review and Peer Evaluation Meeting.

Degree to which the Sub-Program area was adequately covered and/or summarized

- Yes, the program was covered very well.
- Excellent presentation of state-of-the-art and technical goals, and the DOE R&D program in this area.
- The Team Lead has completely described the sub-program structure, objectives, and the funded projects. The presentation clearly states the basis for the structure of the sub-program and the preparation work. The main project areas and major achievements have been well presented with a summary of objectives and participants, with indications (websites) for more details.
- Did a good job of summarizing and explaining a broad range of projects in the technology area.
- Sub-program very well covered and status presented clearly. Presentation well structured, concise, and comprehensive.
- Program was explained well, including new Centers of Excellence.
- The sub-program areas were covered well, providing good guidance to principal investigators in managing and conducting their research activities.
- Excellent guidance to PIs has been provided in the presentation. Specific requirements for storage materials have been clearly stated to PIs in order to meet DOE targets on storage system basis.
- Targets, progress, and roadmap were all well covered by the Team Lead. Basic needs were also covered by the Science presenter. Nature of the challenge was well demonstrated.
- A very clearly presented/comprehensive description of the Hydrogen Storage sub-program.
- In general, very well summarized. Good, quick overview of accomplishments so far. Targets covered well relative to present state-of-the-art limitations (especially solid media). The many sub-program players and relationships (CoEs, independent, etc.) made clear with some good graphics. Sub-program is large and complex. Hard to show all the nuances adequately.
- Targets covered well. Partnerships and coordination with other group well laid out. Well done!
- BES research is based on basic fundamental material behavior. Was confusing who was managing the sub-program.
- Good summary of sub-program, goals, and accomplishments.
- Covered the sub-program well. Comprehensive, yet succinct.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- Yes, the slide on the range was one of the highlights of the meeting.
- The presentation shows in a convincible manner the major needs for key technical parameters (priority on specific requirements). The major scientific and technical problem/issues are adequately summarized and identified. The short- and longer-term plans and expectations for the future activities and major decisions are well organized and structured.
- Yes. The importance of hydrogen storage to achieving the goals of the DOE Hydrogen Program was emphasized and thoroughly explained. Plans for hydrogen storage Centers of Excellence were also well described.
- Main problem areas were clearly identified along with the challenges and future directions. Specific examples were employed to clarify some of the challenges and the respective performance targets. Very nice overview of status, key accomplishments, and future plans.
- Areas and challenges for the future were addressed with various projects to overcome barriers. At this point, however, there is no clear-cut solution to overcome the barriers.

- Yes. All sub-program accomplishments as well as gaps and challenges have been clearly stated in order to guide R&D activities as well as Tech Team activities.
- Yes, the targets and what is needed to reach them were well covered. The presenter also noted that the targets were often misused as material targets and emphasized that they were system targets.
- Yes. In particular, the Centers of Excellence will start to make things gel in terms of unequivocally meeting at least the 2010 "system" goals.
- Problems and issues well summarized, in general. The on-board refueling time problem for solid media (heat rejection) was not addressed. Safety was not addressed. Timetables give the R&D community what is expected for success. Go/no-go decisions are important.
- Addressed "system" well. Showed where funding will concentrate. Well done. Must continue to stress systems issue and how PI's must consider all characteristics in their material discovery.
- Displayed and highlighted system issues of materials well. Energies of three classes were explained well. LHV [lower heating value] is confusing. What is the defining line between hydrogen storage EERE research and BES research? Many look the same.
- Good concise explanation of the thinking behind the targets. Discussion of 2005 versus 2007 [targets] seemed a bit confusing. Said that 2005 targets were very close to being achieved with compressed and liquid storage. However, on "status" slide, the numbers do not seem to back up this statement: 0.5 and 0.8 kWh/L for 350 and 700 bar, compared with the 2005 target of 1.2 kWh/L. Also, it does not appear that compressed is anywhere near the 2005 cost target either.
- For the most part, yes. It is important to re-visit the magnitude of the "challenge" and why it is called "Grand." It is also important to stress a result-oriented program and PI accountability.

Does the Sub-Program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

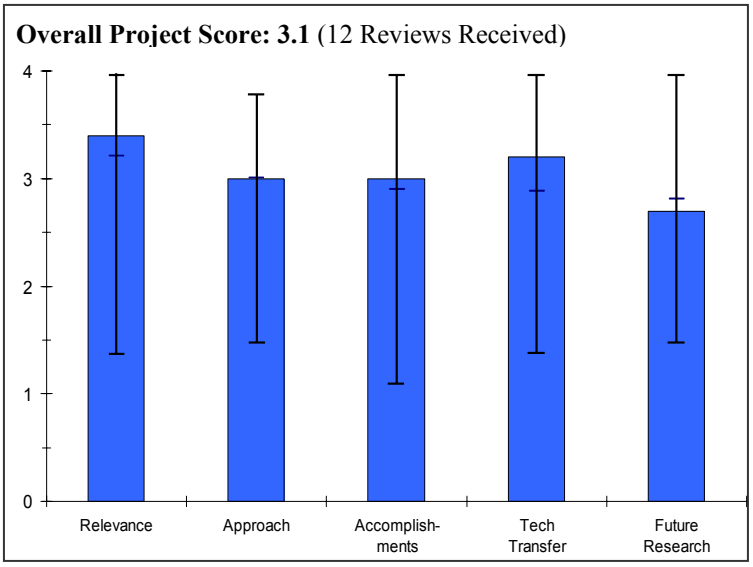
- The sub-program presents well organized and structured activities to meet Hydrogen Program R&D needs. The management appears adequate to face the day-by-day needs and to properly follow sub-program progress.
- Yes. Hydrogen storage is a critical area. DOE appears to be doing as well as can be expected to fund and manage this program area given fiscal and staffing constraints. However, concerned that as the projects move into more advanced stages, more resources at DOE will be needed to monitor and manage the projects.
- Sub-program area very well managed and coordinated. The concept of Centers of Excellence will enhance integration of efforts, collaboration, cooperation, and overall effectiveness. This area is well aligned with the Hydrogen Initiative and the EERE Multi-Year R&DD Plan.
- The Hydrogen Storage sub-program is one of the best managed efforts within the Hydrogen Program. Communication is very strong and transparent. The team is open to new ideas and processes to improve.
- Clearly covering the broad spectrum of research that is needed. Management has organized the large volume of work into centers to help administrate and coordinate. Significant improvement has occurred over the last years, which the DOE program has participated in. Connection shown to BES is desirable and appropriate. Go/no-go decisions will be the real test of the management. Continued stream of new solicitations is critical and a good aspect of program management.
- Yes. Good program management is being provided in order to guide and focus R&D activities in hydrogen storage areas.
- From the point of view of the Team Lead's presentation, one would say yes to this question. However, after hearing most of the oral storage presentations and viewing several of the posters, I find that there are at least a few projects that are not deserving of funding, based on the fact that they either duplicate higher caliber work in other projects or are not very likely to produce meaningful results in a timely manner.
- Yes, no problems in this area.
- Appreciated the definition of BES versus EERE system distinction. Showed Tech Team outputs. The Storage and the Fuel Cell Team Leads have the best managed tech teams of all USCAR activities.
- How is funding split between Tech Teams -- if at all? Will take a few years to eliminate overlap.
- Good focus on technical targets and diverse portfolio aimed at achieving materials discovery, systems engineering, etc., to enable these targets.
- There has been significant progress and it is getting better all the time. The field and the tasks are very difficult. Considering the resources and time, yes, it is managed well.

Other comments:

- Excellent job.
- The close interaction and synchronization with BES in funding is a welcome step.
- Team Lead appears as a strong, well-focused leader with a good vision. Team Lead is flexible enough to recognize potential and the need to redistribute funds when necessary, as stated in the presentation. Recommendations:
    - Earlier in the program, conduct analyses on possible alternative options and 'translate' as fast as possible, the results from materials perspective into systems performance.
    - Recommend a move from materials to system targets.
    - More emphasis should be given to testing and analysis, which is a cross-cutting enabling issue.
    - Closely follow the coordination of partners and projects under the Centers of Excellence – it could be a management challenge due to the size of the consortium and the range of research areas.
    - Investigate the impact, if any, on the program due to the incurred delays in Grand Challenge awards and possibilities of speeding-up progress.
- The DOE team assigned to this sub-program has a high level of expertise and is effective in managing their PIs.
- A difficult challenge, which needs all the funding it can get. If this is really the key hurdle, and given that the program is younger than the fuel cell effort, then one would think it would get more funding than fuel cells. This is clearly not true, and from the level of funding perspective, the management approach may be sub-optimized. While the needs of universities to maintain graduate students is clear, the idea that university programs will not be turned off makes management of these programs effectively impossible. In [DOE] BES programs, this is not a conflict, but in EERE programs it is; work that clearly is not helping reach the goals should not be funded for many years. [DOE comment: Because of the basic nature of the research, BES projects typically get three-year funding and are almost always continued through that time period. EERE projects, being applied research or demonstrations, are reviewed annually and may be terminated due to lack of progress, other Program priorities, etc.]
- The sub-program is and should remain open to really "new" ideas, given reasonable a priori gravimetric, volumetric and thermodynamic justifications. A little too much "hand waving" sometimes obscures reality. There is too much emphasis remaining on the alanates. (This opinion results from more than just the ST1 presentation.) Given the well-established weight, volume and safety shortcomings of the alanates, maybe some go/no-go gates should be considered? The Team Lead understands that alanate projects cannot be stopped on a a dime, but careful future focus must be made.
- In my opinion, the strategy of having 2005, 2010, and 2015 goals is misleading. Decide on what the tangible goals should be for an economically tenable hydrogen storage system for a vehicle and orchestrate a program that works towards those goals. If, for example, the current 2015 targets are the right goals, many of the systems currently under study could be eliminated from consideration immediately by simply making a straightforward determination of the upper limit of their best possible performance level.
- UTRC sounded like task was Mg-amide based.
- BES [DOE Basic Energy Sciences] is fairly new to many of us.
- The progress and results of the previous hydrogen storage work on carbon does not seem to warrant the same level of support as in other promising areas (e.g., complex, metal, chemical hydrides). This definitely seems like a much higher-risk, more exploratory area of research. Why did DOE decide to launch an entire multi-million dollar center based on the relatively high-risk topic of carbon and other nanostructured materials?
- Need to further stress "system" targets (there could be new people, etc.). Very good job/presentation. Stress PI accountability more. For example, when a question was asked about funding or termination of an academic program, it should have been said that PIs are taking a risk and before applying for funding they need to understand it fully.

Project # ST-02: Development of Metal Hydrides at Sandia National Laboratories
Wang, James; Sandia National Laboratories-Livermore CA

Brief Summary of Project



Metal hydride research at Sandia National Laboratories continues to develop new high-capacity hydride materials capable of achieving at least 6 wt.% hydrogen for vehicular applications (system basis). This project review is on Sandia's work in hydrogen storage for FY05.

Sandia employs a parallel approach through work in each of the following areas: (1) Investigate new complex hydrides and other reversible hydride-based materials to achieve higher capacities; (2) Develop new synthesis and doping processes to improve both absorption/desorption kinetics and ultimate capacity; (3) Experimentally characterize the materials' properties; (4) Determine hydriding mechanisms through experimental analysis and modeling; and (5) Determine important engineering materials properties of the materials to ensure that complex hydrides are on track for eventual commercialization.

Sandia has been selected to lead the DOE Metal Hydride Center of Excellence (MHCoE), which is composed of 8 universities, 3 industrial partners and 5 other national/federal laboratories. Current materials under study for the center include advanced complex hydrides, destabilized binary hydrides, novel intermetallic hydrides, and other reversible hydride-based materials. The Center activity will be formally reviewed in FY06. This review is for FY05 work by Sandia.

Question 1: Relevance to overall DOE objectives

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

- The relevancy could be and should be higher. However, the program appears to have reached a technical stagnation plateau over the period of 2004 -2005. The scope lacks sharp definition as compared to 2004.
- The PI showed a complete spectrum of metal hydride investigations suitable for properly addressing the Hydrogen Storage Program Objectives.
- Hydrogen storage is the highest priority research area supporting the vision of hydrogen economy. Centers of excellence are important initiatives supporting development of hydrogen storage technologies.
- Project works on high [capacity] systems that are likely to be critical for potential consideration in the President's Hydrogen Fuel Initiative.
- It is not clear if this work can be successful in meeting H_2 storage targets. There is not explicit progress to DOE goals. The down-select process will be key to not expending scarce resources on dead-ends.
- The project objectives (as those are stated) are consistent with DOE strategies and targets. Technical barriers are stated clearly.
- These materials and systems are steps toward the 2010 target, and are most appropriate. It is suitable and beneficial that there is engineering, material, theory and experimental work together leading to potential synergy.
- This project addresses one of the two key roadblocks to a hydrogen fueled vehicle economy - H_2 storage. MHCoE plans give encouragement that a better organized, more comprehensive and more focused hydrogen storage program will emerge in the coming years.
- No hydrides are discussed with the potential to meet storage targets.

- Most of the materials studied within the project have relevance to meeting the targets.
- There is a good focus on high density materials. This is relevant to the area of materials discovery, since this team is studying a diversity of systems.
- Metal hydride material research is a key enabling technology to improve hydrogen storage, if [operating] temperatures are reduced to FCV [Fuel cell vehicle] waste heat levels, the system could potentially be very simple.

Question 2: Approach to performing the research and development

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

- The group needs to define more criteria for material selection. Look at systems modeling earlier in order to provide more valuable criteria for selecting materials to research (such as material thermal conductivity which has a large effect on tank sizing). Ammonia loss cannot be resolved through "engineering" because it is still a base material loss that will substantially affect material cycling capacity.
- Technical barriers are acknowledged but the priority to address them is not properly arranged. For instance, the approach for material discovery versus the storage system requirement (e.g., container and balance of plant for storage) is not defined. What characteristic should an ideal alloy have to achieve 2010 system targets? Wt.%? Heat of formation? Packing density? Minimum thermal conductivity? Expansion rate? Kinetics? Thermodynamics? The PI has been asked these questions in several other occasions previously and was asked again at this review meeting but failed to answer the question. This is a critical step.
- The approach addresses major scientific and technical barriers that may help to identify new and more suitable metal hydrides for on-board applications. The creation of a MHCoE is very instrumental for a complete coverage of scientific and technical barriers. The MHCoE uses adequate and complementary expertise and techniques to have a complete survey of the present and future metal hydride alternatives. The lack of quantitative objectives for the materials related to the DOE system goals may affect the project's progress and deliverable. The system engineering part is not well presented and appears not functional to meet DOE goals. The final goal of a 1-kg [hydrogen] system appears poor and not ambitious with respect to the resources. [The 1-kg hydrogen prototype system is a DOE specified deliverable.]
- Although the center is just being launched, the approach seems reasonable. There is a concern on the large number of researchers listed which only add to 6 FTEs. Question significance of each contribution when some can only spend a small fraction of their time on the project. Ought to list only the significant contributors.
- Approach includes a variety of materials characterization approaches applied to systems of interest. The nature of the choices and alternatives in the systems chosen were not explained, however relevant systems were being examined.
- Approach well thought out for research underway. Integration into Metal Hydride CoE will help this project.
- It is not clear how various approaches are being selected for the project. There was no techno-economical justification for the approaches except references to some empirical literature data. Examples: incorporation of Si and Mo - the approach appears to be "try and see what happens." Needs systematic approach to new materials discovery.
- Mix of materials experiment and engineering is good. Many processes seem to be focused on seeing how desorption behaves and then see if absorption works. This is fine since thermodynamic data do not exist; but try to define the products and calculate (or failing that test) its possible reversibility as a highest priority. Work on role of Ti is a good addition.
- Lots of people in a 6 FTE effort; task-to-task responsibility structure is not obvious. A variety of characterization methods are used in appropriate ways. I am encouraged to see nanophase materials and combinatorial methods emphasized in MHCoE.
- Good variety of tools to attack problem. Project manager needs to assure that tools are used effectively and results shared and used across program.
- Formation of NH_3 in amide desorption needs even more effort than presented. Cycling weight loss not insignificant in amide, but no effort to address the issue. There is not enough effort on AlH_3. There is not enough effort on borohydride. The $NaAlH_4$ surface catalysis model does not seem to go anywhere or fit any data. What is the significance? Some of the materials analysis experiments not very convincing in terms of stated conclusions. There are too many people on project for funding level.

- Wide-ranging approach, studying a variety of promising materials. Good to see that approach is nimble enough to quickly switch to more promising materials (e.g., the current shift towards Mg-modified amides more than Na alanate). It is not clear what next steps will be in the Mg-modified amide systems. What are the ideas to improve the thermodynamics further?

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

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

- Although the material capacity accomplishments are decent, they are potentially misleading. Do not assume that all metal hydride systems will incur a 0.5x reduction in capacity; every material will result in varying system efficiencies. It is becoming obvious that higher storage capacities will be required in order to account for metal hydrides poor packing capacity and poor thermal conductivity.
- The progress must be measured against the amount of resources a program has received. In that respect while there is marginal and stepwise progress in some technical areas, the overall output and the results are disappointing. There was no mention of progress on some of the engineering areas that was being done last year.
- The PI summarizes excellent scientific results. The project is progressing well with respect to its objectives. The limited comparison with DOE goals makes hard, future selection and choice decisions. The results on Mg-based materials are well below the state-of-the-art and far from a possible use.
- Numerous accomplishments were cited, but none in enough detail to determine which were actually done in the prior year. PI needs to find way to allocate time to provide more in depth discussions of key technical accomplishments rather than one slide on each of over 12 technical topic areas.
- The program is aware of DOE program goals and has structured its plans in order to meet those goals. It will shift from materials R&D to storage system design in a timely manner to maximize probability of meeting program goals.
- 50% through project from time standpoint and overcoming some barriers, but how far to FreedomCAR & Fuel Partnership goals or DOE goals?
- There was no indication as to how the DOE targets can potentially be met with the approaches selected for future work. However, basic knowledge obtained up-to-date appears to be very useful for future research.
- Progress seems to have slowed since last year, but still good for 6 FTEs. Dropped desorption by 40°C, though at loss of kinetics. Characterized the material with X-ray, thermal desorption and Infra-red. Used PCT to show low NH_3, but it [ammonia] is inhibited in that system. Work started on several new systems.
- Lots of work on alanates; adding Mo to borohydrides; loss of ammonia; AlH_3, modified with LiH. This group utilizes theory and modeling and explains the effect of Ti addition. The presenter glossed over many of the data slides.
- Good work on $NaAlH_4$. Materials degradation study is relevant and well done.
- Lowering of amide release temperature good result. AlH_3 looks promising.
- Many novel results in a variety of materials classes. Said that main focus this year was really on Mg-modified amides. However, major result (reduction of temperature upon Mg addition) seems to be similar to last year's data?

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

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

- None indicated. Unless they are working with the same partners indicated in the MHCoE, they did not indicate that they had any partners or industry collaboration (GM aside! but that is supposed to be separate). GE was an industrial partner for manufacturing- should they not include more relevant tank suppliers for either stationary or automotive?
- There is much collaboration within national labs and outside universities. Significant progress on establishing the center of excellence.
- The coordination appears working among MHCoE members and a few external institutions. The three presentations of the PI and GE and HRL do not show good coordination because the various project parts

appear not clearly integrated. The interactions with other institutions and project involved in the DOE Sub-program are not specified.
- Broad and comprehensive.
- A broad group of external collaborators are working with the PI and his colleagues.
- Incorporation into MHCoE is good. New communication tools will be very valuable for further collaboration within this project and the MHCoE.
- Nice interaction and much more to come in the new center. I do not think this is a problem, as SNL is big enough they are effectively 'partnering' internally - they gain the benefits of other minds and equipment that partnering yields.
- The reviewed portion seems like a collection of in-house projects that the MHCoE has now incorporated into an organized/focused program with numerous substantive outside collaborations; hopefully, next year's program will show how all the pieces of the MHCoE fit together.
- Little collaboration is obvious.
- The group is having key collaborations established through center activities.
- Good collaborations; but as the Center gets kicked off, the group will need to be diligent in keeping others in the field "in the loop" in a collaborative way.

Question 5: Approach to and relevance of proposed future research

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

- The approach appears to be more of the same semi-empiricism deployed since 1995.
- While continuing the same work, they need to establish MHCoE partnerships and ensure more industry input. Relying on nano-synthesis as a panacea may not work. Nano-particles tend to agglomerate quickly and thus cycle data may degrade quicker than desired.
- The approach for continuing the work is clearly based on experimental deliverables and results. A well defined multi-year plan is positive in driving the work with changes suggested by results. The engineering part is too limited to favor clear material selection.
- Biggest challenge is going to be managing the center and getting the maximum value out of all the diverse subprojects listed. Will be interested in seeing results from some of the proposed projects.
- Future research plans appear reasonable.
- Continue to build on progress to date, but add effort to on-board storage module work.
- The main focus for 2005 has been stated as Mg-amides modified with Li. At the same time the goal for 2006 has been stated to be new materials discovery. Does it mean that the $2LiNH_2$ + MgH approach is a "no-go?" It is still not clear if the proposed future work has a good probability of success of meeting the DOE targets (needed technical justification for that to be presented).
- Although the planning seems exciting, it could benefit from a few, more precise scenario plans, for example by sticking to higher capacity systems. In some regards this is moot because the Center takes over this work.
- The plans for the MHCoE are the ones that matter here.
- Not obvious that investigator has identified plans for a systems approach? Will work be folded into Center of Excellence?
- Not evaluated because this program will roll into center work.
- Center of Excellence is main future research, and proposal is excellent. Topics for future work at Sandia are good; however, proposed plan for achieving new materials is unclear.

Strengths and weaknesses

Strengths
- There are many years of metal hydride experience with almost all the identified materials. The MHCoE will strengthen their research and expand their vision of materials and systems.
- Excellent technical resources and instrumentation. Experience in material science and engineering. Good collaboration.
- The MHCoE has an excellent coverage of expertise and resources. The present focus of the project is on the most promising candidates.

- There is a good technical team with good facilities.
- Breadth of characterization equipment applied to materials under investigation.
- Strong collaboration; new collaborative tools and the MHCoE will make project even stronger.
- Excellent technical expertise, instrumentation, facilities significant amount of experimental work being conducted that creates useful knowledge of the systems.
- Broad approach with significant theoretical and experimental thrusts interacting with each other. The material classes have some chance to make all goals; specific study materials could theoretically make 2010 goals. Very strong team!
- This is an institution with a lengthy history of performing metal hydride research. A seemingly solid plan for going forward with the MHCoE
- The group leverages materials strengths of national lab. They are talented researchers. They utilize outstanding facilities.
- This project has an excellent team, good research focus, and good technical results. Continue to focus on improved storage densities at desorption temperatures/pressures of interest.

Weaknesses

- Lack of direction from industry. OEMs need to do better job to define real system requirements and what real storage capacities of material will be required. There is a lack of good planning to identify new materials.
- There is inadequate/unclear technical approach. De-emphasis of tank and system requirement. There is low productivity. There are no clear internal targets when screening new materials.
- The material R&D is not properly driven by the DOE goals, because of the lack of clear quantitative objective on materials which are related to system performances. The project approach is not appropriate to facilitate a timely selection of materials for the scale-up and final system delivery. The system engineering is too limited and not adequate for driving activities towards DOE goals. According to the Project Plan, the new material discovery is not flexibly incorporated in the future work, as it should be. The MHCoE appears to be "a closed forum" in which possible future collaborations with new institutions, proposing new materials or solutions can be hardly incorporated.
- Concerned about ability to manage breadth of research planned under the center.
- The presentation did not present logic of materials systems choices/chosen for evaluation. It gave appearance of a survey of all work under support. While the individual efforts appear quite strong, one would wish to give lead PI opportunity to explain logic of systems chosen for further investigation and coupling between efforts supported. When asked, it was apparent that coupling could be stronger between modeling and experiment. Was informed that it would be utilizing theory better in informing choices of systems for study in the MHCoE.
- The progress toward system level targets not addressed. With 24 people working on project, the group is only accumulating 6 FTEs of time does not provide much opportunity for personal ownership by any of those involved. Research is good, but more focus on where it is leading and why, would be valuable.
- Selection of approaches is still empirical. They lack the really innovative "out-of-box" ideas. Philosophy of selection of new materials is still based on looking for known systems from literature that has low probability of success for the new materials discovery.
- 3kWh/kg system at 2.7kWh/L will require a material about 1.5x that capacity; none on the slate. That is OK for now, but some plan to get there would be nice. Concluding NH_3 suppression from PCT data is dangerous because NH_3 pressure inhibits further formation; it is TGA that shows the loss. Note that while capacity fade of LiMgN system is low, one still loses 1/2 of total capacity in 500 cycles. The D_2 experiments do not clearly show the H_2 pulls Ti to the surface.
- The published results of decades of work on metal hydride systems tend to suggest that the material of choice for meeting 2015 H_2 Storage system goals will not be a metal hydride. The presentation was not planned out very well for the time available; the speaker had to speed through portions of the talk near the end that I would like to have heard more about.
- It is not obvious that investigators are working as a team trying to obtain goal rather than individual PIs working on their individual programs. See next comment in this regard. There is real concern over 6 FTEs spread across 24 people. You need smaller groups to insure project ownership by investigators. FreedomCAR targets not mentioned in presentation. They should be focus of program rather than an afterthought. Still too materials focused. Need to combine with analysis to assess materials.
- Too diffuse with too many people doing different things. Materials analyses studies do not seem to be completed sufficiently to address issues and come up with solutions, mechanisms, etc.

- Diversity of materials classes along with the large number of people working on this program gives the impression that coordination of effort could be improved.

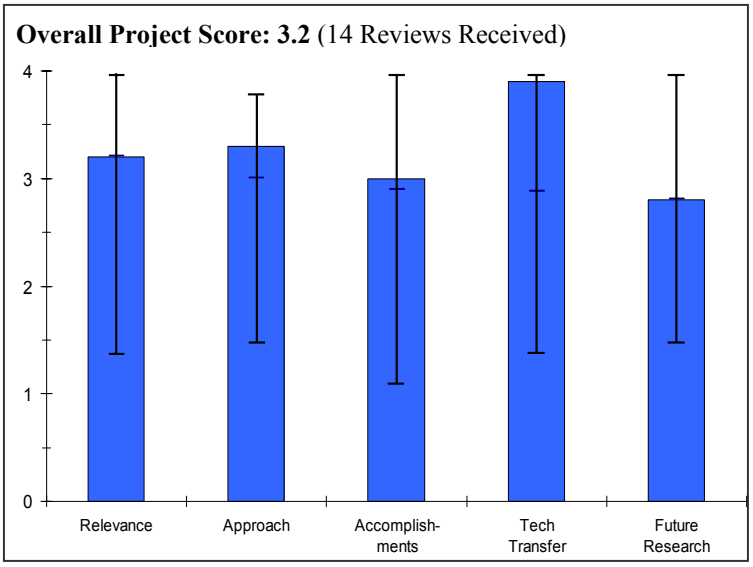
Specific recommendations and additions or deletions to the work scope

- Add effort to on-board storage module work.
- Look at systems modeling earlier in order to provide more valuable criteria for selecting materials to research (such as material thermal conductivity which has a large effect on tank sizing).
- Review ANL [Argonne National Lab] storage system analysis and TIAX LLC analysis to raise their material capacity goals and address thermal conductivity issues.
- The project needs to realign its work scope with the 2010 desired system outcome. Tank and system requirements need to be addressed equally and simultaneously when a material candidate is selected for further testing (for example much work has been done on sodium alanate in the past 10 years but an early overall system analysis would have been able to rule its adequacy). Material safety and its stability have to be a prime factor in screening new materials.
- The system analysis must start earlier in the plan and drive the selection of materials towards DOE goals. The MHCoE work should include also prenormative activities as a result of experimental tests on novel materials that poses safety concerns and may require specific code & standards development: e.g. sensors, testing procedures, and so on: the existence of an independent project on testing may be supported by in-house preparation work.
- Invite lead PI to make an additional presentation on program management and coordination aspects to DOE HQ staff prior to next formal peer review.
- Add system targets to presentation and status to those targets.
- Use more systematic selection of approaches. For example, it can be done by defining all properties of materials required to meet DOE targets (system basis) first. Using these properties as input parameters, conduct modeling of potential structures and compositions that can theoretically meet the system targets (if that is possible) and use combinatorial approach to try to synthesize those materials.
- Probably the addition of the Center will add more aspects that we could recommend. They seem to be trimming less productive routes voluntarily.
- In future reviews it would be good to have a prior year to current year status comparison to help gauge progress. In future MHCoE overview talks that are backed up by other poster or oral presentations, do not spend time putting up lots of data slides if there is no deeply meaningful point to be made from them - especially if other presentations will cover the same material. I suggest to the DOE staff that they allow at least 15 minutes for questioning/discussion of Center of Excellence presentations in future reviews.
- Need to present definite plans for systems analysis and approach.

Project # ST-03: Catalytically Enhanced Hydrogen Storage Systems
Jensen, Craig; University of Hawaii

Brief Summary of Project

The University of Hawaii is developing catalytically enhanced hydrogen storage materials capable of being used in vehicular applications. The objectives of the research are to determine the chemical nature and mechanism of the species that are responsible for the enhanced kinetics of Ti-doped $NaAlH_4$ and to apply the insights gained from fundamental studies of Ti-doped $NaAlH_4$ to the design and synthesis of hydrogen storage materials that will meet DOE hydrogen storage system targets. This project ends in FY05. New work by C. Jensen will continue within the DOE Metal Hydride Center of Excellence initiated in FY05.



Question 1: Relevance to overall DOE objectives

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

- Work is aimed primarily at understanding how catalysts work on metal hydrides or alanates.
- The project is highly pertinent to the DOE's R&D goals.
- The subject matter has been investigated for many years. The current system and detailed cost data suggest that sodium alanate cannot meet the DOE objectives. There have been some limited technical side benefits which may be useful for some other materials.
- The project is fully in line with DOE goals and the President's Initiative. The project, already concluded, and started before the definition of present DOE goals, maintains high relevance for the scientific achievements and the establishment of benchmark results.
- Hydrogen storage is the highest priority research area that supports the vision of a hydrogen transportation economy. This project investigated new classes of storage materials.
- The project is in line with the program objectives. Understanding of the fundamentals of the Ti-doped alanates hydrogen cycling kinetics will assist the development of materials that can meet the DOE targets. Valuable experience has been gained and the PI has wide expertise that can benefit the work program (see Metal Hydrides Center of Excellence).
- Work is of high relevance to attainment of goals of the President's Hydrogen Fuel Initiative.
- Project was originally aimed at fundamental understanding of the effects of catalytic doping of sodium alanate phases and the effects on hydrogen adsorption-desorption kinetics. Project targets were set in 2000, i.e., before the DOE storage targets were finalized.
- While [sodium] alanate will not directly meet the goals, it is a fairly well understood surrogate with several mysteries still unexplained in the improvement of its kinetics. Understanding this material may well help us catalyze other hydrides with higher capacity, and alanate may be a component to alter other hydrogen carriers, another reason to understand it in detail.
- The group focused on dehydriding/rehydriding kinetics of Ti-doped Na-Al-H. This project has been a path-finder activity that has generated much excitement and germinated work of a related/connected nature at other institutions.
- Explained and improved performance for first potentially practical complex hydride for H_2 storage.

- The project is looking in great detail at the classic new generation hydride, doped sodium alanate. This was the first EERE project on the alanates and was then quite relevant to DOE objectives. Nowadays, in addition to potential safety problems, it is becoming well known that this system will not meet DOE targets, so project should soon "move on" to stay highly relevant to ultimate DOE objectives.
- The focus has been on understanding the reaction mechanisms in [titanium-doped] $NaAlH_4$.
- Although, admittedly, $NaAlH_4$ is not the material that will achieve the DOE objectives, a good case was made for this type of fundamental studies of kinetic properties, and how that will likely impact future materials classes.

Question 2: Approach to performing the research and development

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

- The research group seems to be spending too much time on ball milling techniques which will be difficult to scale-up in a practical manufacturing process. Anelastic [relaxation spectroscopy] technique sounds interesting but too little time was devoted to explain method and real value.
- The project covered all the bases and effectively used partners to obtain the necessary expertise for the wide scope involved.
- This was pre-"Grand Challenge" project. The approach was not designed to meet DOE objectives. Consequently, the technical work diverged in areas with little end benefits to DOE. It was never clear if there was any systematic approach beyond traditional empiricism. In the last one and a half year, there was sign of stagnation.
- The approach focused strictly on key scientific and technical barriers. The project has opened the road to a comprehensive R&D including materials "guided" design. The study of the phenomena involved has required the integration of a variety of characterization techniques, of more general applications. The formulation of novel complex hydride has had an impact on further choices on alanates.
- Well respected research.
- The work was innovative with an exhaustive characterization task. An excellent case of fruitful collaboration, even at international level, for tackling scientific challenges. The work as such does not strictly address the technical barriers but the insights gained, will be applied in the design and synthesis of materials that may just be able to do that.
- The approach is reasonable to the objectives of the project. PI is to be complimented for searching out collaborations to supplement in-house capabilities.
- Although, not meeting the immediate hydrogen storage targets, the results provided insights to understanding of the behavior of various chemical hydride systems.
- Dr. Jensen has used a broad range of tools to investigate the actual atomic scale mechanism. He has engaged others when needed to do tasks he is not equipped to do. It would be nice to see this applied to production of higher capacity or faster kinetic material in future work.
- The approach is rather fundamental, as opposed to engineering. It is looking in detail at the doping mechanism in a multi-pronged scientific approach. Good, and hopefully this will be extendable to other complex hydrides. The extension of this effort to developing really new hydrides should be a byproduct of this alanate work. That seems to be slower in coming than I would have expected.
- For the most part, the characterization methodologies were intelligently selected and applied. Important issues concerning hydrogen storage are being addressed in a fundamental way - the science component is strong.
- Good mix of experimental and computational techniques. Went outside Univ. of Hawaii to get the job done when needed! Good! Both practical work (improved doping, cycling) and fundamental (mechanism, computation) were conducted. Not much said about safety. Assume that it is okay, but DOE needs to audit sometime during grant period.
- The group used a large array of solid state methods to apply to understanding alanate behavior.
- Assembled a good team of scientists with a diverse set of experimental skills and techniques.

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

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

- PI did not provide clear evidence that catalysis mechanism has been fully understood. Two year debates of mechanism remain other than rather obvious proposed phenomena of bulk substitution has been eliminated.
- Solid results were obtained that furthered the knowledge upon which we can now build.
- It was not clear if the PI answered the questions that the PI raised last year. The scientific conclusions appeared rudimentary. There is a higher expectation of the PI based on his previous work and accomplishments in the field. The conclusion of this project was disappointing. DOE should review the ways to increase benefits from projects that are expiring.
- The presentation shows significant progress (compared with 2004 results) in complex hydride development and experimental work.
- Although technology does not meet the current hydrogen storage goals, it had the capability of meeting the goals that existed when the project was started. Gained important knowledge of new classes of materials.
- Lots of data and a large amount of information have been collected in a cost effective manner, using different characterization techniques. As a result of this work there is now better understanding of the material behavior but rather slow progress toward a system engineering solution.
- Presentation emphasized results from prior year efforts shortchanging presentation of current efforts. This may be a misevaluation based on poor presentation choices by PI.
- Provided understanding and mechanism of the effects of catalytic doping of sodium alanate on structure and kinetics.
- Good progress in identifying state of titanium [catalyst]. Illustrating the fact the Ti changes state for several cycles is of significant interest. Essentially the fate of Ti is largely pinned down now. Effect of milling is more than size; it is defects. The analytic measurements are not very clear from the presentation but may be quite useful. No update on the advanced materials mentioned last year, had anticipated a year would be enough to reproduce that.
- Diffraction studies probed (1) lattice distortion due to ball milling and (2) pertinent structural transformations during cycling. The speaker had to rush through discussion of the most recent results, e.g., anelastic [relaxation spectroscopy] studies that reveal an interesting and possibly significant point defect structure.
- Improved doping methodology. Established long-term behavior.
- Outstanding progress development of mechanistic understandings of the doping process. Last year it was mentioned that the understandings derived from this project have resulted (theoretical or experimentally?) in a secret new 7 wt.% hydride. I would have liked to see a little more on this possibility this year.
- The group completed a large number of studies, but a model is still not defined.
- Progress made on elucidating where the majority of the Ti is in the microstructure. However, also showed that perhaps that is the wrong question to ask (i.e., that it may well be the minority Ti that is crucial). Disappointing that, despite many years' effort, and extensive list of collaborators, that the role of Ti in enhancing the kinetics of $NaAlH_4$ remains largely a mystery.

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

This project was rated **3.9** for technology transfer and collaboration.

- PI is involved with all the appropriate partners and MHCoE. Evidence of PI involvement is obvious in many other presentations and posters in DOE programs.
- Excellent collaborations with a large number of partners.
- The PI has established a large network and collaborates with many groups.
- Apart from the consultation to other projects (UTRC and UOP) and national collaborations, the PI has created an outstanding network of active collaborations with Norway, Italy and Japan, which added, at no cost for the Sub-program, important experimental results.
- There is a broad network of collaborators; that is well respected.
- Excellent, highly effective collaboration record and great use of resources.

- Collaborations are incredibly strong on this effort both nationally and internationally. PI is to be complimented for his interactions around the world. PI also consults with two domestic commercial concerns and collaborates with a third.
- Excellent collaboration with academic and industrial partners around the world.
- A model of how collaboration can help a program.
- Many well-considered and substantive collaborations.
- Good work leveraging in areas where Univ. of Hawaii did not have the needed tools.
- Excellent national and international collaborations in a wide spectrum of scientific disciplines. There is probably no better example of this in the entire EERE storage program. Good opportunities for tech transfer.
- The group has put together a very good team of materials scientists from around the world.
- Excellent. Prof. Jensen is basically working with just about everyone in the field of sodium alanates, including collaborations in Europe and Japan.

Question 5: Approach to and relevance of proposed future research

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

- PI does not have any future work; involvement with COE is unclear at this point. [C. Jensen/Univ. of Hawaii is a member of the DOE Metal Hydride Center of Excellence; work has started in FY05.]
- The group will be continuing with the DOE Metal Hydride Center of Excellence.
- What has been learned? How can these learnings be applied to new alloys? Or can they at all? As part of the formal closure of this project, it would be imperative to do a comprehensive look-back and determine short-falls and strengths.
- The prosecution of work fully relies on previous results and aims at reducing key technical barriers. The new research work is already funded and started in 2005 on a reasonable timeframe and with acceptable objectives.
- Current project wrapping up. Future work part of the DOE metal hydride center of excellence.
- The experience gained in this project will be valuable for the tailored design of binary hydrides with improved hydrogen cycling kinetics able to meet the storage system requirements.
- PI gave short shrift to description of future plans in presentation preferring to emphasize prior year accomplishments.
- Future work under Center of Excellence will benefit from the results of this project.
- Generally good plans though it would be preferable to close out the $NaAlH_4$ work sooner and move on to more attractive systems.
- Future plans are extensive; for the most part they seem appropriate. Hawaii will become part of the DOE MHCoE [Metal Hydride Center of Excellence].
- This is not really relevant since the project is over [in FY05].
- Perhaps a little too much on tying up loose scientific details over applying what has been learned so far to new hydrides. The group should focus strongly on the 7% material.
- Still focused on alanate.
- Will be a good addition to the DOE Center on metal hydrides, which should be an excellent team.

Strengths and weaknesses

Strengths

- PI seems to understand several metal hydride systems and progress of other PIs without being biased to his own work.
- The group had excellent scientific know-how. There was extensive collaboration even on an international level. The group effectively used their resources.
- Depth of experience and knowledge of the PI in the field.
- The prior work, the experimental results, and the strong research network with national and international partners are a patrimony that must be well integrated in the new project in the DOE MHCoE. The scientific and technical know-how and the resources are more than adequate to carry out the assigned work.
- Good findings regarding effects of titanium which can be applicable to other systems and projects. Appropriate characterization techniques have been found with convincing interpretation of the results.

- Great network effectively used to study the problem.
- The principal investigator has engaged in substantive collaborations to probe mechanisms using the most advanced/appropriate characterization methods available (in most cases). This particular group has lots of momentum in the hydrogen storage community; other projects have built upon findings/knowledge from the Hawaii effort.
- An excellent scientific basis to the project with equally excellent national and international collaborations. Impressive publication and presentation lists.
- Good fundamental approach. This was well executed. Leveraged through collaboration.

Weaknesses

- Continuation of effort to characterize materials that do not have the ability to meet DOE goals. PI was unable to elucidate how fundamental $TiCl_3$ catalysts will transfer to future materials. It is not clear that $TiCl_3$ or similar structured catalysts will become the catalyst of choice in future systems. PI must do more to convince community that this work has basis in future material systems (non alanate based).
- The biggest problem with sodium alanate is that the alloy can not meet the targets.
- Some collaboration is based on voluntary and scientific agreements, which may not be necessarily available in project planning and in future work.
- PI spent too much time early on in his presentation with background material (acknowledging collaborators, discussing approach, etc.) that left too little time for the important technical discussions later. PI had to rush through valuable technical details and never fully explained theory or results in latter slides. PI should be encouraged to exercise more discipline and to more thoroughly rehearse.
- There is no system-based data and no cost information available.
- The project was focused on the material that would not meet DOE targets. Still, a comprehensive concept of the catalytic effects has not been fully developed.
- Time to move on from the $NaAlH_4$ work even if not every aspect is fully settled.
- The published results of decades of work on metal hydride systems tend to suggest that the material of choice for meeting 2015 H_2 storage system goals will not be a metal hydride. The presentation was not planned out very well for the time available; the speaker had to speed through portions of the talk near the end that I would like to have heard more about; over 30 slides for a 20 minute presentation is too many.
- Not always able to relate large number of experimental results to material performance. Lots of correlations, not sure which were really relevant (causes vs. effects).
- Practical implications for future hydride design (as derived from past results) not clear (or at least not disclosed). Are the industrial collaborations (UOP, GE, UT) inhibiting full and open disclosure?
- The research needs to extend beyond sodium alanate.
- PI wasted too much time on the names of collaborators. Missed too much technical content near end of presentation. Please have PI only touch on acknowledgements near end of presentation.

Specific recommendations and additions or deletions to the work scope

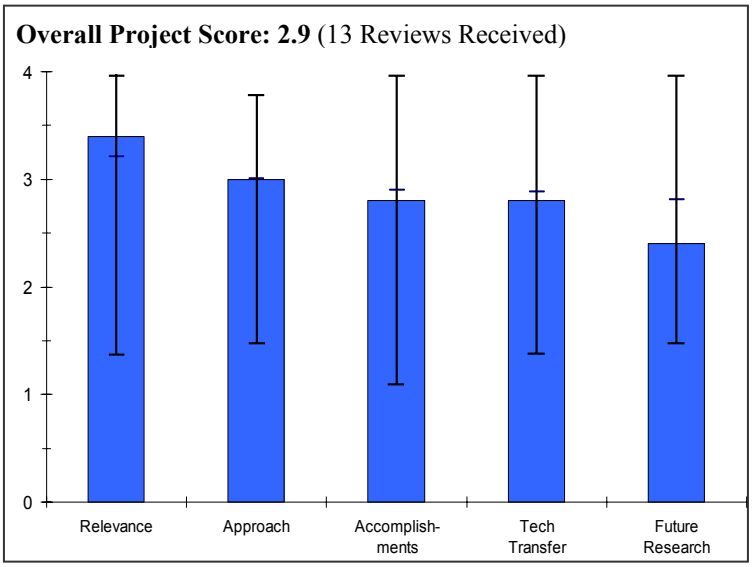
- The project is expiring but the lessons learned here must be applied in the future projects. Considering the resources, the PI has done an outstanding job.
- The indication of better quantified targets for materials in the new project would be functional in selecting materials and clarifying the real possibility for storage applications of alanates. The addition of longer cycle life test will allow for a more complete evaluation of the alanate and complex hydrides possibilities.
- Consider transferring material data and results to system level. That will be a valuable contribution to the Program. As a future undertaking and possibly under the Center of Excellence Project, investigate cost.
- Project is complete now.
- Optimally the $NaAlH_4$ work would be moved to general catalysis of advanced hydrides under DOE BES [Basic Energy Sciences]. This would allow Dr. Jensen to use what he has learned here to create new higher capacity material. The center as a whole needs to move past sodium alanate. DOE BES could fund working out the last iota of understanding.

- For future reviews, prepare a prior year to current year progress comparison to help gauge performance in the budget year. Limit the slides to only the most compelling results; make sure to leave sufficient time for all compulsory details. I suggest to the DOE staff that they allow at least 10 minutes for discussion of individual project presentations in future reviews.
- Is there relevant information in the fundamental work on Ziegler-Natta catalysis done over the last 40 years?
- In the next year, project should develop matrix of likely systems to which sodium alanate results and principles may likely be extended (e.g., amides, borohydrides, AlH_3, others?). What are the possible materials and paths to get to the 2010 DOE targets by 2010?
- For future work, many of the proposed activities are very similar to the previous DOE project on $NaAlH_4$ (e.g. location of Ti species, elucidation of role of Ti, determination of thermodynamics). These activities have been largely studied by Prof. Jensen over the past several years, and more work is unlikely to produce qualitatively new key insights. Recommend to remove these activities from the future plan in favor of the fourth bulleted item: to prepare new thermodynamically tuned materials.

Project # ST-04: High Density Hydrogen Storage System Demonstration Using $NaAlH_4$ Complex Compound Hydrides

Anton, Don; United Technologies Research Center

Brief Summary of Project



United Technologies Research Center (UTRC) is working to design, develop, and evaluate a prototype in-situ rechargeable hydrogen storage system initially employing $NaAlH_4$-based complex hydrides, but capable of using any endothermically discharging hydride media. The hydrogen storage system performance will be suitable for a PEM fuel cell powered mid-size automobile application. UTRC's approach is to develop improved sodium alanate-based materials and to design and fabricate 1-kg hydrogen systems for initial testing. UTRC will attempt the following: to improve the hydrogen charging and discharging rates of the $NaAlH_4$-based materials to meet steady-state PEM fuel cell demand; to increase the charging rates to achieve the five-minute refill requirement; and to increase the reversible weight fraction of hydrogen stored up to the theoretical maximum of 5.5%, through enhanced catalyst compositions and processing techniques. If other suitable high capacity hydride systems become available during this effort, every effort will be made to incorporate them into this prototype system. UTRC will also determine the safety and risk factors associated with the enhanced material compositions.

Question 1: Relevance to overall DOE objectives

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

- At some point, real system tanks need to be built to truly understand the system requirements of metal hydride storage systems on vehicular levels.
- The relevance of this project should have been much higher, had the PI decided on a more effective approach. The value of this program is diminished primarily by the tank design.
- The system development is highly relevant to the DOE goals and the President's Initiative. The PI clearly presents the specific and general advantages to develop a flexible system design and a storage device applicable to any reversible material.
- Hydrogen storage is the highest priority research area supporting vision of hydrogen economy. This project explores an important technological approach that may prove to be a practical storage media, even if it does not meet all the targets.
- Work is of high relevance to objectives of the President's Hydrogen Fuel Initiative.
- Important research, focused on system level targets. While $NaAlH_4$ may not achieve goals, it is working towards system level targets.
- Objective is not directly associated with meeting DOE targets on storage system; it puts system design ahead of the development of basic storage medium. It can be considered as a model to elucidate potential barriers towards system development. The concept still supports the plan objectives.
- Objective is to build and analyze a flexible prototype low pressure/high density hydrogen storage system. Highly relevant to HFCIT hydrogen storage system development; much is getting learned about the development and testing of such systems that will benefit those who follow the storage system RD&D trail.
- Highly relevant as the tank would serve other metal hydrides, which have a chance to meet goals. Learn about making large batches of material and how it differs from the sub gram experience.

- First publicly accessible information on a practical complex hydride storage system. Identified problems going from gram to kilogram scale. Won't meet DOE goals.
- The project provides much needed engineering data on alanate beds, data obviously capable of extension to other (future) hydride systems. This approach is important to the HFCIT program and it fits very nicely within the plan.
- Important to study system issues related to fabricating solid state storage systems.
- This type of system design is very important to have in the DOE portfolio, as the lessons learned will be applicable to emerging materials classes. Currently, only project in DOE to focus on this type of system construction.

Question 2: Approach to performing the research and development

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

- PI has taken the appropriate steps in construction of the vessel and even left packing options open. The PI should have taken into consideration more of the [FreedomCAR and Fuel hydrogen storage] tech team requirements such as min/max flow rate of 1.6g/s. Failure to realize the sensitivity of this requirement will likely lead to an undersized system or lower than expected "usable" capacity of the system. PI should also pay more attention to manufacturing production techniques and attempt to address some of these issues such as [vessel?] structure and packing methods in this program.
- Identifying and documenting the technical barriers that have surfaced will be the greatest contribution of this project to the entire DOE storage program. Because of the proprietary issues, it is not clear how the information will be shared with rest of the scientific community. The specific design of the current tank is not scaleable.
- The approach is reasonable and adequate to face material, system and safety issues.
- Good solid technical approach recognizing that the technology is unlikely to meet the current hydrogen storage targets.
- Approach is generally well thought out however, the project could have looked at a broader array of potential designs for storage systems or presented results of decision analysis that led to this particular design.
- Project well conceived to achieve goals; though it is not clear if barriers can be overcome.
- Comprehensive approach including majority of potential issues in the design of storage vessel. May not be flexible enough to accommodate newest material developments.
- Use of theory and science to guide engineering is highly appropriate. The use of a large cap and then approximating the metrics of the 'real' system without that cap is sub-optimal for demonstration.
- Made some mistakes and some mid-course corrections; hindsight is 20-20. The overall approach is both pioneering and sensible.
- Good safety. Good systems modeling. The group addressed unanticipated problems well.
- The approach is sharply focused on engineering approaches and practical data generation. Logical and very well executed so far.
- The group focused on variations of basic design. It would be valuable to look at other designs.

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

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

- The tank is nearly complete. PI did not report actual final built specifications clearly. Estimated improvements in future systems that can be made by "minor tweaks" are unlikely; - often 3 to 4 times improvement is required which will not be achieved by minor tweaks or engineering solutions.
- The key factors here are: the gravimetric/volumetric storage of the system, heat and mass transfer, and pressure vessel stability. None is promising at this point. No innovative solution has been offered for powder packing and densification. Only when there is a viable engineering solution for removing the stainless steel end cap and the cooling/heating manifold, the cap's weight credit (i.e., reduction from the system weight) should be applied. If this is proprietary information, DOE should appropriately evaluate the technical feasibility.
- The progress of the project appears in line with the schedule and the expectations even if the experimental results are not completely presented. The PI does not specifically emphasize the development of specific test protocols.

- Project was initiated to meet old technical goals and has made good progress toward those goals, but cannot meet present goals. May be harder to ramp up and engineer the way from current status to the volumetric goals than the PI expects. Question viability of next generation prototype to meet targets -- conventional finned heat exchanger may add too much weight and volume, moving system even further from volumetric% and mass% targets.
- Progress has been quite reasonable.
- Although system can not meet targets with current material ($NaAlH_4$), it is advancing system level understanding.
- Project appears to be aimed at identifying potential barriers of storage vessel design rather than at overcoming them. Upscale prototype system has been demonstrated, however, it did not get even close to meeting DOE targets (those are actually stated as technical barriers).
- Built and loaded the vessel. Learned many of the problems in creating material in bulk and loading a tank. Over optimistic on volume target being near.
- Much learned; gravimetric system metric is ca. 50% hydrogen; established many other useful metrics. The system projection chart has great utility; it should be treated as a work in progress and continuously updated. Good technical progress with system assembly and testing.
- Has shown likely parameters for real system: weight, volume, complexity. How about projected cost? Identified tradeoffs involved with charge time and system mass/volume.
- Plan followed very well. I would have liked to see a little more on the potential safety problems. Any new data since last year? Very good engineering data generated so far. Good scale-up data on various catalyst mixing techniques.
- Completed fabrication and made measurements on the bed. Made good progress on processing alanate material for packing into bed.
- Good progress and lessons learned through the course of putting together this project. Projections for future system design seem overly optimistic. Said that volumetric goals are easily achievable with small modifications; however, are currently a factor of 3 below the 2007 volumetric goal (11.7 vs. 36), and even the modified system #1.1 and #2 don't achieve the 2007 goal.

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

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

- PI is conducting project independently. More communication with ANL [Argonne National Laboratory] and TIAX LLC and their modeling work will substantially improve results of this project. Lack of collaboration is evident by lack of attention to resolve all of DOE targets and practical build techniques.
- There are little interactions on the key issues related to the vessel design and powder packing.
- The role of collaboration in the project is not well described. The interaction with the MHCoE may be usefully developed.
- Good mix of industry & academia. Seems to have the right collaborators.
- The level of technology transfer and details of collaborations could have been clearer. Impression left that coordination and collaboration could be improved.
- Stay connected with Metal Hydride CoE work.
- Working with several outside groups to enhance the program. They really need to publish more of the details to count as transfer.
- Well considered partners; collaborations seem to be effectively interfaced and coordinated.
- Good with partners. Not a lot outside of those. Partners appear to have been well selected for tasks.
- Many good collaborations, but more international connections might be advantageous. I get the feeling corporate IP concerns may be limiting collaboration and tech transfer a little.
- Assembled good team.
- Key to communicate lessons learned in this project to the external community; not clear that the communication to the community is open enough to enable all to learn from this work.

Question 5: Approach to and relevance of proposed future research

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

- Not much time is left in this project. PI did not seem to address dynamic performance of this tank or how it will be integrated into a fuel cell system - if at all! [Integration of the storage tank with the fuel cell power plant was not part of the scope of work.] PI does not seem open to reveal testing results or list of build issues. [Some methods were considered proprietary, for example, material-fill methods. Only public information was presented at the Merit Review and Peer Evaluation Meeting.]
- It is not clear what the future steps are, especially in face of the significant technical barriers that the project has encountered. For example, the PI stated that the aluminum foam will be replaced by finned tubes. While this will solve some problems, the ANL analyses work showed a potential major heat transfer penalty. This may also adversely affect the powder densification and containment problem.
- The PI shows the expected design improvements and the future steps. There is no indication of a possible testing plan to be performed on the final prototype and their effective duration.
- Plans seem reasonable and a logical extension of past work. Will be interested to see how close the new design iteration comes to meeting program targets compared to current design.
- Good effort as far as it goes.
- Second prototype proposal to address cost and adaptability is good future approach.
- Future work is planned towards the improvements of primarily gravimetric characteristics of existing prototype without considering other potential storage media that would be closer to the targets.
- Improved heat transfer is good idea. Basically, on the right track. Need to do response work, how does the thing respond in transients.
- Sensible plan for future work that follows logically from what has been learned/achieved so far.
- Is manufacturing for double hemisphere tank simple enough to be realistic?
- PI admits the Na alanates will not meet the 2010 DOE goals. Maybe it is time to move beyond the alanates into newer systems with more potential (borohydrides, AlH_3, amides, etc.)? The engineering data thus far generated should apply to other systems?
- Relatively narrow extension of existing system design.
- The prospects for improving the system design and performance characteristics are not clearly explained, and the numbers seem overly optimistic. Really need to give actual numbers for the built system, rather than projections of the system. How much does the system weigh, how much volume does it take up and how much hydrogen comes out? Would be very valuable to give measured data of hydrogen output, i.e., rates of discharge and charge as a function of filling.

Strengths and weaknesses

Strengths

- Ability to build the system in a timely and safe manner. PI has exercised careful and diligent attention to safety.
- UTRC experience and knowledge. Capabilities and resources.
- The system engineering approach may be the reference point for the Subprogram for better addressing material requirement. The project has set up expertise and facility for complete system testing.
- Important component of the Initiative. This project is to be complimented as to begin to address scale-up problems early in the game even though final material is not known.
- Good presentation with fair portrayal of project's strengths and weaknesses. Focusing on system level targets is important.
- Identifying potential barriers towards the development of storage vessels based on metal hydrides, and generating knowledge and experience in this area. Good focus on safety issues.
- Learning what difficulties lay ahead for making large systems.
- A strong team with the right mix of capabilities. Producing results that give confidence of eventual success in meeting HFCIT H-storage system manufacturing goals (but not necessarily H-storage targets).
- Systems approach. Strong safety emphasis.
- A sound and excellent engineering approach this is much needed.

- Comprehensive project covering all aspects of solid state storage system fabrication. Good combination of modeling and hardware.

Weaknesses

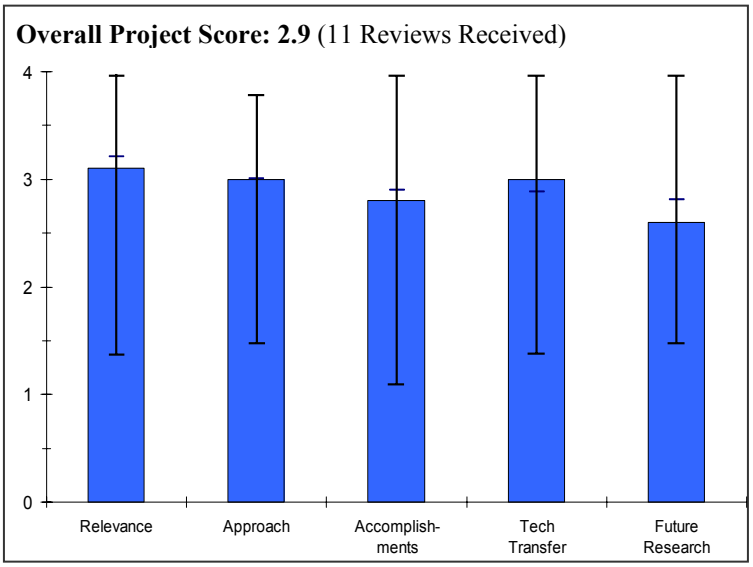
- The project should share information generated publicly as much as possible (within the restrictions of company IP) to prevent unnecessary and expensive duplication. The reviewer's opinion is that this system likely has no chance of commercial success in the automotive industry and that this endeavor is not the first of its kind. The reviewer's opinion is that not all the challenges are resolvable with engineering solutions. The reviewer does not believe that resolving the poor flow rate issues of this system can be resolved with engineering solutions and still maintain adequate usable capacity or range in a vehicle; it is a materials characteristics issue.
- Lack of flexibility. Have not dealt well with the technical challenges.
- The collaboration with MHCoE and other projects should be enlarged to give better support and basic information to better define basic material requirements. The development of testing protocol may be made functional to develop standards and carry out prenormative research work on specific aspects: e.g. safety, sensors, and duty cycles.
- System Projections slide is overly optimistic that all the planets will align to achieve the system projections in right most column. All the listed actions are required to get there: not a trivial task.
- Could have looked at a broader range of systems. Could have explained design criteria better and rationales for choices.
- Current compound [$NaAlH_4$] is not capable of meeting goals.
- Design is based on sodium alanate that will not meet DOE targets. Lack of projected cost estimates and comparative data with other developers.
- Plan did not allow for making the actual designed vessel (needed large heavy end). Density used was low, would have been better to figure out how to densify. Still have not given the simplest number, what they actually accomplished. They assume a lighter larger vessel in the calculations of mass and volume efficiency.
- I wish this team was working on a hydrogen storage material that was something other than a metal hydride.
- Focused on one bed design concept.

Specific recommendations and additions or deletions to the work scope

- Similar work could have perhaps been accomplished by integrating existing Ovonics metal hydride tank or approaching ChevronTexaco or DaimlerChrysler or SRNL [Savannah River National Laboratory] for a much cheaper rate. It is becoming evident that alanate systems will not perform any better than established and perhaps safer LaNi based systems such as Ovonics. It is reasonably safe to assume that all metal hydride systems will perform dynamically similar to each other, further efforts to build tanks is not necessary until a much improved capacity material is available.
- This is one of the most difficult engineering projects in the program. The team should be commended for their hard work and diligence. The current data indicate that the project will miss its target but there are valuable lessons in this work. An important task is to document the failures and lessons learned. It is highly recommended that DOE add a specific scope to quickly capture these items. There are tremendous values in these early experiences that can help the entire program.
- The addition of a preliminary testing protocol and its experimental validation would be beneficial and useful for the work in the Testing Group.
- Broader range of powders, catastrophic failures such that we are better aware of safety design issues.
- Put some focus on more promising storage materials in order to be closer to targets. Create more innovative engineering solutions such as main port design, etc.
- Again - publish details, this is unlikely to ever go on a vehicle in this form. Really, really, need to do transient testing and several different states of charge.
- Forget the 2010 targets for hydrogen storage, start working towards meeting the 2015 targets--the applicable storage system details could change considerably--it would be a shame to waste good engineering work on dead-end materials. For future reviews prepare a prior year to current year progress comparison to help gauge performance in the budget year.
- Might be useful to publish "lessons learned" explaining what the crucial parameters are for future workers, i.e., materials packing, changing rate tradeoffs, etc. Determine and report quantitative tradeoff between charge time and system weight/volume.

Project # ST-05: Discovery of Novel Complex Metal Hydrides for Hydrogen Storage through Molecular Modeling and Combinatorial Methods
Lesch, David (PI); Sachtler, Adriaan presenting; UOP

Brief Summary of Project



The objective of the UOP 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 **3.1** for its relevance to DOE objectives.

- PI is investigating hybrid approach to ternary metal hydrides. Developing modeling techniques to identify potential good candidates.
- The project appears to fit well with DOE's goals and objectives.
- The project is relevant to the DOE Subprogram because it addresses specific requirements in the specific sector of complex hydrides.
- This is a good effort in its relevance. The work is a necessary component of the overall program considering systems that should be considered yet not highly likely to be strong candidates for actual application.
- Project scope and objectives fit well with the DOE vision and targets.
- Good set of materials with the possibility of meeting goals, if they work.
- This project does align with RD&D goals in the key area of hydrogen storage but seems locked into one storage material type [alanates].
- No materials close to targets. Relevance could rise if right material found.
- The effort actively seeks the new hydrides need for the HFCIT multiyear plan. It fits the goals and objectives of the plan.
- Materials being studied relevant to program targets.

Question 2: Approach to performing the research and development

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

- PI conducted modeling to quickly sweep combinations and eliminate poor candidates. PI started from hydrated and dehydrated states to evaluate if end products were the same – good. Why did PI synthesize some components that were thought to be uninteresting?

- The approach seems reasonable to achieve the project goals. It is highly focused on the material discovery process.
- It is not clear what elimination process was chosen to narrow down the field of search beyond the rudimentary storage capacity. For example, how do the PIs view the heat of formation effect? How about the packing properties? Or in general, system effects? There have been some well established criteria on what properties a workable metal hydride must have.
- The approach is interesting and reasonable because it combines theoretical analysis (modeling) and combinatorial methods to prepare and select new materials. The PI clearly presents the method applied for fast experimental screening of a variety of material compositions.
- Again, the approach would be better if there was a better explained theoretical basis for this choice of systems to be investigated. Appears to be a more brute force approach to the problem. A more intelligent or coupled theory approach might lead to the choice of other systems for investigation. Presumably with the collaborations with Ford and UCLA this should be present, yet it was not adequately presented.
- The approach involves systematic studies of alanate-based systems assisted with molecular modeling.
- Combinatorial approach is appropriate. Theory guidance will enhance chances of a discovery that meets goals. There is need for a wider scope however, while the 3 materials chosen are good ones, there is nowhere near the breadth of scope normally used in combinatorial tests by making many libraries. New approach with more elements is more appropriate. They have started with hydrided and dehydrided forms which is a good approach.
- The combinatorial/modeling approach is being applied in what seems to be an effective manner; FY 2005 results are encouraging in the sense of verifying the methodology.
- VHTS good approach. High level QM not fast enough for screening. Combisystem impressive.
- The approach is high throughput combinatorial modeling and experimental validation. It is an approach well worth trying, combining efficient experimentation strategies and predictive first principles.
- Rapid screening approach has some advantages over other methods.

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

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

- Results have been disappointing - as predicted by modeling; at least modeling is partially validated. Potentially interesting compounds proved unfruitful- disappointing.
- Approach appears to be validated with the results obtained so far.
- Difficult to evaluate as it is started several months ago. [The project has been running since ~May, 2004.]
- The main achievements are well in line with the schedule. The screening protocol appears well designed and effective in selecting materials. The PI should give more details about the experimental results.
- Solid, significant progress is being made.
- Even if results are negative so far (in terms of finding a materials that meets DOE targets), the project still helps build a good knowledge database in the area of alanates, and helps move forward towards discoveries of novel systems in other areas of storage materials.
- Showed the reproducibility of method. Completed testing of Mg, Li Na system based on 3 substances chosen. Figured out what was forming and why the results were so much lower than expected. Started doing elemental (dehydrided) synthesis with a broader range of materials. Started virtual screening. Showed that they could measure multiple storage materials at once.
- FY 2005 results suggest that the project is capable of meeting its long term goals, i.e., identification/optimization/verification of H-storage material compositions in relation to HFCIT RD&D targets--the concern is about whether any metal hydrides can meet 2015 targets.
- Good work on Li, Na, and Mg phase diagram work. Systematic study was needed--even if it did not show great materials.
- A reasonable start for the first year, showing the basic utility of the combinatorial approach. But few unexpected or promising results. Give the effort its due course of time.
- Completed first matrix of material combinations. No winners found.

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

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

- Contributions by partners were not described in the presentation.
- There are adequate amount of interactions.
- The collaboration in the project is not well presented. The possibility of further collaborations with other projects and the MHCoE is not envisaged.
- Collaborations with prominent modelers briefly described. Individual contributions to collaborative effort could have been presented in greater detail.
- Already a collaborative team.
- I assume that all the listed team members are making contributions on a regular basis. However, it is not obvious from the presentation who is doing what, in all cases. The benefits of adopting H2C methods are apparent.
- Good interactions with partners.
- The collaborations seem to be more than good, and mix varied expertise quite well.
- Limited team assembled.

Question 5: Approach to and relevance of proposed future research

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

- PI is working down list of heavier elements - seems desperate and lacks vision to move on to other systems - contractual obligations aside. Some of heavier elements may yet produce good results as they produce double the hydrogen bond sites.
- The approach encompasses the future research by ramping up the number of samples that are tested. This should result in an efficient process to identify promising materials.
- It is a mix of newer modeling work with more traditional approach.
- The description of the future activities is vague and uncertain even on the work on new materials (written "if needed").
- Expansion of approach to include work on amides & destabilization of hydrides is to be encouraged as appropriate.
- There is no technical justification provided for including additional elements (Ca, K) in the composition for future HT and modeling experiments.
- Broadened elemental scope and testing at higher rate, plus 1000 virtual tests/ month are all good ideas.
- The future plans follow the charter for this project and do make sense in the context of what has been accomplished/learned so far.
- Given lack of positive results in systems studied thus far, should work move to new systems? New HT system encouraging. What will new bottleneck be? Testing?
- Reasonable. Hard to call “outstanding” with pregnant potential. Obviously investigators need to get beyond Na-Li-Mg pseudo-ternary systems. Results so far are not very promising. Is there some possible overlap with ST6 (UTRC) that should be coordinated or avoided?
- Straightforward continuation of work.

Strengths and weaknesses

Strengths

- UOP is quickly eliminating poor candidates and working with OEM that understands true requirements of the system. PI developed consistent and effective method to test samples concurrently.
- Experienced and knowledgeable people. Access to resources. Good collaboration.
- The effective use of molecular modeling and combinatorial approach may be useful in accelerating synthesis and selection of a large variety of materials.
- Systematic approach to new materials discovery assisted with molecular modeling.
- Theory guided combinatorial testing.

- The combinatorial synthesis/characterization approach seems to work even though it has not pointed to a new/improved composition for the Li-Mg-Na/AlH_4 system (unfortunately, I suspect this will be the case for all the metal hydride systems).
- Strong synthesis/testing capabilities. High throughput screening methods.
- The first systematic modeling/combinatorial approach within the HFCIT program.

Weaknesses

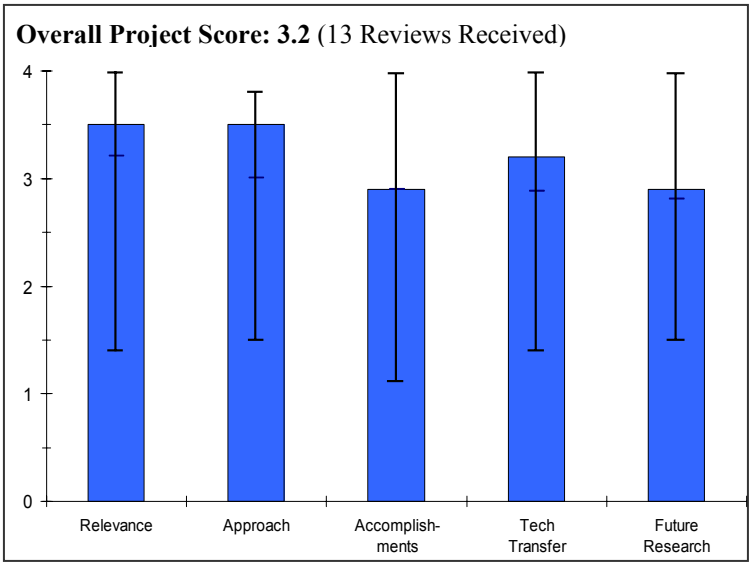
- PI did not demonstrate how models will be improved from recent round of results.
- Unclear alloy vetting selection/rejection process. Need to start considering storage system needs when an alloy is being evaluated for further work.
- The PI does not show clear future work and adequate experimental activities. The collaborations are limited.
- Empirical addition of new elements in the matrix (K, Ca) that dramatically increases the number of experiments but does not increase probability of success overall.
- Restricted systems tested so far. Troubled by not revealing all materials, hard to appraise the work if we can not see it.
- This project seems to be locked into metal hydrides [current scope of work with DOE]; the combi/modeling effort might better serve other storage material types in the longer term.
- Relies too heavily on the overworked alanates, so far. Seems to have possible overlaps with ST6 (UTRC) relative to both techniques and materials.
- Rapid screening encompasses limited level of processing conditions. Although one looks at large number of material constituent contents, it does not look at processing variables or catalyst variables. Not clear how modeling folds into experimental work.

Specific recommendations and additions or deletions to the work scope

- PI should continue to refine model.
- There has been much work in the field thus the PIs are expected to use the lessons learned and effectively manage the project and re-align (with the DOE goals) when faced with technical challenges. Part of this must be measured (by the DOE and the reviewers) as indicated by the number of choices that are rejected before a candidate alloy is chosen for further studies. The program must maintain its goal-oriented focus.
- The project plan should contain more details about the future work. The experimental work and the development of the complete system require better specifications. Better collaboration with other projects and MHCoE is recommended.
- Wrap-up search in the alanate area. Expand the scope into other areas of storage materials.
- They should be freed to expand into materials other than alanates if they wish. Suggest that they confine to test points with materials 4% (for learning purposes) or more.
- Since the modeling of the Li-Mg-Na/AlH_4 confirmed the experimental findings, i.e., that no promising compositions were being missed, can we now assume that we can rely on the modeling to guide us to the systems we should study? -- If so, in principle we should be able to save lots of measurement time and resources by relying on the modeling supported by a few judiciously planned experiments? For future reviews prepare a prior year to current year progress comparison to help gauge performance in the budget year.
- As suggested by PI, move into newer systems: borohydrides, amides, etc. Can there be some coordination with ST6 (UTRC) to avoid duplications of effort? Will corporate IP issues prevent that?

Project # ST-06: Complex Hydride Compounds with Enhanced Hydrogen Storage Capacity
Anton, Don (PI); Opalka, Susanne presenting; United Technologies Research Center

Brief Summary of Project



United Technologies Research Center (UTRC) will develop new complex hydride compound(s) capable of achieving greater than 7.5 wt.% hydrogen capacities and 500-cycle reversibility with 100% efficiency. The focus will be on $Ak_xAe_yM_{+iz}(AlH_4)(x+2y+iz)$ in the multi-dimensional phase spaces formed between alkali (Ak) and/or alkaline earth (Ae) hydrides, metals (M), alane (AlH_3), and molecular hydrogen (H_2). The team will utilize 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 team will conduct three levels of performance evaluations to select compositions for further development, optimize dehydrogenation and hydrogenation catalysis with spectroscopic mechanistic studies and first-principles screening simulations, develop manufacturing processes to reduce cost and scale-up production, and develop business analyses for the commercialization of hydrogen storage systems integrated with fuel cell power plants.

Question 1: Relevance to overall DOE objectives

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

- PI project aims to expand beyond basic alanates.
- The project is in line with DOE goals and clearly states the main goals addressed.
- Hydrogen storage is a key enabling technology that is critical to the success of the hydrogen economy. This project supports the initiative through exploration of new materials for hydrogen storage.
- Project is addressing issues critical to the realization of the President's Hydrogen Fuel Initiative.
- Development of new compound to meet goals is important. Goals clearly identified.
- Aimed at improved alanates and thus build off the proven $NaAlH_4$, which is appropriate. Improved theoretical application would also directly address key questions.
- Clearly linked to goals and objectives of HFCIT RD&D program.
- Relevance will ultimately depend on results.
- Clearly looking to develop new materials to meet DOE storage targets.
- Materials under study important to meeting program targets.
- Focused on materials discovery of new high density hydrides.

Question 2: Approach to performing the research and development

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

- PI project aims to expand beyond basic alanates. Atomic modeling, molten solution processing are welcome alternatives to traditional ball milling techniques which will be difficult to scale up.
- The internal alloy selection process is not clear. As the PI has access to results from ST4 [The UTRC System Prototype Project], this would be a perfect opportunity to use the vessel design issues as part of the vetting process for alloy. However, this has not been addressed.

- The PI illustrates an acceptable approach with a combined use of modeling and synthesis methods. The approach also includes fast selection criteria of technical and economical nature. The approach does not give clear indication about the selection of materials really suitable for the DOE goals.
- Good, logical, sequential analytical, modeling, synthesis and experimental approach.
- Approach which consists of closely coupled theoretical and experimental components is sharply focused and a credit to the PIs and to the overall program.
- Approach is strong with ability to evaluate behaviors before experimentation, thereby not wasting effort on dead-ends.
- The approach has some unique aspects in comparison with other related projects sponsored by DOE. Computation and experiment are closely coordinated. Project is exploring some interesting processing options that have not been deeply studied in the past
- Broad but systematic approach. Predictive thermodynamic tools ensure a good materials selection process.
- Theory guided experimental tests is a good one. Exploring new technique to make material is appropriate as well. Looking at scale-up may be key in future, though of little use this year.
- Balanced approach between theory, synthesis, and characterization. Good variety of synthesis methods. ATM: how many candidates per day can be assessed? Is computational method faster than synthesis?
- An excellent combination of modeling and synthesis techniques ("processing"). The MSP [molten state processing] (Savannah River National Laboratory contribution) approach is innovative.
- Good approach to use different material processing methods to further study development of quaternary compounds. Coupled modeling to experiment. Validated model to experimental results.
- Interesting combination of modeling and processing approaches. Clarification is needed as to why these four specific approaches are combined together in this effort. Need more specifics about compositions/stoichiometries to be studied in each of the systems, as well as more details about the high-throughput experimentation.

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

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

- Poor candidates have been quickly eliminated. PI is showing reproducible results.
- Difficult to consider as this project started a few months ago. [The project has been running since ~May, 2004.]
- The technical achievements are consistent with the objectives expected for the period. There are no clear modalities for comparing synthesis methods, materials and validating models.
- Reasonable progress with concrete results during the past year.
- PIs are to be complimented on the theoretical screening of potential compounds prior to experimental preparation and characterization.
- Integrating experimentation with simulation. New materials discovery work progressing well.
- Developed several theory techniques for use in this field. The SRNL [Savannah River National Laboratory] work is not new so can not really be counted as progress. Albemarle's progress is of unclear value at present. Unclear how many points in each of the 7 systems were actually tested?
- Even if results are negative so far (in terms of meeting DOE targets), it still helps make definite conclusions on the systems investigated.
- Some success in achieving improved performance with molten state processing. Some interesting results from coupled reaction studies. Have not yet identified a reversible metal hydride storage material that approaches 2015 "system" targets.
- Good understanding of materials being studied. "Mapping" of new materials using experimental and predicted thermodynamic properties good approach. Will data be published? TiH_2 work encouraging.
- The use of predictive modeling is very nicely demonstrated. Nice study of the relative merits of three different synthesis techniques. The alanates are rather overworked. OK to show the value of modeling, but perhaps the time to move on.
- Extensive modeling and experimental studies have been completed to date.
- Until theory starts making tangible predictions of new compositions, the experimental effort is a bit of a "shot in the dark." The modeling effort needs to quickly start making predictions for the experimental effort to verify.

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

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

- PI should coordinate with UTRC tank project [ST-04 System Prototype Project] in order to better tailor research on metal hydride candidates. PI otherwise well coordinated with other labs and key researchers.
- There is adequate technical collaboration.
- The collaboration with the MHCoE and other projects should be more emphasized and made operative. The contribution of Norway is valuable and appropriate.
- Good mix of industry and research associates.
- Coordination appears in place and seems appropriate. PI could have spent slightly more time in presenting the contributions of the collaborators.
- Many capable partners contributing to this project.
- Multiple team program.
- A well organized, multi-institutional project; the presentation made clear what each participant is doing.
- Good partners for project.
- Excellent collaborations.
- Would be good to expand collaborations, particularly with CoE's.
- Though there are a large number of partners in this project, there does not appear to be any attempt to coordinate or interact with other projects.

Question 5: Approach to and relevance of proposed future research

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

- PI is pursuing materials past alanates however plan and strategy seems unconnected or weak. Original focus of systematic selection not evident in future work.
- The future work is well connected to the previous results. There is the need to focus more on the selected materials.
- Future plans seem reasonable and logical extensions of work done so far.
- Strong future plan building on current efforts.
- Continue close collaboration with DOE MHCoE.
- Not very well defined.
- Future plans are a logical extension of the presently established capabilities of this project and the findings from FY 2005 activities.
- Transition metal addition good direction.
- PI realizes the need to get to newer families of materials. Has identified a number of possibilities. Precise future directions and materials a bit vague sometimes, presumably because of IP considerations.
- Need to expand beyond alanates.
- Future plans are a bit vague at this point, because the modeling needs to make predictions of compositions to guide the experiments. Otherwise, the experimental effort will have to just pick a few representative stoichiometries. Since future plans involve addition of heavier elements (Ni, Co, Fe, etc.), the focus should be exclusively on compositions and systems where at least a theoretical density is higher than state-of-the-art.

Strengths and weaknesses

Strengths

- Non ball milling techniques are welcome. PI understands impracticality and limitations of ball milling. Solvent processing is generally more amenable to high volume manufacturing, molten process potential amenable to manufacturing if pressures remain modest.
- Organizational capabilities. Resources.
- The combined approach is a valuable method for rapidly developing and comparing materials.

- PI did an excellent job of presenting material in an unhurried, professional manner. Information was clearly presented with examples and sufficient data to illustrate accomplishments. This should be a model presentation for other PIs to emulate.
- Goal focused. Very good collaborative effort.
- Combined predictive methodologies. Broad approach still allowing thermodynamic productions of experimental results, reducing necessary experimental effort. Modeling compliments experiments, good match. Need to increase weight capacity search criterion to at least 9% (currently 7.5%).
- A lot of theory guiding things and checking against each other.
- A well organized/coordinated project. They have established a comprehensive capability to explore hydrogen storage material/concept options.
- The group has a balanced approach with good capabilities; combisynthesis.
- A very nice combination of modeling and synthesis optimization.

Weaknesses

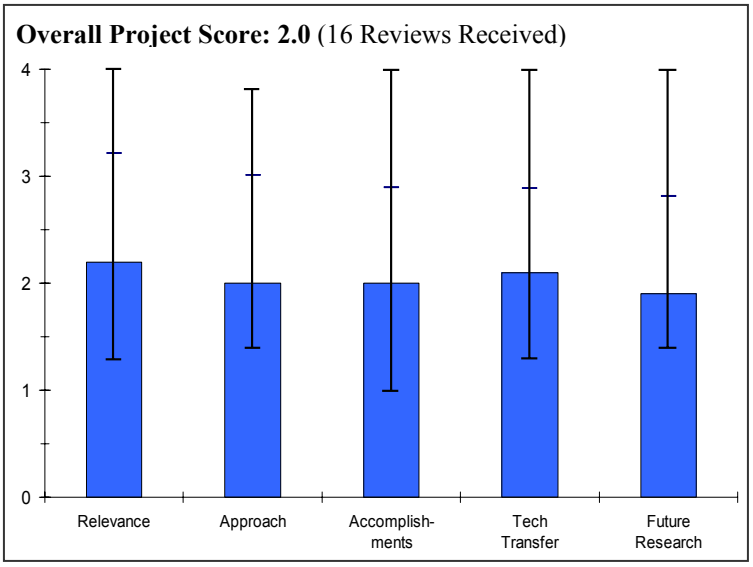
- Rapid elimination process may also miss a few good candidates, however at this stage, that is less a concern than cycling through all the possible permutations.
- There is no clear pathway to integrate lessons learned from other internal studies in the system development.
- The multiple approaches require clearer criteria to compare not only materials but also synthesis methods.
- Not much truly new coming out of the experiments. Need to give more data on what results for the 7 systems in high throughput were.
- Presently focused on metal hydrides.
- Relies too heavily on the overworked alanates, so far. Seems to have possible overlaps with ST5 (UOP) relative to both techniques and materials.

Specific recommendations and additions or deletions to the work scope

- Need to increase search criterion for new materials up to at least 9%wt (currently 7.5%). Focus on alanates decreases probability of success.
- It seems as though excessive ball milling in order to achieve nanoparticles is a misleading practice. Often, in a real tank system, nano particles will decrepitate after cycling and continued vibration and thus any advantages gained will be quickly lost. Most current systems find that due to packing factor and expansion issues, nano particle sizes may not be practical.
- There is a need for additional experimental work on the selected materials.
- Move away from alanates. Use a single preparative method (molten processing, etc) for model compounds as upon cycling the material will come to most thermodynamically stable state anyway.
- Limit the use of Co, Fe etc to amounts that will allow for at least 4%, preferably higher, storage.
- The project staff should consider how seamlessly they could transition their methodologies to other types of materials/concepts for H-storage, e.g., should major breakthroughs occur outside the metal hydride arena. For future reviews prepare a prior year to current year progress comparison to help gauge performance in the budget year.
- As indicated by PI, move into newer systems: borohydrides, amides, etc. Can there be some coordination with ST5 (UOP) to avoid duplications of effort? Will corporate IP issues prevent that?

Project # ST-07: Sub-Nanostructured Non-Transition Metal Complex Grids for Hydrogen Storage
Talu, Orhan; Cleveland State University

Brief Summary of Project



The objectives of this project are to grow sub-nanostructured metal grids (<1 nm thick) with about 50% microporosity (<1 nm wide) and measure hydrogen uptake/release on these novel metal grids for hydrogen storage applications. The premise is that the overall hydrogen dissociation reaction rate will be higher (because the external metal surface area is enhanced), and the diffusion time constants will be lower (because the diffusion path is greatly reduced). In addition, it is hypothesized that conduction in the highly interconnected metal grid and convection in the micropore space may enhance heat transfer. The nanostructured metal grids will be grown from pure and alloyed non-transition metals and the phase diagram is anticipated to be different from the bulk phase diagrams because of the quantum effects that may arise at these length scales.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.2** for its relevance to DOE objectives.

- This is an interesting approach to synthesize new materials; however the likelihood that this can achieve any appreciable hydrogen storage capacity is very low. This material may however have application in micro channel heat exchangers or gas diffusion layers if metals other than Fe are used.
- The project is obviously relevant if it works and can meet the cost targets.
- Fast kinetics is crucial to metal hydride storage.
- The project is relevant to the DOE objectives.
- This was a concept that was worth investigating to see if it could meet the storage goals, but it does not appear to be making sufficient progress.
- Materials development work based on a new concept that could support the Hydrogen Initiative.
- This effort is reasonably aligned with the goals of the President's Hydrogen Initiative. There are also potential contributions of this work to the field of catalysis which are also highly relevant to energy consumption but less relevant to this particular initiative. A higher risk, potentially higher payoff set of objectives.
- Novel concept with new approach. Not much hope of meeting goals even with other materials.
- This project does address specific critical goals of the HFCIT program in the storage area; it just does not do so in a very compelling or convincing way.
- Interesting idea. Questionable economics. Difficult experimental challenge. Path to large scale production?
- The project does not seem to quantitatively address many of the DOE targets, in particular gravimetric and volumetric energy densities.
- Project does not address program targets.
- Topic is largely irrelevant to DOE objectives. The topic is high-risk and no real foreseeable payoff for hydrogen storage. Even if these materials could be made (which would be an extremely major accomplishment) they simply will not enable high densities, and will not survive repeated cycling. Thus, the approach is of limited/no value to this DOE program. Probably should not mention "quantum effects" unless there is some real evidence or reason to suspect they will increase capacity. To date, there seems to be no evidence.

- There is potential for great relevance, however, there are several barriers which must be addressed (e.g. mold removal, loss in volumetric capacity, cycleability, deposition techniques, etc.) before this will be fully relevant to DOE.

Question 2: Approach to performing the research and development

This project was rated **2.0** on its approach.

- The electrodeposited materials that the PI intends to use will hydrate! This will eventually break down the structure of the material after a few cycles. It seems unlikely that significant cycling capability will ever be demonstrated even if acceptable capacity is initially demonstrated. All materials that can be electrodeposited have some capacity to hydrate hydrogen. It would be difficult to see how homogenous alloy layers can be deposited due to the different electropotentials of each metal. Otherwise the approach is very interesting in terms of using simple and cheap zeolite materials to form a "negative" mold. This is certainly valuable work for undoubtedly many other applications.
- The approach is clear and the project seems well designed, but as the project is high-risk and exploratory, it is not clear at this time that it is feasible. But, of course, that is the nature of good, high-risk, exploratory work.
- The proposed approach is very innovative. May not be applicable to metal hydrides of practical interest, namely complex metal hydrides.
- The approach identifies some key issues. More focus and justification are required for the research choices.
- Tried several novel approaches to develop hydrogen storage media, but none seem to be yielding results.
- Novel, interesting approach but not enough evidence about the feasibility of this work program and how the targets are actually addressed.
- Generally well thought out and appropriate approach.
- Good approach given the objective to make a high surface area material of metal for use in storage. However the sought material is highly unlikely to be stable at relevant temperatures and under the stress of hydrogen insertion and removal, so the goal is unlikely to be met. And it can not make the goals.
- Creating nano-porous structures for hydrogen storage is a good idea in a general sense; however, there must be better approaches than what this project proposes (see Recommendations below).
- Are there examples of anyone creating these metal structures from zeolite templates? Hydrogen behavior on small metals is well known. Copper is known to be a very good metal for plating/electrochemistry. Other metals may not work as well. Simple gas adsorption techniques needed to characterize microporous materials. Should consider mesoporous materials like MCM-41 as additional transition to zeolites. Pore blocking by initial deposits is likely to be a problem.
- The whole approach is doubtful to this reviewer. While heat transfer and gas impedance might be improved, one would think gravimetric and volumetric densities would be decreased. Repeating my unanswered question of last year, can PI make any calculation that would suggest that the volumetric and gravimetric DOE targets could be theoretically achieved?
- Only addresses kinetic properties. No capability to synthesis high capacity compounds. Only addresses metal systems which are known to be too heavy.
- Does this procedure work for anything other than metals? Most hydrogen storage materials under consideration today (if not all) are not classical metal hydrides (e.g., $NaAlH_4$, $LiBH_4$, $LiNH_2$, etc.) So, the utility of this procedure is of significantly limited importance for the DOE objectives. Also, the very large surface areas of this approach will also translate into an effectively large volumetric penalty of the hydrogen density (compared, say, to the single crystal density). Gravimetric density is not the single controlling factor behind solid-state storage materials. Volumetric density is, in fact, the whole reason why solid-state materials are considered at all.
- Issues were well explained. Nothing specifically about metal hydride work; is this going to be part of the project? Possibilities for increased hydrogen storage are great.

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

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

- PI has made good progress in synthesizing the material. PI has demonstrated good control in modifying the characteristics of the material.
- Still more exploration must be done before one can say it is going to be feasible.
- The project is exploratory and timely progress is difficult to predict.
- The PI's presentation gives a limited overview of the major results, showing the achievement of the proposed objectives.
- Most technical approaches reported have not been successful. Results of research so far are not meeting objectives. Several approaches have been tried without success. It is unlikely this avenue of research will result in technology that can meet the storage goals.
- May need to speed up the characterization part of the work.
- The rate of progress towards the objectives has been somewhat slower than might be expected given the size of the project and financial support provided.
- Inserted copper in zeolite and started removal. The key will be next year's progress. Have not done any useful metals yet.
- Progress to date is not impressive and seems peripheral to the main goals for the project. The current effort is more about finding ways to make the originally conceived project actually work at some level than about being at the stage of making something to begin testing.
- Good progress on growing films. Real problem will be metal grids. How much copper is getting deposited on zeolite films? Should be able to calculate whether pores are being filled.
- Some modest progress in doing what PI wants, but almost no indication that work can overcome any storage barriers.
- The group has only attempted nonhydride forming material depositions.
- The group did not answer the issues/questions raised by the reviewers from last year.
- Spent lots of time that was wasted due to microcrystal growth closing off electrolyte solution during plating. Nothing stated about milestones.

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

This project was rated **2.1** for technology transfer and collaboration.

- PI is working independently. Given that the work is unique, many collaborators in manufacturing of zeolites or electrodeposition experts could be consulted for help. PI should look to suppliers and manufacturers outside the hydrogen storage community to determine if interest exists for collaboration.
- Most outside contacts are with other researchers at this time. But, given the high-risk exploratory nature of the work, significant involvement of potential implementers is premature at this stage.
- Not applicable due to the novelty of the proposed technique. The PI could envisage some sort of collaboration with the DOE Metal Hydride Center of Excellence. They are planning some overlapping activities in this area.
- The collaborations are limited but appropriate.
- PI is working with respected technical collaborators, although, from the presentation, it is unclear what their contributions have been.
- The project could benefit from more extended collaboration with other institutions. No evidence of technology. To the defense of the program, is this really applicable?
- Coordination and collaborations were less apparent in this project. At this stage in the project collaborations may be less necessary.
- Appropriate outside academic collaboration. No industrial input.
- This project desperately needs some strong collaboration that can send it in a more promising direction (see Recommendations below). It is not clear that the listed collaborators have been or currently are engaged in the project.
- Partners with expertise in zeolites/catalysis would be good addition.
- Minimal collaborations. No experienced hydride people? Tech transfer possibilities not very clear at all.

- Does not appear coupled to rest of program PIs.
- Very little talk about outside work. Some collaboration could be possible for help with metal hydride work, when it starts.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.9** for proposed future work.

- Looks fine. Logical progression.
- The work is not completely clear and does require more experimental work using hydrogen to start the experimental verification of the claimed advantages.
- Question whether future research along these lines will come any closer to meeting the FreedomCAR goals.
- Not entirely convinced future plans can provide the required results.
- Future research plans appear reasonable.
- Can not really be suitable because it just will not work.
- Directions for future work sort of make sense in the context of the present approach but I am not optimistic that results from this particular project will ever lead to a functional, cost effective, durable hydrogen storage/delivery system.
- Need to answer question of metal growth in zeolites sooner rather than later. Real question is whether grid can be made.
- Pd and Mg will not lead very well in the direction of eliminating barriers. Should get to more practical H-storage alloys as soon as possible.
- Proposed metal depositions not consistent with program goals.
- Issues are well known and pathways were explained to overcome these issues. Lots of ideas expressed during presentation but need to focus-in on ideas. Nothing explained about actual metal hydride work.

Strengths and weaknesses

Strengths
- Extremely interesting material research, material synthesis seemingly fits all criteria for economical scale-up and production - cheap , proven base (zeolite) material available which already has many other applications. Could this material also be used as a membrane or filter?
- The PIs are experts in their field of zeolite and related catalysis.
- Good idea.
- Novel way of making nanoporous metal structures that is potentially useful in other applications.
- The preparation techniques of nanoscale grids may be of wide benefit to the Sub-program.
- Innovative approach.
- High risk project that may, if successful have broader impacts beyond hydrogen issues.
- Innovative out-of-box thinking, nice-looking idea.
- High [surface?] area and simple concepts.
- The approach is very much different from that of other HCFIT storage projects, so, in a sense, it represents an alternative to the more conventional hydrogen storage approaches taken by the larger family of HFCIT storage projects.
- Hard to identify any. Might be more appropriate to carbon as hydrogen storage medium.
- Possibilities of this approach are unlimited. May help get the metal hydride area of research over the “hump” and achieve the 2015 DOE goal of 9 wt.%-- a goal conventional metal hydrides have trouble overcoming.

Weaknesses
- Material has no chance of being a practical hydrogen storage material with cycling capability. Unlikely to be cheap enough to remanufacture several times to be used as a chemical hydride type of material.
- None apparent at this time, but could do a better job of presenting concept of electrodeposition; hard to visualize.
- The PI did not properly address the drawbacks of his approach, namely the loss in storage capacity and how to avoid decrepitation of the nanoporous structure.

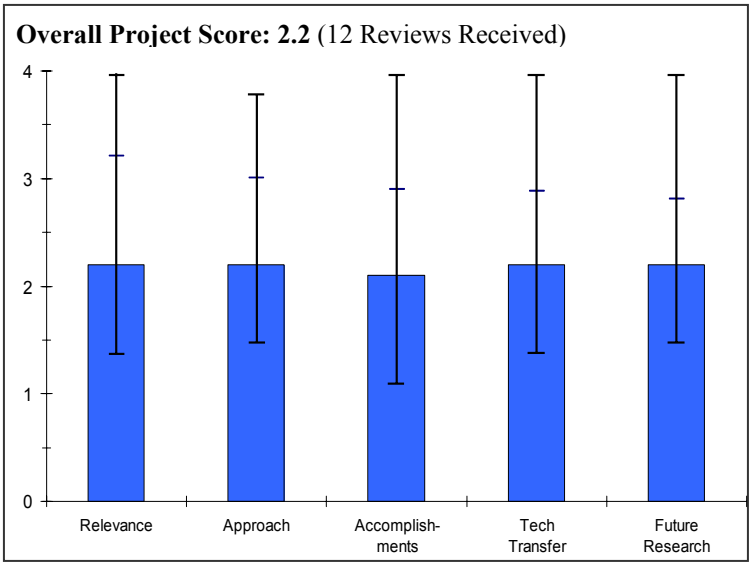
- The advantages on hydrogen storage systems of the developed grids are not yet verified.
- Rather high risk project that does not seem to appropriately address the targets.
- Probability of significant contribution to Hydrogen Fuel Initiative is less than other projects reviewed thus far. This is not a criticism of the project relevance only an assessment of probability of successful contributions to the Initiative. Perhaps this effort got off to a slow start.
- Idea is irrelevant to hydrogen storage applications.
- Still feel the structure will be unstable and the program has almost no chance of success.
- To appreciate the possibilities of success for this project, one must recognize that it is at an early stage of progress and that something exciting could come from the research over the next year or so--personally, I am not optimistic. The basic idea of creating nano grids is a reasonable one, but molecular sieve-type templates may not be the best choice; templates that can be formed in a truly contiguous architecture would work much better.
- Investigators need to look at existing catalysis literature. Doubtful that quantum effects occur. Should increase kinetics, but not capacity. Difficulty of making grids is probably underestimated.
- Of highly doubtful value to eliminating barriers, compared to DOE money spent.
- The fundamental approach of the project does not appear to be applicable to the development of lighter weight, high density hydrogen storage materials.
- Lots of barriers before results can be seen. Weakness of scale-up. Still same issue with 50% loss of volumetric capacity; presenter said "No way to prevent this."

Specific recommendations and additions or deletions to the work scope

- The project should have go/no-go milestones.
- Future work will begin to explore H_2 storage capable materials. PI should shift focus to find materials suitable for heat exchange materials. Could this material be suitable as a heat exchanger for new fuel cell coolants using Teflon based materials? The corrosion to the metal would no longer be an issue.
- This is excellent research despite the poor ratings. This research should continue, however applications with the FreedomCAR body materials group or fuel cell tech team should be investigated. PI should contact manufacturers of micro channel heat exchangers to determine if his research has potential for their application.
- While this is an interesting material development technique, it is significantly out of the focus area of the DOE hydrogen storage work. It is not clear how the successful completion of the preliminary tasks will help DOE's storage targets. As PI stated, in some respects such as volumetric storage it will have the opposite effect. It is recommended to phase this program out of the "Grand Challenge."
- None.
- Need to determine at an early stage the family of metal hydride that could be prepared via this technique.
- The addition of specific experimental work with hydrogen will accrue the achievements of the project.
- Consider ending project or moving it to DOE BES.
- Need to interface with other institutions that can give a 'boost' to the research work with their related experience.
- Recommend looking at uncosted balances for the project and if large, consider stretching out budget and project periods accordingly.
- Consider to change scope of the project completely for relevance to DOE targets.
- This program might have much more application in other fields, or in unknown applications that do not require the metal to ingest hydrogen. This program would be much more appropriate in DOE BES and if a way could be arranged to hand this off to that office it would be more appropriate there. If moved, it should not be a hydrogen project anymore but general material generation.
- I recommend that the project staff arrange visits to one or more of the DOE/BES Nano-Science Centers to explore alternative options for the template structure-type and architecture, e.g., anodized, atomically ordered arrays, deep x-ray lithography, there are others. I have a sense that in the next year or so materials concepts emanating from the nanoscale activities in other HFCIT storage projects, particularly the Centers of Excellence, will overwhelm what this project is trying to accomplish.
- Need to consult with zeolite experts.
- Recommend discontinuing project unless some theoretical calculation is made that shows any hope of reaching DOE gravimetric and volumetric target energy densities.

Project # ST-09: Clean Energy Research at the University of South Carolina
White, Ralph (PI); Ritter, Jim presenting; 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. 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. Both of these electrolysis processes are steps in proposed thermochemical processes. 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). Hydrogen Storage: 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, congressionally directed project started in 2004.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.2** for its relevance to DOE objectives.

- The study of co-dopant is a welcome change from the traditional Ti based dopants. Investigating steam hydrolysis of borohydrides will never be a practical automotive system. Work presented by Dr Michael Matthews was conducted in collaboration with DaimlerChrysler almost two years ago. DaimlerChrysler abandoned this effort due to the unmanageable complexity of a steam based $NaBH_4$.
- A number of research areas are redundant, and covered by other programs. It is not clear how this project will be serving the objectives effectively.
- Hydrogen storage is a key enabler to the success of a hydrogen transportation system. This project has several parts all of which may be able to further the knowledge base; some parts more so than others.
- Goals are reasonable but the diversity of the goals and lack of cohesiveness of the goals of the overall project is a detriment to the potential contributions of the project. The goals of this task (Project II: Development of Complex Metal Hydride Hydrogen Storage Materials) are not particularly novel.
- Several types of projects using this budget. Although the targets are identified, the performance to the targets is not.
- Some of this work applies to storage but the majority is not storage work. That work does not apply to this important task though it does potentially aid the overall goal.
- Materials are not going to meet DOE goals, but good models. Dry steam work interesting, but real problem is regeneration.
- The H_2 storage tasks are relevant to the DOE storage program. The other tasks may have relevance to the other DOE H_2 program areas.
- Note: There are 5 projects covered in the contract. This reviewer covered only the two storage projects (II & III).
- Both projects are reasonably well oriented toward DOE vehicular storage targets.

- Portions of the work appear to be high quality, and well focused on DOE objectives. Combination of work on enhanced kinetics (e.g., carbon catalyst) and high capacity (e.g., $LiAlH_4$) could prove fruitful in the future. Task II (complex hydrides) appears to be very competent, well-aligned, and focused on DOE objectives. Other portions of this project do not appear to be supporting DOE objectives.
- Too much emphasis on sodium alanate, which we know will not meet targets.

Question 2: Approach to performing the research and development

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

- Carbon and iron and possible Zr based dopant may be cheaper than Ti dopants. Use of these dopants seems to be more flexible than Ti-based and may have uses beyond alanates (always a bonus). DaimlerChrysler investigated steam hydrolysis and determined that the energy load required to dry $NaBH_4$ in order to move it as a powder is prohibitive; steam-based systems had a tendency to plug and form "cakes." Moving powders is much more difficult than liquid or gas in an automotive system - Creating a thin layer powder (coating on a glass bead was meant to determine monolayer kinetics of the system only. $NaBH_4$, once reacted with water, will form a $NaBO_2$ shell which is impervious to water and thus the reaction rate decreases dramatically. Attempts to portray this work as a "breakthrough" in improving kinetics is highly misleading.
- The alloy vetting process (with respect to system development) was not presented.
- Technical approaches of most aspects of the research seem competent and logical.
- The breadth of efforts in this project is a detriment and severely limits its potential contributions. The approach followed under this task (Project II: Development of Complex Metal ..., which entails the consideration of dopants in traditional metal hydride systems,) has some aspects that may lead to progress in overcoming barriers but largely is behind the field.
- Although the approach is okay, it appears duplicative of other work going on in the program that is further ahead – the group even admitted as much during its presentation. No funding breakdown for each "mini-project"
- Synergy work is appropriate but needs to be moved to making better systems. Graphite work is interesting though not improvement. The $MgAlH_4$ work is of dubious value as it is irreversible thermodynamically. Hydrolysis work does not approach the key problem which is recycling. But much of the work does little to advance storage.
- Materials being targeted are still at 3-4% wt capacity. Alanates have been extensively studied by others and results have been reported. Dry steam hydrolysis approach for chemical hydrides is interesting to make some improvements in kinetics but does not help in solving principal problems such as storage capacity, regeneration issues, etc.
- Co-dopant approach fairly unique. Carbon doping work promising. Mechanistic work would be good addition.
- The approach is to look at combinations of dopants to enhance hydrogen absorption and desorption in alanates. The novel aspect of the approach is to use sonochemical pretreatment prior to ball milling. The chemical hydride approach is looking at the dehydrogenation of the hydride using steam rather than liquid water to produce a dry product. The objective of the research is to understand the reaction mechanisms to fully utilize the steam and maximize the H_2 delivery rate.
- Combination of dopants with Ti may lead to improvements in kinetics and capacity relative to doping with Ti only. Not attempting to understand the underlying mechanisms of the doping process. Li alanate is fully reversible (only 5%capacity below about 120 C) but not likely to be done at a gas station. Thin films of sodium borohydride.
- Project II is oriented toward catalyzed alanates. Although there is some innovation (e.g., combined carbon-metal catalysts), it seems to significantly duplicate other alanate activities funded by DOE. Project III is oriented toward chemical hydrides, in particular the borohydrides. While the steam reaction approach is innovative, the practicality will be dictated by the difficult challenge of borohydride regeneration costs.
- Much overlap with other projects in program. Only focused on sodium alanate. Dry steam approach not applicable to onboard storage because system parameters would not meet targets.
- Interesting approach to new catalyst materials, and distinct from others in the field (Task II - complex hydrides.) Other Tasks are not as well designed.

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

This project was rated **2.1** based on accomplishments.

- Alanate work seems to be progressing at an acceptable rate. Sodium borohydride work: This is not novel work. This work was conducted almost two years ago for DaimlerChrysler and verified by DCX researchers.
- It is difficult to gauge as most of the work were redundant.
- I could give a better assessment if each technical sub-project were able to be rated separately. The Ritter storage work is novel and overall very interesting. Work with carbon is very interesting; a possible use of low cost materials that can help address the cost targets of the program. The $NaBH_4$ work is neither new nor innovative.
- Accomplishments are at best fair given the resources applied and not a credit to the overall Initiative. There is no evidence of collaboration between the PIs within the project as evidenced by publications thus far. Accomplishments and progress under task II are more substantial that the rest of the project and are good.
- Observed variation from simple mixtures of dopants. This is not fully new, though cheaper catalysts will be of use. Faster desorption with carbon added or with sonocation which pointed to mechanical size reduction. Claim $LiAlH_4$ at 5% at 150C but no data or proof. And it is only reversible under severe conditions so this may not be an accomplishment. Understanding of dry hydrogen is scientifically interesting but ignores the main problem of recycle.
- Studying materials, but results do not appear well documented. Reversible lithium hydride system interesting, but not enough information provided.
- Good progress for first year on doping work.
- Sonochemical pretreatment showed improved uptake kinetics for sodium alanate with 2% Ti dopant. Some preliminary data reported on dehydrogenation kinetics of recrystallized thin films of $NaBH_4$. Initial rates were 2-3 times faster than slurry reactions. However, yields were less than 100%.
- Reasonable and interesting progress on both projects. DOE targets cannot be met with the alanates. Only marginal improvements shown in this project.
- Not competitive with other projects in program that are attempting to determine mechanisms in sodium alanate.
- Good progress on enhanced catalysts in complex hydrides. Rating could potentially be "outstanding" depending on the results of the $LiAlH_4$ reversibility, which were not shown due to IP issues.

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

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

- USC is working closely with SRNL [Savannah River National Lab] which is strong in this type of research.
- Some portions of the project have reasonable levels of collaboration. One aspect has none. Collaboration by other researchers is not reported.
- Outside collaborators exist but their contributions were poorly described.
- Limited collaboration with others. Might be worth trying to incorporate into DOE MHCoE [metal hydride center of excellence] activities.
- Broad team. Adequate collaborators.
- Limited collaborations outside of immediate partners.
- Some collaboration was reported. A means of integrating this work into the CoEs should be explored.
- Reasonable collaborations, but not extensive. Would be good to join MHCoE and CHCoE [DOE Metal Hydride and Chemical Hydrogen Centers of Excellence] to enhance possibilities for tech transfer to industry. Is SRNL collaborating on melt processing with USC and UT (ST6)?
- Only limited interactions with others.
- Storage work is closely connected with external researchers and other DOE projects. Other work seems to be independent and somewhat disconnected.

Question 5: Approach to and relevance of proposed future research

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

- Good plans for alanate and co dopant work. Borohydride work: It is highly unlikely that realistic improved kinetics will be achieved for steam hydrolysis. This system may only be practical in stationary applications where excess steam is available; however, the overall system efficiency will likely not exceed traditional cogeneration techniques.
- Ritter future plans are good and could provide valuable information. Mathew's proposals for $NaBH_4$ do not appear to be breaking any new ground. I question need for prototype systems when there are already industrial companies fielding prototype systems. Weidner plans really need to be assessed in comparison with other hydrogen production projects to evaluate value and direction.
- It is difficult to envisage this project as making significant contributions to the President's Hydrogen Fuel Initiative.
- Future work may yield improvements, but would be much better if this project worked more closely with DOE and the FreedomCAR & Fuel Partnership.
- $LiAlH_4$ is appropriate if onboard reversible but less so if really a chemical processing method. The $NaAlH_4$ probably is not, as it is only showing what is known, that size and dislocations help.
- $NaBH_4$ work: Economics need to include reprocessing!
- The proposed alanate research could lead to improvements but the alanate system is not likely to meet the DOE goals.
- Continues to work on alanate. Proposed methods of study do not appear to have the potential to contribute to greater understanding or to enhance the performance of the alanate. Chemical hydride work is not well defined.

Strengths and weaknesses

Strengths
- The project is in reasonable alignment with the needs of the program and progress is good ($NaBH_4$ work aside).
- The enthusiasm of the PIs. Willingness to change course and flexibility.
- University of SC has strong capabilities. Experience working with DOD.
- Dry steam hydrolysis approach.
- If $LiAlH_4$ is reversed then that would be a strength. Need to address the difference from the expected amount. Seems that it is not actually reversible on-board.
- Steam reaction with $NaBH_4$ is an interesting new approach.

Weaknesses
- $NaBH_4$ work has been evaluated before. The PI does not offer any new expertise or revelations.
- The project is not effectively using the wealth of information already available. The alloy vetting process must be clarified and periodically re-calibrated using system analyses projects
- Too many technical areas in one project to evaluate properly. There are such different technologies being pursued to such different levels of success that evaluating it as a whole is very difficult. Some of the work is really novel and could be beneficial, but the remainder of the project work drags down the evaluations through the need to find an average.
- Project alignment to DOE goals seems to be weak.
- Focusing on well known materials such as alanates, boron hydrides, etc will most likely NOT help in meeting the DOE principal targets.
- Need to address: Is the catalyst work truly learning anything new, is this really just dislocation and size reduction? Does not address the key problem of hydrolysis (recycle); seems to ignore the difficulty of fueling and especially defueling solids.
- Steam work looks at the "easy" part of the problem. The real killer is regeneration of [spent, the oxide] $NaBH_4$.
- Not well coordinated with the CoEs. Only a portion of the funding is being used for H_2 storage research.
- Alanate work (P.II) somewhat duplicative and limited in potential. $NaBH_4$-steam process (P.III) will have the usual borate regeneration cost problems.

- Not in step with DOE program, other constituents (e.g., auto companies), other PIs in program. Most of proposed work does not appear to be relevant to program.
- An earmarked [congressionally directed] project is always going to be a weakness, simply because it does not undergo the rigors of peer-review. In this case, some of the work (specifically the storage work) seems to be well-aligned and of high quality, but for earmarks that is really the exception rather than the rule.

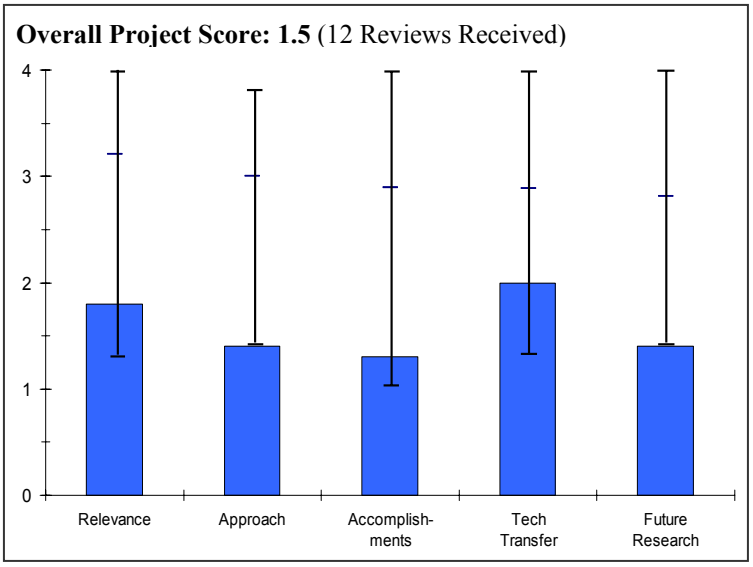
Specific recommendations and additions or deletions to the work scope

- For Project II, I suggest moving out of alanates into other more advanced materials. Work is duplicative of others and cannot likely meet DOE targets.
- Work on the $NABH_4$ system is not novel, it has been evaluated as impractical before by more than one automaker. Stationary applications are unlikely. Regeneration of the $NaBO_2$ would still be an issue which will also never be economical to regenerate due to the large thermodynamic well of B-O bonds.
- The technical approach is disengaged from onboard storage end product development. Specifically, one of the proposed systems required steam to facilitate hydrogen release. There was no clarification or further explanation where the energy for steam and/or water source will come from onboard the vehicle. The program scopes and focus are not appropriate to stay within the Grand Challenge.
- This project really needs to be separated into its parts for individual review.
- Reduce scope to increase focus, adjust project funding downward accordingly.
- Do not present non-storage projects in Storage review session. Work with DOE and the FreedomCAR & Fuel Partnership to get better alignment to goals.
- Redirect storage project scope towards materials potentially capable of meeting capacity targets. Collaboration with DOE Chemical Hydride Center of Excellence on the steam hydrolysis topic would be very useful.
- Move fuel cell and production work to the appropriate budgets. We saw very little about the other 3 areas and were not really competent to review it, all the more reason to split up the program between budgets. It is nice that the DOE team was able to shape this program to actually be of some value when it did not go through the technical review.
- Coordinate work with the CoEs.
- Project II: Suggest discontinuation of work on alanates. Project III: Coordinate with Millennium Cell on cost problems. Cease work if no hope of substantial cost reduction.

Project # ST-10: Fuel Cell and Hydrogen Research University of South Florida
Stefanakos, Lee; University of South Florida

Brief Summary of Project

This multi-faceted University of South Florida project will investigate several hydrogen production techniques; hydrogen storage in the areas of transition metal hydrides, nano-structured materials (nano-composite conducting polymers), and components designed for manufacturability; electrode improvements; water and thermal management; and a testing facility for fuel cells. This is a cross-cutting, congressionally directed project started in 2004.



Question 1: Relevance to overall DOE objectives

This project earned a score of **1.8** for its relevance to DOE objectives.

- The goals of the project are reasonably aligned with the President's Hydrogen Initiative.
- DOE targets (system basis) are stated, however, characteristics of materials being investigated are far below those requirements.
- Some relevance to H_2 storage program is evident.
- Three tasks related to storage: Transition Metal Hydrides, nano-structured Materials, and Hydrogen Storage Components.
- The project scopes are diverging from the DOE's grand challenge framework and goals.
- A cross-cutting project including conducting polymers, nanoscale transition hydride materials as potential storage materials. Just started a study of a manufacturing process based on nanostructured film approach. Starting three "new" tasks - geologic storage of hydrogen, new materials and advance thermal compression of hydrogen.
- Team lacks experience and data does not uniformly seem accurate.
- Materials under study not relevant to program. Researchers are not focused on DOE objectives, and have not answered criticisms from last year's review about analyzing even the theoretical limits of their approaches.

Question 2: Approach to performing the research and development

This project was rated **1.4** on its approach.

- Some of the approach may be OK but other parts lack viability.
- Little conceptual or theoretical approach.
- The approach is limited with respect to materials studied and methods, processes.
- Materials under study have wrong thermodynamics for storage application without possibility of changing them significantly.
- It is unclear if this technical approach will result in any new knowledge or add any significant value.
- The diffuse nature of the project limits potential for meaningful impact on the goals of the President's Hydrogen Initiative. Given the size of the project a more meaningful coordination of the approach is imperative.
- Oxidized catalyst unlikely to survive in hydrogen atmosphere. PI has not identified any metal hydride type material that has potential to reach 2007 or 2010 targets. The PI is not conducting any basic research on these materials that may translate to higher capacity materials.

- One novel approach that is being investigated is the use of electrical charge to desorb hydrogen from the carbon nanocomposites. The second approach is to investigate nanoscale transition metal hydride systems. The third approach with thin films does not appear to present a path to a system with high enough storage capacity to meet the DOE targets. The TGA data seems to indicate 15 wt.% storage capacity.
- Conducting membrane approach is interesting.
- Some of the alloys for hydrogen storage start at very low storage capacities, well below system goals of the program. It is not clear how the PIs will be closing the gap. There are no internal vetting mechanisms.
- Three empirically-selected approaches have been demonstrated without presenting evidences that those can potentially meet targets.
- The use of voltage to maintain capacity seems certain to have excess parasitic loss.
- MgFe is well understood and will not meet goals.
- Why would alignment increase adsorption? Low fields wouldn't ionize or align polymer
- What is path for thin films to be commercialized?
- Materials seem to have low capacities and high cost.
- Electrical process is not clearly on plan. Hydrides with appropriate capacity are not defined.

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

This project was rated **1.3** based on accomplishments.

- No significant results that indicate research direction or results could be capable of meeting storage goals.
- Progress has been slow thus far and is addressing issues largely previously resolved by the community.
- Limited new information/data.
- Slide 12 indicates storage capacities of 8 and 15 percent. This is not possible when the maximum theoretical capacity of Mg_2FeH_6 is only 5.5%.
- Only about 3% wt capacity has been demonstrated in relatively mild conditions for the approach 1.
- Deviation in adsorption and desorption points toward a measurement problem by people new to the technique.
- 5% in Mg_2Fe system at 300°C does not seem relevant to goals nor is it very new, plus only about 2% is really viable.
- Experimental results seem questionable. H_2 capacity is not a simple measurement. 15% H_2 on $LiMgFeH_6$ which should contain max of 6%H_2.
- Hydride material has demonstrated only 5.0 wt.% storage capacity.
- No viable storage media has been identified. Conducting polymer show low hydrogen absorption and even lower desorption. Cannot meet capacity targets.
- Too early to study manufacturing processes on these unacceptable materials.
- New Materials discovery not discussed in any detail.
- Thermal compression has been studied several years ago.
- There has not been a great deal of progress over the past year.
- Volumetric storage density of the proposed materials was not presented.
- The results from the carbon-doped nanocomposite work showed that the H_2 capacity is only about 3%.
- TGA data for Li-modified Mg_2FeH_6 indicates about 15% weight percent hydrogen released from the material at a temperature of about 300°C. Data needs to be confirmed.
- The hydrogen storage measurements (e.g., from TGA) look to be done incorrectly. Data is inconsistent with even the theoretical maximum density for this material.

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

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

- Collaboration among several Florida Universities but no industrial and/or commercial participation noted.
- Speaker mentioned that they are in the process of identifying additional potential collaborators.
- The team is diverse and well connected.
- Interaction with a number of collaborators is evident. Interaction with Hy-Energy should improve the analysis capabilities of the project team.

- Good to see Hawaii, NREL, and SWRI (experts in hydrogen storage) involved as partners in this project. These partners should help guide this effort towards more relevant research activities.
- PI has not collaborated with any established players in hydrogen storage.
- The PI lists many activities and personnel in his "partnership" but it is unclear from the presentation exactly what any of them are contributing.
- Research apparently mostly being done by three Florida universities. Role of other partners not clear.
- Need to make contacts with people who can help you to do the right experiments correctly. Collaborators should be used to examine validity of assumptions involved in polymer work (field alignment, etc.)
- Little apparent interactions with other labs.
-

Question 5: Approach to and relevance of proposed future research

This project was rated **1.4** for proposed future work.

- PI needs to understand 2010 targets and propose realistic mechanisms which may allow them to achieve this goal.
- It is still not clear that a pathway to higher storage capacity has been identified. A clear indication of the science behind the work on destabilized materials is not evident.
- Seems to be just a collection of "ideas" related to hydrogen storage and fuel cells lacking a coherent development plan.
- It is unclear from the presentation comments by the PI that he has a clear plan for next steps in the transition hydrides area.
- Hydrogen compressor work would seem to be better aligned to Production or Delivery. (Note: this is a cross-cut project and the compressor task is aligned under the hydrogen production and delivery work)
- Effect of potential is interesting but ignores the effect maintaining a field will have on net storage.
- LiN system has been well studied; no clear point here. 8% hydride would be more interesting if there was more detail on what it was but based on what was presented this team is not likely to understand the material.
- Need mechanistic work: compressor using electrical discharge very inefficient: better sources of thermal energy. Work with Hera.

Strengths and weaknesses

Strengths
- PIs have more extensive background in electrical engineering. Suggest that PI continue work on novel electro activated composites- these materials have potential for success in a system.
- USF does appear to have some capabilities in hydrogen research.

Weaknesses
- Lacks understanding of definitions of 2010 targets and the demands of automotive or stationary systems put on these storage materials.
- Unfocused activities without clear direction or potential to achieve worthwhile results.
- Though there were slides for previously identified issues, no discussion for them was given.
- Few empirically-selected well known-systems will provide very little probability of success to meet the targets.
- Not clear storage measurements are being made correctly. Needs verification by SwRI. Little understanding of the field or what has been done.
- Experimental approach seems fragmented. Serious questions about experimental results.
- An integrated project plan is not evident. The approaches look to be only loosely coordinated.
- Very large number of participants, but split among different unrelated projects.

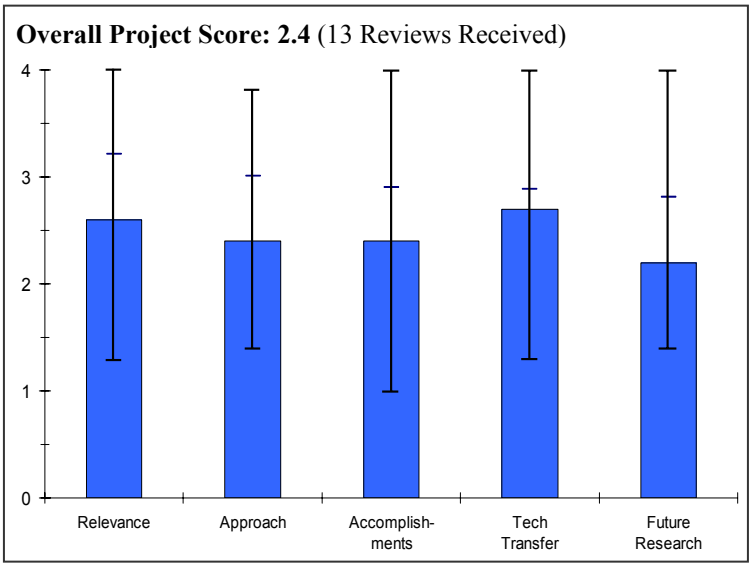
Specific recommendations and additions or deletions to the work scope

- PI should terminate work on Mg_2FeH_6 as it has been researched extensively for many years and concentrate on the conducting polymers which may be more conducive to their expertise as electrical engineers.
- Nanoscale Mg_2FeH_6 has been investigated for several years by Dr Varin Univ. of Waterloo in Canada (mechanical engineering department) please contact him for collaboration.
- Encourage PI to continue in amides where potential for higher storage exists - PI also is encouraged to collaborate with many other PIs on this subject.
- Address previous questions regarding volume density of films and thermodynamics of hydrides.
- PI is highly encouraged to team up with established experts in the storage field.
- Project would be improved if efforts were better aligned to DOE and the FreedomCAR & Fuel Partnership goals.
- Need innovative ideas to generate novel approaches. Conduct go/no-go assessment soon.
- The manufacturing of nanostructured film work (Approach 3) should have a go/no-go decision. The approach does not show much promise for hydrogen storage.

Project # ST-12: Process for the Regeneration of Sodium Borate to Sodium Borohydride for Use as a Hydrogen Storage Source
Wu, Ying; Millennium Cell

Brief Summary of Project

Millennium Cell's project is focused on the development of a reliable low cost regeneration process for sodium borate to sodium borohydride that meets DOE cost targets. The technical approach is to identify electrolytic processes that improve efficiency and reduce cost. It considers both the traditional Schlesinger route utilizing a lower cost sodium source from a novel electrochemical process and direct borate reduction. A key tool is to use hydrogen gas to reduce cell voltage and improve overall regeneration efficiency. This project started in 2004.



Question 1: Relevance to overall DOE objectives

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

- Off-board regeneration of sodium borate to sodium borohydride at acceptable costs is a requirement for this storage technique to be viable.
- Obviously relevant if it works to the extent that the cost targets can be achieved. Borohydride is a major option being considered for hydrogen storage.
- Electrochemical regeneration of a material is the only process identified here that has promise or may translate to other materials. All other process mentioned here are not relevant since the starting materials do not make sense. NaOH is not a trivial material to generate.
- $NaBH_4$ will only have practical application in premium backup power applications.
- Hydrogen storage is a vital technology necessary for the achievement of the goals of the hydrogen program. This research when initiated seemed promising but now appears unable to support a technology which can meet the 2010 or 2015 goals.
- Issue of regeneration is very important to the needs of the President's hydrogen fuel initiative.
- The work is in the area of storage which is important, but the project aims at goals below the 2010 targets, which is troubling.
- The project has failed previously to perform the basic calculations of feasibility that would show if it can be viable, thus the alignment is not clearly good.
- $NaBH_4$ system problems seem to preclude it as transportation storage medium, but any progress in borate regeneration should affect other hydride systems.
- The project is relevant to the DOE Hydrogen Program objectives. Onboard systems based on Millennium Cell technology have been done. Millennium Cell focused on critical step in making the technology viable.
- This technology (the Millennium Cell approach) is not a new approach. It has been extensively evaluated previously by many auto companies, and largely rejected as impractical for a wide variety of reasons, only some of which have to do with regeneration costs. The proposed work seems very unlikely to significantly advance the field or the technology forward.
- Chemical storage is one of several possible storage technologies to meet DOE goals. Cost goals seem to be a major challenge.

Question 2: Approach to performing the research and development

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

- The approach to looking at the regeneration of the borate stream in response to past reviewer comments is the critical step in demonstrating the technical and economic feasibility of the sodium borohydride fuel cycle.
- The technical approach is looking at new processes for reducing the cost and increasing the energy efficiency of the full fuel cycle--both on-board and off-board the vehicle.
- Approaches are based on direct borate reduction, reduced cost for Na production, and co-production of sodium and boric acid.
- Multi-tier approach with timely down selects and go/ no-go decision points.
- Electrochemical approach is unlikely to address energy concern.
- PI does not indicate cost of starting material of NaOH to manufacture $NaBH_4$. This is not trivial and will likely erase any gains made over NaCl as a starting point. This approach does not address the point that if a $NaBH_4$ economy is to happen with their system, the raw material starting point must be $NaBO_2$.
- Thermodynamic energy to break B-O bond will always be too steep to be economical.
- Issues were well explained. Possibilities for increased hydrogen storage are great.
- Considering the thermodynamic barriers, the approach is reasonable. While it is important to try harder to solve these problems, it is not clear what criteria are used to decide on a go-no go on the approach. One suggestion is for the PI to voluntarily re-scope the project as some pathways are failing.
- Logical, reasonable approach based on three identified pathways for regeneration with a down select to the most viable approach. High cost element (Na) identified and focused on to reduce costs.
- The approach is clear and the project seems well designed and focused. A high degree of understanding of the chemical systems being researched is evident.
- It is unlikely that this technology can achieve the technical goals of the DOE storage program. This research is relatively high risk and significant funds are being devoted to it. Despite the unlikelihood that the technology can meet the targets, the research work is addressing commercialization issues like lower cost materials, well to tank efficiency analyses, etc. This line of research seems inappropriate given the technical shortcomings.
- Approach is well focused.
- Have failed to do the basic energy calculation to see what the total energy cost is.
- Improvements of one step process are appropriate.
- Organic solvent has economic incentive but is much more challenging.
- Electrochemical regeneration seems to be a nonstarter for transportation fuels. Inefficiency of electrical generation compounded to process energy normally makes it energetically and economically unsuitable for bulk fuels.
- Labor cost seemed to be larger than DOE target, so how would cost goal ever be met? How three main research areas, but never said which looked most promising.

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

This project was rated **2.4** based on accomplishments.

- There has been some progress but this project requires a major breakthrough.
- A one step electrical synthesis of the borohydride was demonstrated but the yield of the process is less than 10%. An alternative hydride transfer electrocatalyst for conversion of B-O to B-H has not been found. Concluded that the most promising approach is to reduce the cost of manufacturing Na and therefore $NaBH_4$. Even though the results have not been too encouraging, there is evidence of significant effort toward the objectives.
- The PI hasn't proven that the first two approaches have made significant progress, even if progress was made, the overall impact on $NaBH_4$ cycle is not significantly impacted.
- PI has quickly abandoned electrochemical approach of B-O to B-H work due to low yield. This should have been an intuitive assumption prior to starting work.
- Good progress made with cost of regenerated $NaBH_4$ lowered by a factor of three.
- Very good; Narrowing down to the best routes.

- PI reports that feasibility of hydride transfer catalyst has not been established. This is disappointing considering the investment in the project over the past year. Is this an indication of infeasibility of the technology?
- The bulk of the technical accomplishments seem focused on improving materials processing, reducing the cost of materials and recycling materials. This would be fine except for the fact that the technology has not been proven capable of meeting the goals of the storage program. This kind of work ought to follow once the feasibility of meeting the volumetric and gravimetric goals has been established.
- Nice result on the one pot synthesis of $NaBH_4$.
- 8.3% yield of BH4- in one reactor; No success to date on alternate solvent to molten hydroxide.
- Lower energy of formation for Na is an improvement but does not include hydrogen used. It is also based on limitless NaOH at a low price which is not so.
- One-pot synthesis encouraging? How much improvement possible in yield? How much energy required? Any way to recycle Br_2 and recover energy?
- The well-to-tank efficiency slide is almost impossible to understand. This analysis needs to be consistent with the protocol developed under the H2A effort.
- The group has made some improvements in the overall cost.
- The group has explored a number of options.
- Achieved some improvements in efficiency.
- Cost reduction of Na metal compared to current process seems to be not an apples-to-apples comparison. In the end, one will start with borate and regenerate (not hydroxide or chloride) and need to compare various alternatives from the same starting point.
- Cost reduction by co-production of Na and H_2BO_3. $NaBH_4$->$4H_2$ gave 94% mos yield- seems to show they are on right track. No hydride transfer agent found yet?

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

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

- Has a separate project from center work.
- Look for opportunities for greater collaborations.
- Have initiated discussion for collaboration with Rohm and Hass. Millennium Cell indicated that they are collaborating with the Center of Excellence on a separate effort although there is little evidence of this in the presentation.
- Good partners now and will be incorporated into chemical hydride center on a separate effort.
- Continuing collaboration with Air Products.
- Why is PI not collaborating with the biggest producer of $NaBH_4$ Rohm & Hass? (Note: Millennium Cell is actively collaborating with Rohn & Haas through their project with the Chemical Center of Excellence)
- There is very little talk about outside work.
- Fairly limited area.
- Involvement in the Chemical Hydrogen Center on a separate effort as a full partner is very positive.
- Built-in. MCEL and APC would be the potential implementers.
- Industry and academic partners identified.
- Coordination between Millennium Cell and Air Products appears good. Would be nice to broaden collaborations as recommended by previous peer review.
- Well connected to other partners.
- Air Products is good partner with process expertise that's needed here.
- Core business partner in $NaBH_4$ regeneration/production may help project. Air Products only collaborator.

Question 5: Approach to and relevance of proposed future research

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

- Plans are appropriate for this project, but again, it is not clear that the total energy is feasible and that total energy and hydrogen consumption calculation is needed.

- The work plan is appropriate and eventually should lead to a realistic determination of the economic feasibility of the borohydride system.
- The one project worth working on -electrochemical regeneration was abandoned early due to poor yield. PI should investigate dissolving $NaBH_4$ in non aqueous media in order to improve recycling yield. This will eliminate the NaOH stabilizing agent and reduce the excessive bonded water molecules to the borate. PI should investigate the work of Dr Boyd Davis at Queen's University Kingston, Ontario, Canada.
- Issues are well known and pathways were explained to overcome these issues. Lots of ideas express during presentation, but need to focus in on ideas.
- This is no reflection on the PI work but rather the difficulties that they are facing. The thermodynamic energy barriers require new chemistry breakthroughs. It is not clear if the current approach is adequately addressing them.
- Good plan for future research direction.
- Continued well-to-tank efficiency and cost analysis is essential.
- Possibility of co-production of Na and $B(OH)_3$ is positive.
- Looks fine. Logical progression.
- Engineering feasibility and further cost reduction work is unnecessary until capability of technology to even approach the DOE goals has been proven and verified.
- Have outlined a reasonable plan for future research.
- Continue on track already developed.
- Project needs to determine which area it wants to focus on.

Strengths and weaknesses

Strengths
- Millennium Cell has experience in building and operating sodium borohydride based system
- The PI's have long-standing experience in the field. Strong incentive for the success.
- Millennium Cell has initiated this storage approach and thus built up a focused technology base with experience and expertise.
- Strong companies with extensive knowledge in the field.
- Tremendous amount of useful data/knowledge regarding potential regeneration options has been accumulated that can complement future research in this area. In the future, depending upon the situation with materials and energy costs the electrochemical regeneration option may become economically feasible.
- Company expertise with $NaBH_4$; Collaboration with Air Products.
- The team is striving to answer the critical questions related to the energy efficiency and costs of the regeneration of dehydrogenated fuel.
- New approaches given to low cost for borohydride regeneration/production.

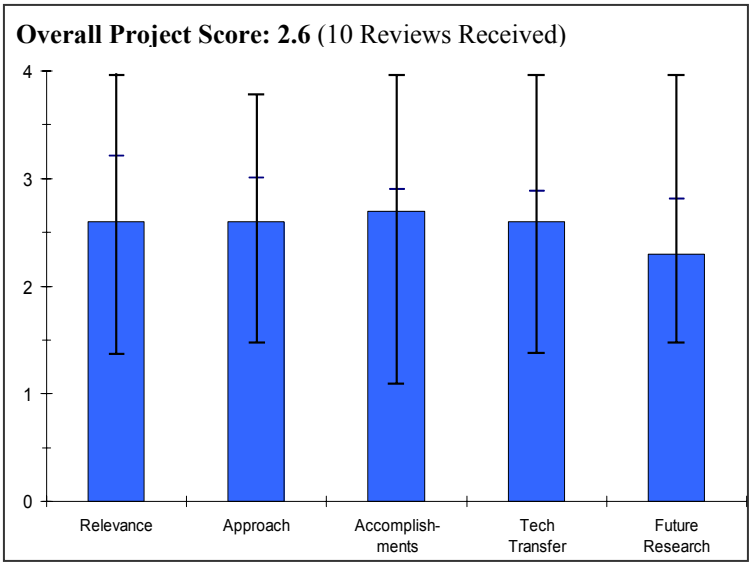
Weaknesses
- PI needs to be more forthcoming in revealing the true system operation and the full energy cycle to DOE which will reduce the appeal of their work.
- Millennium cell is not a materials company. Regeneration techniques should be transferred to members of the Chemical Hydride Center of Excellence.
- Basic thermodynamic barriers.
- The regeneration processes are complex and complicated.
- None apparent at this time.
- It can be disputed whether or not the team has actually achieved the $NaBH_4$ cost reduction goals as claimed in side 18. More details are needed to evaluate.
- The sodium borohydride system does not appear to be a solution for on-board storage applications at present due to overall energy efficiency (cost) barriers.
- Consistently avoids true cost evaluations; always leaves out the hydrogen value.
- Electrochemical route will always suffer vs. primary energy route.
- Somewhat confusing presentation.
- Project seems to be too broad at this stage.

Specific recommendations and additions or deletions to the work scope

- Solid-state material storage level of 21% assumes that the water is available for free. This is not the case and is misleading. PI strongly encouraged to not reporting storage values this way. (Note: PI also reported $NaBH_4$ solution values and system level capacities)
- It is recommended to bring up the go/no-go decision point once the system analyses work is completed.
- It is also recommended to give the system analyses work on this concept a higher priority to accelerate the decision point.
- Cost of NaOH and impurities in NaOH should be considered.
- Investigate possible effects of impurities in NaOH. May not be as "inexpensive" as first thought.
- Complete the project scope as stated, especially economic feasibility study relevant to the processes developed.
- They need to make a system and run it using waste products and show us the full scope of energy and materials added. Then we will have evidence of progress.
- Determine total energy required for "one-pot" synthesis. Any use for Br?
- Still need to correct well-to-tank slide. Feel like last year's suggestion was ignored. Suggest using overall US mix for electrical generation efficiency/CO_2.
- Even though the energy and economic analysis was reported out to DOE, a summary of that analysis should be included in future presentations.
- Need to downselect on best method for borohydride production to best use funding still remaining on project.

Project # ST-13: Chemical Hydride Slurry for Hydrogen Production and Storage
McClaine, Andy; Safe Hydrogen

Brief Summary of Project



The objective of Safe Hydrogen's project is to demonstrate the viability of magnesium hydride slurry as a cost effective, safe, and high-density hydrogen storage medium. The pumpable, high-density slurry offers infrastructure advantages, and high system energy density with high vehicle range. The focus of the project will be on regeneration of the spent slurry and conversion of magnesium hydroxide to magnesium hydride to meet DOE cost targets for off-board regeneration. Work will also be done on mixer, slurry, and system development to meet DOE capacity targets. During the past 11 months of performance, the researchers have increased the stability of a slurry to a week; they have shown that the reaction rate of the slurry is fast enough to allow a small, lightweight mixer; and they have nearly completed the initial cost and efficiency analyses. In addition, significant progress was made in the solid-oxide oxygen-ion-conducting membrane (SOM) process development: the operating temperature was reduced to 1,150 C and the membrane showed little wear (promising low costs).

Question 1: Relevance to overall DOE objectives

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

- Obviously relevant if it works to the extent that the cost targets can be achieved. Slurry idea is excellent in that it may provide an option that mimics current liquid fuels.
- This is a novel proposal of a method of hydrogen storage that needs to be evaluated to determine if it can meet the DOE goals.
- This project is relevant to the DOE program.
- There is partial support of the objectives of this project to the goals of the President's hydrogen fuel initiative. There are other mechanisms for the transport of fuel.
- Irreversible technique that could support both on-board and off-board hydrogen storage.
- The project is in the right directions but can not meet the 2010 system goals so they can not be said to be likely to move us toward the vision. It is an especially safe system which is in line with the vision. Cheaper MgH_2 could be useful if MgH_2 plays a role in other systems.
- On-board system could meet DOE targets. Most expensive part of process is reduction of MgO to Mg.
- Project focused on development of a chemical hydride system that has been tested, but with a different material.
- This project has no potential for automotive or stationary applications. This system uses water which will freeze (unacceptable for automotive) and provides humid H_2 stream which is not required for state of the art high temp low humidity fuel cells currently available. Note most stationary applications have switched to a PBI - Imidazole based membrane (low humidity) and has eliminated condensers and humidifiers. Note this system may be used at forecourt stations however the overall efficiency is unlikely to beat ammonia or liquid hydrogen delivery
- Internal combustion engines are likely better suited to tolerate some level of humidity in the combustion chamber

Question 2: Approach to performing the research and development

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

- The approach is clear and the project seems well designed, well-defined, and well laid-out plan. A high degree of understanding of the chemical systems being researched is evident. Excellent understanding and analyses of the economics of the process.
- Approach seems reasonable.
- Approach is reasonable; project relies on collaborations with other efforts to achieve results on regeneration.
- MgH has high energy density supporting on-board storage.
- PI does not resolve the issue of water in the system- freeze issue. When approached, the PI explained that this was an engineering issue that is solved with using antifreeze instead such as water/glycol. this approach is impractical since it will 1- reduce the system density by water displacement of the alcohol.2- the antifreeze will have a high vapor pressure which will show up in the H_2 stream, 3- the vaporization of the vapor will have cooling effect and reduce kinetics, 4- if the alcohol reacts, methane and other hydrocarbons could be formed or more damaging aldehydes will form.
- Use of bladder system is completely impractical for any system that generates H_2 based on heat without catalysis. Waste product will always return hot to the fuel tank. Heat will conduct through a membrane and cause premature H_2 evolution on the fuel side that must be contained or purged. How will PI address this issue?
- The main task here is testing various Mg recycling schemes.
- Compares favorably to compressed and liquid hydrogen storage in life cycle efficiency (although this comparison may be optimistic).
- Making slurry stable is well approached. Making MgH_2 is an area where study should help either here or in other Mg systems. Unfortunately the base system can not make the goals so the whole program approach is not sound.
- Some new information has surfaced for carbothermal reduction that shows promise. Regeneration of the MgO by a carbothermal process rather than an electrolytic process could lead to lower cost methods for regeneration.
- Developing a system to meet the 2010 weight targets.

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

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

- PI has investigated many regeneration techniques.
- PI finally demonstrated system with Mg, all previous work was based on Lithium experiments.
- The PI's have been delivering their milestones so far. They have been very responsive to comments and directions provided by DOE and the tech team. They have also provided consistently system based analyses.
- Good progress has been made in this project. Cost estimates at large scale production are encouraging. However, the requirement to recycle 95% of the by-product is challenging.
- Very good. Making good progress.
- Progress appears to have been good over the past year. Study of regeneration processes should be complete next year. Analysis shows promise for meeting the DOE H_2 cost target. Addressed reviewer comments from last year very well.
- PI has shown some good results and provided back up data supporting graphs showing that the technology can almost meet the DOE goals. However the back up analyses supporting the graphs will need to be carefully studied. It appears the PI has been optimistic about some of the cost elements which may explain the good results.
- Progress is modest to reasonable thus far. Will the oil used need to be recycled/cleaned-up in some way after use? Wear on the pipeline and pumping systems was not presented nor was it discussed and may be an important component of the economics of a slurry system.
- Energy efficiency calculation looks like it may unfairly penalize the reference systems based on the recycle process chosen. This needs to be checked. None the less, the efficiency is much higher than might be expected and, if proven, is a step forward for this system.

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

This project was rated **2.6** for technology transfer and collaboration.

- Sufficient academic collaboration seams evident; however, PI has no industrial support.
- It would be helpful to have partnership with one of the Mg producing company or even vertically integrated collaboration through the Mg cycle.
- Significant collaborations are impressive.
- Addition of slurry expert is a positive development.
- Built-in, Safe Hydrogen could be a potential implementer. Good to see Prof. Uday Pal at Boston University involved as he knows the SOM MgO reduction well.
- Some partners are listed, mostly from industry, but it is unclear what their roles have been.
- Mention was made of collaborators but no meaningful detail was presented regarding coordination and technology transfer.
- Appropriate slurry experts enlisted. Good contacts with others in the MgH_2 formation field.
- The project has a number of partners.
- Good use of H2A protocols/models.
- Only limited collaborations/interactions.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Has the purity of the H_2 stream been evaluated? This stream comes out fully humidified which is not required for future low humidity membranes. Stationary high temp PEM cells are already operating on low humidity which will render PI system incompatible.
- PI underestimates the size of heat condensers required. Most automotive systems have low temp coolant loop at around 50C which reduces the delta temp and thus increase the condenser size. PI is assuming ambient temp condenser. In either case PI does not account for the increased size in heat exchanger required.
- Proposed future activities logical and straightforward. Looks fine, logical progression. Future plans seem reasonable, especially plans to do scale up experiments. Future research plans appear in-line with project goals. Plans are appropriate.
- Future research should lead to determination of the feasibility of the system both on-board the vehicle and on a life-cycle basis. Need to focus on building system, and demonstrate whether proposed energy densities are really feasible or not. This should be the top priority.

Strengths and weaknesses

Strengths

- Good management: flexibilities in their approach open to suggestions and easy to understand.
- Good to see the system-level calculation so explicitly spelled out.
- Strong team approach, building a strong base for future development of MgH storage materials including regeneration.
- Strong company with extensive knowledge in the field.
- May be making progress on Mg generation.
- Brought in slurry experts to aid in choosing dispersants to make slurry stable for months at a time. Very good presentation that is easy to follow. The energy analysis was done according to the methodology developed by the H2A group.
- Project may have application in marine applications where salt water can be achieved for free.

Weaknesses

- The biggest problem is the enormity of the tasks: onboard heat management, Mg recycling infrastructure development.
- Unlikely to meet goals. Excess water to keep the byproduct liquid so it can return to the waste bladder is not counted in the mass calculation.
- Methods and equipment used to determine yield are crude. At reaction temperatures suggested by PI, the mineral oil used in the slurry will have a vapor pressure with hydrocarbons in it that the PI has not accounted for. Suggest PI have H_2 stream evaluated for Purity. Suggest that PI evaluate sample with SWRI.
- PI neglected to mention that at least 20% excess water is required in the mixer to ensure kinetics and acceptable viscosity of waste product. PI does not account for this in the bladder design or the fact that the waste product has larger volume than the two reagents. This again will increase the tank size and reduce storage density. Use of bladders is not practical for reasons mentioned above and also that slurry will have a coarse texture which will quickly deteriorate any bladder membrane.
- If hydroxide forms these calculations will not hold, and that needs to be proven not assumed.
- Mass and volume number need vetting, but seem better than might be anticipated. Not at all clear the gravimetric and density calculations are based on numbers that can be truly achieved, condenser for exhaust and tank of only 3 pounds for example.
- There are concerns about his analysis results. They seem to be inconsistent with other analyses.
- For system calculations, several assumptions are made that are not realistic: 1) flow rate is only about half of what is needed; for a more realistic flow, heat exchangers, condensers, etc. will be heavier and take more volume; 2) Fuel tank seems unrealistically light; 3) May need extra water (other than what's used in hydrolysis) to keep system fluid.
- PI is assuming that the removal and re-introduction of the mineral oil will be accomplished for free in calculations (i.e. no loss in yield or energy required) this is impossible.

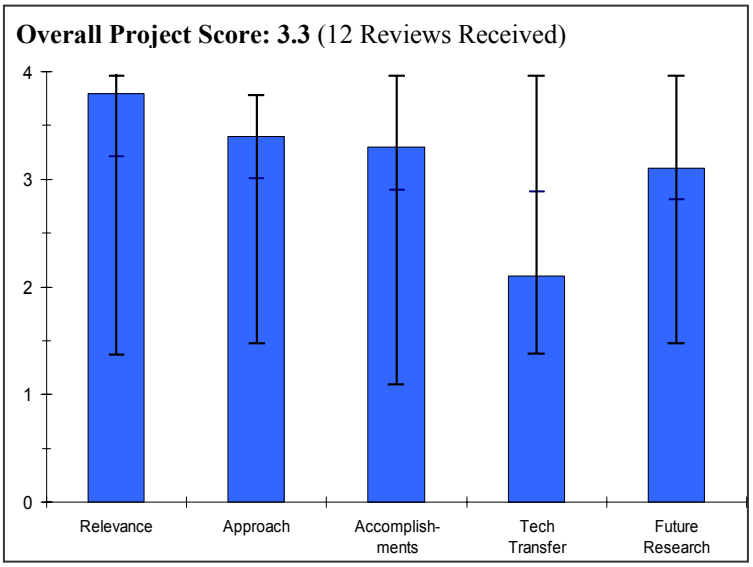
Specific recommendations and additions or deletions to the work scope

- Need to focus on constructing system to prove that it has the predicted characteristics (or prove that it doesn't). Progress along these lines needs to be significantly accelerated.
- PI needs to build a system to prove out the estimated system efficiency and cost projections shown in the presentation. The technology seems to be promising, but it is unclear if it is because of its actual feasibility/capability or because of optimistic projections for system efficiency and cost.
- This project will never achieve the 2007 targets. PI should not assume any water will be available from the fuel cell. Future fuel cells will have even less water available than current systems. PI must incorporate FULL thermal management load, condensers, heat exchangers etc.
- System likely to form hydroxides which will significantly decrease the densities of the system. Need to build system and demonstrate that it has the attributes claimed.
- Energy efficiency calculation looks like it may unfairly penalize the reference systems based on the recycle process chosen. This needs to be checked.
- What water displacement method is being used? Unless the hydrogen is extremely dry, h_2 flow meters or mass flow meters will have significant measurement error.
- Recommend to have one of the system analyses teams verify Safe Hydrogen system analyses. This is the key to the go/no-go decision.
- Should be rerouted to delivery where it has more chance. Really they need to make one of these slurry systems as calculated and justify the calculations to show the vehicle based system is viable.
- None. An interesting synergy might be the use of SOM for production of magnesium for this and other purposes such as lightweight automotive structures, which (synergy) could help lower costs due to economies of scale. Suggest talking to FreedomCAR Automotive Lightweighting Materials about possible co-funding of the SOM work.

Project # ST-14: Development of New Carbon-Based Sorbent Systems for an Effective Containment of Hydrogen
Cooper, Alan; Air Products

Brief Summary of Project

Novel hydrogen storage systems are being developed which depend on a facile, reversible catalytic hydrogenation of highly conjugated organic molecule substrates. Current focus is on devising liquid-phase organic carriers that can store and supply hydrogen for both on-board vehicular and stationary applications.



Question 1: Relevance to overall DOE objectives

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

- Very interesting project - only one to date that beneficially captures the exothermic reaction of the regeneration process.
- May prove to be a feasible proposal to meet 2010 storage goals. If so, it will fully support hydrogen initiative.
- Work is highly relevant and critical to the issues of hydrogen storage transportation generation and regeneration.
- Goals identified and well defined.
- Process takes advantage of existing industrial practices and uses familiar liquid handling techniques to transport the hydrogen. Air Product is a very capable company in understanding manufacturing and shipping of fuels and gases
- A reversible storage technique using available waste heat.
- Has potential for 6.9 wt-% storage capacity.
- Obviously relevant if the ideas work to the extent that the cost targets can be achieved. Provides an option that mimics current liquid fuels.
- Liquid materials have significant advantage in fuel/refuel and thus align well. Room for improved systems to meet goals.
- Also may make good use of "waste heat", and helps on transport costs.
- Conformable system which is very good.
- Very relevant to the DOE program.
- This concept looks very promising for meeting program targets.
- Good focus on high density reversible liquid carriers. Have the potential certainly to push the state-of-the-art, and possibly even to achieve the 2010 goals.
- What is the volumetric density of hydrogen in these liquids?
- Very innovative new approach (liquid carrier recharged off-board) to the storage problem, which also has implications for infrastructure and production.

Question 2: Approach to performing the research and development

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

- The approach is clear and the project seems well designed. The plan is not articulated, but seems obvious: cut-and-try based on accumulated knowledge. Use of the waste heat is good.
- Target of research takes advantage of utilizing existing or similar infrastructure. Public acceptance of this product will be much easier compared to other storage methods.

- Process to capture regeneration heat will improve well to wheels efficiency dramatically.
- Focused on narrow field of materials - may be limiting on the flip side though.
- There are some under estimations on the difficulties facing this project, namely, a significant progress on the catalyst (lower precious metal loading, with higher activity), new molecule synthesis (with around 100C dehydrogenation), and significant improvement in onboard reformer design and associated balance of plant.
- A methodical, systematic approach.
- Novel approach to meeting storage goals especially addressing regeneration of storage media and dealing with waste materials.
- Approach appears quite reasonable to the goals of the project.
- Approach strong, planned results being achieved, and new carriers identified.
- Suitable overall approach.
- Use of waste heat is a good target.
- Screening many catalysts to get improved rates is appropriate.
- Checking impurities is a good practice.
- New carriers sought with lower temperature of dehydrogenation.
- Focused on carbazole and similar compounds that have low dehydrogenation energy and clean hydrogen separation.
- Secondary focus on porous ionic solids.
- Liquid approach has significant advantages over other approaches.
- Conformable tank design capability.
- Exothermic off board regeneration suggests overall energy efficiency should be good.
- Need to lower temperature of desorption to be consistent with fuel cell vehicles. What are the prospects for doing so?

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

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

- Significant progress in candidate carrier and catalyst identification has been made and verification and validation is underway.
- Very good. Seems to be making good progress especially in the liquid-phase materials. The solid materials are only based on theoretical computations so far, and this work on the ionic solids should not be allowed to affect the work on the organics.
- Kinetics and temperature still unsuitable for automotive applications.
- May already be suitable for home heating or power applications. Since home gas units have consistent pilot burner on for safety purposes, the pilot burner could be used to keep reactor hot to ensure fast startup.
- Interesting approach turning disadvantages of metal hydrides into advantages.
- Conformable storage system is an advantage.
- Data seems to indicate cycling stability but only over 3 cycles.
- Pleased that more technical details are now being shared by PI.
- Capability for high throughput screening of catalysts is to be complimented.
- Met year 1 performance milestone for dehydrogenation (3wt%@150 C).
- New liquid carriers show promise, but lower hydrogen capacity may limit further progress.
- New high gravimetric and volumetric carrier identified.
- Five to six percent delivery in 5 hours at 200C (slightly slow but close to need), rehydrogenation in a few minutes.
- Fourteen hour desorb at 150C. New catalyst has doubled the rate. New isolated material with 6.9% that may cycle.
- Have identified some compounds that look promising, 6.9% demonstrated. High hydrogen quality. Performance of these compounds under hydrogenation-dehydrogenation cycling needs to be verified through more demonstration. Data on possible long-term degradation of the compounds needs to be acquired.
- Have found some promising systems.
- So far purity of output stream looks good.
- Thermodynamics look good in these materials.

- Excellent progress in discovering several new liquid carriers in the range of 5-7 wt.% and with operating temperature of 150-250C.
- Volumetric densities of these liquids should be considered. (Particularly so, since these liquids could have conformable tanks, and therefore could benefit from a "bonus" due to conformability).

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

This project was rated **2.1** for technology transfer and collaboration.

- Air Products is a reputable company with extensive experience in producing and delivering hydrogen and many other fuels or gases. Air products have excellent understanding of economics and life cycling of equipment etc.
- Air Products could benefit from some more coordination with OEMs in order to fully understand system requirements and practical engineering solutions.
- There doesn't appear to be any external collaboration at the present time.
- Apparently some interaction with outside storage community but no direct involvement.
- Built-in. APC could be a potential implementer. Other collaborators could be the Chemical Hydride Center of Excellence.
- PI has been very vague about outside collaboration.
- There is little outside interaction however, I am not sure that further collaboration would add much to this project. I was informed in private conversation with the PI that other aspects of this project (to be reviewed in another session) have substantial external collaboration.
- Some collaboration with OEMs, but nothing formal.
- Air Products project only.
- Not a great deal of collaboration.
- Going forward Air Products could collaborate with CoE, but it is not a part of a center.
- Partnerships/collaborations not mentioned.

Question 5: Approach to and relevance of proposed future research

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

- Reasonable plans for future - the project seems to be headed in the right direction.
- Looks fine. Logical progression.
- Looking forward to further confirmation of good results presented here.
- Future plans seem reasonable. It would have been nice to hear more details as to plans for finding new carriers.
- Future activities planned to further year one's work.
- Plans are not very precise, but the route and direction are correct.
- PI is focused well on the remaining barriers of reducing temperature and catalyst loading. PI is using a very high concentration of noble metals at the moment and reducing this may be difficult.
- PI needs to work with materials in the 10kcal/mol dehydrogenation rage (limited materials) for this business model to work.
- Environmental assessments will be done when a suitable candidate is found.
- Straightforward continuation of current work.
- Not clear why the ionic solids work has been delayed. If there is no time or incentive to work on this prediction, should disclose the compound so that others might test the idea.
- Future work should be directed at even lower temperatures of operation than currently stated (6 wt.% and 200C for Year 2), and/or higher densities.
- If densities could be significantly improved (i.e., H:C ratios higher than one) with the thermodynamics sufficiently tuned, it might be possible to consider burning some H_2 to heat the liquid to the desorption temperature.

Strengths and weaknesses

Strengths

- This is perhaps the only project in which the fuel is returned to the factory and regenerated exothermically in a manner which the heat can be captured for practical use. This significantly improves the overall system efficiency and integration with other industrial processes.
- Overall, this has been the most promising presentation in the merit review. PI has well thought out the entire process, and recent revelations of materials and progress has done much to provide credibility to project
- Pumping a gasoline like fluid is desirable and understood by the industrial community. This non abrasive fluid has a much better chance of success to work in a bladder based system.
- Since the fluid requires a catalyst to evolve the hydrogen (presumably non temperature dependent in the 0-200C range) the risk of hydrogen evolution in the fuel tank due to hot waste should be minimized.
- Water is not required - big plus!
- Safety of this fluid seems to be better than an ammonia based system- leaks seam containable.
- One of few systems that are conformable and operate at low pressures.
- Competent PIs and has good resources.
- Strong, innovative concept based on Air Product experience and expertise.
- Strong company with apparently extensive knowledge in the field.
- Well organized, with methodical approach, and focused on goals and performance.
- The approach of liquid hydrogen carriers covers several areas such as hydrogen transportation, delivery as well as off- and on-board storage. The project is critical to potentially achieving the DOE targets in multiple areas.
- Clearly see need to stay as liquid; only cycle hydrogen (no cleavage); low volatility; and right range of heat of dehydrogenation; low cost.
- Most completely innovative program we have right now.
- Working with liquids that look a lot like transportation fuels.
- Very innovative and exciting new idea about the combined approach to hydrogen storage/infrastructure/production via liquid carriers. A very important program in the DOE portfolio.

Weaknesses

- Could PI indicate what the volatile components that are released in the reaction are? Even though volatility is low. VOCs must still be present to some degree.
- Reducing catalyst loading is going to be difficult.
- Window of opportunity (delta H) is very narrow for the concept of infrastructure to work.
- What is the viscosity and volatility of the waste fluid? Please reveal more characteristics. Are there several reaction paths that lead to several different waste streams?
- Volumetric density is still low, however conformability of fluid and low pressure will partially compensate for this weakness.
- Would prefer more openness regarding partners and technical details of process.
- PI should present ALL data and details in each presentation. I understand that the presentation in Hydrogen Production and Delivery session was more detailed, but I was unable to attend it because of other meeting/review commitments. Reviewers should not have to base their evaluations on incomplete information.
- Not much collaboration.
- No significant "show-stoppers" have been revealed so far.
- Need to regard low temp operation (will it freeze). Tempss remain too high to use the free heat of the fuel cell.
- Carriers'capacities are still a little too low.. Will need to discover new carriers to reach DOE ultimate targets.
- Slow hydrogen release kinetics in material to date.
- Materials with greater hydrogen capacity are needed.

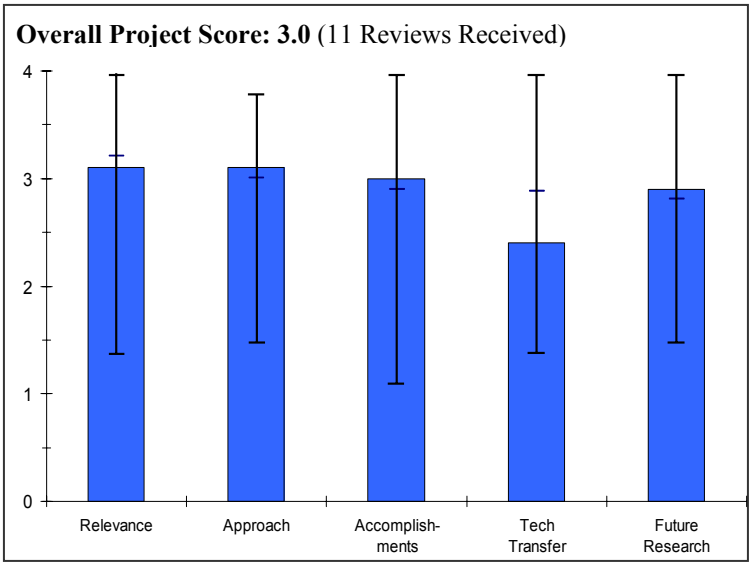
Specific recommendations and additions or deletions to the work scope

- Please have PI present the Production and Delivery presentation to the H_2 Storage tech team. Apparently that presentation addressed some of the onboard system design which was lacking in this presentation.
- This is one of the projects which is ideal for system analyses work. While it is asymptotically clear that a new molecule with lower dehydrogenation temperature and much higher activity catalyst are required, an early system analyses could reveal other hidden factors that are needed to be addressed. For example, what is the minimum material-based hydrogen capacity which can yield a 6 wt% (system basis) capacity? Can the PEM fuel cell exhaust adequately provide enough heat? Do we need to burn some of the hydrogen? What is the round-trip energy efficiency? In short, it is recommended to establish the minimum molecular target requirements for this kind of approach.
- Look at potential health effects of new liquid carriers to see if there could be any show-stoppers like there was with MTBE. Make sure the work on the ionic solids does not detract from the work on the organics.
- Would like to see many more cycles run to confirm preliminary results of cycling stability.
- Consider benefits of opening up to more collaboration.
- More focus on critical issues such as catalyst development for dehydrogenation and continuous search for the carrier molecules with improved thermodynamics.

Project # ST-15: Low Cost, High Efficiency, High Pressure Hydrogen Storage
Ko, Jui; Quantum

Brief Summary of Project

Quantum Technologies' project goal is to deliver a cost-effective and safe high-pressure hydrogen storage system that will meet DOE targets. The technical plan entails a three-pronged approach: lowering the cost and weight of the storage system (via material optimization, process evaluation and use of lower cost carbon fibers); reducing the amount of material required through the use of sensor technology to monitor storage system health; and increasing the density of hydrogen by filling and storing at lower temperatures.



Question 1: Relevance to overall DOE objectives

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

- Although compressed hydrogen storage may not be able to achieve the 2010 DOE goals, it is a technology that is feasible today (only) in prototype use in fuel cell and H_2 ICE vehicles. However, more development of the technology is needed before it can move from the research and development phase and into product engineering phase; thus, continued federal funding is justified.
- Obviously relevant if the ideas work to the extent that the cost targets can be achieved.
- Work addresses compressed storage needs in general.
- Will address short term needs and potentially future systems. Can PI indicate what relevance this project has to stationary cost, weight etc requirements?
- The project is relevant to DOE'S near-term storage goals.
- Obviously relevant if the ideas work to the extent that the cost targets can be achieved.
- This is an important component of efforts to meet the storage requirements necessary for the President's Hydrogen Fuel Initiative. However, it will be difficult for 10ksi H_2 to be able to meet the 2010 goals for H_2 storage.
- Likely to be launched for many vehicles if nothing better comes about. This is aligned with the president's vision. May be useful to other solid material systems.
- Unlikely to meet goals.
- Quantum is seeking to improve compressed storage and thereby lengthen what is considered the short term viability of for compressed storage.
- Good relevance to the DOE program for systems that can meet the 2005 gravimetric targets but not volumetric targets.
- Composite tank development is relevant to a number of different storage technologies besides compressed gas
- Needed for short term applications.
- One of "commercially available" storage components.
- Claimed that objective is to try and enable 2010 targets, but volumetric density of 10000psi hydrogen is already below the 2010 target. Is cool-fuel going to be able to achieve 2010 volumetric target (particularly at 5000psi)?

Question 2: Approach to performing the research and development

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

- Reasonable approach addressing various aspects of compressed tank R&D necessary for advancing the technology as far as possible
- Approach to the problem is realistic and sharply focused to try and meet storage requirement goals and is a credit to the project and the program.
- The approach appears sound and capable of meeting the project and DOE goals
- The general approaches are clear and logical: minimize the amount of carbon fiber or use less costly carbon fiber by substituting efficient process design ("Cool Fuel") and sensors.
- Compressed hydrogen tanks can only meet near-term goals.
- Reducing fiber cost is right direction. Use of sensors to reduce over design is right direction.
- Increase hydrogen loading by cooling is a possibly useful approach.
- State-of-the-art compressed storage systems that can be integrated into fuel cell vehicles. If volumetric requirements could be relaxed, maybe could satisfy the range requirement of FCVs.
- Good approach to developing a smart tank. Addressing questions of how to do a smart tank by studying damaged tanks by Cool fuel looks promising to achieve full fill.
- Sensor approach should help reduce amount of fiber needed which will help weight, volume and cost.
- Cool fuel approach may result in improved energy density for compressed gas storage systems.
- For cool-fuel, "dormancy" (in terms of temperature) of the fuel should be considered. If one fuels the tank, then parks the vehicle for an extended time, does the system vent hydrogen, or allow the pressure to rise?
- Wasn't explicitly stated, but this cool-fuel system is at 10000psi?
- What are the safety implications that the fuel should heat up?

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

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

- The project made good progress and is accomplishing its objectives. The use of commercial grade carbon fibers has made significant improvements in meeting cost targets.
- Very good. Seems to be making good progress. Has reduced potential cost from $18/kWh to $10/kWh. Still long way to go to $4 and $2.
- Interesting to see that small damage to the tank had minimal impact on burst pressure.
- Cool Fuel system needs to well to wheel efficiency predictions validated.
- Could system be cooled a little further for cool activated carbon applications? What is the well to wheel efficiency of this system?
- Quantum continues to make progress improving the volumetric and gravimetric efficiencies of tanks.
- They are now assessing the total system, an improvement over last year.
- Progress has been quite good in terms of tank building, testing, the incorporation of sensors and strain evaluations.
- Improved the capacity while switching to lower cost fiber.
- Strain monitor allows damage detection. Thermal model developed to evaluate temperature in fill and operation.
- Unclear the cool fuel will be viable, seems to discount the idea people fill before the weekend and then sit there over the weekend fairly often.
- Very good progress on a number of fronts.
- Demonstrated sensor application to tanks and studied sensitivity of sensors to help determine number of sensors needed.
- Cost analysis of tanks for 3 tracks followed in program.
- Not very much progress has been made, despite the fact that the project appears to have been going on for a year, and that some of the objectives (composite optimization) appear to be quite similar to previous work by these PIs. Need to show more progress more quickly.

- Marginal cost improvements are being made; more effort should be made in order to achieve conformable systems. Cool fuel technology could be interesting if cooling costs and system fill specs drawbacks can still result in overall system improvement.

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

This project was rated **2.4** for technology transfer and collaboration.

- Quantum is working with GM and other suppliers to develop tanks. Manufacturing costs still need to be addressed for practicality of these systems.
- The collaborative effort as discussed in the project presentation is mostly with car manufacturer OEMs to determine customer requirements.
- Fine. Quantum could be a potential implementer, so technology transfer is essentially built-in. Working with GM.
- Although not extensive, collaborations seem appropriate for the technology R&D program.
- Quantum cited work with major OEM manufacturers. No details were provided.
- It would be beneficial to have broader collaboration with developers and manufacturers of carbon fibers.
- Suitable transfer and collaboration.
- Works with major OEMs on tank development. Possible collaboration with solid storage concepts should be encouraged.
- Only limited partnerships/interactions, not defined.

Question 5: Approach to and relevance of proposed future research

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

- Future plans are incorporating lessons learned thus far and appears focused on the key technical barriers.
- Work on replacing fibers with sensors would continue.
- More effort in general is required to reduce carbon fiber costs - this would have advantages for all hydrogen applications - Quantum should not necessarily conduct this work.
- Looks fine. Logical progression.
- Future plans seem reasonable and logical extensions of prior research.
- Future plans appear to reasonably build on past work.
- The approach is appropriate for their goals.
- System validation is expected in 2006.
- Results will build on current results.
- Would have expected some of the items listed in future plans to have been done already; need to accelerate the effort.

Strengths and weaknesses

Strengths
- Quantum has experience in building high pressure tanks.
- Strong company with extensive knowledge in the field.
- Good near-term solution for on-board storage. Concepts are applicable to storage vessels for "chemical" storage applications as well.
- Making progress on fiber use.
- Cost share is above average for a demo project. State-of-the-art for Type IV all-composite tanks.
- System values include balance of plant. One of the few realistic system values presented at the review

Weaknesses

- Quantum has experience in building high pressure tanks but is system designed to get max fill with cool gas? Does this mean that slight increases in temperature necessitate purging? It is not practical to expect that cool temperatures can be maintained simply by driving the vehicle and discharging the tank. This is practical for stationary applications where the discharge rates are more predictable though.
- None apparent at this time.
- Compressed hydrogen will not meet 2010 and 2015 targets.
- Cool fuel system was not very fully elucidated, so it is hard to evaluate if it can work in real use.
- Even with improvements, may not be able to meet DOE volumetric targets.
- Cost higher for cool fuel concept, so goals of higher energy density and lower cost are counter to one another.

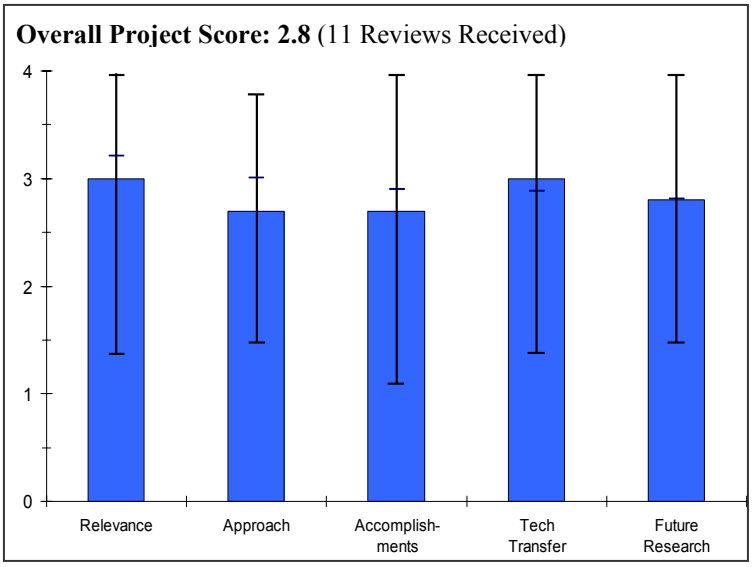
Specific recommendations and additions or deletions to the work scope

- Would be interesting to have PI perform evaluation of cool fuel tank design for low temperature activated carbons at ~150K and 100bar (system). How thick is the liner with insulation and carbon fiber for these conditions?
- Need to re-evaluate this project and quickly reach a go/no-go point.
- Many of the critical tasks here could be delegated to other groups in LLNL or ORNL.
- Look at possible effects of DOE's low-cost carbon fiber efforts and emerging long-fiber (chopped) technologies.
- Need to find a way for tank developers to work with materials developers to identify lower cost materials.
- Need to work with codes and standards setting organizations to explain value and feasibility of sensors in assessing strength and fatigue of compressed gaseous hydrogen tank systems to avoid establishment of unnecessarily stringent standards.
- Need to run the cool fuel concept through some typical fueling cycles to see if it will work.
- Consider how these tanks could be applied to solid storage systems or how this technology could be adapted for insulated pressure vessels.
- For cool-fuel, "dormancy" (in terms of temperature) of the fuel should be considered. If one fuels the tank, then parks the vehicle for an extended time, does the system vent hydrogen, or allow the pressure to rise?
- Wasn't explicitly stated, but this cool-fuel system is at 10000psi?
- What are the safety implications that the fuel should heat up?

Project # ST-16: Advanced Concepts for Containment of Hydrogen and Hydrogen Storage Materials
Aceves, Salvador; Lawrence Livermore National Laboratory

Brief Summary of Project

Lawrence Livermore National Laboratory's (LLNL) project goal is to develop conformable hydrogen tanks and advanced high pressure/cryogenic tank concepts to meet DOE targets. Conformable tanks have the potential to optimally utilize available space in a vehicle and thus greatly improve volumetric efficiency. Concepts developed by LLNL may also be applicable to long-term materials based technologies for hydrogen storage.



Question 1: Relevance to overall DOE objectives

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

- The program provides a critical support to the transition as well as long-term solutions to the storage problem. It is the common denominator for nearly all the ongoing projects.
- Obviously relevant if the ideas work to the extent that the cost targets can be achieved.
- Hydrogen storage is critical to the success of the hydrogen program, so need to evaluate various approaches to try to find one that can meet the DOE targets. This project looks at several possibilities.
- The goals of this effort are laudable and one aspect of efforts that are critical to meeting the storage goals of the President's hydrogen initiative.
- Projects don't offer significant breakthroughs in development. Approaches will likely not achieve DOE targets.
- Liquid hydrogen tank and H_2 in N_2 systems aren't clear. Better definition of these systems could result in higher grade.
- Addressing many DOE storage issues.
- If successful, this work could be valuable to OEM's.
- Multiple projects including conformable tanks, insulated pressure vessels and H_2 absorption in LN2.
- Well aligned in that the goal is to get conformable vessels which is a step toward the vision.
- Very relevant to DOE H_2 program.
- Most aspects of the project relevant to storage program.

Question 2: Approach to performing the research and development

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

- This is an innovative approach.
- The general approaches are clear and logical. The dual-use tank idea is neat.
- "Squished" balloon tank - same results as last year, where is the new direction, stress areas are obvious- why is this work continuing PI unlikely to achieve any interesting shapes with this method that significantly improve volumetric density for automotive or stationary applications.
- Cube tank - a cube or rectangle is almost as difficult to implement in a vehicle as cylindrical - perhaps this has a better footprint for stationary applications - although the support rods required will likely diminish gains made
- Liquid H_2 tank - not sure what is different compared to other systems? Is purge H_2 regulated to the outer shell? Large improvements in density seem unlikely; PI must show calculations and assumptions. If it is to be used with cryogenic carbon materials, a heat leak would need to be incorporated into the system.

- H_2 absorbed into liquid N_2 - how much reduction in density with liquid N_2? How much N_2 is lost in the H_2 stream to the fuel cell? Suspect it would be significant enough to seriously alter fuel cell performance or purge protocol.
- Supercritical hydrogen requires high pressure and cold temperature Energies and wall thickness will be greater than current pressurized system. Seems unlikely that any DOE goals can be achieved even if materials can be found.
- Project contains a number of independent sub-projects that seem to be a scattered approach. Initially the conformable tank research seemed promising, but the direction of the research project continues to move away from this to other radical and not-so-radical concepts.
- Approach is reasonable. Both components of the two pronged approach are to be complemented.
- While compressed storage is not the long term solution, this project is focused on optimizing the packaging of compressed systems for the nearer term and ultimately may be used for other types of storage.
- Use of struts for thermal management is a good step.
- Conformable containers need to be demonstrated.
- Cryotank approach can limit dormancy losses.
- Approach to meeting the 2015 storage targets.
- Impact of macrolattice approach not clear. Do we need this?
- Given the size of the budget/personnel involved in this project, five separate ideas and thrusts seems to be too much for the PIs to adequately explore. The PIs should downselect the most promising one or two ideas and really focus on trying to make them work.

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

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

- Appears to making excellent progress towards project goals. Second generation tank is a significant improvement. Problem -- haven't yet tested the second generation tank.
- Very good. Seems to be making good progress. Really pushing limits of knowledge.
- Only item built is the cube which is not an impressive breakthrough in technology. Gasket used seams very prone to imperfections in manufacturing and installation is no trivial task, likely not amenable to high volume manufacturing.
- No proof of other systems having been built to validate claims is evident.
- The PIs had significant progress in the last 12 months.
- Macrolattice pressure vessel: nice to see the model, but what are the plans to test something to see if there is any real potential there?
- Insulated pressure vessel: not clear that there are any significant advantages over a traditional liquid storage vessel. It appears to be overly complex and could be very expensive. Need to see what testing with BMW will yield.
- H_2 absorption in N_2: unclear with info presented if there is any real value in this line of research.
- Have chosen design and have built a prototype with high volumetric efficiency.
- Insulated pressure vessel demonstrated proof of concept with Sun Line.
- Insulated pressure vessel meets 2005 DOE targets.
- Did analysis of semiconformable continuous fiber tank and developed test configuration.
- Made test sample of square pressure vessel with struts. Mass and volume numbers are impressive but do not seem to include the full system. No tests of strut vessel yet.
- Not clear how the insulated pressure vessel does better on mass and volume than a dedicated liquid tank - this was accomplished using assumptions.
- Capacity claims are not proven and assume complete fill which has risk if the tank warms.
- Major accomplishment was to turn the cryotank on its side by working with the level sensor (capacitance) manufacturer.
- Demonstrated a working prototype of macrolattice concept.
- Completed design of second generation cryogenic high pressure tank.
- Measured hydrogen absorption in liquid nitrogen.
- Good to see that prototype of macrolattice idea has finally been built!

- Thermal management via these struts is an interesting idea.
- Supercritical hydrogen seems quite unrealistic; if these PIs are serious about this option; they at least need to consider the energetic cost of getting the fuel to these ultra low temperatures and ultrahigh pressures. Will any materials be able to survive these conditions?
- Considering the relatively small funding, it is not too surprising that there seems to be modest progress.

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

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

- PI seems to have sufficient connections with academia and industry.
- Building tanks for BMW is interesting. BMW continues to support liquid based systems.
- PI might benefit to collaborate with tank builders for future work.
- It is somewhat limited but there are good reasons for this.
- Excellent. Lot of partners including potential implementers.
- Some collaboration exists, but beyond 3 industrial partners, presentation is unclear with whom ("many universities") and doing what.
- Stated existence of collaborations with universities and a number of companies provided some details, could have provided more detail.
- Very strong collaboration with industry, universities and other National Labs.
- Suitably connected.
- SNL works with major OEMs and tank developers to understand vehicle requirements.
- Partnerships with tank manufacturers.
- Good to see they are working with an automotive company, at least on one of the activities. Will be very interested to hear about the testing of the cryo-compressed tank in collaboration with BMW.

Question 5: Approach to and relevance of proposed future research

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

- Future research plans are reasonable.
- Should continue to build the pressure and liquid tanks to validate claims
- PI needs to address the N_2 release from tank to fuel cell, can both N_2 and H_2 be filled from the same port? In what ratios? How often? etc.
- The project looks fine and has made logical progress.
- OK plan, but I really want to see where the conformable tank research is going.
- Future research plans are reasonable.
- Future work is ambitious with well thought out and focused goals.
- If accomplished these would be good plans, but I do not see that many of these items can realistically be approached.
- Have identified pathway to meeting 2010 and 2015 targets.
- Insulated tanks can meet 2010 targets.
- Much lighter tank concepts are required for 2015 targets.
- Fabrication of second generation cryogenic tank will validate current estimates on weight and volume.
- Too many disparate ideas; need to focus on the most promising ones.

Strengths and weaknesses

Strengths
- PI is knowledgeable in hydrogen characteristics in the low temperature regime. H_2 in N_2 may be interesting if more characteristics are revealed.
- The project is very innovative.
- Extensive experience in the field.
- The group is very flexible.

- Strong R&D organization with apparently extensive knowledge in the field and well partnered.
- Taking on several storage challenges.
- Prototype testing being initiated.
- If successful, it can be a near-medium term solution for on-board storage. Concepts developed here can contribute to other projects potentially utilizing pressure vessels at low temperatures (such as chemical storage sorbents having low binding energies of hydrogen).
- Clever concepts out of the box thinking.
- Near-term solution of the storage problem. Can meet 2005 and 2010 targets and have developed some thoughts on paths to the 2015 targets.

Weaknesses

- Supercritical hydrogen tank likely will never be practical due to excessive energy required to achieve those conditions. Finding a material that can handle those extremes will likely not come cheap either.
- PI does not have experience in tank building. PI should partner with a tank company.
- None apparent at this time.
- A lot of work is being undertaken for the current level of funding.
- The approach is limited by low density of liquid hydrogen to meet 2015 targets and by potentially high system costs.
- 80K and 1000bar is not very realistic thinking.
- Testing results are not presented yet. Promised for later this year.
- This was a very confusing presentation, how many projects is PI working on and how many are interrelated? PI needs to clearly define separation of projects.

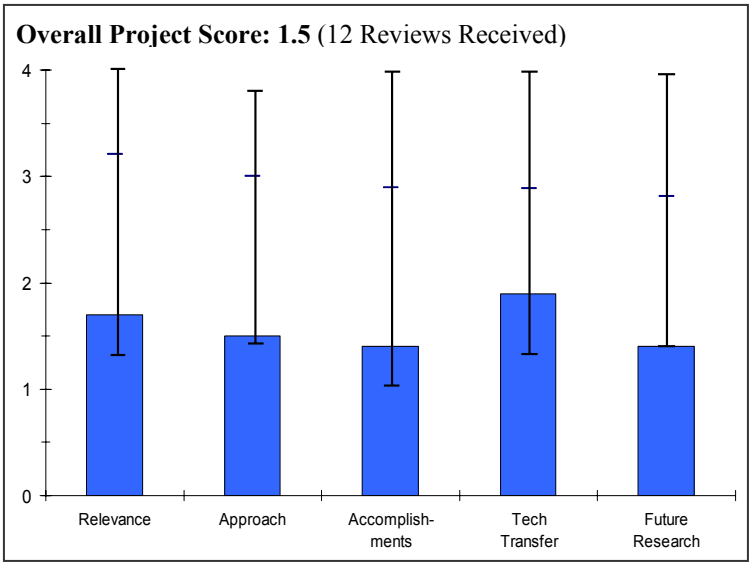
Specific recommendations and additions or deletions to the work scope

- Rational behind the energy release as a function of temperature and pressure is not clear. Even if the nitrogen is liquefied, will all the hydrogen be dissolved? Where is the pressure coming from? Is PI suggesting that the dissolved H_2 will be slow to desorb from the liquid N_2 in the case of a leak? Systems that leak aren't adiabatic in real life, heat will always be absorbed.
- There have been several significant steps accomplished this year but more importantly this momentum must be maintained.
- If the pace of the progress in this area is maintained, the status of the program should be extended to a full center.
- Look at emerging chopped long-fiber technologies to see if they have any chance.
- LLNL ought to try to narrow down the number of alternatives being evaluated; eliminate those with no likelihood of meeting cost, feasibility or DOE goals; and focus on those with some hope.
- Detailed cost analysis considerations for each approach are recommended to be included in the project scope.
- Test something other than the old existing system. Need real numbers, not estimated numbers.
- Work with Quantum to define pathway to meeting the 2015 targets.

Project # ST-17: Advanced Manufacturing Technologies for Renewable Energy Applications
Ryan, Chuck; National Center for Manufacturing Sciences (NCMS)

Brief Summary of Project

Working with DOE and the private sector, the National Center for Manufacturing Sciences (NCMS) will identify and develop manufacturing processes for the affordable manufacturing of hydrogen and fuel cell technologies, and will leverage manufacturing expertise from other industrial sectors. They will conduct workshops and solicitations and assess the feasibility of manufacturing technologies identified as key to cost reduction. This Congressionally-directed project started in 2004 and received an additional earmark in mid-2005.



Question 1: Relevance to overall DOE objectives

This project earned a score of **1.7** for its relevance to DOE objectives.

- While the ability to manufacture hydrogen enabling technologies is critical to the federal hydrogen initiative, it is unclear if this project will significantly contribute to the goals of this program.
- While activities may support the President's Hydrogen initiative, alignment to DOE Storage targets is not clear. In theory, this is not a bad idea; however fuel cell and hydrogen storage technologies have not advanced to the point where volume manufacturing should start to be considered. This forum is particularly premature for any hydrogen storage systems since no system has demonstrated practical performance at this point.
- These funds are best used for educating researchers that the materials and processes they are working on need to be amenable to manufacturing techniques, and to eliminate research options and materials that have no practical scale-up opportunities. Perhaps the DOE can conduct their own workshop with all the PIs in conjunction with the end user OEMs to discuss manufacturing options and requirements in order to better direct PI research.
- There is no clear indication as to how this center is calibrating their work versus DOE storage goals.
- Unable to judge until the proposals are selected.
- Project assumes that the private sector is unmotivated and incapable to respond to motivation of the initiative itself and that the PI's organization is more capable than the balance of the private sector in finding and coordinating manufacturing research and development efforts. Both assumptions are questionable.
- Basically only held a workshop and that is not part of the vision.
- Potential to be meaningful, but need to actually do something.
- The project nominally supports the hydrogen initiative as evidenced in its objective/mission statements. However, until the projects are fully defined, it is not possible to determine if this is so.
- Project selection process at NCMS is not conducive to choosing projects that best meet goals and objectives set by DOE and the President's Initiative.
- Projects chosen could be completely outside the critical needs of the program.
- Appears to be in conflict with the funding agency (DOE).
- Program is not focused on DOE objectives, does not support the President's Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

This project was rated **1.5** on its approach.

- The general approach to developing the collaborations is obvious, but the approaches to the specific proposals remain to be seen.
- Approach seems reasonable, but after 7 months there is no significant progress to show.
- The approach is reasonable.
- The approach is poorly designed, and not well integrated with other research in the DOE portfolio. Underway for 6 months, the technical approach is not defined.
- Ideas were biased toward pre-chosen presentations/proposals by members.
- NCMS being heavily based on automotive manufacturing, is completely ignoring the guidance of the OEMs. DCX, Ford, GM, and Toyota were all present at the workshop.
- PI has no practical experience with fuel cells or hydrogen storage and thus has no way to determine the level of technology and what issues require attention. A one day workshop attended will not suffice to learn the issues and thus their funding decisions will be gravely misguided.
- This is obviously a "money grab" since any participant who has been awarded a project will need to start paying dues to NCMS. This is not in the spirit of DOE funding (although it can be assumed every tax payer pays dues to the DOE through taxes).
- Limited discussions on the barriers and how they were addressed.
- The project selection process appears to be somewhat closed. Not sure how much input DOE will have.
- The only items of value are the projects selected which may or may not be relevant to DOE Program needs. DOE must have the final approval on which projects go forward.
- The only value added by NCMS is project management.
- Appears to be in conflict with the funding agency (DOE).

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

This project was rated **1.4** based on accomplishments.

- NCMS has conducted one workshop. Results were confusing since most attendants agreed that the level of technology was advancing too rapidly to discuss manufacturing of already obsolete components. Why should anyone fund a cheaper humidifier when the aim is to eliminate them with low humidity membranes?
- Most of the topics they highlighted are still basic materials research which has nothing to do with manufacturing at this point.
- PI states that this project is 23% complete but they have nothing to show for this other than a report of a workshop and a list of participants providing "preliminary program inputs".
- Presentation was perfunctory; no valuable information was presented and no progress or accomplishments reported.
- The accomplishments thus far are modest and could have been achieved through other means.
- Only workshops have taken place with proposals solicited.
- While NCMS is collecting proposals, no proposal details or technical accomplishments mentioned.
- Project has recently started. No technical accomplishments have been demonstrated.
- Virtually no meaningful progress.
- Not much to report for $3 million in FY 2004.
- Workshop appears to be the major accomplishment.
- Two workshops and a website with project proposal forms do not constitute progress.
- Should have shown all the project proposals received, which were rejected, and why.
- The group has made no technical progress. .

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

This project was rated **1.9** for technology transfer and collaboration.

- Technology transfer and collaboration have yet to be seen.
- PI listed many company names but gave no indication if any of them will actually be participating in federally funded research.
- NCMS is not strongly supported by the OEMS. DCX is not a member.
- This project will do little to educate the participants on manufacturing issues. This is best left to the normal business practice of OEMs specifying their requirements to researchers. NCMS is a redundant entity.
- The type of information to be gained and shared by this project could be done through other means without the need for the support of a separate project.
- Many participants involved in project.
- Excellent connections.
- Each project could collaborate with the others but the projects have not been selected yet. It is unclear if any storage-related work will result. This is a potential strength that NCMS could bring to the table.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.4** for proposed future work.

- The proposed research may lead to improvements but it was quite difficult to tell based on the information provided.
- Have had workshops to initiate project solicitations, so all work is "future" work.
- Proposed scope appears too broad. Focusing on critical few aspects would ensure better progress towards objectives.
- Can't say, they really were not shown.
- Projects have not been defined yet.
- The proposed future research depends on which projects are selected.
- The proposed future work as currently stated is unlikely to change the relevance or progress of this activity.
- Perhaps the single most obvious "low hanging fruit" project that NCMS could have funded was not selected. [A project submitted by a rubber hose maker to manufacture.]
- Wait and see.
- Want to see what the proposals are first. Disappointed that the PI did not provide the opportunity to review the kinds of manufacturing projects suggested for future expenditure of federal funds - if only from the broadest perspective.
- Strong lobbying committee that knows how to solicit funds.

Strengths and weaknesses

Strengths
- None apparent until the proposals are seen.
- Many participants involved in collaboration.
- Concept of the project is expected to address critical manufacturing issues that are vital for implementation of new hydrogen technologies.
- Significant cost share [to be provided by proposers (not by NCMS)]
- NCMS could bring together a diverse industry group to address manufacturing issues, if properly utilized.

Weaknesses
- NCMS is a redundant and unnecessary entity. The OEMs are capable of addressing their own manufacturing requirements with suppliers.
- Difficult to assess this project.
- None apparent at this time.

- The presentation was superficial and lackadaisical, almost as if NCMS does not value their participation in the process or the input received. This is truly disappointing considering that almost $3 million of taxpayer dollars are about to be expended on research projects funneled through NCMS.
- No mechanism discussed or otherwise presented for developing a ranking and prioritization of manufacturing needs or differentiating between that which is possible now and that which will need further development. Project was poorly motivated by speaker and in overheads provided both of which did not make a strong case for the need of this work.
- Project, as presented, doesn't identify any technical goals, progress or approach. While it is understood that this project has not been running for long and therefore may not have much in terms of accomplishments, technical goals and approach should be defined.
- 23% task completion has been stated, however, no new concepts of ideas have been reported so far.
- This program was given a large list of valuable projects prior to the workshop. These were ignored when the workshop was held. It is unclear what they will actually do.
- Storage work will be only a portion of the available funding.
- No guarantee that projects will be selected that address the technical barriers and goals of the President's Initiative.
- Too much of the monies are going directly to NCMS, which does not have the expertise in-house to provide a significant contribution to the advancement of the Hydrogen Economy.
- High NCMS structural cost overhead.
- Having NCMS going around the country talking to fuel cell/hydrogen storage developers/etc to explain the work and gather work is a waste of funding, since DOE has already done that.
- Difficult to make constructive criticisms on such a project. Don't see a purpose for the project.
- This program is extremely weak; it is poorly connected with DOE objectives and other research in the DOE portfolio.
- No real attempt to even address the goals or further the vision.

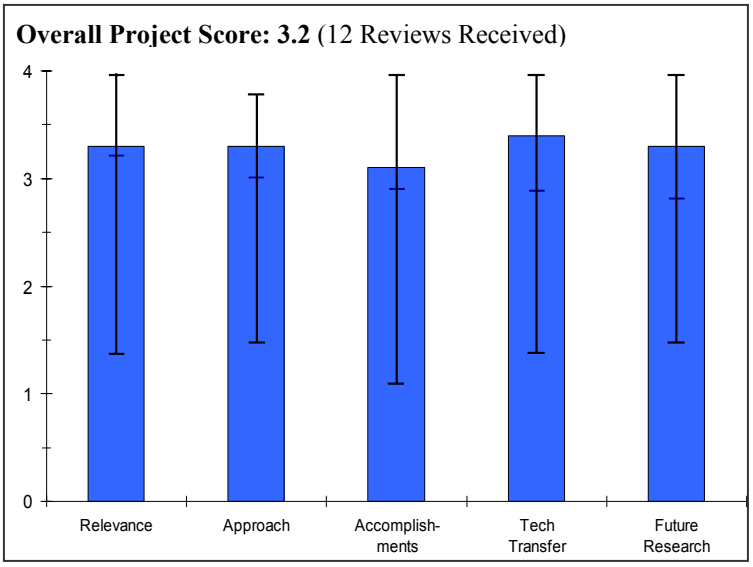
Specific recommendations and additions or deletions to the work scope

- Project team should spend time with DOE to focus on and establish goals and then identify an appropriate technical approach for accomplishing those goals. A milestone chart with timing and deliverables as addition to the project plan can be recommended.
- Try to steer storage work to address manufacturing issues with high-pressure and possibly cryogenic storage containers.
- Delete the task to provide a clearinghouse of information to promote technology utilization; this is a cleverly disguised support activity for the library staff at NCMS.
- Delete the task on development of the website http://hydrogen.ncms.org. It is a duplication of information that can be found on the DOE website.
- Delete the roadmap development task led by NCMS. FreedomCAR and Fuels Partnership Fuel Cell and H_2 Storage Tech teams (industry and DOE members) should offer advice and consent to the project portfolio to ensure alignment with the President's Initiative.
- Do a benchmark on the NCMS project management fees relative to other organizations to ensure they are competitive.
- Keep on working the proposals.
- Presentation of non-Storage activities should be withheld from Storage review sessions.

Project # ST-18: DOE Carbon-based Materials Center of Excellence: NREL Activities and Overview
Heben, Mike; National Renewable Energy Laboratory (NREL)

Brief Summary of Project

The focus of the National Renewable Energy Laboratory's work is on hydrogen storage in carbon-based materials in conjunction with the DOE Center of Excellence (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. The main focus in 2005 was on producing materials that reproducibly store hydrogen with capacities greater than 4 wt% at room temperature and pressure.



Question 1: Relevance to overall DOE objectives

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

- Hydrogen storage is the highest priority research area supporting the vision of the hydrogen economy.
- Centers of excellence are important initiatives supporting development of hydrogen storage technologies.
- In line with the program objectives.
- It is likely that most of the novel material breakthroughs will come out of this area of research. It currently seems that the most efficient systems will be achieved with materials in this 10 to 50kJ/mol range. Center of Excellence should remain open to all materials (not just carbon) in this range.
- These materials don't seem to have many of the hazards associated with metal hydrides (pyrophoric, etc) and don't require as much rare earth components.
- The Center of Excellence addresses a variety of DOE plan objectives and is relevant to the President's Initiative.
- The PI clearly presents the objectives and the goals of the Center of Excellence and the related projects.
- Good science; thorough investigation of carbon-based materials potential; it has moved beyond SWNTS.
- This project is one aspect of efforts that are critical to the achievement of the goals of the President's hydrogen fuel initiative.
- Goals identified with strong alignment to the project.
- Can carbon be a cost effective solution for H_2 storage?
- A light material that has a moderate strength bond is well in the scope. However the goals are highly unlikely to be met with normal nanotubes based on current understanding of physics. Metalized carbon materials have potential to meet goals if light metals will work. So the program has had mixed relevance but is moving in the right direction.
- Very relevant to the objectives of the DOE H_2 storage program. Originally only appeared to have a chance to meet 2005 targets but have shown pathways to engineer materials with up to 8-9 wt. percent.
- Generally relevant to program, but many of the materials under study will not meet volumetric targets.
- Good to see focus on new ideas, potential higher-density materials, etc.
- The newly proposed doped-C60 materials have the potential to meet the 2010 gravimetric goal, though volumetric densities are less clear.

Question 2: Approach to performing the research and development

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

- The project adopts the acceptable approach to focus on a variety of technical issues to investigate alternative and innovative solutions.
- PI has broadened the R&D approach beyond carbon nanotubes to other carbon based storage technologies increasing the chance of success.
- The taken approach of reproducible activation and rational design of adsorbents brought a breakthrough and moved the work a step forwards. Very promising work.
- Approach is quite reasonable. There is a nice coupling of theory with experiment.
- Project is well organized and strongly focused.
- Project is integrated with other research including new Carbon CoE.
- PI has assembled a strong team of industry and academia.
- PI has expanded scope beyond SWNT.
- PI is beginning to focus activities using modeling to predict stable materials and, as appropriate, equipment in order to synthesize materials.
- PI has done a good job in addressing measuring techniques of these materials which to date have been difficult and sensitive.
- Need to tie in the material development work with the final storage system requirements.
- Develop a clear material vetting process for down-selecting the candidate.
- No clear pathway is given for the synthesis of the molecular modeling work.
- The metalized work makes sense to follow through on.
- The work on simple nanotubes was less likely to yield functional compounds.
- Use of theory to guide experiments is good (generalized gaussian DFT).
- Approach to materials other than SWNTs gives NREL hope to achieve higher wt. percents that have a chance to meet the 2010 targets.
- The rational design of adsorbents appears to be a good approach and one that is necessary to move beyond the SWNT work. A better approach than some form of combinatorial investigation.
- Using modeling, different synthesis techniques, different carbon structures; attempts to look at many aspects of absorption process for hydrogen storage.
- The main idea on which the majority of the work is based is theoretical; need much more information as quickly as possible about the experimental viability of such an idea: how can (large quantities of) these materials be made, will they be stable, and of course will they store the amount of H_2 predicted?

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

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

- The highlight of the last year is the portfolio diversification of the possible carbon-based storage molecules. It is critical to continue this pathway and create a well-balanced program with proper risk mitigation.
- The results well support the progress also in consideration of the effort to have the CoE starting with the related project operative.
- Good work beyond CNT. Many research paths being followed by technically competent researchers resulting in very interesting progress.
- Modeling of higher capacity materials has been synthesis of these materials has not been demonstrated yet. These materials cold be extremely difficult to manufacture at first. If manufacturable, it will take some time before the processes are robust and efficient.
- Advances in material development via reproducible processing.
- Promise of high reversible storage with carbon-based adsorbents.
- Advances in measurement techniques for materials and accurate sample screening.
- Accomplishments have been quite substantial; in many ways leads the CoEs in accomplishments relative to funds expended.
- Significant accomplishments, making progress towards goals.

- Since the Center was formed, the NREL project scope has been broadened significantly so that a number of innovative ideas and approaches have been generated targeting new materials discovery.
- Calculated binding energies in various carbon metal complexes. Challenged for volume goal but perhaps other materials of the same sort will yield better density.
- Apparently show that simple nanotubes are not holding much more than what the metals bind.
- Made BN single wall nanotubes. Built multisample test station.
- Not a lot of lab movement but good theory progress.
- Excellent progress in past year on a number of fronts. Understood the effect of metal introduction into the SWNT on uptake of H_2. Developed materials that show reversible 6-7 wt. percent capacity.
- Experimental results have been verified by others. Addition of CoE partners offers good prospects for achieving higher weight percents.
- Appears to have validated measurement techniques.
- Has improved understanding of source of earlier variability in measurements.
- Should give more attention to the projected volumetric H_2 density of these modified C60 structures. In a relatively optimistic case, the projected density is 43 $kgH_2/m3$; do these materials have (even theoretically) a chance to meet the 2010 or 2015 goals?
- Showed binding energy for single metal atom on a single pentagonal ring. However, is there metal-metal interaction (i.e., is the binding still strong for a fully covered C60 molecule with 12 Sc atoms?). Is the fully-doped system stable with respect to decomposition into C60 + metal or C60 + metal hydride?
- The desirable H_2 binding energies were calculated with GGA, but later it was stated that LDA is generally closer to experiment. This raises the obvious question: what are the LDA binding energies for H_2 onto these metal-modified C60 molecules? Are they still in the "desirable" range?
- Assuming the best-case scenario (full implementation of the H_2 economy), what is the total amount of Sc that would be needed? Is there that much Sc in the world?

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

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

- The Center of Excellence has a well structured network of internal and external contacts, with appropriate connections with some independent project, such as SwRI testing and Argonne system analysis.
- PI has assembled a strong team of industry and academia.
- PI is using tank modeling to understand material requirements in a viable automotive system.
- The direct technology transfer to industry requires more effort.
- Broad and well respected collaborations. Only possible concern is that there may be too many institutions working on too many things to be able to adequately manage and direct.
- Very good collaboration record.
- An extensive list of publications.
- Good record of conferences organization.
- Work is well coordinated with external partners. Developments from the partners and external community are incorporated where appropriate into Center efforts.
- Very strong collaborative effort, including several universities, other National Labs and industry members
- They are well connected. They did point out the size and diversity of the team. Little industrial collaboration, but they are far enough from production that is not problematic.
- Collaboration has increased over the past several years as NREL has interacted with other organizations attempting to duplicate their results. Addition of Center of Excellence partners will only increase the collaboration in the future.
- Collaborations include universities, industry and staff at NREL

Question 5: Approach to and relevance of proposed future research

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

- The Center of Excellence is the most appropriate mechanism for integrating knowledge, expertise and for pooling resources in a range of research areas.
- The future work plan is to be complemented on the detail with which it was presented and the degree of incorporation of other organizations (i.e., Air Products & Rice) on the steering committee.
- Future work directed to support goals and accommodate previous review comments.
- Expanding beyond SWNT is critical.
- Revisiting the pseudo cold/pressure region is recommended, this may be the "sweet spot" or at least the happy medium where overall system efficiencies can be acceptable in terms of compression/cooling energies and transfer from forecourts to vehicle tanks - especially if bulk transport of hydrogen will continue to favor liquid hydrogen.
- Difficult to manage breakthrough. However a key metrics is how many materials are rejected. Optimal use of the resources will be a critical issue.
- The future activities require more specifications.
- The work appears based on experimental results and progress.
- Future plans to roll research into the center of excellence make sense.
- Need to continue on path away from carbon nanotubes to evaluate other alternatives.
- The future plans described here are building on past experience making appropriate steps forward.
- The group is encouraged to examine work beyond nanotubes.
- Interesting plans but they are really for the new center program so it is hard to evaluate.
- Work plans at NREL build on past work and really move off the SWNT path.
- Addition of CoE partners really broadens the work front and gives the carbon-based approach a chance to meet the 2010 goals and position the group to propose pathways to reach up to 8-9 wt. percent material.
- Will fold into CoE.

Strengths and weaknesses

Strengths

- Center has aligned itself with the appropriate partners and is learning what potential systems may look like.
- Systems in the 100bar, 100K range seem to have good potential. The usable capacity of the materials exceeds any metal hydride systems.
- The materials come from cheap sources- time will only tell if synthesis of the large carbon structures will be cost effective.
- Excellent resources.
- Outstanding personnel with superb scientific background in the field.
- The integration of expertise, equipment and know how's in the CoE may be an accelerating factor in reaching the proposed objectives.
- The CoE has developed instrumental resources of valuable and general interest.
- Strong collaborations with key organizations.
- Strong team delivering very good science.
- The group is very well organized.
- Significant collaboration with industry, universities and other labs.
- Broadening scope of the project towards generating novel concepts and new materials discovery, focus on fundamental understanding as well as systematic approach (modeling) increases overall probability of success.
- Theory to guide experiments is a good improvement. Move to metal doping gives a possible success route that does not need new physics. Adding low temp work is a good move.
- Partners bring wealth of experience and new ideas to the field of C-based H_2 adsorbents. CoE appears to be off to a solid start.

Weaknesses

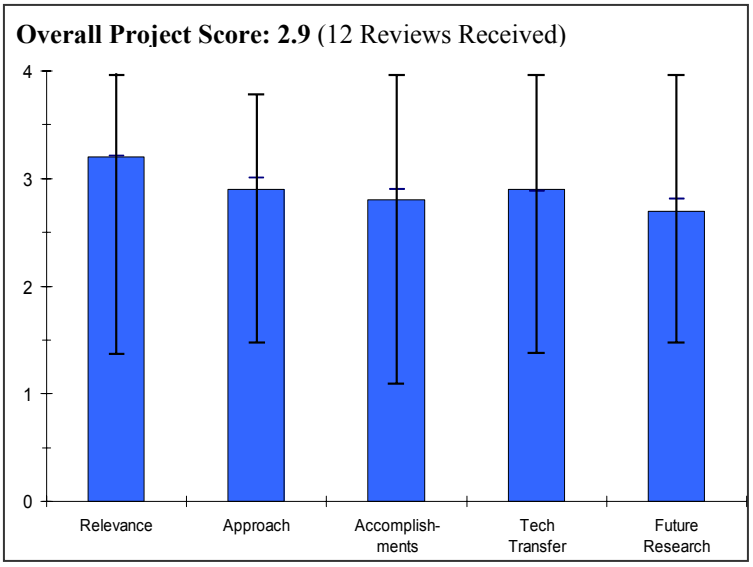
- Should increase flexibility.
- Difficult but try to find the optimum balance.
- Need to establish the internal material selection process/criteria.
- The start of the CoE and the establishment of defined projects may reduce the flexibility of small research groups to freely establish collaborations (outside the CoE).
- Still a risk area that may not deliver.
- With all of the parties involved coordination could be very challenging.
- Still some approaches consider 5-6%wt storage capacity (material basis). In addition, volumetric and cost targets aren't always given a priority.
- Still a very difficult thing to accomplish. Metals may not stay put. Density is too low but they may be able to find a solution.

Specific recommendations and additions or deletions to the work scope

- Center should continue to expand activities beyond carbon work. It seems that all materials with low bonding energy 10 to 50kJ/mol would be appropriate for this center and the systems would be similar.
- The system analyses work on SWCNT should be given a high priority to establish the minimum baseline requirements for the material.
- The key issue with these materials is the volumetric storage. Powder packing and densification (as shown in metal hydride) is one of the important parameters. Need to address the structural vulnerability of these materials early on.
- The future work plan has to include better specification on a complete characterization (including cycleability) of the selected materials with protocols agreed to with other CoEs and with SwRI.
- DOE ought to consider a go/no go decision regarding single wall carbon nanotubes as it is clear that early promise has not been repeated or confirmed and likelihood of meeting program goals is slim.
- Carefully follow the planned milestones.
- Carry on with the work on a wider variety of carbon-based materials.
- Ensure an appropriate mechanism is put in place for the effective management of the Center of Excellence; this could be challenging in view of the size of the consortium and the diversity of the expertise there.
- Continue with the forward plan with an addition of go/no-go decision points for every approach, having in mind meeting all the targets (system basis) as a top priority.
- Should try to verify that metals on nanotubes do not move. This would best be done with single atoms but one may well get the answer with patches.
- Synthesis of these advanced carbon materials seem difficult to manufacture cheaply. Could PI present more information on general production methods and economies of scale?
- Should start narrowing the gap between molecular engineering work and marcro system requirements.

Project # ST-19: Analyses of Hydrogen Storage Materials and On-Board Systems
Lasher, Stephen; TIAX LLC

Brief Summary of Project



TIAX will evaluate 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). System-level conceptual designs, process models, activities-based cost models, and lifecycle performance/cost predictions will be 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 will be 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 **3.2** for its relevance to DOE objectives.

- This type of modeling is crucial in order to provide guidance to material researches as to the true requirements of the materials they are developing. This information needs to be consistently relayed to all other PI researchers.
- Good to see where we currently stand with cost, performance and overall model refinement.
- One of the most critical projects. The main criticism is why DOE did not start this type of work much earlier.
- This project provides analysis and validation of fuel cell system performance, and projects cost factors and life cycle analysis based on current state-of-the-art inputs from suppliers.
- Recognize need for independent financial analysis of hydrogen storage systems.
- A critical component of the initiative. It will help assure viable pathways are identified and followed for economic onboard storage systems. Wish they were designing for DOE storage rate, volume goals -- hence a slight downgrade on relevance.
- Idea is well aligned.
- Alanate is probably the best material to use at the moment though it will not meet goals it is likely to be similar in many regards to "the material" and so will be a suitable surrogate. Model is probably only the first cut.
- Very relevant to the objectives of the DOE H_2 storage program. Materials developers need the benefits of a systems approach to understand the implications of BOP components on the characteristics of the storage system. DOE needs the ability to compare storage options on a consistent basis.
- Very relevant to R&D plan objectives. Analysis of all storage options on a self-consistent basis very important to understanding where DOE should put its emphasis.
- Supports program and program participants by modeling projected costs of storage system types.
- An important program in the DOE portfolio. Important for not only DOE, but generally for all of the PIs to be aware of these sort of system-level issues, and will help guide new and existing projects.

Question 2: Approach to performing the research and development

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

- PI did good job of accumulating much data from reliable sources.
- Modeling is based on current technology without being overly optimistic about novel approaches.
- Well thought out with many factors incorporated. The effects of cycling/lifetimes should be integrated into the results.
- The rating could have been 4 if the mid-point estimate would have been re-calibrated to a more realistic case; and the most optimistic case and pessimistic case would be given some probabilistic quantity.
- It is imperative to develop a rigorous model as the results may have profound implications on future directions of the DOE's grand challenge.
- Thorough, systematic approach back on realistic input from suppliers and potential users.
- Approach is logical and reasonable, but question whether or not there is sufficient technical progress and information in the various technology areas to begin making financial assessments at this early stage of the program.
- The approach is reasonable and generally well thought out.
- Not very detailed model technically but that may be OK for the moment. An assumption of current assembly process is disturbing if this is ever used for cost prediction. Cost model is adequate though not complete. Sensitivity analysis is a key point.
- Approach is to develop a generic system design that is based on available literature, information from developers, and theoretical analysis where data are not available. The cost of manufacturing this system is then estimated and the energy efficiency and well-to-wheels efficiency can then be estimated. This approach allows a consistent comparison across the various storage options. It gives materials developers a sense of the complexity involved in designing a realistic storage system.
- Have all appropriate components.
- Not adequate consideration of various system designs.
- Limited scope to date.
- Doesn't provide information that PIs or program managers can use to direct projects toward better storage systems.
- The approach seems sound. There are many assumptions involved in any such analysis, but these assumptions are clearly stated; the method needs to be flexible enough to be able to easily react to changes in the assumed input parameters as new data is generated and new materials are discovered.

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

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

- The project is showing significant progress towards meeting goals.
- Sobering results reveal extremely heavy systems
- Results are exhaustive and realistic given current data available.
- Achieving results will be more complicated with chemical hydrides as the potential amount of systems permutations will be much larger.
- Continue work for cooled activated carbon like materials.
- Used lots of design work from UTRC's project and lots of the numbers used (60% packing) are very optimistic based on the actual progress of the UTRC project.
- Very good start.
- Significant progress - compressed gas and metal hydride storage analysis completed.
- It seems to be too early in the development process of a NaAlH system to exercise the amount of rigor necessary for financial analysis. It is too early in the program to expect any directionally correct results.
- Gathered input data.
- Model based on current UOP assembly process which is very suboptimal.
- Model constructed to test this.
- Have looked at some sensitivities.

- Properly finds this system will not meet goals.
- The results are surely in the right direction but the answer was known beforehand.
- Value is in sensitivity identification and generalization (which is not clearly present).
- Results for a sodium alanate system were presented. The assumptions used in developing the analysis were conservative. Sensitivity analysis can lead to alternative designs and point out areas needing further development. This analysis represents the base case which will be updated as advanced materials are developed.
- A good start for the first year.
- Good to use the Na-alanate for the first base-case study. Pretty much shows what we all would expect.
- Completed most of analysis for specific design.
- Has done limited sensitivity analyses.
- Has not considered many of the aspects of the design considered.
- Good progress on the alanate system design. The results show the serious challenges involved in using this material in a system.

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

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

- Collaboration with academia is very good in order to define material characteristics.
- More coordination is required with tank manufactures to determine practical tank building techniques; current schemes are not amenable to mass production.
- Lots of data used is based on UTRC goals which they have had problems meeting.
- Active team approach includes university partners. Also receives feedback from noted labs, developers and other stakeholders.
- Are the collaborators chosen sufficiently expert in the fields of engineering and manufacturing to contribute to cost assessments?
- The project has not provided significant evidence of working with equipment manufacturers regarding developments in the industry. This question was adequately addressed in the Q&A time period.
- Suitably connected.
- Collaboration with other organizations is good. It is particularly strong with developers as each system is developed.
- Urge that close connections remain with national labs and centers of excellence.
- Might wish to expand collaborations to industry projects (e.g., UT, P.S4). Would help to reduce any possible duplication of effort.
- Clearly has good potential for tech transfer to industry and others.
- Consistently references work/individuals providing input to model.

Question 5: Approach to and relevance of proposed future research

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

- Active plans to complete analytical tool for chemical hydrogen and carbon-based storage systems to allow "level playing field" comparisons and the leading storage technologies.
- Thermal integration to the stack will be important next step to evaluate if any system mass reduction is possible and effect of slow kinetics from the fuel cell coolant loop on hydrogen demand.
- Continue to coordinate with ANL tank modeling and industry; will be crucial to refine data.
- Will be interesting to see results on chemical hydrides and activated carbon. There might exist several model cases for chemical hydrides which will make it difficult to dedicate sufficient time to each one.
- Vehicle integration. Work with Millennium Cell for work beyond alanates.
- The project needs to do more at a much faster pace. The urgency for these analyses is acute. However, this urgency is not reflected in the future work.
- Here again, while the need for cost and WTW analyses are recognized, it seems premature to begin to do so until there are more fully developed systems to base the analysis on. Can the process be delayed until there is credible information that can be used?

- Future research plans are reasonable.
- Incorporation of storage materials that won't meet targets.
- Logical further work, but again this will show that these materials will not work. Also concerned about improperly optimistic supplier numbers twisting the analysis as TIAX is not informed enough to know when they are being led.
- Work plans call for each class of system to be analyzed. Vehicle integration issues need to be considered and could impact the system design.
- Baseline designs for all the storage systems need to be established early on. This may be difficult if the developers make major changes in their system designs. Some means should be developed to keep the baseline This is a good project; it will last five years.
- Will properly look at all the basic forms of hydrogen storage on a self-consistent basis.
- Topics are right, i.e., doing analyses on other storage technologies.
- How good the future work will be depends on how the analyses are performed.
- Value of WTW analysis will depend on how well it's done and how the information is used by constituency.

Strengths and weaknesses

Strengths
- TIAX has a strong history in modeling systems. The level of detail and completeness is better than most models. TIAX consistently seeks the inputs from it current and potential clients and will model many systems to determine feasibility.
- Good overall understanding of issues. Working well with various people to have accurate model.
- The project has good approach and methodology.
- Generating practical data for storage systems using existing materials and feeding R&D with input parameters for designing of new materials.
- One strength is that it is being done at all.
- Experience in developing cost estimates based on referenceable system designs. Rational system designs for hydrogen storage systems can be fed into integrated vehicle designs/analysis. DOE needs this type of comparison to make decisions regarding future direction of storage program.
- Capable independent analysis shop.

Weaknesses
- TIAX may not necessarily have all the technical background required to model these systems. It is important that they continuously seek and solicit knowledge from the appropriate sources.
- The values used in analysis vary from the actual values achieved in storage projects.
- Need more frequent update and coordination with the Tech team.
- Faster turn around.
- Using data obtained in R&D labs for system design that may not be accurate enough.
- Based on the current low volume process which is not reasonable. Based on materials known not to meet goals so has limited value. Concerned the well to wheels will come out saying that this is representative of what these sorts of systems can do and it is not, it is a snapshot in time.
- Am I correct in sensing some overlap with Project ST20 (ANL)? If so I suggest better coordination.

Specific recommendations and additions or deletions to the work scope

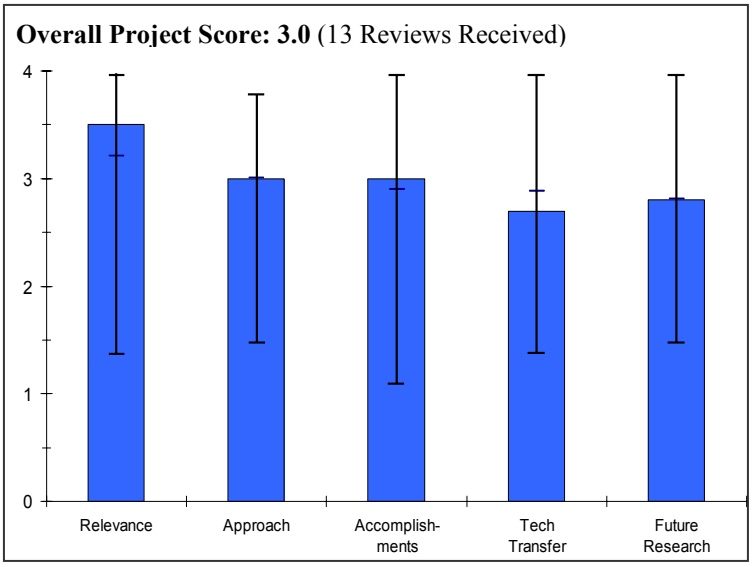
- Continue work on metal hydrides with thermal integration from a high temp fuel cell (120C).
- Work on the activated carbon tank and coordinate with ANL prior to more complex chemical hydride system.
- The analyses projects in the storage area are critical to the success of the entire program. Their scopes and resources should be expanded. The analyses groups should be protected from external pressures. Their assumptions (in details) have to be reviewed in detail often.
- It is important to have concurrent projects in the area to further validate the results.
- It is recommended to change the work outline from a sequential process to a multi-task parallel work group.
- The early and extra investment in this area will invariably yield short and long-term dividends for the DOE.

- It will be key for the DOE to capture the source code for this model so that it can be updated as needed for better refinement, and so new data can be "dropped in" to look at new materials.
- General recommendation to DOE: establish a more visible (Center of Excellence) system design team to establish baseline designs and rapidly incorporate latest designs that are being proposed by the storage system developers.
- Continue into new storage media as they are discovered. Evaluate amides at once. Maybe AlH_3 soon.
- Expand collaborations with hands-on storage researchers.
- Need to constantly work towards keeping all of the PIs of the other DOE projects "in the loop" on their work. (They appear to be doing this currently).
- The UTRC results seem to show even significantly worse performance characteristics than proposed from this model; the PIs should work to identify whether this discrepancy is due to unrealistic assumptions in their model, or represent an opportunity to improve the UTRC design.

Project # ST-21: Standardized Testing Program for Chemical Hydride and Carbon Storage Technologies
Page, Richard; Southwest Research Institute (SwRI)

Brief Summary of Project

In this project, Southwest Research Institute (SwRI) will develop and operate a standard testing laboratory aimed at assessing the performance, safety, and cycle life of emergent solid state materials (e.g., metal hydride, complex hydride and carbon) and systems for the storage of hydrogen. As part of this program, SwRI will work with industry, DOE and other stakeholders to develop an accepted set of performance and safety evaluation standards. Once fully operational, SwRI will apply those standards to provide analytical and R&D services at a national level.



Question 1: Relevance to overall DOE objectives

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

- The project provides DOE with facilities to independently assess the performance of solid state storage materials.
- It fully supports the RD&D plan.
- The project is highly relevant to the DOE's objectives, because it accomplishes the role of independent testing institute.
- Need to have an unbiased and reliable testing facility to evaluate performance of storage materials developed under the DOE program.
- This is a much needed activity for independent standardized testing particularly in view of the little unanimity of storage values reported so far in literature for carbon-based materials and contradictory storage claims.
- The goals of this effort are critical to the verification and validation of developmental claims of other components of the initiative. Hence this effort is critical to the realization of initiative goals.
- Key to accomplishing the hydrogen program goals.
- Focused on providing measurement/validation capability.
- This facility and its associated capabilities will be useful in the short term but will become progressively more critical to the HFCIT RD&D program in the longer term.
- Very relevant to the objectives of the DOE H_2 storage program. DOE requires an independent assessment of the storage capabilities of newly developed materials. Developers also need independent confirmation of their results.
- Provide independent validation of material performance.
- Important program in DOE portfolio. This program will not provide the breakthrough discovery, but will help to enable such a discovery.
- Establishing consistent measuring techniques is crucial to the variety of testing conditions that exist and chance for error in measurement.

Question 2: Approach to performing the research and development

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

- The approach is to develop facilities and protocols to characterize small quantities of solid state storage materials. SwRI visited the leading labs making measurements of hydrogen uptake and desorption when specifying and installing facilities. The lab is part of a round-robin testing program to verify its capabilities.
- Centralized location is probably necessary to ensure consistency.
- Largest impediment to project will be shipping restrictions on new materials imposed by DOT etc. Time to classify and approve these materials may hinder progress for individual PIs.
- Need to put more effort on characterizing smaller size samples (in the 5-50 mg range).
- The approach is not completely convincing.
- The main technical aspects related to the project objectives are not completely addressed or described.
- Good work developing and setting up lab.
- Good planning and methodology for a working solution.
- Extensive communication with researchers is important for moving forward.
- A well thought out facilities design was made and implemented. High quality components and testing and characterization equipment have been utilized in the preparation of the facility.
- The approach has been well reviewed and is appropriate.
- Good job has been done in the planning and implementation of this facility; application to samples, e.g., during the round robin stage, will undoubtedly bring some shortcomings to light, but this is to be expected--I'm sure they will be manageable.
- Multiple measurement capability.
- Development of sample handling protocols.
- Addresses pre-measurement sample conditioning.
- Gas purity levels in excess of typical laboratory.

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

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

- PI has done a thorough job in setting up a safe and through facility and accommodating the needs of the users.
- Burst chamber for tanks will certainly be too small to test automotive scaled tanks.
- Hardware is in place but the PI did not show any meaningful results.
- After 75% of work completed, the facilities set up have not yet shown up experimental results, apart from the realization of the laboratory.
- The development of protocol is not clearly supported by a real progress: the same flow chart of last year without any comment supporting the progress.
- SWRI has assembled world class test facilities and personnel.
- Round robin testing of samples should provide confirmation of lab capabilities.
- Hard to evaluate this as not adequate information was given with respect to the standard operating procedures and the protocol development. Are these confidential data?
- There were delays in the round robin testing which will actually lead to the validation of the protocol development.
- Not enough information was supplied on the system testing.
- Reasonable progress has been made in the project thus far. A number of comments were made in the Q&A regarding the breadth of testing and certification procedures available. This is not meant as a criticism but to open the door to consideration as to whether the effort is broad enough to meet all the needs of the initiative.
- Not as much has been actually accomplished this last year, the vast bulk of this talk is also in last years talk; indeed many slides are entirely unchanged.
- Gas sampling system was installed.
- Full size system test pipe ready.
- Revising/upgrading SOPs.
- Completed and tested most subsystems. Started round robin testing. Have encountered some delays.

- Progress has been good. The facilities are installed and are being exercised as part of the round robin testing. Protocols and safety plans have been drafted and reviewed by experts in the field.
- Facility nearly operational.
- Progress in facility completion and validation seems to be slower than one might expect.

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

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

- PI could work with some more of the researchers to determine their individual requirements and develop the system to capture as many possibilities as possible.
- Could improve significantly as the project moves on to the next phase.
- The described collaborations appear insufficient and not adequate to the ongoing effort: collaborations with other projects testing components may assist the development of a "standardized" procedure.
- Good list considering scope of project.
- This is a very important project which is greatly benefiting from collaboration with the universities and government research laboratories.
- More interaction with industry is highly recommended for the transfer of procedures and protocols.
- Collaboration in the form of the round-robin testing scheme is to be complemented. Efforts of this type are critical to achieving and maintaining high credibility of results.
- Connectivity is low but this is not a big problem as DOE is doing the key connectivity item - the round robin.
- The presentation did not make clear what the partnering organizations are contributing.
- Collaboration with other organizations is good. It is particularly strong with developers of advanced storage materials.

Question 5: Approach to and relevance of proposed future research

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

- The future research is reasonable, but does not present clearly the activities on the system testing.
- PI should consider installing a larger burst tank should the industry find it necessary.
- Two issues: how to fully utilize the center in the future and how to maintain its independence.
- Effort should be focused on obtaining real systems accuracies.
- Expect round robin testing to confirm capabilities of lab and personnel to permit commencement of testing of samples for evaluation assessment.
- Good planning, moving step-by-step to a fully operational standardized testing facility.
- Future plans are reasonable. Have there been any efforts at international standards of testing and protocol development?
- Essentially the plan now is to measure samples and that is appropriate.
- Round robin is key to being trusted by DOE contractors.
- The future plans are sensible in the context of what the testing program is designed to do.
- Round-robin validation completion, final versions of testing protocols, and completion of construction is scheduled for the remainder of 2005. The facility should be ready to accept first samples in July 2005.

Strengths and weaknesses

Strengths
- Through setup, excellent knowledge of lab setup and commissioning.
- Good hardware facilities.
- The establishment of an independent testing facility with a complete set of equipment and procedure is of paramount importance for the program.
- Excellent facilities appropriate for a verification/validation laboratory.
- Independent testing.

- At this point in time, it is difficult to recognize what strengths mean for this project because of its new/unique nature - in time, performance will reveal the strengths.
- The management of the project appears to be very good so far.
- SwRI is establishing its lab as a state-of-the-art facility that researchers can have confidence that the data generated will be correct.
- First rate equipment used in this facility.

Weaknesses

- PI could communicate more with researchers on requirements. Workshop at merit review is a good start.
- The use of commercial characterization equipment does not permit the kind of resolution needed to characterize small size samples.
- The lack of clear experimental validation data of the instrumentation raises doubt about the readiness and usability of the facility. The protocol is only limited to material characterization.
- Further development of technical expertise.
- Facility restricted to solid-state hydrogen storage.
- There is not enough evidence given on ability to obtain reproducible and accurate data.
- Hopefully, the test facility is ready to deal effectively with the wide variety of sample types it will be receiving
- What is going to happen when the test facility gets a significantly different (e.g., lower) value for the storage capacity of a sample than the provider claims? That's when the fun starts!
- Shipping samples to the facility may be an issue from a regulatory standpoint.
- It is not clear as to how well staffed this facility is or will be. What percentage of time do the PIs spend on the project at this time?

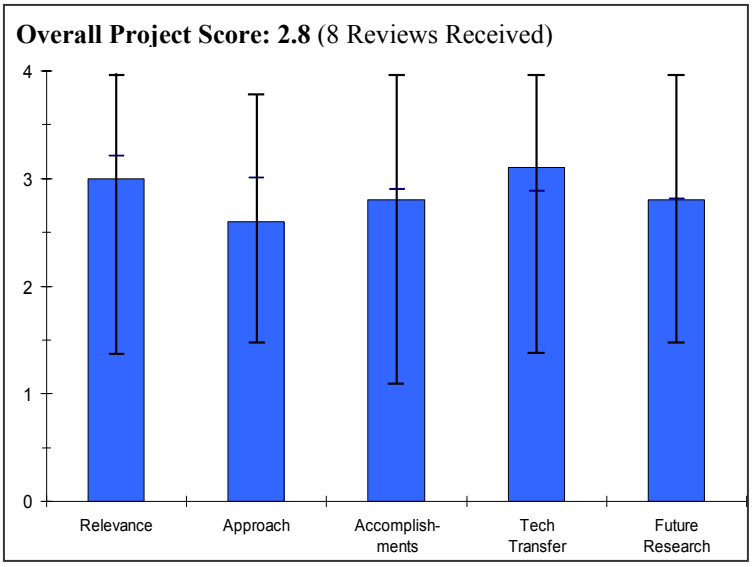
Specific recommendations and additions or deletions to the work scope

- The work on the protocols (material and testing) should integrate feedback from other projects (CoEs, UTRC and so on).
- Conduct thorough investigations to address reproducibility and accuracy of measurements. Publicize these results in order to gain full acceptance of the quality of the measurements by the research community. This will justify the role of an independent validation/verification testing facility.
- As new storage materials and concepts are emerging, added flexibility to evaluate those materials will be important (e.g., liquids).
- DOE team needs to enforce any improvements implied by the round robin results.
- In the early stages of the round robin testing, focus on samples that have straightforward sample handling/activation requirements, i.e., types that are like the ones used in sub-system testing.
- It won't go over well in future peer reviews to stand up and say that results for certain samples received from certain providers are proprietary; the results of measurements made in the context of this particular project should in all cases be available for review and comment.
- Try to determine if the facility/staff have the necessary equipment and skills to understand why measurements differ from those made by the researcher, as they most certainly will from time-to-time.
- In the future, the connections with the Centers of Excellence should be emphasized and perhaps even formalized.

Project # STP-58: Development and Characterization of Novel Complex Hydrides
Zidan, Ragaiy and Motyka, Ted; Savannah River National Laboratory

Brief Summary of Project

This particular project is a 3-year CRADA with United Technologies Research Center as part of their project, "Complex Hydride Compounds with Enhanced Hydrogen Storage Capacity," ST-6. Savannah River is employing its recently developed method to modify or form new complex hydride structures as potential high capacity hydrogen storage materials. The new method is based on fusing different known complex hydrides at elevated temperature and pressure at or near their molten state. This method, named the molten state process, is robust, efficient and can be scaled-up to produce large quantities of modified or new complex hydride materials.



Question 1: Relevance to overall DOE objectives

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

- The molten state process is a welcome alternative to the traditional methods of preparing metal hydrides such as ball milling and solvent ball milling methods. The molten state process will likely scale-up better than ball milling for volume production; however, the high pressures (3000-psi) may limit production to small batches at a time. The PI has created a process which allows hybrid forms of metal hydrides previously inaccessible to be explored. Still- these are alanate materials with poor conductivity and similar system integration issues. It would appear that much higher storage capacities will be required (>13wt.%) to create a practical storage tank.
- Will create more uniformity among the hybrids.
- This project holds significant promise toward meeting DOE's hydrogen storage goals. The outcome should facilitate an end-product that has a relatively low cost of manufacture and appears to be highly scalable.
- The project is relevant to the DOE's objectives.
- Clearly aims to develop new hydrides aimed at DOE 2010 targets. Materials chosen may have limits short of targets.
- This project supports the larger UTRC project [ST-06].

Question 2: Approach to performing the research and development

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

- New process has opened many opportunities and permutations. Covering these permutations may take a long time. PI should develop a rapid screening process similar to UOP or UTRC in order to cover as many possibilities as possible. Currently, PI requires one week to prepare and analyze each alloy- too long
- Addressed several hurdles need to overcome.
- The approach used builds on prior knowledge to be able to quickly explore new possibilities for promising compounds.
- Comparatively somewhat limited with other methods but meets the adequate. The minimum projected output from this project should be about 20 to 35 characterized alloys. However, it is not clear what internal vetting process will be used to down select alloys.

- The approach clearly stated specific technical challenges. The application of the molten salt process (MSP) may be of twofold use: further validation of the process and production of low-cost novel hybrid alanate compounds.
- CRADA with UTRC (Proj. ST6) a good approach. The MSP approach is innovative and offers a route to new materials. Not certain it can be extended beyond the low melting alanates and borohydrides. The project is oriented mostly toward alanates and related complexes. This area is overworked within the HFCIT program.
- Focused heavily on a specific material synthesis approach. Limited analysis of material hydrogen properties, e.g., only TPD measurements. Limited material analyses, e.g., mainly XRD.

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

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

- Reduction in dehydrating temperatures is very good progress. Demonstrating higher capacities is yet to be shown.
- Still very new project. [Project was funded in FY-04 and FY-05.]
- Progress this year has been significant given the funding level provided and results are exciting.
- Considering the amount of resources, the project has shown modest progress. The key is the potential usefulness of the project.
- The technical achievements are well in line with the planned activities and motivate an acceptable progress of the work. The results are promising and a focus on a few developed materials would be preferable.
- Good initial results with MSP. Will the MSP process (high pressure and temperature) be amenable to commercial scale-up?
- Some progress has been made in synthesizing a complex hydride phase. Modest progress may be due to limited funding of project.

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

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

- PI has collaborated with presumably an industrial partner which will hinder transfer of knowledge to research community considering that this is still fundamental research. PI is otherwise fully capable of completing work themselves, not too much collaboration is required. If PI is collaborating with an OEM, then understanding of systems requirements should follow.
- Worked specifically with UTRC and Norway Institute Energy Technology.
- Collaboration with private industry and universities has been highly effective.
- The collaborations are adequate and well integrate with the planned work.
- Excellent national and international collaborations. Good opportunities for tech transfer.
- Partnerships limited to UTRC, which this project is a part of, and with analyses partners. Could use some materials development partners.

Question 5: Approach to and relevance of proposed future research

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

- Seems to repeat some work that has already been shown to be unsuccessful as far as finding new complexes. Will help with uniformity of hydride production.
- Future plans are solid for becoming more focused on the investigation of new compounds.
- The future research is based on the previous results. The characterization must include some cycling test.
- Future work identified as continuation of current effort.

Strengths and weaknesses

Strengths

- PI is a very capable and amenable researcher, to degree of legal limits, PI is willing to share knowledge. PI has very capable lab equipment and synergy with many other strong researchers in near proximity.
- Hits on several key issues needed to move forward with the hydride research. Molten state process.
- PI's prior experience and extensive work in the area. The basic technical approach is set. Can produce larger sample amount and potentially simpler route for large scale production.
- SRNL has an innovative preparation process (MSP) and facility to analyze the characteristics of the produced materials.
- Project is aimed at new materials discovery. Molten State Processing method of synthesis is novel, and may be utilized as one of the synthetic approaches for other classes of hydrogen sorbents.
- A new and innovative approach of making doped samples (MSP).

Weaknesses

- Material is still alanate based with all the drawbacks of alanates and poor system performance. PI will need to aim for greater than 7.5% density to achieve DOE goals. Achieving 7.5% at reduced temperatures and good cyclic qualities will still be impressive.
- Comparatively longer time is required for sample processing and characterization but larger sample amount is produced.
- The current IP arrangement could actually limit the PI's success and effectiveness in the long run.
- Main limitation of the project is focusing on alanate systems that have not shown any promise to meet gravimetric targets (system basis) nor produce novel phases. It has been also shown in the past (Jensen et al) that initial synthesis method is not important to the structures after cycling.
- Too much focus on the overworked alanates. They are covered in detail in other HFCIT and offshore projects. They also have some safety problems.

Specific recommendations and additions or deletions to the work scope

- The alloy down select process should be aligned with the system analyses work.
- Continue on the current path. Develop rapid processing techniques to expedite research. Try to apply similar techniques to amineborane materials which have higher capacities and potentially similar molten states.
- Good plan to optimize the MSP process and determine its limits. Would help to have a go/no-go decision point on the alanates. When to move on?
- Still too new.
- The program should be supported but the progress and effectiveness of the technique must be periodically gauged against other competing methodology. When additional data are available within the next 12 months, the project scopes can be revisited.
- The addition of cycling test in the next research period may help in the material selection.
- Need to soon move to materials with higher gravimetric and volumetric hydrogen densities (e.g., borohydrides, amides and AlH_3). The borohydrides may have some potential for MSP.

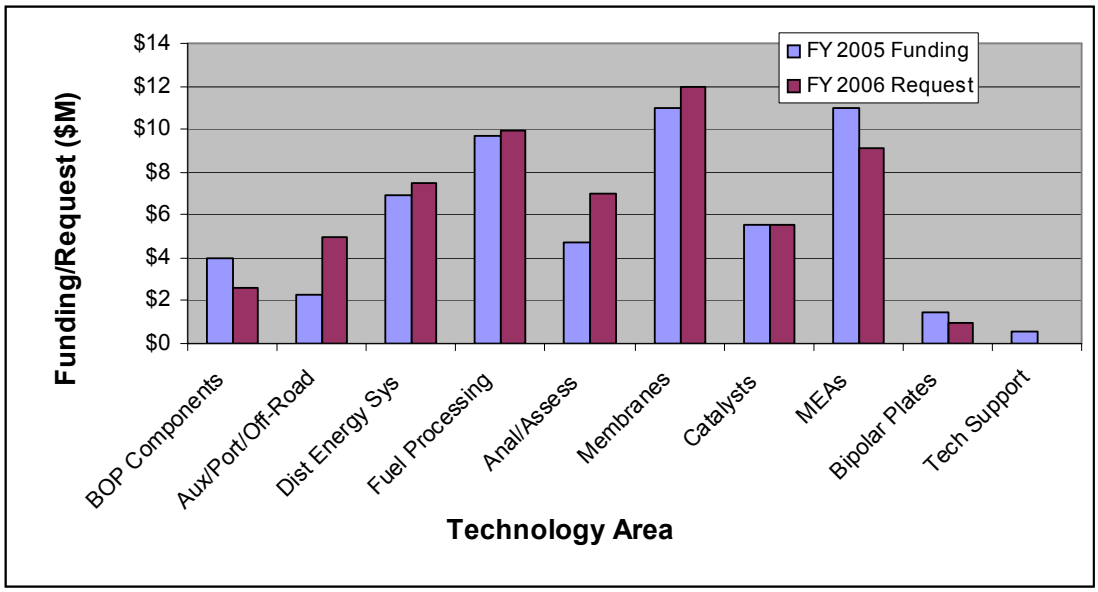
Fuel Cells
Summary of Annual Merit Review Fuel Cells Subprogram

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. As in 2004, reviewers emphasized the use of go/no-go decisions as part of the projects' work plans. Reviewers also requested that presenters show progress against the targets as they are identified in the Multi-Year Research, Development, and Demonstration Plan. Another recurring comment was that limited disclosure of technical data for some projects made evaluation a challenge.

Fuel Cell Funding by Technology:

The Fuel Cell Technology Subprogram continues to refocus in response to the National Research Council's recommendation to increase government funding on R&D dedicated to the research on breakthroughs in fuel cell materials for durability. Cost and durability of stack components i.e. membranes, catalysts, bipolar plates, membrane electrode assemblies, etc. 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 3.85, 2.93 and 1.91 for the highest, average and lowest scores respectively. The scores are indicative of 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.

- **Membranes –** Interest continues in developing membranes that operate at higher temperatures and lower relative humidity. Improved understanding of degradation mechanisms, cyclic stresses, and durability are needed. The developers of innovative membrane concepts need to partner with industry to implement the new technologies. Additionally, membrane cost needs to be considered throughout the life of the development. Acid loss and degradation in acid-based membrane systems for high temperature applications has not been adequately addressed.

- **Catalysts –** The research in the catalyst area is well-balanced between fundamental understanding and development of new catalyst materials. Researchers report promising results for low platinum catalysts. Non-platinum catalyst research remains challenging. Researchers are encouraged to perform rotating ring disk electrode testing and compare all results against the DOE targets for electrocatalysts. Developments in non-carbon catalyst supports, i.e. ceramics and bimetallics, are encouraging and should continue.
- **MEAs -** Reviewers commented that Principal Investigators (PIs) should better integrate theory and experimentation, should collaborate more with fuel cell companies, and should publish more of their work. Reviewers also stated the importance of research protocols that incorporate cyclic test conditions and down-selection steps for manufacturability. Nanoscale pre- and post-mortem in-situ analysis of MEA components will be key to a better understanding of a fuel cell's failure mechanisms.
- **Bipolar Plates –** The Porvair carbon plate project met all of DOE's technical targets and assuming sufficient volumes the cost targets can be met. A 20-cell stack was demonstrated with performance equal or better to machined graphite. ORNL, NREL and PNNL are looking at metal plate surface modification, corrosion protection, and cladding. These projects are too early in the research stages to determine if they will show promising results. Cost-effectiveness of proposed approaches were questioned by reviewers. ORNL has good initial results with Ni-Cr and should focus on more cost-effective Fe-Cr next year.
- **Characterization, Evaluation and Analysis –** The analysis activities are providing valuable insights into fuel cell operation, especially characterizing behavior that leads to performance decay and failure. Fuel cell manufacturers need to supply more experimental data to validate the models, and the PIs need to make the results of their work publicly available. More emphasis is needed on post-mortem analysis.
- **Distributed Energy Systems –** The key benefit of the distributed energy fuel cell activity is in cell stack assembly development and testing where improvement in stack durability and low cost components can be applied to all fuel cell development efforts. DOE should focus on the development of small prototype systems. Only after these projects meet scale-up milestones on system operation, efficiency, and durability should the building of full-scale systems be considered. If performed, economic and life-cycle analysis should be coordinate very closely with Original Equipment Manufacturers.
- **Portable Power –** Reviewers stated that portable technology is not applicable to transportation applications (the key focus of the Hydrogen Program) and should be de-emphasized. Work on DMFC technology, if continued, should focus on membrane and MEA development, specifically addressing durability, performance gains, and ability to function in different methanol concentrations.
- **Auxiliary Power –** The development of auxiliary power units for heavy duty vehicles has a weak tie to the overall Hydrogen Program goals. Concern remains over the ability of the ceramics within a solid oxide fuel cell system to withstand the vibration of a heavy duty truck. The projects in this area should be better coordinated to avoid duplication.
- **Fuel Processor R&D –** The fuel processor R&D area remains in a state of transition as the on-board activities are coming to completion in FY05. With the refocus from reforming gasoline for transportation applications to reforming natural gas for stationary applications, the program needs to work with stakeholders from the stationary fuel cell community to define more specific goals and targets for the fuel processor R&D activities. Specifically, more focus should be placed on durability and cost.
- **Balance of Plant Components –** Development of compressors for fuel cell systems is essential as long as there is a need for pressurized stack operation. Cost, efficiency, and durability of compressors being developed still need to be addressed. The sensor projects with industry are concluding this year, and a limited amount of laboratory R&D in the sensor development area will continue. Projects should culminate with a relevant database of chemical and physical sensors available to fuel cell system developers. Given the Program fuel processing re-direction, the majority of remaining work should focus on developing relative humidity sensors for transportation and sensors specific to stationary system performance requirements. Thermal and water management is essential to compact, reliable, durable fuel cell power systems. Several humidification designs were selected (e.g., enthalpy wheel, membrane humidifier) and heat exchangers are being evaluated. Multiple paths are appropriate for down-selection. A better quantification of effects of system variables (such as relative humidity and operating temperature) and of the environment (such as freezing ambient temperatures, shock, and vibration) on the component design requirements would be beneficial.

Project # FC-01: Fuel Cell R&D
Lightner, Valri; U.S. Department of Energy

Brief Summary of Sub-Program

The purpose of this Fuel Cells sub-program overview was to describe sub-program objectives/targets, budgets, barriers, and approach to R&D. The presentation also highlighted technical accomplishments and collaborations, and it described subprogram future directions including the upcoming solicitation. As such, this overview set the stage and put into context the R&D and analysis projects presented in this sub-program area during the Annual Merit Review.

Degree to which the Sub-Program area was adequately covered and/or summarized

- The Team Lead covered the program adequately, including an outline of who was funded, the breakdown of funds, and key technical achievements.
- The team lead gave a good summary for the short time alloted for the presentation.
- The presentation provided a very good summary of the program, an excellent transition between current and future goals, and identification of areas of future work.
- The team leader gave a few examples of results rather than overview of whole program.
- Activities in fuel cell area were described completely and efficiently.
- Barriers and targets were clearly presented for all fuel cell applications.
- Changes were explained for focus and targets.
- Progress highlights were covered.
- The presentation covered good balance of highlights and goals.
- The team leader provided an excellent general overview of program.
- The issues were adequately tabulated but not prioritized. Some of the description had been covered in the plenary sessions and, therefore, was redundant.
- An excellent summary of main FY04 achievements was provided.
- One slide dedicated to Stationary/Distributed Energy Systems would have helped to balance presentation's focus on transportation applications.
- The sub-program area is broad and diverse and difficult to handle in a short briefing period. Thus the focus was on broader sub-program goals such as cost and schedule rather than technical details of barriers.
- The Team Lead very effectively and comprehensively covered the challenges, targets, and timing.
- It was a good summary.
- The sub-program area was covered as completely as possible in the time frame allotted.
- The presentation provided a clear idea of the scope of the sub-program.
- The team leader did a good job, with few statements susceptible to technical quibbling and no outright technical misstatements (not easy to do).

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- The Team Lead discussed key issues for a successful future.
- The Team Lead was unclear on future solutions to solve key issues.
- Solicitation announcements do not satisfy the future plans of the DOE funded projects.
- Multiple areas were addressed -- selected topic presented in more detail.
- Yes, critical challenges are identified and addressed through the next round of funding.
- It is not obvious that basic research is aware of necessary details of critical fuel cell problems.
- Critical challenges were described well -- status versus goals. Key recent accomplishments and progress toward goals were well summarized. A major solicitation was described, but more details around future funding would have been useful.
- Issues of cost and durability were discussed in detail, and activites in these areas were listed.
- Future go/no-go decision points were described.
- Yes; budget zero balance was addressed, with lessons learned from a year of functioning without funds.

- Issues/targets were adequately discussed.
- The presentation lacks information on plans – not enough details other than new solicitation.
- Issues/problems were identified but not really discussed enough (hard to do in such a short time). It would be helpful to provide values for priorities of issues and rationale for why. Plans for future would be helped by this information.
- The analysis of cost estimates for volume production (status versus target) is very useful.
- Important conclusions from cost estimates were drawn: highest need (opportunity) for cost reduction for transportation PEMFC to meet 2015 targets lies in MEA, stack and, ultimately, system.
- As mentioned above, the overall view from a program management perspective was the focus of the presentation. It did not get into the technical details that are the bottlenecks in achieving these program goals.
- The challenges were well identified. It was good to have 'Hydrogen Quality' included, since this aspect has been overlooked in the past and could be a significant economic barrier to commercialization. Please include 'Durability' in gap (status versus target) for transportation chart.
- The challenges and barriers were well identified.
- Challenges are well defined and focused. This sub-program clearly has benefited from a consistent and long-term effort in planning and prioritizing with input from industry and researchers. This program benefits from consensus building, and the challenges and plans reflect this consensus.
- The issues needing improvements and the approaches for addressing the problems were appropriately presented.
- Yes; future directions and administrative mechanisms towards moving in those directions were properly covered.

Does the Sub-Program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Program seems to be managed very well by the Team Leader.
- Key targets have been met thus far.
- Detailed time-line was shown – briefly.
- There is adequate focus in area where multiple approaches have to be evaluated at early stages.
- Yes, critical needs are effectively addressed and managed in a focused and forward looking way.
- The Team Leader stressed the need for basic research actvities to coordinate with applied technology development.
- Yes; the only area that might need further coordination is the effect of higher purity requirement for hydrogen use in fuel cells on potential hydrogen cost.
- The team leader demonstrated a focused program but with the ability to be flexible and change as needed.
- The sub-program appears well focused, but ambitious cost targets (slide 6) require breakthrough technologies. Where are they?
- Yes, the subprogram is well managed -- addressing needs of ’05 multiyear plan.
- The area appears to be focused, but management and program effectiveness is not so strong. It is not clear from the presentation how the DOE management team works to help the program achieve the goals once they have been set and, more importantly, how the program should respond to changes in the goals that may arise from the progress made.
- The presentation showed a good refinement of technical targets. The projects focused on individual stack components are an especially good idea and are yielding significant progress in very challenging areas.
- The individual projects do address the needs of the program in various areas.
- No problems here -- the program is well-balanced and targeted.
- Many parallel project efforts cover the areas as well.
- This sub-program area is very focused and the management has been very effective as demonstrated by technology progress in the last several years. Current management appears to be building on the successful model used to date, while adding additional management assessments and formal criteria to ensure effective use of resources and project execution.
- The sub-program appears to be in the right direction. No other ideas.

- Yes it does, within the higher-level policy constraints that expend some resources in areas (SOFC's, DMFC's) that provide no real benefits to the advancement of PEMFC technology. And, given the reality of federal programs in those irrelevant areas, it is probably more efficient to administer these under the FreedomCAR fuel cell program than to set up another administrative structure to maintain subject-area purity.

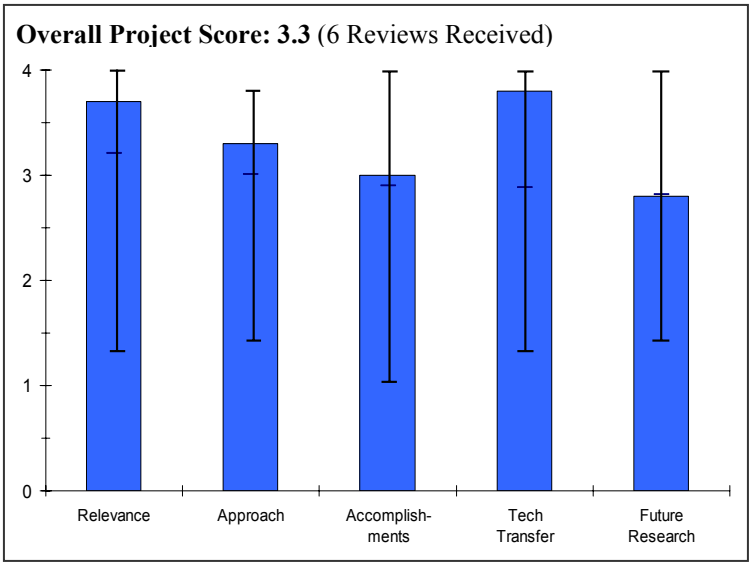
Other comments:

- The Team Leader did not explain why technical targets were changed.
- The Team Leader should have shown cost progress pre-2004.
- The Team Leader did not comment on what technical achievement would get fuel cells over the hump.
- The presentation was clearly professional.
- Focus on components is not needed – need to focus more on whole MEA system to meet specifications. Changes to any single component are unlikely to be sufficient to meet 2010 specifications.
- The presentation had well-illustrated slides with nice collage of pictures on first two slides. However, since only fuel cell vehicles are shown, one could be led to believe that this program is exclusively for transportation applications. Suggestion is to include pictures from other applications in the future
- The presentation was clear and very well delivered.
- A management-style summary of technical issues encountered in sub-program areas that highlight the tough technical challenges would help focus the tenor of the meeting into a critical problem solving mode and, thereby, might lead to the airing of new ideals and approaches to addressing the critical bottlenecks.
- The 5,000 hour durability target should be qualified as being achieved under realistic application load (power) profiles (simulated drive cycles) and not just constant power. Cost reductions achieved through reduced Pt catalyst loading must not be at the expense of durability -- Team Leader needs to verify this.
- Longer time allotment for this kind of presentation may help give more details of the sub-program tasks.
- Team Lead got the job done quickly and competently, then turned the stage over to the contractors, to whom the show belonged. What more could one ask?

Project # FC-02: Integrated Manufacturing for Advanced MEAs
DeCastro, Emory; De Nora North America

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 0.4A/cm^2 at 0.8V or 0.1A/cm^2 at 0.85V operating on hydrogen/air with precious metal loadings of 0.3mg/cm^2 and are amenable to mass manufacturing technology. Advances from this work as well as from development of a membrane which operates at 120ºC and 25% RH will be integrated into pilot manufacturing, aimed at delivering stack scale components and testing these components at the stack scale.



Question 1: Relevance to overall DOE objectives

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

- Reduced Pt loading and low RH conductivity are key to objectives.
- Both high-temperature membranes and reduced catalyst loading are critical to PEM fuel cell commercialization.
- Big program. Catalyst work seems very good.
- Strong project team.
- Reduction in precious metal (PM) loading significant.
- Durability questions need to be addressed in parallel with development activities.
- Project supports President's Hydrogen Fuel Initiative.
- Project appears to be repeating information in literature.

Question 2: Approach to performing the research and development

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

- Coordination of Pt loading, gas diffusion layer (GDL) and HT membrane developments could be improved.
- De Nora has assembled a team and defined a logical program which substantially addresses DOE's technical barriers. The program is integrated with other PEM fuel cell efforts at De Nora and at its subcontractors.
- How is OCV stability at 100°C, 25% RH, H_2/O_2? OCV tests are more direct than "Fenton."
- Solid focused approach.
- Good utilization of team members.
- Project addresses very difficult issues, low cost catalyst electrodes and high temperature membranes capable of operating at sub-ambient temperatures as well.
- Project scaling up platinum catalyst production.
- Project developing analytical method for evaluating wet-ability of materials.
- Ion deposition approach appears creative.

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

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

- Ion Beam Assisted Deposition (IBAD) process may not be efficient enough for use with large scale fuel cell production.
- Good progress in developing GDLs for optimum water management.
- DeNora recognizes the importance of hydrophobicity and has identified techniques for accurate measurement.
- Platinum content has been lowered and automated bench-scale MEA fabrication processes have been developed.
- BA candidate plot very confusing.
- High temperature work is promising.
- Project scheduled to conclude in one year. No mention of integration of material into larger pilot scale manufacturing.
- Cost issues of high temperature membranes not addressed.
- Extensive discussion about influence of Teflon on performance is old technology. Well understood and patented long ago.
- Catalyst loading of 1mg/cm^2 total was not an improvement. Not sure why Pt/Cr was picked.
- IBAD process looks very promising. Good results, but they rediscovered Pt/Co.
- HT membrane appears to be making progress but strong dependence on RH observed.
- Membrane V appears to have too large a dependence on RH to be acceptable yet claimed as successful research.

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

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

- Team has diverse and broad capabilities.
- Exceptional degree of collaboration.
- Team members include Nuvera Fuel Cells, Dupont, Case Western Reserve University, and Northeastern University.
- Work is being disseminated through significant number of presentations/publications within the last year from most of the partners involved.
- Partners are full participants in the work.
- Good coordination and assignments of task.
- Good partnerships identified.
- Publishing and reporting data excellent.

Question 5: Approach to and relevance of proposed future research

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

- Need to focus on optimized electrodes/GDL for HT/Low RH membranes.
- The general direction is reasonable but specific steps were not sufficiently described.
- Good to see modeling proposed as structure design guide.
- Need to realistically assess if IBAD process lends itself to commercial process or is it limited by batch ability.
- A large number of potential directions have developed in this work. Don't loose your focus trying to equally address all areas.
- Program is correct on identifying durability data for catalysts.
- Good planning for BA membrane.
- No real approach detailed on AE membrane.
- Not clear why so much emphasis on development of hydrophobicity techniques. Methods exist in literature.

Strengths and weaknesses

Strengths
- Wide range of talents to attack problems. Broad scope of objectives.
- Very strong team.
- Overall excellent project.
- Extremely strong team with complimentary skills.
- Project coordination implemented very well.
- Multiple potentially successful projects (High temperature membrane, Low PM loading (IBAD)).
- Good team.

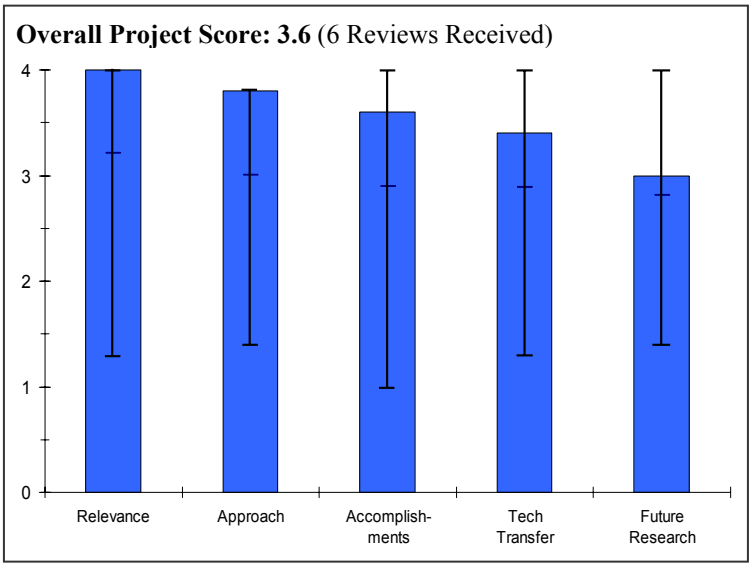
Weaknesses
- Lack of focus on optimizing system.
- The cost of the IBAD approach was not discussed.
- Hard to judge integrated progress.
- Be careful not to dilute efforts by loosing focus.
- More durability (catalyst and membrane) information would be useful.
- More catalyst stability information needed.
- Too many activities to report in such a short time. Additional reporting time needed.
- Literature appears to be ignored regarding hydrophobicity testing and evaluations.

Specific recommendations and additions or deletions to the work scope

- What are membrane choices for downselect to be made?
- Prioritize to ensure continued focus.
- Recommend serious focus on transition to continuous IBAD coating. This will be a major step toward determining commercialization potential.
- Continue with membrane durability studies.
- May want to consider accelerated membrane testing.
- Characterization of water retention properties of BA membrane is needed.
- Strong positive results for IBAD would indicate catalyst emphasis should focus on this material.
- Results on membrane V do not appear to warrant scale up.

Project # FC-03: Advanced MEAs for Enhanced Operating Conditions, Amenable to High Volume Manufacture
Debe, Mark; 3M

Brief Summary of Project



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 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 next generation, thin film, ultra-thin layer catalyst electrodes [nanostructured thin film]; optimize perfluorinated sulfonic acid based ionomers modified for enhanced durability at low RH; match MEA components for enhanced performance under demanding conditions; utilize roll-good fabrication processes for lower cost) and to develop membranes for operation in the range of 120 < T < 150ºC (new proton exchange membranes that do not rely solely on standard modes of aqueous proton conduction; understanding relationships between materials, proton conductivity, T and RH; and screening materials and fabrication processes).

Question 1: Relevance to overall DOE objectives

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

- The project addresses key technological issues related to the PEM fuel cell, aiming at the development of a more efficient and temperature resistant membrane electrode assembly (MEA).
- The project also addresses PEM fuel cell cost reduction issues with the reduction of the amount of platinum in the MEA.
- This project is aligned with the goals of the HFCIT RD&D plan and addresses major cost factors (catalysts and membranes) and performance limitations.
- The progress made in this program is indicative of the strong technical team.
- High-volume production of high-performing MEA's is key to achieving fuel cell cost, durability and performance targets.
- The Relevance of the work is outstanding and if successful will lead to success.

Question 2: Approach to performing the research and development

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

- The project is well designed, and well developed.
- This project is focused on key technical barriers for improving performance and lifetime.
- Integration of catalyst and ME.
- 3M and its team thoroughly understand the technology and have formulated an approach which should be successful.
- Progress shown is impressive.
- Approach has proven to be effective.

- The approaches to Tasks 1 & 3 are outstanding. The approach to Task 2 is limited and seems to be an afterthought. Task 2 needs to be better integrated in the overall program

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

This project was rated **3.6** based on accomplishments.

- The project shows excellent results: the developed membrane withstands higher temperatures, has a larger lifetime, no loss of conductivity when compared to Nafion, and has been produced in pilot scale (1000 ft).
- Very impressive improvements in catalysts.
- Reduced the Pt loading to 0.12 mg/cm^2 total -exceeding 2015 target.
- Have improved catalyst durability.
- Appear to be very close to achieving target of 0.2gPt/kW (rated).
- Good improvements in membrane durability.
- Membrane conductivity at low RH needs improvement.
- 3M and its team have followed a well planned development plan that covers critical areas and includes parallel approaches where needed.
- Accomplishments are outstanding.
- This project is an example of a successful industry project.
- The accomplishments in Tasks 1 & 3 are outstanding. The accomplishments in Task 2 are disappointing. More needs to be done in this area. The testing at (0^{0}C and low humidity shows great promise but one would like to see more extensive testing at 120Ct to determine if stability issues and other unexpected durability issues arise

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

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

- The project is being developed with a significant number of partners.
- The amount of papers published and presentations made so far are impressing.
- Good collaborations with groups looking at the fundamental science aspects.
- Could improve collaborations with end-users (fuel cell manufacturers).
- Excellent technology transfer to all partners is very evident.
- University, laboratory and company partners all seem to be contributing.
- There are excellent collaborations involved. However, some of the roles for these collaborations are not very clear- difficult to cover everything in the presentation.

Question 5: Approach to and relevance of proposed future research

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

- Future tasks are strongly relevant to the project goals.
- Membrane work for 85<T<120º C should be continued before MEA testing is started. Membranes evaluated to date under low RH conditions have low conductivity.
- It appears the program appears to be on well planned path toward success.
- Project ends this fiscal year.
- Tasks 1&3 look good. Task 2 seems vague, particularly with respect to the behaviors of the membrane materials in the MEAs and how they will interact with the electrode surfaces. Some nasty surprises may be in store there.

Strengths and weaknesses

Strengths
- The project planning and designing.
- Technology transfer and collaboration.
- Technical accomplishments.

- Catalyst work has shown great improvements in reducing loading and improving durability.
- Membrane work has led to understanding of degradation mechanism and improvements in lifetime.
- Excellent approach to the whole system and demonstrates ability to go to manufacturing.

Weaknesses

- Not identified.
- Membrane performance at low RH needs improvement. From the graphs shown, 3M membrane has lower conductivity at low RH than standard PFSA. At 60%RH, 3M membrane has a lambda of ~3-3.5 and a conductivity of ~ 0.01 S/cm, the standard PFSA has a lambda of ~5 and a conductivity of ~0.02. It is only after the lambda value increases above 5 (at an RH >80%) that the 3M membrane has an advantage over standard PFSA in terms of conductivity.
- May need to incorporate additives or look at other membranes to obtain good low RH performance in this temperature range.
- The high temperature work is very exploratory and should be much further along.

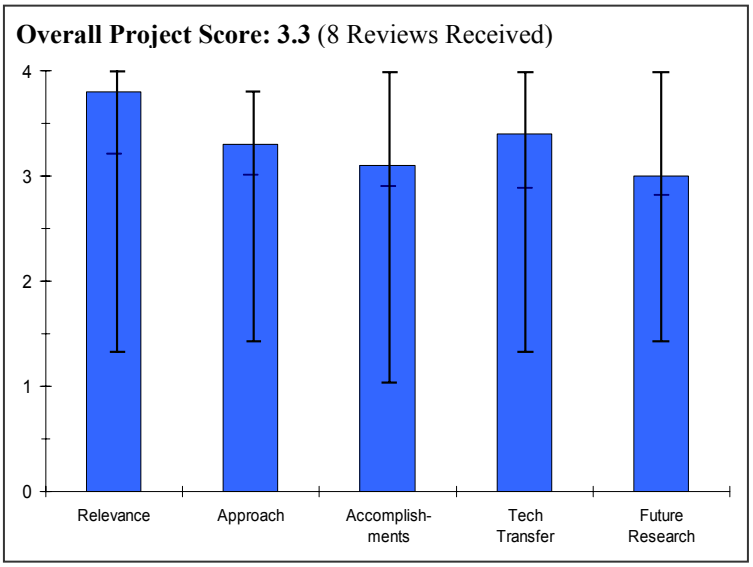
Specific recommendations and additions or deletions to the work scope

- The project could include the technique of Scanning Electrochemical Microscopy (SECM) to study the electrochemical reactions at the electrodes/electrolyte interface.
- Look for new membrane options for low RH operation in the 85-120^{0}C temperature range.
- Strong team that will make additional contributions in the future.

Project # FC-04: Development of High Temperature Membranes and Improved Cathode Catalysts for PEM Fuel Cells

Protsailo, Lesia; United Technologies Corporation

Brief Summary of Project



In the area of high temperature operation, United Technologies Corp Fuel Cells (UTCFC) is assessing and optimizing fuel cell materials that define and influence performance and durability of PEM fuel cells at operating conditions of 100-120^{0}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 alloys fabrication procedures are being developed; catalysts are fabricated and electrodes in membrane electrode assemblies are optimized. Performance and durability of these catalysts are evaluated through the use of ex-situ techniques (Rotating disc Electrode (RDE), liquid half-cell tests) and in-situ fuel cell tests.

Question 1: Relevance to overall DOE objectives

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

- Power density improvement and lower cost.
- The catalyst stability is addressed very well - this is critical to durability and electrode stability DOE goal.
- The project is of great interest for reducing costs and improving long-term durability. Both issues are important for commercialization.
- The increase of operation temperature and improving the oxygen reduction kinetics are important steps to increase the fuel cell efficiency.
- Catalyst activity and voltage-cycling durability work is exactly the kind of experimentation that has been needed to make a path to durable low-Pt systems plausible. High-temperature catalyst durability is a helpful side benefit. Low-humidity, high-temp work has been well-targeted but, as yet, has not been as successful.
- Shows results on hydrogen/oxygen primarily;

Question 2: Approach to performing the research and development

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

- Many partners but interactions need to be more clearly defined. Need a better plan for integrating disparate elements into future activities.
- Electrode stability studies outstanding.
- Membrane durability studies weak - the new materials are interesting, but the data showing durability is not nearly as strong as the electrode work.
- Multi-component cathode catalysts have the potential to substantially lower the amount of Pt in, and lower the cost of, the catalyst.

- Collaborating with universities to obtain fundamental understanding of high-temperature polymers should be very helpful.
- Durability study seemed very limited in scope considering complexity and importance of issues. Results on Pt dissolution are inconsistent with those reported by others. Determination of the baseline for durability of current membranes at low RH does not seem adequate.
- Difficulties of fuel cell startup due to low membrane conductivity have to be evaluated.
- For the catalysts, very nicely thought-out and executed experiments. The initial university work brought in a good diversity of approaches, and down-selects were made intelligently. The shift to a less-positive upper potential limit for the 120°C cycling work, while perhaps logically inconsistent, was probably the practical thing to do.
- The Rotating Ring-disc Electrode (RRDE) peroxide results in TetrafluoroMethane Sulfonic Acid (TFMSA) needs further explanation. Peroxide usually peaks at lower potentials than shown, even in other weakly-adsorbed acids such as perchloric.
- High-temperature membrane work was also well-planned; it just hasn't panned out as well.
- Why was the membrane portion strongly de-emphasized?
- Cathode work is high quality, with solid focus on operating fundamentals, durability.
- Low RH is not the same as high acid concentration in solution. It is unclear whether any conclusions from the latter will reflect reality of the former.

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

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

- The PtCo and PtIrCo work is clearly industry leading.
- The membrane developments are not as strong, but overall, quite reasonable.
- The Pt-Ir-Co catalyst appears to be very promising, based on the activity and stability data presented.
- Both approaches to high-temperature membranes are showing promise.
- The failure of Nafion membranes at low RH needs to be better understood because it will impact development of alternative membranes, e.g. why will polymer X have greater stability than Nafion if you don't know why Nafion fails.
- The reasons for the improved performance of PtCo/C were not discussed.
- Good Progress.
- Catalysts: The alloy systems studied here are perhaps not the most imaginative or innovative in the world, but this work gives a more solid demonstration of simultaneously-improved activity and durability than had been previously publicly reported.
- Membranes: Results have been perhaps a bit disappointing, but the demonstration of improved durability of Sulfonated Biphenyl Sulfones (BPSH) through lower oxygen permeability and therefore decreased peroxide is a nice piece of work.

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

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

- Gives appearance of several smaller projects each making progress but limited collaboration; will catalyst and membrane work together and be compatible?
- Who will commercialize MEA they develop?
- The contractor is a commercial developer. Close collaboration with their own research center and the three universities can enable rapid implementation of advances as they occur.
- Close interaction between universities and a major developer.
- Need to establish collaboration with membrane OEMs.
- None of the partners listed has the expertise of mass manufacturing of membranes.
- Much of this work started with input from universities. The best parts of that work were taken in to the industrial part of this program. This project appears to have worked as a seed that has stimulated other industrial

interest in some of the Va. Tech membrane work. It also has helped to stimulate catalyst manufacturers to work harder on bringing Pt alloys to a fully-commercial state.
- Solid Collaboration.
- Need to involve catalyst makers, MEA makers in industry.

Question 5: Approach to and relevance of proposed future research

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

- Not clear how all results will be integrated and meaningful data obtained in next 6 months.
- The plan is reasonable relative to understanding and development of durable MEA, but I would like to see more interactions with MEA suppliers. Otherwise these promising new MEAs may not reach large scale commercial realization.
- Testing of the new catalysts in full-size cells and in a stack will be complemented by fundamental studies of catalyst stability and membrane durability.
- Continuing work in high-temperature membrane development is mostly experimental demonstrations.
- Use of surface and bulk analytical techniques may help to understand the PtCo/C catalytic activity.
- Should spend a little time addressing manufacturability and costs. This is where a membrane partner will greatly contribute.
- Future work is well planned, though since the project still has little time to go, the scope is a bit limited.
- No clear path provided to achieve improved MEAs for high T or to lower loading.
- Durability focus reasonable.

Strengths and weaknesses

Strengths
- Basic work being performed by wide array of qualified participants.
- Strong team that uses strengths of individual subcontractors/partners very effectively.
- A good blend of fundamental understanding (e.g. catalyst stability, peroxide generation, etc.) and experimental work to develop new approaches to solve durability issues.
- UTC Fuel Cells has an excellent depth and breadth of experience in fuel cell development, and in addressing issues of catalyst activity and stability.
- They have teamed up with well-respected academic groups for both the cathode catalyst and high-temperature membrane work.
- Activity and stability of the PtCo/C catalysts.
- Good progress.
- Good use of federal money to survey technical developments in two fields, drawing the best from universities, and subject them to hardheaded, relevant, industrial testing. Subsequent good industrial development to solidly establish which of those technologies are actually useful. Good response to priorities that changed a bit over time. Good communication of results. Good mid-course correction to avoid money wasted on too-large-scale demonstration of materials that weren't yet ready. Short-stack testing of the more mature alloy catalysts should give the DOE program a bit firmer base for program decisions.
- Good team.
- Important accomplishments in catalyst durability.

Weaknesses
- Specific program targets undefined other than "improved". Role of UTC not clear. Have any specific milestones been accomplished? I.e. has the membrane been selected for Phase 2? Has the optimum catalyst-membrane combination for phase 3 been identified? Greater clarity on status and metrics required.
- Who/how will successful results/developments be commercialized given that neither UTRC nor any of its partners in this program make MEAs?
- The team has strong polymer chemists, but is weaker in testing and understanding of membrane durability and the development of fundamentals that lead to membrane degradation.

- Although the Pt-Ir-Co catalyst material is referred to as an alloy, is it really so? Is alloying needed for the shown activity and stability?
- Only one catalyst composition was presented and discussed - is this the result of some optimization, or is some optimization planned in future work?
- In the high-temperature membrane work, the results presented are not encouraging for successful operation under essentially dry conditions.
- Fundamental techniques for material characterization seem to have been given a low priority.
- Absence of critical analysis of the chance of success of the new membranes.
- Lack membrane supplier/manufacturers involvement.
- Early settling for 50% RH at 120C for membrane work, since apparently corrected. Membrane results to date have been a bit disappointing. More could have been done with improvements through modified catalyst supports.
- No real path to lowered catalyst loading.
- Need to adjust for low RH; membrane approach does not tend in t hat direction.

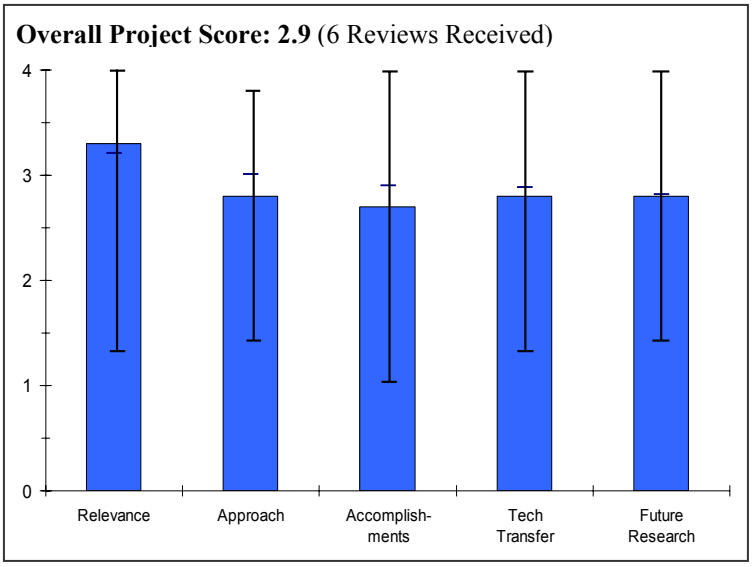
Specific recommendations and additions or deletions to the work scope

- Scope is fine but need further definition of Program Metrics. What constitutes success? All results should be incorporated in stack test and full results given including stoichiometry operating conditions, etc to make project beneficial throughout industry and not just to project participants. Fundamental understandings should be broadly disseminated.
- Develop/present plan for how developed MEA technology will be shared in the industry, or if it won't, how the team is planning to commercial them.
- Is anode CO tolerance still a goal for this project? If so, should the durability target for the high-temperature membrane be extended to 40,000 hours?
- Will they be providing the improved cathode catalyst to other developers and researchers for testing and evaluation?
- Need to establish collaborations with membrane suppliers/manufacturers.
- If there is a follow-up project, more attention should be given to corrosion-resistant supports.
- Should transfer findings to industry from universities.

Project # FC-05: Electrocatalyst Supports and Electrode Structures
Wilson, Mahlon; Los Alamos National Laboratory

Brief Summary of Project

Work on this task will build upon the ongoing collaboration with Brookhaven National Laboratory (BNL) on testing low-Pt content catalysts, but will primarily focus on investigating alternative low-cost supports. A secondary goal in developing new support materials is mitigating corrosion (i.e., carbon corrosion). Certain aspects of these new materials may enable the development of electrode structures that further improve catalyst layer performance and manufacturability. This year, LANL is also initiating an atomistic-level modeling effort to guide the development of these new supports.



Question 1: Relevance to overall DOE objectives

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

- Project is mostly aligned.
- Durability and reduction of Pt are important critical aspects of the R&D plan and are addressed in the program.
- Reducing Pt usage is certainly relevant. Project very limited in scope.
- So-called testing function, which appears to be a significant part of the effort, is weakly coupled to R&D Plan objectives.
- Improving the electrode structure and/or the catalyst properties will help improve the fuel cell efficiency.

Question 2: Approach to performing the research and development

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

- Project is heterogeneous: Testing catalyst for durability has little to do with the support development.
- Pt, Pd and Ru are all fairly expensive and therefore reduction in Pt content does not lead to cost reduction if substituted with Pd, Rh, etc. The approach using Ni, Au (example given) is more promising.
- Focus on support development is novel. Lack of extensive analytical characterization may limit understanding and progress (e.g. microstructural characterization of surface area changes, H_2O_2 generation).
- What value is added by LANL in this so-called catalyst testing? What is the approach to developing alternative supports to carbon? None described that I can see.
- Excellent approach and attitude toward technical barriers and reevaluation.
- Characterization of the conductivity of the ceramic material is crucial.

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

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

- While 3000 hours of BNL catalyst testing is impressive, the performance of the catalyst is rather inadequate; less than 0.6V at 0.6 A. Not in agreement with high RDE activity presented by BNL.
- Good progress demonstrated in relatively short time.

- Good progress for relatively new project. Need to complete analysis of BNL materials and concentrate on CS candidates.
- Testing of BNL catalyst poorly done. Test regime (constant current) not relevant to fuel cell application. Programmatic need to define durability test protocol for PEMFC the way the HEV program has done for batteries. In the meantime testing must include variable loads, start-stop and hot standby. Pure Pd catalyst should have also been used as a control as at such low potentials there is little difference between Pd and Pt. The presentation of the new "ceramic supports" was so obscure it was useless.
- Steady state polarization measurements with the CS materials are required.
- Check for temperature effect on the polarization response and materials stability.

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

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

- Better coordination with BNL would be beneficial for understanding the fuel cell performance.
- BNL and internal LANL collaboration demonstrated.
- Good collaboration with BNL.
- Where is the collaboration with a fuel cell or catalyst developer?
- Interact more outside national labs.
- Only national laboratories are involved in the project. This is all right because materials are still very far from practical applications.

Question 5: Approach to and relevance of proposed future research

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

- Study the compositional changes of BNL catalyst.
- High surface area ceramic support is innovative approach which should be pursued further.
- Need to quickly demonstrate potential CS approach under fuel cell conditions.
- Where is this project going?
- A realistic analysis of the chance of success with the CS is missing.

Strengths and weaknesses

Strengths
- The risk in pursuing the ceramic core support is worth it.
- Good exploratory project.
- Understanding of complex materials.
- Strong theoretical base to support the test results.
- Good scientific quality and potential of practical applications of the multi-layered metallic catalyst particles.

Weaknesses
- Not clear if the solutions developed are applicable for high volume manufacturing.
- Narrow focus on substrate for Pt. Lack of strong analytical support (characterization/mechanisms of degradation under fuel cell-like conditions).
- Too many to list. Fundamentally they don't know what to do.
- Lack of fundamental criteria for choosing the new support, which was not based on previous evaluations of stability, conductivity, surface area.

Specific recommendations and additions or deletions to the work scope

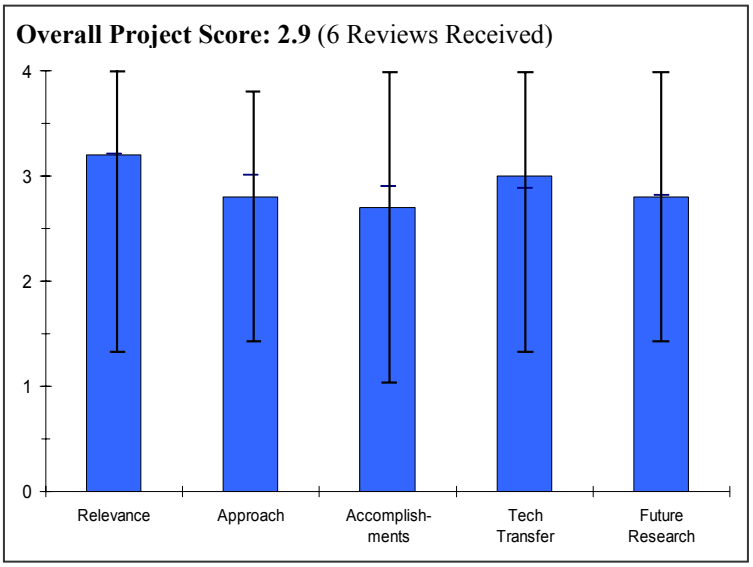
- If possible, split the project in two: durability and the support.
- Compare how ceramic supports improve durability relative to carbon supports including graphitic carbons.

Project # FC-06: Development of New Polymer Electrolytes for Operation at High Temperature and Low Relative Humidity

Zawodzinski, Tom; Case Western Reserve University

Brief Summary of Project

Case Western Reserve University is developing membranes for PEM fuel cells, targeting operation at low relative humidity and/or T>100ºC. The project includes proof-of-concept activities to find ways of achieving conduction under demanding conditions.



Question 1: Relevance to overall DOE objectives

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

- Address critical barriers to PEMFCs, especially mitigation of poisoning and improved performance at higher temperatures.
- With the drive to have on-board hydrogen storage, what is the driver for this project?
- High-temperature, low-humidity membranes are key to PEM fuel cell commercialization.
- If successful, would expect program to yield valuable information to the design of high temperature MEAs – certainly making the program aligned with goals and objectives.
- Change in emphasis to low relative humidity improved the relevance of this project.
- This work, particularly with its newly-claimed emphasis on low-humidity applications, seeks both to gain fundamental insights and, through a building-block approach, to develop improved membranes and ionomers for use in electrode layers. If these aims could be brought to fruition, they would substantially advance PEM fuel cell technology.

Question 2: Approach to performing the research and development

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

- Good breadth of synthetic approaches. Good balance of exploratory and more advanced (MEA, stack-level) studies. Good correlation of chemical and physical properties with expected device performance.
- The program is focused only on conduction to gain insight into mechanisms with the intent of discovering something which will enable a breakthrough or development of the optimum membrane.
- The program has adjusted to DOE interest in low humidity operation.
- The nature of the program leads to the appearance of a shotgun approach.
- With so many various partners, a standard stability or "performance" protocol should be enforced, such as a thermal stability test, conductivity, and Fenton-stability in order to gauge across all partners the relative strength of the approach.
- The "John Kerr" approach of designed structures to obtain theoretical answers is highly endorsed.
- Stated approach is good (e.g. seek fundamental understanding of proton-conduction mechanism), but evidence of this approach actually being the guiding focus is lacking.
- Somehow, major elements of this project seem to have taken forever to get started. It is not clear to this reviewer why this is the case. Are there problems with having one university serve as coordinator for the work of others? The basic tenet of trying things to gain understanding of fundamental properties, coming back to try to patch the durability later, is OK as long as forward momentum is maintained. But temporarily putting aside

durability to check out performance ideas is a bit dangerous, as the durability shortfalls of current material sets are probably more serious than the performance shortfalls and their thermal consequences.

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

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

- Order of magnitude improvement in conductivity for network structures important. Significant number of variables to work with for each proposed electrolyte; not clear whether progress is due to empirical luck or well thought-out science.
- This project is highly fundamental in nature and appears to have no real emphasis on testing for durability.
- Progress seems slow, partly because some of the subcontractors have received funding fairly recently.
- Some promising results were reported but there is a long way to go.
- Although just starting the program, the array of approaches appears to already shed important information on approaches and theoretical limitations.
- Impressive array of compounds synthesized and tested (as expected for the funding level), but what is the end deliverable: (a) A better fundamental understanding? If so, this is not evident - yet. (b) Materials with high conductivity at low RH? If so, this is not evident - yet. (c) Materials that meet all DOE requirements? If so, this is not evident - yet. And, not likely with very little emphasis on stability.
- It looks like some of the block copolymer and structured composite work is starting to bear fruit. Some of the other work still seems a bit like futzing around with all ideas that are plausible through one line of reasoning without enough down selecting through the application of other lines of reasoning. For example, in the imidazole work is anything being done to answer questions about poisoning of ORR kinetics on Pt by imidazole?

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

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

- Most projects in early stage, so not surprising transition not happening yet. Institutions within project could accelerate progress with better interactions.
- The program includes Foster-Miller, Mitsubishi Chemicals, two national labs, four universities, and the Naval Air Warfare Center Weapons Division.
- Ideas and results seem to be cross-pollinated between team members.
- Industrial Partners or validations were not listed.
- Obviously, the collaborations with other universities and national labs are excellent.
- However, PI claims numerous interactions with industry, but other than some samples from Mitshibushi Chemical and discussions with Dupont, it is not clear what the plan is or who are the potential industry collaborators.
- Bits and pieces of knowledge from diverse sources are being put together into a greater collection of experience that could lead to wise choices in material development. But there seem to be rough edges in even the formal collaborations within the program - why didn't a couple of contractors get in updates on their work in time for inclusion in this presentation? It's not clear that there has been a significant flow from case to industry in this project, though such contacts are working well when nominally led from industry.

Question 5: Approach to and relevance of proposed future research

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

- Lacked specific goals/milestones for each project for the coming year.
- High level of focus on low RH conductivity measurements, not enough focus on longevity/durability testing.
- Future work follows logically from the progress to date. Plausible paths forward have been identified.
- There don't seem to any off-ramps in the program to deal with dead ends.
- Additional electrochemical stability testing or simulation thereof appears to be missing.
- If fundamental understanding is the goal, then why test MEAs?

- Future focus should be establishing some hypothesis for proton conductivity in polymer electrolytes (that includes a good explanation for the critical role of hydration) and testing/validating these hypotheses.
- Plans seem good, though durability considerations shouldn't be put off for too long, as they are probably the most important considerations in the choice of technically useful membranes.

Strengths and weaknesses

Strengths
- Good focus on conduction mechanisms and understanding, without trying to solve too much (mechanical stability, long term performance) on new exploratory materials.
- Very strong teams with good experience in establishing sound motives for design, and the capability to experimentally verify strategies.
- Increased emphasis on low relative humidity and decreased emphasis on high temperature was a good change.
- Have synthesized some unique materials that offer the potential to study and understand ionic conductivity mechanisms.
- LBNL results were very good, since it addresses fundamental mechanism (although conclusion of 10-2 is not clear).
- Diffusion length scale results on BPSH also good, since it does result in some interesting hypothesis.
- Good gathering of ideas from diverse sources. Building-block approach may eventually yield important results. P.I. knows his stuff, has good technical judgment, and seems to have an inspirational enthusiasm for the work.

Weaknesses
- Complimentary approaches, but seems like each team member working solo - need to hold several team meetings each year to improve interactions. What is the roadmap for this project? Not clear what/when the endpoint is. Any downselect plans to focus effort?
- Some sort of electrochemical simulation or testing strategy needs to be added.
- Needs to establish standard tests for conductivity, thermal stability, mechanical strength, and oxidative stability (such as Fenton's) that are performed by each partner on their most promising candidates/approaches in order to be able to rank across the board.
- Despite stated intent, still not enough emphasis on fundamental understanding (e.g., what is the conductivity mechanism in polymer electrolytes and why is hydration so critical?). There are certainly a lot of polymers that have been synthesized and tested here, but not much progress towards answering the fundamental question above, even though conductivity mechanism is claimed to be the main focus. If progress on fundamental understanding is being made, it is not clear from this presentation.
- It has taken a very long time for this work to gain traction. One keeps expecting the PI, who is an excellent scientist with a clear passion for the work and lots of good ideas (and who is a top-notch communicator at times) to give a review talk that will knock the socks off the audience. But it hasn't happened yet in this program. Maybe there is too much to cover, so that work that actually has significant depth appears superficial. But a bit more attention to the flow of the talk, and a better mix of data and words on the same slide (less phase separation of word slides and data charts) would probably help. Some good stories about ideas, testing of ideas, and revised hypotheses should be distilled out of what tends to look like uncoordinated activity. This is a good project, but by all rights it should be a great project. This presentation appeared to be put together without full dedication.

Specific recommendations and additions or deletions to the work scope

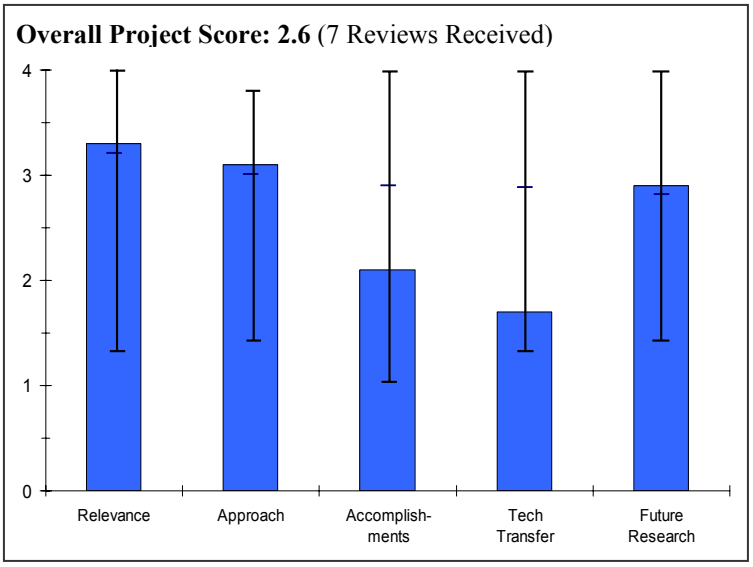
- Maybe fewer fronts would be more effective. Drop the least promising approaches.
- Develop a RDE method for simulating electrochemical environment on small samples of material derived from the program (such as casting a film of the polyelectrolyte over catalyst on the RDE tip, etc.).
- Establish standard tests that all partners perform on their most promising candidates/approaches.
- Reduce the diversity of materials being studied to a limited number of "model compounds" to uncover the key to ionic conductivity and the importance of hydration (e.g. more quality and less quantity).
- Focus on what the PI claims is the goal (namely, obtain fundamental understanding of proton conductivity) and less emphasis on actual conductivity values and/or plethora of systems. If this is being done, it is not obvious from the presentation, i.e. what have we learned about proton conductivity in polymer systems?

- Ignoring durability aspects is fine, as long as definite progress is being made on understanding proton conduction.
- Generate some strong data about conduction mechanisms so that the community can more readily buy into the idea of temporarily relaxing the noose of plausible durability. Might formal downselect dates help to keep things moving along and keep the lines of communication more open?

Project # FC-07: High-Temperature Polymer Electrolyte Membranes
Myers, Debbie; Argonne National Laboratory

Brief Summary of Project

Argonne National Laboratory (ANL) is working to develop a proton-conducting membrane electrolyte for operation at 120-150ºC and low humidities to meet DOE's technical targets. The project is investigating dendritic macromolecules attached to polymer backbones, cross-linked dendrimers, and inorganic-organic hybrids.



Question 1: Relevance to overall DOE objectives

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

- DOE Targets for conductivity, cost, durability and temperature clearly addressed.
- Project focused on working towards DOE targets.
- Fundamental long-term work.
- Focus of project very relevant.
- Good focus on high temperature and low RH membrane improvements.
- High temperature, low relative humidity membranes are vital to the long term viability of fuel cells.
- The $200/m^2 cost target seems a little high. Presumably this is not a long term cost target.

Question 2: Approach to performing the research and development

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

- Approach good dealing with dimensional stability issue, while addressing higher temperatures and conductivity
- Need to incorporate ionomer in catalyst layer as part of study - what ionomer will be used in catalyst to evaluate MEAs?
- Once temperature stability issues are solved, need to evaluate Fenton's test at higher temperatures.
- Clear directions laid out.
- Narrow but appropriate focus on dendritic macromolecules and inorganic-organic hybrids.
- Presenter did not quantify how the approach is expected to reach a 120ºC minimum operating temperature target (e.g., what fundamental material property limitations might limit maximum temperature and how does the approach address those potential limitations, quantitatively, not qualitatively).
- Design seems to focus on high water uptake membranes that must have increased dimensional instability. It is not clear that sufficient water will be retained at low RH, high temperature conditions.

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

This project was rated **2.1** based on accomplishments.

- Goals clearly identified.
- Good progress this year, but dimensional stability issue remains problematic.
- Not much progress in FY '05.
- Still no conductivity data at target conditions (120-150ºC).
- Progress vs. spending lover than many other projects.

- It is unclear how the low temperature operation is addressed.
- Even with cross links conductivity drops off at approximately 90ºC.
- 100 hour durability test is not very long.
- Comparisons of dimensional stability were made between the cross-linked membrane and benchmark membrane at different relative humidities, so meaningful comparison was difficult.
- Cross-linked membrane showed dimensional stability transition improvement from about 80ºC to about 85ºC, which is only a 5ºC improvement, whereas a >40ºC improvement in the transition is ultimately required.
- Low conductivity even at moderate temperatures and pressures. Perhaps the plan to increase the organic component in the composite will address this issue.

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

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

- Consider bringing MEA suppliers into the project.
- To date, no work outside of ANL, although interactions with GM/Giner planned.
- Appears to be research without interaction with manufacturers.
- Need to establish collaboration/interactions with membrane and MEA OEMs.
- Tech transfer is not really addressed.
- Collaboration with industry (e.g. GM) is good for verification.
- More collaboration with industry (e.g. materials manufacturing companies) may reduce the potential for "dead-end" research.

Question 5: Approach to and relevance of proposed future research

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

- PI has identified and is working on the primary challenges of this project including dimensional stability and conductivity.
- Based upon preliminary data, future plans look reasonable.
- Need more emphasis on MEA assembly.
- Needs a quicker timeline.
- Should address the low temperature operating capacity.
- Good overall approach to addressing issues identified to date.
- Pathway for future research was clear.
- Quantification of potential for dendronized polymer path would improve confidence, however.

Strengths and weaknesses

Strengths
- PI (project) is focused on DOE technical R112 good fundamental research
- Fundamental study.
- Debbie's presentation was clear and clearly articulated. Good use of data.

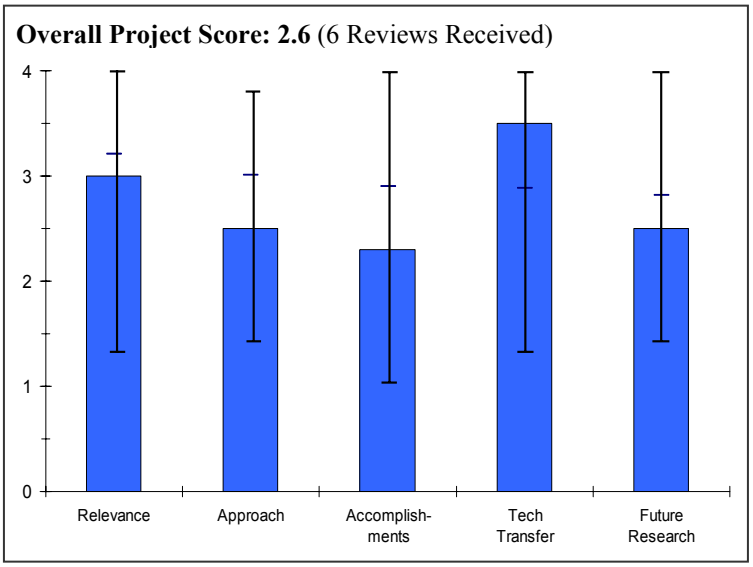
Weaknesses
- Challenges of dimensional stability still problematic.
- Slow pace.
- No MEA testing yet.
- Dimensional stability of materials.
- Low temperature stability of backbone, although plan in place to evaluate more stable materials.
- Sense of urgency is needed.
- Lack membrane supplier/manufacturers involvement.
- Need to articulate quantitatively what leads the researcher to believe the targets are ultimately achievable using the proposed approach.

Specific recommendations and additions or deletions to the work scope

- Continue testing at higher temperatures >120ºC.
- More MEA testing.
- Review of goals vs. out years with industry ties is recommended.
- Need to establish collaborations with membrane suppliers/manufacturers.
- Address the low subzero operation capacity.
- Would like to see more work done in the inorganic-organic hybrid area.

Project # FC-08: Development of Polybenzimidazole-based, High Temperature Membrane and Electrode Assemblies for Stationary and Automotive Applications
Staudt, Rhonda; 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. Over the past year, Plug Power continued the rapid screening of candidate PBI materials in 50 cm^2 MEAs; performed detailed electrochemical characterization of MEAs made with selected PBI polymers; evaluated low cost acid-absorbing materials for phosphoric acid management within the system; continued design and development of bipolar plates with PBI-specific flow fields; and initiated development of a PBI membrane-based MEA with advanced electrode structures providing high catalyst utilization and performance exceeding that of Nafion-based proton exchange membranes.

Question 1: Relevance to overall DOE objectives

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

- This program focuses on residential applications rather than transportation.
- The change in program direction to stationary only limits the benefits.
- This project would be more relevant if it was applicable to automotive stacks in addition to stationary fuel cell stacks.
- Higher temperature fuel cells, i.e., SOFC, will provide higher efficiency and higher value heat for residential applications.
- Stationary PEMFC operating at high temperature are an important near-term PEMFC application due to their better suitability for existing fuel infrastructure and carbon-containing renewable fuels (CO-tolerance).
- CHP aspect of high-temperature PEMFC systems increases their overall efficiency.
- High-temperature PEM in general is also desirable for transportation applications to facilitate thermal management.
- Near-term application has potential to accelerate cost reduction and commercialization of all (PEM) fuel cells
- Strong team.

Question 2: Approach to performing the research and development

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

- The task structure is logical.
- RPI effort is very good.
- Unanticipated system upsets that lead to water in the stack will potentially cause catastrophic loss of acid and stack failure.

- The approach addresses the acid evaporation issue at the system level rather than at the membrane level.
- Current density is not specifically addressed; it has a direct impact on stack cost.
- One Reviewer commented that there is no apparent validation of the University of South Carolina acid flow model, and he questioned whether membrane mechanical properties are being characterized.
- To minimize fuel cell costs it would be better if stationary and automotive applications shared as many components as possible.

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

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

- Initial power density of the MEA is low and no data was provided on decay with acid loss. The basis for the 40,000 hour life was not presented.
- Solution for membrane creep was not found.
- Membrane mechanical properties require more work.
- Mechanical stability is still a major hurdle that may require additional time to solve.
- Open circuit voltage should be improved.
- The presentation did not include a plot showing performance (cell voltage and cell resistance) as a function of time.
- There was minimum information on trade-offs between design choices.
- Plug Power should do a ballpark screening on cost earlier in the program.

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

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

- There is good cooperation between university and industry.
- At this stage, the partners are appropriate.
- The project has a complete range of partners from fundamental science providers to systems integrators.
- The Principle Investigator has involved external resources for the specific challenges of this program.
- Fundamental understanding and optimization of membrane structure is important.

Question 5: Approach to and relevance of proposed future research

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

- With one year in the program, fundamental problems still exist: acid evaporation, low power density, and membrane mechanical properties.
- These issues should be addressed before progressing on to the construction of a full size module.
- How is acceptable acid loss rate determined?
- What can be learnt from the PAFC literature?
- Survivability: does PBI survive crystal formation of H_3PO_4 upon cooling of stack to room temperature or below?
- How is stability of nano catalyst at 160ºC measured?
- Acid loss should be measured in full-size stack.
- There is substantial effort remaining to be completed and no indication of comprehensive cost analysis.
- If the cost analysis shows the membrane to be exorbitant, we'll know this information LATE in the project.
- A new approach to solving dimensional stability is needed.
- One reviewer commented that he would like to see data on the effect of operation at low relative humidity.

Strengths and weaknesses

Strengths

- High temperature membranes have the potential to provide CO tolerance, improved heat rejection, and higher value heat for CHP applications.
- They have a good team.
- There is good integration.
- A complete range of partners are collaborating on the project.
- It is refreshing to see a speaker show targets and status for THEIR project on a slide. It makes it much easier for reviewers to assess progress.
- Focused Ion Beam analysis of electrode 3-D porosity is a good technique.
- Acid loss appears to be manageable.

Weaknesses

- The technology has fundamental issues, and this presentation did not show interactions between these. For example, low power density was attributed to oxygen concentration limitations; however, increasing air stoichiometry with higher air flow would increase acid evaporation.
- Does membrane-creep lead to catastrophic failure of the stack or gradual loss of performance?
- The commercial supplier of the membrane should be addressing the fundamental issues with the technology, while the Principle Investigator should be focusing on implementation of the technology into a stack and system.
- There is no indicator of improvement over existing PACC stack designs; cell performance is marginal. Minimal data was provided to support technical targets table.
- No endurance data was shown, except for acid loss rate over ~5800 hours. Current density is not mentioned in that graph. However, acid loss calculation was based on 0.2 A/cm^2, which leads to the assumption that endurance tests were performed at that low current density. If this is true, and if there is indeed an issue with operating at higher current density (such as PAFC experience suggests), electrode area per kW would be significantly higher than in PFSA-based PEMFC, leading to a concern for increased stack cost. How can the current density be increased to meet requirements?
- Acid management requirement results in additional system complexity and cost. These should be quantified/estimated.
- It is not clear "extraction" problem is solved.
- The choice of Albany Nanotech for electrode modification seemed to be based on geography rather than capability.
- Lack of applicability to automotive stacks will limit cost reduction ability.

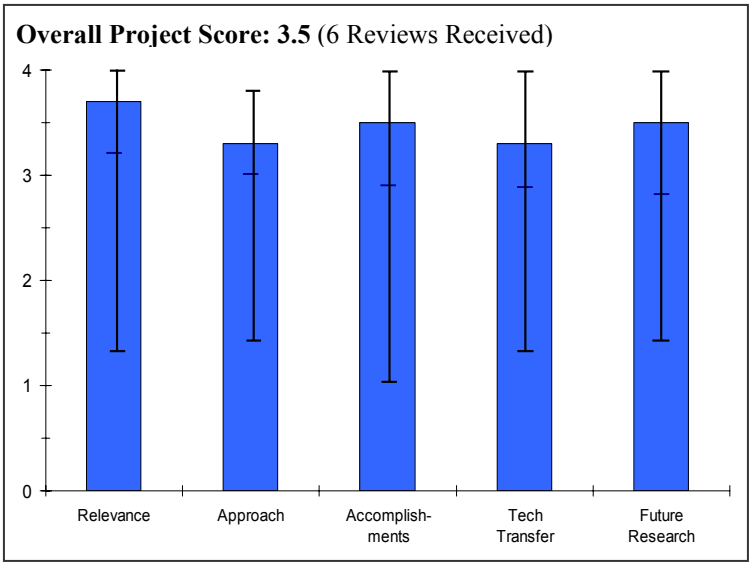
Specific recommendations and additions or deletions to the work scope

- In the balance of the project, focus should be on resolution of the fundamental issues rather than fabrication of a full size demonstration unit.
- The program needs to generate performance degradation data versus acid loss and creep of the membrane.
- Shift membrane reformulation and electrode development effort to PEMEAS, while Plug focuses on use of the membrane in a stack, i.e. flow field design and sealing.
- Include a comprehensive cost analysis.
- Include a comparison to traditional PAFC stack design.
- Suggest durability testing on (simulated) reformate.
- Sensitivity to upsets causing water condensation on membrane should be addressed.

Project # FC-09: Enabling Commercial PEM Fuel Cells with Breakthrough Lifetime Improvements
Escobedo, Gonzalo; Dupont

Brief Summary of Project

This Dupont project will utilize both experiments and modeling to develop an 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.



Question 1: Relevance to overall DOE objectives

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

- Well focused on cost and durability. Aware of need to address performance later on.
- Improved performance and durability is needed for fuel cell to meet lifetime requirements.
- PEM membrane durability and performance critical to EERE's program.
- Long term durability of 5000hrs. For vehicles uses and 40,000 for stationary have to be achieved if Hydrogen Fuel Cell Initiative is to be realized.

Question 2: Approach to performing the research and development

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

- Good combination of theory and experiment. Good balance between fundamental and cell level tests and their correlation.
- Focus of approach is improved membrane performance. Modeling and experimental approach yields balanced effort to maximize technical contribution of this project.
- Peroxide formation prevention does not seem possible within current scope.
- Alignment with automotive applications supports EERE program.
- Well thought approach to understanding peroxide attack of Nafion membranes and to develop new materials improvements that reduce membrane and edge seal degradation.

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

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

- Water influence on chain motion important to understand to improve existing materials and develop new ones. Good progress in Gen A membrane performance, with additional ideas for further improvement.
- Steady progress in membrane performance and durability.
- Excellent correlation between Fenton's test fragments and fuel cell tests.
- Excellent progress in membrane stability and identification of degradation mechanism.

- Excellent progress. New insights are being gained in membrane degradation and the role that water humidification and swelling play in this process. Membranes have been made with significantly improved mechanical strength and chemical stability with new tougher polymers and reinforced membranes. Methodology for tracking degradation- fluoride emission rate show significant 7X reduction.
- It is not clear what conditions the membranes were exposed to during all tests (RH, humidity). Is the membrane being tested under low RH, high temperature conditions?

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

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

- Complimentary team expertise and close interaction definitely accelerating this project.
- Excellent partners in this project with complimentary experience and expertise focused on membrane development.
- Dupont / UTC teaming strong collaboration, but there is a lack of publications for this size project.
- Good joint venture between a membrane supplier, Dupont, and a fuel cell maker UTC.

Question 5: Approach to and relevance of proposed future research

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

- Well defined road map and milestones. Strong iterative theory and experiment process for continued improvements.
- Testing in short stack fuel cells will quantify the improved performance of the membrane materials (and seals) developed in this project.
- Clear strategies for UTC and Dupont, unclear future collaboration with USM.
- Acceleration test and factors, and identification of chemical degradation modes likely to lead to best long-term advances.
- Critical application of degradation reduction beyond cells to short stacks. Will also explore a new edge seal design, another key durability issue.
- It is not clear that the project is working toward the tough temperature and RH targets.

Strengths and weaknesses

Strengths
- Strong team with appropriate skills to accomplish technical objective.
- Long term durability of 5000hrs. For vehicles uses and 40,000 for stationary have to be achieved if Hydrogen Fuel Cell Initiative is to be realized.
- Excellent progress. New insights are being gained in membrane degradation and the role that water humidification and swelling play in this process.
- Membranes have been made with significantly improved mechanical strength and chemical stability with new tougher polymers and reinforced membranes. Methodology for tracking degradation - fluoride emission rate- show significant 7X reduction.
- Next year work on edge seal design, another key durability issue.
- Comprehensive approach that is addressing the obvious failure modes of the MEA in a systematic way.

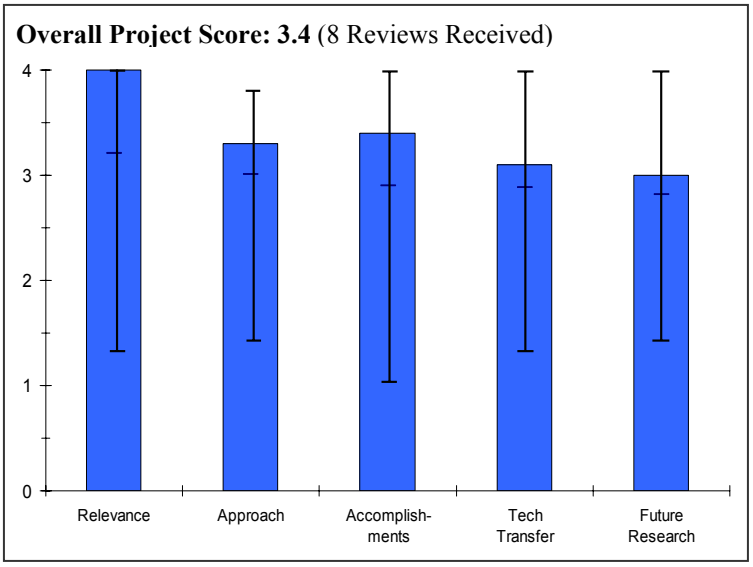
Weaknesses
- Slide 13: How is stability factor defined? Mechanical test may provide some guidance, but as described doesn't mimic cyclic stresses in fuel cell (only half a cycle).
- Unclear what role USM plays in project or what they have contributed to project. No clear USM results to date.
- Unclear from the presentation what conditions the membrane is exposed to during durability tests. To meet overall DOE program goals, temperature and RH targets are significant. Are these the conditions being used?

Specific recommendations and additions or deletions to the work scope

- Simulation to correlate accelerated testing conditions with actual test data.
- Publish degradation mechanisms.
- How does durability change from top to bottom in a stack and side to side? Are there tools like the NIST neutron imaging that can used to monitor degradation onset? What is the dependence of MEA degradation with operating conditions and is there an optimum point.

Project # FC-10: New Electrocatalysts for Fuel Cells
Ross, Phil; Lawrence Berkeley National Laboratory

Brief Summary of Project



For this Lawrence Berkeley National Laboratory (LBNL) project, new catalysts for both anodes and cathodes are being developed 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 will be based on computational screening of PGM core-shell nanostructures using newly developed (under BES funding) Monte Carlo simulations. LBNL will pursue 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.

- Very well aligned and supporting the goals and objectives of the DOE R&D plan.
- Strong intersection of theory and experimentation to address an enduring, fundamental challenge
- Basic work such as this is critical to provide the basis for the development of active catalysts with reduced cost and higher activity.
- The understanding of the ORR mechanism is crucial for optimizing catalyst design.
- Critical project that attempts to understand catalyst performance.
- Fuel cells only work as well as catalysts work.
- Establishes the fundamental science towards the durable activities of ORR catalysts 4x those of Pt that are needed for automotive fuel cells to become a reality.
- Addresses fundamental aspects of peroxide generation, a major source of degradation in fuel cells.
- Low-temperature work further substantiates ability of PEM stacks to operate so long as gas flow isn't blocked by ice.

Question 2: Approach to performing the research and development

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

- It is a well-established and precise approach, followed throughout the years.
- Current phenomenology and state-of-the-art tools.
- Would benefit from more field data.
- Using key questions as captions is very effective.
- Clear path on enhancement of catalyst activity for ORR. Unclear approach on mitigation of H_2O_2 formation.
- Excellent approach to developing a basic understanding of electrocatalysts and reaction mechanisms.
- The approach is clearly fundamental and in the right direction.
- Bimetallic catalysts (alloys) are well studied. Large literature in this area.
- Good combination of experiment and theory.

- PI needs to tell how to stabilize alloys for a long lifetime.
- Single crystal and other flat-surface catalysts simplify the catalytic environment just enough to allow clear experimental identification of critical parameters.
- Experience and perspective cast diverse pieces of data into a coherent whole that is relevant to operation of active fuel cells.

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

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

- Well done on evaluation of peroxide formation as function of temperature and Pt crystallite size is new and an important contribution.
- Agreement of theory and experiment confirms understanding and hypotheses, and paves way for progress.
- More connectivity to actual stack lifetime and durability campaigns and data would help establish connection to audience and ultimate pursuits.
- Good to identify significant factors of size and shape effects. Poor explanation how to obtain H_2O_2 formation mechanism by using test data presented.
- Significant progress in characterization of peroxide formation as a function of potential and in verifying the effect of alloying on the d-band center of Pt.
- Very interesting and relevant results were reported. The analysis provides clear ideas of the important factors affecting the ORR mechanism.
- Progress seems slow. The progress made over the past three years is not obvious.
- Good basic science work. Keep it up!
- Ross continues to make real, significant contributions.
- Blending of theory and experiments on model catalysts to explain why some alloys are effective.
- Quantization of peroxide formation as function of catalyst composition and operating temperature.
- Demonstration of the importance of catalyst particle size and shape in controlling activity.

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

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

- Excellent collaboration and support of applied development with fundamental research.
- Very strong MEA developer (3M) and integrator (GM), but would encourage involvement of more partners.
- No strong indication of Technology Transfer in this presentation, though past presentations have shown strong collaborations with GM and 3M.
- Collaborations are mentioned, but their contributions seem negligible.
- Need to establish collaboration with more MEAs and catalyst manufacturers/suppliers.
- Excellent interactions with industrial partners.
- Formal interactions with 3M have benefited both this project and the 3M advanced MEA project by establishing importance of catalyst structure and legitimizing what otherwise seemed to be magical aspects of the claims for the 3M whisker systems.
- Less formal consulting within DOE and with many elements of the fuel cell community has opened many eyes towards productive directions in solving durability issues.

Question 5: Approach to and relevance of proposed future research

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

- Non-carbon support work and PtRe catalyst evaluation. Work planned is of importance.
- Consideration of bimetallics would be strongly benefited by tighter collaboration with catalyst companies.
- A clear justification for the future research directions was not presented, but was alluded to in the answers to questions.
- Proposed future works are with the same high quality as those of the present steps.

- Good to focus on shape effect and support material.
- What is the value of conducting a remanufactured MEA? Especially when all MEAs are different in shapes, thicknesses, size, etc.?
- How is all this knowledge transfer when Ross retires?
- PI intends to finish these studies and then retire.
- Important to get these important results fully documented.
- It is not clear that Re is the best choice as a non-Pt core for a shell electrode. Pt-Re alloys showed no activity improvement upon annealing, so it's not clear that a stable Pt-only surface would have or maintain increased activity. Re is also very expensive. Wouldn't W or maybe Ta also satisfy the surface energy requirements to give a Pt surface (they would, admittedly, be harder to prepare with an initial oxide-free surface)?
- Plan of the PI to retire would deprive the fuel cell community from full-time access to an important resource. It is to be hoped that significant part-time fuel cell activities will continue.

Strengths and weaknesses

Strengths
- Use of scientific fundamentals to address key industry challenges.
- Good focus on shape effect and support material.
- Research such as this, that bridges the gap between very basic research and applied materials development efforts, is necessary. Understanding the basis for not achieving the expected activity improvements with Pt_3Co is also important. Also, the understanding of peroxide formation is necessary to understanding degradation mechanisms and to avoid operating conditions that will cause membrane degradation.
- Clear understanding of the factors affecting the ORR mechanism and electrocatalysis.
- Some very good basic science work.
- Carefully thought-out and executed experiments to demonstrate the fundamental origins of improvements in activity.
- Blending of theory and experiments on model catalysts in the context of operation of real fuel cells.
- Modeling capability on catalyst.

Weaknesses
- Lack of more familiar durability datasets.
- Lean collaboration team.
- Modeling capability on catalyst.
- Lack of rationale in the presentation for choosing the Pt-Re system and indication of how to achieve the desired nano-scale catalyst structures (e.g., rods vs. cubo-octahedra particles).
- Real fuel cell testing required.
- Need to establish collaborations with suppliers/manufacturers of MEAs and catalysts.
- It is unclear how the work of Dr. Ross will be transferred as he retires. I hope that all the investments over the past years are not lost in the transition.
- How is this work transfer to catalyst and MEA suppliers/manufacturers?
- Historically alloys have not been stable. For example cobalt dissolves from Pt/Co alloys, leaving a high surface area-residue of Pt. That Pt slowly rearranges. The result is a measurable voltage enhancement, but that is a short-lived result. There is a possibility that dissolved Co will enter the membrane and decrease conductance.
- This alloy stability issue needs to be considered. A set of experiments that describe the catalyst (size, shape, and composition) as a function of fuel cell run time remains to be done.
- Not all of the quantitative concepts are always carried forward quantitatively as far as they could be. For example, does the equation put forward to explain the effects of OH_{ads} on ORR experiments quantitatively explain the downward concavity in the Tafel slopes, using reasonable adsorption parameters?

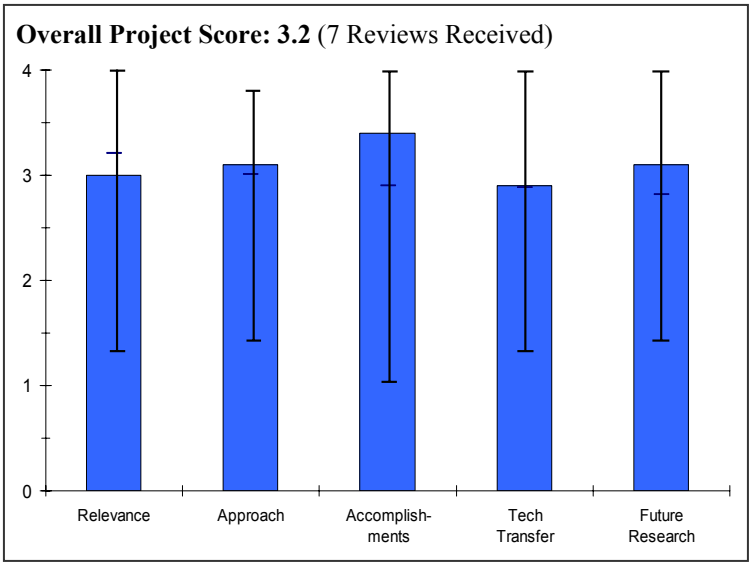
Specific recommendations and additions or deletions to the work scope

- Characterization of additional skin-core structured catalysts with a wider range of compositions (i.e., ternaries?).
- Conduct single cell testing with some collaborators.
- Need to establish collaborations with suppliers/manufacturers of MEAs and catalysts.
- Need to figure out how the work of Dr. Ross will be transferred to colleagues and industry as he retires.
- PI's results show that peroxide is produced at the anode as well as the cathode. This explains a lot why thin membranes have such poor durability. Oxygen is appreciably soluble in fluorinated materials (like Teflon = Nafion). Membrane developers need to measure oxygen solubility in their membranes, because these results suggest that polymer decay is the result of oxygen transport, and depends, being facilitated transport, on solubility. Air bleed into the anode obviously is good way to make anode peroxide. Probably not a good idea.
- Consider another choice than Re for the cores of shell/core catalysts.

Project # FC-11: Non-Precious Metal Catalysts
Zelenay, Piotr; Los Alamos National Laboratory

Brief Summary of Project

This Los Alamos National Laboratory (LANL) project on non-precious metal catalysts for polymer electrolyte fuel cells focuses on performance stability and the mechanism of oxygen reduction reaction (ORR) on pyrolyzed N_4-chelates, including identification of the active catalyst sites. In a later phase, the project will also include an initial viability study of other non-noble ORR catalysts, such as metal oxides (NiO, Co_2O_3, $NiCoO_2$, perovskite LaSrCo oxide, Cu-Mn oxides) and, in another group, cluster-like materials derived from ruthenium selenides (Ru_xSe_y).



Question 1: Relevance to overall DOE objectives

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

- There is a certain possibility to develop the catalysts with satisfactory properties to replace Pt using this approach.
- In principle, the project addresses one key barrier, of cost, by focusing on non-precious metal catalysts. It will be important to consider all aspects of cost; particularly process costs if one of the materials sets being considered were ultimately to be scaled up for required quality control for real applications.
- Performance target is unclear. Not consistent with MEA performance target.
- Reduced cost electrocatalysts are a key enabler for automotive fuel cell commercialization
- A gap seems to exist between the potentiality of the non-precious catalyst performance and the general expectation for the fuel cell efficiency.
- Reducing catalyst costs cuts device costs.
- Essential to find lower cost catalysts.
- Focused on one critical barrier.
- Exploration of non-precious metal catalysts important research area.
- Specific DOE targets/barriers are not highlighted.

Question 2: Approach to performing the research and development

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

- The approach is based on the analysis of the results of several groups that were engaged with some success in similar studies. This was a good approach for the start.
- Their approach is based on some established material paths suggested in the literature and are credible.
- The role of the silica was not clear.
- Theoretically good to identify active site and focus on its distribution to improve performance. However, performance target is not supporting DOE MEA target.
- Methodology is adequate and will surely lead to conclusive results.
- Important synthesis of catalysts done off-site at a university, perhaps prepared without adequate documentation.
- Program requires reliable, reproducible catalyst sources.
- Parallel research in the areas of catalyst activity and stability studies is good approach.
- Approach is logical.

- Approach is very academic.
- Industrial contributions should be incorporated into the project from the beginning.

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

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

- For identifying the active centers of pyrolyzed macrocycles, more sensitive techniques seem necessary than those used so far. Excellent activity was obtained with Co-TPP.
- The CoTPP and chalcogenide performance results are amazing given how quickly they were achieved compared to many other research groups' efforts in the area of non-precious metal catalysts, and how relatively few research dollars were required. The tests to check that there was no chance of Pt on the cathode are good. It is a concern however if the excessively high anode loadings have any effect in some unexpected way, e.g. a fraction of a per cent of Pt migration from the anode to the cathode. It would only take a few micrograms/cm^2 of Pt on the cathode to give the performance seen. It is critical then to characterize the tested MEAs for Pt migration after fuel cell testing.
- Non-PM catalyst performance is equal to, or best to date. Good progress for this one-year-old program. However, there is still a very long way to go before these, or any other, non-PM catalysts exhibit viable performance for automotive application.
- Quite significant accomplishments were achieved. A large gap between the performance of a Pt/C and of the non-precious catalysts was clearly evident.
- Fuel cell results are less exciting than expected.
- Performance goal should approach 0.7 W/cm^2! Results are far from this goal, too far.
- This catalyst system is just not useful in acid solutions; this is well documented in the literature. (The catalysts do work well in basic solutions, say KOH fuel cells, and there they show better performance and lifetime than PM catalysts.) There needs to be some real thought about this stability issue.
- Good progress.
- Significant number of milestones met or exceeded.
- Unclear if on track to meeting key DOE technical barriers/targets.
- Date for project to conclude unclear, 2005? 2006? Not apparent that targets will be achieved.

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

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

- Excellent collaboration from the start of the project.
- Very good collaborations at this time, but it was not explained why non-precious metal catalysts would be as critical for small portable devices where the $/kW are orders of magnitude higher than for automotive.
- Collaborations with the different research centers seem not significantly implemented.
- Making catalysts is a complicated task.
- Essential to find other catalyst vendors who can provide a well-documented and reproducible set of materials.
- Large number of collaborative partners.
- Continue to strive for participation of all partners in overall project.
- Dissemination of non-proprietary results through additional publications recommended.

Question 5: Approach to and relevance of proposed future research

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

- The plan is realistic and addresses the key problems of these catalysts.
- Good objectives, particularly to carefully try to identify the ORR site. Further emphasis on understanding mechanisms rather than demonstrating the highest fuel cell performance should be considered. The PI should carefully continue to look for alternative artifacts that could somehow contaminate their Cathode with Pt or Ru. Look carefully for traces of Pt on the tested MEA cathodes. Another suggestion is to use a non-Pt hydrogen

oxidation catalyst that will still be adequate for the cathode current densities seen but which will completely eliminate any chance of Pt contamination, e.g. Pd the HOR catalyst.
- Formation of H_2O_2 should be investigated also.
- Properly isn't rushing to scale up.
- Future works seem to present the right focus and aim to a better understanding of the phenomena reported during this year.
- Reasonable focused work.
- Material cost should be discussed, if it is in the DOE target description.
- Good overlap between catalyst activity work and stability work.
- Catalyst stability vs. durability should be addressed.

Strengths and weaknesses

Strengths
- This is an excellent group of researchers that can address this difficult task.
- Strong fuel cell fundamentals team. Significant experience of U. of NM in synthesis of these materials.
- Implementation of electrochemical measurement on catalyst evaluation.
- Non-PM performance is equal to or better to date.
- The large progress on the research, and the impressive performance of the non-noble catalyst.
- The DMFC data are interesting.
- Strong team.
- Important technical issues addressed.
- Sound technical approach.

Weaknesses
- Performance is still far below that useful for automotive commercialization.
- There is still a gap between the Pt and the non-precious metal catalyst performances.
- CoPP types of catalyst are just not sufficiently stable for consideration.
- The goal is to get the fuel cell system to be low cost. The catalyst cost is part of that cost. A poor catalyst, no matter how cheap, will never result in a low cost fuel cell. Higher voltage performance needs to be demonstrated soon, or this class of catalysts needs to be eliminated from consideration.
- No discussion of project status relative to DOE targets/barriers.
- Total project timeline vs. budget is unclear.
- Significant performance improvements remain relative to commercially available Pt catalyst.

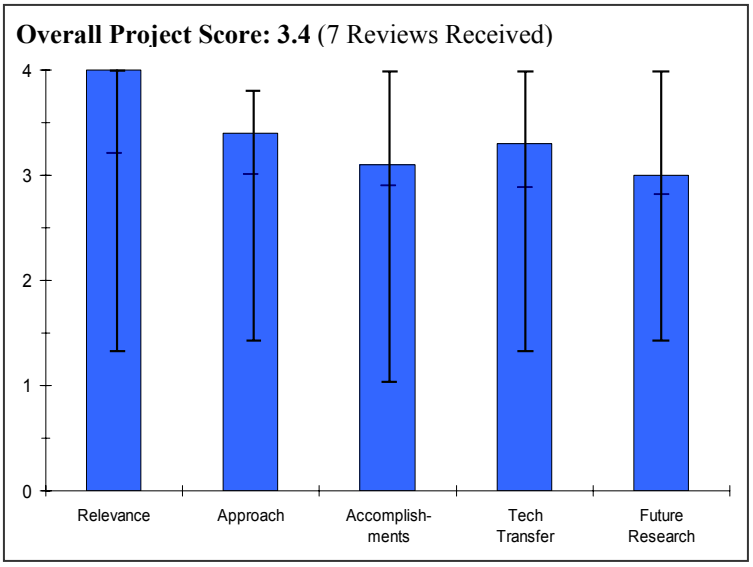
Specific recommendations and additions or deletions to the work scope

- Identity performance target which is consistent with DOE MEA performance target.
- When applicable, concentrate on performance in the 0.6-0.8 V regime. High power density at <0.6 V is not useful for automotive as efficiency will be too low.
- When performance improves, expand to investigate low RH conditions.
- Report results in the format of non-PM catalyst roadmap target: activity per volume of supported catalyst. 2010 target: >130 A/cm^3 @ $800mV_{ir\text{-}free}$.
- Work on direct-methanol is not applicable to DOE program: discontinue.
- Without a strategy for long-term stability (10,000 hours), this project needs to be reconsidered. The use of pyrolyzed porphyrins was well-studied on PEM systems earlier. Those data showed good performance, but not for long. The challenge is to achieve lifetime with these materials.
- More industry participation.
- Include timetable, milestones, go/no-go concepts into future plan thought process. Otherwise screening can continue indefinitely.
- Need to realistically assess if proposed catalysts are feasible relative to ability to commercially manufacture.
- Need to compare material costs of proposed non-precious metal catalysts to material cost target.

Project # FC-12: MEA and Stack Durability for PEM Fuel Cells
Hicks, Mike; 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, meets 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 will be on MEA component development, MEA characterization and diagnostics, and defining a system operating window.



Question 1: Relevance to overall DOE objectives

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

- The project is very relevant to the R&D targets.
- Strong team
- This project is extremely relevant to the DOE objectives of quantifying and improving durability and performance.
- Many aspects of fundamental durability issues critical to PEM fuel cells for all applications are covered.
- Improved membrane durability is critical to the success of the hydrogen program.
- 3M makes a good point regarding system effects on component durability. Project breakdown to component effects is indeed helpful.
- Progress towards automotive targets as well as toward stationary should be presented, even if the fuel cell developer associated with the project is in the stationary business.
- The Principle Investigator's answer (in response to question on progress toward automotive targets) that accelerated tests for stationary are just automotive conditions, is largely correct, but a bit of explicit attention to the automotive situation would improve effectiveness of the work for both applications. Page 19 comments that catalyst support degradation is not a critical barrier seems to indicate a restriction of thought processes to stationary applications – note that even with proper mitigation strategies in place, automotive applications put a lot of stress on to catalyst supports.

Question 2: Approach to performing the research and development

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

- Components behavior and ex situ studies are good as the first effort. In situ methodology should be developed.
- 3M uses a well thought approach to evaluating the various mechanisms contributing to the loss of performance.
- How about 100 degrees C/25% RH H_2/O_2 open circuit voltage (OCV) stability?
- The strategy used to integrate system level issues into test plan is excellent.
- The approach using model compounds to study membrane degradation processes is excellent.
- Membrane degradation should be quantified using additional physical property characterizations rather than just weight loss and fluoride ion release.
- 3M illustrates a good understanding of gas diffusion layer (GDL) permeability re: pore size. There is a clear distinction between chemical (peroxide effects) and mechanical failure modes, but are F^- ions the only evidence?

- Government-funded research should concentrate on optimizing MEAs and components for durability. The second topic on slide 4 "Optimize System Operating Conditions to Minimize Performance Decay" is a topic better covered in a competitive development environment. At least in the early stages of the project, it has seemed to emphasize the former. The 2000 hour system demonstration target could entail primarily the latter. This demonstration, however, may have arisen from terms of the original DOE solicitation that perhaps should be reconsidered.
- Overall approach is good -- component-by-component evaluation of individual degradation modes and means to mitigate them, primarily through materials solutions.
- Attempt to match projections from accelerated tests to actual lifetimes under other conditions is a very nicely-planned experiment -- it will be interesting to see how it comes out.
- Accelerated degradation testing approach is good.
- 3M will need to establish correlation between accelerated and actual degradation.
- Recognition of the effect of system operation on durability is good.

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

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

- 3M has made excellent progress in all areas.
- Good improvement of the MEA stability was achieved in accelerated tests.
- 3M shows good progress in identifying reasons for loss due to GDL oxidation at high voltage operation.
- There may be too much relevance placed on "Fenton" test and "fluoride" release.
- The presentation was clear and well articulated. However, interactions of separate component improvements on overall improvement were not explained well.
- New 3M membrane materials, with stable end groups and appropriate additives, would appear to provide an important tool towards reaching durability goals.
- Result that tear strength is unchanged after up to 25% membrane weight loss is quite remarkable; it helps set targets for allowable membrane chemical degradation rates.
- From the presentation it was not immediately clear how some of the accomplishments, e.g. membrane additives, differ from the accomplishments presented last year or under earlier 3M projects.
- Identification of the class of additives used, e.g. antioxidant or Fe2+-sequestering agent, would help fuel cell developers better plan to make use of the advantages offered by advanced 3M materials.
- CO + air bleed effect increasing F^- generation is nice work, but wasn't the effect of CO + air bleed in increasing peroxide generation demonstrated in the open literature a while ago?
- Results to date are very promising.
- The presentation did not address the improved-durability membranes' effect on performance or cost.

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

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

- There are multiple collaborators at universities and in industry.
- It is not clear how the partners, i.e., CWRO and Plug Power, participate or contribute to the program.
- Work assignments are not very clear -- what did University of Miami do?
- Interactions with Plug seem to be going and probably will become more apparent later in the project. The presentation did not really define how the collaborations with Case Western are advancing the art.
- The collaboration has improved since last year.
- One reviewer asked, "Will other companies benefit besides 3M?"

Question 5: Approach to and relevance of proposed future research

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

- It is difficult to judge with undisclosed chemistry.

- Detail on future research plans is lacking.
- Need to evaluate effect and contribution of catalysts to the failure or decrease of performance of long term test experiments.
- 3M needs to clarify which components are expected to add the most value.
- One reviewer expressed concern that too much of the future effort will be wrapped up in a system demonstration rather than in further improvements in materials and accelerated testing methodologies.

Strengths and weaknesses

Strengths
- Syntheses and production facilities are excellent.
- Development of accelerated test protocols is important and well addressed in the program.
- Integrated approach is excellent.
- The approach is very methodical: GDL permeability, chemical/mechanical degradation correlations, segmented cell to see inlet/outlet variations. Good work was performed on GDL surface chemistry effects.
- 3M presented some apparent significant steps forward in membrane durability.
- There appears to be improvement in understanding of how to make more durable electrode structures. (However, insufficient details were shared to allow a real judgment to be made).
- 3M has made a nice start on an attempt to map results of accelerated tests onto unaccelerated lifetimes.
- Recognition of the effect of system operation on durability is good.
- Accelerated degradation approach is good.

Weaknesses
- The presentation did not address the effect of catalyst degradation.
- Mechanical strength should not be the only measure of membrane performance degradation after Fenton's test. Instead of using mass loss to determine membrane degradation, use a physical property measurement, such as viscosity or conductivity. Degradation of the MW and loss of sulfonate groups may not be evident in mass change measurements. Shouldn't the performance of the MEA be the metric for the quality of a component rather than fluoride ion release and projecting that this will affect the performance? Other factors may affect performance; fluoride ion release is an indirect method.
- COOH end-groups are known to be unstable (which was recognized in the course of this project). Why not combine GDL effects and membrane improvements in the study? It is not very clear how the lifetime predictions were made.
- Page 13 addresses break-in of MEA, not startup of fuel cell (language susceptible to temporary misinterpretation).
- The presentation should have clarified what is new (versus accomplishments from last year or from previous projects.)
- Given the broad scope of the project title, one might have expected a bit more attention to catalyst durability (but attention to this topic has been given under another 3M project.)
- 3M needs to address other targets in concert with durability (e.g., cost, performance).

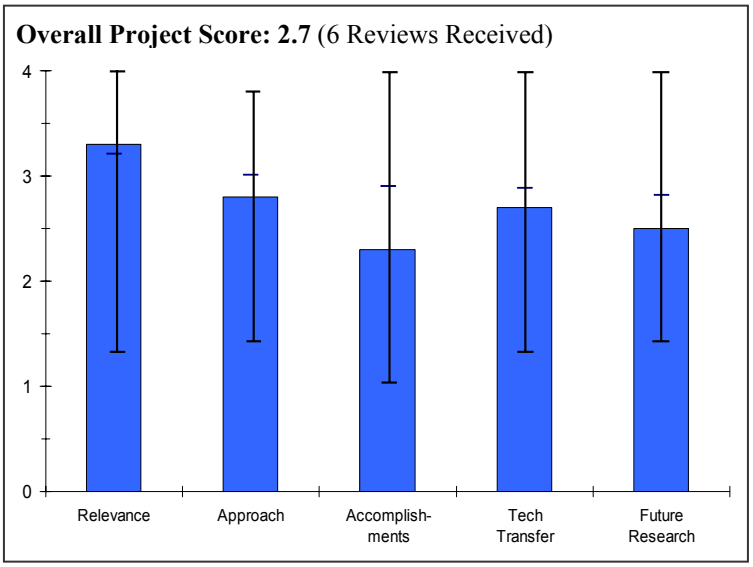
Specific recommendations and additions or deletions to the work scope

- Evaluate catalyst degradation and how it affects the overall system/MEA durability.
- Perform additional characterization of membrane physical properties and effect of aging on these properties.
- What about considering reinforced membranes? Determine the source of F^-: membrane or electrode formulation? 3M needs to clarify which component added the most value from A to C improvements.
- Extensive hot/dry accelerated testing should be supplemented by some sensitivity testing of individual phenomena, lest surprises arise from cases where hot/dry isn't the most aggressive condition. Page 6 is a case where the program already properly addressed this kind of issue.
- Enhanced collaboration on the system operation effect on membrane durability could improve understanding of decay mechanisms and solutions.

Project # FC-13: Development of Transition Metal/Chalcogen Based Cathode Catalysts for PEM Fuel Cells
Campbell, Stephen; Ballard Power Systems

Brief Summary of Project

Ballard Power Systems intends to develop a non-precious metal cathode catalyst for PEM fuel cells that will be as active and as durable as current platinum group metal (PGM) based catalysts, at a significantly reduced cost. The plan includes development of composition and structure, process development (can be scaled up), and evaluation/demonstration in fuel cells and stacks.



Question 1: Relevance to overall DOE objectives

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

- There is a certain possibility to develop the catalysts with satisfactory properties to replace Pt using this approach.
- Critical to reduce or eliminate the high cost component of MEAs - platinum.
- Supports the goals of the HFCIT program for decreasing catalyst costs.
- Objective of project is highly relevant to DOE goals to develop non-platinum group catalysts.
- Non-PGM catalysts clearly on critical path to cost target of $ 40 /kW.

Question 2: Approach to performing the research and development

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

- Studies of new chalcogenides with well justified fundamentals for an expected activity for the ORR could produce interesting catalysts. This aspect should be visible in future work.
- The project has a lack of focus on durability testing prior to incorporation into stacks.
- How does the cost compare to conventional materials?
- Good to see congruence between sputter-deposits and supported catalysts, although some chalcogens can be multi-phase so there may always be the possibility of having to make sure the best sputter-made compound(s) can be made in supported form.
- Strongly endorse the approach of checking for oxygen substitution: would suggest simple “shelf life” tests open to the air to make sure stable to slow oxygen replacement.
- Incorporate some sort of “OCV” testing in durability plan.
- Thin films can provide well characterized structures with defined surface areas.
- Films expose only one crystal face, while particles will have other faces exposed also.
- The approach of using sputtered thin films for comparison and structural/compositional/activity characterization is excellent to avoid the difficulties with synthesis and characterization of high surface area catalysts.
- A combinatorial approach is probably a good one for projects pursuing non-PGM catalysts, but a 3x3 matrix can hardly be considered "combinatorial". No justification was given for such a limited "library".

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

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

- Despite good research effort, the activity of the best catalyst for the ORR was low.
- There is a need for a new approach in selecting promising systems.
- Good analytical basis on results
- "Absolute" electrochemical performance has a ways to go.
- Fe and Cr selenides do not look promising at all, while Co selenide shows a little promise.
- Thermodynamic potential for Co selenides probably too low to be useful.
- An impressive number of catalysts have been screened and characterized. The activity of these catalysts for the ORR is very low, even when going to carbon-supported catalysts indicating that these catalysts may just intrinsically have low activity, not limited by surface area.
- For a combinatorial approach, I consider the results to be rather poor. None of the catalysts were even close to meeting the targets, not even close to the results on non-PGM catalysts reported by others at this meeting. One of the benefits to the combinatorial methods is that you can identify trends in activity without knowing the mechanism of action. But that is not possible if the library is so small. Very disappointing effort.

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

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

- Collaboration with two Universities seems to be in place.
- Moderately good collaborations.
- Collaboration with a catalyst group or physical chemistry group specializing in surface adsorption techniques to develop a method for measuring surface area of the chalcogenides would be helpful.
- Not clear what university partners are doing to contribute to project or that a strong collaboration exists.

Question 5: Approach to and relevance of proposed future research

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

- More promising systems should be selected.
- More electrochemical testing. See recommendations below.
- Has the PI formulated a complete matrix for selecting acceptable catalysts? For example, minimum current density, stability under defined conditions, etc?
- Investigating the sulfides is the next logical step. If thermodynamics of these systems is no better then direction should be changed to find a thermodynamically beneficial system.
- A clear route to obtaining higher activities was not established, especially since it appears that the material is intrinsically inactive with a low thermodynamic potential for ORR.
- No vision.

Strengths and weaknesses

Strengths
- Good science and characterization possibilities.
- Solid analytical/structural capability: good use of an extensive portfolio of analytical instrumentation.
- Good approach of using sputter method for new lead development, and reduction to "catalyst" through supported form.
- Provides well defined systems to look at so rate information and mechanistic details easier to obtain
- Approach to looking at well-defined and characterized thin films as a screening method before attempting synthesis of high surface area materials.
- Incorporation of a Go/No-Go decision at the end of FY'05.
- Ballard has the resources and seemingly the interest in a combinatorial synthesis-rapid screening protocol.

Weaknesses

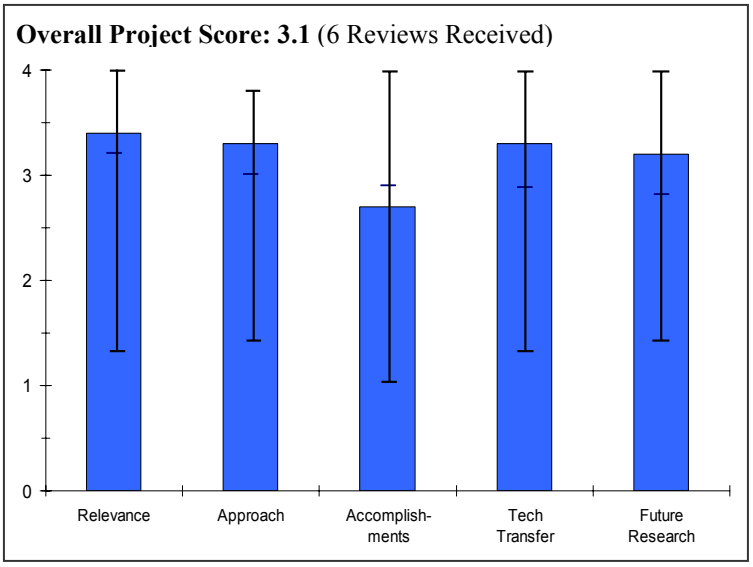
- Electrochemical testing should be expanded in the analytical direction (See RRDE below).
- Durability to "open circuit potential" or simulation thereof is missing in the approach.
- Problems with film growth and adhesion have slowed progress.
- Should normalize ORR currents to catalyst surface area (BET area perhaps?) or catalyst weight to allow comparison of activities.
- The combinatorial synthesis-rapid screening protocol was poorly executed, consequently producing neither a lead to a new catalyst nor any new information.

Specific recommendations and additions or deletions to the work scope

- Incorporate RRDE measurements to check for peroxide: if it's too late for RRDE purchase, use plots on Koutecky-Levitch and use the slope to estimate effectiveness for 4 electron reduction.
- Open circuit testing for stability, or simulation of process.
- A dual benchmark should be presented on the yearly review: fuel cell polarization or electrochemical test comparing internal progress compared to initial efforts/catalysts and the same for state of the art chalcogens/alternatives from the catalyst community.
- General comment for all "non-platinum" catalyst programs: Original PEMFC cost targets derived from sophisticated model relating loading of platinum, power output, and current density of MEA to final stack cost. There was a recommended optimum based on the lowest platinum needed to achieve a reasonable current density (do not want large stacks as part costs increase). Does the model need to be re-examined for non-platinum case and a new performance target set (should the 0.6g/kW target be converted to something like $12/kW effective catalyst cost, which would include prep costs, etc.?).
- Would be beneficial to develop a methodology to measure the surface area of these chalcogenide catalysts to allow comparisons of work on dispersed catalysts.
- Conclude project in as timely a manner as possible.

Project # FC-14: Novel Approach to Non-Precious Metal Catalysts
Atanasoski, Radoslav; 3M

Brief Summary of Project



This 3M project will develop and demonstrate non-precious metal cathode catalyst to lower cost (goal of 50% less vs. target of 0.2g Pt/peak kW) and to reduce the dependence of proton exchange membrane fuel cell catalysts on precious metals. 3M will identify opportunities for system cost reduction through breakthroughs in key areas of the fuel cell, the catalyst, and application of cost-effective processes for membrane electrode assembly (MEA) fabrication closely associated with the development of the new catalyst. 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.

Question 1: Relevance to overall DOE objectives

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

- Development of a viable non-precious metal catalyst with sufficient performance and durability would reduce fuel cell costs - a major DOE technical objective.
- Critical to reduce or eliminate the high cost component of MEAs - platinum.
- Reduced cost electrocatalysts are a key enabler for automotive fuel cell commercialization.
- The objectives are good and address the longer range issues that are important to the program.
- This project is extremely relevant to DOE's mission if significant activity improvements are made.
- Lower cost electrocatalysts are critical to achieving cost targets for PEM fuel cell systems.
- It is not sufficient to simply eliminate precious metals from the electrocatalysts, but also demonstrate low-cost synthesis methods (this aspect could not be assessed).

Question 2: Approach to performing the research and development

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

- A systematic, combinatorial approach incorporating synthesis of many catalyst samples in the Fe-N-C system.
- Outstanding and comprehensive approach covering all aspects of catalyst discovery: theory, synthesis/experiment, and reduction to practice.
- The use of outside expertise at the National labs and the university communities are excellent uses of available resources and may provide the needed expertise to maximize success.
- Good balanced approach with combinatorial, characterization, and modeling efforts.
- The electrocatalyst development work is augmented by modeling, high throughput screening, and state-of-the-art characterization. At the same time, the high throughput screening methodology used only provides information on initial activity (not durability) and hides information related to the effects of synthesis techniques.
- There has been no real effort to date on evaluating durability of the electrocatalysts.

- It is not clear from the information provided on the "nanotechnology" synthesis process (i.e., none) that the process is scalable to low cost production. It is insufficient to state evidence of scalability based on the "number of fuel cell samples prepared".

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

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

- Steady improvement in catalyst activity has been achieved but performance is still only "approaching results reported elsewhere".
- Increase in actual current density with non-precious metals substantial.
- Use of 10% Pt/C unusual as benchmark: this is commonly used in phosphoric acid fuel cell, and occasionally as anode in PEMFC, but not often on cathode.
- Needs stronger durability component.
- Demonstrated considerable progress in catalyst performance in the last year.
- However, still far below viable performance for automotive systems and below modest "interim milestone #2".
- Good progress has been made on a tough technical problem.
- Significant progress has been achieved in improving initial activity levels.
- The infrastructure for catalyst development appears to be well established.
- Durability should have been assessed much earlier in this project.

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

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

- Collaboration with universities and a national laboratory.
- Project would benefit from collaboration with fuel cell and/or commercial catalyst supplier.
- Very good use and coordination between partners.
- One of the biggest pluses for this activity is the ability of the company to address scale up and processing issues hand in hand with the latest research results.
- Excellent collaboration with university partners and clear indication of what they are contributing to project.
- Aside from the collaboration with Dalhousie University, it is not clear that other subcontractors are actively engaged in the project.
- Technology transfer is inherent to the project, since 3M is ultimately capable of commercializing the technology if technical and economic viability is proven.

Question 5: Approach to and relevance of proposed future research

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

- Good plans for completion of project - potential benefit depends on results obtained in the remainder of the project.
- Request emphasis on stability/durability beyond hydrogen peroxide test (OCV stability, cycling, resistance to acid corrosion, temperature stability).
- Scale up ("1-2 kW") should be delayed until performance is within sight of that of Pt catalysts.
- There is a well organized plan for how to address future activities and they are well prioritized.
- A clear path to the necessary activity improvements was not presented.
- Durability testing was listed as an activity for 2006 – this activity needs to occur sooner rather than later.
- A manufacturing cost analysis should be performed – to verify that cost targets are indeed achievable.

Strengths and Weaknesses

Strengths

- A thorough, systematic and committed approach.
- Comprehensive approach and good team to cover not only catalyst discovery but reduction to practice.
- National lab and university involvement. Ability to translate results to the "processing" scale.
- Excellent array of catalyst characterization techniques, collaboration with the Dahn group and BNL, and modeling effort.
- Good combination of synthesis, screening, characterization and modeling are being utilized on the project.
- Excellent technical progress is being made toward identifying active electrocatalysts.

Weaknesses

- Surprising to see that with this level of sophistication in approach, more detailed electrochemical characterization (such as RRDE to look for hydrogen peroxide) or stability tests (thermal, acid, open circuit potential) seem to be missing.
- Being limited by intellectual property considerations from interacting freely and openly with the community at large.
- Catalyst activity is still low, despite improvements, relative to both the Dodelet results on similar materials as these and to platinum.
- Lack of durability testing as a key criterion during compositional screening.
- No real evidence that the synthesis process is scaleable at low cost.

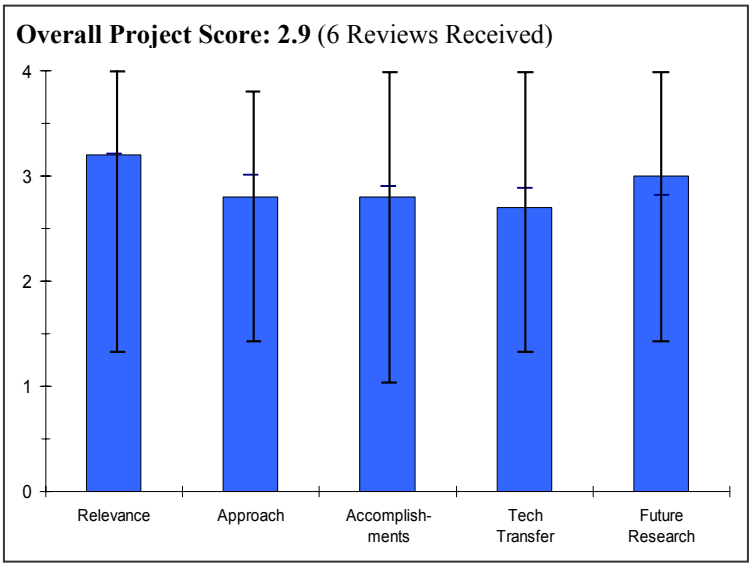
Specific recommendations and additions or deletions to the work scope

- Incorporate RRDE measurements to check for peroxide.
- Open circuit testing for stability: compare to Pt benchmarks.
- Increase routine screening for thermal, chemical (acid), and electrochemical stability (OCV).
- General comment for all “non-platinum” catalyst programs: Original PEMFC cost targets derived from sophisticated model relating loading of platinum, power output, and current density of MEA to final stack cost. There was a recommended optimum based on the lowest platinum needed to achieve a reasonable current density (do not want large stacks as part costs increase). Does the model need to be re-examined for non-platinum case and a new performance target set (should the 0.6g Pt/kW target be converted to something like $12/kW effective catalyst cost, which would include prep costs, etc.?).
- When applicable, concentrate on performance in the 0.6-0.8V regime. High power density at <0.6V is not useful for automotive as efficiency will be too low.
- Some poll curves should use conditions relevant to automotive: 1-3 bar (absolute) air.
- Report results in the format of non-platinum catalyst FreedomCAR Fuel Cell Technologies roadmap target: activity per volume of supported catalyst. 2010 target: >130 A/cm3 @ 800mV ir-free.
- When performance improves, expand to investigate low RH conditions.
- I would suggest that some effort be given to evaluating the best low Pt content catalysts in the MEA configuration as a "control" and challenge point to future Fe catalyst development.
- Suggest incorporation of a Go/No-Go decision at the end of FY'05.

Project # FC-15: Novel Non-Precious Metals for PEMFC: Catalyst Selection through Molecular Modeling and Durability Studies

Popov, Branko N.; University of South Carolina

Brief Summary of Project



The University of South Carolina will synthesize novel non-precious metal electrocatalysts with similar activity and stability as platinum (Pt) for oxygen reduction reaction (ORR). They will focus on high activity for ORR, mass production methods, corrosion resistance, low cost, and improving understanding of reaction mechanism of oxygen reduction on non-precious metal catalysts. 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 metal electrocatalysts as a substitute for Pt currently used in membrane electrode assemblies.

Question 1: Relevance to overall DOE objectives

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

- There is a certain possibility to develop the catalysts with satisfactory properties to replace Pt using this approach.
- Addresses in reality, one of the critical issues – lower cost through catalyst cost reduction. Until the performances of any such materials can be made competitive with Pt, it is too early to claim they will help with durability.
- Critical to reduce or eliminate the high cost component of MEAs – platinum.
- Non-PGM catalysts are clearly on the fuel cell electric vehicle roadmap to meeting long term $40/kW target.
- Use of new catalysts based on non-noble materials is of large importance for reducing the fuel cell cost.
- Exploration of non-precious metal catalysts important research area.

Question 2: Approach to performing the research and development

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

- The authors selected Co-based alloys and C precursors for making the non-precious metal catalysts based on promising data in the literature.
- Objectives appear to include everything on a wish list – too many to be realistically accomplished with the resources and time allotted, e.g. it is unlikely the PI will be able to accomplish low cost catalyst through mass production methods at a university - simply because his lab has access to large ovens. Also, to achieve high activity, durability and low peroxide production all at the same time will be difficult in one contract – people have been trying to do that with Pt for years. The PI is aware of the important issues but has inadequate time and financial and personnel resources to accomplish these goals. Various transition metals, including Co and Fe supported on carbons have been studied before, by commercial catalyst suppliers (e.g. Johnson Matthey). It is not clear what will be new here except the removal of the Pt. Ru is a precious metal too – is it being studied as a serious candidate?
- Good approach with outstanding emphasis on electrochemical characterization.

- Increase non-electrochemical stability tests: thermal and acid.
- Start OCV tests and compare to Pt.
- Co chelate is of low cost but the performance may affect negatively the fuel cell efficiency.
- Ru-based materials are of less importance if the aim is to reduce costs.
- Parallel research in the areas of catalyst activity and stability studies is good approach.
- Approach is logical.
- Potential poisoning of membrane should be addressed.
- Approach is very academic.

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

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

- Making carbon active for a four electron reduction of O_2 is interesting for some specific applications.
- The activity of Co-C is very good, while the high activity of Ru is not found in the presentation.
- Effect of surface modifiers for influencing the catalyst format and activity is good, but how much of this is already practiced by commercial catalyst suppliers? Working with such a catalyst supplier, as a consultant might be useful and allow moving faster – such as an Engelhard, TKK or JM. In general, too many topics are being pursued, and most do not seem to be on a path that can meet the targets.
- Very good results on innate (RDE) electrochemical activity.
- Good relative absolute activity in single cell testing “within 100 mV of Pt.”.
- Again the slides were rather "chaotic" making it difficult to sort out in what the direction the results were going. The results with Ru-based catalysts were said to be just for testing the method of catalyst preparation, but most of the results and the summary appeared to focus on the Ru-based, which are PGM and thus not consistent with the objectives. But some of the results with Co-Fe were interesting and possibly the most promising in all the non-PGM catalyst presentations.
- Performance of Co chelate is still very far from satisfactory. However, stability seems promising.
- Good quality data generated.
- Project scheduled to conclude in two years. A significant amount of research remains in the areas of durability, material costs, and cost of manufacturing the catalyst and the stack.
- Material and manufacturing costs not discussed for any of the catalyst alternatives.

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

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

- There are several active collaborators on the project.
- The collaborations could be usefully broadened. The PI should consider working with a commercial catalyst supplier to gain non-proprietary knowledge on catalyst support modification. Also, some fundamental modeling should be able to answer if any form of Co-Fe could ever have the activity of current state-of-the-art Pt. Current Density Functional Theory (e.g. Mavrikakis at U. of Wisconsin. Madison) should be able to provide some insight.
- Include industrial partner beyond University alliances for external validation.
- So far, the work was quite exploratory and is being conducted satisfactorily with no need of help.
- Work is being disseminated through significant number of presentations/publications within the last year.
- Increase in activity by CWRU and NEU apparent. CWRU and NEU participants are encouraged to also publish contributions to the project.
- Increased industry participation/collaboration strongly encouraged.
- Industrial contributions of Fuji Film and Faraday Technologies not apparent. Neither company listed as partner/collaborator.

Question 5: Approach to and relevance of proposed future research

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

- The authors should explain the role of quinones and their stability.
- Again, the proposed work is very ambitious. The stated future work is primarily a list of goals without an indication of what will be done to achieve them.
- Endorse approach to using RRDE/hydrogen peroxide detection as part of catalyst characterization. However, add other qualifying tests such as OCV testing (or simulation), stability to acid, and thermal stability.
- There is progress to build on. But the Future Work slide was unclear about what approaches were going to be used to improve the Fe-Co catalysts.
- Plans are in the right directions.
- Good overlap between catalyst activity work and stability work.
- Ambitious plan.
- Screening/development activities are still proposed. Will need to focus on specific non-precious metals and begin extensive studies over the next year.
- Catalyst stability vs. durability should be addressed.

Strengths and weaknesses

Strengths
- Excellent synthetic skills.
- Good approach from a materials viewpoint as well as thorough examination of fundamentals behind creation of a successful catalyst.
- Some reasonably good ideas based on previous work in literature are being pursued.
- Good performance of the Ru-based alloys. Very comprehensive studies of the non-noble metal catalysts.
- Solid, experienced team.
- Important technical issues addressed.

Weaknesses
- Need more efforts on non electrochemical stability tests.
- Not clear things are being done in a systematic manner.
- Low prospects for Co chelates for practical applications.
- Project would benefit from additional collaboration.
- Proposed work still very broad.

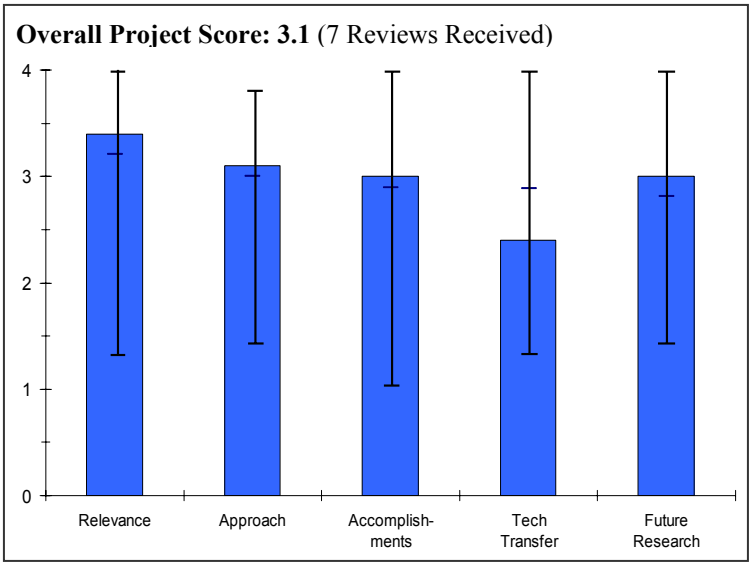
Specific recommendations and additions or deletions to the work scope

- Consider focusing on either the metal-free or the non-Pt metal catalysts, one or the other, for faster progress. A more in-depth understanding of one system would be preferable to less understanding of two.
- Increase routine screening for thermal, chemical (acid), and electrochemical stability (OCV).
- Benchmark current cost/performance based on DOE cost targets.
- General comment for all "non-platinum" catalyst programs: Original PEMFC cost targets derived from sophisticated model relating loading of platinum, power output, and current density of MEA to final stack cost. There was a recommended optimum based on the lowest platinum needed to achieve a reasonable current density (do not want large stacks as part costs increase). Does the model need to be re-examined for non-platinum case and a new performance target set (should the 0.6g Pt/kW target be converted to something like $12/kW effective catalyst cost, which would include prep costs, etc.?).
- Try to be more systematic and lay out the results in a clearer manner.
- More industry participation.
- Prioritize proposed studies.
- Include timetable, milestones, go/no-go concepts in future plans. Otherwise screening continues indefinitely.
- Need to realistically assess if proposed catalysts are feasible relative to ability to commercially manufacture.
- Need to compare material costs of proposed non-precious metal catalysts to material cost target.

Project # FC-16: Low-Platinum Catalysts for Oxygen Reduction at PEMFC Cathodes
Swider-Lyons, Karen; Naval Research Laboratory

Brief Summary of Project

The Naval Research Laboratory (NRL) is targeting the DOE goal of 0.2 g Pt/rated kW before 2010 by focusing on lowering 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, and ionic mobility of oxide supports. Objectives for 2004-2005 include rigorously characterizing active and inactive catalysts, devising mechanism(s) to explain catalyst activity, and designing new active and stable catalysts.



Question 1: Relevance to overall DOE objectives

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

- Fundamental approaches to PGM reduction in stack critical to achievement of stack cost targets.
- Project addresses important PEMFC cost reduction issues through the use of low platinum catalyst.
- Project well focused on targets for coat, performance and durability.
- Low cost, durable ORR catalysts are essential for achievement of DOE fuel cell targets.
- Catalyst reduction will help reduce cost but it was not reported how big a factor catalyst reduction will benefit program.
- Important problem.
- Unclear how this project will be translated into work that catalyst companies can use.

Question 2: Approach to performing the research and development

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

- The project did not build up on the successes presented a year ago.
- Development of microkinetic model would enhance understanding of the mechanism, rather than reliance of potential scan curves and Tafel slopes.
- Kinetic model would provide prediction of rate versus temperature and concentration. This would provide validation of the underlying mechanism.
- Kinetic model parameters could then be correlated to quantum mechanical models of the various materials
- Project approach is suitable to the planned goals.
- Approach redirected per 2004 comments and recommendations.
- Approach is well reasoned and implemented, but this is a difficult technical challenge.
- Very good approach.
- Appears to be well thought out.
- Is there a conductivity problem using TaPO?
- Strong combination of methods used for characterization.
- Well-reasoned approach - a little off-center, but we need this to make progress.
- Needs to get this catalyst into fuel cells.
- Needs still more characterization to understand nature of observed effect.
- PI is particularly well-attuned to guidance from reviews.

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

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

- Two many different experimental attempts, not necessarily all contributing to the goal of the project.
- Project has gotten insights into the mechanism by comparison of the electrochemical performance of the Ta and Fe forms of the catalyst.
- Project has shown the potential of this approach for reducing platinum loading.
- Project shows clear accomplishments concerning the electrochemical properties of the new catalysts.
- Project is making progress towards performance goals, but it's not clear on progress towards cost and durability goals.
- Good progress in understanding of mechanistics- the 'key' to success.
- Good progress on new catalyst.
- Good representations of data.
- Problem with Pt metal and Pt ions clearly identified even though a negative result. Still helps to make decisions.
- Really needs some fuel cell testing of catalysts.
- Some interesting results and observations.
- Steady progress.

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

This project was rated **2.4** for technology transfer and collaboration.

- Not clear why the project has difficulties finding partners for large scale testing. Some explanations were provided.
- The PI could interact with other universities to gain further insight into the mechanism and kinetics.
- External interactions appear to be focused on testing and evaluation.
- PI needs to strengthen / increase collaboration with other institutions.
- Project shows good number of publications, including two patent disclosures.
- Good collaborative effort, but should get better engaged with industry partners. Not as involved with GM & E-TEK this year.
- Early days yet. Collaboration will definitely help with the assessment of durability in MEAs.
- Technology transfer did not appear to be done in this last reporting period.
- Needs to provide path forward for tech transfer.

Question 5: Approach to and relevance of proposed future research

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

- Focus on Pt; avoid venturing in non-Pt yet. The support is complicated as it is.
- Future effort should include effort to quantify mechanism with kinetic and quantum mechanical models.
- The models would provide validation of the mechanism (temperature and concentration dependence) and guidance on how to enhance materials and improve sulfur tolerance.
- It is not clear if the project will be able to overcome the technical barriers in a timely manner.
- Future work focused on DOE goals.
- Profitable areas of future work have been well identified by the PI.
- Future work appears to be well-planned and has good mix of chemistry, electrochemistry, and analytical methods.
- Not sufficiently addressing how to build on her understanding and extend lessons learned.

Strengths and weaknesses

Strengths
- Good potential for catalyst development. The novelties have to be exploited further.
- Fundamental approach to reducing platinum loading.
- Project results related to the electrochemical properties of the new catalyst.
- Technology transfer: good number of publications.
- Goal focused.
- Addressing one of most important challenges – cost.
- Approach is based on sound, proven technology i.e. oxides or metals supported on oxides- greater prospects for success.
- Good approach with lots of characterization.
- Very conscientious PI.
- Essentially fundamental project with results that the community can learn from.
- Approach is not the usual one, but we need this type of creativity to make progress.

Weaknesses
- It seems that the project was slightly defocused in 2005.
- Should compare activity of catalysts on catalyst surface area basis.
- Project relationships with industry and other research institutions.
- No definition of the percent of completion.
- Project activities have drifted away from industry partners in last year.
- Progress to cost and durability goals not well addressed in presentation.
- Need to progress soon to assess durability, particularly since stability/oxidation issues already identified. Do not claim10x reduction in Pt until durability confirmed.
- Needs to provide path forward to taking this essentially fundamental approach and taking the steps necessary to see it to practicality - use in fuel cells.

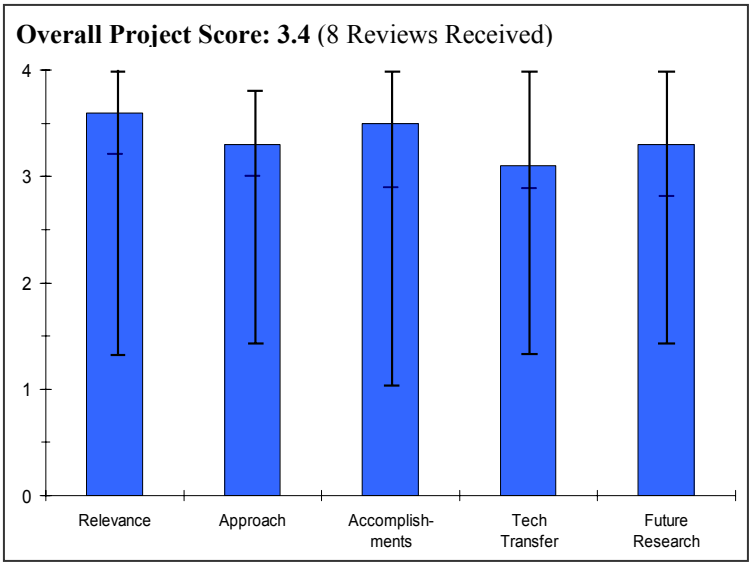
Specific recommendations and additions or deletions to the work scope

- The project should focus on the synthetic aspects of new low Pt catalysts based on the variety of substrate materials. In-depth materials characterization should be limited for the most successful catalysts and mechanistic ORR considerations on these less understood catalysts should be delayed.
- Re-connect with industry partners.
- Work with MEA suppliers.
- Future work outlined in the presentation is appropriate, and overall this project is good value for money spent. Later in the project, need to have any claimed cost reduction independently verified, i.e., what $/kW catalyst cost are we looking at?
- The program should be continued even though some difficulties are evident. The approach is very creative.

Project # FC-17: Low Pt Loading Fuel Cell Electrocatalysts
Adzic, Radoslav; Brookhaven National Laboratory

Brief Summary of Project

The purpose of this Brookhaven National Laboratory (BNL) project is to develop low platinum-loading electrocatalysts. The objectives are: to demonstrate the stability of the Pt monolayer electrocatalysts for O_2 reduction in fuel cell tests (milestone experiment), to further the understanding of the properties of Pt monolayer electrocatalysts, to improve the activity of Pt monolayer (Pt/Pd/C) electrocatalysts, to improve the syntheses of electrocatalysts with ultra-low, or no Pt content: Pt/Au/Ni and Pd_2Co, and to test them in fuel cells (milestones 2004) and to explore a novel class of electrocatalysts for O_2 reduction consisting of mixed monolayers of Pt and late transition metals.



Question 1: Relevance to overall DOE objectives

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

- Achieving lower Pt loadings with cheaper electrode materials at higher fuel cell performance is critical to cost effective fuel cells.
- Platinum remains key cost. Program is well aligned with goals and objectives.
- This effort is facilitating a fundamental understanding of low catalysts loading-essential to achieving long term goals.
- Further reductions in noble metals required are necessary to meet automotive cost targets and could be beneficial for other applications with less aggressive cost targets.
- Directly addresses catalysts Pt loading and durability improvement goals.
- Critical route to cost reduction.
- Innovative approaches to high activity with low loadings of Pt group metals are needed if fuel cells are to meet automotive cost goals. This project investigates several such approaches.
- The work helps to establish how little of an alloying element (or Pt) is needed to give the full activity of a properly-prepared alloy catalyst.

Question 2: Approach to performing the research and development

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

- A number of interesting material options are being investigated - Monolayer Pt/Pd/C, Pt/Au/Ni and Pd_2Co - that minimize materials usage or are lower cost. Electrochemical characterization of the potential of these materials is good for the scoping exercise to date.
- In the future would like to see more confirmation of results achieved so far on realistic fuel cells of the most promising candidates in terms of activity and stability to ensure that this is not just a laboratory curiosity. Since materials scale up is not a problem, this should not be that difficult.
- Very good combination of theory, experiment, and scaling to single-cell evaluation to validate findings.
- Approach is solid. Uses facilities, technique and resources appropriately.
- It is clear what theoretical considerations are used to determine the materials that are synthesized and tested.

- However, fuel-cell testing appears to have been limited to steady-state holds, to date. This is unrealistic, especially for transportation applications.
- Monolayers are a good approach to addressing Pt loading.
- Approach is sound.
- The catalyst synthesis methods are not robust (following reproducible conditions using well-characterized starting materials.) There needs to be discipline here, assuring that catalyst preparations give reproducible results. A series of experiments with the same catalyst formulation done repeatedly is needed to convince. (So many other factors I in the MEA formulation may also affect results).
- Overall approach is excellent, particularly the pursuit of monolayer catalysts first in smooth, often single-crystal, electrodes, then in supported systems.
- Biggest weakness is too much emphasis on Pd as essentially a support. One cannot assume that Pd would remain cheaper than Pt, and while a few sources of Pd are largely independent of Pt, large-scale use of Pd would mean most of the Pd would come from the same mines, all in a geographically-limited area, that produce most Pt.
- Pt/Au (Ni) work shows a possible way to avoid the use of Pd while maintaining the advantages of the Pt monolayer catalysts. Relatively little Au seems to be needed. Au is always cheaper than Pt, and Au comes from more widely-distributed sources.
- Nice blend of theory and experiment. Explanation of different tafel slopes for monolayer and mixed-monolayer catalysts vs. bulk Pt is especially thought-provoking.
- It would be good to do the RRDE work at 60ºC rather than 25ºC to make the data more directly relatable to fuel cells.

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

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

- Achievements of 20x improvement in Pt activity in PtRe/Pd/C and in Pt/Au/Ni/C catalysts that have been validated by preliminary fuel cell test results at LANL is a solid achievement and confirms that this is a promising approach.
- Precious metal content and lifetime goal achieved.
- Verification by third party (LANL) key milestone.
- Excellent progress. Expanding and building on past. 3000 hour test lends credibility to approach.
- (3000 hr) durability test protocol needs to include stress factors, consistent current testing is not sufficient to evaluate stability.
- Project has good grasp of what needs to be adjusted and has shown excellent progress.
- Activity improvements are impressive.
- Interesting results, but other precious metals are involved (Ru, Pd Ir) which are also costly.
- Pt/Re/Pd data are exciting.
- Activity per mass of total noble metal appears excellent, particularly for the Pt/AuNi system.
- Long-term (3000 hr) stability of Pt/Pd/C seems a bit questionable- 140 mV is a lot to lose.
- Demonstration of mixed-monolayer effect suggests a design rule for alloy catalysts.
- It should be noted that achievement of 2005 target of 2.7gPt/kW is not anything that special. Normal catalyst systems are already bettering that value (status in roadmap is now below the historical 2.7 figure).

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

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

- Good interactions established with LANL and Plug Power but would like to see fuel cell testing enhanced in the future. Are Pt monolayer materials survivable in a fuel cell environment? Can the OH-OH coverage in the Pt monolayer catalysts be done reproducibly?
- Agree with last year reviewer's comments that there should be faster tech transfer. This type of activity will establish what the real problems are with these materials. Until this is done these interesting results are laboratory curiosities.

- Plug Power interaction applauded.
- LANL role is critical to success of program.
- Addition of Battelle is noted.
- He has fully engaged appropriate "partners."
- More appropriate industry collaboration would be with companies that make catalysts, rather than fuel-cell OEMs like Plug Power and GM.
- Numerous papers and presentations have resulted from this project.
- Time to team with a quality electrocatalyst supplier and prepare sufficient quantities of several of these materials for extensive testing.
- Collaboration with LANL seems to be working very well. Industrial interactions on cathode catalysts have started. Plug has tested an anode catalyst.

Question 5: Approach to and relevance of proposed future research

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

- The small $330K budget should be increased to allow more fuel cell testing of the activity and stability of these promising electrode materials. If this is not possible, the 2006 effort on 4 candidates should be concentrated on the 2-3 most promising candidates with more fuel cell testing at companies like a Plug Power or a DuPont. 2006 work plan is not focused enough with the current budget and testing resources. Faster tech transfer is needed.
- Strongly endorse PI's suggestion of OCP and potential cycling tests.
- Suggest adding post-mortem diagnostics (ECSA, imaging, XRD, etc) to confirm that reported 21μV/hr decay rate with constant current is due to non-catalytic processes.
- PI did not indicate preparation scale, but suggest achieving 100 g level.
- Determine Pt content after test. Post-mortem analysis should be performed.
- Non-PM is most consistent with DOE MEA target.
- Plans are clear and make sense.
- Potential cycle testing is essential.
- Promising catalysts (those with best Pt utilization) need to be tested thoroughly.
- Should try to wean from dependence on large amounts of Pd sooner in timeline.
- Intention on voltage cycling testing of monolayer catalysts is just what is needed.
- Don't spend an excessive amount of time on PdCo unless % Pd can be driven very low.
- Some concerted effort on trying to scale up the UPD exchange monolayer synthesis process is needed.

Strengths and weaknesses

Strengths

- A number of interesting material options are being investigated - Monolayer Pt/Pd/C, Pt/Au/Ni and Pd_2Co- that minimize materials usage or are lower cost.
- Impressive results on all 4 candidate approaches.
- Achievement of 20x improvements in Pt specific activity in PtRe/Pd/C and in Pt/Au/Ni/C catalysts that has been validated by preliminary fuel cell test results at LANL is a solid achievement. Confirms that this is a promising approach.
- Preliminary fuel cell validation of promise of these materials.
- Strong theoretical underpinnings provide good basis for design of catalysts.
- LANL test relationship critical and an important partner.
- Solid team. Technically strong. Facilities/ resources rich.
- Good approach (e.g. not combinatorial, but driven by theoretical considerations) and good progress.
- Experimental plan has good theoretical base.
- Good insights on the origin of enhanced ORR activities. This can cure the fixed mindset that a bulk alloy phase, preferably with long-range order, is needed.

Weaknesses

- Good interactions established with LANL and Plug Power but would like to see fuel cell testing enhanced in the future. Are Pt monolayer materials survivable in a fuel cell environment? Can the OH-OH coverage in the Pt monolayer catalysts be done reproducibly?
- Although PI has achieved g/kW goal due to the ultra-low PM content, should also remember that there is a power goal as well, namely 0.8 V at 0.4 A/cm^2: encourage additional work to boost voltage at 0.4 A/cm^2.
- New and additional fuel cell testing. Not enough funding.
- Keep in mind that the relative prices for Pd and Pt change with relative demands. Always try to minimize total noble metal content, or at least the total platinum-group metal content.

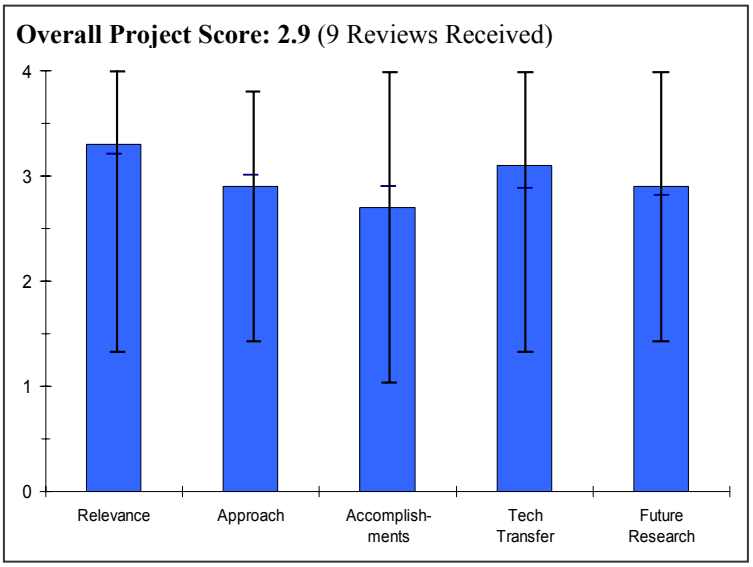
Specific recommendations and additions or deletions to the work scope

- The small $330K budget should be increased to allow more fuel cell testing of the activity and stability of these promising electrode materials. If this is not possible, the 2006 effort on 4 candidates should be concentrated on the 2-3 most promising candidates with more fuel cell testing at companies like a Plug Power or a DuPont. 2006 work plan is not focused enough with the current budget and testing resources. Faster tech transfer is needed.
- Although "catalyst manufacture" is not part of BNL mission, suggest scaling best catalyst to 100 grams to confirm PI's assertion that process is "straightforward slurry chemistry." (PI did not indicate current prep scale).
- With partner LANL, work on boosting cathode performance to DOE target at 0.8 V of 0.4 A/cm^2.
- Should continue to explore new catalysts. Expand Pd effort.
- Include repeated potential cycling tests as part of durability screening of promising candidates.
- These data are well enough along to begin a testing program documenting performance and durability. Such testing requires well-characterized catalyst preparation and careful single cell measurements.
- Cutting the platinum requirement by a factor of 4, as might be possible, is a very important task.
- Do small-scale development of the processes needed to make 50 cm^2 MEAs and, eventually, short-stack quantities of catalysts.

Project # FC-18: Development of a Low-Cost, Durable Membrane and MEA for Stationary and Mobile Fuel Cell Applications
Foure, Michel; Arkema Chemicals

Brief Summary of Project

The objective of this Arkema Chemicals project is to develop low-cost, high-durability membranes by optimizing chemistry and process, validating scale-up, developing membrane electrode assemblies (MEAs) based on these membranes, optimizing MEA for new membrane, validating MEA performance, and validating the MEA performance in single cells and in stacks.



Question 1: Relevance to overall DOE objectives

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

- Project addresses key objectives in the HFCIT R&D plan.
- This project addresses cost and durability, which present two of the most significant barriers identified by the DOE program.
- Operation at low relative humidity (25-50% RH) and elevated temperature is desirable.
- The relevance to the program is good and the focus is on achieving the program's performance goals both in cost and endurance.
- Initial choice of Kynar and polyelectrolyte-simplistic. Affordable, durable, high-performance membranes are key to achieving fuel cell cost and durability targets.
- Strong project team.
- Lower-cost, high durability membrane development is one of DOE's key objectives.
- Durability questions need to be addressed in parallel with development activities.

Question 2: Approach to performing the research and development

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

- Approach is reasonable given the fact that they are trying to understand the weakness (degradation) they found in their membrane product.
- Approach could lead to very inexpensive membranes.
- Lack of detail and data makes it difficult to assess whether the specifics will allow them to get there.
- Only durability target listed is 5,000 hours; also need to consider 40,000 hours for stationary applications.
- PVDF is partially fluorinated. Is it as stable as fully fluorinated polymers? (PTFE & PFSA polymers)
- The approach is well thought out but the presentation didn't give enough chemical structural details to be able to evaluate the actual strategies being used and the chemical viability of the approach.
- Approach has improved, with more emphasis this year on understanding degradation mechanisms to guide further development.
- Not clear that membrane is being tested under benign or extreme operating conditions. Key DOE targets require high temperature, low RH membranes.
- Approach is logical.
- Process scaled to pilot plant.
- No mention of potential membrane material or manufacturing costs or MEA costs.

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

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

- Durability testing needs to include stress factors.
- Good idea to start- looks like it doesn't work due to polymer degradation. Presentation lacked information on plans to resolve fundamental polymer limitation.
- Membrane shows good initial performance.
- Have made progress in durability.
- No data was provided for low humidity conditions.
- No new endurance data.
- Significant improvement in life is needed; M31_2 reached End of Life (EOL, defined by DOE as 10% loss compared to Beginning of Life (BOL)) after 1,500 hours while operated in constant current mode; DOE goal is 40,000 hours and 2004 Status is 9000 hours (MY Plan pages 3-86 and 3-88, foot note c).
- Open Circuit Voltage (OCV) on H_2/O_2 is low (0.85-0.9V); could be increased by up to 0.1.
- Significant progress has been made in identifying the mechanism for performance degradation.
- Good progress in learning
- Good postmortem failure analysis on finalized samples.
- Clearly need to extend polymer durability, but team seems to have a good plan to address.
- Good quality data generated.
- Project scheduled to conclude in two years and appears on track.

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

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

- Not really working with major players who would implement technology if successful.
- Collaboration with Oak Ridge should allow for increased understanding of degradation mechanism.
- ORNL is valuable addition to the team.
- No results presented from Georgia Tech work.
- Georgia Tech capabilities appear not to be fully utilized (water sorption measurement, mechanical properties, and spectroscopy?
- There are good coordination efforts with team members that are beneficial to the progress of the effort.
- Development not ready for tech transfer.
- Partners are helping identify/quantify degradation mechanisms to the overall benefit of the project.
- Makes good use of collaboration opportunities.
- Partners are full participants in the work.
- Good coordination and assignments of tasks.
- Dissemination of non-proprietary results through additional publications recommended.

Question 5: Approach to and relevance of proposed future research

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

- Proposed approach will help understand degradation.
- Details on the chemistry of sulfur release and plans for ameliorating it are vague.
- Should specifically address milestone 51 from MY RD&D Plan (2000 hour test with advanced membrane).
- The future work plan is well organized and thoughtful. The focus is appropriate.
- Future work plan is improved.
- Should evaluate whether project funds are best spent on process scale-up, or whether further material improvement is needed.
- Should consider higher voltage (OCV) testing to find problems caused by chemical attack.
- Reasonable focused plan.
- High temperature operation is not the primary focus of research; however, it should be looked at, briefly.

Strengths and weaknesses

Strengths
- Polymer chemistry, reasonable beginning-of-life performance.
- Good concept and good initial performance.
- Collaborations in place should allow for further advancement.
- Interesting approach to novel low cost membrane.
- Analysis of membrane degradation mechanisms.
- Lack of fluoride generation, lower gas permeability and promise of low hydroperoxyl radical formation.
- Demonstrated learning.
- Large number of ex-situ tests used to characterize membranes.
- Overall well thought-out and managed project.
- Innovative approach.
- Strong team.

Weaknesses
- Lack of collaboration with major players (Ballard, Gore, 3M, Dupont) who would implement this technology.
- Have not provided enough data to show that they can decrease sulfur losses to acceptable levels.
- Data showing increased conductivity at low RH were not provided- the ability to operate at low RH is a key goal for DOE.
- Progress appears to be slow.
- Only new cell data reported concerns membrane conditioning procedure.
- Reason for testing under pressurized conditions not apparent; recommend testing at atmospheric pressure.
- No data presented for conductivity or cell performance at low hydration conditions.
- No mechanical properties reported.
- No clear advantage over Nafion in terms of performance, durability or cost shown to date.
- No discussion of membrane morphology- is it microporous or is it a dense film? PVDF is widely used in the membrane industry.
- It is possible that testing is not being done under the extreme kinds of conditions (cycling, low RH, high T, degradation at OCV). The test conditions were not made clear in the presentation.
- Project would benefit from more external dissemination of non-proprietary results (publications).
- Membrane material and manufacturing costs not discussed or compared.
- Resulting stack material and manufacturing costs not discussed or compared.

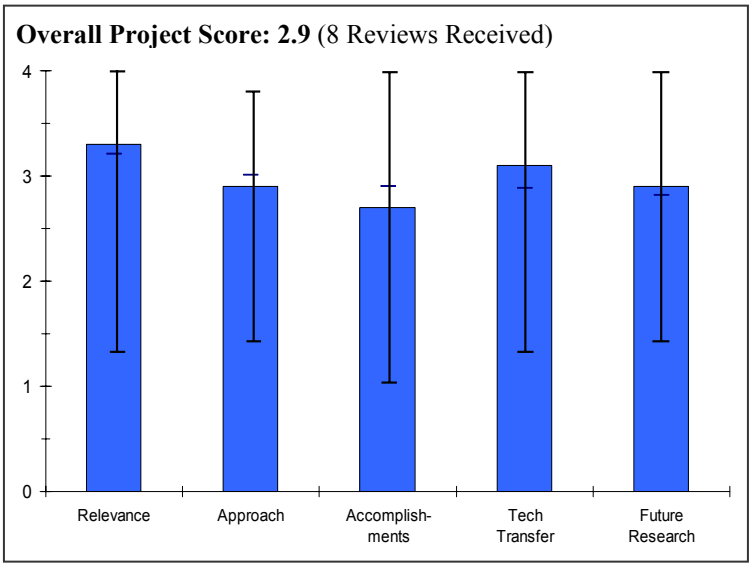
Specific recommendations and additions or deletions to the work scope

- Should utilize a durability protocol including cycling, especially when a more durable membrane is obtained.
- Test under more realistic conditions: use Hydrogen/Air (instead of Hydrogen/Oxygen) and low relative humidity (25-50%).
- Test mechanical properties of hydrated membrane (in addition to dry).
- Fluoride ion release into water effluents should also be given as fraction of total F in MEA.
- Measure and report fuel cell high frequency resistance.
- Permeability data did not include specifics on membrane area and pressure difference; hydrogen electrochemical permeability should also be given in mA/cm^2.
- List complete testing conditions for all data reported (including gas utilization, electrode loading and cell area).
- Please list units for all numerical values (including effluent concentrations.
- Has this team considered trying to crosslink the sulfonated polyelectrolyte with multivalent metals -post membrane formation. If the polyelectrolyte is acrylic based, i.e. AMPS, then suitable post formation cross linking based inclusion of other reactive acrylic monomers are indicated.
- Should consider postponing process scale-up.
- Continue with membrane durability studies.
- May want to consider accelerated membrane testing.

Project # FC-19: Development of High-Performance, Low-Pt Cathodes Containing New Catalysts and Layer Structures

Atanassova, Paolina; Cabot Superior MicroPowders

Brief Summary of Project



This is a four year project led by Cabot Corporation to develop and apply 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 in 2005. Specific objectives include completing the development of rapid testing equipment – Dupont Fuel Cells; starting high throughput synthesis of ternary alloy compositions in a discovery mode; 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 **3.4** for its relevance to DOE objectives.

- Identification of low pt catalyst is key to low cost fuel cell goal.
- Well focused and relevant project.
- Further reductions in noble metals required are necessary to meet automotive cost targets and could be beneficial for other applications with less aggressive cost targets.
- Combinatorial approach might yield new information that would allow lower Pt loading.
- Important problem.
- Cost appears to be emphasized in turn minimizing other key factors like performance and durability

Question 2: Approach to performing the research and development

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

- Searching some ternary catalysts may be a good approach towards finding the active ORR catalysts.
- It will be more difficult to optimize for durability, than identify the best catalysts out of all possibilities. Care should be taken not to reject durable candidates in the initial screen for better activity.
- Good approach in that it is a unique process/fabrication method that provides an alternative to the standard methods for making dispersed catalysts. Combinatorial capability makes a lot of sense.
- Focusing on ternaries is also a good focus point given the known literature results.
- It is not clear if the carbon support technology is also being screened, or if their approach is limited to coating on the gas diffusion layers.
- Rapid screening is a good experimental approach. Needs theoretical approach also to identity high performance catalyst.
- It is not clear whether theoretical considerations have any bearing on deciding what materials are screened. Despite speed of screening, some theory could improve efficiency of this program.

- To date fuel-cell testing appears to have been limited to steady-state conditions. Decay rates are high for steady-state conditions. Do these numbers reflect recoverable decay?
- Must include repeated potential cycling testing to assess durability.
- Interesting combinatorial-high thru-put screening approach based on carbon supported catalyst produced by spray pyrolysis. PtM1M2 library covers many systems already studied but also covers new regions.
- Is there a chance that this approach might only lead to conventional catalysts prepared by an alternative method?
- What advantages will be derived from this alternative approach.

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

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

- A very good improvement in activity was accomplished with the ternary catalysts. Their long-term stability has to be carefully evaluated. The origin and duration of the effect of leaching should be established.
- Excellent progress in ternary selection and demonstration of performance in MEA. Good advances in pretreatment and characterization.
- Several hundred catalyst types being screened in the past year is good progress. Surprising there is not more variation in activities with composition of the multiple libraries. Two fold increases in activity for alloys is consistent with that seen with alloy made by conventional methods.
- Progress on improving performance has been steady since start of the contract, validating the approach. Performance, catalyst utilization and durability still need improvement.
- Given that over 500 samples have been made and screened the quantity of work is stupendous. Good progress.
- Must show activity normalized per mass of total noble metal, not just Pt mass. This is essential since the identity of the alloying elements is not disclosed (i.e., no way to assess ultimate material costs of compounds).
- Need to differentiate between non-recoverable and recoverable decay in "long-term" testing. Recoverable decay can be recovered by simply reducing the cathode to the anode potential for a brief time.
- Claims to have found new compositions of PtM1M2 ternary catalyst, but activity lies in band of Pt3Co now reported by several labs. Not clear there is really anything new.
- Solid progress toward stated goals but not much toward real DOE targets.

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

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

- An effort to enhance collaboration has been made.
- Need to build relation with Dupont to understand all durability issues especially electrode/membrane interactions.
- Close working relationship with a primary membrane supplier.
- The interaction with Dupont is good and obviously beneficial.
- Although presenter said she explained it, I did not understand the role of Dupont.
- Who did the high thru-put screening?
- Substantial work on vertically integrating this effort.

Question 5: Approach to and relevance of proposed future research

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

- The tests under potential cycling conditions should be started soon.
- Need to build a battery of durability tests including H_2O_2, F^- to study the effect of the catalyst on membrane and overall catalyst life time.
- Opportunities to alter or modify the carbon support materials in the CSMP process should be a part of any catalyst durability studies. Much is already known about the carbon's limitation against oxidation at high temperature and high voltages. Data is also appearing now on the limitations of fine dispersed catalysts under

cycling conditions. Their future work indicates that they will continue to screen and characterize the catalysts, but it was not clear how their approach will minimize or improve on any of these fundamental limitations.
- Good plan for the future.
- Unclear what the compounds are or if they are cost-effective, difficult to judge future plans here.
- Cycle testing is essential.

Strengths and weaknesses

Strengths
- Production capabilities.
- Expertise in powder synthesis and characterization collaboration with fuel cell membrane supplies.
- Significant capabilities and expertise in catalyst fabrication. Close working relationship and alignment with a primary membrane supplier. Novel catalyst synthesis/deposition method.
- The ability to scale up is a strong point. The existing partnerships are fruitful.
- High thru-put synthesis of catalysts in the same form as they would be used in cells.
- Excellent technology.
- Significant effort to involve MEA makers.

Weaknesses
- Closer collaboration with GM or other fuel cell developer needed to understand durability issues.
- Not clear, but if the CSMP approach is limited to catalyst deposition on the GDL to form GDE's rather than coating on the membranes to form CCM's, then they may be fundamentally limited in reaching higher levels of catalyst utilization.
- The Edisionian approach can be both a strength and a weakness depending on what input is being used to guide the "parameter space" being screened. I would hope that the PI is keeping a close eye on the other catalytic research results.
- Approach (e.g. combinatorial) does not appear to be influenced by theoretical considerations. Progress not clear since total noble metal data are not provided. No way of knowing if progress against cost has been made.
- It doesn't appear that they have found anything new with the library used.

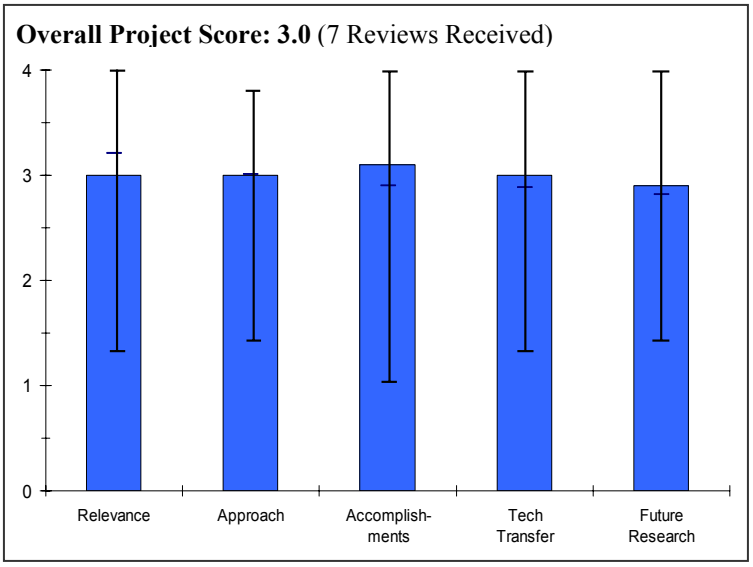
Specific recommendations and additions or deletions to the work scope

- Include repeated potential cycling tests as part of durability screening of promising candidates.
- Improvements in mass-transport regime may be due to the removal of trace foreign cations that reduce conductivity of the ionomer in the catalyst layer. Reduced ionic transport in the catalyst layer will result in similar results, although it can be diagnosed by the dependence of performance on oxygen partial pressure (e.g., see: M. Perry, J. Newman and E. Cairns, Journal of the Electrochemical Society, Vol. 145, No. 1, pp. 5-15, 1998).
- Why not repeat library with a different support, i.e. an electronically conducting oxide.
- This approach is an elegant one for synthesizing/preparing catalysts but they need an infusion of ideas to provide something worth making. Perhaps the investigators can team up with some other partners who are making interesting materials. They should also think through what particular advantages accrue to their approach in the context of improving the cathode.

Project # FC-20: Platinum Recycling Technology Development
Grot, Stephen; Ion Power, Inc.

Brief Summary of Project

This Ion Power, Inc. project will assist the DOE in demonstrating a cost effective and environmentally friendly recovery and reuse technology for Platinum Group Metal containing materials used in fuel cell systems. The initial objectives include development of lab scale processes for solubilizing catalyst coated membranes, development of a lab scale processes for catalyst and ionomer materials, development of test methods to determine vitality of the recovered materials, and partnering with the key stakeholders in this technology area.



Question 1: Relevance to overall DOE objectives

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

- This is good and necessary work that needs to be done to answer questions that will ultimately be. However, it is not so critical an aspect of cost reduction as some other things and that is the only reason for the 2.
- Development of a cost model is also necessary, because cost should be a primary driver of this project to meet DOE Target.
- Platinum Group Metals are a major cost factor in PEMFC; recycling of PGM is needed for a sustainable PEMFC industry.
- Directly addresses HFCIT Multi-Year RD&D plan milestone 55.
- Viability of high efficiency recovery of platinum has been demonstrated convincingly.
- Recycling and lifetime are important to meet fuel cell targets.

Question 2: Approach to performing the research and development

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

- Size of autoclave is good – large enough to demonstrate practicality. It is a good alternative to burning.
- Questionable whether used PFSA will ever be useful for direct recycling as PEM for fuel cells, but important to verify one way or the other. It may not be performance only but water management behavior and durability of recycled PEMS as well that will need to be evaluated before a decision can be made whether it is viable to reuse the material for a fuel cell membrane. More basic properties should be measured.
- Sound approach to ionomer recycling.
- Not clear if suggestion is to reuse carbon supported catalysts, or just recover the PGM; if it is the former, much more in-depth analysis than reported catalyst vitality test is needed.
- Autoclave reactor has provided an HF free alternative to classical combustion i.e. environmentally friendly objective has been achieved.
- Keep the focus on catalyst recovery. Do not waste time on membrane recovery.
- Need to be aware of the energy cost of recycling.
- Good idea to recycle both catalyst and membrane.
- Autoclaving will solubilize Nafion-like materials.
- Catalysts may be useful exactly as recovered, just like the polymer is useful. (Of course the supported materials may need some cleanup steps.)

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

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

- Needs to investigate chemical structure change in recycled ionomer and durability of recycled membrane in MEA. Good demonstration of the performance of a re-manufactured fuel cell membrane.
- Beginning of Life (BOL) performance of MEA with re-manufactured membrane is close to that of MEA with new membrane; degradation rates of re-manufactured and new MEAs should be compared.
- Good recovery rate of starting materials.
- MEA recycling cost model should be refined.
- Platinum recovery excellent, but recovery and re-use of membrane material are only of marginal value.
- A good start to initially defining an approach to fuel cell recycling.
- Good progress. Need to keep in mind external factors that would affect the overall efficiency of the recycling process.
- Reuse of Nafion is an interesting result.

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

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

- Good list of collaborators and mix of large and small companies, industry and academia.
- Need to include manufacturing in cost (financial)/recycle model.
- Added a number of good partners for obtaining End-of-Life (EOL) MEAs.
- Partnering with catalyst manufacturer would strengthen EOL catalyst characterization and re-manufacturing/recycling.
- Good broad-based partnering.
- Need to establish collaboration with stack manufacturers/suppliers. The wider the experiences the better you will become at addressing recycling.
- Right organizations are on board.

Question 5: Approach to and relevance of proposed future research

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

- The economic analysis will be important. But rather than strive to show a stack running on re-manufactured MEA's using recast membranes, (i.e. functional tests) it would make more sense to characterize the fundamental properties of the membranes to see how those have changed – e.g. EW, water uptake versus water activity, conductivity versus lambda (water molecules per sulfonate group), mechanical properties like puncture strength, durability in Fenton's reagent, ICP analysis of impurities, etc.
- Not clear if re-manufactured MEAs will contain re-manufactured membrane with new catalyst (as in this presentation), or recycled catalyst will be used.
- Focus on concluding/ optimizing the platinum metal recovery process and determining process economics.
- What is the value of conducting a remanufactured MEA? Especially when all MEAs are different in shapes, thicknesses, size, etc?

Strengths and weaknesses

Strengths
- Also recycles ionomer, which has the potential to lower MEA cost.
- Good membrane recycling process.
- Primary objective of high content platinum recovery has been achieved using environmentally friendly process
- Demonstrated feasibility of recycling of both membrane and electrode.

- Data is good enough to provide input information about end-of-life value and then to include those numbers in cost analyses. Most likely most of the precious metal cost can be recovered, and that could impact FCV costs. (This must be similar to nickel metal hydride batteries, again a system where at end-of-life the metals, which contribute to cost, are recycled with good return of value).

Weaknesses

- Emissions from catalyst combustion not addressed in presentation.
- Purity of Pt recovered after combustion is not known.
- Pursuit of re-usable membrane polymer is not justified. No fuel cell manufacturer would give a warranty on a fuel cell system that used reclaimed membranes.
- Recycle targets may change in future as technology develops.
- Need to establish collaboration with all MEAs supplier/manufacturers.
- Need to establish collaboration with all stack supplier/manufacturers to understand disassembly techniques. It may affect recycling.
- Need to consider some concepts for other membrane materials, in additional to Nafion. Recycling costs might impact future membrane selection.

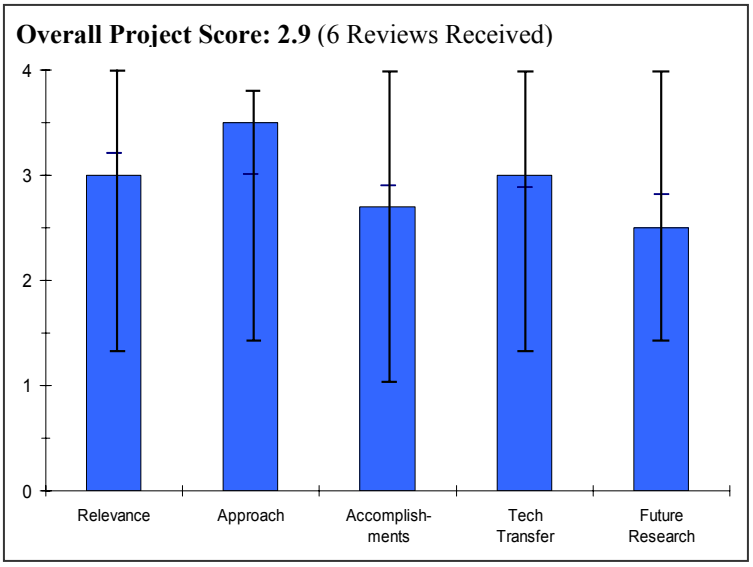
Specific recommendations and additions or deletions to the work scope

- Suggest endurance testing MEAs with cast membranes made from virgin Nafion and re-manufactured Nafion under the same conditions to determine whether EOL performance is similarly close as at BOL
- Suggest addressing recovery of Pt alloy catalysts, such as PtRu/C.
- Suggest elemental analysis of virgin and re-manufactured Nafion to determine effectiveness of filtration in removing contaminants.
- Suggest elemental analysis of recovered PGM and estimation of cost to purity to catalyst manufacturer specifications.
- Indicate net cost savings i.e. PEM value minus recycling process costs. Proposed ageing and rebuilding of 5kw size fuel cell stack is of limited value.
- Some interaction with chemical processing and polymer producer's actual processes should be valuable here.
- Need to establish collaboration with all MEAs supplier/manufacturers.
- Need to establish collaboration with all stack supplier/manufacturers to understand disassembly techniques. It may affect recycling.
- Need to reconsider the value of conducting a remanufacture MEA? Especially when all MEAs are different in shapes, thicknesses, size, etc.
- Might consider techniques to "clean up", "restore", etc. a PEM fuel cell without tearing up the device and regain original performance. This could involve removing impurities from the MEA, like, for example, chloride ion. May want to evaluate rebuilding compared with recycling

Project # FC-21: Platinum Group Metal Recycling Technology Development
Shore, Larry; Engelhard Corporation

Brief Summary of Project

This Engelhard Corp. project will examine 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 Platinum (Pt) and Ruthenium (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 precious metal (PM) recovery from metal monoliths, which are used in stationary reformers and potentially in hydrogen production, will be evaluated.



Question 1: Relevance to overall DOE objectives

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

- Very good and necessary work. However, it received a 2 rating because it only addresses one cost reduction barrier.
- Extremely good progress made to the realization of objectives.
- High platinum recoveries will be required to launch the fuel cell industry on a large scale.

Question 2: Approach to performing the research and development

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

- Could the recovered ionomer be reinserted at some point in the membrane synthesis steps? Approach seems limited to extraction of Pt/C from catalyst coated membranes. What about catalyst coated on the gas diffusion layer (GDL)?
- Good assessment that in reality the wide variation of MEA components and constructions may make any one approach limiting. Sole focus on recovering Pt for refining and ionomer recovery for some type of recycling makes sense.
- Good use of partners to address different approaches.
- Comparative evaluation of the three alternative processes very valuable and informative.
- Keep the focus on catalyst recovery. Do not waste time on membrane recovery.
- Keep the awareness of the negative side effects of recycling.
- Need to be aware of the energy cost of recycling if using microwaves.
- Multiple approaches to removing the platinum from the membrane is a good strategy.

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

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

- Good progress in that definite conclusions have been reached on relative capabilities of several unique approaches for catalyst recovery as apposed to a pyrometallurgical approach.

- Only completion of work on delamination of electrode materials from PEM in past year seems to be a low rate of progress.
- Why is it necessary to wait to get an aged lot of MEA's from Greenlight – aren't these available already?
- The program should move along faster. Downselection of best approach and focusing of resources on one process looks like best approach.
- High pt energy (>98%) very effective demonstration.
- Good progress. Need to keep in mind external factors that would affect the overall efficiency of the recycling process.
- The key element in the project is the elimination of HF emissions, but there was no discussion about how this is being accomplished.
- Good analyses of exhaust gas identified other items of concern in the exhaust gas, but again, not clear how these will be contained.

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

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

- Should consider MEA's in which the catalyst is coated on the GDL also before bonding to the membrane, and work with a supplier for these.
- Good group of partners.
- A broader based industry partnering would be beneficial.
- Need to establish collaboration with all MEA manufacturers. The wider the experiences the better you will become at addressing recycling.
- Could establish greater range of companies to supply materials for reclamation to determine applicability across wide range of makers.

Question 5: Approach to and relevance of proposed future research

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

- Consider working with a membrane supplier to evaluate if the ionomer recovered can be reused for making MEA's, even if it is a mix of different starting monomers.
- Also, an economic analysis of the various processes was not explicitly stated to be an objective of the project but should be seriously considered for one or more of the methods under test.
- Select preferred process for pt recovery, optimizing it and then establish process economics.
- Need to conduct recycling efforts on major MEAs from Ballard, UTC, etc.
- Not clear what the approach is going to be to eliminate HF emissions.

Strengths and weaknesses

Strengths
- Key expertise to understand the viability of the PGM recovery.
- Strong partners and well laid out approach.
- Development of emission free pt recovery process is within sight.
- Good progress and is aware of the potential negative side effects.

Weaknesses
- Rate of progress appears to be slow, need to accelerate efforts.
- Recycle targets may change in future as technology develops.
- Need to establish collaboration with MEAs supplier/manufacturers.
- Need to establish collaboration with stack supplier/manufacturers to understand disassembly techniques. It may affect recycling.

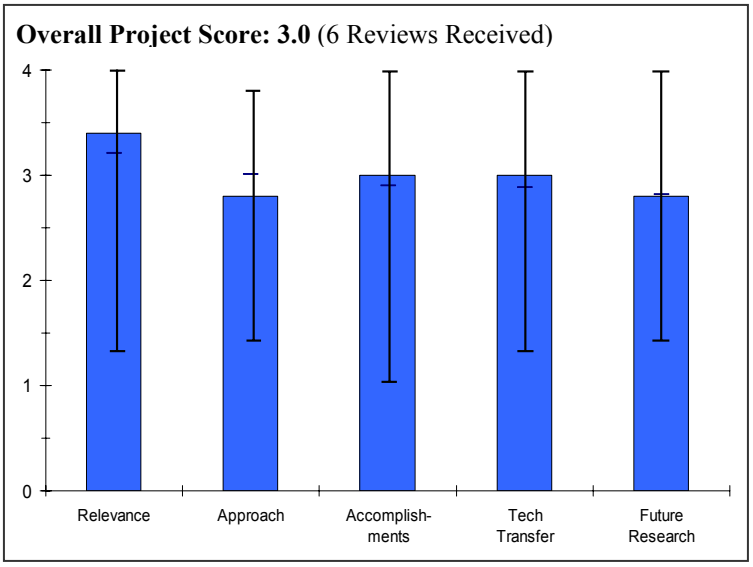
Specific recommendations and additions or deletions to the work scope

- Accelerate timeline to drive selection of best approach and focusing of resources.
- Back-up option of pyrometallurgical approach should not be dismissed because of loss of polymer, which is only of marginal value.
- If not yet done, a general economic study of solvent vs. microwave v. combustion would be informative.
- Need to establish collaboration with MEAs supplier/manufacturers.
- Need to establish collaboration with stack supplier/manufacturers to understand disassembly techniques. It may affect recycling.

Project # FC-22: Scale-Up of Carbon/Carbon Bipolar Plates
Haack, David; Porvair Fuel Cell Technology Corporation

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 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 **3.4** for its relevance to DOE objectives.

- Bipolar plate cost and performance are important drivers for PEM fuel cell commercialization.
- Project addresses stack performance and cost goals.
- An important component of fuel cell systems with PI demonstrating knowledge of cost, weight, conductivity, and mechanical performance demands.
- Low cost bipolar plates are an industry priority but presenter did not articulate the connection of work presented to objectives. Project may be relevant but speaker was not convincing.
- Affordable, design-appropriate bipolar plates are key to achieving fuel cell cost and durability targets.

Question 2: Approach to performing the research and development

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

- Porvair has scaled up bipolar plate manufacturing and is now attempting net-shape molding.
- PI aimed at development of a low cost net shape molding process that is amenable to high volume manufacturing. Validated with a robust 10kW sealed plate stack demonstration. Cost- outs of process to high volume manufacturing to be done based on learning in 10kW demo.
- No discussion of plate features or manufacturing methods.
- The "reference" plates were not described.
- Production is happening, but what is involved?
- There was no elaboration on the production cost model, why should anyone believe the cost projections?
- The approach was simply not clear.
- Approach seems unlikely to meet cost targets (bonding two monopolar plates to get bipolar plate, minimum of two pyrolysis steps, remaining questions of permeability and dimensional control).

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

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

- Porvair has met DOE's technical targets. Cost targets are more difficult but Porvair has estimated that they can be met at sufficient volumes.
- Difficult to access accomplishments based on data presented. Results presented show cumulative cost without breakdown by material and process.
- Impressive demo of 20 cell stack with performance equal or better to machined graphite. Good VI results and stability. Cost analysis indicates that low cost, high volume.
- Manufacturing is possible with sensitivity factors identified.
- No discussion of design and mfg process options or different scenarios ("investments" used loosely).
- Only one cost curve was presented, and without any substantive justification.
- Polarization curves were treated casually, and were assumed fixed- isn't technical progress envisioned?
- What about curves for comparison?
- No description of specific manufacturing outlays or investments required to build capacity.
- Though progress has been made since project inception, progress this year seems minimal.

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

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

- In addition to UTC Fuel Cells, Porvair is collaborating with developers of both stationary and automotive fuel cell stacks.
- Collaborations not discussed in this presentation. Porvair products being evaluated by stack developers.
- Work done with collaboration of a major PEMFC manufacturer, UTC, for testing validation.
- UTC is a strong partner, but this project necessitates collaborating with ALL plate users/developers.
- Reliance on single partner will bias the project.
- No commentary on how bipolar plate geometry and materials might change, project should include consultation of relevant experts.
- Collaboration with UTC good, but UTC design requirements differ from most of emerging industry.
- No other partnering attempts apparent.

Question 5: Approach to and relevance of proposed future research

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

- Description of future work was vague but followed logically.
- Project complete and Porvair efforts will be guided by developer feedback and needs.
- DOE project concluded in April 2005. With robust demonstration of technology, project is moving forward at Porvair for further scale-up. Initiated product qualification with a number of fuel cell customers. All of this is a positive indicator of project success.
- Plans for the future were not clearly explained.
- The model must incorporate much wider input, explore scenarios, and be articulated clearly to be credible.
- Project is complete.

Strengths and weaknesses

Strengths
- Success of this technology will be demonstrated by its adoption by developers.
- Impressive demo of 20 cell stack with performance equal or better to machined graphite. Good VI results and stability. Cost analysis indicates that low cost, high volume manufacturing is possible with sensitivity factors identified.
- Very promising for $4/kW.

- Making predictions of future costs is important (but it MUST be credible, and the assumptions clear).
- Project Relevance to the Hydrogen Program. Project Accomplishments.

Weaknesses

- Project directed toward commercialization of this technology. Limited public knowledge developed in this project.
- Lack of explanation of project details and ambition.

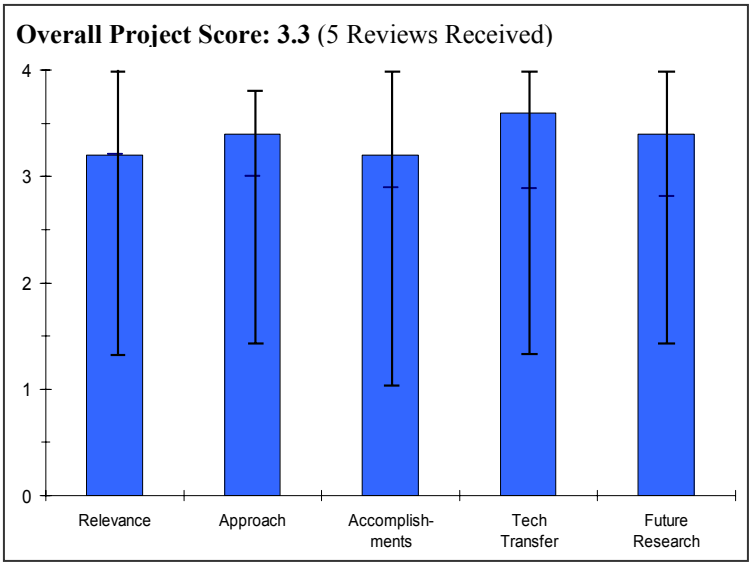
Specific recommendations and additions or deletions to the work scope

- Cost model should be a publicly available deliverable.
- Time to let the market decide if this technology is desired.

Project # FC-23: Cost-Effective Surface Modification for Metallic Bipolar Plates
Brady, Mike presented by More, Karren; Oak Ridge National Laboratory

Brief Summary of Project

Oak Ridge National Laboratory (ORNL) and National Renewable Energy Laboratory (NREL) are developing a surface treatment to protect metallic bipolar plates by thermal (gas) nitridation of Cr-containing alloys to form a pin-hole free Cr-nitride surface. Results from a post-fuel-cell-test assessment of model nitrided Ni-50Cr test plates (long term, cyclic conditions) are discussed. Work to form protective nitrides on cheaper alloys, such as commercial Ni-Cr base alloys and Fe-Cr based stainless steels, will also be pursued and the amenability of this approach to stamped foil will be assessed.



Question 1: Relevance to overall DOE objectives

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

- Low-cost bipolar plates with long-term durability are essential to PEMFC technology.
- Achieving cost effective, durable BSPs is important.
- Many organizations are working on metallic stack technology.
- The need for metal coatings and treatments for metallic fuel cells has not been confirmed. Nuvera does not encounter lifetime issues associated with metal through 5000 hr.
- Project addresses a key component for reaching the DOE objectives for fuel cells.
- Very focused project with good integration with program goals.

Question 2: Approach to performing the research and development

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

- The approach is systematic and logical.
- Infiltration approach to growing Cr-nitride overcoats is a good one and has been used in other applications such as making SiC protective overcoats on graphite susceptors for epitaxial silicon semiconductor applications.
- Testing of anode plates at GM and FuelCell Energy as well as corrosion studies provides robust evaluation.
- Nitriding process was not adequately described, in terms of mass, energy, and time (and cost) requirements.
- Not clear what the steps of process improvement are, e.g. warping challenge claimed to be overcome, but no description of how.
- The project is sharply focused on addressing cost and performance issues associated with bipolar plates.
- The approach is methodical and well-reasoned.

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

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

- Steady progress has been made although the pace of the work seems to be fairly slow.
- Gets good marks. Proof of concept established that nitrided Ni - 30Cr alloys with high quality corrosion resistance can be made and stamped (GenCell collaboration). Learning applied to more cost-effective SS alloys with pinhole-free nitrided layers made. Results are encouraging but corrosion resistance needs further improvement. Project responsive to reviewer comments.
- Cost effectiveness of nitriding process, and roadmap for achieving it, were not clearly explained.
- Connection of corrosion curves and associated test conditions to actual stacks requires more diligence.
- Showing good durability results on FCs containing treated materials is good, but group must ALSO show the reference case without treatment, otherwise meaningless.
- Why do alloying elements help? This was not explained.
- Have made significant progress and demonstrated corrosion resistance of Nitride film in Fe-Cr system.
- Have shown resistivity meeting DOE goal for 2010 of 0.01 ohm-cm^2.
- The productivity and accomplishments are good, especially considering the low funding level.

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

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

- Excellent collaborations and plan to transfer the technology to industry when the project is complete. If the project meets its objective transfer should not be difficult.
- Good interactions established with major fuel cell companies -GM, FC Energy, MTI Micro for evaluation of coated BSPs with results available now not just being talked about. POC stamping done with a company GenCell that does this for a business.
- Good, but encourage more proactivity in seeking to work with other metal stack developers, e.g. Honda, Nuvera, etc.
- Collaborations in place with University, other National Labs, automakers, and fuel-cell manufacturers.
- Good interactions and collaborations.

Question 5: Approach to and relevance of proposed future research

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

- Plans to complete the project are straightforward provided a commercial alloy meeting the nitriding requirements can be identified.
- Very promising based on results so far. Focus on Fe-Cr alloys is good with major issues being addressed. Need to also address long term interactions of nitrided BSPs with MEA.
- Process development roadmap needed.
- Well thought out and methodical.

Strengths and weaknesses

Strengths
- Builds on strong ORNL materials experience and capabilities.
- First results are encouraging.
- Evidence of ability to perform nitriding and produce sample.
- Understanding of characterization methods.
- Collaborations in place for fuel cell testing and for tech-transfer to occur to customers.
- The viability of the approach is good and should be easily scaled up.

Weaknesses

- The requirement for "minor alloy additions" was not discussed in any detail -- the cost and processing impact should be quantified.
- Corrosion improvements on Fe-Cr not yet demonstrated but to be focus of 2006 work.
- Lack of process details and cost model.
- No results confirming the need for coatings/treatments (corrosion curves do not always reflect fuel cell operation reality).
- I would like to see some alternate thinking used and tried. What about taking a stainless steel stamped part and electrodepositing Cr followed by nitriding?

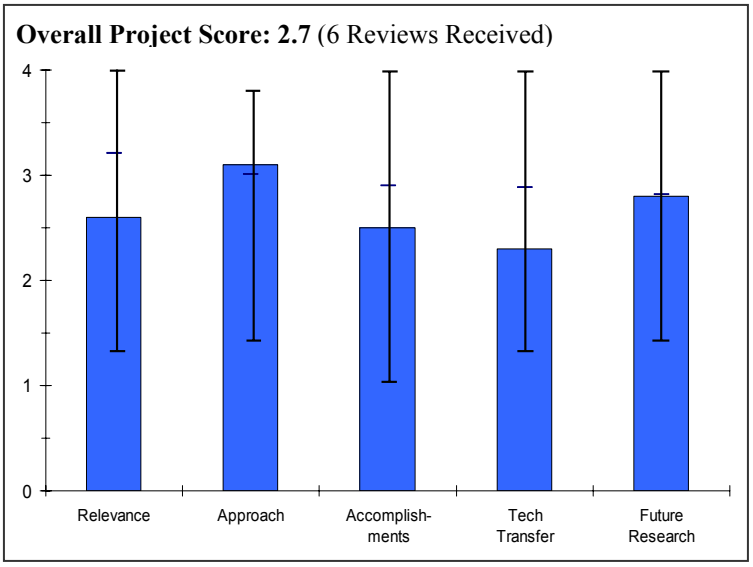
Specific recommendations and additions or deletions to the work scope

- Accelerate schedule and complete the project no later than the end of FY06.
- Continue the good work.
- Focus on cost-effective Fe -Cr alloys next year.

Project # FC-24: Water Gas Shift Catalysis
Krause, Theodore; Argonne National Laboratory

Brief Summary of Project

This Argonne National Laboratory (ANL) project will develop water-gas shift (WGS) catalysts which, when compared to Cu-Zn and Fe-Cr WGS catalysts, will be more active (higher turnover rates); less prone to deactivation due to temperature excursions; more structurally stable (able to withstand frequent cycles of vaporizing and condensing water); and more resistant to sulfur poisoning. This project is intended to improve the understanding of reaction mechanisms, catalyst deactivation, and sulfur poisoning, as well as define operating parameters (e.g., steam: carbon ratios, temperature, gas hourly space velocities, catalyst geometry) to optimize catalyst performance and lifetime.



Question 1: Relevance to overall DOE objectives

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

- The re-directed focus from on-board to off-board fuel processing limits the benefits.
- Can support off-board hydrogen production if superior catalysts to those commercially available are developed.
- Refocusing the project appears to be moving this in the right direction.
- Water gas shift catalysts are most demanding catalysts in reforming process. Current generation of commercial WGS catalysts is significant technical barrier to low cost reforming of hydrocarbons.
- Improved and cost effective WGS catalysts are important to a number of hydrogen production and fuel processing systems for PEM fuel cell systems.
- Engineering solutions exist for using commercially available catalysts for stationary systems, so the project will not have wide-scale applicability without a major breakthrough.

Question 2: Approach to performing the research and development

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

- The objectives were clearly defined.
- Good they're moving away from precious metals and beginning to look at durability.
- Good use of advanced analytical techniques to identify catalytic mechanisms.
- Good to see more focus on non-precious metal catalysts.
- Appreciated seeing comparison of activity to Fe-Cr catalysts under same conditions.
- An impressive array of characterization and modeling work is being performed to gain a fundamental understanding of WGS catalysis in the PGM/ceria and base metal systems.
- Catalyst durability is being tested routinely.
- Ruthenium and rhenium do not provide a path to low cost WGS catalysts.
- If the target application is for LPG, then sulfur is a key issue that needs to be addressed. There is no reason to expect that the base metal catalyst formulations being pursued will be even moderately robust against sulfur impurities.
- The issue of pyrophoricity is not being addressed.

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

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

- A good number of catalyst systems have been tested.
- Needs more concrete definition of objectives: performance & cost targets.
- Not clear if performance is better than commercial Cu-Zn WGS catalyst.
- Copious data presented, but difficult to tell how much was done in 2004 and how much was done in 1998-2003 period project has been active.
- Considerable insights have been gained related to WGS reaction mechanisms, both in PGM/ceria and base metal catalysts.
- The issue of methanation is being addressed, and progress has been made, but there is still a long way to go and no clear evidence that the problem can be solved with the approaches being pursued.

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

This project was rated **2.3** for technology transfer and collaboration.

- There a few program partners.
- Collaborate with commercial WGS developer?
- Have past collaborators; little indication of any present collaborators.
- Need to make stronger "official" tie to industry.
- Need to renew contacts with industrial catalyst companies. Much progress in WGS catalysts has been accomplished by industry, but not reported in literature.
- There is no evidence of collaboration and no apparent interest by catalyst companies in licensing the technology. If the primary benefit of the project is improving our fundamental understanding of WGS catalysis (as admitted by the author), wouldn't this benefit be strengthened by providing details on catalyst formulations and synthesis methods?
- From the recent patent literature, it is apparent that most of the large catalyst companies worldwide are pursuing development of high performance WGS catalysts – is this project duplicative and/or necessary in light of larger efforts being funded without DOE support?

Question 5: Approach to and relevance of proposed future research

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

- Program requires more emphasis of fuel system integration.
- Appears to be addressing the correct issues including impurities.
- Will Ruthenium and Rhenium meet cost targets?
- Need more focus on real-world failure mechanisms such as multiple start/stop cycles (with water condensation), cyclic exposure to low levels of sulfur-breakthrough, and over-temperature exposure.
- Future work on the ruthenium-rhenium system should be abandoned unless loadings can be reduced significantly and performance (and perhaps sulfur tolerance) can be improved to the point where the cost is justified.
- The primary focus of future work needs to be on improving performance and durability and reducing methanation. Without some progress in these areas, it is questionable whether effort should be expended in other areas (e.g., kinetic modeling, effects of impurities, etc.).

Strengths and weaknesses

Strengths
- The modeling effort will be beneficial to other programs.
- Team has a strong portfolio of diagnostic capabilities.
- Very good catalyst characterization capabilities.

- Strong use of data.
- Very good use of analytical infrastructure to unravel catalytic mechanisms.
- PI showed good understanding of problems with current commercial Cu/Zn WGS catalysts.
- PI showed good understanding of fact that sulfur continues to be major problem in all hydrocarbon reforming processes, including natural gas.
- Good combination of characterization, testing and modeling are being utilized on the project.
- Fundamental understanding of WGS catalysis and reaction mechanisms being obtained.

Weaknesses

- There was minimal discussion about impurity effect. No discussion of the integration into a full system (one of the barriers addressed). Future work is limited.
- Program needs better definition of quantifiable targets.
- Is there a strong need for WGS catalysts or would a better approach be to engineer around the limitations of conventional WGS catalysts?
- Refocusing of team needs to be ensured. We need to see this technology developed.
- Testing does not mimic failure mechanisms seen in real world fuel processors and thus makes it difficult to determine if catalysts would be durable under those circumstances.
- Minimal progress toward developing WGS catalysts that will meet the needs of industry.
- Path toward achieving technical and commercial viability is not clear.

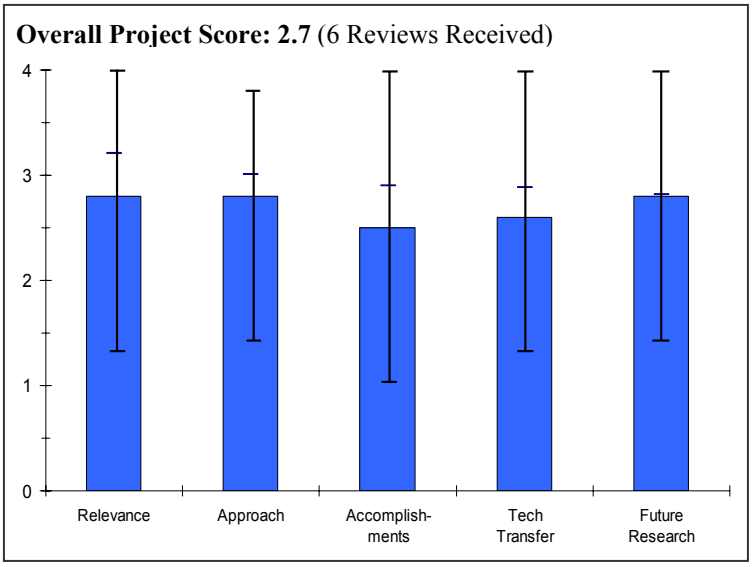
Specific recommendations and additions or deletions to the work scope

- Include Cu-Zn in performance plots as a comparison to program's progress.
- Move more rapidly away from catalysts containing expensive materials (Re, etc.).
- Continue project.
- Discuss WGS failure mechanisms seen in current generations of fuel processors with people developing those fuel processors.
- Implement tests that mimic upset conditions to determine WGS vulnerability to those upsets.
- The issue of pyrophoricity needs to be addressed.
- Better and more realistic milestones need to be defined. These milestones should be based on both cost and performance, and be defined such that progress toward commercially-viable catalysts can be assessed.
- A go/no-go decision on this project should be implemented which should be based on progress toward meeting milestones and by the level of industrial interest.

Project # FC-25: Catalysts for Autothermal Reforming
Krause, Theodore; Argonne National Laboratory

Brief Summary of Project

Argonne National Laboratory (ANL) plans to develop advanced fuel processing catalysts which will, when compared to Ni-based steam reforming catalysts, be able to process complex fuel mixtures such as gasoline; process these fuels at higher rates; be more resistant to coking and sulfur poisoning; improve our understanding of reforming reaction mechanisms, catalyst deactivation, and sulfur poisoning; and define operating parameters (e.g., air: fuel and steam: fuel ratios, temperature, gas hourly space velocities, catalyst geometry) to optimize catalyst performance and lifetime.



Question 1: Relevance to overall DOE objectives

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

- Should specify target performance in objectives.
- The change in direction to off-board reforming limits the program benefits.
- Development of new reforming catalysts is important for hydrogen production from fossil/renewable fuels.
- Current commercially available ATR catalysts are quite good and not large cost component compared to other sorbents and catalysts.
- Improved and cost effective reforming catalysts are important in distributed hydrogen production PEM fuel cell systems.
- Engineering solutions exist for using commercially available (nickel-based) catalysts for stationary systems so the project might not have wide scale applicability.
- Establishing a fundamental understanding of reaction conditions and mechanisms, and effects of fuel contaminants will be valuable to fuel processor system developers.
- Making hydrogen is essential for the hydrogen vision.

Question 2: Approach to performing the research and development

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

- Should consider other catalysts since national gas and LPG are simpler hydrocarbons than gasoline. Excellent choice of key experiments and parameters to understand catalyst behavior.
- Reactor studies and catalyst characterization will be beneficial.
- Good use of advanced analytical techniques to identify catalytic mechanisms.
- Not clear that more study on nickel or nickel/Rh catalysts is more valuable than work on less investigated non-precious metals.
- Testing approaches being pursued at Argonne appear to be very suitable for the work being conducted. The work is being augmented by extensive catalyst characterization using a variety of tools.
- Catalyst durability is being tested for each catalyst, in addition to activity and selectivity.
- The bi-metallic approaches being pursued are a logical way of reducing precious metal loadings.
- Commercially available catalysts should be tested to provide a baseline for comparing performance. Perhaps this has been done, but the data were not reported.

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

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

- Good progress in understanding support-catalyst interactions. On right path to address carbon/coping formation in these catalyst systems.
- Did not address the barrier related to "fuel cell power system integration".
- It can take time to switch over to new fuels.
- This project has been in progress since 1995. Materials developed don't appear to be competitive with commercially available ATR catalysts.
- Although a number of tests have been completed, it was difficult to assess the relative performance of these catalysts without realistic comparisons to commercial catalysts.
- Low deactivation rates have been observed, but it is difficult to accurately claim comparative differences because of considerable scatter in the data.
- Good conversion results using ANL catalysts formulated with Rh.
- Ni-Rh catalysts are interesting, but not novel.

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

This project was rated **2.6** for technology transfer and collaboration.

- Complimentary capabilities throughout team accelerating program and avoiding effort duplication (i.e., propane experiments done where set up exists rather than re-create).
- There are limited program partners.
- Not clear if there are any present collaborators.
- Two publications submitted, work with GE, and multiple university contacts are good indicators of collaboration.
- It is not clear that the mentioned collaborations with Alabama, Puerto Rico, GM, and Minnesota were especially active ones.
- There doesn't appear to be active interest in this project by catalyst companies or other potential commercialization partners.
- It might be useful to establish collaboration with Los Alamos, where reforming work also has been ongoing for several years.
- Need to work with catalyst companies in addition to universities.

Question 5: Approach to and relevance of proposed future research

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

- Logical follow -on based on prior results. Should define plan to address sulfur issues (bullet, but no strategy).
- Program requires more focus on integration.
- Good to see plan for more focus on sulfur sensitivity and durability testing.
- It is a good idea to evaluate Pd and Pt as co-catalysts with nickel, although at some point the focus should move toward non-precious metal catalysts (if the project goal is to deliver a non-precious metal, sulfur-tolerant ATR catalyst in FY'10).
- I agree with the Argonne's decision to include the effects of sulfur (H_2S) impurities within their testing matrix (despite a comment to the contrary from a reviewer last year).
- If LPG and natural gas are to be the focus of this project, then future testing should be conducted with real LPG fuels (or more realistic surrogates).

Strengths and weaknesses

Strengths
- Good team with complimentary expertise. Well coordinated effort.
- The numerous publications will benefit discussion and learning.
- Very good use of analytical infrastructure to unravel catalytic mechanisms.
- Good combination of catalyst synthesis, characterization and testing are being utilized on the project.
- Pragmatic approach to catalyst development.
- Interesting experiments.
- Emphasis on understanding of bimetallic catalysts is a good approach.
- Concern on carbon deposition is essential.

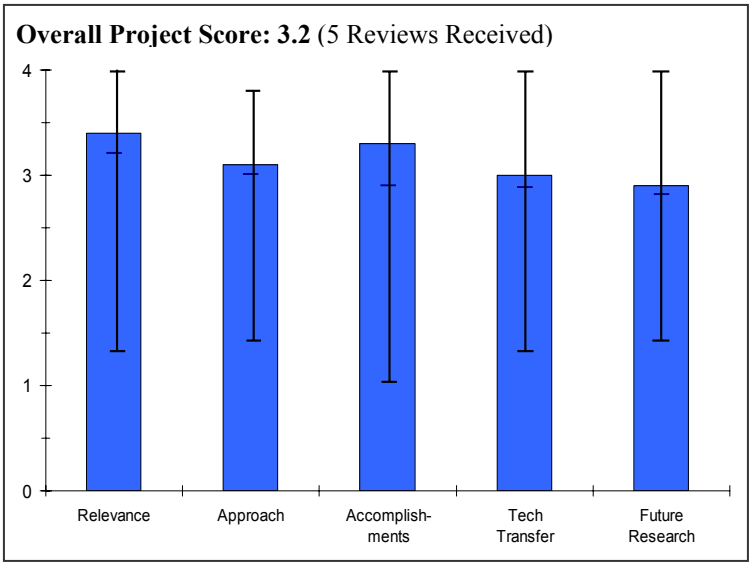
Weaknesses
- Catalyst has a high rate of deactivation.
- Did not show compelling reasons for nickel work.
- Path toward eliminating precious metals from the catalyst is not clear.
- Existing technology to remove H_2S from natural gas works well. This is true for mercaptans used for odorants as well. Probably need to do remove sulfur, so sulfur tolerance in catalysts is not critical.
- Rhodium has a long, successful history in POX. The question is, do you need to use Rh?

Specific recommendations and additions or deletions to the work scope

- DOE/industry need to address planned off-board reforming strategy, since need (or lack of need) for sulfur-tolerant reforming catalysts has significant consequences for catalyst development approaches. For off-board, a separate desulfurization step may make more sense.
- The program should focus on either optimizing catalyst for ATR or steam reforming.
- Pick up pace of catalyst formulation optimization.
- Apply advanced analytical techniques to determine mechanisms of sulfur poisoning and effects of promoters that can reduce these effects at low sulfur levels.
- Testing of real (and not simulated fuels) needs to be conducted.
- Baseline testing with commercially available catalysts needs to be performed.
- Experiments that include steam and other oxygen-containing compounds to explore soot implications would be interesting.
- Some soot could be volatile. Might want to add a “soot detector” to analyze the gas phase effluent.

Project # FC-26: Selective Catalytic Oxidation of Hydrogen Sulfide
Schwartz, Viviane; Oak Ridge National Laboratory

Brief Summary of Project



The goal of this project is to develop and optimize an oxidative process to reduce sulfur levels to the parts per billion range in a hydrogen-rich gas stream using low-cost, carbon-based catalysts to produce a low-sulfur fuel for use in fuel cells. Oak Ridge National Laboratory (ORNL) is identifying and using activated carbon catalysts that show potential for complete conversion of H_2S to elemental sulfur without formation of undesired products; investigating different activation protocols and carbon-based precursors that can lead to improved catalytic properties; characterizing the microstructures, surface properties, and impurity level of the catalysts and correlating to catalytic activity, selectivity, and durability; and performing catalytic studies of catalysts in hydrogen-rich and model reformate gas streams and investigating reaction mechanisms.

Question 1: Relevance to overall DOE objectives

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

- Very relevant- sulfur impurities, purification approaches with advanced materials very desirable.
- Very important and relevant project for the program.
- Sulfur removal is key technical hurdle in reforming sulfur containing feedstocks such as natural gas, LPG, biomass, and logistic fuels.
- Improved and cost effective approaches for eliminating sulfur are important for distributed hydrogen production when natural gas, propane and other hydrocarbons will be employed.
- Commercially available sulfur sorbents can be used in many distributed hydrogen generation applications but the selective oxidation technology has promise for reducing overall system size and minimizing maintenance.

Question 2: Approach to performing the research and development

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

- The approach is addressing key barriers and very well designed.
- Objectives and approach well defined. There are issues, however, with adding air/oxygen to the fuel stream.
- Strong approach to a difficult problem.
- Well designed test program with good mix of testing commercially available carbons and "lab made" carbons.
- Really need to see data on effects of many regeneration cycles on capacity and selectivity.
- The testing approaches being pursued appear to be viable for evaluating different catalyst formulations and reaction conditions.
- As the project proceeds, it will be important to conduct testing for longer durations and under conditions that more closely resemble the targeted application.

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

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

- Very good progress and improved understanding achieved.
- Project requires further definition of how the process will be integrated into a complete system.
- Outstanding progress towards a viable system. Especially strong considering limited funding.
- Good progress for first year.
- Good presentation and explanation of raw data.
- It appears that ORNL has developed a superior carbon-based catalyst for their application based on comparisons with commercially available carbon catalysts.

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

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

- Major companies with interest in the technology are approached.
- Limited collaborations with industry.
- Good team.
- Need to finalize collaboration with industry partner.
- Discussions have been initiated with UTC Fuel Cells, Conoco Phillips and Chevron-Texaco. These relationships should be pursued, both as a means of finding commercial outlets and for identifying the best testing conditions.
- An impressive list of publications and presentations from the past year.

Question 5: Approach to and relevance of proposed future research

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

- Focusing on improved catalyst by using supported carbons and by ranging the structure and morphology of the carbon catalysts is excellent goal.
- Very well reasoned future plan.
- Now that good materials have been identified, need to focus on process development such as regeneration of spent bed.
- For the most part, the future work plan is acceptable.
- Industry stakeholders should be consulted regarding testing conditions required to establish feasibility and generate interest in the technology.
- A manufacturing cost assessment should be conducted.

Strengths and weaknesses

Strengths
- Program includes strong investigation into limiting side reactions.
- Good viability.
- Novel approach to dealing with high levels of sulfur. No other technology appears to be as good fit for reforming.
- High level of expertise in group.
- Good understanding of technical hurdles of real world reforming.
- Systematic approach to catalyst development.
- Quality of results obtained given a relatively low budget.

Weaknesses

- No clear plan on how to remove sulfur from oxidation sites.
- Lack of technical details limits the amount of feedback that a reviewer can give. What are the precursors to be looked at? Will a pillared clay or silicate be used as a template? Exactly what will the new carbons be made from and what design variables will be employed?
- Need more focus on process development.
- Relatively low space velocity (about 3100 h^{-1}) means large beds.
- Need more application-specific testing conditions and longer term test durations.

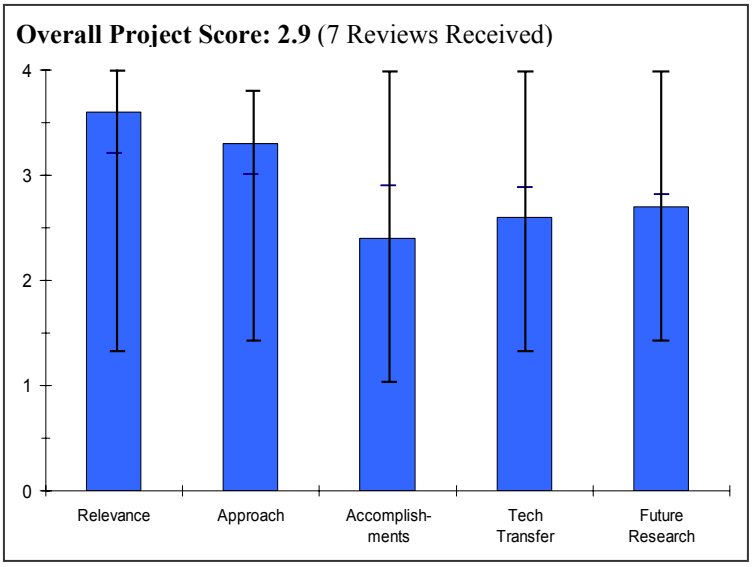
Specific recommendations and additions or deletions to the work scope

- Develop regeneration methods.
- Test effect of regeneration on carbon activity and capacity.
- Estimates of cost were not presented.
- A manufacturing cost analysis for the catalyst and for the process is needed for an assessment of viability.

Project # FC-27: Cost and Performance Enhancements for a PEM Fuel Cell Turbocompressor
Gee, Mark K.; Honeywell

Brief Summary of Project

Honeywell is developing an enhanced turbocompressor configuration for integration into a PEM fuel cell that reduces costs while increasing design flexibility. Honeywell is utilizing their expertise in automotive and aerospace turbo machinery technology, variable nozzle turbine inlet geometry, mixed flow type compressors, and contaminant/oil free, zero maintenance compliant foil air bearings to achieve this objective. The final product will have a modular design, high efficiency, and variable speed motor controller topology design



Question 1: Relevance to overall DOE objectives

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

- Turbocompressor and motor development for pressurized automotive application.
- Cost was identified as a major issue. There was no analysis plan presented which might lead to reduced cost.
- Building on existing turbomachinery technology a logical approach- but will it be sufficient for this very challenging application?
- While important, less so as developers move to lower pressure.
- Explicitly addresses a need identified by DOE.
- Definite need for efficient compressor for transportation systems.
- Project is important.

Question 2: Approach to performing the research and development

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

- As long as there is a need for pressurized stack operation this project is essential.
- Basic approach is correct; modularization is key.
- Hardware development and testing is a good blend towards objective.
- Work based on applying best use of existing technology.
- Have made excellent progress on their design.
- Apparently good although little useful information presented.
- It is unlikely that this configuration can ever come close to cost targets.
- Good understanding of technical barriers and customer needs.
- Approach is logical.
- Unclear what input may have been obtained from fuel cell power system/automotive OEM'.

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

This project was rated **2.4** based on accomplishments.

- This has been a slow project but with steady progress.

- The incorporation of variable nozzle geometry to flatten efficiency curve and foil bearings for durability were important steps.
- Features impact cost of course and cost is a key criterion for automotive applications.
- A ruler to indicate actual size of the hardware would have been helpful. Pictures of turbine wheels would be useful. Good mechanical design with built in flexibility for future vendors.
- Detailed design done with significant improvements to the modular design to make it easier to adapt to different applications and most cost effective to manufacture.
- Inability to reach peak power goal a little troubling, although part-load power good.
- No evidence presented that any of the components will meet targets.
- Virtually everything presented was "estimated" or "Projected".
- Relatively little seems to have been accomplished for the time and money invested.
- Progress after 8 year project life is disappointing.
- Motor controller volume too large.
- Progress seemed slow. The 4th objective should belong to a power electronics supplier. There are quite a few of them out there. Honeywell should focus on the turbomachinery itself.
- Appears as if initial DOE targets have been met.
- Progress remains slow. Technical objectives 2 and 3 remain incomplete.
- Unclear progress made since 2004.

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

This project was rated **2.6** for technology transfer and collaboration.

- Limited collaboration shown. Honeywell is not enough. Involve the big three.
- Collaboration with Argonne and fuel cell /automotive OEM's and important to define requirements. However it is not clear how the technology will be transferred to industry.
- Although some communication with potential users, it is not clear the extent to which this design will be able to meet the needs of users.
- Some interactions claimed but little evidence of real exchange.
- No links mentioned in presentation, but in answer to question from audience presenter mentioned confidential negotiations.
- Need to establish collaboration with power electronic suppliers and initiate OEM involvement for air system testing.
- OEM contributions not detailed.
- Argonne National Labs contributions not specified.
- Dissemination of non-proprietary results through additional publications recommended.

Question 5: Approach to and relevance of proposed future research

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

- No contingencies addressed. Program completion is tight, no chance for design corrections.
- There was no discussion of plans beyond defined program objectives.
- Approach to future work seems reasonable.
- A continuation of hardware development with no mention of critical reviews, test plans, etc.
- Need to complete project as planned.
- Need to focus on the turbocompressor, provide test results for the unit.
- Proposed work will conclude project.
- Additional details would have been useful (What is needed to complete design, what remains in the fabrication and assembly areas)?

Strengths and weaknesses

Strengths
- Solid hardware design and build program. Utilizing past experience and industry expertise.
- Program incorporates good blend of hardware, testing, and analysis.
- Based on existing production technology- good chance to be as cost effective as possible
- Modular design improved and the right approach
- An organization with known strengths in turbomachinery design and development.
- High degree of mechanical engineering skills demonstrated.
- Design work.
- Strong background in the design and manufacture of turbocompressors.

Weaknesses
- Program plan needs time for second build although modular approach is beneficial in shortening turnaround time. Add feature of testing with integrated fuel cell system.
- It is evident that current test hardware won't meet DOE guidelines. There should be a path (analytics) which highlights areas that must be addressed to meet those goals.
- Input power is 2X too high and no approach identified to reach that goal.
- Potential matching issues to fuel cell applications- no data shown to demonstrate if they can satisfy the requirements of a range of fuel cell sizes and applications.
- Although this design can operate as only a compressor, existing power consumption problems are likely to be magnified if no expander section included- need data to understand this situation.
- Did not show how to improve transient response time.
- It appears that this program is getting relatively little priority within the organization. Accomplishments seem to be much less than they should be at this stage.
- Little evidence of collaboration or use of outside experts.
- No cost estimates presented.
- Need to build and test system.
- Details significantly lacking.
- Data on why this design is expected to be superior to previous designs,
- What were the results from Technical Objective 1?
- No discussion on how efficiency, cost, weight, etc. may compare with what is currently available

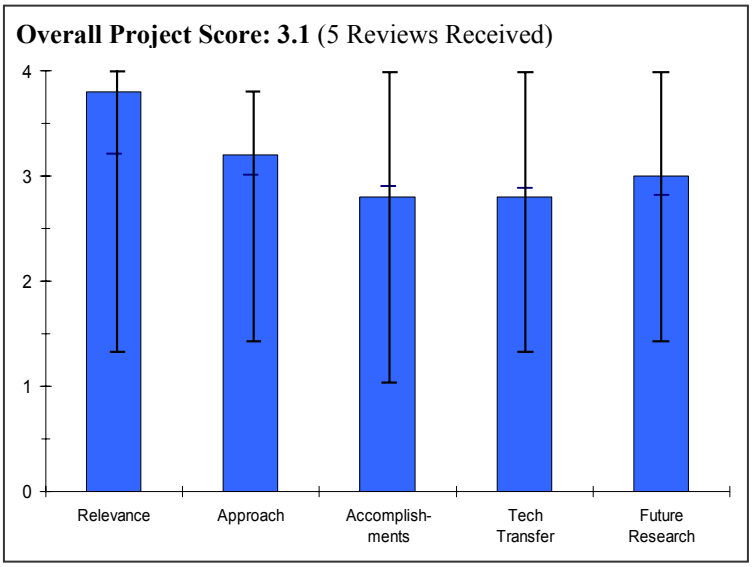
Specific recommendations and additions or deletions to the work scope

- Program scope is about right except for integrated fuel cell system test. Could be in a simulated configuration.
- Minor things only: (1) Provide data to show that this design can meet requirements for a range of fuel cells (2) Focus on solving input power issue- or show % of operating range they meet power targets (3) Discuss operation as a compressor only - show power required (4) More emphasis on improving transient response to meet targets
- Do a careful review of the project, including a critical design review, to see if there is any reasonable hope of meeting targets.
- Cost projections should be done with and without the expander.
- Need to establish collaboration with power electronic suppliers and initiate OEM involvement for air system testing.
- Initiate involvement with Ohio State University for air system testing.
- Detail fuel cell cost and performance enhancements as a result of implementation of the turbocompressor.
- Document interactions and contributions from team members.
- Publish.

Project # FC-28: Development and Testing of a Toroidal Intersecting Vane Machine (TIVM) Air Management System
Bailey, Sterling; Mechanology, LLC

Brief Summary of Project

Mechanology's overall objective in this project is to develop the innovative Toroidal Intersecting Vane Machine (TIVM) concept into working compressor/expander/motor hardware that satisfies the FreedomCAR guidelines and is easily adaptable to individual car system requirements. Mechanology measures the TIVM air management system performance with the goal of providing the global automotive fuel cell market a significantly improved air management product.



Question 1: Relevance to overall DOE objectives

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

- The project addresses a vital component in the "balance of plant" for automotive fuel cells- a pump (compressor) for the air handling system.
- The relevance is demonstrated by project goals that track DOE requirements.
- Hardware being addressed would greatly benefit DOE fuel cell objectives if successful.
- Compressor/expander is a key element in successful fuel cell commercialization.
- Low cost, efficient air compressors are a key automotive fuel cell system enabler.
- This new concept, at this stage, has great prospects.

Question 2: Approach to performing the research and development

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

- The approach, while containing a high degree of technical risk, is fundamentally sound and apparently working.
- The project is designed to meet DOE objectives.
- Program has developed some impressive, but complicated hardware. Approach seems to tinker with the hardware to achieve performance enhancements.
- Innovative but high risk approach.
- Complexity implies high cost- high part content.
- This might not be the best application for this component.
- This project is a major development effort using a completely new approach- is the payoff worth it?
- Appears to be very novel concept.
- The innovative designs, particularly the 'layer cake' version, offer the potential of achieving most, if not all, of the primary goals.

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

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

- The PI reports that all objectives are being met, or are expected to be met, with one possible exception—the input power requirement.

- Additional work is required to reduce friction (increase efficiency).
- Analysis needs to be done on the effect of shock and vibration on an operating device (maximum speed), to see if the device can withstand automotive operating conditions. (The device has a large number of surfaces, machined to tight tolerances moving past each other at high speed.)
- Safety analysis does not include companion electric motor as potential ignition source.
- Progress at low/intermediate speeds looks promising; key will be when operating at high speed. It is not clear that low production costs will be achievable when seal and tolerance impacts appear at high speed.
- This project seems not to be on track for completion on time.
- Appears to have made significant progress on an improved design but I am not sure it is enough.
- New design features simplify parts.
- Improved air handling directly improves efficiency.
- New seals needed - design being developed.
- At the beginning of developing seal materials.
- Some progress made as machine has achieved half rated speed.
- The seals are key to achieving efficiency, durable design, and a contaminant free air stream. These have yet to be demonstrated.
- Air machine efficiency has yet to be demonstrated.
- Excellent progress made in relatively short time.

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

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

- PI spoke of a number of interactions with auto makers.
- Patent disclosures have been made on the technology.
- Journal publication not relevant for this project.
- Major interaction has been with Argonne National Laboratory. PI only.
- Although vague, indications are that sufficient collaborations with potential users and high volume manufacturers exist.
- Appears to be a single-team effort. Help needed on seal design.
- Close collaborations with end-users is imperative- particularly at the evaluation/validation stage.

Question 5: Approach to and relevance of proposed future research

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

- PI proposes appropriate activities to attempt to achieve goals and to provide contract deliverables.
- PI spoke of spin-off product that may be commercialized in CY 2006 with private funding.
- PI did not present a path beyond achieving currently defines objectives.
- The plans laid out seem appropriate but assume solutions to several large problems (like sealing) will come quickly. This is a major developmental effort on a complex new mechanical device that has many issues.
- Future direction addresses the correct issues. The question is the probability of achieving the targets.
- Proposed future work is on target. Need to see efficiency map data.

Strengths and weaknesses

Strengths
- Innovative approach to designing an air pump, with characteristics appropriate for fuel cell use.
- Program is making good progress in hardware development and testing.
- A new approach with many potential applications.
- Significant progress made but a lot more to go.
- Novel compressor/expander design.
- Capable of a range of pressures at all flows which is desirable for stack operating condition control.
- Notable strength is the combination of innovative designs and the PI's exceptional enthusiasm.

Weaknesses

- Durability of design still uncertain since it remains to be analyzed and tested.
- Ability to achieve and to maintain low power load and design efficiencies during use yet to be established.
- The major barriers to achievement will not be known until high speed testing of integrated hardware is accomplished. That is when the impacts of friction, seals, and tolerances will be fully understood in a dynamic thermal environment.
- Shock and resistance not really addressed.
- No discussion on the motor/controller needed.
- Initial data is not that encouraging - have not demonstrated ability to attain pressure.
- No data on efficiency.
- Significant sealing issues with no real idea how to solve them.
- Still have friction issues.
- Have not tested at temperature or under moist conditions.
- Low speed, positive displacement (PD) devices generally are large for a given airflow. This device is claimed to be smaller, but it is unclear how much so. It is not predicted to reach DOE target. Data is required.
- PD devices tend to be less efficient than dynamic machines. The developers need to demonstrate that this machine is capable of the high efficiencies promised.

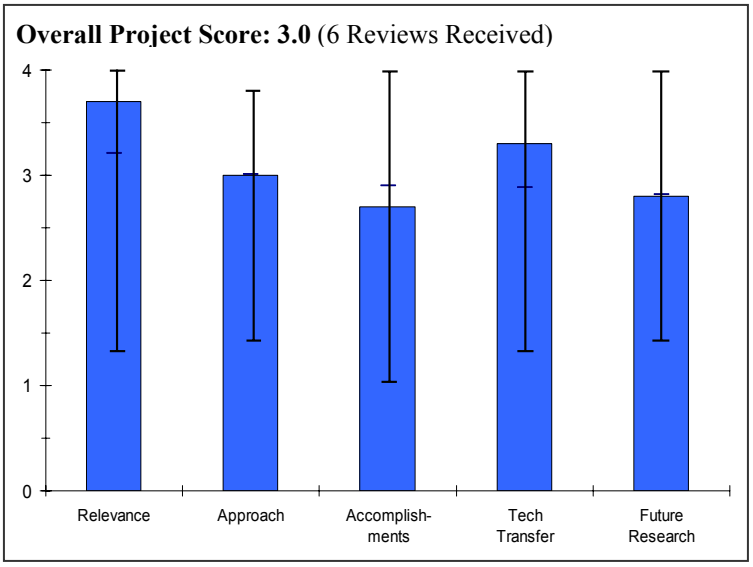
Specific recommendations and additions or deletions to the work scope

- The operating concept is difficult to understand; the early concept vu graphs did little to help. Video would have been useful.
- I am not sure that a fuel cell application is really the right one for this device. This is a high risk development project that is making progress but has little chance of being completed on time while solving all the problems that remain. Significant progress on producing data relevant to fuel cell applications is needed. Although achieving the appropriate boost levels for fuel cell applications appears to be possible, many other fundamental issues have not been addressed such as efficiency (a critical issue), shock and vibration, operation at temperature and under moist conditions, and the very formidable sealing issues (which bear on friction- still a problem- efficiency and durability).
- If the program managers deem this high risk R&D project to have great potential, be ready to extend the time and funding to solve these serious un-resolved developmental issues. My judgment from the data presented here is that we don't really have the information to determine if this device really has enough potential to continue its funding. It will be an up-hill battle to develop this device for fuel cell applications. My own feeling is that is has other potential applications that are not as severe as fuel cells where it would likely make more sense IF the significant issues can be solved satisfactorily- and that is certainly not a given.
- Just get the data already! Good luck!
- Have independent, third party verification of claimed attractive cost estimates. More efficiency V's load data needed.

Project # FC-29: Development of a Thermal and Water Management (TWM) System for PEM Fuel Cells
Pont, Giullermo; Honeywell

Brief Summary of Project

Honeywell's project is to assist DOE in developing a humidification and cooling system for PEM fuel cells in transportation applications. Objectives of the project are to focus on cathode humidification, and fuel cell heat rejection for an 80 kW fuel cell power system; study pressurized thermal and water management (TWM) system performance; analyze steady-state automotive operating conditions for comparison of concept schematics; establish TWM system/component specification; and demonstrate the performance of a breadboard TWM system with research hardware.



Question 1: Relevance to overall DOE objectives

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

- Well focused on thermal and H_2O management issues. Doesn't address integration with other system components.
- Supports complexity resolution regarding H_2O dynamics in large PEM systems.
- The project addresses technologies critical to obtaining the HFCIT goals for fuel cells.
- Essential components for successful FC commercialization.
- This type of (apparent) in-depth investigation is extremely important in pursuing optimum configurations.
- TWM is essential to compact, reliable, durable fuel cell power systems.

Question 2: Approach to performing the research and development

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

- Good survey of relevant approaches.
- Multiple paths appropriate to be able to downselect. QFD used effectively. Why 60% RH requirement when we know PEM's need higher water content?
- Approach is well thought out.
- Opportunity for integrating enthalpy wheel with heat-management should be investigated.
- Humidification to 60%RH at cathode only may be insufficient humidification for some designs-may need to humidify anode or go to higher RH to prevent dehydration and ensure adequate membrane lifetimes in other MEA designs.
- Humidification approach on target.
- Thermal options based on existing heat exchangers- will this be adequate?
- No mention of models or simulation techniques.
- Sophistication and validity of analyses are unknown.
- Choice of alternative configurations and especially materials seem too limited.
- The evaluation priority scheme is a way to choose between a number of less-than-adequate approaches.
- Weighting and priority need input from OEMs and system developers.
- Apparently no consideration to date of startup (dry?), transients or wet shutdown (freeze?)
- Approach should not lose site of importance of non-steady operation.

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

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

- Criteria prioritization system well defined. Good basis for system down select. Critical subscale tests promising. Show target can be met for water management.
- Most of these methods have been available- not sure what is new.
- Good progress made in design studies and developing system targets.
- Systems that can meet targets have been identified.
- Most work on humidification- but the real challenge is for thermal management.
- Two humidification designs selected and developed- one appears better than the other but design and evaluation is continuing.
- Aerospace and automotive heat exchangers evaluated- AL foam, microchannel and aerospace designs selected all need significant development for mass production and cost reduction.
- Also need validation of performance of heat exchanger materials and designs.
- Apparently good. Too few details given to be sure.
- More alternatives probably should have been considered.
- Selected options have been evaluated several times over the last decade by others and judged inadequate.

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

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

- Strong group of collaborations with critical expertise in necessary areas.
- Should enhance and broaden relationships.
- More collaboration with OEM's and automakers would help further define target.
- Improved collaboration with fuel cell community described.
- Need more collaboration with organizations that can assist heat exchanger material and design development.
- Appears that there is excellent collaboration.
- Appropriate collaborations are underway.
- No technology to transfer at this point.

Question 5: Approach to and relevance of proposed future research

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

- Need to identify targets better. Are WM and TM independent or interdependent and how is this being addressed?
- Must focus on system as a whole. Emprise reliability should be assessed.
- On track to finish water management components.
- Work focusing on designing and developing the advanced heat exchangers.
- Follow-on proposed should address many important issues.
- Planning adequate but uninspired.

Strengths and weaknesses

Strengths
- Systematic approaches with rational down select based on design and subscale experiments.
- Well organized component selection process. Lots of experience with aerospace engineering in-house.
- Project team has the capabilities needed.
- Thorough work on humidification design.
- Organization has excellent known appropriate capabilities.

Weaknesses

- Looks like power requirement (target) changed from 50 kW to 80 kW. Is this program adjusting spec for future work? Doesn't address if TWM system requirements are overly sensitive to specific PEM used. Is there a universal TWM solution for low T and high T PEM?
- Cathode only - why not anode? Design is based on a low RH. Complexity of controls should be considered. Selection of technology will be fuel cell system design specific. Can this work be generalized?
- Have not looked at freeze issues in humidification system.
- Have not built in vibration and shock analysis to humidification system design screening.
- Have not considered possible recycled water contamination issues.
- Not made significant progress on the heat exchanger development- this is the real challenge and the largest technological stretch.
- May not have adequately looked at all the potential heat exchanger material options.
- The work does not seem to have the depth that it should have.
- The 1 to 5 rating approach is really poor.
- Project unlikely to advance state of the art.

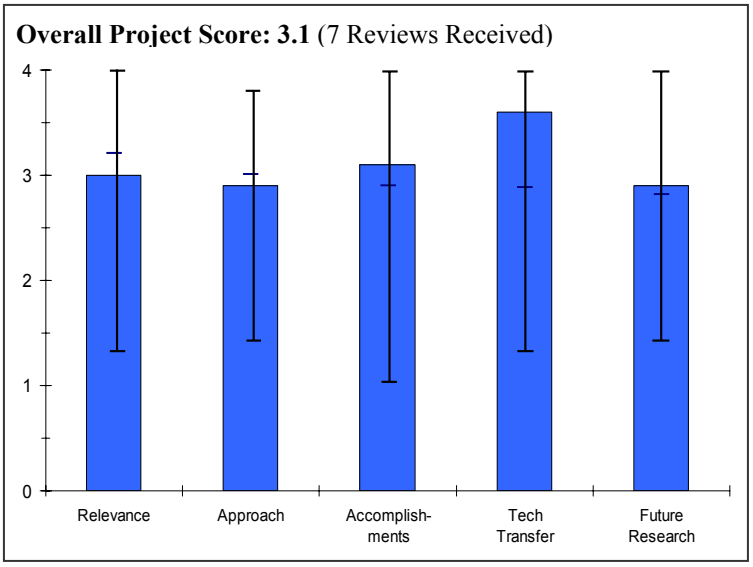
Specific recommendations and additions or deletions to the work scope

- Thermal management comparison to standard auto not clear. What are the targets? (No numbers on graph). Are targets significantly different for fuel cell systems than current automotive systems?
- Include freeze protection design considerations.
- A better quantification of effects of system variables (such as %RH needed or temperature of operation) on system requirements would be beneficial.
- Need to address freeze issues, shock and vibration requirement in humidification design.
- Need to analyze and address possible water contamination issues with recycled water.
- Status of heat exchanger work troubling. This key technological barrier needs more effort. May not have adequately considered all material and design options.
- Significant issues remain in cost effective manufacture and durability of aluminum foam heat exchangers - more emphasis on this seems prudent. Perhaps a new collaboration partner in this field is appropriate.
- A rating approach for attributes can be very useful, but should be better thought out than this one.

Project # FC-30: Development of Sensors for Automotive PEM-based Fuel Cells
Knight, Brian; United Technologies Corporation

Brief Summary of Project

United Technologies Corp. Fuel Cells (UTCFC) and its team are developing physical and chemical sensors for proton exchange membrane fuel cell power plants for automotive applications aimed at low cost (<$20 per sensor) at 500,000 quantity. Work in chemical sensors includes process streams before, in, and after reformer and before and in fuel cell stack; CO, H_2, O_2, H_2S, NH_3 types; and safety (hydrogen). Physical sensors focus includes temperature, pressure, relative humidity, and flow.



Question 1: Relevance to overall DOE objectives

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

- Sensor development critical for the development of robust systems and to meeting life targets.
- This project presumably started out as being applicable to reformate-based automotive fuel cell systems. With the change to direct hydrogen systems for automotive use, many of the chemical species sensors will be of interest only to stationary systems, for which performance requirements (e.g. response time) are quite different from those for automotive systems.
- The program aimed to produce a sensor database, which would be useful to a variety of vendors.
- The UTC team includes some of the premier developers of sensor components.
- New automotive-grade sensors contribute to successful fuel cell commercialization.
- Reformate-based sensors can contribute to off-board hydrogen production technologies.

Question 2: Approach to performing the research and development

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

- Question the need for T, P, and flow sensor development. Can business case be made for these sensors compared with existing sensors?
- Chemical sensors have immediate need.
- Approach relies on selection of multiple suppliers to develop the sensor technologies to UTC requirements.
- Only two of the team members (ATMI and NexTech) are doing any developmental work. UTCFC, UTRC, and IIT appear to primarily be doing identifying existing sensors or sensing approaches, and testing sensors developed or provided by others. Indeed, there appears to be little or no development needed for the physical sensors (T, P, RH, flow).
- ATMI deserves credit for exploring the MEMS-based platform with multi-layer sensing films.
- Program well structured to develop (as necessary), evaluate, and report on a wide range of sensors applicable to future fuel cell vehicle development.

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

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

- Sulfur sensors need lower sensitivity.
- Sensors need additional sensitivity testing for cross-sensitivity (such as RH, S).
- Difficult to gauge progress by the data shown, however, drift in sensors appears to be an issue.
- Would be useful to have results plotted against target sensitivity levels, response curves, and drift with time.
- Approaches for addressing drift not discussed.
- The main developments have been by ATMI and NexTech.
- ATMI has shown promising results with ambient (safety) hydrogen sensor and process hydrogen sensor, including reducing some of the baseline drift rate (although that is still too high), as well as the H_2S sensor.
- NexTech's H_2S sensor data look promising, but the performance of the CO sensor is still inadequate - it seems to get "flooded" by high levels of CO so that it is difficult to correlate response to the exposure level.
- The results reported appear commensurate with program goals and resources applied. Program embodied a good technical team.
- Each sensor's operation and reliability are making progress.
- Good benchmarking work. Need to share information with other sensor development work besides just UTC.
- Keep the focus on sensitivity, reliability and durability. Cost should be the lowest priority for this task.

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

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

- Good integrated team.
- Large team assembled.
- Would be helpful to see allocation of resources against partners and sensor area.
- The project team includes academic, research, and commercial (end-user) organizations, so that advances can be readily validated and incorporated into prototype and commercial systems.
- Project had excellent collaboration with relevant participants with necessary resources to accomplish goals.
- Close technical coordination is evident and the progress shows this.
- A strong team of developers and testing capability.
- Good collaboration but lack OEM involvement.

Question 5: Approach to and relevance of proposed future research

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

- H_2S cross-sensitivity with CO sensors critical.
- RH cross-sensitivity critical for all sensors.
- PI did not present clear picture of how subcontractors will address gaps in performance. Whether problems are fundamental in nature or engineering/design issues.
- As the project objectives have shifted from automotive to stationary systems, with the concomitant change in performance requirements, future activities should change accordingly. On the other hand, since this project is close to completion, the identified future activities appear to be the logical ones to bring the project to an orderly conclusion.
- Although continued development and evaluation of various sensors should be continued, this effort should conclude with a meaningful technical report available to all fuel cell and automotive OEM's. Publication of this report was not discussed.
- The future work appears to address the open issues identified.
- Given Hydrogen Program re-direction, majority of remaining work should be on RH sensor.

Strengths and weaknesses

Strengths

- The project is being directed and coordinated by an end-user, a fuel cell company, which helps ensure that the work is relevant and practical considerations are not ignored.
- ATMI's MEMS-based approach has the potential to yield, compact, low-cost, and sensitive sensors.
- This activity investigated a wide variety of potential fuel cell sensors in relevant operational conditions.
- The UTC team is unique. Each sensor will be tested in a 'real' UTC fuel cell and stack test stand using established test protocols.
- A strong multi-disciplinary team.
- Appears to have shown very good progress in most sensor designs with commercialization achieved in a few.
- Good results.

Weaknesses

- Work seems primarily only anode sensors; need cathode chemical sensors also.
- The long-term baseline drift in the H_2S sensor makes it likely only for an application where it can be recalibrated (lab).
- It would be useful to understand how sensor requirements derive from system requirements and how success against these requirements would impact system performance and life.
- Data presentation could be improved to show performance relative to requirements, time scales relevant to response times.
- The sensors are primarily for fuel processor-based systems. Since these will be stationary systems, the performance requirements are different from those needed for automotive systems and these different requirements should be incorporated into program, such as longer lifetimes and, perhaps, somewhat higher costs.
- PI did not discuss progress against established cost targets.
- Need to share data to other sensor developers. Sensor is not UTC core business.

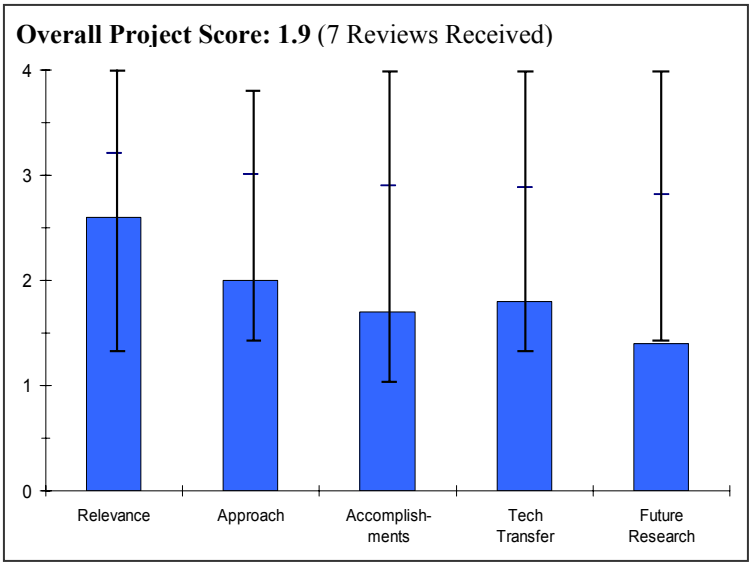
Specific recommendations and additions or deletions to the work scope

- Stack sensor tested 2.5-40% hydrogen - should be evaluated to 100% hydrogen.
- H_2S sensitivity needs to be measured for all sensors.
- RH sensitivities need to be measured for all sensors.
- 10 ppb H_2S detection limit should be lowered. 10 ppb levels of H_2S still are an anode poison.
- This project is coming to a close.
- Program should culminate with a relevant database available to all participants in the DOE program.
- Given Hydrogen Program re-direction, majority of remaining work should be on RH sensor.
- Need to work with a few OEM on the development and validation.

Project # FC-31: 2005 DOE Hydrogen Program Sensor Development
Gehman, Richard; Honeywell

Brief Summary of Project

This Honeywell project is leading to the creation of physical sensors suitable for monitoring and controlling a proton exchange membrane fuel cell system. Key tasks include defining sensor requirements, developing sensors, building and testing prototype sensors, and field testing.



Question 1: Relevance to overall DOE objectives

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

- RH sensors are needed for anode, cathode, and fuel processors.
- Achieving DOE objectives will require well-controlled fuel cell operation, which in turn requires accurate and stable sensors of operating parameters and conditions.
- Although program was well planned, disappointing results will result in program termination.
- This program in impossible to rate. Honeywell's effort may have been good but the results were not acceptable to the DOE program.
- Automotive-grade RH sensors would contribute to successful fuel cell commercialization.
- There was less need for development of other physical sensors (T, P, air flow), which is reflected in the mid-program decisions to de-emphasize their development.
- Inexpensive, fast, robust sensors will be essential to deployment of reliable fuel cell power systems.

Question 2: Approach to performing the research and development

This project was rated **2.0** on its approach.

- Approach did not work (in hindsight).
- The project is not only "unlikely" to meet objectives; the project has not met objectives one by one until it has reached the point where the PI is voluntarily terminating the project early.
- The PI was conscientious to address technical barriers in the DOE technical plan and industry needs.
- The PI was conscientious to work with potential customers to ensure that research and development met their current needs.
- The approach is "poor" overall because the PI is simply giving up.
- Approach was sound, goals well defined; results were fairly and honestly reported.
- Approach of trying to specify, adapt and package existing technology has proven to not achieve goals.
- The discussion on hydrogen sensing does not sound right. There is a real need for wet hydrogen flow metering.

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

This project was rated **1.7** based on accomplishments.

- Design for RH sensor did not show stability. Possible that pre-conditioning might make it more stable.
- The project is terminating early for failure to meet its overall technical objectives.
- When asked what useful information was generated by this project despite its early termination, PI rattled off a list of achievements that could play roles in future sensor development.

- Examples of valuable lessons learned include methods for dealing with condensation in relative humidity sensors, methods for decreasing response time, and methods for measuring airflow.
- Results will lead to no meaningful enhancements to DOE program.
- Cannot fault investigators for re-direction to hydrogen, which made the majority of the physical sensors obsolete.
- Some progress on the RH sensor, which still has application to DOE automotive goals.
- Project apparently terminated and company investment stopped.
- Little to show for effort.
- Lack comparison against sensors on the market. How do these benchmark against the existing sensors?

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

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

- The PI described much effort to coordinate work with potential automotive customers in order to meet their needs.
- This project suffered from Day 1 with "not invented here" syndrome- project was scoped to apply applicant's existing product line to fuel cell application, rather than to use the applicant's knowledge, skills, and experience to seek out and apply best available technology from whatever source.
- Contractor had all necessary in house resources to accomplish this effort.
- No indication of useful collaborations.
- No technology to transfer.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.4** for proposed future work.

- Project being terminated.
- The PI should be commended for his honesty to halt a line of research when it became clear that this line would not meet program objectives.
- When asked about thoughts for directions of future research on relative humidity sensors, PI rattled off a list of ideas, such as designing a sensor based on thermal conductivity.
- It is incongruous that the project voluntarily terminated early rather than beginning to address alternate means identified that could have advanced program and project goals.
- More or less N/A as program is scheduled for early termination.
- Unclear as to why RH sensor was judged not promising for further development.

Strengths and weaknesses

Strengths
- Commendable honesty
- Developed transferable knowledge about techniques in design of sensors for automotive fuel cell applications.
- Good approach and objectives, contractor developed and tested sensors in accordance with plan.
- The team has strong capabilities in physical sensor development.

Weaknesses
- Business case does not work for most sensors proposed.
- Narrow scoping/outlook of project to adaptation of applicant's current products led to questionable decision to terminate project early.
- Contractor discovered three of the four sensors were irrelevant to DOE Program; fourth sensor could not satisfy program objectives.
- Unfortunately Honeywell decided to terminate its effort for business reasons and no sensor products that could be "shoe-horned" into the DOE program.

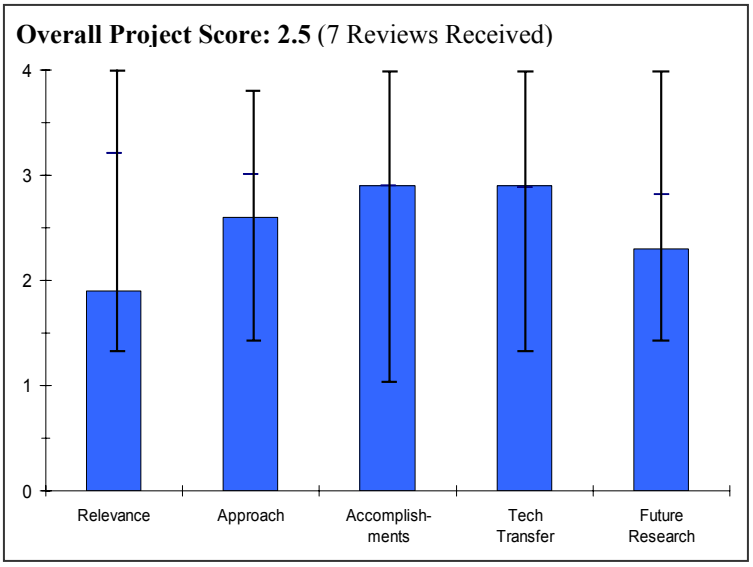
Specific recommendations and additions or deletions to the work scope

- Since approach is not working, and apparently will not work, project should be terminated.
- Possible that pre-conditioning of sensor/and sensor material might limit sensor baseline drift.
- The RH sensor performance should be compared to that in the Honeywell program, and against other concurrently developed commercial and lab-grade devices. Perhaps there is still an application here?
- Ensure that learning's, especially on RH sensor, are documented and made available to continuing programs.
- Decision to terminate project seems correct.

Project # FC-32: DMFC Prototype Demonstration for Consumer Electronics Applications
Sievers, Bob; MTI MicroFuel Cells

Brief Summary of Project

This program, led by MTI and including a number of industrial partners, will deliver a demonstration and real-world validation of a complete, integrated portable direct methanol fuel cell (DMFC) system for consumer electronics. High volume manufacturing techniques will be developed to reduce cost to levels appropriate for consumer electronics market entry. The fuel refill infrastructure and product design will be driven by the evolving regulatory framework, such that unrestricted and ubiquitous use of methanol powered devices can be achieved.



Question 1: Relevance to overall DOE objectives

This project earned a score of **1.9** for its relevance to DOE objectives.

- DMFC (direct methanol fuel cells) fit fairly well in the hydrogen fuel initiative program.
- The fuel is methanol and not hydrogen.
- Consumers will be exposed to fuel cells pre cars and stationary applications which is beneficial.
- I like the application, but the sponsor and contractor need a much more effective way of articulating the synergy of this program - it looks like corporate welfare.
- Not clear at all what the $6.2 Million is paying for.
- Not clear which DMFC technology could translate to other hydrogen fuel cell initiatives.
- Critical for early adoption of fuel cells, but it's not clear if timing fits 2010 goals.
- The relevance of the project lies in the advancement of fuel cell technology generally. The project has less relevance to the DOE mission to reduce energy consumption and reduce the Nation's dependence on foreign oil. A successful demonstration will provide an opportunity to advance manufacturing process technology for fuel cell technology.
- The connection between small-scale DMFC's for consumer electronics and the future hydrogen economy (e.g., stationary and/or transportation) is not clear to the reviewer and was not well articulated.
- Synergies between DMFC's and stationary/transportation applications seem to be weak. Even if similar membranes are used, DMFC work does not appear to be advancing membrane technology, and the membrane volume even at high production levels for consumer electronic applications is miniscule compared with the volumes required for transportation (see recommendations below for calculations). Thus, little stands to be gained from economies of scale/learning curve standpoint.
- The DMFC technology is much nearer the market requirements for performance, cost, size, and weight than for other fuel cell applications. Thus, it seems to the reviewer that DOE funding of such applications is not required and will be undertaken by industry.
- While it is important to get some fuel cells out there, it is unclear how this helps toward any goals; technology is totally different from what will be used for any of the main applications.

Question 2: Approach to performing the research and development

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

- The approach of balance of plant and manufacturing is well thought out.
- MEA (membrane electrode assembly) optimization work is very important.
- MEA work was not covered sufficiently.
- The project has a lack of focus on durability. The project has no goal to improve efficiency, which may lead to some of the projected power goals.
- Reducing cost via system simplification is good, but the cost versus volume was not adequately described.
- Design looks mature already, and presentation gave impression that it was - no significant technical challenges were outlined or explained.
- What is the manufacturing process? No flowcharts explained, nor itemization of process challenges.
- What explicit steps will be taken to reduce cost?
- Approach seems overly simple.
- Plans to reduce MEA cost not addressed.
- Approach is focused on developing low cost manufacturing processes. This approach may not meet all of the DOE targets without hybridizing the system.
- The approach is well thought out. In order to replace batteries in consumer electronics, a low cost manufacturing process is a necessity.
- Simplified system approach is excellent.

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

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

- DMFC prototypes have been built by PI's company.
- Codes and Standards have been examined and modified.
- Need much more data, performance of cells AND details about mfg, e.g. QA and SPC.
- Codes & standards work gives a good impression, and the vision of the product is clear.
- This is difficult to judge because so little was said about technical challenges and what was being done to address them.
- Status against objectives was well-presented, but not well-explained.
- Reasonable progress given start date.
- Significant progress is evident in less than a year. Codes and standards development is important. However, a significant development effort is still required to achieve the project technical targets. Also a low cost manufacturing process still remains to be defined.
- Results to date very promising
- Annoying lack of detail on anything technical, other than the blandest platitudes. We need to know what the detailed needs are to address key hurdles.
- Just getting started.

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

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

- PI works with many partners.
- The PI needs to discuss partners' roles and future work with greater detail.
- Very company and application-specific program.
- The synergies and connectivity to hydrogen systems need to be much better explained.
- Good collection of partners with vested interest in technology.
- Unclear if partners are contributing.

- Collaboration with other organizations is evident. Duracell provides an entry into the very large consumer battery market.
- High degree of collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- PI presented good road map for more changes in codes and standards.
- Good work plan for manufacturing improvements.
- PI needs to present future work plans of partners - none was discussed.
- While the objectives and performance metrics are clear, the pathway and work required are not.
- Need to define timelines and roadmap.
- MTI indicated that the specific fuel cell system design has not been finalized. Issues remain with regard to membranes, air and thermal management and fuel concentration. Technical targets have not been met requiring further development. Most of this development will apparently be done by their suppliers. Little or no details were provided. Emphasis in the presentation was on manufacturing studies.
- Good plan for company goals but little insight into how these align with needs of the fuel cell community.

Strengths and weaknesses

Strengths
- MTI is one of the sole American companies that manufacture DMFC systems.
- MTI has experience in this field.
- Product development and commercialization focus, especially with regard to codes & standards, is excellent.
- Collection of collaborators.
- Work to establish standards.
- Passive fuel delivery approach.
- Good teaming is evident with their manufacturing partners. Good cost share. Leading the way on codes and standards.
- Solid plan.
- Excellent work toward commercializing product.

Weaknesses
- PI needs to further discuss roles of partners - MEA is key design element and nothing was mentioned.
- PI needs a realistic view on true energy density and power density taking into account the whole package (including methanol) to be truly comparable to Li-ion batteries.
- Lack of explanation of synergy to DOE goals.
- No focus on MEA cost.
- Lack of timeline/roadmap.
- Lack of overall technical detail – talk was more of a marketing talk than a technical talk.
- Still a long way to achieve the technical targets. Not all of the objectives may be met.
- Why is DOE funding this? In a time of limited resources, DOE needs to maintain focus.
- What will the field learn? This project is simply pitched as a black box- shovel money in, we'll try to make a product but little insight provided into who, what, when, where, how or why.

Specific recommendations and additions or deletions to the work scope

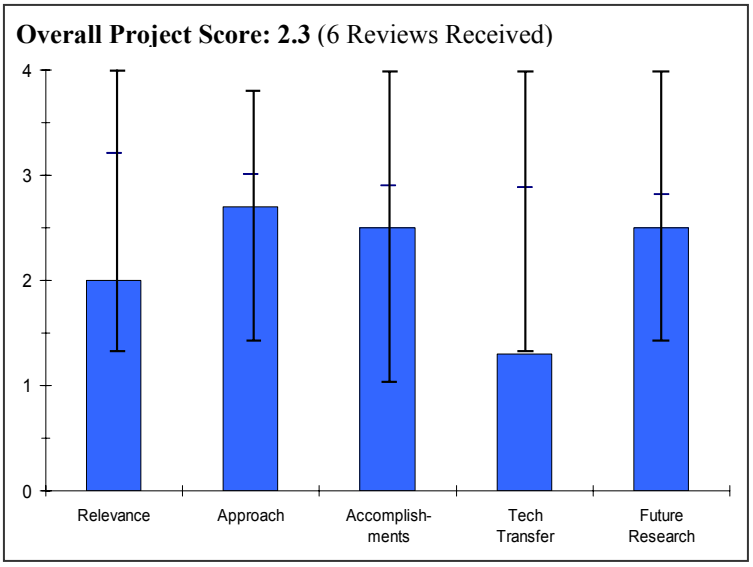
- The presentations are supposed to be non-proprietary. It is better not to show poor resolution images than put them in the presentation and say they are proprietary.
- Roadmaps should detail how the required technical advancements are going to be achieved.
- It is not clear to the reviewer why the DOE should be funding this technology development.

- As a further specific example for the low degree of potential benefit from economies of scale to be achieved from cell phone applications, it should be noted that even at a high sales volume of 1,000,000/year, the annual total production, on a power basis (which will be roughly proportional to total membrane area) would be around 450 kW (assuming a 450 mW device, per the 2007 projections in the presentation). This is roughly equivalent to a whopping 3 stationary power plants (150 kW per plant), or roughly 6 vehicle stacks (at ~75 kW/stack). Thus, the small-scale consumer electronics market would do next to nothing to develop the large-scale manufacturing processes and supplier base required for stationary and/or transportation markets for membranes, which is the ultimate goal and will be required to have any impact on energy consumption, petroleum imports, etc. Similarly, there is no overlap on balance of plant, and little, if any, overlap of manufacturing or assembly processes for the stacks themselves.
- Minimize the length and dollar commitment for this project. The company should be compelled to provide an intellectual contribution to the field. Collaborators should be named so that the key contributors at state of the art are identified.

Project # FC-33: DMFC Power Supply for All-Day True-Wireless Mobile Computing
Wells, Brian; Polyfuel, Inc.

Brief Summary of Project

PolyFuel, Inc. is developing a novel direct methanol fuel cell (DMFC) system for use as an all-day power source on a laptop computer. The general approach is to simplify the system as much as possible, particularly by reducing the overall size and complexity of the balance of plant. The heart of the project is the development of a next generation hydrocarbon membrane that will be engineered to best meet the requirements of the complete DMFC system. Work will be done to miniaturize and integrate the remaining balance of plant components into a package small enough to fit into a laptop computer.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.0** for its relevance to DOE objectives.

- DMFC (direct methanol fuel cells) fit fairly well in the hydrogen fuel initiative program.
- The fuel is methanol and not hydrogen.
- Consumers will be exposed to fuel cells pre cars and stationary applications, which is beneficial.
- Not clear which DMFC technology could translate to other hydrogen fuel cell initiatives.
- Critical for early adoption of fuel cells, but it's not clear if timing fits 2010 goals.
- Project is aligned with DOE goals for portable power fuel cells.
- It is difficult to make a case that this project supports the President's initiative, although there are some commonalities in membrane and catalyst areas. That said, successful commercialization of this technology would attract investment in fuel cell technology, which will have considerable benefit to the Initiative.
- The connection between small-scale DMFC's for consumer electronics and the future hydrogen economy (e.g., stationary and/or transportation) is not clear to the reviewer and was not well articulated.
- Synergies between DMFC's and stationary/transportation applications seem to be weak. Even if similar membranes are used, DMFC work does not appear to be advancing membrane technology, and the membrane volume even at high production levels for consumer electronic applications is miniscule compared with the volumes required for transportation (see calculations below). Thus, little stands to be gained from an economy of scale/learning curve standpoint.
- The DMFC technology is much nearer the market requirements for performance, cost, size, and weight than for other fuel cell applications. Thus, it seems to the reviewer that DOE funding of such applications is not required and will be undertaken by industry.
- While it is important to get some fuel cells out there, it is unclear how this helps toward any goals; technology is totally different from what will be used for any of the main applications.

Question 2: Approach to performing the research and development

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

- The PI and Polyfuel have recently developed a DMFC membrane and MEA (membrane electrode assembly) that demonstrates some strength -- lower MeOH cross-over.
- The PI has no experience building a DMFC system.

- The PI has no experience with electronics.
- System design – membrane interaction approach is well thought out.
- Approach to co-optimize the system strategy and membrane should lead to best overall solution.
- The approach being pursued appears to be a rational plan for building a DMFC system around the PolyFuel membrane platform.
- It is unclear that PolyFuel, as a membrane company, has all of the required competencies to carry out the work required to design and build a system that will be competitive in the market.
- The approach does not come close to the goal suggested by the title.
- The central technology here is a new membrane but the balance is weak, even if one accepts the implicit premise that the membrane can solve all problems.
- Desperate need for partnering.

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

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

- Membrane work towards 1M MeOH feeds have been demonstrated - no work with neat MeOH.
- No work on real systems - only models.
- Reasonable progress with new membranes given start date.
- Have defined requirements for the power supply.
- Have developed preferred architecture and identified key membrane properties.
- Based on the way data were presented, it is difficult to assess the technical merits of PolyFuel's membrane platform. Compared to conventional perfluorinated membranes, the PolyFuel membranes have lower methanol crossover and electro-osmotic drag coefficients. However, no quantitative (or even comparative) data on conductivity or fuel cell performance was presented.
- Just getting started.

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

This project was rated **1.3** for technology transfer and collaboration.

- The PI needs to develop strong collaborations with electronic companies.
- No collaborations are in place.
- No collaboration with outside organization.
- At this time, project appears to be self-contained at Polyfuel.
- There is no evidence of external collaborations, except for interactions with vendors for specific components.
- PolyFuel appears to be going at this alone, which is extremely risky.
- PolyFuel should consider adding a team member with expertise in balance of plant and system design to augment the skills they are growing in-house.
- There is a desperate need for partnering to realize the goals of the project. The presenter did not provide a single detail about how they planned to get to a system via strategic partnering and the resources to get a system are not apparent.

Question 5: Approach to and relevance of proposed future research

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

- The PI should focus on their strengths - membrane and MEA development for DMFC applications.
- The PI does not appear to have the personnel or expertise to complete the proposed work on systems.
- Well defined- model work key.
- A lot of 'system' work for a 'membrane' company.
- The future work plan is sketchy, but includes the activities required to proceed with development of a DMFC system.
- Elements of a plan are there but the teaming required for rapid and efficient progress to end-goal is lacking.

Strengths and weaknesses

Strengths
- The PI has shown promising data on new membranes and MEAs for DMFC applications.
- Membrane technology.
- Passive fuel delivery approach.
- Membrane technology and expertise are the main strength.
- The PolyFuel membrane appears to be a good platform for a DMFC system.
- Solid plan.

Weaknesses
- The PI has demonstrated no competency in system design or assembly of DMFC applications.
- No collaborations.
- Could probably benefit from collaboration with systems people or fuel cell developers.
- Lack of collaborators with a stake in the outcome of the project. If vendors are being requested to deliver specialized components, then it might make sense for them to be formal collaborators on the project.
- PolyFuel is going it alone on this project, attempting to develop a "holy grail" product in a market space where other companies (in Germany, Japan and Korea) are many years ahead in system development and have the manufacturing, marketing and distribution infrastructures already in place. If PolyFuel remains intent on going it alone, their chances for commercial success will be nil, regardless of whether they achieve technical success.
- Why is DOE funding this? It seems to be a project driven by a 'squeaky wheel' asking for funding rather than a contributor to the critical needs of the main points of emphasis for DOE.
- This group has long hill to climb to get to a compelling commercialization scenario.

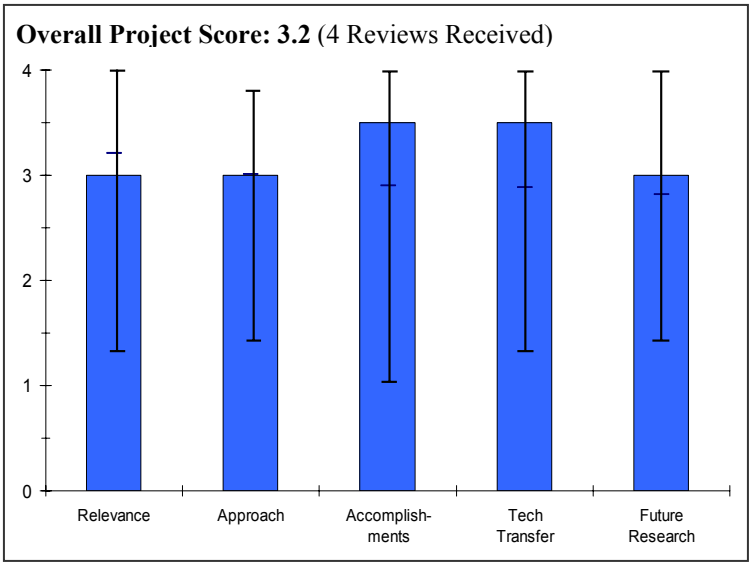
Specific recommendations and additions or deletions to the work scope

- The PI should focus all of their attention on membrane and MEA development. Many questions in that arena still need to be addressed: 1. Durability, 2. Performance gains, 3. Ability to function in different MeOH concentrations.
- The PI should abandon DMFC system design unless a strong collaboration with an electronic company is formed.
- Would recommend they interact with some systems developers/modelers.
- This project requires real milestones with quantitative metrics, so that progress can be more accurately assessed.
- It is not clear to the reviewer why the DOE should be funding this technology development.
- As a further specific example for the low degree of potential benefit from economies of scale to be achieved from laptop applications, it should be noted that even at a high sales volume of 1,000,000/year, the annual total production, on a power basis (which will be roughly proportional to total membrane area) would be around 15000 kW (assuming a 15 W device, per presenters comments). This is roughly equivalent to a 100 stationary power plants (150 kW per plant), or roughly 200 vehicle stacks (at ~75 kW/stack), or about 0.001% of the vehicle market. Thus, the small-scale consumer electronics market would do little to develop the large-scale manufacturing processes and supplier base required for stationary and/or transportation markets for membranes, which is the ultimate goal and will be required to have any impact on energy consumption, petroleum imports, etc. The case for economies of scale/development of supplier base, is somewhat stronger for laptops than for cell phones (due to the larger power requirement for laptops), although still weak. Similarly, there is no overlap on balance of plant, and little, if any, overlap of manufacturing or assembly processes for the stacks themselves.
- Minimize the length and dollar commitment for this project.
- Put maximum effort into building a vertically integrated team.

Project # FC-34: Direct Methanol Fuel Cells
Zelenay, Piotr; Los Alamos National Laboratory

Brief Summary of Project

This Los Alamos National Laboratory (LANL) project on direct methanol fuel cells (DMFCs) currently targets development of novel catalysts for DMFC cathode with smaller particle size, higher activity, better methanol tolerance and lower cost (focus on both Pt-based and non-precious materials); improving DMFC stability by minimizing or eliminating major factors impacting long-term performance of the DMFC (catalyst and ionomer loss, ruthenium crossover, etc.); and design of membrane-electrode assemblies, both Nafion and alternative-membrane based, with superior performance and improved long-term stability.



Question 1: Relevance to overall DOE objectives

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

- Key objective for the President's military readiness "electronic soldier."
- May support President's Initiative, but alignment to DOE goals not clear for electrocatalysis and membranes & MEAs.
- Identify goals/barriers to be overcome more explicit for electrocatalysis and membranes.
- DMFC may provide early commercialization opportunities which could mitigate industry disenchantment with distant hydrogen fuel cell.
- Clearly focused on R&D goals. Project is critical to realization of portable power targets.

Question 2: Approach to performing the research and development

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

- Excellent overall approach on each critical area of DMFC performance/durability.
- Approach for each area included in this project is not consistently clear.
- Lower-crossover membrane and durability are primary barriers and need to be addressed.
- Project utilizes expertise from a number other sources. In past year, project has added emphasis on industrial collaborations to capitalize on impressive lab achievements.

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

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

- Catalyst improvement.
- Membrane evaluation/analysis/advancement.
- Achieved high specific power in stack assemblies, although stack work outside scope of this program.
- Close to achieving 2006 goals. Durability improvement remarkable: Only 15% loss in 3000 hrs.
- Progress to milestones very good, but direct linkage to DOE goals not clear for electrocatalysis and membranes/MEA research. Very good progress for stack performance for portable applications.

- Significant technical progress achieved on multiple fronts. A number of milestones achieved or surpassed ahead of schedule.

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

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

- Significant improvement over 2004 – especially the addition of Mesoscopic Devices.
- While industrial collaborative efforts are being negotiated, not enough information is provided to determine if they will be enough.
- Excellent transfer through publication, sufficient breadth of collaboration partners, and licensing effort.
- Excellent collaboration on all fronts especially with academia. Probably could benefit from further industrial collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Agree on PI's suggestion of work on electrode structure and elucidation of best structure.
- Future objectives are appropriate to advance technology.
- Assuming advances in lower-crossover membranes, then methanol-tolerant cathode would become lower in priority.
- Strong work plan for future to address remaining DMFC barriers. Didn't discuss much on contingencies or alternate paths. Probably not much of an issue given the projects broad approach.

Strengths and weaknesses

Strengths
- Probably the highest "value" of return on research investment of any of the DOE supported programs: outstanding combination of fundamentals, and practical solutions to overcome limitations of the technology.
- Project focused on achieving milestones.
- Specific milestones identified.
- Excellent project. Well designed and managed with impressive technical achievement.

Weaknesses
- Level of funding commitment not on par with the needs of the community.
- Missing a strong modeling component to guide proposed electrode design.
- Linkage to DOE goals for electrocatalysis and membrane activities not clear.
- No significant weakness.

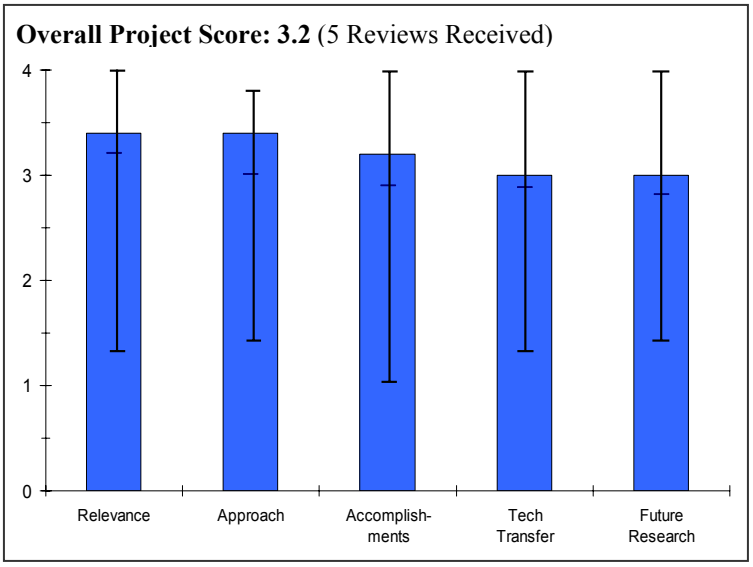
Specific recommendations and additions or deletions to the work scope

- Add off/on cycling.
- On the strategy for using concentrated MeOH: why stop at 5M? Pure MeOH should be the final goal to maximize storage energy density.
- Form alliance with modeling group to solve issue of electrode design: Professor Newman?
- Provide funding breakdown for each topic area.
- Clarify approach for each topic area and goals for those areas not already containing them.
- Consider further simplifications in water management.

Project # FC-35: Fuel Cell Systems Analysis
Ahluwalia, Rajesh; Argonne National Laboratory

Brief Summary of Project

For this project, Argonne National Laboratory (ANL) will develop a validated system model and use 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 versatile system design and analysis tools, and to apply the models to issues of current interest.



Question 1: Relevance to overall DOE objectives

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

- Validated system model.
- Gives important feedback of which areas to focus on and which combinations of stack operating conditions and BOP give the best system efficiency and flexibility.
- Effective simulation essential for (1) establishing proper targets (2) minimizing experimental hardware (3) minimizing test requirements.
- Did not compare adequately his anti-freezing strategy with other alternatives.

Question 2: Approach to performing the research and development

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

- Writing code, obtaining results and validating. Work on humidifier models excellent and long overdue.
- Covered both automotive and stationary needs.
- Validation of models with laboratory data is very important to continuously improve models.
- Appears good but little verification by comparison with actual data; virtually no explanation of models or simulation; without verification, results of complex processes are highly suspect.
- The objectives of this project should be updated since the DOE program has included the Technology Validation.
- This program should focus on understanding the data from the Technology Validation program on the fuel engine part of the vehicle and then develop generic models to evaluate it.
- Support DOE on goal setting and Metrics.

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

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

- Experimental validation of humidifier models outstanding. Stack freezing/startup models broadened understanding. Hybrid/ fuel cell models comparison helpful.
- Very interesting data on humidification options and start-up from subfreezing.

- Utilization of the simulation has been rather limited. No convincing verification that results are valid (they probably are but no evidence is presented). This can be an extremely important tool but needs verification and much more utilization.
- Lack collaboration with NREL and other universities that are developing models of fuel cell automotive systems.

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

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

- Excellent but could add OEM stack/system developers. Many excellent presentations.
- Validation of models with data from fuel cell developers is important.
- Primary collaboration seems to be with Honeywell. More collaboration with other developers is needed.
- The interface with NREL seemed weak. This relationship should be strengthened to help the program establish validity with model results.
- Responsive to last year's reviewer comments.

Question 5: Approach to and relevance of proposed future research

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

- Inclusion of CHP applications for future was a plus.
- Good plan; study of CHP applications will be helpful.
- Please continue to investigate good solutions for high-temperature membrane systems for atmospheric, low RH, 120°C membranes.
- Important issues are planned for investigation, but more verification and more collaborative investigations are needed.
- Weak future work proposal, the future work first priority item should be heavily focused on utilizing data from the Technology Validation program to bring validity to the vehicle-based model.

Strengths and weaknesses

Strengths
- Solid approach with excellent validated results for many models.
- Good breadth of systems and applications studied.
- Good capabilities in developing models and simulation techniques have been previously demonstrated.
- Has a pretty good model to build on.
- The energy measurements give us a realistic overview of the FreedomCAR goal setting.
- Insulated stack with electrical re-heating appears to be an efficient strategy to prevent freezing. Start up only once every 30 hrs at -20°C.

Weaknesses
- Some models cannot be verified now because no complete system data are available.
- Have made little effort to establish credibility of simulation capabilities through verification by data comparisons.
- Have made little effort to explain models and simulation techniques (and corresponding limitations, which all models and simulations have).
- Model lacks real world vehicle data to validate system simulation results.
- Is there any collaboration with NREL on this particular topic or is this just overlapping work?
- Continue work on freeze-start of fuel cell systems.
- Continue collaboration with Honeywell on thermal and water management system.

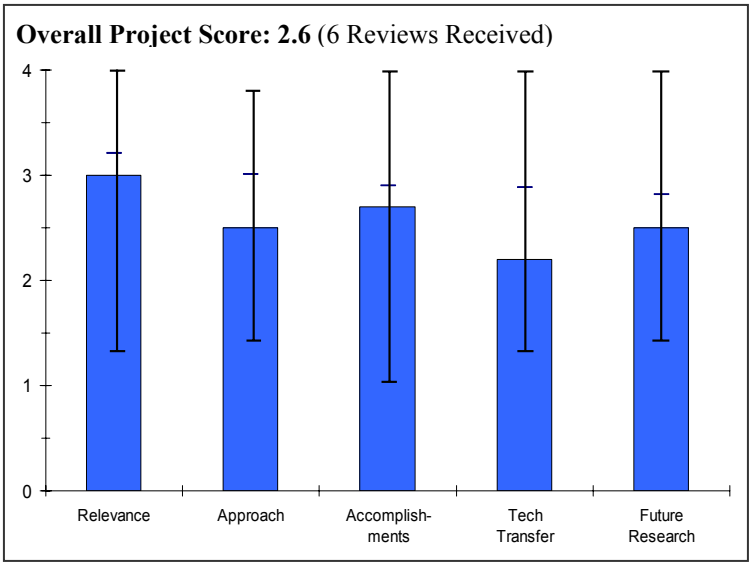
Specific recommendations and additions or deletions to the work scope

- Incorporate stationary systems in models as another means of code validation for a real system.
- Suggest doing more modeling for 120°C operation to identify favorable stack hardware and operating conditions as well as critical BOP elements for both stationary and automotive applications. Suggest that model is based on best existing High Temperature Membrane (HTM) from current DOE contractors, as well as 2010 DOE HTM targets. Present results at HTMWG meeting for discussion and further input.
- Prepare and make available documentation to inform others of systems analysis capabilities (and limitations).
- Expand collaborations.
- Verify models with data where possible and note limitations where verification is not possible.
- I would like to see more collaboration between ANL and NREL on this topic.
- Continue work on freeze-start of fuel cell systems.
- Continue collaboration with Honeywell on thermal and water management system.
- The Technology Validation program provides critical real world data to ANL model yet I do not see a high priority to use such data to improve and build credibility with their model. This should be this program's first priority for this year and the coming years.
- Energy storage should continue to be an important part of systems analysis.

Project # FC-36: Bipolar Plate-Supported Solid Oxide Fuel Cell "Tuffcell"
Carter, J. David; Argonne National Laboratory

Brief Summary of Project

Argonne National Laboratory (ANL) is developing an improved solid oxide fuel cell (SOFC) for auxiliary power units and other portable applications that address SOFC startup time, durability to temperature cycling, vibration and shock resistance, and materials and manufacturing costs.



Question 1: Relevance to overall DOE objectives

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

- This project seeks to develop low-cost, SOFC bipolar plates with sufficient durability to meet DOE cost and performance goals.
- Approach could solve some long standing issues associated with SOFC's.
- The project fits directly in the hydrogen program. It is a revolutionary but ambitious proposal because of the technical barriers inherent between differences in the metal and ceramics properties.
- APU development seems to have a loose relationship to the Hydrogen Program. APUs will assist SOFC commercialization primarily in stationary applications but will also assist application to hydrogen production via electrolysis. Reduction of dependence on foreign oil does not appear to be a significant driver for this work.
- In particular, the advantage in cost of the presented design versus anode design was based on erroneous assumptions with respect to current SOFC anode supported technology. Typical anode supported designs are 0.3 - 0.5 µm and may also be fired in one step. The current < 0.5 µm anodes invalidate the cost comparison and the number of firing steps on a 24/7 basis using high quality industrial equipment is not significant even with multiple firing steps.
- Project supports goals and objectives of RD&D plans.

Question 2: Approach to performing the research and development

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

- The approach supports development of metallic bipolar plates to improve durability while minimizing the requirement for high-cost ceramics.
- The project started in October 2001. Technical barriers are expected because the PI must master the processing techniques of different materials to get adhesion between metal and ceramics, sintering, etc., at a relatively low temperature.
- The essential difficulty with a monolithic single fired stack is the yield. It is impossible to QA single cells after firing prior to forming the stack with this design. Given the series connection one poor or bad cell invalidates the entire stack. The yields of ceramic parts are a difficult problem and single fired stacks essentially make the issue untenable.
- Performance in a single fired stack (particularly large stack) is usually substandard due to the incompatibility of the multiple component firing schedules, i.e., different materials require different temperatures. In this case the multiple materials are exposed to identical conditions which do not optimize performance.
- The metallic support plate is ductile, therefore it must be thicker than a non-ductile ceramic to ensure twisting or out-of-plane deflections do not occur. The cost-benefit analysis of the much thicker metal versus ceramic is not addressed.

- Project has solid conventional approach focusing on technical barriers. Well received, strong emphasis on cost analysis up front. Project should follow through on future emphasis of improving single cell performance.
- Metal seal design is interesting. Approach does represent a cost saving over thick 1 mm anode-supported designs.

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

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

- Good progress with cost analysis completed, 2- and 3-cell stack completed and tested and single cell tests on cycleability and start-up initiated.
- Improved seal design - now need to improve cell/stack performance.
- Stack test results are not satisfactory. PI notes progress in spite of difficulties and small amount of funding. PI must interact with SOFC developers.
- Given the fundamental limitations of the approach, reasonably good progress. Fifty percent complete. The performance results to date have been relatively poor. Low power density will add to cost of the stack as a first order effect. The demonstrated start time of approximately 80 minutes is not a significant improvement with respect to other SOFC designs that have been designed for high cycle applications. A < 30 minute start would be a significant improvement but this has not been demonstrated. Although a significant improvement, a < 30 minute start would not meet automotive requirements of < 1 minute.
- Solid technical accomplishments especially innovative cell design and self-sealing concept. Further progress needed on improving power density and overall single cell performance.
- 0.14-0.25 mW/cm^2 is too low. Needs more careful stack development.

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

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

- Collaborators and interested parties participating in project. "Hope" to transfer technology.
- There is no indication about technology transfer yet. Must interact with SOFC developers.
- Korea Advanced Institute. Motorola. Idaho National Laboratory. These organizations have expressed an interest but are not actually collaborators.
- Project would benefit from increased collaborations especially at the basic research level to improve cell performance. Mentions collaboration with Korea Advanced Institute of Science and Technology but is not clear on the extent of collaboration.
- No evidence presented.

Question 5: Approach to and relevance of proposed future research

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

- A more detailed plan for future work is needed.
- Focus on improving cell/stack performance including long-term performance. Find ways to look at performance of individual components and interfaces (i.e., find sources of poor performance and focus on improving these).
- Technology transfer, scale-up and system integration, and stack demonstration are planned for the future.
- Based on the presentation, future effort appears focused on demonstrating the technology. This is very premature given the level of maturity presented. More development and proof-of-concept on a small scale is required.
- Solid conventional future work plan. Work plan would benefit from more details and explanation of potential paths to improving single cell performance with built-in contingencies.

Strengths and weaknesses

Strengths

- Good analysis and testing program underway.
- Novel stack design which could overcome some SOFC stack hurdles.
- Progress in spite of the difficulties of processing different materials to get adhesion between metal and ceramics, sintering, etc, at relatively low temperature, and small amount of funding.
- Solid project which has identified and shown feasibility of innovative SOFC cell design which can lower costs as well as self sealing concept.
- Metal supported SOFC may be important for automotive and APU applications. Approach does eliminate sintering steps.

Weaknesses

- Start-up time 10°C/minute is still slow.
- Poor cell/stack performance.
- Interactions with SOFC developers.
- Not clear if cell performance will be completely acceptable.

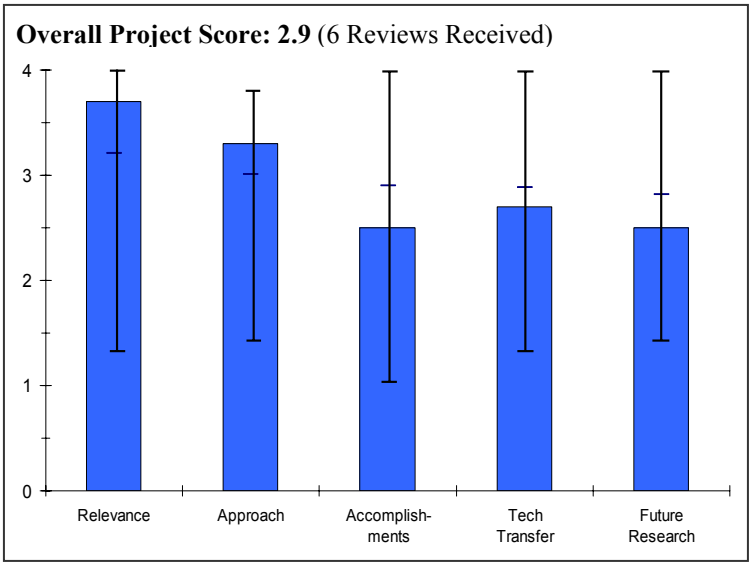
Specific recommendations and additions or deletions to the work scope

- Need experimental data showing improved stack performance.
- As previously outlined in this review, monolithic firing of complete stacks has fundamental issues, among the more important, meeting cost-effective yield requirements. The basic concept of co-firing a powder metal mixture and the ceramic on a cell level does have some interest. There are many technical uncertainties that will require development. Continued work should focus on optimization at the cell level.
- Monolith designs (co-fired) are difficult development efforts.
- Project would benefit from expanded collaborations especially at the basic research level to improve cell performance.

Project # FC-37: Effect of Fuel and Air Impurities on Fuel Cell Performance
Garzon, Fernando; Los Alamos National Laboratory

Brief Summary of Project

This task is focused on evaluating and alleviating the effects of impurities in the fuel and oxidant streams. The effects of fuel impurities such as hydrogen sulfide and air impurities such as sulfur dioxide and nitrogen oxides are specifically targeted in this study. The investigation includes the elucidation of potential mechanisms of action on fuel cell components. One important goal during this year is to develop a method for cleaning sulfur-poisoned catalyst layers.



Question 1: Relevance to overall DOE objectives

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

- This program is important and will identify PEMFC operation in gas environments that are real (not clean and ideal).
- A better, and more widespread, understanding of these effects will certainly be beneficial.
- Diagnostic methods and instrumentation are critical to the development of Fuel Cell basic science development.
- Temperature and Humidity measurement is useful; it is questionable if a temperature sensor alone is useful.
- Diagnostic instrumentation and methods must be as nonobtrusive to the existing fuel cell as possible in this analytical development.

Question 2: Approach to performing the research and development

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

- The PI should consider putting more emphasis on understanding mechanisms and sources of impurities and less on recording functional responses (studying effects?) to various impurity challenges. Many sulfur-bearing impurities can originate in the membrane electrode assembly itself as decay products which can then poison the catalysts. These cannot be filtered out as easily as externally generated impurities.
- LANL has assembled a good team. ORNL provides critical post-test analysis.
- Evaluating both the effect of impurities and the possibility of recovery is good.
- Including both dry and wet conditions is good too.
- Focus on CO/NH_3/ H_2S /HCN reformer products should be rethought relative to the list of contaminants of concern found in the Hydrogen Fuel Quality specifications and in the JARI list of species. Not convinced that diesel fumes are that big a problem.
- I recommend the elimination of this aspect on the approach because this should be conducted by the fuel cell manufacturers.
- Find ways to mitigate negative effects of impurities.
- They should know their system best and how to overcome the discovered issues by this research.

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

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

- Although a fairly new program, it was not clear how much of the results shown were really new or just revisiting prior work done at LANL (e.g. pulse method for cleaning catalysts, and impurity studies from several years ago).
- It appears the progress is slow and not consisted with the funding level.
- Showing that sulfur contamination is reversible is excellent, however an even simpler recovery process is possible, which is simply to expose the stack to air on both electrodes. No external power supply is required and no worries about individual cell voltages in the stack with no unusually high potentials on some electrodes.
- Would have been nice to show some foreign cation results as well (e.g., exposure to NaCl).
- The work on SO_2 should correspond to ambient concentrations of SO_2; most locations are not 1.5 ppm.
- Single Cell Test Protocol vital to success of fuel cell program.
- The progress on this research could be vastly expedited if the researcher spends more time on discovering impurities on fuel cells instead of looking for solutions to these issues that are not applicable in a system perspective or could potentially create more problems for the fuel cells.
- A proposal of 1.2 V per cell at a starting point of 0.4 A/cm^2 is just not realistic. Most fuel cell stacks have over 350 cells and the cells are at least 500 cm^2.

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

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

- It would be beneficial for this project to become more closely aligned with one or two fuel cell companies who have been acquiring information on failure modes analysis to learn what kinds of impurities they have experience with.
- Better interaction should be possible at these funding levels.
- Not totally clear how these results will be disseminated to the industry. Will a recommended fuel specification be the end result?
- Strengthen ties to the FreedomCAR Codes & Standards Technical Team (CSTT). Review the project work at one of the CSTT meetings. Also review this work with the FreedomCAR Hydrogen Storage Tech Team for their input
- UTCFC and Ballard should be involved in this process. Are they?
- Is there a joint effort with the work at the University of Hawaii?
- Where is the collaboration with the Codes and Standards and Hydrogen Storage Tech Team on the impurities effects?

Question 5: Approach to and relevance of proposed future research

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

- The current plan is directed at studying the degree to which external impurities affect current catalysts and other MEA components. But if filtration media can be used to minimize these effects, that should be considered as a mitigating strategy also, rather than changes to the MEA components to try and make them more robust in that respect. The internally generated impurities, from membrane decomposition to bipolar plate corrosion are more difficult to deal with and identify, but potentially as important. Their future work should include a plan to address these types of impurities.
- Based on the results thus far, the future work should emphasize the most critical life factors.
- Future plans are not clear.
- I am not convinced that diesel fumes are that big a problem. Do a rough assessment of how real this potential is before devoting a large fraction of resources to it. Sulfur poisoning model development and mitigation good.
- Solid proposal, collaboration with industrial partners and the eFreedomCAR Tech Teams is highly encouraged.

Strengths and weaknesses

Strengths

- Great capabilities and expertise.
- Excellent choice for a DOE-sponsored research. These effects are of industry-wide interest. Conducting tests under both dry and wet conditions ensures that different types of fuel-cell systems are covered.
- Engagement of US Fuel Cell Council is extremely important to the success of this project.
- Developing a good knowledge database on impurities and their effects on fuel cells; this is very important to the overall fuel cell program.
- Future work proposal is solid, but could lack the collaboration with the industrial partners.
- A very important problem.

Weaknesses

- Complete deliverables of this program are not clear. What is the complete list of impurities that are planned? (Changes in plans are understood to occur and are OK, but it is difficult to provide feedback on the plan when the plan is not clearly stated).
- Will fuel and air specifications be a deliverable? Will prevention methods (e.g., filters), as well as recovery methods, be included?
- I am not convinced that impurities from reformers are the highest priority species to be evaluating.
- Many sulfur species (not H_2S) are present in natural gas.

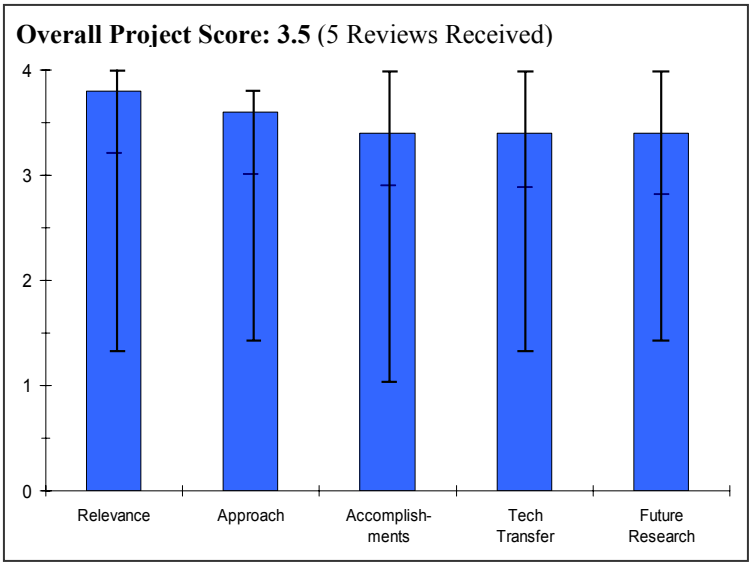
Specific recommendations and additions or deletions to the work scope

- Broaden the scope to include internally generated impurities that can and will result under the stressful automotive operating conditions with recycled water. Understanding the magnitude of those impurity generation levels and dependence on operating conditions will provide a sound basis for determining where improvements in robustness need to be made.
- Focus on gaseous contaminants. Delay studying the effects of salt.
- Attempt recovery from sulfur poisoning by simply exposing both electrodes to air while the cell is still hot. (Although recovery may not always be complete, sufficient recovery may be obtained).
- For each impurity, an analysis should be made using a transportation lifetime target (e.g., 5,000 to 8000 hours) of the maximum exposure of that impurity that can be tolerated without significant performance losses (e.g., > 5%). This could result in some very useful deliverables, for example: 1) Is NaCl in the air in coastal areas an issue for automotive applications, e.g., is mitigation methods (e.g., filters) required or not? 2) Recommended hydrogen purity specifications for transportation applications.
- Consider including the evaluation of the effectiveness of mitigation methods (e.g., filters) for each class of impurity.
- Develop complete list of contaminants, prioritize them, develop a test protocol to evaluate them, carry out the actual testing, and develop a mitigation strategy. Should at least include also formaldehyde, formic acid, and non-methane hydrocarbons (NMHC). This project work should be included in an overall Hydrogen Fuel Quality Specification project tracked under the Codes & Standards Tech Team.
- I would like to eliminate these technical objectives for this program (1) Find materials and devices able to mitigate negative effects of impurities, (2) Develop test procedures for reactivating electrodes poisoned with sulfur compounds. I believe that this is not the best use of National Lab's resources. I would like to see a focus on finding problems that we are not aware of and the mechanism of these problems. Leave the finding of applicable solutions to the fuel cell manufacturers. Please focus on identifying real world impurities which have an adverse effect on fuel cells with industrial and other National Lab partners.
- Need to extend contaminates to PM10 particulates and trace metals.

Project # FC-38: Neutron Imaging Study of the Water Transport Mechanism in a Working Fuel Cell
Arif, Muhammad; National Institute of Standards and Technology

Brief Summary of Project

This National Institute of Standards and Technology (NIST) project is intended to develop an effective neutron-imaging-based, non-destructive diagnostics tool to characterize water transport in PEM fuel cells. Objectives include: providing research and testing infrastructure to enable the fuel cell industry to test commercial grade fuel cell flow field designs; training industry to enable them to use the imaging facility independently; and transferring data interpretation and analysis algorithms/techniques to industry to enable them to use research data more effectively and independently.



Question 1: Relevance to overall DOE objectives

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

- New device will be much more conducive to real fuel cell experiments than current device. Potential for 3D imaging is exciting. Imaging has to be combined with other analytical techniques to characterize water management.
- Understanding basic mechanisms of fuel cell operation.
- Very useful diagnostic and tool enabling further development MEAs and stacks.
- Thermal and water management are key issues for the successful commercialization of fuel cells.
- Diagnostic methods and instrumentation are critical to the development of Fuel Cell basic science development.

Question 2: Approach to performing the research and development

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

- A technique-specific project has to make sure it is applied to right questions and gives answers that are useful. Water transport is a key issue but technique has to be combined with other studies/models to be really useful.
- Fundamental approach to understanding movement of water in plates and membrane.
- Potential for understanding hydrogen storage.
- Unique capability.
- The approach is uniquely qualified for probing MEA/Stack dynamics in real-time.
- Test commercial grade fuel cell for water management.
- Provides infrastructure and facilities for testing fuel cell.
- Evaluate impacts of research.
- Transfer technology.
- Continuous improvement of capabilities of neutron imaging- a very dynamic field- very sensitive to water formation- can look through 10 - 50 microns of water.
- Added real-time imaging in 2004- 30 frames/sec.
- Building a new, larger system in '06.
- Proprietary work now 70%, want to move to 50%.

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

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

- Real time imaging is crucial to studying transient effects. Need to better pin down location of water through the thickness of the cell (membrane/GDL/flowfield).
- Taking independent initiative to improve capability and resolution for larger stack samples.
- New 3D imaging capability is very promising.
- Measured water content as a function of the polarization curve.
- Are developing new approach to disconsolation and reconstruction to do 3D imaging of the fuel cell system.
- Direct measurement of GDL water uptake.
- Are publishing 4 papers this year.
- Are increasing their work with modeling.
- By end of year, increase resolution to 25 microns; in two years, down to 10 microns.
- Faster progress will only help the industry. Please allocate more resources to make this enabling technology available to OEMs and suppliers faster.

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

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

- For this to be really successful, the technique developers will have to do much more than just provide training on operation. They will have to go at least part way in understanding the real questions.
- Extensive list of participants and individual projects.
- Publishing reviewed papers.
- A lot of work with outside institutions.
- Good collaborations; training efforts particularly good.
- Should promote more publications from users.

Question 5: Approach to and relevance of proposed future research

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

- The potential for 3D imaging is very important to utility of the technique for water transport issues.
- Potential for 10 micrometer capability is substantial improvement.
- Real time imaging in 3D should dramatically increase understanding.
- Setting up new imaging unit is critical to success.
- Will complete new facility in Fall 2005.
- Have great plans to expand the usability of this tool.
- Will be able look at laminar flow in all levels of fuel cell stack at once.
- Three dimensional analysis is critical for stack analysis.
- Two dimensional measurement of a single cell is limited. Please make 3-D available ASAP.
- Is it possible to provide a vertical beam for horizontal analysis of fuel cell in operation?

Strengths and weaknesses

Strengths
- Unique experimental capabilities.
- Unique test facility and imaging know-how.
- Took initiative to bring technology to fuel cells.
- Teaching others to use the technique/equipment.
- Unique facility that produces valuable data unavailable anywhere else.
- Continuous improvement increases the resolution and analytic capabilities.

- Good collaboration with industry, national labs and universities.
- Great analysis tool for the industry.

Weaknesses

- Fuel cell researchers may not understand how best to utilize capabilities. NIST analytical experts may not understand what the real questions are.
- Need more exposure.
- Limited publications.
- Need to continue to make results of their work publicly available - need to make steady progress towards 50% public research goal.
- Should provide resources to supply hydrogen and oxygen if possible to small stack analysis.

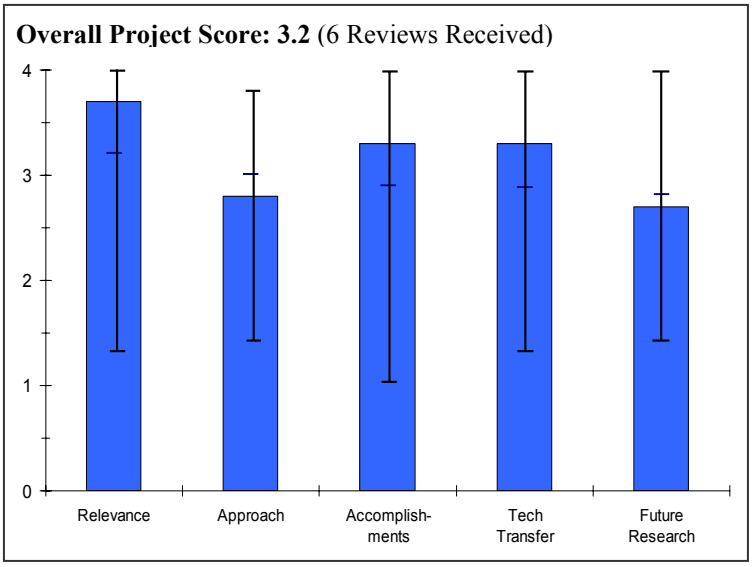
Specific recommendations and additions or deletions to the work scope

- Additional financial support of planned improvements.
- I would like to see more National Labs and Universities involved in this diagnostic process.
- Please commit enough resources to implement the full capability of this diagnostic instrument for the industry and others to use.

Project # FC-39: Microstructural Characterization of PEM Fuel Cell MEAs
More, Karren; Oak Ridge National Laboratory

Brief Summary of Project

Oak Ridge National Laboratory (ORNL) is focused on gaining a better understanding of membrane electrode assembly (MEA) degradation mechanisms, including structural and compositional changes as a function of MEA processing, correlation of microstructure with performance, and morphological changes occurring during MEA aging/use. In addition, ORNL will collaborate with PEM fuel cell developers and manufacturers to evaluate MEAs using advanced microstructural characterization techniques and provide feedback for MEA optimization.



Question 1: Relevance to overall DOE objectives

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

- Development of methodologies and fundamental work to enable evaluation of factors affecting the durability is highly relevant.
- Providing fundamental diagnostic tool to evaluate MEA durability.
- This work requires extraordinary analytical techniques. Results are critical for PEMFC success.
- Characterization of the structural and morphological changes of the MEAs components caused by aging is important to find ways of improving fuel cell durability.
- It is important to understand degradation in PEM systems, and this project makes sense on that pathway.
- Very relevant to durability.
- Impact could be much greater if scope were wider.
- Need to work on industry-standard materials.

Question 2: Approach to performing the research and development

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

- Good progress in technique development, approach is well-thought out and expanding on the ways to evaluate important factors such as uniformity of 3-phase structure.
- Solid techniques developed.
- Need more effort to determine decay mechanisms.
- Proper analysis and interpretation of post-test results are the primary focus.
- The approach is adequate to accomplish the proposed tasks.
- Emphasis is more based on analytical methods than on solving fuel cell problems. This is OK for *first year*.
- The approach is a solid research angle.
- The lack of statistics is most distressing.
- Need to work on industry-standard materials; LANL materials may provide different results than those of 3M, DuPont, and Gore.

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

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

- Excellent progress in comparing Pt crystallizable size for newly prepared MEAs and after cycling test conditions.
- Very good images.
- The ORNL staff has shown their expertise in this area. There are very few labs in the world that compare.
- Very significant results were obtained, showing the important changes of the MEA structure after cycling and log term fuel cell operation.
- The tools are good. There needs to be a more focused set of problems. For example, the study of the influence of one variable on the structure effects.
- Within the bounds of concerns about relevance, this work provides some truly interesting clues into the nature of catalyst layers.
- Too limited in number of results. Did they spend all of the resources for an entire year studying a single MEA, as implied by the talk?

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

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

- Broad interactions with other national labs and industry.
- Working with several fuel cell companies.
- ORNL and the developers work in close co-operation.
- Need to team with those who are working with commercial MEA designs. LANL materials may not be relevant.
- It seems as if some partnerships are forming with industry.
- Needs a method to communicate results more than once per year if National Lab is to fulfill their support mission.

Question 5: Approach to and relevance of proposed future research

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

- Good plan for non - and proprietary research. Continue good work. Add statistical data and imaging analysis software.
- No info provided except working with industry.
- Need to establish experiments to elucidate decay mechanism.
- ORNL has an excellent plan for FY 2005 and FY 2006.
- There are no specific goals regarding the search of other causes of fuel cell degradation.
- Planning is not well-communicated, other than continue on the path.
- How do the workers intend to get beyond observations to provide broader insight?

Strengths and weaknesses

Strengths
- Collaboration with industry.
- ORNL staff is very skilled in the microstructural analysis.
- The sample preparation technique and the catalyst mapping.
- Intriguing results make this effort worthwhile.

Weaknesses

- Few publications on results.
- Not all mechanisms of fuel cell operation are considered.
- The electrodes shown were very heterogeneous. Most likely they are not reproducible. It is hard to make much sense out of looking at a small section of a heterogeneous sample. There needs to be some statistical analyses techniques that can address this.
- A combination of "Oswald" ripening and particle migration is a well known problem with PEM fuel cells.
- Critical need to extend the work to include MEAs made by industry standard approaches. Also, the researchers need to provide context for their work to go beyond the purely descriptive approach.
- More than a few samples per year should be studied, even with somewhat limited resources.
- Not much bang for the buck.

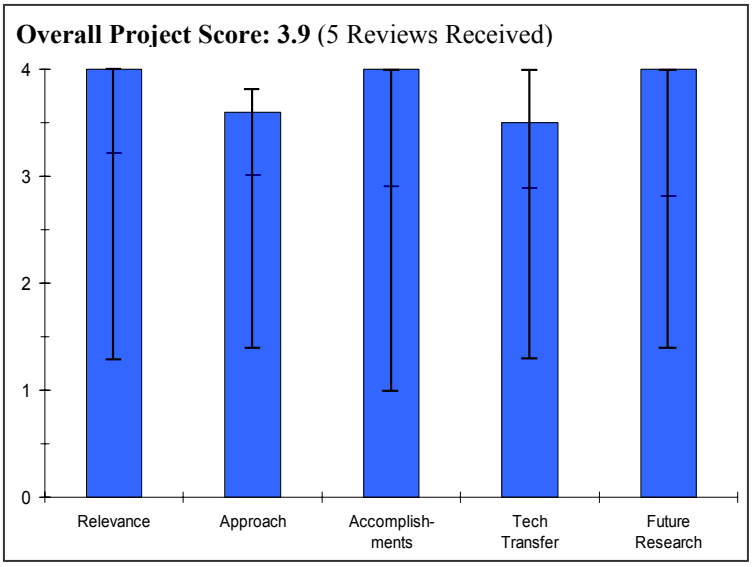
Specific recommendations and additions or deletions to the work scope

- Incorporate image analysis software for collecting statistical data on Pt particle sizes and distributions before and after aging.
- Use of other analytical techniques may allow characterization of chemical changes in the membrane.
- There needs to be more care in experimental planning. It is very important to study both new samples and then samples which have been VERY CAREFULLY tested.
- This work provides insight through pictures but there simply needs to be much more of it and an effective method provided for rapid communication to the field. National lab work is often good but poorly disseminated, and not in a timely way. The labs should be putting results out in near real time, regardless of their claims of IP. The results should be available to those who make the technology real. The lab insistence on keeping key results quiet slows progress of the entire field.

Project # FC-40: PEM Fuel Cell Durability
Borup, Rodney; Los Alamos National Laboratory

Brief Summary of Project

In this project, Los Alamos National Laboratory (LANL) will identify and quantify 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.



Question 1: Relevance to overall DOE objectives

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

- PEM durability fundamentals, understanding and improvement.
- Determining decay mechanisms.
- Very relevant work to understanding the issues related to automotive duty cycle operation.
- Durability, cost and performance are the main barriers for PEMFCs.
- Understanding of degradation mechanisms is key to improving durability.
- New tools for understanding degradation are right on target.

Question 2: Approach to performing the research and development

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

- Basic no-nonsense experimental approach combining results from different tests as a means to achieve understanding of impact on durability.
- Good view of the big picture.
- The approach is common with that being used by many other groups in the industry who are working on this problem. I hope that the approach used at the National Lab will emphasize mechanistic understanding and not just materials screening. Authors are also working on many different aspects of MEA durability; membrane degradation, catalyst durability (C corrosion and Pt dissolution) and GDL durability. Do they have resources to tackle all of these issues? I would rather they focus in one area and gain a deeper understanding.
- Very comprehensive *in* and *ex situ* testing.
- This work is a great beginning.
- LANL's small team needs to focus on getting understanding of specific parameters. Hopefully, next year some definite, well-planned and documented tests will be presented.

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

This project was rated **4.0** based on accomplishments.

- Many learnings identified for real world operating conditions: effect on catalyst growth, carbon corrosion, thinning etc.
- Hydrophobicity impact especially important for automotive and stationary systems.
- Investigated many parameters important to durability.

- A lot of good early results were shared. The PIs have been able to rapidly develop accelerated test methods for a number of key degradation mechanisms in duty cycle operation. Can they now use these tests to develop an understanding of the degradation mechanism and predict materials solutions and lifetimes?
- Important conclusions drawn from well-documented data.
- These results point out the complexity of fuel cell operation.
- There needs to be effort spent on non-uniform temperature during fuel cell operation.
- There also needs to be understanding of the uniformity of current density during fuel cell testing. Fuji Electric showed good data that PEM fuel cell operation resulted in "hot spots" which were transitory. LANL needs to understand the effects of this non-uniform current density. Measure temperature in the plane of the MEA.

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

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

- Good presentation list. Could have more interactions with OEMs.
- Interacting with many institutions.
- Numerous collaborations were reported. Can LANL approach the automotive OEMs to get better insight into the expected fuel cell duty cycle operational conditions? How will these conditions influence what they have already learned?
- Synergistic team.
- Need to work with suppliers who have SOA MEA products, and not really spend much time on poorly-documented samples.

Question 5: Approach to and relevance of proposed future research

This project was rated **4.0** for proposed future work.

- Needs more work on GDL and hydrophobicity.
- Experiments are planned to evaluate the key durability variables.
- Focus on the mechanistic understanding.
- Good plan.

Strengths and weaknesses

Strengths
- Solid laboratory approach with excellent analysis of results.
- High confidence in applicability and validity of results.
- Team expertise.
- Evaluating key parameters in systematic way.
- Solid understanding of fundamentals.
- Strong team.
- Excellent beginning to identify degradation mechanisms. Historically all experiments like this (how fuel cells change with time) show extensive catalyst migration even though the devices continue to operate, more or less, well. Good tools are in place.

Weaknesses
- Expansion to stationary applications and requirements (40,000 hours) would be a plus.
- Additional interaction with stack and system OEM's a benefit.
- Have yet to address ways to improve durability.
- This is a classical "many-variable" problem. LANL needs to focus on specific problems and work with very well-characterized electrodes. Perhaps they need to build (or buy) MEAs that have very well-characterized properties and study them as model systems. Many variables, all connected. Need to get through this with very carefully planned experiments.

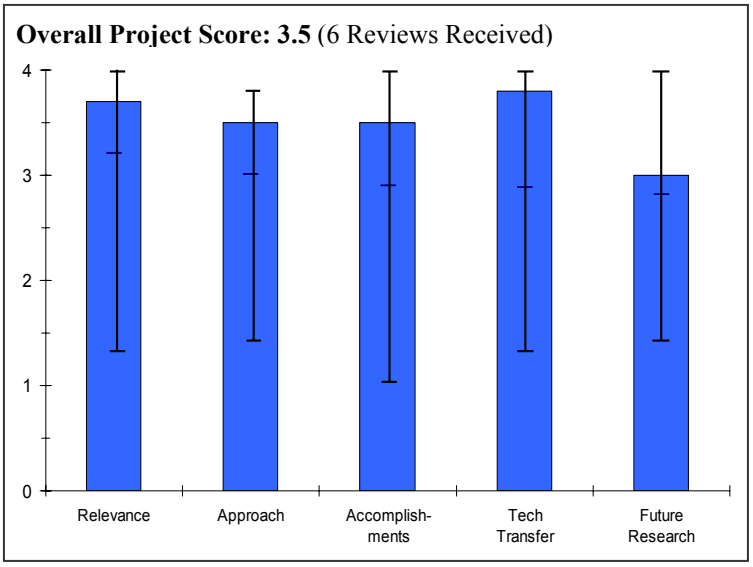
Specific recommendations and additions or deletions to the work scope

- Continue as is or expand if possible.
- Proposed future work looks good.
- Study temperature effect of electrode carbon corrosion at low relative humidity (especially at 120°C).
- Investigate Pt sintering as a function of Pt loading on carbon support.
- The DOE should seriously consider establishing a "fuel cell diagnostic center" built on the foundation presented here. This would be a user facility where developers would come (with their products) and learn both what they have made and how their products can be improved. This would be parallel to a similar center recently announced by the Japanese government to serve their fuel cell industry. This center should take a major responsibility in scripting test procedures.

Project # FC-41: Sub-Freezing Fuel Cell Effects
Bryan Pivovar; Los Alamos National Laboratory

Brief Summary of Project

This Los Alamos National Laboratory (LANL) project investigates start-up and survivability at sub-freezing temperatures of fuel cells, issues known to be a concern in fuel cells. This effort consists of hosting a DOE-sponsored workshop to address research needs for the freeze issues and experimental investigation of freeze effects on fuel cells and components. The result of this work will lead to a better understanding of the limitation of fuel cell systems due to sub-freezing temperatures.



Question 1: Relevance to overall DOE objectives

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

- Project initiated to address specific DOE need.
- This project is a great example of how the National Labs can support the industry.
- Critical to understand and quantify.
- A better, and more widespread, understanding of these effects would certainly be beneficial.
- The project is relevant to the DOE hydrogen and fuel cell program as it seeks to understand the fundamentals and quantifying effects of freezing on fuel cell durability and operability.
- Project generates useful fundamental knowledge and will lead to improved public-record perspective on freeze issues.

Question 2: Approach to performing the research and development

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

- Basic approaches are still under development but the work already done is very sound.
- Organization of workshop is applauded.
- Ambition to assess much wider range of materials is appropriate and necessary.
- Expert personnel and state-of-the-art facilities are well-aligned to the topic.
- Should add structural issues: impact of frozen water on membrane.
- Starting with a workshop to survey the community about the issue was an excellent idea and resulted in a good starting point.
- Good identification of the barriers/challenges.
- The approach utilized the workshop format to discuss the research needs for developing a fundamental understanding of the effects of freezing on fuel cell component performance and defining precompetitive research efforts.
- Approach will be clearer once the R&D roadmap is developed.
- The workshop appears to have functioned well.
- Survey of previous public-domain freeze work is thoughtful and appears to have been thorough.

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

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

- Combining workshop findings with internal work has made an excellent start. Need to do more thermal cycles (>100) to look for high cycle failure modes.
- It's early in the program; need to work with industry to ensure test matrix reflects the full spectrum of materials being used by developers.
- Standard test protocols for components would be very helpful.
- Initial characterization work should provide additional insight into path forward.
- Good start to working on fundamental understanding, which should continue to be the focus of this program.
- Organizing and managing the workshop was the most significant accomplishment. LANL quantified membrane electrode interfacial resistance over time. LANL did not see performance loss until temperatures decreased to -80°C whereas others have reported performance loss at temperatures below about -30°C.
- Some good initial fundamental studies of water in membranes as function of subfreezing temperature.
- Initial studies of level of sensitivity of single cells to freeze-thaw conditions help to put limited data from literature into context.
- Very good start in the field helps to establish the appropriate size of the further commitment of resources.

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

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

- Have incorporated knowledge of large number of investigators.
- Workshop demonstrates commitment to working with widest range of stakeholders, must maintain this.
- Too early.
- Workshop was an excellent means of initiating collaborations, as well some transfer of current knowledge/beliefs on this subject.
- Collaboration with other organizations in the workshop was good.
- Workshop seems to have drawn productively a bit of data out of the woodwork and brought together diverse opinions for consideration.

Question 5: Approach to and relevance of proposed future research

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

- One additional study that should be added is changes in low temp behavior of aged fuel cells.
- Creating an economical test matrix representative of materials and components spectrum and operating protocols is a tall order, but LANL is a top candidate for the job.
- Impact of GCL needs to be included. Neutron imaging would be extremely valuable.
- Good and clear presentation of future plans.
- Future plans were a little vague. They may become more focused when the R&D roadmap is developed.
- Future plans not given in great detail, as perhaps is appropriate with initial results not showing major evidence of problems as serious as are clearly present in other durability areas.

Strengths and weaknesses

Strengths
- Ability to draw on both internal and external knowledge.
- Technical team expertise and leadership.
- Consensus of issues through workshop.
- Ambitions to elucidate technical issues associated with freezing.
- LANL has the resources.
- Excellent choice for a DOE-sponsored research. These effects are of industry-wide interest.

- This presentation put freeze issues into a well-considered perspective. Carefully-considered project selection in concert with that perspective would help to assure a positive return on the investment of appropriately-scaled federal resources.

Weaknesses

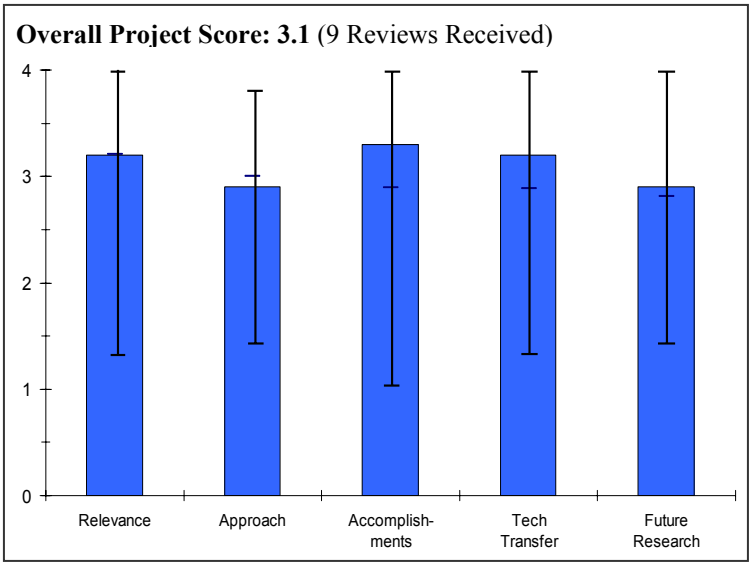
- None yet.
- None identified.
- Implicit assumption that a major federal investment in freeze research and development activities would necessarily be highly productive.
- Take care that one doesn't *a priori* assume that a phase change of water in a membrane will necessarily cause problems (other than the clear additional heat needed to melt the phase).
- Not all automotive OEMs actively contributed to the workshop. Further attempts at persuasion might have been helpful.

Specific recommendations and additions or deletions to the work scope

- Add neutron imaging.
- Continue to engage industry, especially those working on automotive applications, because this is a rapidly evolving area and perceived issues may change, as well as willingness to disclose concerns/results.
- Tough to stay away from engineering design considerations that are considered proprietary. Program needs to be integrated with the outcome from the workshop and remain focused on fundamentals.
- Be prepared to shift activity to other areas if fundamental studies here and in other projects, plus developer experience, suggest that freeze issues do not pose as pressing challenges as other durability or cost issues.
- Make sure that the project does not devolve into running the same tests on an infinite series of membrane materials and diffusion media. But be flexible and prepared to jump in if significant freeze issues do irrefutably arise.
- Continue to keep in mind the boundaries between pre-competitive research and competitive development. Put the federal dollars in the areas where they can best advance the technology.

Project # FC-42: Fiber Optic Temperature Sensors for PEM Fuel Cells
McIntyre, Tim; Oak Ridge National Laboratory

Brief Summary of Project



The objectives of this Oak Ridge National Laboratory (ORNL) project include the development of small and rugged fiber optic temperature and humidity sensors for monitoring critical fuel cell operational parameters, development of a measurement platform that enables a multi-point measurement capability for mapping conditions throughout the fuel cell, and utilization of these diagnostic tools, in concert with fuel cell developers, to validate design models and operational strategies to maximize performance. Furthermore, coupling of these measurements to miniature, non-optical methods for analyzing gas composition within the membrane region will provide the fuel cell community with a performance analysis test bed to support future design and development efforts.

Question 1: Relevance to overall DOE objectives

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

- Sensors are a critical need in order for fuel cells to meet DOE performance targets.
- Project is relevant to the DOE Hydrogen Program once its objective is to provide measurement and diagnostic tools to fuel cell developers for system performance optimization and model validation.
- This effort has high potential for significant understanding of fuel cell operation inside stack/cells.
- Real-time intra-fuel cell transient temp diagnostics.
- Diagnostic sensor development can be used for insight into internal fuel cell processes which can address performance and durability.
- Diagnostic methods and instrumentation are critical to the development of Fuel Cell basic science development.
- Temperature and humidity sensors are useful; it is questionable if a temperature sensor alone is useful.
- Diagnostic instrumentation and methods must be as nonobtrusive to the existing fuel cell as possible in this analytical development.
- Sensors are not critical to the realization of President's Hydrogen Fuel Initiative, but will provide useful information during the development and use of fuel cells.
- The program develops analytical tools to better understand the characteristics of fuel cells.

Question 2: Approach to performing the research and development

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

- Five-sensor platform developed and in testing in-house and at industrial partner with intra-cell temperature, pressure and species concentration measurements.
- Project embodies good mix of sensor development, experiments, and analysis.
- Developed luminescent sensor.
- Develop microcapillary for species measurements.
- Investigator's emphasis should be more on the diagnostic development than on application to the in-house developed stack.
- Requires more validation that readings are accurate (especially RH).

- Need to consider how this method can be applied to thin bipolar-plate-type fuel cells. Carbon-based plates are not for automotive applications for much longer.
- Project objective not entirely in agreement with outlined technical barriers and targets. Project objective is directed toward fuel cell developers, while the target includes not only lab development but operating fuel cells in automotive applications.
- If lab sensor is the target, then approach is logical.
- Very creative approach.
- The temperature-measuring device will open a greater understanding of fuel cells.
- Not completely sure chemical probe will give real-time data.

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

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

- Excellent progress with integrated sensor platform under test at two locations. Meaningful data being collected and analyzed.
- Results to date provide unique insights into the dynamics of an operating fuel cell.
- Demonstrated free space measurement.
- See transient thermal operations.
- Can ID water activity and some species measurements.
- Temperature dynamics quite slow - 1 to 10 minutes.
- Interior temperatures can be higher or lower than manifold temperature.
- Spatial distribution valuable - but not enlightening.
- Some localized humidity during cooling - good correlation between RH and cooling.
- Could identify local limiting processes.
- Sensors appear to be sufficiently developed to be useful tools.
- Developing a good understanding of measurement results and confidence. This is good.
- Maintain focus on further developing the tool and refraining from investing too much time on testing for characterization of fuel cells under various conditions.
- Good progress.
- Response time for sensors is good.
- Clear demonstration of temperature and species determination through use of the sensors.
- No comparison with currently available technology.
- Project scheduled to conclude in 2005. Not apparent that targets will be achieved.
- The results are outstanding and very creative science.

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

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

- Excellent collection of partners and potential users including major auto manufacturers.
- This project has an excellent fuel cell OEM onboard. Efforts to broaden OEM base will help to spread knowledge and ensure that results are not fuel cell design specific.
- Improved number of research partners - it could always be better, but attracting GM is important link to transportation applications.
- Work with outside experts to develop test plans and analysis of results. Suggest LBNL and LANL programs, amongst others, which are focused on durability and performance and that have complementary spatially distributed diagnostics.
- Large number of collaborative partners.
- Partner contributions unclear.
- Work has been disseminated through presentations/publications within the last year.
- Results are well-published.

Question 5: Approach to and relevance of proposed future research

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

- Good plans for future work.
- The PI identified an excellent set of future development ideas. These need to be better focused in the next year to prioritize future experiments.
- Move to measuring anode and cathode separately.
- Could identify rate-limiting processes, e.g. blockage of active sites.
- May have insight into next level performance- but did not clearly identify what is done next nor how they were to interact with their new partner - GM.
- Concentrate more on diagnostics: including reduced size, perhaps RH transient response (time constant was not stated).
- Investigate potential intrusive impact of the diagnostics.
- This topic should be eliminated from your future work objectives.
- More controlled operation variations (drive cycle) to identify origins of local efficiency limitations.
- Reasonably focused work.
- Although cost may not be a significant barrier, it is in the target description and should be stated.
- Discuss methods to demonstrate accuracy of measurements.
- Agree with approach and look forward to next year's results.
- Application to stacks is great idea.

Strengths and weaknesses

Strengths
- Strong and supportive team.
- Relevance and approach.
- The project has provided unique and valuable insights into the interior dynamics of an operating fuel cell stack. Project facilitates analysis of the chemistry and physics of the electrochemical reactions within an operating fuel cell.
- Has the potential for showing how to overcome issues with MEAs and show the path for improving fundamental fuel cell efficiency.
- A nice diagnostic tool has been developed.
- Developing good analytical instrumentation which will be useful for fuel cell development.
- Learning about transient, localized conditions during stack operation is necessary to optimize fluid-dynamic designs. This is a good first step.
- Strong team.
- Technology very appropriate for application.

Weaknesses
- Not identified.
- It is not clear that the location of sensors within the flow field impacts the results.
- Not clear what the next set of experiments are or will show.
- No information on new directions or how they will improve or expand their fuel cell measurements and capabilities.
- Long species sampling times limit their usefulness but can still show trends.
- Did not state the improvement of this approach over conventional approaches used to measure temperature or diagnose these issues.
- This diagnostic method is not easy to implement into fuel cell system. A little more time should be spent on understanding how these sensors can be implemented in a less intrusive manner.
- Probe is sufficiently large to perturb flow and give data that may not be representative.

- This activity needs a CFD modeling project that operates cooperatively. Most likely, the calculations will be essential as flow field design tools. The thermal physics here are pretty simple, although the fact that steam can be "supersaturated" in the gas phase (maybe a fog) will complicate the issue. Los Alamos developed a series of "down the channel" models that could be useful to replicate these experiments.
- No discussion of sensor accuracy.
- May not meet targets.

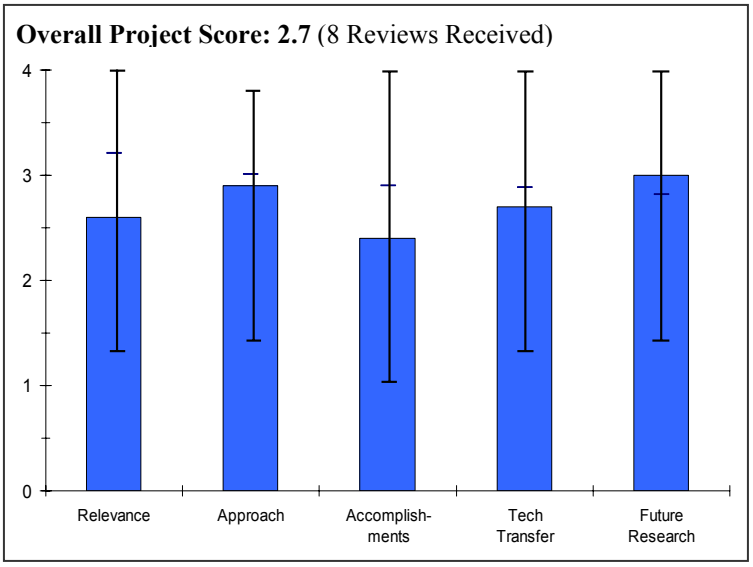
Specific recommendations and additions or deletions to the work scope

- Increase sensor sensitivity and data collection rate to resolve detailed fuel cell stack kinetics.
- Project should be expanded to place sensors in more locations within individual cells to ensure flow field geometry is not impacting results. Fuel cell stacks from additional fuel cell OEMs should be incorporated into the program.
- Need to correlate these measurements with known issues from fuel cell development studies.
- Show how this data has led to changes in fuel cell design to overcome issues identified with this technique.
- Too much emphasis placed on its application to a 2-cell in-house stack. Work with other DOE programs to apply to problems of interest. Again, suggest LANL, LBNL.
- More validation needed, especially RH sensor.
- Investigate potential intrusiveness of the present diagnostics. Are the probes small enough to not have significant impact on cell operation?
- I recommend that this project should not be a part of your program objectives. This work should be left to the fuel cell supplier to deal with. This work should be eliminated from your future research plans.
- Need more controlled operation variations (drive cycle) to identify origins of local efficiency limitations.
- There needs to be integration with this work and the studies done at NIST (neutron scattering).
- Prof. Trung Nguyen/KU knows about modeling this system and he would be a useful collaborator.
- Prioritize proposed work. May not be able to accomplish all during remaining project time.
- Continue interactions with automotive fuel cell companies.
- Continue activity.

Project # FC-43: Research and Development for Off-road Fuel Cell Applications
Simpkins, Erik; IdaTech

Brief Summary of Project

This IdaTech project, supported by Donaldson, Toro, and UC Davis, will characterize the effects of off road conditions (air quality, shock, and vibration) on fuel cell and fuel processor systems. Analysis of off road (greens keeping and similar environments) air quality and operating conditions will be accomplished, and load profiles and mitigation strategies will be designed and tested for impact on the power generation system.



Question 1: Relevance to overall DOE objectives

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

- This project addresses issues with field conditions that a fuel cell system will have to operate under.
- Project is narrowly focused on and off road application with limited relevance to DOE automotive fuel cell objectives.
- Project does address the need to validate the potential for application of fuel cell to off-road vehicles, albeit to a limited size, niche market.
- Off-road vehicles may have an easier market entry on some points (power requirements, price point), but fueling infrastructure may well limit.
- Target application will not have a significant impact on reducing oil imports.
- Off-road vehicles do have a significant effect on air emissions, and this is relevant to the program.
- Operating conditions are more demanding than on-road light duty vehicles, and this presents a useful challenge to system designers and will advance the technology.
- There is some connection with the hydrogen vision; off-road applications importantly also speak to clean air targets.
- The program address vehicles which are not necessarily transportation structured.
- The program does offer opportunities to make improvements to fuel cell systems that could translate to transportation applications.

Question 2: Approach to performing the research and development

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

- Logical approach to first understand application requirements for both power and environment.
- Would consider preparation of system and performance models in parallel with collection of the data in the early tasks.
- The plan to generate operational data from and off road fuel cell vehicle is sound. One concern is that the application may not be representative of a variety of off road applications and vehicles.
- Sound systems engineering principles have been applied to fully understand the requirements of the application i.e. power and torque profiles.
- Solid systems approach of gaining relevant data on application load profiles, power requirements and impulse and vibration.
- Fuel cell and power system design not being undertaken this year – DOE should follow closely in out years.

- The project seeks to deploy a hydrogen-fuel cell to power a utility electric vehicle. This application is a battery substitution.
- The approach offers good opportunity to identify methods of cleaning the air into the air side of the fuel cell.
- The approach may provide important information regarding vibration protection for fuel cells.
- The approach has activities that are not generally beneficial to transportation fuel cells for the hydrogen economy.

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

This project was rated **2.4** based on accomplishments.

- Project appears to be moving along as planned.
- Conclusion to use fuel cell hybrid is reasonable.
- Results on air quality for the applications considered not presented.
- More specific data (from existing information) should be identified to design single cell testings.
- This project is in the early stages of development. Results to date appear commensurate with resources expended. The progress will accelerate once vehicle development and testing are further along.
- Program has only been funded a few months, so progress is limited to data collection on operating envelope and requirements.
- The accomplishments to date are modest. The tasks are certainly necessary, but not very challenging.
- The program is in a start up phase having only been signed in the last 5 months. For that short period, the program has collected basic data for analysis.

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

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

- Appropriate team selected to address modeling, air filtration, and development of application requirements, fuel cell development, and demonstration of system in the application.
- Does team have the appropriate expertise to effectively develop controls for the hybrid system controls and power electronics?
- A good team has been established to complete identified program goals.
- A modest team of partners but adequate for niche applications.
- Partnership with Toro is good.
- Donaldson is involved in cathode-air filtering in this and other projects.
- UC Davis involvement will help ensure learning from a specific platform is transferable.
- Collaboration is between an OEM (Toro) and a fuel cell vendor.
- The basics for collaborations with other industries and with a university are established. It is too early in the program to expect substantial technology transfers to have occurred.

Question 5: Approach to and relevance of proposed future research

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

- Bulk of the project lies ahead in terms of hardware development and field demonstration in various environments (7% of budget expended).
- The identified program objectives and tasks are responsive to the goals of this program. Future work beyond the designed program must omit future results.
- Next year should bring definition of major system specifications.
- Planning seems appropriate for project scope.
- The follow on work involves installation of a fuel cell system and a fuel system into the commercial platform. Those tasks seem straightforward, perhaps more “development” than ‘research”.
- Well organized and scheduled future work.

Strengths and weaknesses

Strengths

- This program will gather detailed information of fuel cell operation in an actual vehicle.
- These team members have a good understanding of the off-road application market and vehicle hybrid power train architecture.
- Working with a proven electric (battery) platform is an excellent starting point and Toro has a good technical record in this area. The team seems on track and doing the correct things.

Weaknesses

- Presentation did not clearly describe the system configuration intended for use in the demonstration.
- This program addresses a very specific application and vehicle. H is not apparent that data developed will be widely applicable.
- None evident yet.
- Weakness is in project relevance.
- The vibration study probably is mainly about shock performance of the fuel cell system. Those results will be very system dependent and therefore that system needs to be well characterized (mass of components, mounting details, mechanical strength, etc.) before the vibration study makes sense. (Obviously Toro designed the battery hardware with full understanding of the shock requirements for the vehicle.). This is also true about the "load profile". Seems like those tasks would be required to have already been done (by Toro), and it is not necessary to repeat them, seems to this reviewer.

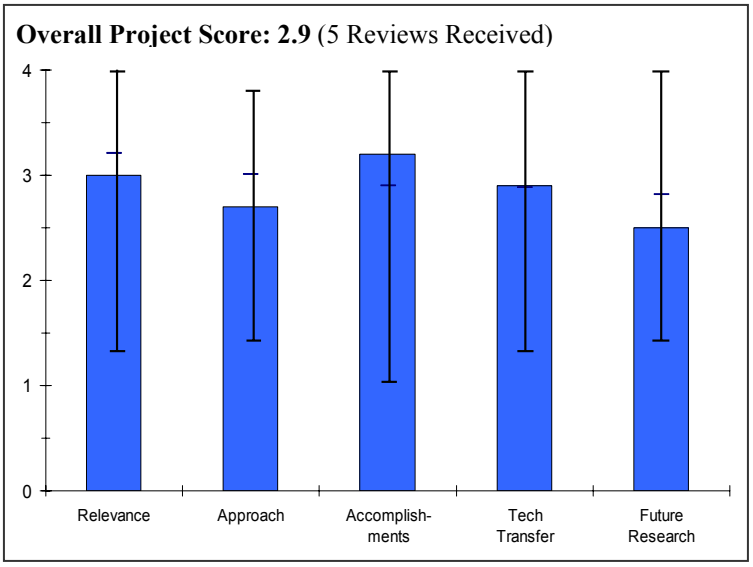
Specific recommendations and additions or deletions to the work scope

- Need to ensure that environments monitored during field tests and results correlated with performance of the filters and performance of the stack versus time.
- No changes needed yet. Employ computer modeling to understand trade-off between fuel cell and battery sizing.
- Project and DOE should strive to make learning as broadly applicable as possible, since target application is weak.
- Program should emphasize air purity and clean up information that can be transferred to fuel cell transportation applications.

Project # FC-44: 50 kW Absorption Enhanced Natural Gas Reformer
Stevens, Jim; ChevronTexaco Technology Ventures

Brief Summary of Project

In this project, ChevronTexaco Technology Ventures will assist the DOE in developing hydrogen based distributed generation technology with significant cost advantages in reduced reformer and PEMFC system operating costs. These cost advantages will be achieved through improved fuel efficiency, reduced capital costs, reduced system complexity, and reduced reformer + fuel cell system costs. The first year focused on catalyst and sorbent testing with small scale reactors. The second year will be devoted to scaling up the reactors. The final product will be a natural gas reformer sized to provide hydrogen to a 50kW PEM fuel cell.



Question 1: Relevance to overall DOE objectives

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

- This project has the objective of producing hydrogen from natural gas in a simple, efficient manner. Projected cost savings and efficiency improvements are quite significant.
- Hydrogen production from natural gas is a key technology for distributed fuel cell power generation.
- The technology being developed has potential to greatly simplify natural gas fuel processors and reduce cost of such systems.
- Cost of hydrogen will be tied directly to cost of natural gas.
- The idea is very innovative, but the benefits for hydrogen production (while claimed) were not well-articulated.
- Purported benefits were not quantified in the presentation.

Question 2: Approach to performing the research and development

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

- The system design is unclear and it appears they are still exploring basic concepts such as the clean-up steps. Before scale-up is finished, it seems that the system design should be finished, as these systems invariably have basic thermal interactions.
- To have complete hydrogen cost, efficiency/thermal integration needs to be understood.
- Dynamic process modeling is important for predicting benefits, but no evidence of such analysis was shown.
- Modeling and control approaches were not explained.
- Focus on absorbent longevity is appropriate and should be precursor to go/no-go decision.
- By simultaneous removal of CO_2 (and sulfur) during the reforming process, the equilibrium is shifted to producing more hydrogen, even at the higher reforming temperatures than would be required for a separate water-gas shift reactor to reach the same low level of CO.
- This approach, however, will not produce even 98% pure hydrogen (initial target), much lower than the 99.99% hydrogen (revised ?) target.
- An excellent approach is being pursued on this project, with catalyst materials being qualified in relatively large reactors (1-kW) so that scale-up to 50-kW reactor size is less risky.
- There is a focus on long-term and cycling tests so that catalyst durability is being addressed earlier rather than later in the project.

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

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

- There is good improvement on physical stability of reforming extradites; however, overall durability still appears insufficient.
- The outcome of the cost-study (which was reportedly done) should have been presented in order to understand the viability of the project.
- Absence of modeling is a weakness.
- It would be helpful to show CO_2 absorption data to compel the concept.
- Show more process details, e.g. heat and mass balances and their temporal character in cycling.
- While adding inerts to sorbent is important for stabilization, inerts also add thermal mass and cause efficiency loss. There was no description of impacts.
- No work on methanation step was described.
- Modeling effort is very beneficial. More information on cost breakdown should have been provided.
- The primary accomplishment has been to test for and demonstrate cycle life for the sorbent. It has also been shown that the "kinetic" capacity is significantly lower than the "thermodynamic" capacity, perhaps by a factor of almost 2.
- The other accomplishment has been to build the two sub-scale (1 kW) reactors to demonstrate the concept.
- They have also completed a "rough" cost study, but the results were not presented or discussed.
- An impressive amount of data has been collected during the past year of the project, and advances in sorbent durability were demonstrated.
- Much of the inherent risk associated with sorbent durability (e.g., related to sintering and/or attrition resistance) appears to have been mitigated by the results obtained.

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

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

- The partners in this project all appear to be fully engaged in the project.
- Partner seems appropriate for absorbent.
- One reviewer commented that it is unclear how any technology is being transferred (2 presentations), but looks like limited interaction with Cabot and Engelhard as their materials are incorporated in the system.
- This point was not discussed explicitly, but it was indicated that Cabot Superior Micro Powders is marketing the developed materials. It is not clear if the prime contractor, ChevronTexaco, intends to make this technology, once proven, available to others interested in this approach to hydrogen production.
- Chevron is certainly qualified to scale-up and commercialize the technology when this project is completed.

Question 5: Approach to and relevance of proposed future research

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

One reviewer commented that the future plans are fine, but other reviewers noted the following:

- Closer interaction with Engelhard and more reforming catalyst development is still warranted.
- Considering the durability doesn't appear to be sufficient for a production/commercialized system, scale-up to 50kW is probably not the best use of their time.
- It is not clear if the sorbent cycling stability issue has been solved.
- Modeling-based process and control optimization needs attention.
- The "future plans" require more detail of tasks and milestones as well as expected lessons learned.
- The future plans were given in generic terms. Overall, the activities listed are OK, but specific activities were not identified.
- It was hinted at during the talk that some further development of the commercial reforming/shift catalyst may be needed, but no future activity related to that was indicated.

Strengths and weaknesses

Strengths

- Innovative concept shows well insofar as exploratory R&D.
- Program benefits from strong industry experience.
- The project team includes commercial partners who can identify the likely commercialization issues and address them throughout the course of the project.
- Strong team, with good approach and plan, has excellent results to date.

Weaknesses

- After ~ 45 cycles, it appears the AER has about 3.5% CH_4, 2% CO + 1 % CO_2 in the effluent. Methanation of the CO and CO_2 will consume another ($CO + 3\ H_2 \rightarrow CH_4 + H_2O$) 6% hydrogen and ($CO_2 + 4\ H_2 \rightarrow CH_4 + 2\ H_2O$) 4 % hydrogen – to sum to 13.5% CH_4 slip out of the fuel processor system. This leaves a tremendously high amount of CH_4 unconverted in the system. No information was provided to predict the overall efficiency of the system and to determine whether the unconverted CH_4 heat value can be used.
- The decrease in activity of AER and CO_2 sorbents do not appear to have sufficient activity. They seem to have high decreases in activity or capacity during < 100 cycles.
- Relevance for fuel cell systems was not quantified or adequately described.
- Maybe this project would be better suited for joint funding with Industrial Applications sector of federal funding?
- Detailed cost analysis is required. The presentation should include results of 1 kW reactor testing.
- The 1-kW reactors are small enough that they cannot really operate in adiabatic mode. Extrapolating the data obtained from these reactors to the 50-kW reactor may not be as straight-forward as the team proposes.
- It is not likely that 98% hydrogen can be obtained from this process directly. Further purification to 99.99% (or whatever the purity target may come to be defined as) will require additional steps and processes.

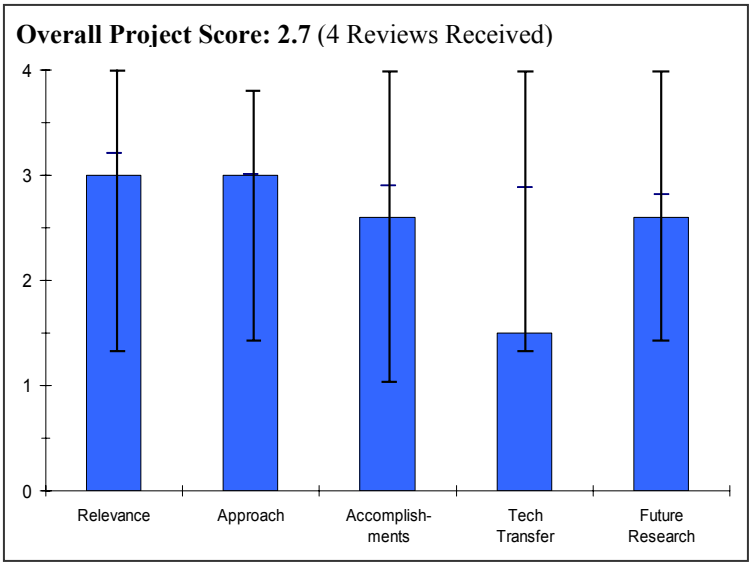
Specific recommendations and additions or deletions to the work scope

- No data was shown on sulfur tolerance of the AER materials. The reviewer assumed the natural gas odorants are removed before the AER step but did not see an overall system design.
- Project should continue to improve materials, increase materials durability, and perform a good cost study. More long-term improvement will be accomplished by that than the planned scale-up to 50 kW.
- As stated in presentation, improving the purification of the hydrogen stream well above 98% is needed.
- Cycle times for reforming and regeneration need to be explicitly defined and discussed. The influence of cycle time on reactor design (and size) and system operation does not appear to be given the significance it deserves.
- Sulfur issues need to be addressed explicitly.

Project # FC-45: Cost-effective High-efficiency Advanced Reforming Module
Holmes, Tom; Nuvera

Brief Summary of Project

Nuvera will develop an advanced, flexible fuel processing module for use in stationary applications. A new generation of low pressure steam reformer will be produced to address the widest range of specifications with the lowest risk. The reformer will be designed for high efficiency and long life, low capital cost in accord with DFMA principles, and will respect emissions standards and afford scalability. The intent is to develop a low-cost fuel processor module (FPM) that is scaleable to produce from 500 to 2,000 scfh of hydrogen. A 1,000 scfh FPM will be built and evaluated at Argonne National Laboratory. Lifetime assessment will be achieved through accelerated aging. Cost structure will be confirmed via third party audit.



Question 1: Relevance to overall DOE objectives

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

- Cost of hydrogen will be tied directly to cost of natural gas.
- The project is very relevant to DOE goals.
- Hydrogen production from natural gas and LPG is a key technology for distributed fuel cell power generation.
- Based on information presented, it was difficult to assess whether the technology being developed will be sufficiently cost-effective for distributed generation applications.

Question 2: Approach to performing the research and development

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

- Nuvera is doing a commendable job of preliminary design/thermal integration before trying to scale-up system–including cost trade-offs between capital and durability.
- Concept development is not directed towards DOE targets. There is no indication of lessons learned from first program.
- The approach is balanced with modeling efforts to aid in the design of fuel processor and in cost investigation.
- A formal decision making methodology was applied to the determination of the design approach being pursued.
- Nuvera used CFD modeling effectively to solve an issue related to combustion uniformity.

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

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

- Nuvera built two systems with a down-select which is a significant accomplishment. However with limited (or no) data, it is difficult to understand the progress.
- There was no discussion of expected hydrogen purity.
- There was no detailed cost analysis.
- It was difficult to evaluate the accomplishments due to the proprietary nature of the work.

- It is difficult to assess the technical accomplishments during the past year, due to the lack of performance and cost data presented with respect to the new design.

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

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

- Presentation did not indicate any development partners.
- No outside interaction exists. Even though this project is proprietary, collaborations with universities or national labs on fundamentals could aid the development effort and allow advancement of the state of knowledge in this area.
- There is no evidence of external collaboration on this project, although Nuvera appears to be fully capable of conducting this project.
- No publications, presentations, or interactions are noticeable.
- Nuvera is certainly qualified to establish the relationships required to scale-up and commercialize the technology when this project is successfully completed.

Question 5: Approach to and relevance of proposed future research

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

- Evaluation by ANL will be final proof of concept.
- Nuvera should have provided details of prototype test results versus DOE targets.
- Future plans are clear and follow the past progress.
- Detailed information is absent in view of the proprietary effort of this work.
- The plan appears adequate for meeting the project goals, although the level of detail presented relative to their current status makes it difficult to assess.

Strengths and weaknesses

Strengths
- Preliminary design and evaluation is a strength.
- Nuvera has excellent, integrated work.
- Nuvera has a strong fuel processing technology platform on which to build a fuel processing system.

Weaknesses
- Nuvera based down-selection of technology on overall score, however the system selected had the lower durability score. Durability should be improved – it is unclear if a concept exists to meet durability requirements.
- No presentations or publications were indicated.
- Emissions require further definition (not just ppm), for example (g of CO)/kg hydrogen produced should be included.
- Decision matrices include numerous criteria that do not relate to the ultimate DOE goals, such as schedule and patents.
- Nuvera lacks collaboration with other partners that may give some perspective on "out of the box" thinking.
- Although the efficiency of the processor is adequate with respect to cost, burner emissions are little bit on the high side. More focus should be considered to reduce emissions, i.e., are there other technologies that could be investigated, for instance catalytic combustion?
- Not enough information was provided to determine whether any weakness exist on this project.

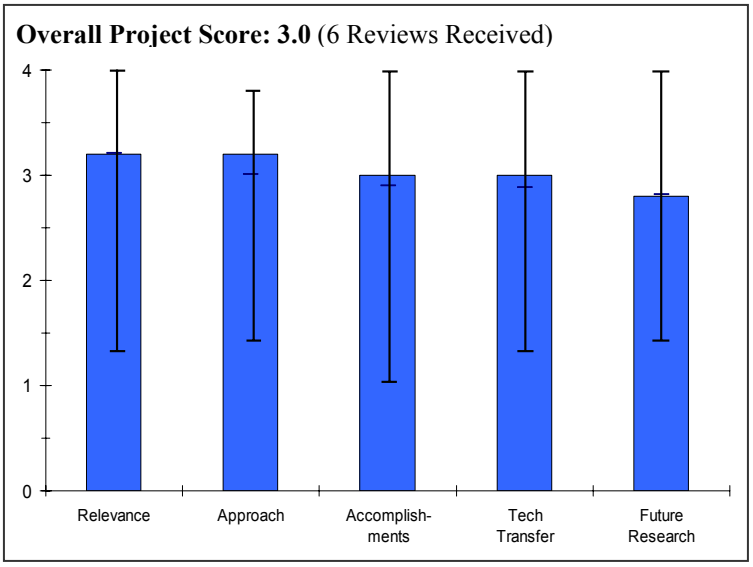
Specific recommendations and additions or deletions to the work scope

- Nuvera should participate more in publishing and presenting their work.
- Perhaps the presentation of data from system cost and efficiency models (which are assumed to exist) will provide a compromise with respect to intellectual property issues and allow for the possibility of a fair and accurate peer review.

Project # FC-46: 150 kW PEM Fuel Cell Power Plant Verification
Clark, Tom; United Technologies Corporation

Brief Summary of Project

The United Technologies Corporation (UTC) Fuel Cells and UTC Power's Stationary Power Plant project will resolve critical cell component, cell stack, and power plant reliability issues. Testing will be conducted in 20-cell stacks and 150 kW power plants.



Question 1: Relevance to overall DOE objectives

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

- This stationary program fills a need in overall hydrogen fuel cell transition for large-scale distributed power.
- Development of an industrial, stationary reformer-based power plant has limited relevance to DOE Fuel Cell automotive program. The best overlap between stationary and automotive is in the areas of endurance and cost reduction.
- This project is clearly important for the viability of stationary systems. Why were PEMs and low-grade heat chosen versus SOFCs? Why is the project only for 150 kW?
- This project is extremely pertinent to DOE fuel cell commercialization objectives.
- A stationary fuel cell system will be desirable when hydrogen replaces fossil fuels.
- This reviewer's impression of PEM fuel cell status is that there is still significant fuel cell development work required to achieve the stationary fuel cell system requirements. Integration of a 150 kW system is an expensive integration project for an immature technology.

Question 2: Approach to performing the research and development

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

- Overall plan is good but UTC needs to outline more detail and "b" & "c" level plans to address such issues as stack life. The current plan is not explicit enough.
- This program is a very heavily hardware-oriented. The upfront analysis of low cost components (e.g., seals) and membrane durability is valuable to all fuel cell applications.
- There is good balance between component issues versus systems challenges and excellent inclusion of market challenges into the overall program.
- This project is well-targeted to overcoming two crucial barriers to commercialization, namely cost and durability, in a timely manner.
- Work is focused on durability of 20 cell stacks, verification of design and reliability, waste heat utilization, seal material stability, and fuel processing.
- Seals with chemical and mechanical stability are needed and will be developed.

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

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

- "Ganging" stacks, thermal management, and balance of plant issues are being addressed. UTC will have to down-select designs soon. Initial stack progress is positive but limited.

- Program appears on track for success. It is still early in the program; initial membrane durability and low-cost seal analysis have provided significant insights.
- Membrane endurance is impressive. Seal issues are well-defined. Durability test procedures were not well-explained, well-based, nor grounded on past experiences.
- Non-silicone materials for seals were tested.
- Accelerated testing shows great than 20,000 hour lifetime – demonstrated for 11,500 on 20 cell stack with unreinforced membrane.
- Excellent progress has been made at this early stage of the project – particularly on seal materials.
- Work focused on membrane lifetimes through accelerated testing, use of non-silicone materials, 40,000 hour seals, implementation of a steam reformation system, and 150 kW system design.
- The accelerated testing appears primarily addressed toward mechanical lifetime issues. Although an element of the chemical and environmental effects is inherently included in mechanical issues, the chemical and environmental effects did not appear to be addressed in detail. It is unclear that the accelerated test will provide "true" results with this approach.
- The use of a steam reformation system is generally more efficient than other fuel processing methods and will in general provide a more pure fuel. The downside is that a steam reformation system is far more complex and much larger in size than say, for example, a partial oxidation approach. It must be determined whether a steam reformation system can provide (under the best assumptions) a system that will meet the stated $1,800/kW system price before expending substantial effort. If not, alternatives must be found.

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

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

- UTRC is the only partner currently. EPRI is planned to participate.
- UTC has incorporated key players in the stationary power market-place. It is not clear that automotive OEM participation would be meaningful.
- Comprehensive choice of stakeholders and other players in the value-chain. However, UTC needs to identify the component partners better.
- At this stage, UTC has limited its partnering to user utilities, but it is understandable with 50% cost share.
- UTC has a good list of potential demonstration partners, but they are not yet engaged. Given that the project has many detailed technical issues to resolve, more collaboration with R&D organizations may be suggested.

Question 5: Approach to and relevance of proposed future research

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

- Focus additional efforts on balance of plant. Run tests using reformate. Perform cycling and transient tests. Perhaps an accelerated test correlation study should be performed.
- This program is a highly integrated; defined future tasks must be completed to achieve overall program objectives.
- The project is somewhat pedestrian – need to seriously compare SOFCs and PEMs in stationary applications.
- The next 4 years are on target to be very profitable in terms of R&D success – especially with regard to durability targets (40,000 hours).
- Test the 150kW unit with steam reformation and pressure swing absorption.
- Continue accelerated testing on membranes.
- Advanced seals
- The future work scope is reasonable, but must be tempered with the reviewer's view that significant detailed technical issues, that are not included in the project scope, require resolution.
- The post 2005 work plan is structured adequately to meet overall program activities.

Strengths and weaknesses

Strengths

- Organization has extensive "system" experience. UTC can adapt PEM automotive experiences to this effort.
- The key benefits of this project to DOE rests mainly in cell stack assembly (CSA) development and testing. Improvement in stack durability and low cost components are broadly applicable to all fuel cell development efforts.
- The project has very solid incorporation of past experiences. The broad (large), well-funded, comprehensive program balances component-scale evaluations up to systems integration challenges. The fuel processing systems and thermal integration challenges are well-analyzed.
- The PI's extensive experience and knowledge in stationary fuel cell systems is a great asset. PC25 experience with fuel processing is also of high value.
- Improved compression set predicts 43,000 hours of lifetime.

Weaknesses

- Long life stationary requirements are not the same as auto PEM requirements – these requirements might require a totally different mindset. What is the impact of "ganging" stacks? UTC should focus more on system intersection issues.
- It is not clear that efforts on catalytic systems reforming, pressure swing absorption, and thermal integration have any applicability to automotive applications. The stationary power plant market assessment does not support any of the DOE program objectives.
- Need to be more open (revealing) about component improvements (which could be of benefit to other small players). Market assessments left out U.S. Department of Defense (DOD) and Japan. Why only natural gas as a fuel?
- Efficiency will always be low on natural gas – 30-37% using lower heating value. Must use hydrogen.
- Accelerated testing protocols are always suspect.

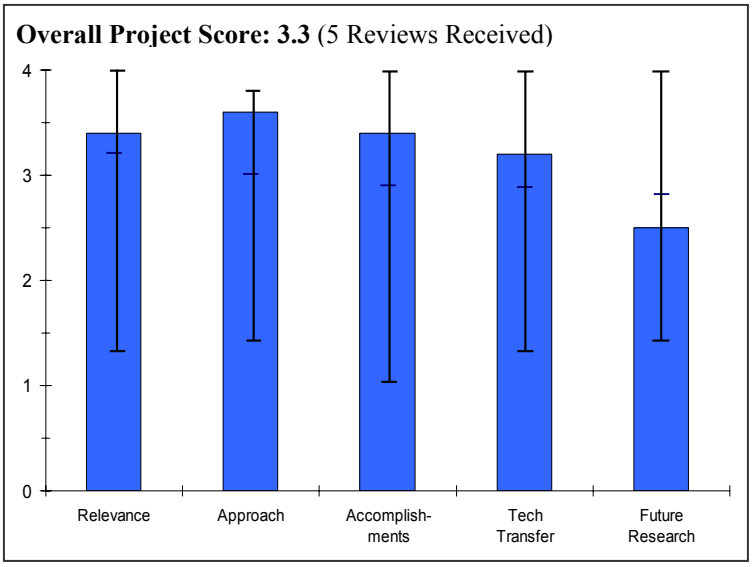
Specific recommendations and additions or deletions to the work scope

- Tests should be performed with reformate. Cost analysis should include cost of compression of fuel.
- Cold weather testing should be performed.
- Desulfurization sub-system.
- Add U.S. DOD and Japan market analysis. Need complimentary study to compare and contrast PEMFC and SOFC systems.
- Focus on smaller proof of concept of sub-systems and integrated sub-system tests before expending substantial resources on a 150kW demonstration.
- New Reformer (non-POx) may be improvement but CSR/PSA will still not produce >74% efficiency.
- One reviewer noted his belief that no changes are necessary.

Project # FC-47: Back-up/Peak-Shaving Fuel Cells
Vogel, John; 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 will develop, build and test three identical fuel cell back-up systems and field test them at three sites, including an industry host site to identify technical barriers. Other objectives are to develop a cost-reduced polymer electrolyte membrane fuel cell stack tailored for hydrogen fuel; to develop a modular, scalable power conditioning system tailored to market requirements; to design scaled-down, cost-reduced balance of plant; and to certify design to Network Equipment Building Standards and Underwriters Laboratories.



Question 1: Relevance to overall DOE objectives

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

- This project addresses the need for APU systems and UPS systems.
- These applications are key in launching the hydrogen economy – excellent application.
- This project is very relevant to determining whether fuel cell based UPS/peaking shaving power sources are technically and commercially viable.
- Back up power systems do not appear directly significant to U.S. national goals but will provide experience.

Question 2: Approach to performing the research and development

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

- The approach involves demonstrating many of the key issues in fuel cell performance and cost goals.
- Good combination of development, testing, validation.
- The broad, system-wide approach is effectively addressing all major cost/reliability challenges to commercialization.
- The back-up system will use H_2 (no reformation). Use of propane with a small hydrogen start reserve would seem more practical for the customer.
- Use of stored hydrogen will impact operational cost but will enable a less expensive price. The developer stated that stack cost is most important.
- The PEM catalyst dissolution issue at open circuit or near open circuit is well known and may impact the practicality of this concept.

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

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

- Plug Power is making steady progress toward meeting the project's goal and objectives.

- Plug Power is building on a good foundation of prior work. Plug Power has developed a complete platform design based on requirements for sub-systems and complete system.
- This excellent approach to testing and selecting components should work successfully and economically in the niche power backup applications.
- The flexible platform design has 50% cost reduction over prototype design while maintaining high efficiency.
- Numerous successes have been somewhat tempered by a number of No-Go disappointments, but it was important to investigate them.
- Power Conditioning System efficiency appears excellent, roughly 95 - 97%.
- Plug Power determined what technology will be fully incorporated into the system.
- Membrane is degradation severe. Target is 1500 hours over 10 years. Again, given the well known catalyst dissolution issue above roughly 0.9 volts and other issues of system life without respect to operation, this long of a lifetime may be an issue.

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

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

- Partners are generally users who will provide on-site operational performance and durability data from a user perspective.
- Excellent combination of interaction with suppliers, potential users, and validation partners.
- User community is well represented.
- Argonne National Laboratory; Bell Labs, etc. are potential demonstration partners, not collaborators.
- Because this project involves commercialization, the necessary customers have been involved.

Question 5: Approach to and relevance of proposed future research

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

- Plans are in place to complete the project in a timely fashion and on-schedule.
- A lot remains to be done in the remaining project time.
- No future "research" is planned. Future plans include engineering and packaging systems for the demonstration.

Strengths and weaknesses

Strengths
- Strong capability, based Plug Power experience and fuel cell expertise; this project will result in a commercial product.
- Excellent approach to develop a commercially-viable fuel cell back-up power unit that has a high probability of success.
- The presentation described a clear, thoughtful road of product development and a plan for beta testing at customer sites.

Weaknesses
- None apparent – great work!
- In retrospect, it is possible that the overall scope of the project was too ambitious.

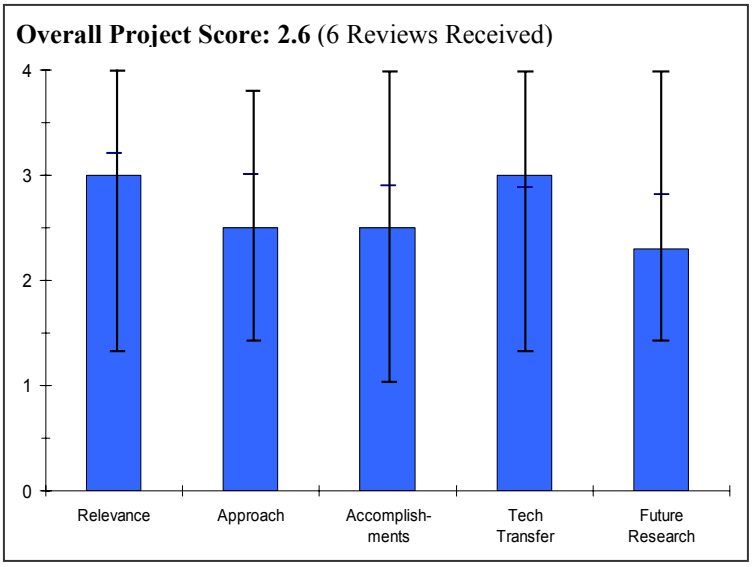
Specific recommendations and additions or deletions to the work scope

- None – the project appears to be very well conceived and operated – it should be a great success for Plug Power and for the DOE program!
- Review feasibility/merit of pursuing all of the remaining (yet to be completed) tasks.
- The project needs to ensure that target specifications can be met even under favorable technology assumptions. If not, then the project should focus on resolving the technology issues for this application.

Project # FC-48: Economic Analysis of Stationary PEM Fuel Cell Systems
Stone, Harry J.; Battelle Memorial Institute

Brief Summary of Project

Battelle Memorial Institute and its team will develop 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 stationary 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.



Question 1: Relevance to overall DOE objectives

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

- Project assesses viability of PEMFC technology to meet cost targets for selected stationary markets.
- Stationary markets are not the primary focus of the Hydrogen fuel initiative.
- Commercial products are already available. Market dynamics are already in play.
- Analysis and comparison to competing technologies is important to be able to focus the efforts on the most critical barriers.
- Excellent, since cost and durability are the major barriers to fuel-cell commercialization, a better understanding of economic factors is critical.
- Program is on stationary fuel cells and contributes to transportation fuel cells in an indirect fashion.

Question 2: Approach to performing the research and development

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

- This project approach is being conducted as if no prior work has been performed. Why did Battelle focus on propane rather than natural gas?
- Approach is flawed since there is no explanation or review of conclusions with stakeholders and opportunity for feedback. Industry is not involved in decisions on selected applications. Much greater interfacing is required with stakeholders.
- Battelle team's limited knowledge of fuel cell industry and markets is apparent.
- Overall, the approach is good. Battelle has tried to consult with technical experts to verify their assumptions, but Battelle needs to broaden collaboration with fuel-cell OEMs to ensure more realistic input to the models.
- Approach is industry based and brings in the data from multiple industry sources.
- Program has ability to integrate design concepts and marketing approaches from several industries.

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

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

- Deliverables fit the project's objectives to date.
- Product specs are already available – why set targets now? Cost analyses is not supported. Costs do not take into account recycling or manufacturing costs.
- Choice of applications and economics are critical to the success of the analysis. A 50kW stationary propane application and targets are not consistent. Study depends on matching of market requirements, and application-data is not shown.
- Limited market data for size selection; customers are not involved for all applications.
- Selections were not linked to market input but rather to an averaging of inputs from potential suppliers who had not been in markets.
- Time spans are off – 5kW backup system is now.
- Status Table and sensitivity analyses are useful.
- The project has shown good progress in collecting data.
- The assumptions for many of the data points need to reviewed among the several industry contacts.
- Large-scale jumps from 1000 to 10,000 to 100,000 in manufacturing capability may not reflect the true nature of the growth of the industry.
- Lifetime of fuel cells seems excessive even in the back-up power mode.
- Power assumption for fuel cell is too low.
- True nature of fuel cell stack cost and balance of plant for air and thermal management are lumped together. Subsequent breakout of cost yields a better understanding.
- Some of data needs discussion of assumptions which was not given.

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

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

- At this stage the backup power units are commercial. The most relevant data would be proprietary. Who is serving whom?
- No interactive feedback of conclusions with stakeholders; decisions are arbitrary.
- Cost analysis is not realistic and industry based. Fuel cell knowledge base needs improvement.
- System designs are several years old and need updating for their 15 year projection.
- 15 year projection is ridiculous – use 5 or 10 years instead.
- Large number of partners are providing stakeholder input.
- Good, but need to expand interaction with OEMs (see Recommendations below).
- The program appears to have full support from industry.
- Battelle noted several publications and conference proceedings

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Based on results to date, Battelle should start fresh and work more closely with OEMs who have fuel cell products. Economic and life cycle analysis must be based on realistic applications and costing. We have had 200kW systems in the field for years. Let's not plow that field again. Much data is available already.
- Good, however, iterations should include closely verifying that key parameters are correct. One example is that UTC Fuel Cells has demonstrated PEM fuel-cell power densities of ~ 1 W/cm^2 at acceptable efficiencies and standard catalyst loadings. Using this value could really influence the conclusions here.
- Proposed research is consistent with program plans.
- Assumptions should be reviewed and detailed for readers.

Strengths and weaknesses

Strengths
- Limited.
- Battelle is good at organizing workshops with industry representatives.
- Battelle has had many interactions with stakeholders.
- The program is well-organized and structured.

Weaknesses
- This analysis is not treating the state of the art as "real". The overall cost modeling activities show no apparent depth. Each "system" will be proprietary – data input will be tenuous.
- Some key assumptions (e.g., power density ranges) are not realistic (too low). The assumptions that are shown to be critical through the sensitivity analysis must be confirmed with industry leaders. Include both state-of-the-art values, as well as projections provided by pragmatic experts.
- Program is dependent upon inputs from industry and does not appear to have means of cross-checking the validity of the industry inputs.

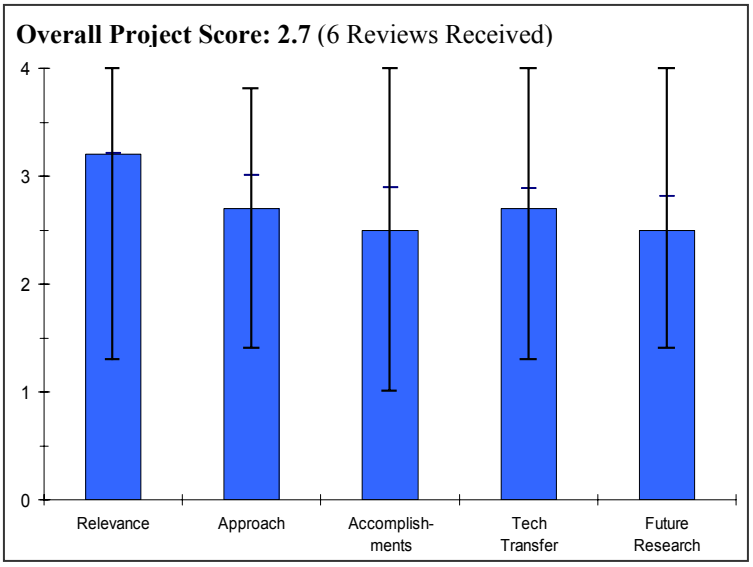
Specific recommendations and additions or deletions to the work scope

- Terminate this program. Let the Relion's, Ballard's, IdaTech's and Plug Power's of the world proceed with commercialization and market activities.
- Perform a complete review of this project with tighter definition of the assumptions, approach and deliverables developed.
- Expand interactions with fuel-cell system OEMs, including industry leaders (e.g., UTC Fuel Cells) to ensure that assumed values are correct.
- Develop means of cross checking industry inputs.
- Input more recent performance data into program; data is available in open literature.
- Evaluate the cost of warrantees, installation, and set up on the overall cost of a powerplant.

Project # FC-49: Advanced Buildings PEM Fuel Cell System
Taylor, Kyle; IdaTech

Brief Summary of Project

The objective of the IdaTech project is to demonstrate high electrical and overall efficiency, reduced energy consumption, and reduced emissions for hotel and follow-on applications. IdaTech will overcome technical and cost barriers through the engineering, design and construction of an integrated system with advanced fuel cell, fuel processor, and balance of plant subsystems. The 50 kW PEM fuel cell system design will be validated through field testing at three separate properties to be co-selected by Marriott International, Sempra Utilities and Puget Sound Energy. IdaTech will use the information provided from this demonstration to target early market entry opportunities.



Question 1: Relevance to overall DOE objectives

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

- Building demonstration program is very important.
- Stationary applications are an important part of launching the hydrogen economy.
- All objectives of this project are supportive of DOE's goals.
- A stationary fuel cell system will be desirable when hydrogen replaces fossil fuels.
- Project is focused on integrating and increasing the commercial viability of a stationary fuel cell power system.
- The lack of clearly identified technical performance objectives hinders the ability to ascertain the project metrics.
- One reviewer remarked that the development of an industrial, stationary reformer-based power plant has limited relevance to DOE Fuel Cell automotive Program; the best overlap is in the areas of endurance and low-cost.

Question 2: Approach to performing the research and development

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

- Approach is good but execution is weak. There is too much to perform in the remaining 18 months.
- Demonstration of alpha and beta stationary power systems is laudable. It is unclear what technical benefits accrue to the DOE Program.
- IdaTech aims to achieve the goals in the technology roadmap.
- The project is basically a field demonstration program – building 3 prototype integrated systems of current technologies for field test.
- Fuel cell module is scale up of 5 kW existing IdaTech module.
- Classic systems engineering/integration approach is being applied to the major subsystems, i.e. steam methane reforming, pressure swing absorption, balance of plant (BOP), and membrane electrode assemblies (MEAs). However, project resources might not be adequate to fulfill objective.
- The project focuses on membrane development – IdaTech will use off-the-shelf BOP and steam methane reforming and pressure swing absorption systems.
- IdaTech will use existing MEA.

- IdaTech is attempting to overcome technical and cost barriers via system engineering versus technology improvement.
- IdaTech will demonstrate high electrical and total efficiency.
- No estimate was given of the best system performance based on even favorable assumptions. It was difficult to assess the validity of this approach or whether more technology development is required.
- The BOP seems complicated. Cost targets were not provided but a complicated BOP would impact them.
- Approach does not clearly lay out the specific technical barriers in each of the development areas (fuel treatment, fuel processing, stack, BOP).
- The approach does not clearly lay out the technical performance criteria for each of the four phases.
- Milestones lack technical specificity and dates.
- Approach does appear to leverage existing industrial equipment in some areas.

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

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

- Component validation is good. Scale up will be challenging. Reviewer cannot assess status of stack progress.
- Although the project was stated to be 49% complete, there were very few pictures of actual hardware. Goals stressed stack material, manufacturing costs and durability; there was little mention of any progress here.
- IdaTech operated half-scale fuel processing system and validated subsystem operation.
- IdaTech is scaling up reactor design for "optimized" performance.
- IdaTech developed packed-bed model for the reformer design.
- CFD was used to help design system flows and heat transfer.
- IdaTech has completed full-scale reactor design and begun fabrication.
- Fuel cell module is under design – a scale-up of an existing product.
- IdaTech identified off-the-shelf inverter to step up voltage.
- IdaTech has designed the complete system – on schedule to be operational this summer.
- At the 50% completed stage, this project will have to have a very industrious 18 months in order to achieve its objective of field evaluation of the Beta systems.
- Most work was focused on validating sub-systems. This approach is good; it provides flexibility in configuring different approaches and avoids the expense and inflexibility of testing large complete systems.
- Technical accomplishments appear to be progressing on a number of fronts, but are very generic. It is difficult to fully assess their significance due to the lack specific technical data. Accomplishments are mostly on the integration side – not specific improvements in component performance.

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

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

- The extent of the collaborations is not clear (too early?)
- Contractor has a good support team for stationary power beta demonstration project.
- There is good participation from field test sites.
- More collaboration from key component suppliers would be helpful.
- Reviewer is not convinced that the collaboration and expertise of the partnership is broad-based enough to deliver the goods in a timely manner.
- There is a good list of potential demonstration partners, but it is not clear to what level each is engaged.
- Project ostensibly seems to have a number of partners, but they appear to be heavily weighted toward latter field evaluation. Role of partners is not clearly delineated.
- Additional collaborations would help improve component performance. For example, a firm specializing in stacks would be beneficial to IdaTech in their efforts to optimize the MEA.

Question 5: Approach to and relevance of proposed future research

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

- Plans to complete beta testing appear realistic.
- Basic modules will be constructed.
- Assembly documentation for alpha is underway.
- Long term durability testing for alpha will be later this year.
- Future work appears to be real stretch to achieve in the time remaining.
- Future work is not research, but the sub system validation approach is good. The design process appears disciplined.
- Contingencies and off-ramps are not well developed.
- Potential show-stoppers and the methodologies and partners to help overcome them are not well addressed.

Strengths and weaknesses

Strengths
- There is a coordinated effort on subcomponents.
- Contractor understands stationary power requirements very well. Contractor has laid out a realistic plan to achieve objectives.
- The development is straightforward using off-the-shelf parts.
- The project will give a good benchmark of existing fuel cell technology.
- IdaTech appears to have strong overall system integration capabilities.
- Significant effort has been placed on identifying partners for the demonstration and evaluation phases of the project.

Weaknesses
- There is little time to address critical issues if they arise.
- There was very little progress in the system development. Presentation did not show much actual hardware. It is unclear if the contractor truly appreciates the amount of effort required to scale the technology from 5kW to 50kW.
- The project does not advance the state-of-the-art.
- The project seems like an effort to jump-start a market for stationary fuel cells and a specific company rather than R&D (corporate welfare?).
- There is considerable duplication in reformer design – it is not likely to be significantly better than other design efforts supported by this program.
- There is no data yet on full 50 kW stack – very vague on configuration and no real indication of efficiencies or durability – which is a little disturbing for a program that is 50% complete.
- IdaTech did not discuss plans for data collection and analysis for either the Alpha system or the Beta field test systems.
- Project scope appears to be overly ambitious.
- Project is very generic and badly needs specific technical performance targets for each of the four project areas and clearly delineated approaches to achieving them.
- Project would benefit from additional collaborations to improve specific component performance.

Specific recommendations and additions or deletions to the work scope

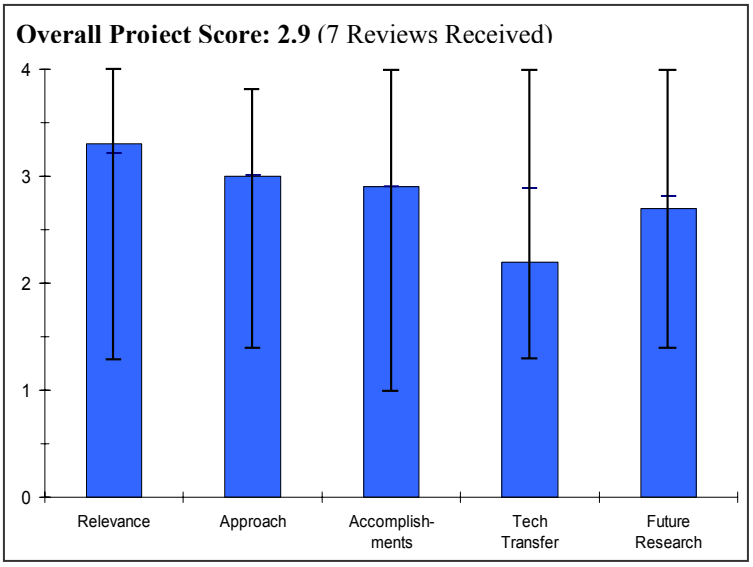
- Compare the efficiency of IdaTech's reformer systems compared to others developed by the DOE program.
- Compare the efficiency and durability of the IdaTech fuel cell stack with others developed by the DOE program.
- Closely monitor Alpha system operation, efficiency, and durability with possible modifications to program based on those results – if they don't meet DOE targets or suffer problems or delays, consider reducing the scope or canceling this project.
- Review and prioritize remaining activities and maybe rationalize appropriately.

- More clearly determine system targets relative to current technology available. Determine path forward based on this analysis.
- Clearly-defined success and failure metrics should be identified.
- IdaTech may benefit by focusing even more on being the technology integrator, as opposed to the technology developer

Project # FC-50: Investigating Failure in Polymer-Electrolyte Fuel Cells
Newman, John; Lawrence Berkeley National Laboratory

Brief Summary of Project

This project focuses on examining various types of fuel-cell failure, including both modeling and supporting experimental studies of fuel cell water and thermal management under various conditions including subzero (i.e., freeze) operation. The advanced mathematical models describe the relevant phenomena that occur in the fuel cell under the various conditions of interest. 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 optimization of material properties, operating conditions, and possibly start-up and shut-down scenarios.



Question 1: Relevance to overall DOE objectives

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

- Key next step to fuel cell commercialization: operation/start-up at sub-zero temperatures.
- Extremely relevant.
- Durability is key issue facing PEM fuel cells.
- It is unclear which failure mode will be focused on in this project. This size of project cannot cover numbers of failure modes.
- MEA durability is one of the primary technical barriers to fuel cell commercialization. A fundamental understanding of the failure mechanisms is critical.
- Good theoretical approach - badly needed.

Question 2: Approach to performing the research and development

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

- Strong experimental and modeling effort with appreciated goal of using results for optimization of materials.
- Although water 'content' certainly number one concern for freezing conditions, should other aspects of the materials be examined (will PTFE and/or PFSA membranes have phase issues?)
- Approach is good but quite ambiguous. Modeling combined with experiment will be valuable in addressing issues.
- Combining model with experimentation should provide valuable insight.
- It is good to take both model-based approach and experimental approach but it is unclear how to obtain material properties to characterize and optimize MEAs.
- A fundamental cell model and good property data, as proposed here, are vital to understanding of fuel cell performance failure.
- Not clear if a dynamic model is planned. As failures due to transient operation were mentioned, the approach would certainly benefit from such a dynamic model.
- The incorporation of accurate two-phase flow is key, and certainly non-trivial. Could be a significant contribution.

- Not clear what validation is planned.
- Needs to incorporate the "three states of water" ideas.

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

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

- Good progress on very difficult two-phase modeling ventures.
- Key issues: initiated modeling and testing of critical parameters. Good start at delineating key design issues.
- Model looks promising.
- Results consistent with start date.
- Program just recently commenced, thus score is very tentative.
- New project.

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

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

- While UTC is one of the fuel cell system leaders, not sure they are best collaborator.
- Too early.
- Limited interactions.
- Focused on only UTC system.
- It is recommended that the PI consider leveraging other project outcomes to identify failure mode mechanisms. Material property information should be incorporated by material suppliers.
- Unclear as to the roles of LANL and UTCFC, other than the affiliations of two other LBNL contributors.
- A strong validation effort for freeze, spatially resolved temperatures, currents, RHs, failure mechanisms and sites, etc. is vital. LANL, UTC, and others can play a valuable role.
- New project.
- The UTC and LANL collaborations will be crucial, and must be maintained to bring experimental verification and applications for model results.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- Some of the membrane-related issues cited by the PI may be best addressed with a partner.
- Needs more specifics.
- Include other experimental data to develop his model.
- The opportunities and issues with low RH operation are most important to address. What surface energy(ies) is needed for multi-temperature, multi-RH-capable fuel cell MEAs?
- Include other experimental data to develop the models.

Strengths and weaknesses

Strengths
- Premier modeling group with a long and consistent track record of success in grappling with complex systems and producing systematic understanding with “reality-checked” models.
- Very qualified group.
- Combining modeling with experimentation.
- The principal investigator's 30 years of fuel cell experience.

Weaknesses

- While it is agreed that water is the main culprit in sub-freezing conditions, are there other materials problems that need to be considered, such as freeze-related destruction issues ("frost heave") that could be anticipated with a model?
- Is the group trying to accomplish too much?
- Only one system – UTC. Will results translate to other system designs?
- Light on details. Provide more specifics on plans and collaborations.

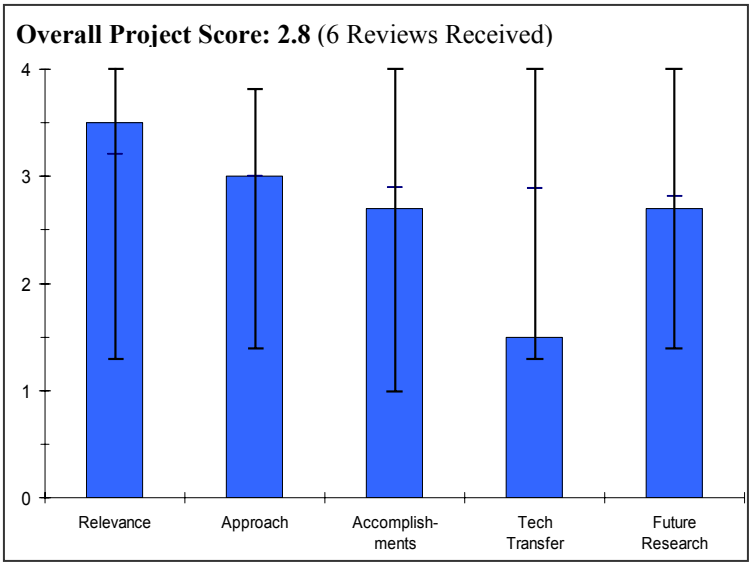
Specific recommendations and additions or deletions to the work scope

- While UTC is widely acknowledged as a fuel cell technology leader are they the most appropriate for a freeze related program? The UTC "water-filled" bipolar plate and matched GDL design are quite different from the majority of the industry and may not be the best system to mate with these modeling efforts.
- As the PI discussed, some membrane-related issues in sub-zero operation suggest forming collaboration with a membrane leader.
- This project should focus on model-base approach.
- Consider developing a dynamic model if this isn't already so. May need to be a simplified model for computational reasons.
- Consider exploring mal-distribution of temperature and flows (air, H_2, coolant, due to channel and cell tolerances). These may be strong contributors to failure that will not be explained by a perfect single-channel model.
- Land effects should be included.
- Co-, counter- and cross-flow should all be modeled.

Project # FCP-01: Montana PEM Membrane Degradation Study
Spangler, Lee; Montana State University

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.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Understanding and validating degradation mechanisms is key to the success of the PEMFCs and the Hydrogen Fuel Initiative.
- Needed fundamental work on decay mechanisms.
- The project goes deep into the membrane degradation evaluation which is one of PEM technology obstacles.
- Novel approach- but hard to interpret.
- Durability increases are central to the Initiative.
- Addresses membrane durability and degradation goals and objectives.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The PI utilized Nafion 117 for his NMR experiments - Nafion 112 would more applicable for PEM applications.
- A comparison of extruded versus solvent cast Nafion would also be of interest - this might provide further information on the difference in manufacturing techniques.
- The PIs approach of voltage signatures for MEA degradation is incredibly innovative.
- How all data will be analyzed to find 'signatures' for decay mechanisms is unclear.
- Excellent use of MRI.
- The approach from three different analysis methods is interesting and with experts it should bring out some good results.
- The monitoring and NMR are useful techniques.
- Approach to getting individual membrane data is a good approach. Would like to see better tie of data to potential failure mechanisms.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- No voltage signatures to predict MEA degradation have been found to date.
- No correlation of NMR studies to fuel cell experiments and MEA degradation have been done to date.
- Very interesting results with imaging of new membranes.
- No data on tested/aged membranes.

- So far there are no final results but the preliminary results seem interesting.
- Four membranes? 117? Publications light for a university.
- The stack used, nature of the membranes used, and protocol is not alluded to in the poster.
- Publications light for a university.
- Poster information does not include the kind of stack used, the nature of the membranes, or any summary of the research protocol.
- The T2 heterogeneity of the membrane observed is a provocative result. Needs confirmation with extensive controls described.
- Lots of data but limited analysis so far.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.5** for technology transfer and collaboration.

- This project should work with Dupont. Currently there is no collaboration/interaction.
- No collaborations with outside researchers. Collaboration with MEA supplier would be beneficial.
- There is no internal organizational collaboration. At least there should be membranes and MEA manufacturers involved in the project.
- Don't see much.
- The program would benefit from collaboration with other institutions that might combine these results with further chemical and mechanical insights into the failure modes.
- Should partner with a membrane/MEA manufacturer and/or user.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- The proposed work could potentially be excellent if enough progress is made.
- It is not clear if the PI has the personnel and time to complete the proposed research project.
- Image plans very good.
- Unclear how data mining data for signatures will work.
- The preliminary results show some general failure systematics. So those areas should be studied deeper.
- If time and/or budget allow, consider AC impedance and hydrogen/nitrogen crossover current as additional electrical properties which may give insight into degradation.
- PI should try to make connections between the data collected and the potential failure modes.

Strengths and weaknesses

Strengths
- The concept of finding voltage signatures on MEA degradation is one which could be incredibly interesting.
- NMR analysis of various sections of the MEAs could bring more insight on membrane degradation in fuel cell operation.
- Well defined methods for imaging.
- Good theoretical know-how in three different but supplementary areas. Enthusiastic researchers.

Weaknesses
- No voltage signatures have been found yet - progress has been slow.
- Pre-treatment of membrane for NMR studies could alter membrane morphology/chemistry.
- No involvements with outside researchers- need to work with MEA manufacturer.
- Lack of industrial collaboration.

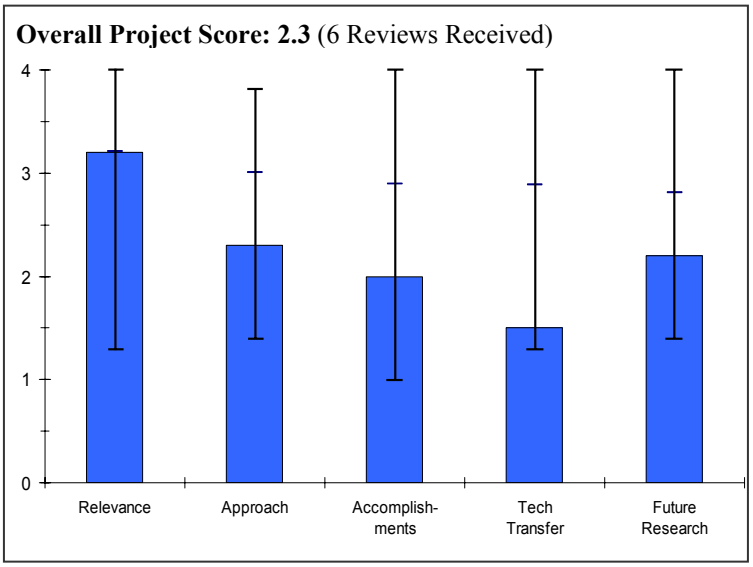
Specific recommendations and additions or deletions to the work scope

- Excellent concept - slow progress to date. Interaction with Dupont could help program.
- Integrate membranes or MEA manufacturers into this project.
- Including potential users as partners might help in determining how to use data to identify failure modes.

Project # FCP-02: Development of Higher Temperature Membrane and Electrode Assembly for Proton Exchange Membrane Fuel Cell Device
DeCarmine, Tony; Oxford Performance Materials, Inc.

Brief Summary of Project

Oxford Performance Materials, Inc. will develop novel sulfonated polyether ketone blends for use as fuel cell membranes. The focus will be to fabricate stable MEAs for operation at 120 C and low relative humidity from SPEKK-based membranes and catalyst layers. The targeted performance objectives are to surpass the electrochemical and mechanical durability performance of Nafion at similar operating conditions.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The PIs goal of developing a high temperature/low relative humidity membrane is a viable part of the hydrogen fuel initiative.
- Higher temperature/low RH membranes and MEAs (membrane electrode assemblies) have the potential to reduce the complexity and cost of FC systems.
- The project goal of improving PEM-FC membranes for higher temperature operation is important for fuel cell program success.
- It is not clear how (nor was the PI able to explain how) the candidate materials being studied would be superior to membrane materials other than Nation that have already been developed for high-temperature operation (e.g., Celanese Celtec PBI, 3M, etc.).
- The high temperature membranes are one key area of transportation PEM systems.
- The project does a good job of addressing a critical area of need for the program.
- The relevance of the project is good and it addresses important issues.

Question 2: Approach to performing the research and development

This project was rated **2.3** on its approach.

- Initial concept was innovative.
- PEEKs (poly ether ester ketone membranes) are very chemically and mechanically robust materials.
- PIs modest data shows the approach was lacking - no clear viable path to successfully produce viable fuel cell membranes using PEEK as the base material.
- The "Technical Approach," as presented, is merely high-level generality (e.g., "Replace Nafion with Novel Polymer"), and "Improve Properties").
- Lacks a systematic research plan.
- Membrane research is very much trial and error. When this is done by experts it may lead to good results.
- Methodical approach to the problem addressed.
- The approach is confusing and difficult to review. The presentation is not clear on exactly what the project is trying to show

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.0** based on accomplishments.

- Minimal progress.
- Modest accomplishments.
- PI is still speculative with no clear pathway for membrane success.
- Cross linking of membrane may help.
- Membrane has many technical issues - dissolves in water.
- The presentation included few quantitative results.
- The presentation included little description of experimental method or design (even though construction of "test station" was major accomplishment of period).
- Considering the low level of funding and project duration outstanding progress.
- The data presented this late in the project is not very comprehensive.
- Very little characterization of the materials was presented and no fuel cell operating data was available.
- Only a few conductivity points are shown at 25C. No data about higher temperatures is given and the other results indicate numerous problems to be overcome in future. For example, what happens to the blends at higher temperatures? How do the blends behave in the MEAs in proximity to the electrode? The presentation talks about MEAs but the presentation does not give confidence that the problems of the interface re understood.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.5** for technology transfer and collaboration.

- Collaboration with UConn - papers published.
- PI needs input from stack developer in order to obtain key membrane requirements.
- The collaboration with UConn- so important in previous reviews of this project- is gone and no longer in evidence.
- UConn was the source of supply of SPEKK material, now no longer available.
- The publication record is weak- two of three cited publications are "Abstracts" (not peer reviewed).
- There is no or very little collaboration.
- No specifics on this presented but there are indications from the technical presentations at meetings that this is being disseminated.
- There does not seem to be any collaboration with other organizations.
- There does not seem to be any active collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.2** for proposed future work.

- Membrane evaluation straight into a MEA and fuel cell experiments is pre-mature.
- PI should acquire knowledge to run appropriate ex-situ tests before MEA fabrication.
- Future research plans do not articulate goals for new materials that would be technically superior in some respect to the recent membrane material products made by others.
- PI has many ostensibly good ideas he intends to try.
- PI's optimism would be more credible were there to be more reported data from the project.
- Too difficult to say because the proposed membrane is not developed yet.
- Logical future plan. Direct discussions with the presenter revealed a good understanding of the options for good progress to be made.
- Project is nearing completion date and chances of success by that time seem low.
- The stabilizing strategies may help but there seems to be a lot of issues to resolve about the materials. Since the presentation does not explain the phase separation properties it is hard to know how this will pan out.
- The plans for MEA fabrication need to be spelled out more clearly e.g. just exactly what properties are sought.

Strengths and weaknesses

Strengths
- Base membrane, PEEK has good potential.
- PI is enthusiastic about what he might accomplish.
- Good practical and experimental know-how. Good knowledge about the manufacturing and about the markets.
- Unique high temperature capable polymer systems with interesting materials characteristics.

Weaknesses
- Membrane potential has not been achieved - none of the pathways have resulted in viable membrane.
- Membrane materials have low conductivity and dissolve in water.
- PI lacks expertise to perform ex-situ analysis.
- Lack of well-defined goals with respect to current state-of-the-art.
- Project has appearance of "starting over" with lack of continuity from first year.
- Lack of quantitative results from previous work.
- Industry may have other interests than doing experiments.
- As noted by the presenter- the engineering challenges surrounding the condensation of superheated steam in the hydrophilic polymer matrix could prove problematic in general for all the higher temperature membrane systems under consideration by the Hydrogen Program.
- This work is very preliminary and I believe is trying to demonstrate the proton conduction mechanisms enhanced by the blends and phase separation. Since some of these materials are unlikely to be stable in a fuel cell some of the experiments seem to try to scale up inappropriately at this time. There seems to be much confusion about the goals of this project.

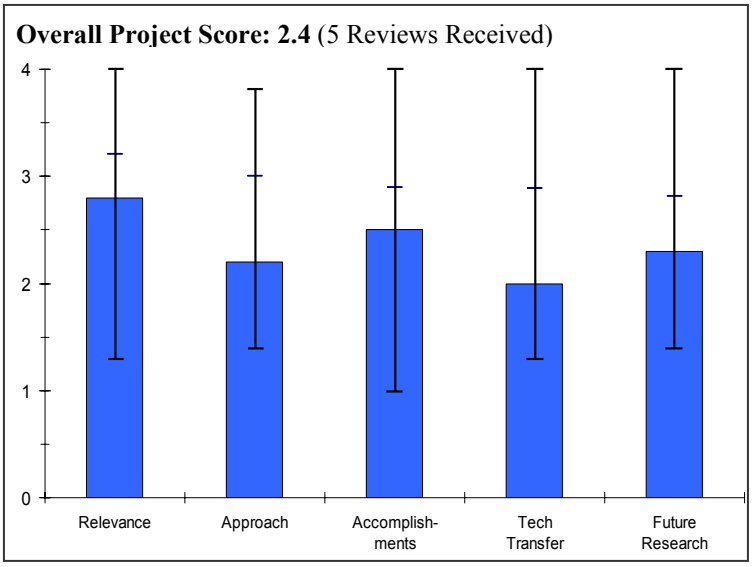
Specific recommendations and additions or deletions to the work scope

- Not to do MEA fabrication or fuel cell experiments until quality ex-situ data is obtained.
- While PEEK has initial promise, PEEK does not seem to be or become a viable building polymer for fuel cell membranes.
- Link this project with university teams to do the experimental work.
- As discussed with the presenter various cross linking options should be evaluated with the hydrophilic component. I recommend post membrane formation treatments.

Project # FCP-03: Advanced Fuel Cell Membranes Based on Heteropolyacids
Turner, John; National Renewable Energy Laboratory

Brief Summary of Project

The goal of the project at the National Renewable Energy Laboratory (NREL) in partnership with the Colorado School of Mines is to acquire an improved fundamental understanding of a class of inorganic proton conductors (heteropoly acids and their salts) that exhibit high proton conductivity at low humidities (below 25% RH) and at elevated temperatures (well above 100°C) and to apply that understanding to fuel cell membrane technology.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- The PI understands the need for high temperature/low relative humidity membranes for PEMFC applications.
- High temperature and low RH will help minimize the complexity of fuel cell systems and cost.
- Concept may or may not be low cost depending on how the acid is "membranized" and/or immobilized.
- Important problems.
- Relevance to 'solution' of problem unclear.
- The project directly addresses how to operate fuel cells at temperatures that are difficult to use water as a solvent. In addition to addressing heat management problems, significant system simplifications would result that will lead to lower costs. This one interesting approach to retaining "water "in the membrane that allows retention of known polymer materials. This project will become outstanding when and if the performance in the MEA is addressed. This aspect is lacking and plans to study the issues in the interface need to be formulated.

Question 2: Approach to performing the research and development

This project was rated **2.2** on its approach.

- Nafion doped in heteropoly acids (HPAs) has been shown to be feasible in 1997 by Malahotra and Datta - the PI is in need of new insight.
- The PI is in need of a realistic perspective on membrane conductivity needs for viable membranes. "Fixing" HPAs in silica/polymer matrix to be the sole proton conduction mechanism will be insufficient to obtain any practical conductivity values.
- Fundamental approach to understanding which heteropolyacids to try is sound.
- Need durability studies in actual operating fuel cell conditions.
- Concept is interesting, but it will be very challenging to stabilize the resulting compounds.
- Heteropolyacid (HPA) approach is sound as a demonstration but water solubility must be addressed—ignored here.
- HPA work is old.
- New work on polymer seems to be less than impressive. How will these polymers help the cause?
- Particularly like the scientific approach to the proton carrier's structures rather than a sand in the membrane approach. This approach should lead to understanding and not just whether it works or not. Real scientific conclusions should result.

- The one weakness is the lack of an approach to the MEA. How will the material behave in close proximity to electrode surfaces and how will adhesion be maintained?

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- Proof of concept demonstrated by PI has been shown eight years ago by another lab.
- PI needs to present conductivity values for membranes with "fixed" HPAs in silica/polymer matrix.
- Reasonable results given that this is a new program.
- One of the few new, alternative ideas for membranes in the whole DOE program.
- Unable to determine accomplishments - too early in project.
- This year's technical accomplishments toward DOE goals appear to be minimal.
- No new results on HPAs to make them more compelling.
- Making polymers but without apparent rhyme or reason.
- Excellent progress in membrane performance has been made. The MEA should be next. Gas permeabilities should be estimated.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- Good collaboration between university and lab.
- The PI did not discuss or present collaboration with 3M - unclear what the collaboration is for or when it will begin.
- None shown with major players - 3M listed but what is their role, if any, in the program?
- Too early to tell.
- Needs to expand collaboration since they rely on University partner for HPA expertise, the most promising part of project.
- Needs influx of good polymer chemistry ideas.
- The collaboration with CSM is strong although the presentation does not make the respective roles of NREL and CSM clear. The collaboration with 3M does not seem obvious apart from reporting data to 3M for them to use in their presentation. One would think 3M would have input to clarify what is meant by mechanically strong, film-forming properties and most of all what is likely to be required in the MEA structure.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- The PI presented future research in regards to HPAs/silica polymer matrices. If composite membrane does not work, no other ideas were presented.
- If you believe the approach is worth doing, their future work is a reasonable path.
- Initial proposed ideas seem OK.
- No clear path provided to achieve lower solubility, improved materials that can meet DOE goals.
- Need to address the different needs of the MEA. This could be a show stopper and 3M should provide guidance.

Strengths and weaknesses

Strengths
- Solid basic understanding of polyacids.
- University collaborator.
- Large collection of HPA chemistry to choose from.
- Good scientific approach.

Weaknesses

- PIs focus on HPAs is unoriginal and lacks real technical feasibility for PEMFC applications.
- It is well known that acids readily wash out of polymer films during fuel cell operation - though this team recognizes that, I remain unconvinced based upon their approach that they will succeed in solving this problem
- Limited fuel cell testing and/or capabilities, especially for "real" conditions, i.e., thermal and RH cycling, long-term durability tests, detailed diagnostic testing capability, etc. This area is where a strong industrial partner could really move the program forward quickly and effectively.
- Polymer work is second-rate.
- This work seems to be the vanity project for the PI—the focus on what is really needed by DOE is lacking.
- Polymer part is weak – need to involve 3M more actively.

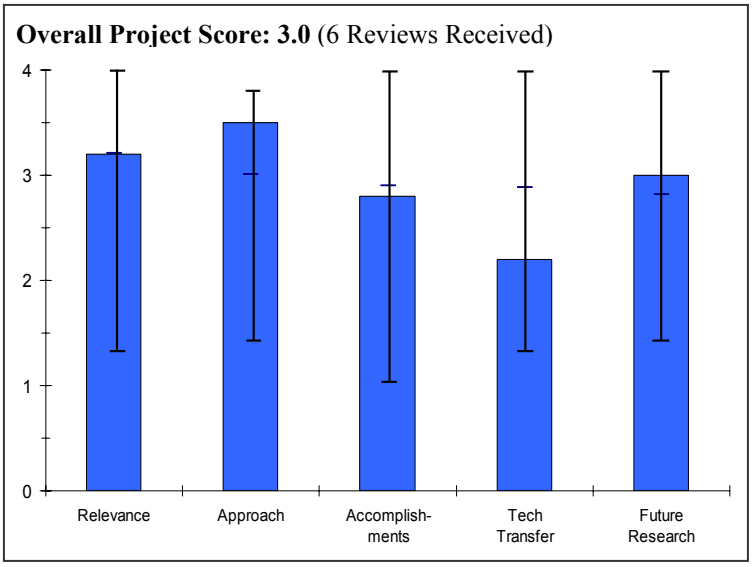
Specific recommendations and additions or deletions to the work scope

- Collaboration with 3M would help this project tremendously in regards to practicality and research direction - the PIs should start/increase collaboration.
- A high risk program with a low level of funding. The potential rewards of success, though, are not that high, I would certainly not increase funding level for this off-beat concept as I saw little that indicated they would be able to make a reasonable membrane, much less one where the acid was immobilized.
- If it is continued, real-world testing with thermal and RH cycling as well as long-term durability fuel cell tests should be included.
- Real collaboration with a major player (3M, DuPont, Gore, Ballard, UTC, etc.) must be included to allow this program to have any chance of success. If they can sell the program to a real commercial venture (who is willing to invest some of their time and/or money), then maybe I am wrong, and it should continue to receive funding.
- Increase role of university. Provide support for that effort. More teaming or idea exchange with polymer chemists. Integrate with High Temperature program.
- MEA behavior/design needs to be added.

Project # FCP-04: Non-Nafion Membrane Electrode Assemblies
Pivovar, Bryan; Los Alamos National Laboratory

Brief Summary of Project

This Los Alamos National Laboratory (LANL) project investigates the use of alternative (non-perfluorosulfonic acid) ionomers in fuel cells. Alternative ionomers offer significant potential advantages in terms of cost, durability and performance, due to easier processing, lower cost of base materials, higher glass transition temperatures, and a wider range of tunable characteristics. The performance and degradation mechanisms of these systems will be investigated in terms of membrane-electrode compatibility and catastrophic failure in terms of pin-hole formation to understand better and extend the performance and limitations of such systems.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Developing and understanding non-Nafion MEAs (membrane electrode assemblies) are key to the success of the Hydrogen Fuel Initiative.
- Non-Nafion membranes reduce the cost significantly.
- Low cost membranes are critical-effort represents good start on a new system.
- BPSH has been around a long time now. If it were really that great, it would have been adopted by some commercial MEA supplier.
- Focus on interfacial effects really only relevant to BPSH, and don't translate well to other polymers and/or other industry standard materials and their issue.
- Relevance depends on the price of Nafion (and similar materials) when manufactured in large quantities.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- Technical barriers have been addressed and overcome.
- Very good approach to focus on one class of membrane materials and study its issues and potentials in some depth. Not all the programs do this. The information being learned, e.g. on electrode interface formation and durability, should apply to any other MEA systems as well.
- All aspects are being covered apparently: polymer, electrodes, and testing. Well organized effort.
- Approach is solid and well-thought out if you buy into the value of what they are doing.
- Technically, this approach seems very good and based on sound science.
- Trying alternative chemistries is the only way to find a Nafion alternative.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Excellent data has been obtained in MeOH.
- Excellent model has been postulated which is applicable to other membrane systems.
- Very good work. This project has focused on a real and difficult problem, the interface of electrodes and membranes, and provided critical new insight.
- Good progress in membranes and electrode fundamentals. Much more covered than reported (per in-depth discussion with presenter.)
- Good work on polymer of peripheral interest to DOE goals.
- Thus far, results look promising for a lower-cost (albeit not necessarily as good) alternative.
- Identified probably failure mechanisms.
- Alternative polymer backbone is unstable.
- Additive doesn't improve stability.
- No clear pathway to improving stability of backbone or additive.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.2** for technology transfer and collaboration.

- Good interaction with universities.
- Stack developer involved may help this project further.
- The challenges of making a good performing MEA include the catalyst electrode structure and gas diffusion layer technologies as well. Just comparing to Nafion MEAs as a benchmark will not necessarily allow the new MEA to be optimized. The program would benefit from collaborations with institutions having extra expertise in these areas.
- Time to expand partnerships.
- I didn't see any.
- Poster shows little collaboration, but verbally, they say that considerable is occurring.
- No technology to transfer yet.
- Good collaboration with universities.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- To apply same methods to PEMFC will be crucial.
- The poster and presenter did not comment on the reversible decay processes apparent in slides 8 and 12. This is a key indicator that something other than interface delamination is occurring. This type of issue is seen in Nafion-based MEAs due to reversible impurity adsorption. The PI should consider any possible sources (from the new membrane?) that could cause the catalyst to be temporarily poisoned. If not, what is the source of this significant current density loss?
- Would like to see more H_s-air over methanol.
- Again if you believe in the work, the proposed future work is ok.
- Continuation as proposed seems to be justified.
- Need to demonstrate significant progress on stabilization.

Strengths and weaknesses

Strengths

- The program has shown excellent progress on a key concept which needs to be dealt with for Non-Nafion membranes ever to have a chance against Nafion.
- Strong fuel cell resource team experience and understanding. Collaboration with world class membrane materials source.
- Group plan for R&D is good- all aspects are being covered-solid plan.
- Polymer chemistry at VT - Jim McGrath is top notch.
- Facilities and capabilities at LANL.
- Approach should yield lower-cost materials if they give acceptable performance.
- Good understanding of failure mechanisms of alternative membrane.
- Should provide guidance in selecting still other membrane chemistries.

Weaknesses

- Group should not consider the electrode as a fixed entity - adapt it to the membrane.
- Interactions with real players in industry who could/would implement results in real products.
- Knowledge of real world issues in production and operation of fuel cell stacks in actual field conditions (this is especially important for new materials development and introduction).
- Cost is the primary motivation but little mention is given to cost projections for alternative materials. It seems that this should be considered from the beginning for any candidate material.
- No clear pathway to meet stabilization requirements and durability targets.
- Need to move beyond argument of "non-optimized" membrane as reason for instability.

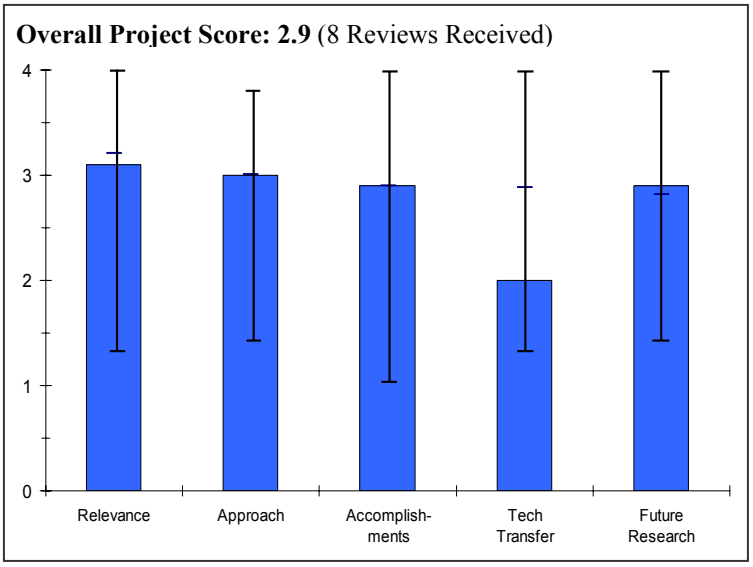
Specific recommendations and additions or deletions to the work scope

- Broaden the scope to involve stack developers.
- Publications in order to inform fuel cell community of work.
- Eventually the group needs to look at costs and manufacturability.
- This polymer has been around long enough and there has been enough work on it that it is time for the marketplace to decide whether it is viable. If it is commercially viable, let a commercial vendor license the rights to it, and bring out a product. I cannot recommend funding beyond the current cycle, not because the work is bad, but just because it's time to commercialize the material, and DOE/LANL are not the ones to do it
- Give more attention to cost projections for both Nafion-like materials as well as candidate alternatives, both produced in large volume.
- Optimize the membrane.
- Stop project or switch to other chemistries if significant progress toward stabilization isn't made within the next 6-12 months.

Project # FCP-05: Hydrocarbon Membrane
Cornelius, Christopher; Sandia National Laboratories

Brief Summary of Project

This Sandia National Laboratories (SNL) polymer electrolyte membrane (PEM) and catalyst coated membrane (CCM) development effort is an alternative approach to address the physical property limitations of perfluorinated PEM materials such as Nafion. Several alternative polymer electrolyte materials have demonstrated better fuel cell performance characteristics than Nafion. Polyphenylenes have the potential of being used in a PEM because of their excellent thermal and chemical stability and ability to form mechanically robust films. The chemistry afforded by the parent polyphenylene represents a system that has tunable chemical structure and properties.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Project has done an excellent job of identifying targets aligned to the HFCIT Multi-year Plan and the barriers it is trying to overcome.
- Good progress; many questions.
- High temperature, low relative humidity membranes critical to advancing DOE fuel cell goals.
- Key technology that must be enhanced.
- Important problems: high T, low RH.
- MEA-driven work presented—good.
- New ideas that are a little off-center.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The new membrane materials (SDAPP) certainly have promising characteristics for higher temperature operation. The observed results however are often as much a result of MEA interface details as the bulk component properties. The PI should consider focusing on improving one or two primary membrane properties for one version of the polymer, such as improved sulfonation and understanding water uptake per sulfonate group to try and improve the poor sensitivity to low RH. Then they should also study the electrode/membrane interface to understand how its deficiencies may be affecting the results and conclusions being drawn about the different SDAPP bulk properties.
- Project appears well organized and focused.
- Presentation of task and milestone schedule is appreciated.
- Very systematic studies; quite productive.
- Appears to be little challenge to the team, they know the results before executing the work.
- Testing is conducted at high temperature and low RH, unlike some other projects.
- Approach is lacking in compelling motivation as to why this even works.
- Catalyst layer work seems good but totally empirical.
- Idea seems flawed on the surface: weaker acid provides better catalyst layer? What is the improvement?

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.9** based on accomplishments.

- Not applicable, program is too new, based on dates provided, yet a significant amount of data is presented and shows promise for this membrane approach. It was not clear what was achieved under the period of the contract.
- Project just started in March '05 and is scheduled to last approximately 6 months.
- Project appears to be on track to task and milestone schedule (30% complete).
- Could use bit more "hard" data.
- For the short project period and low funding level, have shown significant progress and a possible pathway to high T, low RH membranes.
- Good to see attention being devoted to MEA fabrication.
- Need to measure conductivity of new membranes as a function of temperature and RH so that membrane properties may be considered separately from MEA advances. Endorse subtask 1.1.2.
- Moderately successful results, but good given the amount of time project has been underway.
- This year's technical accomplishments toward DOE goals appear to be minimal.
- Lags the field in important way.
- Making polymers but without apparent rationale.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- Working with an institution having electrode and MEA integration expertise would be beneficial. The goal should be to see what the best performance and durability can be with the new membrane system. This cannot be achieved by just varying the membrane component of the MEA.
- No partners involved in research, only SNL performing work.
- With short duration of project, significant collaboration may not be possible.
- Need to develop industrial contacts.
- Consider collaboration with other institution to do MEA durability tests.
- Too short a timeline to comment.
- Needs to expand collaboration to get more information on the 'why' of the effort.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- The proposed work appears to be too broad for the resources available. Extending to HPA additives before the basic polymer properties are optimized or understood should be reconsidered. The PI should consider taking the best polymer structure candidate to date and first trying to optimize the electrode structure and interfaces with it to try and achieve the best performance possible (and not assume that Nafion is the best – it is not) and study the issues that arise with the new membrane "system".
- Future work may lead to improved membrane performance, therefore increasing stack life.
- Need to have more aggressive challenges.
- The enhanced acidity and polymer structure/property approaches may be promising.
- Direction of future work seems logical.
- No clear path provided to achieve further gains, a result of lack of emphasis on the underlying rationale.

Strengths and weaknesses

Strengths
- Current project is a quick hit (6 months) keeping the scope manageable.
- Team is enthusiastic about work.
- Good three-way fundamental efforts.

- Good facilities for MEA characterization.
- Right subject matter.
- Good synthesis platform.
- All elements of team for testing are in place.
- New source of good ideas.

Weaknesses

- No collaboration with other organizations.
- Too small, too short of project to change the game.
- Needs more basic insight to emerge.

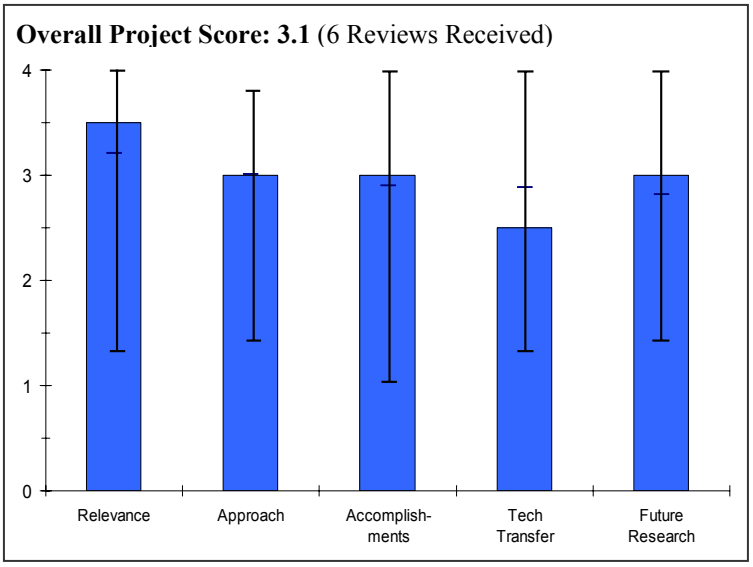
Specific recommendations and additions or deletions to the work scope

- Consider collaborating with others in future proposed work, particularly membrane producers.
- Either invest more or not continue program.
- Perversely, I would suggest that funding increase for this effort. The team is not bound by totally traditional approaches and could provide some routes to fresh ideas. Teaming should increase to obtain more detailed fundamental understanding.

Project # FCP-08: Cathode Electrocatalysis: Platinum Stability and Non-Platinum Catalysts
Myers, Debbie; Argonne National Laboratory

Brief Summary of Project

This Argonne National Laboratory (ANL) project will develop 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. ANL is also elucidating the rates and mechanisms of the degradation of PEMFC platinum electrode performance by characterizing the effects of electrolyte, potential (holding and cycling), time, and temperature.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Good basic research program to understand existing catalyst and explore new catalysts.
- Well-aligned.
- Non-platinum catalyst an important field of research. Evaluation of stability/dissolution of Pt at cycling conditions very relevant to the R&D goals.
- Completely relevant.
- Performance degradation of Pt and development of more active catalysts are important tasks for improving the fuel cell durability and efficiency.
- Project is modest but addresses critical point driving DOE decision making for next-generation membrane.
- Seeks to evaluate GM claim that was based on substantial extrapolation of almost no data.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Pt stability experiments done very carefully with strong combination of *in situ* and *ex situ* techniques. Balanced approach to non-Pt catalysts with solid criteria for contamination or abandonment of approaches.
- The project has two unrelated topics.
- Work on validation/evaluation of Pt solubility is great addition to the work plan. The extension to supported catalyst is very important.
- It is reasonable to try a diverse screening of many non-Pt potential catalysts.
- Standard procedures are proposed for the research and developments.
- Approach is a good start.
- Probably needs to find ways to do this with more realistic conditions next.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Elucidating Pt surface effects on dissolution behavior. Good comparison/correlation with experiments/theory of others-important results. Good initial results for Au-based systems. Solid decision to abandon carbide/nitride approach.
- Pt dissolution work is good.
- Good progress. Benchmark studies in the area of Pt and alloy catalysts stability is very important topic.
- Pt electrode degradation mechanism elucidation is valuable.
- Screening large number of non-Pt catalysis based on Au is no guarantee that one of the proposed catalyst formulations will work.
- Extrapolations of the present corrosion results for real systems are required.
- Too poor activity of the non-platinum catalysts, compared to that of Pt under the same conditions.
- Very important results coming from a modest effort.
- Key findings pointing to potential red herring from GM analysis.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- Says "will collaborate with LANL"- not clear if it's happening.
- Good start. Could be expanded.
- Response to last year comment about partnering with someone with more MEA experience is inadequate.
- See no evidence that the claimed LANL collaboration is underway.
- The studies are of fundamental nature and no collaborations were required.
- Needs to expand collaboration somewhat but they are just getting started.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Solid plan based on prior results. Curious to see initial results for metal centers on polymer backbone.
- It seems the project will be better focused.
- Testing in MEA is critical before going too far and synthesizing multiple samples. It is not clear what the focus of the work on bimetallic Au-Me/C or /TiO_2 is? Are alloys formed and targeted, why such low loadings?
- Success is not guaranteed.
- The reason for the choice of the new catalysts was not evident.
- Definite appreciation by PI for next directions.
- This could still be much more.

Strengths and weaknesses

Strengths
- Well-defined milestones and good progress.
- Benchmark studies such as the Pt dissolution are a good addition to the work plan.
- Payoff is high if ANL identifies non-Pt catalyst.
- Reasonable results for the Au-based catalysts, compared to other non-platinum catalysts.
- Very important problem.
- It's great that these workers will be generating key data.
- Competent group to assess key point.

Weaknesses

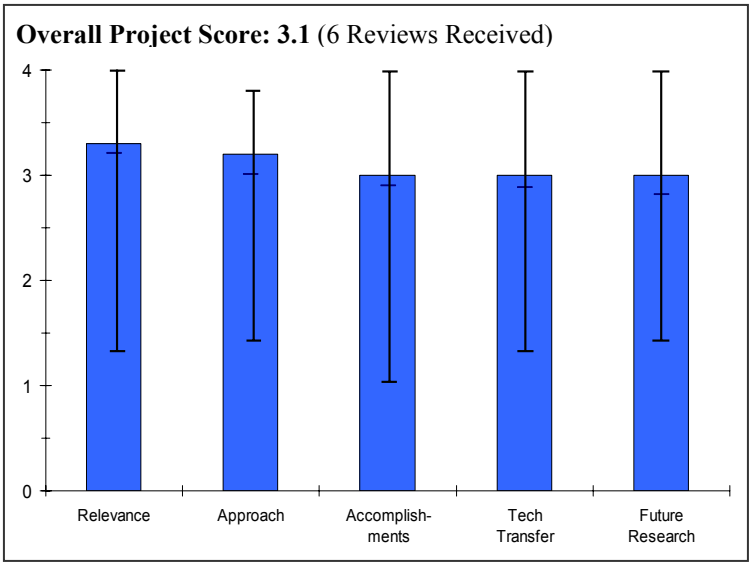
- Split effort on two projects (Pt stability and non-Pt catalyst) could hamper progress on non-Pt catalysts efforts by limiting range of experiments. Need to pull in other characterization techniques to better understand behavior of bimetallic systems (microscopy, etc.).
- The work on bimetallic (Au-based), transition metal carbides and nitrides, and metal centers attached to electron-conducting polymer backbone is too broad area for such relatively small project.
- Shotgun approach doesn't guarantee success.
- Trial and error method seems to be used for catalyst choice.
- Needs to expand approach to include more realism.

Specific recommendations and additions or deletions to the work scope

- Before you pick one or two of the above areas (plus the benchmark studies) and expand the research in 2, instead of all 4 types of material.
- Add LANL as collaboration partner and show connections at next review.
- This activity really demands additional support. The catalyst durability aspects truly are among the most important problems and this group shows immediate results in debunking!
- Project should expand to include drier conditions and polymer electrolyte interface.

Project # FCP-11: Modeling and Control of an SOFC APU
Khaleel, Mo; Pacific Northwest National Laboratory

Brief Summary of Project



This Pacific Northwest National Laboratory (PNNL) project provides SOFC-based APU development with control algorithms to optimize fuel efficiency and operating life, and models and experiments for stack response and structural failure under dynamic loading. Controls work includes developing dynamic system models, determining typical APU usage patterns, collecting electrical usage data from a working truck, and designing control algorithms to optimize fuel efficiency and stack operating life. Shock and vibration tasks are identifying failure modes under characteristic dynamic loading, determining material behavior under dynamic loading, determining guidelines for durable SOFC/APU systems, measuring truck excitations, experimentally validating the models, and defining requirements for APU isolation.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- APUs represent an early application of fuel cell technology where fuel cells have important customer value-added features. SOFC technology is attractive because of its high stack efficiency and because of its direct operation potential without the need for an external reformer. Such applications are important for the development of a fuel cell manufacturing industry in the foreseeable future and will lay the ground for the eventual transition to a hydrogen economy.
- System control and seal durability for SOFC are important issues for successful fuel cell APU commercialization
- Addresses DOE desire to develop fuel cell based APU's for heavy duty trucks.
- Areas for heavy trucks are not directly part of Hydrogen Program (RD&D plan objective). There is a more appropriate DOE program those objectives of the project are suitable.
- The work is closely coupled and complimentary to the DOE FE SECA work and critical to the development of SOFC APU systems and application of SOFC to shock and vibration environments.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Two important issues with SOFC APUs are being addressed. Project aims at using a modeling approach to: develop new algorithms that will optimize fuel efficiency and system operating life tuned to typical APU usage loads and forecast the impacts that shock and vibration could have to damage real life SOFCs.
- Models will be validated with real data from working trucks.
- The simplistic, invalidated model for SOFC is only a first approximation of real devices.
- Lack of cooperation from partners has significantly hurt the progress of the project.
- The work on the SOFC cell seals is basic and not correlated to real world data - they really don't know the shock and vibration profile to design to 2.5 years into the project.
- The project is appropriately designed and feasible.

- In this period the PI considered the key parameters to include whole system in the model (weakness pointed out in the last evaluation).
- The shock and vibration work needs to be heavily focused on coupling input frequency spectrum with natural frequency and damping characteristics of the SOFC. More focus on this area is suggested. The approach appears to be more heavily weighted to a deterministic stress analysis approach versus an input frequency/resonance coupling approach. The seal failure work is an example. With the exception of these comments the approach is good and clearly there has been some amount of frequency/resonance work, it requires more emphasis.
- This team is the best possible team to work on this effort. PNNL has developed models under SEIA which greatly leverages DOE's EERE investment.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Excellent progress made towards developing working models for: Systems and Control Analysis of stack and APU load including electrochemistry and thermal transient response to an APU electric load.
- Shock and vibrational impacts on glass ceramic seals, crack growth at seal/metal interfaces. Models also include thermal stress mismatch at electrolyte/ electrode interface.
- From responses to last year reviewers' comment, it's clear that this team has developed a very detailed and robust model.
- The project is far behind and not likely to be able to meet its deadlines.
- Major elements of the control system side of the project are missing (the power electronics models) and the simplistic lumped capacitance SOFC model constructed is invalidated with no apparent way to do so.
- The approach for developing a control strategy for the APU system is simplistic, incomplete, and flawed. Transient operation is not considered, and the project is far away from developing a system that can improve vehicle efficiency when the truck is moving through component electrification.
- Basic data need to successfully complete the SOFC seal part of the project is not in hand and work so far has been to apply standard statistical analysis approaches that have limited usefulness to SOFC developers.
- The weaknesses pointed out previously are progressively being solved. No information about the cost.
- The operational performance of an SOFC has received considerable attention in this work, i.e., electrochemical performance, lumped thermal model along with some amount of stress based failure, e.g. seal failure. This requires parameterization in a dynamic model; the dynamic model methodology requires more emphasis to ensure that generated data fits the analysis scheme. The progress has been good and the work excellent; the focus may have overemphasized ancillary issues.
- Developed lumped parameter dynamic model for SOFC for use in control and vibrational analysis.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Effective interactions with Delphi, a leader in SOFCs and auto component developments and PACCAR, a leading truck maker. Interactions with GE could lead to important applications to aerospace SOFC APUs now under development.
- Five publications/presentations.
- Contact and participation with collaborators very poor, and it has significantly hurt the project.
- Needed data for both aspects of the project has not been forthcoming and the relevance and usefulness of the project has been severely compromised.
- There are interactions with university and industry but the technology transfer is not clear in the presentation. No interaction with other PNNL project group was detected.
- PACCAR, University of Illinois/Chicago, Delphi, GE.
- Excellent Collaboration.
- Coordinating with SECA, PACCAR, Delphi, and University of Illinois.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Focus is good on making models more robust and on validation on real truck data.
- The team knows what it needs to do but can not practically accomplish it in the time left for this project.
- There is no indication that the fundamental problems with this project can be or will be addressed.
- If the project run smoothly, the planning is adequate. No optional path is presented.
- More emphasis is required to develop the analytical framework in a global sense rather than detailed failure modes such as seals or essentially existing performance electrochemical and lumped heat transfer models. The methodology for an input frequency/resonance/damping model must be developed first so that collected data is in the correct form for application in the model. Detailed non-dynamic stress based failure modes is a focus of the SECA program. It is unlikely stress based failure will be predicted dynamically, the dynamic analysis will require a frequency analysis. Dynamic load factors require development for application to steady state stress analysis when done.
- Future work plan will complete the tying together of SOFC and BOP dynamic models. Vibrational analysis on entire APU then will be completed including stress determination in the stacks/cells.
- Would be good to experimentally validate the models or at least parts of the models.

Strengths and weaknesses

Strengths
- Addressing important issues associated with an early application of FC technology with modeling. Truck APU Fuel cells have important customer value-added features.
- Excellent progress made towards developing detailed robust working models for: systems and Control Analysis of stack and APU load including electrochemistry and thermal transient response to an APU electric load and shock and vibrational impacts.
- Models to be validated on working truck data with major auto supplier and truck maker.
- From responses to last year reviewers' comment, it's clear that this team has developed a very detailed and robust model.
- Understanding of problems associated with SOFC is good.
- Excellent collaboration team.
- Excellent relevance of the topic area.
- Building of MARC stress model for task 2 involving vibration. Coordinating with University of Illinois, SECA Dynamic modeling efforts for BOP. Obtaining vibrational input information from PACCAR.

Weaknesses
- The control system modeling is too simplistic, not based in reality, and will likely not be useful to a truck manufacturer.
- The SOFC seal element is not developing any new insights, is not based on the real needs of a truck application (no real data yet), and is not likely to be of much value to a SOFC manufacturer.
- This project is superfluous given the two other SOFC APU projects with truck manufacturers building and demonstrating working applications.
- The relation with Hydrogen Program.
- More focus required on the development of the general analytical framework.
- More focus on a dynamic frequency based model coupling input frequency, resonance and damping as well as determine dynamic load factors.

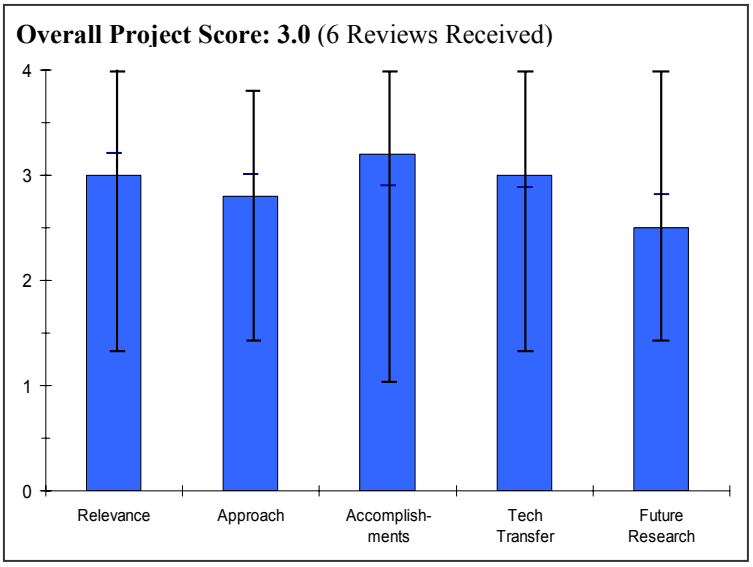
Specific recommendations and additions or deletions to the work scope

- Recommend termination of this project.
- Duplicative with projects FCP 30 and 31 with truck manufacturers.
- No unique or valuable insights likely from this work.
- Specify the handling of the exhausted gas.
- Specification of SOFC type (reforming diesel, direct diesel oxidation, etc) and consider it in the system model.
- Safety code, standards, etc.
- There are two SOFC APU projects for heavy duty truck applications which were recently awarded by the HFCIT Program. PNNL should attempt to collaborate with these projects.

Project # FCP-12: Corrosion Protection of Metallic Bipolar Plates for Fuel Cells
Turner, John; National Renewable Energy Laboratory

Brief Summary of Project

This project at the National Renewable Energy Laboratory (NREL) is concerned with the identification and characterization of metal alloys and coating 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.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Low-cost bipolar plates with required durability are needed for PEM fuel cells to meet DOE cost and performance targets.
- Addresses barriers of stack cost and durability.
- Metal plates with rugged surface treatments may offer manufacturability advantages over carbon plates.
- Claim cost reduction, but will metallic plates really be cheaper than carbon?
- Based on raw material costs, not an obvious benefit to metal, especially if need special alloys or surface treatments that add cost.
- Manufacturing processes (e.g., stamping) do already exist, but molding of carbon composites is also available.
- Has anyone done cradle-to-grave comparison?
- Size is a benefit, due to superior metals strength.
- Narrow focus on one aspect of reduced stack costs.
- Research is focused towards meeting performance requirements of HFCIT MYPP. Metallic bipolar plates offer a number of inherent advantages.
- Project is well directed towards development of low-resistance, low-corrosion coatings on affordable materials.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- A logical, systematic approach has been developed to identify metal alloys with and without corrosion protection coating that have required durability at acceptable costs.
- This line of research necessitates more perspiration than inspiration (to use Edison's distinction), and the presenters have been diligent in acquiring needed data.
- Measuring corrosion currents and ICR is good.
- Measuring ion concentration in PEMFC (slide 17) is good.
- Do not understand why coating process used by PV industry is considered to be "low cost" although alloys tested (e.g. SnO_2) are low cost.
- Why not show Sn concentration in slide 17?

- Why not compare ionic conductivity of membrane before and after exposure in slide 17?
- Why not include carbon-plate baseline in slide 17?
- Why is PEMFC exposure time just 7.5 hr?
- Why not conduct potential-cycle testing?
- Well thought out approach to addressing corrosion protection.
- Phosphoric acid environment is a reasonable guess of yet unknown high temperature PEM fuel cell environment.
- Project is focusing on resistance and corrosion barriers to metallic bipolar plates. Project would benefit from a more structured approach as to which alloys will be targeted and why. Project may be a little too broad looking at nitride alloys, coatings, and high temperature operation given modest funding levels.
- Generally good approach towards the problem. Has adequate care been taken to ensure that the ferritic stainless alloys proposed here are sufficiently ductile to be stamped into bipolar plates with all of the necessary channels? It's well and good to develop a coating with good corrosion properties, but if the material can't be economically formed, the good corrosion results would be for naught. The old DOE target table entry of 16 $\mu A/cm^2$ corrosion current should be thoroughly expunged - at least temporarily, it should be replaced with 1 $\mu A/cm^2$. Have the economics of the thermal nitriding processes been adequately looked into?

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Good progress has been made on this project including performance and corrosion testing. The project is moving on to identify plate materials and coating suitable for higher PEM fuel cell operating temperature which may place new durability requirements on bipolar plates.
- Data shows trends towards DOE goals; too early to tell when diminishing returns will be reached.
- Good results to date, especially for funding received to date.
- Relatively new start (2004) but has quantified performance of a number of alloys. Research progress is measured against performance indicators. Project lacking in projected cost data for various alloys and coatings. Project should conduct cycling tests to be more representative of automotive cycles and extend duration of corrosion tests.
- Work with more affordable substrates seems encouraging, but the stampability of the AISI446 should be double-checked. The corrosion behavior after nitriding, while improved, still doesn't seem stellar. It looks like some effort has gone into characterizing the true nature of the protective films (oxy-nitride) but more thorough work along these lines, correlated with longer-term corrosion data, would give more confidence in the level of durability that could be expected from these materials.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Two National Laboratories are collaborating on this project and an industrial partner is involved in the fuel cell testing of prototype bipolar plates.
- An industrial partner interested in commercializing the developed bipolar plates is needed.
- Collaboration between NREL and ORNL provides some advantages.
- Project has significant publication record in journals, conferences, and a patent disclosure.
- Good; already working with both stationary and transportation fuel cell OEMs (e.g., Plug Power and GM, respectively).
- Numerous papers and presentations.
- Relatively little collaboration only ORNL and Plug Power. Not clear as to exactly what NREL's role is with regards to evaluating nitrided alloys - ORNL has been doing this for several years.
- Good collaboration between two National Labs. Looks like a productive interaction with Plug Power may give some results useful for stationary, but with the danger of diverting efforts away from materials useful for automotive applications.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- Plans for completing this initial phase of the project appear to be adequate and reasonable.
- This project is intended to investigate one plate strategy, so contingencies and off-ramps are not much an issue.
- Many good ideas remain to be pursued, and the presenters intend to pursue them.
- Not especially clear, especially for a program that is at this level of completion.
- Recommend conducting testing with repeated potential cycling, since this is realistic for transportation applications, and this could be a real issue for these materials (i.e., analogous to Pt dissolution with potential cycling).
- Proposed timeline and long term focus for future not laid out. Clear decision points not established.
- Excessive emphasis on high temperature. Better focuses would be on durability under normal operating conditions of a material that can be readily formed.

Strengths and weaknesses

Strengths
- The national laboratory collaboration in this project is impressive utilizing the strengths of both labs.
- Testing a variety of materials with different treatment approaches and compositions.
- Focusing on direct need for low cost, high performance bipolar plates. Has quantified performance (resistance and corrosion tolerance) of a number of alloys is relatively short time.
- Some successes in corrosion protection, through nitriding, of iron-based alloys.

Weaknesses
- Could more clearly explain justification for these materials, and the processes required, and why they are projected to be low cost?
- Could do more analysis and testing of materials tested.
- Could provide theoretical justification(s) for the materials and coatings that are being tested (e.g. why is it though that they will be more stable under the relevant conditions?).
- Could benefit from a more structured approach for determining potential future alloy candidates. Project is not very clear on future cost projections. Project may be too broad (nitriding, coatings, high temperature operation) at this point.
- Not all the corrosion rates are all that low. Excessive emphasis on high temperatures and on an electrolyte that throws away all but one-third of the activity of Pt for ORR.
- Make sure materials are processable before investing a huge amount of time on corrosion protection.

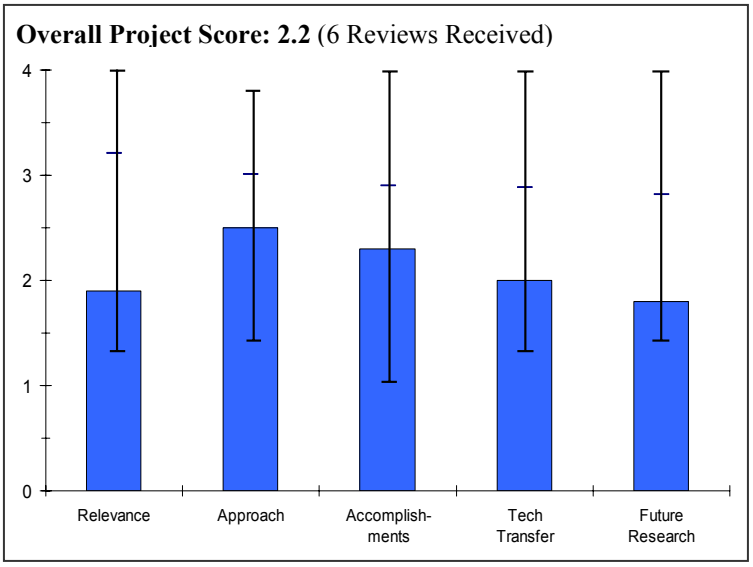
Specific recommendations and additions or deletions to the work scope

- Identify and involve a commercialization partner.
- Include repeated potential-cycling testing to simulate real-world conditions!
- DOE should consider undertaking a cradle-to-grave comparison of metal and carbon bipolar plates to obtain a better understanding of the potential benefits (e.g. which takes less energy to produce or recycle?). The auto companies certainly have a bias towards metal plates, but other than the strength/size benefit is this bias really justified or is it primarily due to the auto industry's familiarity with metal and metal processes?
- More clearly define role of ORNL and NREL with regards to nitriding. May be beneficial for NREL to focus on coatings. High temperature operating regime not clearly defined yet therefore may be best to delay this area of work. Additionally, setting operating regime at 150-170°C seems high - 120-140°C may be more appropriate.
- Avoid excessive emphasis on the polyphosphoric acid >150°C environment. Wasn't enough materials development work done on that back in the PAFC days? This topic is of possible interest only for stationary. Can one innovate to cut down the costs of nitriding?

Project # FCP-14: Fuel Processors for PEM Fuel Cells
Thompson, Levi; University of Michigan

Brief Summary of Project

The overall goal of the University of Michigan project is to discover and develop new material and reactor concepts that will enable the production of low-cost, high-efficiency systems to convert hydrocarbons like gasoline into hydrogen. These new materials will include high capacity sulfur adsorbents for liquid fuels, and high activity and durable autothermal reforming, water gas shift and preferential oxidation catalysts. Thermally integrated microsystem-based fuel processors incorporating these materials will be demonstrated and evaluated against key DOE performance and cost targets.



Question 1: Relevance to overall DOE objectives

This project earned a score of **1.9** for its relevance to DOE objectives.

- On-board fuel reforming had no-go decision. This project does not look like a cost-effective method for stationary-distributed hydrogen generation.
- This project lays out DOE technical targets for fuel processors, but as was evident from last year's review comments, it still is not clear what technical targets are being addressed in this project. The message has to be better communicated in both the written slides and presentation.
- The decision to eliminate on-board reforming limited the relevance of this program.
- The relevance of the project to the DOE program objectives decreased when a no-go decision on on-board fuel processing was made. However, some of this technology may well migrate to stationary fuel cell applications.
- Gasoline reforming is no longer a priority.
- The project does not have path forward for meeting current hydrogen fuel purity specifications.
- Reformer design is more appropriate for on-board reforming.

Question 2: Approach to performing the research and development

This project was rated **2.5** on its approach.

- The low-cost material approach to sulfur adsorbents for liquid fuels, and catalysts is excellent.
- Plate reactor systems do not appear to be able to come close to fuel processor start-up time and energy targets.
- Again details are missing as to the particular approach being used to develop high performance, low cost materials. Was this just going to be a vendor engineering project to selecting catalysts and then just test? Or was there a specific technical approach used as to what materials were focused on? What methodology was used to select/ rationalize cost effectiveness of catalyst candidates? A poster effort was described as development of catalyst materials, but approach to doing this is ambiguous. Was this just the usual suspects?
- One of the objectives is to develop low-cost materials that are also high performance. The approach to investigate individual components of the reformer rather than an integrated system is sound. That said, at some point an integrated design must be developed because the assembled parts do not appear to be well integrated with each other.
- The approach was reasonable at the time the project was initiated.
- Smaller, integrated units are also desirable for stationary systems.
- Catalyst and sulfur sorbent work is relevant to DOE goals.

- Difficult to compare autothermal reformer and water gas shift catalyst performance to current industry standards due to novel reactor design.
- Selective oxidation of CO cannot meet current DOE CO specifications.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- O/C ratio of one is too high for high efficiency system.
- Adding precious metal (Au and/or Pt for water gas shift) is not a low cost approach.
- The PrOx catalytic wash coat seems to be far away from what is required and where other systems are at.
- Autothermal reformer non-noble metal catalysts look to have promise -- should do more development, testing and evaluate durability.
- A quantitative improvement in catalyst performance is not clear from the data presented. The activity of the autothermal reformer, water gas shift, PrOx is claimed to be enhanced, but by how much and against what standard? How is the 78-80% energy efficiency goal of fuel processors to be impacted by this work? The fuel to which this work is applicable is not clear. Since a microvaporizer was designed and demonstrated, this implies a liquid fuel. Why?
- Micro-channel reactors were described as being off-the-shelf??
- The program has achieved reasonable improvements but falls short of main targets (SU energy, SU time, etc).
- The program is ending. The University of Michigan autothermal reformer catalyst performs as well as the Sud-Chemie catalyst, at least over the first 10 hours of operation. University of Michigan is still having difficulty in bringing the CO concentration down to 10 ppm at the PrOx outlet.
- Predictive model was developed.
- There is no evidence of significant cost, performance or durability advantage.
- Amount of work performed has been good, but is in the wrong direction.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- Interactions and tech transfer are limited.
- Publication record is good.
- Sud Chemie and Osram Sylvania are mentioned, but this interaction does not appear robust from the data presented.
- Program could benefit from collaborating with a fuel cell developer.
- Collaboration with other organizations appears limited. University of Michigan should have more interactions with the OEMs given the proximity to Detroit. There has been some interest by catalyst developers in select results.
- There is little apparent interaction except with catalyst suppliers.
- University of Michigan needs to get industrial and DOE feedback to prevent continuing down wrong path.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.8** for proposed future work.

- Plans are to build a 1 kW system in FY06, but will not lead to results that help anyone.
- The project should concentrate on fundamentals.
- The goals are good, but deficiencies up to this point in communicating results clearly need remedying. Clear, quantitative improvements need to be demonstrated.
- For stationary systems, microreactors have limited benefits.
- The project is finishing this calendar year. A 1-kW demonstration is planned.
- Not applicable, since project will apparently not continue beyond 2005.

- University of Michigan must incorporate path to reaching DOE hydrogen quality specifications.
- University of Michigan must compare catalyst formulations to currently available commercial catalysts.

Strengths and weaknesses

Strengths
- Catalyst development, specifically non-precious metal catalysts and testing, appears to be the strength of this program.
- Enhanced performance of autothermal reformer, water gas shift, and PrOx catalysts is claimed.
- Microsystem fuel processor design is interesting.
- Initial approach.
- There is a large group of collaborators with expertise in multiple disciplines.

Weaknesses
- Catalyst durability is not established.
- System efficiency is ~ 68% and 51%. This efficiency is too low for nearly all applications.
- It is still not clear what technical targets are being addressed in this project. Criticism on goal ambiguity of last year is still evident.
- Particular approach being used to develop high performance, low cost materials is not clear.
- Quantitative improvements in catalyst performance are not clear from the data presented.
- How is the 78-80% energy efficiency goal of fuel processors to be impacted by this work (needs better rationalization)?
- Message and impact has to be more effectively communicated in both the written slides and the presentation
- The overall approach has not demonstrated substantial improvement in the technology.
- It is difficult to see this reformer meeting the DOE weight targets. The poster did not address costs even though that is stated to be one of the project objectives.
- There is too little concern with durability and cost.
- University of Michigan has not adapted to change in emphasis away from on-board reforming to high purity hydrogen.
- University of Michigan needs industrial input.

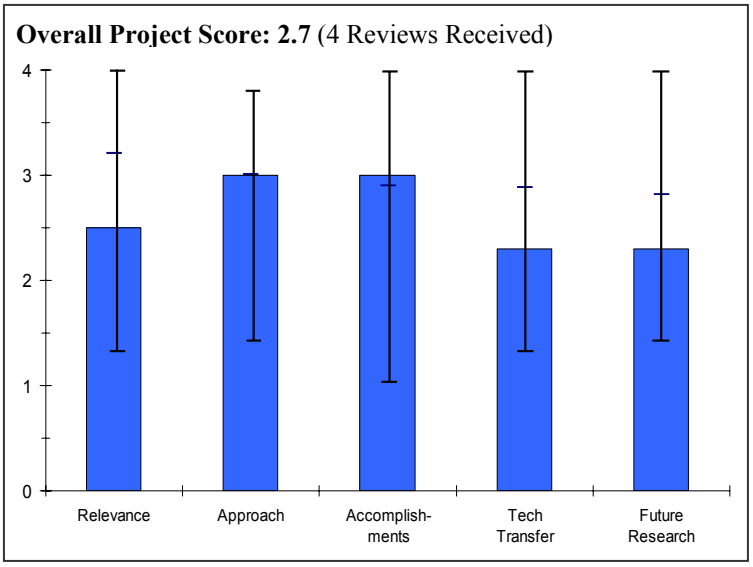
Specific recommendations and additions or deletions to the work scope

- I think this project would make better progress in concentrating on high performance autothermal reformer catalysts and examining catalyst durability, rather than building a 1 kW system.
- An integrated system design should be started as soon as possible. It doesn't appear that this will be done until the last quarter of the project when the 1 kW prototype is made.
- Focus on catalyst development and compare new and commercial catalysts for activity, specificity, durability, and likely failure modes.
- Drop PrOx from reactor design and consider how to do energy integration with purification methods such as hydrogen selective membranes or pressure swing absorption.

Project # FCP-15: Plate-Based Fuel Processing System
Yee, David; Catalytica Energy Systems, Inc.

Brief Summary of Project

Catalytica Energy Systems, Inc. is conducting this project to develop new catalytic reactor designs and reactor technology for processing hydrocarbon fuels to PEM quality hydrogen through developing advanced catalyst materials compatible with these reactor systems; designing and fabricating prototype steam reforming units in the 2–5 kW(e) scale; demonstrating steady-state and transient performance; and evaluating rapid start-up performance.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- On-board fuel reforming has no-go decision. This project does not look like cost-effective method for stationary-distributed hydrogen generation.
- The decision to eliminate on-board reforming limits the relevance of the program.
- The reforming is very important component of fuel cell.
- More relevant for transportation systems but also possibly for stationary.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Plate reactor systems do not appear to be able to come close to fuel processor start-up time and energy targets.
- The objectives and approach are well defined.
- The doer really knows the problems, methodology and systematically approaches the goal.
- Such novel approaches needed.
- Seems reasonably well thought-out and executed.
- Potential scale-up and cost issues not considered.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Good reduction in T-operation and S/R ratio.
- Will not meet targets for on-board reforming.
- By utilizing existing HCAT exchanger plates, the su times and energy was substantially above targets (su-start up).
- The reformer catalysts show 10-times improvements. Cost is going down several times.
- Progressive is impressive.
- Proven advantages in CO reduction.
- Potential (not proven) advantages in start time.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- The doer is practically doing everything by itself. The results are not shared or disseminated.
- Little evidence of significant collaboration or technology transfer.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Program closing out.
- This is the end of the project due to the no go decision even though there might be good potential for products.
- Little detail for future plans.
- Need to be more specific than "other possible applications"

Strengths and weaknesses

Strengths
- Strong theoretical and practical know-how. The doer is doing already business and R&D with catalysts and reformers.
- Novel approach.
- Apparent good understanding of catalyst issues.
- Potential to make steam reforming more practical, especially on a small scale.

Weaknesses
- Improved catalyst durability (~ 40 hrs) a long way from what is required.
- Oxidative regeneration of WGS catalyst will be difficult for a commercial application.
- Program does not include methods to eliminating sulfur compounds or other impurities. Program does not include a detailed cost analysis.
- Relatively small company doing everything by itself.
- Primarily directed towards transportation.
- Need more consideration of durability and cost issues.

Specific recommendations and additions or deletions to the work scope

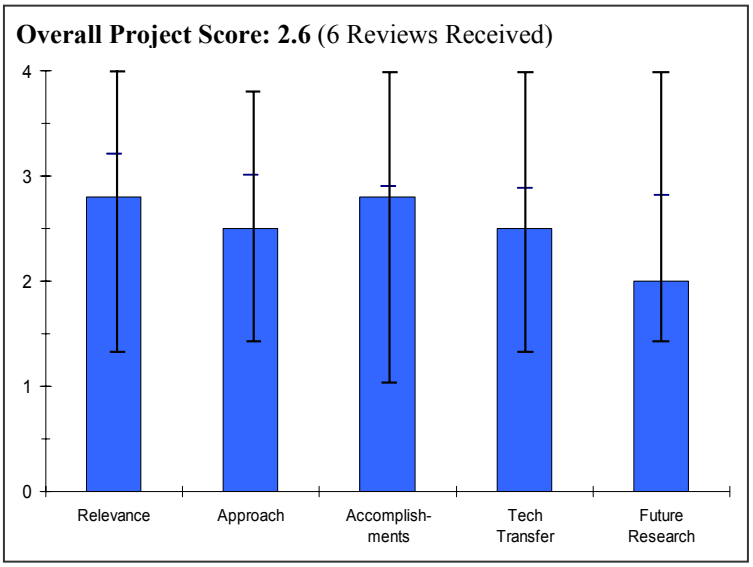
- If applications for ONR and TARDEC exist, they should fund program, not EERE. Does not appear to fit into EERE program.
- This project could have good potential for a good and relatively small and cheap reformer.
- Concentrate more effort to a similarly novel approach to PROX.

Project # FCP-16: Application of Advanced CAE Methods for Quality and Durability of Fuel Cell Components

Kelly, Ken; National Renewable Energy Laboratory

Brief Summary of Project

This project will work directly with fuel cell industry leaders to accelerate implementation of fuel cell technologies by integrating the latest computer-aided engineering (CAE) methods with advanced design techniques to overcome key manufacturing and technical barriers to improve quality, reliability, and durability of fuel cell components. Over the past two years, the National Renewable Engineering Laboratory (NREL) has worked with fuel cell industry representatives to successfully apply advanced CAE techniques to address specific fuel cell design hurdles. The application of these techniques has led to a number of design innovations and helped to improve product development time by reducing the number of component prototypes.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Excellent. Computer aided engineering (CAE) modeling project targets is improving durability, life-time costs of PEM fuel cells. Such analysis is now used in other industries to shorten product development lifetimes.
- This analytical work should be helpful to developers for cell and stack improvements.
- This work is supposed to improve cost and durability, but no evidence of significant impact on these barriers was presented.
- How does demonstrating that CAE is useful in designing components, which is already known, help us meet the DOE targets?
- Manufacturing variability will be an important issue when fuel cell designs are fixed.
- This project appears to be application of modern, but not to-the-purpose-developed, tools to the solution of specific developmental problems. It is not clear how this falls within the mission of government-sponsored-work as pre-competitive advanced research.
- It seems questionable whether the determination of proper bolt patterns for a specific developer's product is a proper use of scarce and expensive national lab resources. If p. 12's "innovative design concepts" could be more completely explained, perhaps the research content would be clearer.

Question 2: Approach to performing the research and development

This project was rated **2.5** on its approach.

- The approach is good for shrinking design times and achieving optimal architectures. NREWL uses CAE to develop robust designs of fuel cell components that take into account variations in material properties, loads and manufacturing processes. The reviewer is not sure that the dynamic variations of physical or chemical properties inside an operating stack can be taken into account for this optimization.
- Many of the important cell and stack parameters seal design, flow field design and the compressive forces needed for good performance and endurance are being addressed.

- This work is not generically applicable (i.e. it is not fundamental) and therefore does not address the barriers in a universally applicable way.
- Results appear to be limited to the optimization of specific designs and are not useful to others.
- At best, this project just demonstrates that CAE is a useful tool for designing complex products, which is widely known.
- CAE is approach vital to reduce costs.
- The project consists of many unrelated modeling efforts.
- Coupling manufacturing variability data with PEM is a good approach to characterize performance impact.
- P. 5, "Approach" lists the tools that were "utilized" (not developed) in this work. If new tools with unique functionality were actually being developed, then one could better argue that this work lies within the mission of the national labs.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- With a relatively small amount of money, this project has already gotten results on best sealing designs and designs that minimize crack formation of BSPs during manufacturing. NREL is on the cutting edge with results on Plug Power high temperature stack.
- It appears the results obtained in stress analysis have helped a developer improve PEMFC performance.
- Progress, as expected, is limited to the optimization of specific designs with no real and new generic learning that can contribute to the optimization of other designs.
- NREL provided gasket failure and sealing recommendations used by Plug Power.
- The presented work is more data collection intensive than technology intensive. The techniques required for the analysis are well established. Progress is good with respect to analysis development and collection of a minimum level of data.
- The work appears to show a high level of technical competence.
- The customers for the individual subprojects were likely very pleased to have their detailed design work done for them.
- The use of modern design tools was taught to some of a customer's engineers.
- The work advances details of particular pieces of hardware and improves the design skills of developers who have sought such aid. It is not clear that it makes significant progress towards general advancement of the fuel cell field.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.5** for technology transfer and collaboration.

- NREL has very effective interaction with Plug Power on next generation, high-temperature system. NREL conducted 2-day workshops on CAE Methods to get word out. Dispersion of this knowledge will help lower lifetime costs of fuel cell systems as manufacturing emerges for this industry.
- More developers other than Plug Power need to be involved.
- Interaction is limited to Plug Power, to date.
- There is no evidence that significant interaction with others is planned.
- There is plenty of National Lab/University collaboration.
- NREL is working, so far, with only Plug Power.
- Get more developer contacts to ensure that the CAE model gets used.
- Hydrogen storage safety plans should be reviewed with H_2 storage tech team and with Codes and Standards Tech Team.
- Work appears focused on a single company. Collaboration should be expanded.
- Full coordination exists, but the lab is doing industrial firefighting and training in advanced firefighting.
- Technology transfer from federal programs generally should be at the verified-concept level, not at the perfected-detail-of-design level.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- This relatively small project should maintain a focus on fuel cell issues and not scatter its resources working on hydrogen storage at this point.
- This work should concentrate on cell and stack designs to improve performance and life.
- Plan is to just continue aiding Plug Power with optimization of specific components.
- NREL provided no evidence that generically-applicable learning will result from this work.
- Project is too unfocused for amount of funding it receives.
- Pick 1-3 areas for CAE, and do those.
- NREL is trying to cover all areas with only $200K funding.
- The effort should focus more on establishing functional/parametric relationships between assembly/manufacturing & performance. The technique presented will be useful only if very precise relationships are developed between the assembly/manufacturing and its impact on the critical fuel cell performance and reliability issues. The future work appears focused on continued analysis methodology development and creation of assembly/manufacturing distributions versus focus on the performance and reliability relationships.
- Future plans look like more of the same. The apparently considerable skills of the project's participants should be brought into play at a higher level of abstraction – to identify broad design concepts with general utility or to develop (not just apply) innovative design tools with unique general functionality.

Strengths and weaknesses

Strengths
- This presentation shows excellent development of computer aided engineering (CAE) modeling to improve durability and life-time costs of PEM fuel cells. Such analysis is now used in other industries to shorten product development lifetimes.
- The approach is good for shrinking design times and achieving optimal architectures. NREL uses CAE to develop robust designs of fuel cell components that takes into account variations in material properties, loads and manufacturing processes.
- Project has already gotten results on best sealing designs and of designs that minimize crack formation of BSPs during manufacturing.
- The project is on cutting edge with results on Plug Power high temperature stack.
- Methodology is being dispersed through in-depth training courses.
- NREL completed a decent amount of design work, for Plug Power's benefit, with funding level available.
- CAE modeling is valuable.
- Relationship with Plug Power is vital to ensure that the model and its results get implemented.
- The analysis methodology is being developed.
- The project participants appear to be very skilled at bringing computer design tools to bear on specific design problems.

Weaknesses
- The small project should maintain a focus on fuel cell issues and not scatter its resources working on hydrogen storage at this point.
- Can dynamic variations of physical or chemical properties be taken into account for this optimization that result from operation inside a stack?
- This appears to be an excellent example of "corporate welfare," since only one company (Plug Power) is benefiting from this government-funded program.
- Unfocused. NREL needs to pick 2-3 areas for deeper analysis.
- Collaboration with industry is not sufficiently expansive.

- Work does not focus on the most important issue, functional/parametric relationships between assembly/manufacturing and performance/reliability, it is focused on providing an analysis methodology which doesn't represent a significant advance in technology (i.e., the analysis methodology is available without a significant effort to fit it to the application).
- The project has not been set up to broaden the conceptual basis through which the national labs can best contribute to the technical and economic advancement of the nation.

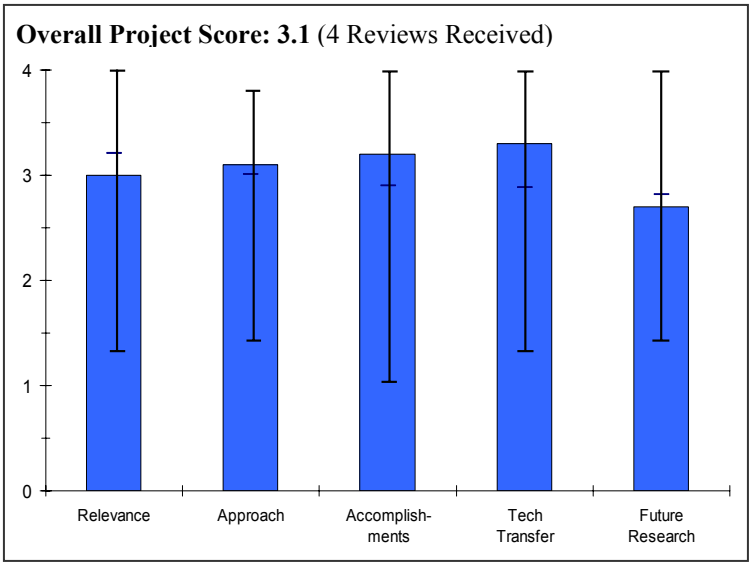
Specific recommendations and additions or deletions to the work scope

- The small budget for this project should be increased based on good results obtained so far.
- Focus on cell and stack designs and configuration. Involve more developers.
- In lieu of continuing the "subsidized-design work" as planned, ask the PI to produce a report that explains what generically-applicable learning's have been generated to date. If generic learning has resulted, then these should be put in a format that can be utilized by the industry (e.g., a paper, presentation, or design manual). This documentation is the best hope of generating something that is universally useful from this work. Before continuing additional design work, an outline of the generic learning that might result from this work should be provided. If there are no generic learning offered, then stop this work.
- NREL should participate in upcoming fuel cell manufacturing workshop and use that as a guide to focus and reduce the number of different modeling tasks planned for FY06.
- Expand industry collaboration and focus on establishing the relationship between assembly/manufacturing and performance/reliability.
- A national lab is not a parts design shop. Pursue a concept rather than a part. At a minimum, distill design rules with general applicability out of work to improve particular pieces of hardware.

Project # FCP-17: Residential Fuel Cell Demonstration by the Delaware County Electric Cooperative, Inc.
Schneider, Mark; Delaware County Electric Cooperative

Brief Summary of Project

Delaware County Electric Cooperative, Inc. will validate 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, they will measure and report technical performance, provide raw cost data and economic viability analysis, document maintenance and operations concept enhancements specific to residential fuel cells, share safety related vulnerability analysis and lessons learned, and promote education of state and local consumers.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The project strongly supports need for real world experience.
- This is a demonstration project using available commercial products; there is little if any technology development component of the project.
- Project provides some visibility on current state of stationary fuel-cell power sources, which is that they basically work, are not yet cost effective, and heavy loads like hot tubs require some compromise.
- Project neither helps nor hinders the President's objective of achieving efficient, cost-effective, "no excuses" hydrogen energy options.
- This is a very concrete systems project with clear targets.
- The project is of relevance to the stationary fuel cell initiative (but limited to single home). There is strong interplay of load-sensing and controls.
- The technical program is well-designed and if executed well, it will show the pros and cons of fuel cell systems for home use.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- Data gathering allowed for sensible design and load control strategy. May need to have more direct involvement by Plug Power and other suppliers for support --maintenance and operation optimization.
- This project is not about "technical barriers."
- Within its scope, gathering data about home electricity consumption and electrical power quality, the project is well designed.
- Project generates voluminous data, which is only of marginal value or relevance to other R&D.
- Project could improve by articulation of how data generated would influence future course of hydrogen technology: be specific with respect to decisions that must be made and how experiment is designed to develop the necessary data to support those decisions. ("Decisions" may be also taken to mean engineering design practices.)
- There are a lot of companies around the project sharing information and developing the system jointly.

- Very solid and methodical (including the "precaution" to remain grid-connected). Data gathering on load variations and possible load management is indeed appropriate.
- Excellent state of collaborators are participating, which will show the potential (or lack thereof) of future commercialization.
- The program is yet to start, but the depth and breadth of program are impressive.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Installation of complete data gathering hardware is a significant accomplishment, as is finalization of design.
- Project is making excellent progress in its own objectives of documenting residential power usage through extensive measurement.
- Project does not advance DOE technical objectives (e.g., efficiency, cost reduction, reliability improvement) beyond its value for "show and tell."
- It is difficult to say after 6 months design phase.
- Program has yet to begin but as a system demonstration the plans are well laid out.
- The program just started, but the plan to run off fuel cell and storage (with minimal grid connection) is bold.
- There is a good plan for thermal integration.
- There is a solid plan for data monitoring (including PQ event monitoring.)

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Get strong commitments from hardware suppliers to work together to resolve any issues.
- PI makes a good effort to establish relationships with interested technical organizations, technology suppliers, and the local university.
- I would hope to see a publication record as the project moves forward.
- There are good number of companies and other organizations around the project.
- The stat of collaboration is excellent, including a rural electric cooperative.
- The project has strong public relations and outreach plans.
- There is a good understanding of the stakeholders involved.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- In addition to tracking performance, final report should include a cost benefit analysis of component size tradeoffs.
- This project is a closed-end, with little purpose for continuing beyond scheduled completion. (Little additional new information would be learned).
- It is difficult to say at this early stage.
- The technical program is sound, but plans for the economic analysis are unclear.
- The length of planned research may not sufficiently explore fuel cell reliability and durability.

Strengths and weaknesses

Strengths
- This is how early implementation is likely to really occur -- at small scale by relatively small providers - so it is a really good test of how this will work.
- The project provides a tangible example of fuel-cell technology in use.
- The project is very quantitative; the test house is well instrumented.
- The project has public-relations values.

- The project group is good, and the project has a practical and concrete approach.
- The project is well-planned with a strong set of stakeholders.
- The data gathering and monitoring plans are excellent.
- The choice of energy storage, fuel cell, and thermal integration is good.

Weaknesses

- Company may be in over its head if anything serious goes wrong. Get strong commitments from hardware suppliers to work together to resolve any issues.
- The project generates little useful technical data to advance fuel-cell technology.
- The sample size of the study is 1 (house), limiting the value of conclusions that might be drawn from the study.
- Were the technology being demonstrated commercially viable, this sort of project would normally be sponsored by private sector.
- The energy storage specifics need to be identified.
- Scientifically this project is not very relevant.
- Data transparency to the outside world is needed.

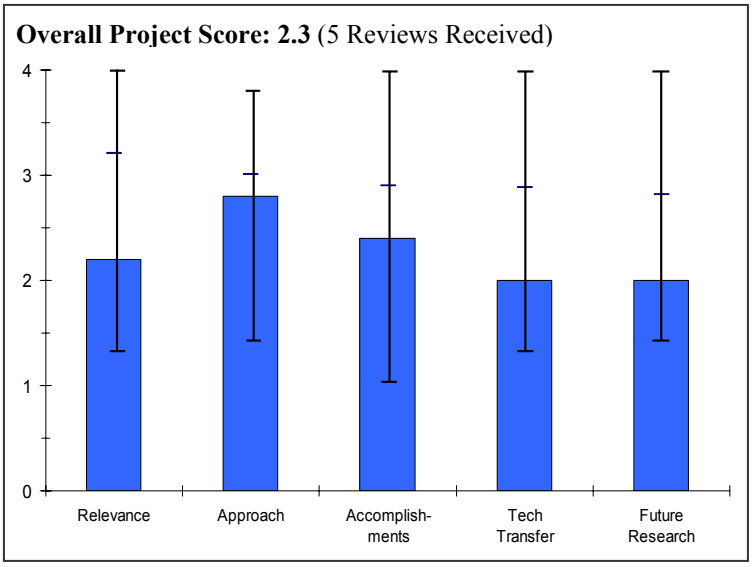
Specific recommendations and additions or deletions to the work scope

- The project might play better as a "technology validation" project than as a "fuel cell" project. However, it would need to align to Technology Validation program goals.
- See how this system works and publicize the results.
- Ensure performance data (good or bad) is widely available to share fuel cell data broadly.
- A parallel effort with another fuel cell supplier may help develop further competitive data.

Project # FCP-18: Smart Fuel Cell Operated Residential Micro Micro-Grid Community Grid Community
Alam, Mohammad; University of South Alabama

Brief Summary of Project

The University of South Alabama project will build on the work of year one by expanding the smart control algorithm developed to a micro-grid of ten houses and will perform a cost analysis, evaluate alternate energy sources, study system reliability, develop the energy management algorithm, and perform micro-grid software and hardware simulations.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.2** for its relevance to DOE objectives.

- This demonstration project uses available commercial products. There is little technology development component of the project.
- Project provides some visibility on current state of stationary fuel-cell power sources -- that they are temperamental, are not yet cost effective, and cannot supply peak loads without compromise.
- Efficient, cost-effective, "no excuses" fuel-cell energy options are neither helped nor hindered by this project.
- Paper study of mini and micro grid meets DOE needs but lack of actual hardware demonstration will limit utility.
- This systems study is done theoretically. I think it will be outdated in few years.
- The program really expands upon the DOD/DOE demos with much more comprehensive data. Incorporation of energy management schemes is very good (also thermal integration).
- Project does not appear to be consistent with the DOE's Fuel Cell RD&D Plan (e.g., no milestones related to project, no tasks addressing this project).
- Value proposition of this micro-grid approach is very unclear and not articulated in the poster session.
- The "Education Value" of fabricated system architectures with an unclear business case that does not have appropriate industry collaboration (e.g., no housing developers) or support is highly questionable.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- Developing understanding of load leveling in micro/minigrids is useful. Need to compare cost/benefits of micro/mini to single house system.
- The project is a combination of theory and experiment.
- The experiment demonstrates a 5 kW residential fuel cell electric power generator – there is little new there.
- The theory models neighborhood minigrids using hypothetical layouts and reliability and load models.
- The theoretical approach is generally well thought out, leading to an algorithm the author calls "smart control."
- This project occurs too soon on the technology development curve: it uses well-known engineering methods that break no barriers, and it would have more impact when commercial products have advanced to the point where actual value could be added by demonstrating engineering technique.
- Again, systems study should be done by practitioners, not based on theories.
- Systemic and comprehensive project moves up from single home to 10 residences -- good modeling.
- Cost estimation source and validity are not clear.
- Fuel cell stack/power plant costs do not appear in any cost analyses.

- Value proposition and business case for a micro-grid has not been articulated. The reviewer expects the business case will not be strong, even at low future projected cost for fuel cells (due to high degree of cooperation/collaboration among housing developers/homeowners/housing associations, fuel cell system providers, etc.).

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- Analysis of central hot water approach is good but what conclusions can be drawn as to cost versus benefit? Where will the reliability data for individual failure modes come from?
- The project makes good progress on its own -- self-defined goals (theoretical modeling).
- The project does not provide much progress toward DOE fuel cell development goals (efficiency, cost, reliability, etc.).
- There are no metrics on the improvement in "understanding and comfort" that the project aims to correct.
- The accomplishment seems to be software modules in a software platform and optimization algorithms. I think this is quite basic.
- Solid from a system integration viewpoint, but system reliability may be compromised by reliability of fuel cell stacks (and system). Need more data on weakness of fuel cell systems.
- Economic analysis claimed to be "complete," but results presented are quite simplistic (e.g. only total estimated cost presented, without sources provided, for thermal system only -- no payback time, comparison of alternatives, business case, etc.).
- Little or no analysis was shown for required capacity.
- Reliability analysis results were not shown for a grid-connected system, which should have much higher reliability.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.0** for technology transfer and collaboration.

- Coordination seems limited to software development.
- PI characterizes most interactions as merely having "made contact."
- PI has modest publication record from project.
- There is only one software company involved. Lack of equipment providers, utilities, constructors is a weakness.
- The project needs collaboration with local utility. No clarity on how different component suppliers are helping (or not) with the program.
- No collaboration with entities ultimately responsible for purchasing/installing such a micro-grid (e.g. housing developers, homeowners, homeowner's associations), which leads to a limited understanding of the market needs and business case.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- Not sure how load control at the appliance level will be managed to avoid consumer upset.
- This course of research is not timely or well-connected to the efforts to remove technical barriers, lower costs, and improve reliability.
- The model can be improved infinitely without any practical goals.
- The project has a strong academic approach to systems integration and interactions. Presentation does not show clear pathway to co-opting key commercial interests.
- Future work is vague, general, and not well articulated.

Strengths and weaknesses

Strengths
- University of South Alabama has an understanding of systems interactions.
- This educational effort may involve students in a fuel cell application engineering activity.
- The researchers are very enthusiastic.
- The project has serious systems integration approaches to understand component interactions and overall system reliability.
- The project is academically appropriate moving stepwise from a single home to multiple residences.

Weaknesses
- There is a lack of plans for hardware validation of models.
- The project does not provide high-value information of any urgency.
- There is a heavy reliance upon hypothetical performance data and models.
- The project lacks systems engineers, equipment providers, customers, or utilities.
- There are no clear collaborations with other key stakeholders (e.g. local utility or t&d concern). University of South Alabama needs to highlight more clearly the weak and the strong components (with the current available data) to enable focus on key hurdles to system commercialization.

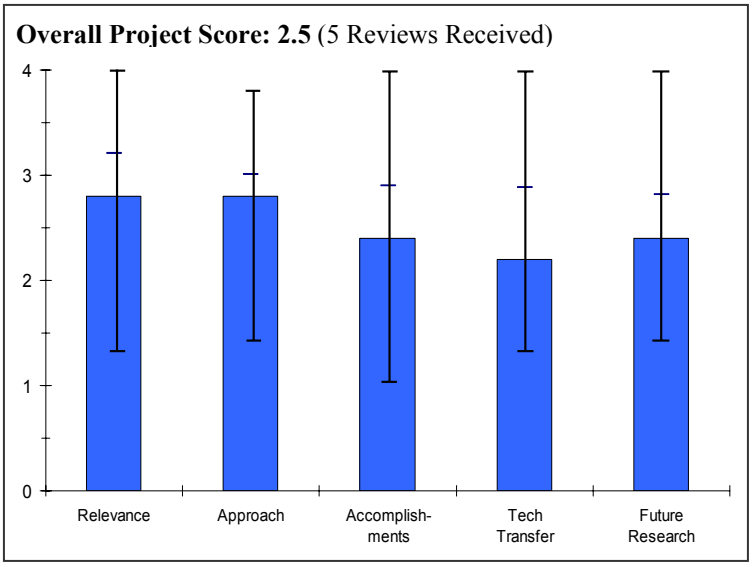
Specific recommendations and additions or deletions to the work scope

- Focus on approaches to optimize load leveling in microgrid and compare microgrid approach to single residence.
- Broaden the project group by equipment manufacturers, systems providers, utilities, constructors, etc.
- Get other stakeholders involved. Expand the program with other collaborations (up and down the supply chain). The program has strong demonstration potential, but must extend beyond the university.
- Value of this project to the overall goals of the DOE Hydrogen Program is not at all clear to the reviewer. It seems to be a very large dollar value effort ($3.5M) with little potential return on the investment.

Project # FCP-19: Fuel Cells Vehicle Systems Analysis (Fuel Cell Freeze Investigation)
Pesaran, Ahmad; National Renewable Energy Laboratory

Brief Summary of Project

For this project, the National Renewable Energy Laboratory (NREL) will provide DOE and industry with technical solutions and modeling tools that accelerate the introduction of robust fuel cell technologies, quantify benefits and impacts of Hydrogen Program development efforts at the vehicle level (both current status and future goal evaluation), and highlight potential system level solutions to technical barriers.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Patent search and literature survey relating to fuel cell performance under freeze/thaw conditions.
- Fuel Cell Freezing Alternatives Investigation.
- This is a literature review program only. Literature reviews need to be done by developers who are trying to solve problems, not by third-party non-experimentalists.
- Project is very relevant to HFI and the DOE MYPP. Overcoming freeze/thaw barriers in an energy efficient and cost effective manner is essential to consumer acceptance of fuel cells.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- A systematic approach to quantifying the present state-of-the-art in fuel cell freeze/thaw operation.
- Started with extant patent literature to determine knowledge base and progressed to high level analysis.
- This program does not help the industry deal with freeze-thaw - it simply enumerates what has been done before.
- Excellent approach starting with comprehensive patent/literature search, narrowing of options, systems analysis for evaluation. Effectively leverages utilization of existing systems tools. PI effectively leveraging similar efforts with batteries and applying knowledge to fuel cells. Focusing on key issue of power at startup in 30 seconds. Provides comprehensive listing of issues facing freeze and rapid startup.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- Significant progress toward accomplishing the objectives of the project.
- Duplicates ANL work in several cases.
- Analysis at high level only.
- No successful path developed.
- Reviewed older approaches.
- They have done what they said they would do - it just has little value.
- Relatively new start (FY04). Accomplished patent categorization. Pros and cons of various options identified. Estimated energy and power needs to thermal insulation method and for thawing.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.2** for technology transfer and collaboration.

- Participated in workshop with industry, academic and laboratory organizations.
- Identified several organizations who seek more in-depth participation in the project and its findings.
- Need to add involvement of OEM participants.
- Need to draw conclusions and disseminate with industry comments.
- Didn't see any real collaboration - just listing of a few names.
- Needs more partners ranging from fuel cell system OEM to automotive OEMs.
- Utilizing University of South Carolina model for freeze/thaw minimizing new model development. Working with General Motors with regards to patent search analysis but additional emphasis should be placed on coordination/leveraging with other entities.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- Good plans for continuation of project to completion.
- In particular, recommendations of combining the identified methods to address operation under freezing conditions will be very useful.
- Must identify new approaches if existing approaches not successful.
- Test out brainstorm ideas.
- No pathways to leverage the data from the technology validation program.
- Future approach is clear and well developed.

Strengths and weaknesses

Strengths
- Appropriate approach to achieve desired results - analytical capabilities of NREL.
- Approach is objective.
- The team has appropriate tools to complete the objectives.
- Good patents analysis. Keep in mind that these are existing systems, not future system.
- Well defined, logical approach leveraging existing patent literature. Accomplishments reasonable to date.

Weaknesses
- Need to include system developers as sources for potential solutions and evaluate their ideas analytically.
- There is no research and development, only literature searching and mapping. This has very little value in solving the real problems in fuel cells unless is performed by those who are DEVELOPING solutions. This team is, apparently, not doing so, nor is it really closely connected with anyone who is.
- Need to collaborate with more systems OEMs and Automotive OEMs.
- Need to come up with potential issues that have not been patented.
- Project could benefit from expansion of partnership/ collaboration.

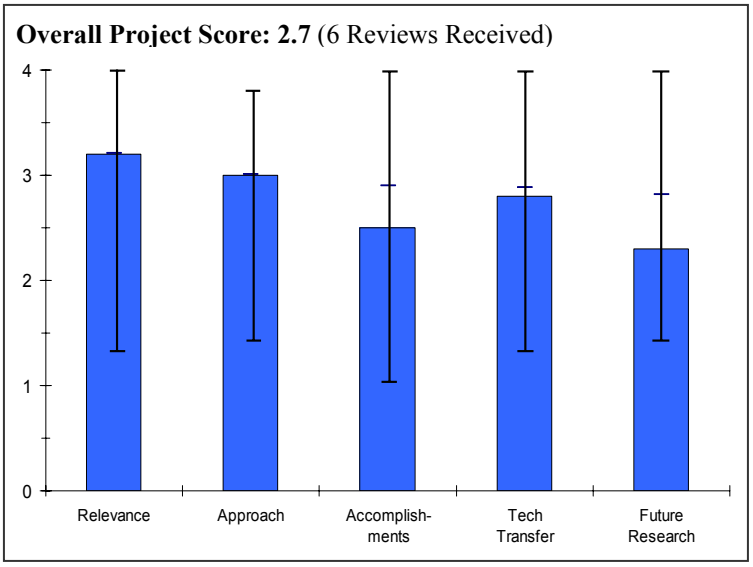
Specific recommendations and additions or deletions to the work scope

- Project scope needs input from system developers. Include stationary applications as well.
- End program. It is of little value and it costs a lot for what they are producing. I have paid commercial vendors to do similar work for 1/10th the cost and they can produce it in a few weeks to boot.
- If there is some political reason to continue funding, then REQUIRE the team to connect in REAL ways with a group or groups who are working on solutions to the problems being addressed.
- Need to leverage technology validation program data.
- Need to expand partnerships to build credibility to model.

Project # FCP-20: Graphite-based Components for Thermal Management in Fuel Cell Systems
Lara-Curzio, Edgar; Oak Ridge National Laboratory

Brief Summary of Project

Oak Ridge National Laboratory (ORNL) will develop compact, low-weight, effective thermal management components for fuel cell power systems using carbon-based materials. The objectives are to determine the feasibility of weaving high-stiffness graphite fibers into complex 3-D architectures; to assess the effect of fiber architecture on heat transfer, permeability and mechanical strength of woven fiber structures; to design, in collaboration with industrial partners, thermal management system components based on 3-D woven graphite fiber preforms.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Radiator sizing is an important issue.
- Graphite is an intriguing approach, but there is still some work to do to establish superiority over other metal-based compact HX technologies.
- Carbon-based heat exchangers.
- Thermal management is a significant factor in fuel cell automotive application.
- This technology, if successful, will yield an incremental improvement. But it is not a game-changer and doesn't have near as much impact as increasing stack temperature.
- Developing more effective heat exchangers is certainly important for FCVs.
- Effective heat transfer is a huge issue in fuel cell transportation systems.
- Novel approaches like this are badly needed.
- Very relevant to thermal management needs of fuel cell vehicles. In future, has potential to address system-wide (not just components) thermal management needs.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Progression of project is rational, and prototyping supports vision very well.
- Articulation of design rules for these types of systems is important.
- Pressure drop needs to be considered more carefully.
- Many obstacles - need practical input from commercial radiator supplier and auto industry.
- Cut and try approach.
- Need more data.
- The team appears to be taking a competent, systematic engineering approach.
- While not stated, I assume the key will be the carbon fiber material cost. More needs to be said on this in the presentation. And if so, emphasis on material cost will likely need greater emphasis.
- This appears to be potentially promising route to developing an efficient and lightweight heat exchanger.
- I would like to see some analysis of cost, up-front, so that it is at least understood not to be a potential show-stopper.
- Approach is good but there is little evidence of effective modeling and simulation.

- There is too little consideration given to cost issues.
- Targeting sweet spot of optimized structure performance/cost. Excellent approach building off past ORNL experience with graphite fibers and leveraging expertise outside ORNL. Clearly addressing potential barriers of cost via 3D graphite structures utilizing graphite fibers with different properties in different directions. Working to address potential barriers of fouling via modeling.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- Feasibility is established, pressure drop/effectiveness now needs to be more carefully assessed.
- Low cost manufacturing can be envisioned.
- Potential benefits not well defined.
- Many basic issues like durability and plugging not addressed.
- Stagnation effects on flat fibers may lead to plugging.
- More analysis required of ideal case to determine direction.
- Some progress shown in developing basic diagnostics and modeling.
- Presentation doesn't indicate progress in system performance or cost.
- Some good progress, but some thermal performance results would be a great addition, even if they are preliminary.
- Certainly good results for the funds spent.
- Very little information given on progress.
- 3-Tex has developed methods for weaving graphite fibers around tubular structures. Methods conducted for evaluating permeability and pressure drop of woven graphite fabric structures. Project will need a harder look at determining potential costs of these methods. Costs of fibers etc. have been ascertained but information has not assimilated for entire thermal management system of a component.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- Would like to see collaboration with a HX manufacturer more than a fuel cell company.
- This project has a high value of return for the investment, and seems a very good licensing opportunity if successful.
- Some interactions shown-more needed.
- Should engage an automotive radiator supplier to leverage their design and manufacture capability and understanding of automotive thermal fundamentals.
- Commendable that the requirements were developed with the assistance of a fuel cell OEM (Ballard).
- If lab results are good, than more industry collaboration will be warranted, as planned.
- Little evidence of collaboration.
- Focusing on thermal management system for radiator using Ballard input is excellent. Leveraging of DOE/DER resources for modeling at University of Western Ontario is good. Relying on established textile manufacturer is good.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Need more definition and timescale with go/no-go points.
- Needs more definition.
- Correctly points to demonstrating a prototype to indicate possible advantages to the technology.
- Hard to judge at this point because it really depends on how promising are the initial thermal results.
- No evidence of current or future efforts to identify problem areas.
- No evidence that effective modeling or an analytical approach will be pursued.

- Future approach is logical and idea of extending to system-wide thermal management strategy spring boarding off inherent advantages of graphite woven structures is good. Project may need additional insights into optional paths should cost targets prove unattainable.

Strengths and weaknesses

Strengths
- Combination of theory, experiment, and prototype, all for very reasonable budget.
- Novel idea.
- Unique approach to an issue that needs to be addressed.
- Novel approach with real possibilities for success.
- Well defined logistical approach utilizing input from fuel cell supplier (Ballard) for radiator performance targets and leveraging of modeling (University of Western Ontario) and textile manufacturing expertise (3-Tex).

Weaknesses
- Lack of practical design rules (probably forthcoming as full scale prototype develops).
- Lack of HX company visibility as validator.
- Moving too slowly without demonstrated plan/schedule.
- Team would benefit from collaboration with company with design and manufacturing expertise.
- No thermal results yet.
- Not much seems to have been accomplished.
- No apparent plans for modeling/simulation.
- Cost is still an unknown.

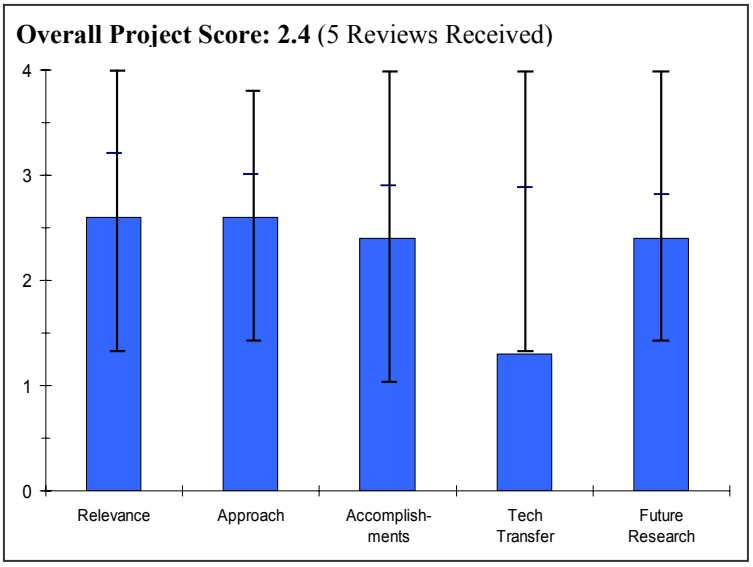
Specific recommendations and additions or deletions to the work scope

- Engage an automotive radiator supplier.
- Establish air side pressure drop and fan power targets (should receive from Ballard or auto radiator supplier).
- List anticipated magnitude of benefits (weight, volume reduction).
- Will galvanic corrosion between carbon fibers and Al tubes be an issue?
- Establish realistic milestones and schedules and incorporate modeling as feasible.
- More emphasis should be placed on ascertaining cost projections for various combinations of graphite fibers and weaving and bonding (epoxy) methodologies.
- In future, emphasize fuel cell system wide integrated thermal management strategy in concert with an OEM.

Project # FCP-25: Fuel Processor R&D
Ahmed, Shabbir; Argonne National Laboratory

Brief Summary of Project

ANL will study reforming of liquefied petroleum gas (LPG) for distributed fuel cell power by establishing the kinetics of propane reforming, both steam reforming and autothermal reforming, and addressing LPG reforming challenges, especially the effect of propylene on reforming and sulfur in LPG.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- Intersection of work and DOE goals not clear.
- Modeling work for LPG is not original or substantive, and seems like rehashing of prior work, and who is "customer" for the kinetics studies?
- Optimal thermal trajectory analysis for WGS is already known.
- Derivation of separation membrane specifications in WGS frame is more original, and helps push boundary of reactor design innovation.
- Presentation mentions barriers, but does not provide specific goals it is addressing.
- Fundamental kinetic test work is useful in many ways.
- Project #1. Efficient approaches to use LPG as a fuel are needed for implementing distributed generation in rural areas.
- Project #2. Improved and cost effective approaches for reducing the size and weight of WGS reactors are important for distributed hydrogen production when natural gas, propane and other hydrocarbons will be employed.
- Project #2. The single-stage WGS membrane/reactor concept is an intriguing one, but it's not clear that the approach can be cost-effective unless it eliminates all downstream CO cleanup steps.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- Modeling is, as usual, thoughtful and comprehensive.
- Design and prototyping of WGS reactor will be of technical interest, innovating combinations of technologies in single reactor embodiments has general value.
- Who is "customer" for single-stage WGS?
- Approach addresses kinetics and water gas shift objectives, but less on the reforming challenges.
- Experimental set-ups and test data are quite good.
- Would like to see kinetic data for components comprising >99% of LPG.
- Temperature-gradient WGS work similar to this project has been reported in literature several times. Not clear what this work adds to base knowledge.
- Project #1. The approach to reactor design and testing is sound.
- Project #2. A rational modeling and design approach is being pursued for the membrane reactor. The primary question is availability of WGS catalysts that are sufficiently active for the full advantage of the approach to be obtained.

- How is the efficiency of this whole process compared to ammonia? There is a vast use of ammonia in the rural regions.
- Good transition of work from CNG/Gasoline reformation knowledge base.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- A little heavy on the analysis side.
- Project shows progress, but without specific goals or targets identified it is difficult to determine how much.
- Project has just started. Experimental apparatus is in place and working.
- Both of these projects are relatively young, so it might be a bit early for an assessment of technical progress. Based on results presented, it appears that both projects are on track.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.3** for technology transfer and collaboration.

- Not clear who the "customers" are.
- This work would be helpful to establish DOE goals and specifications, but the intent of the prototyping efforts is not clear - commercial entities are doing this work.
- Project work appears to be performed by ANL exclusively.
- Did not see evidence of collaboration. Would be worthwhile to see tests of commercial catalysts in these test reactors and conditions to see if kinetics are same for materials with different precious metal loading and promoters.
- There is little evidence of outside collaborations on either project.
- Project #1. It would be useful to obtain input from stakeholders to provide directional guidance as this project proceeds.
- Project #2. There is a significant amount of membrane development work at other DOE laboratories (e.g. NETL). Collaborations with these other labs should be pursued.
- This project is new so this low score is probably not a fair rating.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- Future work should be better focused on more specific goals/targets.
- Good kinetic data on C_{2+} reforming is lacking as well as effect of sulfur on activity at low temperature.
- Project #1. The future plan is reasonable, with testing of “surrogate” fuels to assess effects of impurities on the reforming process. Testing on commercially available LPG fuels should be included in the matrix.
- Project #2. The technical aspects of the plan are sound. A manufacturing cost analysis of the membrane/reactor approach should be conducted, so that the economic viability can be confirmed.
- Should be better leveraged from previous work. Some of the proposed work seemed repeated from the previous years of work.

Strengths and weaknesses

Strengths
- Team capabilities in analysis.
- Lab capabilities.
- Well designed test equipment.
- Highly skilled personnel and excellent lab infrastructure.
- Both projects leverage a large experience base at Argonne.
- LPG conversion is a good leverage of previous gasoline reforming and CNG reforming work.

Weaknesses

- Lack of utility of results.
- Lack of customer and/or driver.
- Not enough details with respect to goals and targets for this research.
- Not sure LPG will have major role in stationary fuel cell power systems.
- Lack of collaborations and stakeholder input.

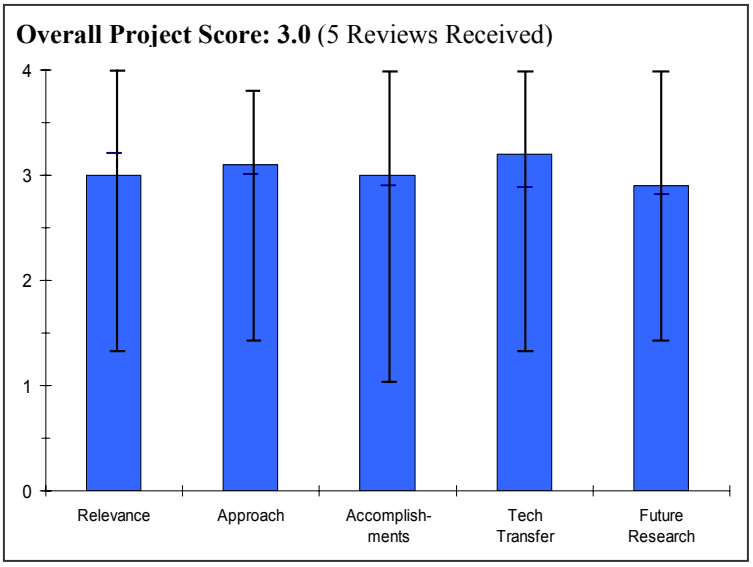
Specific recommendations and additions or deletions to the work scope

- Establish more detailed goals, aligning to DOE goals and structure research accordingly.
- Consider using same test equipment for obtaining kinetic data for reforming components of likely liquid fuels that will be used in stationary fuel cell power systems such as diesel or JP8.
- Need to compare such reformation process against ammonia which is vastly used in the rural area on an overall energy efficiency process.

Project # FCP-26: Fore Court Fuel Processing
Whyatt, Greg; Pacific Northwest National Laboratory

Brief Summary of Project

The objective of this Pacific Northwest National Laboratory (PNNL) project is to adapt the micro-channel steam reforming technology, previously being developed for on-board reforming, to station-based hydrogen production from natural gas. The micro-channel approach enables high rates of heat and mass transport resulting in substantial process intensification with improved efficiency. The current year's project will integrate a micro-channel steam reforming system with an adiabatic WGS reactor and Cu-Pd membrane separator to provide a demonstration of a highly efficient, small scale (5 kg hydrogen/day), hydrogen generation system.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Efficient hydrogen production for distributed power generation from natural gas as transitional approach is relevant.
- Distributed hydrogen is quite relevant and this project fits the plan elements for a hydrogen economy.
- The decision to stop on-board fuel processing limits the relevance of the program.
- Good potential for cost reduction and technology transfer to other projects.
- This project provides a path for cost effective and efficient natural gas fuel processors for distributed generation applications.
- The PNNL team has identified that their micro-channel technology provides significant technical advantages for steam reforming and is developing a fuel processor based on that platform.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- Builds on the micro-channel technology developed for on-board reforming. The application of this technology for compact small size application is relevant. The application for large scale does not seem as good a fit.
- Solid approach. All aspects covered- economics, modeling, prediction, hardware development, and testing.
- The use of microreactors has questionable benefits for stationary applications.
- Has adapted well to change in emphasis away from on-board reforming.
- Integration with PSA and membrane technology to meet DOE H_s quality specs is attractive.
- A rational approach to reactor and system design, modeling, testing and demonstration is proposed.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Modeling and flowsheets amplified, parallel evaluation of membrane and PSA as alternative approaches is good.
- Project has progressed well considering its length. Multiple paths developed and to be evaluated.

- The modeling effort was comprehensive and beneficial.
- Significant progress has been demonstrated.
- Good to see that it worked when reforming liquid fuels and integrated with PEMFC and Pd membrane.
- Demonstrated reduction in air pressure drop is important for reducing blower parasitic power.
- The design work conducted to date appears to be solid.
- Although it was confirmed that fabrication cost (and not materials cost) is the key to achieving cost targets, there was no discussion how the fabrication costs can (or will) be addressed.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Large number of interactions; need to focus where advantages of this technology are most applicable.
- Initial interactions constructive. Still early.
- Collaboration with a fuel cell system integrator would be beneficial.
- Several industrial collaborations were shown.
- PNNL has been extremely successful in transferring this technology to a range of fuel processing applications, and working to have the technology commercialized.
- Collaborations with a membrane manufacturer could provide significant benefit to the project.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.9** for proposed future work.

- Adequately planned for the amount of funding. Reevaluate if 15,000 kg hydrogen/day is the most relevant application vs. focusing on small systems 5 kg hydrogen/day.
- Plan forward is in place. Continue to evaluate short term tests on small scale prior to 1500 kg/day.
- Program should include detailed cost analysis for scale up to 1500 kg/day.
- Planned scale-up to larger sizes good step.
- Technical aspects are sound. Cost analysis should be extended from raw materials to manufacturing costs.
- Depending on the results of the manufacturing costs, it may be necessary to direct some effort toward reducing fabrication costs.
- The cost of the membrane separator should be addressed.

Strengths and weaknesses

Strengths
- Working off of prior experience.
- Beneficial use of modeling effort.
- Good potential for reducing parasitic power and heat loss to environment.
- Working to integrate with PSA and membrane purification systems.
- Solid micro-channel technology platform around which to design and build a fuel processing system.
- Aggressive efforts aimed at transfer of the core micro-channel technology to other fuel processing applications.

Weaknesses
- Needs more detailed cost study for building larger scale reformer.
- Needs long term durability testing that include multiple start-up and shut-down cycles.
- The manufacturing cost may be prohibitive for large-scale distributed generation applications.

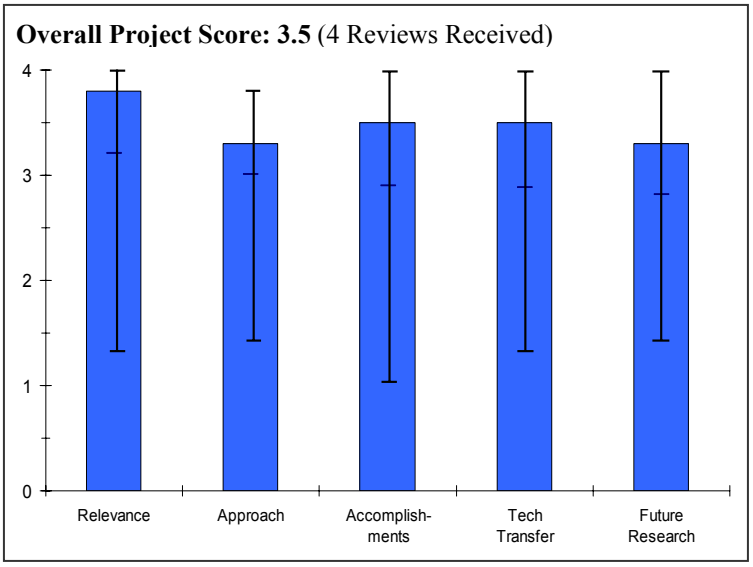
Specific recommendations and additions or deletions to the work scope

- Need to work with industrial partner to get better cost estimates for building multiple larger units.
- The cost analysis should be extended to include fabrication costs and the membrane.

Project # FCP-27: Component Benchmarking
Rockward, Tommy; Los Alamos National Laboratory

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.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Common protocols for material evaluations are critical to progress.
- Standardization is a key milestone to the realization of any widely-adapted technology. Although this is an often over-looked activity, historically shown as essential to adaptation.
- This program provides the mechanism and procedures to establish the test protocols for comparisons of PEMFC testing.
- Component benchmarking is not specifically stated as a DOE objective in the RD&D plan. However, it is a necessary task and is relevant to the DOE objective of fuel cell development.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- Break-in may depend on specific materials - protocol should suggest when break-in is complete.
- DOE/LANL began the activity, and through member companies of USFCC effectively used the consensus approach with industry concerns to establish a procedure.
- The incorporation of statistical analysis is applauded.
- LANL has assembled a good team to help develop the SOP for comparative testing.
- The approach is good, but should be broadened, if possible with the limited funding, to include multiple MEA types and operating conditions. The conditioning/break-in procedure may be MEA-specific.
- Calibration of the instrumentation, or a standard calibration procedure, should have been established and conducted first prior to the round-robin testing.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- Excellent progress in establishing protocol for initial performance and variability. Need to include cell assembly variability in protocol or establishing standard assembly protocols.
- The completion of a first phase of round-robin testing is a significant accomplishment.
- Adequate progress toward establishing the protocol is apparent.

- Good progress, but the MEA test results could have been more meaningful if a calibration procedure were established first.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- May wish to involve ASTM or some other standard testing organization for assistance on establishing protocols.
- The ability to manage often conflicting interests of USFCC member companies and still obtain a standard test method with round-robin testing is a significant testimony to outstanding Tech transfer/collaborations.
- Having the industry accept SOP for testing is not easy.
- Outstanding collaborative effort, as is necessary for this type of project.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Extension to durability issues is key to supporting long-term DOE test needs.
- Future plans, after refinement of the standard plan, should now move to standard durability test methods.
- More details of how LANL will implement/recommend the SOP need to be stated.
- Proposed future research was short on details, but the overall plan looks good.

Strengths and weaknesses

Strengths
- Attention to detail that is necessary for this kind of work.
- LANL widely regarded as the leader of single cell testing and diagnostics of failure modes when non-reproducible results are obtained.
- Strong collaborative effort.

Weaknesses
- Need to bring in more analytical resources to support durability testing.

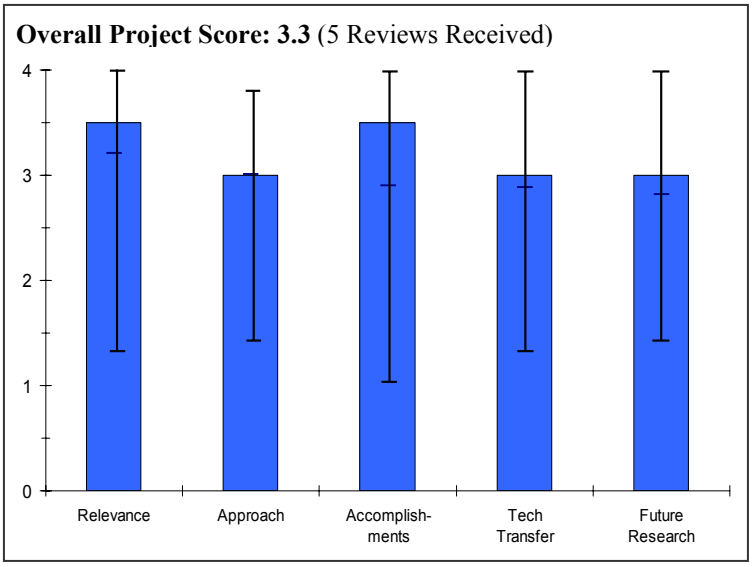
Specific recommendations and additions or deletions to the work scope

- Through USFCC become the advocate of standard method for durability test protocols.
- May be beyond the scope of this work, but should use position as leader to promote the widespread adoption of this standard (for example, report results based on USFCC standard method).
- Inclusion of other MEA types.

Project # FCP-28: Fundamental Science for Performance, Cost and Durability
Pivovar, Bryan; Los Alamos National Laboratory

Brief Summary of Project

Los Alamos National Laboratory (LANL) is developing a fundamental understanding and technical underpinnings of technologies for reduced cost and improved performance and durability. This effort focuses on modeling from the molecular level for increased understanding of proton conduction to the phenomenological level to interpret membrane-electrode interfacial breakdown and interpretation of reference electrodes.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- This project addresses some key needs for the program.
- Areas selected important for achieving long-term goals and ripe for investigation.
- Reference electrode work will be crucial to understanding severity of local durability stresses during non-steady-state operation.
- Delamination model is of potential interest, but it appears that only clear experimental case of in-cell delamination is irrelevant -80°C to 80°C case (plus possible mild case in UTC freeze workshop presentation).
- Humidity effects on ORR kinetics are probably worth studying, but there may be a more straightforward way to make more relevant measurements.
- Higher pH electrolytes are a hard sell and are probably irrelevant to automotive applications. In an alkaline H_2/O_2 fuel cell, water is produced at the anode, where there is a much lower gas flow rate (and little inert-gas component) to help sweep water out of the cell to prevent flooding. Imidazole is well known to poison Pt electrodes. So one would need a new imidazole-resistant cathode catalyst and a new anode structure that is more efficient at clearing water than any present cathode structure. Seems like a long shot.
- Program addresses developing of measurement methods, analytical models, and understanding of reaction kinetics to fuel cell processes.
- A direct correlation with a successful development of a reference electrode or some of the models with successful realization of the Presidents Hydrogen Fuel Initiative may not be possible

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Outstanding approach to each of the technical issues addressed.
- For reference electrode, outstanding.
- For delamination, good modeling, on the assumption that delamination does occur to a significant extent with competently-constructed MEAs in good cells. Make sure that simpler interpretations of resistive effects, such as uniform increases in electrode ionic or electronic resistance, are not overlooked.
- On humidity effects on ORR kinetics, wouldn't it be simpler and more directly relevant to just run in a 50 cm^2 at very high stoichiometries (differential conditions) so that the RH is essentially constant throughout the cell?
- The approach to the reference electrode is from the backside of the electrode and requires membrane be incorporated in the GDL. This could change the reaction properties of the system.

- The delamination model would be valid for single cells but not valid for fuel cell stacks. The model does not identify where (in the x-y plane) the delamination occurs, for example at air inlet or air exit. The value of the model may be limited.
- Effects of hydration on ORR are important for systems operating on dry air.
- It is well known that changing the pH changes the oxygen reduction kinetics to the point that at high pH, carbon is a good oxygen reduction catalyst when combined with a transition metal (peroxide decomposer). Studies in buffered solutions, pH 8 (sodium carbonate cell) are well established. It is not clear what new information is being sought by this program.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- Exceptional progress for such a new project.
- Reference electrode modeling and experimental work is very nice and should have a rapid impact on the understanding of some severe durability issues.
- Delamination modeling seems to give important insights as to the potential impact of delamination, whether or not it is a significant factor in real fuel cells.
- High-pH imidazole is an interesting concept that could be tried if everything else fails.
- Good progress is being made on the approaches under study. The value of the progress against the President's Hydrogen Initiative is not obvious.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Outstanding interactions with a variety of players in the areas addressed.
- Results likely to be published and made accessible to fuel cell researchers lessen need for close collaboration with multiple partners. Collaboration with VT is appropriate.
- Reference electrode work has already been transferred in a way that should provide useful fruit in a short time.
- Other work generally in too early a stage to expect a lot of transfer yet.
- No collaboration with industry identified, no collaboration with other National Laboratories, and only one collaboration with a university.
- Information is published.
- Information should be published in refereed journals.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Outstanding and thoughtful delineation of the future plans to address very important problems.
- Future work on membrane-electrode interface would be stronger if it could be expanded to more membranes than Nafion and BPSH.
- Prospect of improved Pt ORR with higher-pH electrolytes or non-precious metal ORR catalysts is not uncertain. Could Ni really work with imidazole-based electrolytes? If known, cite literature. If not, consider as an area of future work or expanded scope.
- Nice ideas and some good exploratory work. Make sure exploratory effort hasn't already produced 90% of the potential useful insights before committing significant continued resources.
- Rating of 3 is for humidity oxygen reduction work. Other work would be rated 2.

Strengths and weaknesses

Strengths
- Exceptional team and execution.
- Nice combinations of experiments and models, some of which have produced immediate fruit that are enablers of other important work, and others of which are pretty fruity.
- Strong talented research group.

Weaknesses
- Strongly consider changing the title, which, with excessive honesty, seems to imply a $600K general technical sandbox in which to play (which might be effective but might prove hard to defend through the budgeting process in the long run). High-pH approach faces clear technical challenges as well as the tremendous momentum (backed by demonstrated technical promise in important applications) behind acid PEM development.

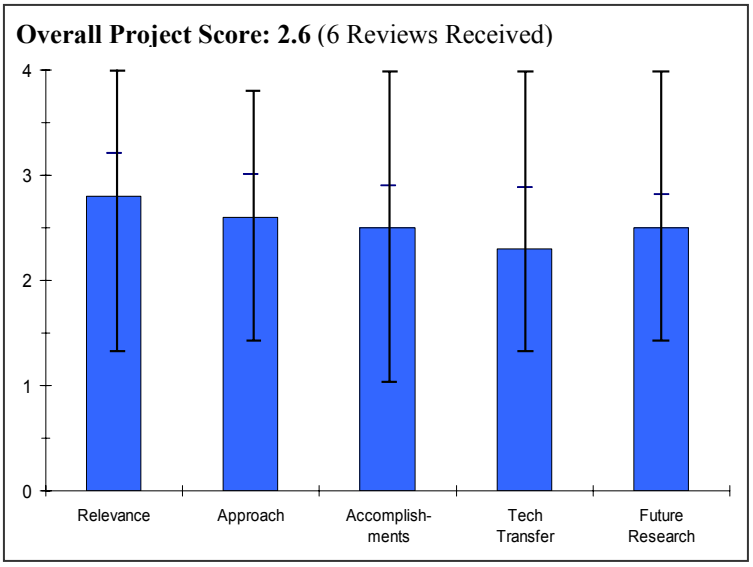
Specific recommendations and additions or deletions to the work scope

- Suggest dropping plans for imidazole work - the general area already has too much research effort devoted to it, and the alkaline twist proposed here has inherent disadvantages. Reference work is very good, but does it make sense to continue reference development? Strong case for relevance of delamination to practical fuel cell operation would have to be made to justify an extensive program.
- This is wonderful exploratory work with some creative concepts, but sometimes it's best to wait a little to see if the practical world catches up to areas of exploratory interest, rendering them compellingly important, before bringing out the heavy guns (and dollars).
- Program should identify fundamental needs of industry and develop understanding that would accelerate industry meeting goals of President's Hydrogen Initiative.
- Eliminate delamination effort.
- Justify pH work. Review literature on low temperature carbonate fuel cells.
- Should this program be in Basic Sciences?

Project # FCP-29: Fuel Cell Testing
Bloom, Ira; Argonne National Laboratory

Brief Summary of Project

Argonne National Laboratory is conducting independent evaluations of polymer electrolyte fuel cells and stack systems developed under the DOE Fuel Cells for Transportation Program. The present facility can test stacks and systems rated for 100 W to 50 kW. The results from this project will provide DOE with an independent assessment of DOE contract deliverables, determine the status of the state-of-the-art fuel cell technology, and provide data for model development and validation.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Testing facility is needed.
- Independent stack/system validation is important.
- Good opportunity for input for the development of standards and standardized test procedures.
- Vital to measure the current status of fuel cell systems against the targets.
- Not sure how necessary "independent" ANL testing is to advancing to advancing fuel cell state of the art.
- Independent assessment of SOA fuel cell technology is critical.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- Comprehensive test procedures.
- Testing protocol is thorough.
- Testing approach is reasonable and in line with standards if they exist.
- Project identifies procedures and protocols for testing. Indications are that there are no show stoppers with regards to testing- only dated equipment limitations. No mention of coordination with other testing facilities such as LANL or SwRI.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.5** based on accomplishments.

- Hard to judge accomplishments due to lack of test data (at least include an overview table listing what kind of stacks were tested).
- Impossible to judge, since all the data generated go to DOE HQ under a confidentiality agreement.
- The lab is functional.
- Little said about what has been actually tested to date and the results of the testing.
- Safety systems well thought out and implemented. No specific barriers to really overcome except possibly lack of comprehensive universally recognized testing procedures. May be beneficial for project to be more involved with organization (i.e. ASME, SAE) developing test procedures. All funding for fuel cell testing and most (90%) for battery testing comes from DOE and USABC would be beneficial if private sector utilized facility.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- Would appreciate if more of the test data could be published.
- Impossible to judge, since all the data generated go to DOE HQ under a confidentiality agreement.
- Need to provide a process for people to use this lab.
- Tech transfer not discussed.
- Project seems to operating somewhat in a vacuum. Not much coordination or information flow with other testing facilities or private sector. Does seem to be coordinating somewhat with FreedomCAR testing and modeling activities although extent is unclear.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- Stack durability testing for automotive and stationary applications would be very helpful.
- Can't comment. PI can't talk about his work.
- Where is the return on investment and who are the future users of this resource and investment?
- Project seems to be reflexive of immediate DOE testing needs but not particularly proactive in identifying future needs and methods of interacting/ leveraging resources outside ANL. For example, freeze/thaw analysis at NREL are producing models and estimates of energy requirements to warm up fuel cell from cold stock. There is likely some synergy here to be exploited.

Strengths and weaknesses

Strengths
- Independent performance verification.
- Independent testing/validation of developer claims vital to program.
- Written protocol for testing.
- Good lab for testing.
- Clearly there is a need to benchmark technologies and gauge technical progress of fuel cell program. Project also help fills need of providing testing data to validate models of OHFCIT and OFCVT.

Weaknesses
- Most test results remain proprietary.
- Must depend on DOE HQ to judge the quality of this project.
- Impossible for reviewers to rate this project.
- Who is using it?
- Does it make sense to put more money into it without collaboration or future users?
- Project seems to operate almost autonomously without strong communication and coordination with other testing entities, private sector, mom??? organizations establishing's test procedure. Project needs to be more proactive in establishing future wishes/needs.

Specific recommendations and additions or deletions to the work scope

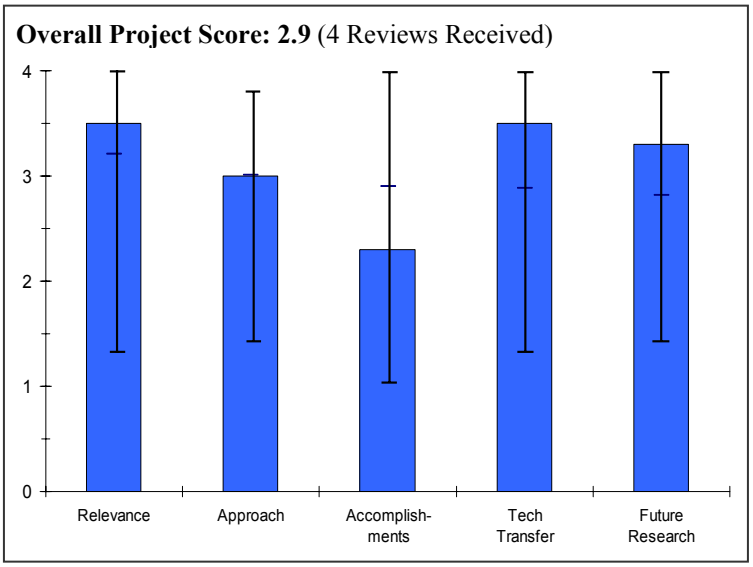
- The testing facility seems ready before the fuel cell stacks are ready for testing. Needs thinking how to utilize it in the meantime.
- Look into possibility of working with USFCC Materials and Components Working Group to establish stack testing protocols.
- Work with DOE Stack Components contractors with most promising results (in final stages of their contract) to endurance-test stack materials in standardized hardware; include results in future review presentations.
- ANL and DOE HQ work together to figure out how to show averaged results that don't identify developers.

- Need to address the topic of future users of this facility, how does it support the testing objectives and the standards/guidelines it supports.
- Would like to see comparison of ANL test results with fuel cell manufacture's test results.
- Coordinate/collaborate more with other organizations to leverage resources/activities. Seek private sector interest, needs, funding support.

Project # FCP-30: Solid Oxide Fuel Cell Development for Auxiliary Power in Heavy Duty Vehicle Applications
Simopoulos, George; Delphi

Brief Summary of Project

This DOE program will further advance the state of the art of solid oxide fuel cells (SOFCs) in the commercial trucking industry as auxiliary power units (APUs). The program's objective is to define system requirements and design, build, and test a SOFC-based APU system capable of running on low sulfur diesel fuel in commercial heavy-duty truck applications. Fuel cell APU technology allows for the reduction of primary engine idling emissions and an increase of overall vehicle fuel economy. Delphi has teamed with original equipment manufacturers, PACCAR Incorporated and Volvo Trucks North America, to define the system level requirements, and enlisted Electricore to provide administrative assistance.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The project is entirely relevant to DOE's goals and objectives.
- Relevant to the DOE objectives for auxiliary power.
- The area of heavy trucks is not directly part of hydrogen fuel initiative and there is a more appropriate DOE program for the project. The construction of SOFC adequate to APU, and APU are proposed.
- APU development seems to have a loose relationship to the Hydrogen Program. APUs will assist SOFC commercialization primarily in stationary applications but will also assist application to hydrogen production via electrolysis. Reduction of dependence on foreign oil does not appear to be a significant driver for this work.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- The project would be better if the APU were tested in an over-the road truck instead of in a simulation.
- Major tasks are listed but key milestones and dates are not described or scheduled.
- Necessary development of the reformer/stack package isn't addressed. Have the APU package and its components already been developed and qualified?
- The scope of the project is limited for the funding level considering the project is heavily leveraged by Delphi's SECA-funded activities.
- Considering the experiences of the main company and collaboration with two industries, the project is feasible. But the inclusion of SOFC developer is strongly recommended.
- The work is focused on system assembly, not directly R&D, more engineering oriented. Within this scope the approach is good.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- Program accomplishments are not adequately described or compared to the original schedule.
- The project is relatively new, but it appears that the only accomplishment in the past 8 months has been a planning meeting. Considering Delphi has been working in this area for a number of years under SECA funding, the pace of the project should be rapid.
- The project is in the beginning. Must inform about others efforts made in the different laboratories. The project will benefit hydrogen program because of SOFC development being important the inclusion of SOFC developer or interaction with specialized laboratory. No information about the cost.
- Very little progress. Progress has consisted of signing the agreement, establishing collaboration and submitting a report. No technical accomplishments identified.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- The team contains the necessary elements including a heavy truck manufacturer to provide real-world perspective and requirements.
- It is not clear who will provide the reformer/SOFC stack package or its individual components.
- Excellent collaborative effort.
- There are the truck companies as collaborators. But there is no indication about the technology transfer, yet. Must inform about others efforts made in different laboratories.
- PACCAR.
- Volvo.
- Excellent collaboration, key organizations that provide all the required technology for the project.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Future work addresses the project objectives and logically follows progress to date.
- Limited in scope.
- The parameters to be considered is not enough nor clear for the PI. Planning is very simple.
- The list of future work is complete to provide an APU system for a truck. A thorough engineering development effort that will produce a product.

Strengths and weaknesses

Strengths
- Strong background in SOFC and reformer development for APU applications.
- Integration possibilities with other research.
- Excellent collaboration.
- A thorough engineering effort is planned.

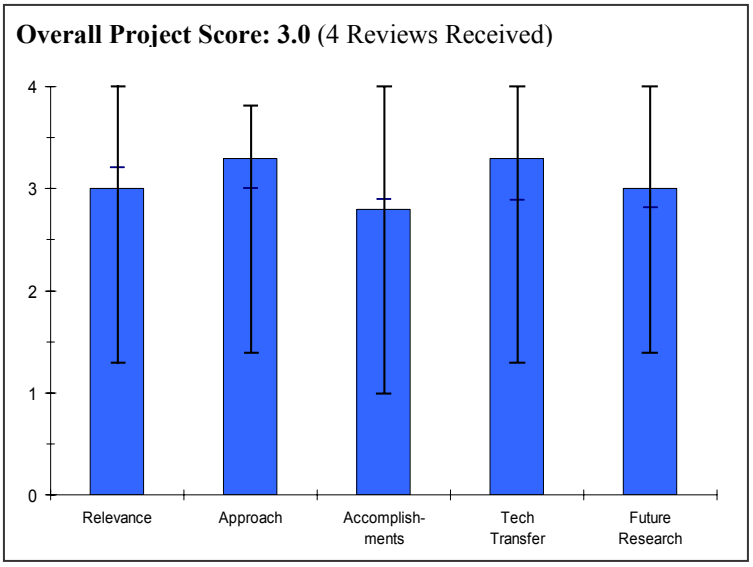
Weaknesses
- Limited scope given previous experience in the area.
- The parameters to be considered for APU development are not clear for the PI, yet.
- Very little progress.
- The product will not be tested on a truck.

Specific recommendations and additions or deletions to the work scope

- Understanding of problems associated with SOFC.
- Interaction with SOFC developer is recommended.
- Must include safety codes in the project.

Project # FCP-31: Diesel Fueled SOFC for Class 7/Class 8 On-Highway Truck Auxiliary Power
Norrick, Dan; Cummins Power Generation

Brief Summary of Project



Cummins Power Generation, with team members SOFCo-EFS Holdings LLC and International Truck and Engine, will produce and demonstrate a prototype solid oxide fuel cell (SOFC) based auxiliary power unit (APU) for application to Class 7/8 on-highway trucks. SOFC APUs are potentially clean and efficient energy sources for sleeper cab cooling, cab and engine heating, and electrical power to enable main engine shut-down during driver rest periods or other extended waits where truck main engines have traditionally been idled. Project challenges include accurately characterizing energy requirements, development of an application specific diesel fuel reformer, ruggedized system integration, and the combination of product cost and efficiency to meet stringent industry payback expectations.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Relevant to the DOE objectives for auxiliary power.
- The use of fuel cells to meet the auxiliary power needs for trucks is relevant to the DOE objectives. Successful development of APUs can reduce diesel fuel consumption and emissions.
- The PI addressed the problems and the importance of his project. The construction of SOFC (stack with 140 cells) in collaboration with a company and APU for heavy truck is proposed. The area of heavy trucks is not directly part of hydrogen fuel initiative (RD&D plan objective) and there is a more appropriate DOE program for the project.
- APU development seems to have a loose relationship to the Hydrogen Program. APUs will assist SOFC commercialization primarily in stationary applications but will also assist application to hydrogen production via electrolysis. Reduction of dependence on foreign oil does not appear to be a significant driver for this work.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- An understanding of the stack survivability from vibrations is important.
- Fuel reforming using recycled anode water is a good approach.
- The approach is sound. System requirements and load profiles were developed first before hardware specifications. Development issues related to the fuel cell and reformer systems have been identified.
- The project started in 9/01/2004. Several technical barriers are addressed. The PI presented alternative pathway in some cases but in others, he needs collaborations with SOFC specialized group to solve. The project is feasible.
- The project is broken into key technical issues. Combined fuel cell/battery design is an excellent and possibly essential approach. The substantial focus on Shock and Vibration issues is outstanding. This is a key issue for use of SOFCs in mobile applications.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Little progress.
- Much of the background has been established in the SECA-funded program, therefore progress should be rapid.
- The project started in September 2004 and is only about 15% complete. Analysis of thermal and electrical load profiles is underway. Micro reactor studies are also underway to support reforming catalyst development.
- The project is adequately led. The PI must inform about others efforts made in the different laboratories. The cost goal is considered as a parameter.
- Good start on the specific key issues. One issue is complete (water management). Most technical issues are still open but good progress. An excellent vibration input spectra was displayed, an essential first step in mitigating any S&V issues.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- Excellent interactions with industrial partners.
- Collaboration between Cummins, International, and SOFCo is evident.
- There are the truck companies as collaborators. But there is no indication about the technology transfer, yet. Must inform about others efforts made in different laboratories.
- International Truck and Engine.
- SOFCo - fuel processing.
- Excellent collaboration, key organizations that provide all the required expertise for the project.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Vibration studies are important to determining survivability.
- Good work plan for future research.
- Future work plans were presented. Both experimental and analysis tasks were described.
- The variables to be considered in the APU development must be detailed to the planning.
- Requires more detail. Insufficient detail regarding truck system integration. Integration of the final product in a truck is important. Task list for key technology work is good.

Strengths and weaknesses

Strengths

- Good understanding of BOP, power conditioning, heat and water management.
- Good understanding of operating conditions.
- There is evidence that the team is well integrated with each organization assigned specific analysis or experimental tasks. The team should be able to demonstrate a prototype system in real over-the-road trucks. The project appears off to a good start. Poster presentation was well organized and informative.
- Good understanding of problems associated with SOFC.
- Excellent collaboration.
- Excellent focus on key technology that requires development.
- Integration in a truck will be a significant achievement.

Weaknesses

- Can an all ceramic SOFC stack survive APU conditions?
- Vibration characteristics could cause problems with solid state fuel cells. But the team is investigating this issue.
- Requires more detail with respect to system engineering and integration.

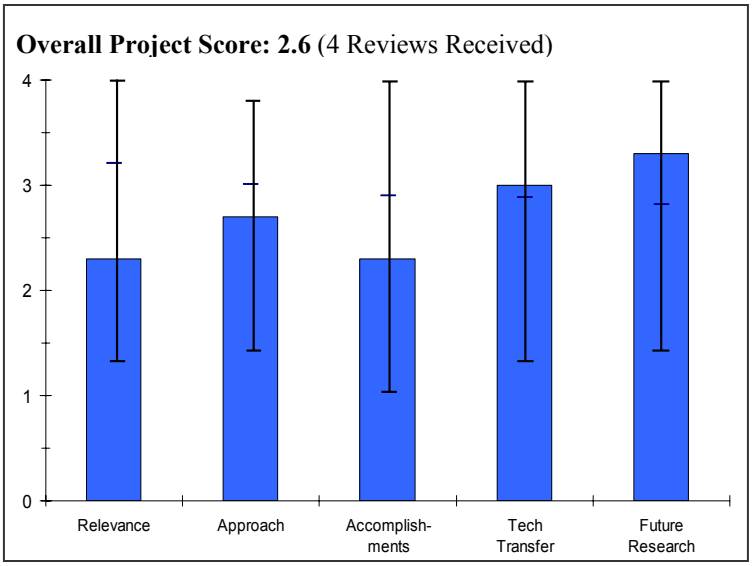
Specific recommendations and additions or deletions to the work scope

- Must include safety codes in the project.
- The project should utilize the shock and vibration analysis methodology under development at PNNL. Both projects would benefit.

Project # FCP-32: Fuel Cell APU
Brodrick, Christie-Joy; University of California, Davis

Brief Summary of Project

The Institute of Transportation Studies at the University of California at Davis (ITS-Davis) integrated and demonstrated a proton exchange membrane fuel cell system (~5 kW) in a transport refrigeration unit (TRU). ITS-Davis conducted a trade-off study of the fuel cell TRU system including quantifying cost, noise, and emissions.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.3** for its relevance to DOE objectives.

- Fuel cell-based power units for operating trailer refrigeration units can decrease fuel consumption along with emissions.
- The area of refrigeration by fuel cell is not directly part of hydrogen program.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- The approach has been to first quantify the magnitude of emissions as a function of power rating of conventional units, ambient temperatures, and determine the operating power profiles of such units.
- After that, the main activity has been to configure a fuel cell/ battery / diesel engine unit to verify operational characteristics and emissions.
- The project started in 03/01/2004. Several technical barriers are addressed such as the problem to purchase a SOFC. The PI substituted with PEMFC provisorily. The project is feasible but additional time is required because of the substitution of PEMFC by SOFC, and the problems associated to SOFC are different from those of PEMFC. The PI belongs to a University. It has difficulty to get collaboration.
- The hybrid SOFC/Battery approach is a good approach for transient loads. It is also a good cost approach but can impact the size for a size limited application. The power electronics approach was not appropriate to a 3-phase system. A motor generator type approach would appear better, the PI recognized this.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- They have been unable to obtain a solid oxide fuel cell (SOFC) as initially planned. They have, however, redesigned the project to use two commercially-available polymer electrolyte fuel cell (PEFC) stacks. This has entailed significant changes from what was originally considered. For example, rather than reforming diesel to power an SOFC, they will use separate on-board hydrogen to power the PEFC.
- They have successfully developed a power inverter for this application.
- Must inform about others efforts made in the different laboratories, mainly about the problems associated with SOFC. It is difficult to evaluate the progress toward goal because of the problem to purchase a SOFC. The contribution of this project on the hydrogen program is not clear. No information about the cost.

- The prime is not an SOFC developer and has been unable to obtain an SOFC. This does not invalidate the project but is undesirable. The substantial power electronics work is excellent but is unfortunately not a desirable approach to a 3 phase system required for the application. Development of an integrated fuel cell/battery system is excellent work. Significant work has occurred; unfortunately it does not appear that the project has achieved progress toward goals.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- They are working closely with a commercial vendor of truck refrigeration units as well as a trucking company that is a potential commercial customer of such units. Being unable to obtain the desired SOFC in time, they have obtained two stacks from a commercial PEFC developer. All of these entities (and their relevant industries) stand to gain from successful development and commercialization of this technology.
- Actually, the developed system is tested on board but PEMFC is used. There is no indication about the technology transfer, yet. Must inform about others efforts, using SOFC, made in different laboratories.
- Carrier Chevron.
- In principle an excellent collaboration that provides the right expertise. It is not evident from the work that the collaborators are significantly involved.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- For the rest of this project, they will complete system assembly and testing, first on the bench, and subsequently, on a truck.
- Even though this falls short of operating the fuel cell APU on diesel fuel, the project will still generate useful data on configuring a hybrid fuel cell / battery / internal combustion engine power plant for trailer refrigeration.
- Despite some false starts the work plan does recognize what must be done. If completed the work will result in an installed and integrated system.

Strengths and weaknesses

Strengths
- The project plan was well laid out. They were able to reconfigure the project to still obtain useful information on a fuel cell-based APU, even though an SOFC has not become available. The type of fuel cell used is, of course, a very important consideration; there is still a wealth of useful information that can be obtained from the balance-of-plant as well as on power conditioning, duty cycles, etc.
- Since the development by using PEMFC was done, the comparative study with the system using SOFC is interesting.
- In principle an excellent collaboration.
- Good overall work plan.
- Integration in a truck will be a significant achievement.

Weaknesses
- This project needed to realize early on that generating fuel cell quality fuel gas from the truck's on-board fuel (diesel fuel) would be a key consideration that has to be addressed. Running a fuel cell APU on hydrogen has been demonstrated earlier (by the same organization). Even with an SOFC, the need for a diesel fuel processor would not have been obviated.
- It would have been helpful if they had identified a potential partner to address that issue.
- Interactions.
- The lack of an SOFC and development of power electronics not suitable for a three phase system has hurt progress.
- Shock and vibration issues are not considered.

Specific recommendations and additions or deletions to the work scope

- This project has been of value in identifying non-fuel cell issues, such as the power conditioning needed, the hybrid architecture options that may be considered, and the need to work closely with commercial vendors that can bring a wealth of relevant practical information.
- The key issues that remain are how to get from diesel to "clean" reformate, and whether the low-sulfur diesel is low enough in sulfur for such systems to be practical. Follow-on activity, either with this organization or with other researchers, should address these two issues.
- Include comparative study between PEMFC and SOFC.
- Must include safety codes in the project.
- The project should utilize the shock and vibration analysis methodology under development at PNNL.

Technology Validation
Summary of Annual Merit Review Technology Validation Subprogram

Summary of Reviewer Comments on Technology Validation Subprogram:

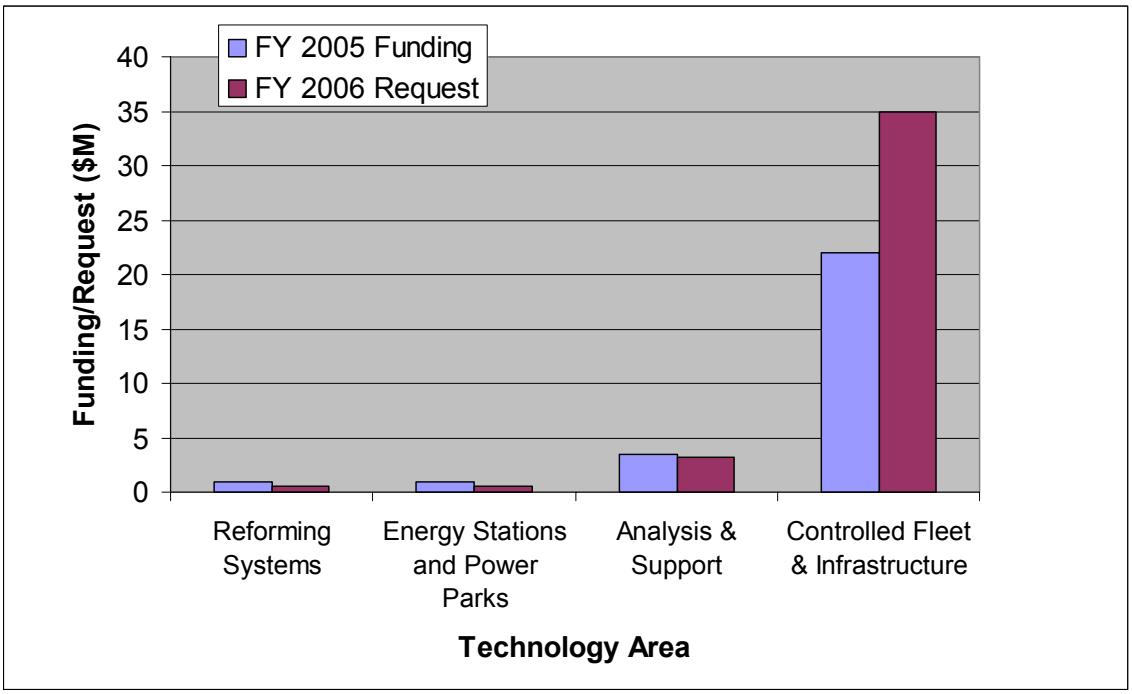
Reviewers identified the technology validation of key hydrogen energy technologies and systems to be an essential component of the Hydrogen Program mission and critical to the President's Hydrogen Fuel Initiative. The program results were seen as providing an important data base. The projects were considered to be appropriately diverse and strongly focused on addressing key targets of RD&D objectives across several options such as natural gas to hydrogen stations, power parks, energy stations, and learning demonstrations. Reviewers noted that the projects will collect a critical data base for the operation of fuel cell vehicles, delivery of hydrogen and the production of hydrogen and that the information dissemination will provide a confidence level for the public. The analysis efforts that supported the learning demonstrations and the power parks were seen as instrumental to DOE making program decisions. Both principal investigators of the analysis effort had a commendable appreciation of what data is needed and had successfully structured the means of getting it through cooperative participation.

Reviewers thought highly of the Technology Validation Subprogram's approach of conducting large ***learning demonstrations*** that emphasize co-developing hydrogen infrastructure in parallel with hydrogen fuel cell-powered vehicles. They felt it lends credibility and is of paramount importance to the research efforts and are vital to support a 2015 industry commercialization decision. Reviewers indicated that the California Highway Project (an earmark) should be placed in coordination with automotive OEMs and DOE's learning demonstration projects in the future. Technology validation of hydrogen energy systems that crosscut into all technology R&D subprograms is an important facet of the Hydrogen Program. As such, there is a need for strong coordination between these subprogram elements and a clear feedback loop to ensure that lessons learned are translated to next generation technology designs in the shortest possible timeframe. Reviewers also expressed the need that detailed project results, failure modes, root causes, etc. be fully shared with the rest of the program and in particular the Safety, Codes, and Standards Subprograms to ensure that safety and liability issues are sufficiently linked and adequately addressed. The reviewers also iterated that proof of economic viability is an important part of technology demonstration. The reviewers felt that power parks were a good demonstration of integrated systems for transportation, infrastructure and electric generation in real world operating conditions. There were concerns expressed about better focusing some of the efforts and the need to minimize some of the options being considered. There were diverse reviews of high temperature fuel cell energy stations. Some of the reviewers felt it was an important concept to future low-cost hydrogen production and a good transition concept, but one reviewer felt it was a re-invention of a 10-year-old government program with little new to offer and was proprietary to the team investigating the concept.

Congressionally directed projects received lower scores and had greater negative comments attributed to them than the competitively-selected projects. The previous year's comment by reviewers that felt that it would be important to have specific information on the project funding level and schedule of activities appear to have been addressed this year. However, there were significant critical comments expressed about assuring that the projects were addressing DOE targets, especially for Congressionally directed projects.

Technology Validation Funding by Technology:

The funding portfolio for Technology Validation addresses the need to validate integrated hydrogen and fuel cell technologies for transportation, infrastructure, and electric generation in a systems context under real-world operating conditions. The FY 2006 funding profile (subject to Congressional appropriation) addresses key aspects of the Hydrogen Program mission and the cross-cutting issues associated with the National Academies' Report and system integration activities.



Majority of Reviewer Comments and Recommendations:

In general, the Reviewer scores for the Technology Validation Subprogram were on average with those of the other subprograms (the maximum, minimum, and average scores for Technology Validation projects were 3.63, 2.32, and 2.87, respectively). These compare to the overall maximum, minimum and average project scores of 3.92, 1.55, and 2.91, respectively. The major recommendations for the Technology Validation Subprogram are summarized below. DOE will act on reviewer recommendations as appropriate for the overall Hydrogen Technology Validation effort.

- **Power Parks Analysis** – Focus on making available data public and be careful in some of the projects to focus efforts on meaningful technologies. Continue integrated system analysis in support of these projects and making economic projections.

- **Energy Station Demonstration/Analysis** – Could be a potentially attractive option for the low cost production of hydrogen. Be careful to use the best technology and not investing in mature proprietary systems.

- **Vehicle/Infrastructure Learning Demonstration** – Focus activities to ensure that lessons-learned become public information. Emphasize the dissemination of information to research and development program and the public. Continue analyses in support of these projects and secure data room to promote information collection and dissemination.

Project # TV-01: Technology Validation Sub-Program
Gronich, Sig; U.S. Department of Energy

Brief Summary of Sub-Program

The purpose of this Technology Validation Sub-program Overview is to describe goals/objectives, budgets, barriers/targets, approach to R&D, technical accomplishments, interactions and collaborations, solicitations and awards, and future directions. Projects in this sub-program area set the stage for providing a validation of the technology status and unique system configurations to meet cost and performance targets and an important feedback loop to manage the R&D programs.

Degree to which the Sub-Program area was adequately covered and/or summarized

- A very clear overview was presented with goals and targets.
- Very thorough.
- Covered all areas adequately.
- Excellent.
- Presentation very effectively and comprehensively covered all aspects of the Technology Validation activities.
- Excellent overview of sub-program describing breadth of activities.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- Significant challenges were discussed, with plans and projects intended to address each.
- Some important issues were identified but it is not obvious that all significant issues are noted.
- Yes, very well.
- The tasks outlined are very well placed to supply operational, real-world duty cycle data that is essential for moving towards commercialization.
- Technical barriers were presented and described very well.

Does the Sub-Program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- The area is well-managed, with a project portfolio that addresses the goals and targets.
- Overall goals are focused but too many details were described about projects rather than putting them all in perspective.
- Yes, very well.
- The plans have been creditably formulated, and implementation is progressing on schedule.
- DOE management of this sub-program is limited by Congressional Directives, which generally are not supportive of the President's Hydrogen Initiative.

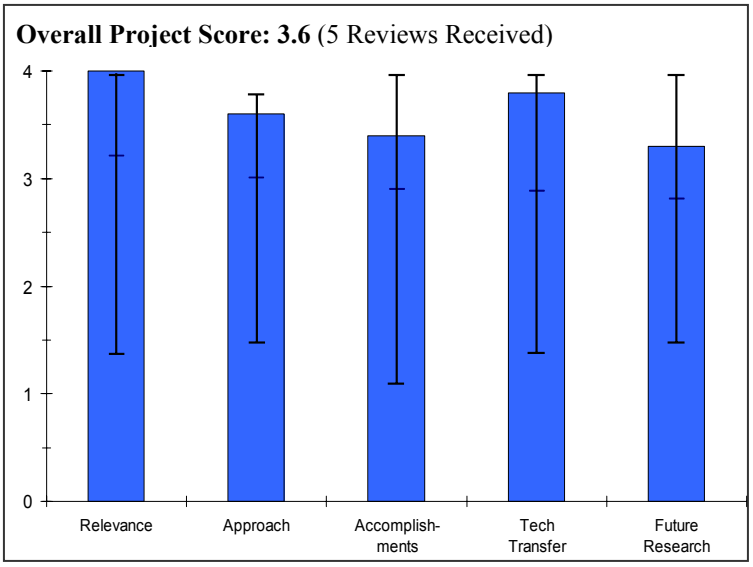
Other comments:

- Extremely well done. Very impressive.
- The Technology Validation Sub-Program is a much needed addition to DOE's hydrogen and fuel cell program.
- Subprogram objectives and plans are excellent, and DOE is doing a good job of integrating Congressional Directed activities.

Project # TV-02: DTE Energy Hydrogen Technology Park
Regan, Rob; DTE Energy

Brief Summary of Project

In this project, DTE Energy will develop and test a working prototype of a hydrogen-based power park concept that utilizes a combination of renewable and non-renewable power (including on-site solar) with electrolysis and stationary PEM fuel cell technology; the system will 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.



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- Technical and safety issues were properly addressed.
- Results align fully with DOE targets.
- PI must consider analyzing economic aspects of co-production in more depth, like as estimated cost for end consumer.
- Excellent demo of integrated system for transportation, infrastructure, and electric generation to be operated in real-world operating conditions.
- Directly applicable.
- Project is aligned with multiple HFCIT program goals.
- Involvement of utility company in HFCIT power park technology validation is highly appropriate.
- Program addresses key targets of RD&D objectives and demonstrations will be critical to realization of the President's Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- Barrier C was not fully addressed, as the project has not investigated hydrogen and fuel cell electricity costs.
- The solar power system appears to be of little value to the demo.
- Looks highly focused.
- Ambitious integration of many elements.
- Program systematically addresses key technical objectives.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- Very good results were found.
- Technical weaknesses were fully mitigated.
- Electrolyzer not yet operational.

- Good progress, next year should yield a lot of data.
- Getting this multi-element project operating was a significant accomplishment.
- Presentation should include more specific operating data examples.
- Program has successfully met or is aggressively addressing objectives in an organized and methodical fashion.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- The participation and the role of each partner was fully explained.
- Excellent team with tie to vehicle validations.
- Expanded participants.
- Project should emphasize near-term transmittal of specific operating data to other HFCIT program activities.
- Program data has been published repeatedly in last year.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Future work seems correct and coherent with whole project targets.
- PI to consider including economic aspects, mainly hydrogen and electricity production costs.
- Next phase should yield a lot of good data.
- Plans should increase emphasis on data generation and sharing.
- Goals consistent with meeting program objectives.

Strengths and weaknesses

Strengths
- Very useful demo project with excellent team.
- Use of commercially-available hardware. Good collaboration with partners.
- Multi-element project will provide early technology validation data.
- Utility company involvement and perspective is valuable.

Weaknesses
- Solar power system appears to add little value to the project.
- Geographic location not ideal for renewable resources.
- Needs more emphasis on data sharing.

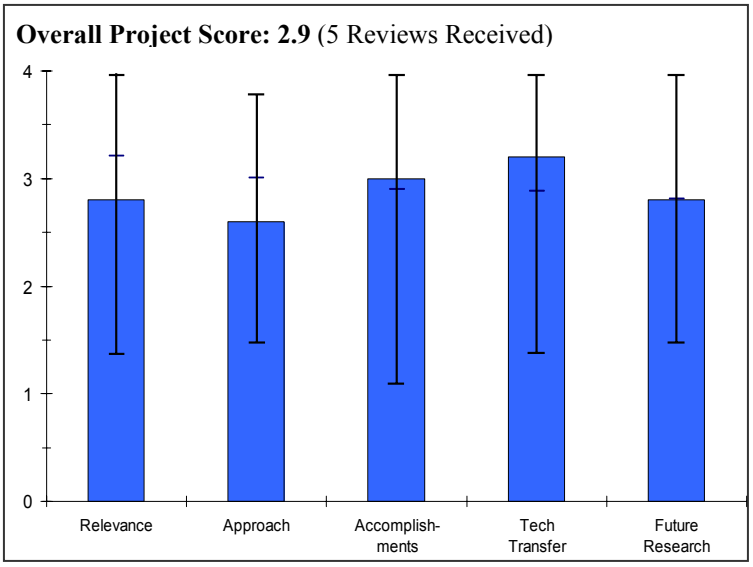
Specific recommendations and additions or deletions to the work scope

- Include hydrogen and electricity production cost estimation.
- None.
- Ensure that specific quantitative operating data will be published and available to all legitimate researchers (not just SNL).
- This program appears near completion. Support of controlled fleet should be continued but does not appear to be a major effort.

Project # TV-03: Hawaii Hydrogen Center for Development and Deployment of Distributed Energy Systems
Rocheleau, Richard; Hawaii Natural Energy Institute

Brief Summary of Project

On this project Hawaii Natural Energy Institute (HNEI) will develop and operate a test bed for validation and characterization of hydrogen technologies in a real world setting; characterize the effect of trace level contaminants on the performance and durability of PEM fuel cells; develop novel multi-terminal device configurations which offer potential for higher efficiency photoelectrodes for solar hydrogen production than achievable with current thin film approaches; and investigate critical steps for hydrogen production from biomass, including biomass and syngas conditioning/cleanup, optimal pathway assessment, and characterization of selected biomass gasification technology.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- The project aims to validate different technologies, which is going to help DOE to decide between alternative approaches for the hydrogen economy.
- Comprehensive systems.
- Targets/goals not clearly described or quantified.
- All elements are relevant but effort is dispersed.
- Addresses HFCIT program objectives.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- An approach for cost analysis should be considered.
- An approach for comparison between technologies, including cost, performance and best niches for utilization should be considered.
- Leverage DOD investment and data collection, with intensive guidance from industry.
- This project is a set of four largely unrelated efforts, none of which appear to be unique or add much value for the program.
- Individual areas seem focused but they are too diverse i.e., 4 projects. Some projects seem to get too detailed i.e., evaluate MEA's rather than just keep to fuel cell systems.
- Good leveraging of funding.
- PI did not explain if or how the four project elements are integrated.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Many good results were achieved.
- PI responded adequately to previous year comments.

- Data reproducible.
- Largely some simple tests of commercial equipment.
- Good progress. Needs more data generation.
- Good progress considering delay challenges.
- PI should present more explicit planned vs. actual accomplishments schedule.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- The transfer of protocols for fuel cell building, operation and testing will improve the quality of results.
- Working with small business.
- Several partners were indicated, with unclear roles for some.
- Good collaboration with many others. Good leverage of other efforts i.e., DOD and State.
- Excellent example of early data generation and open data dissemination.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- Recommended modifications were fully addressed.
- Inclusive approach.
- Goals/objectives are unclear.
- It is a logical progression but should be narrowed to concentrate on fewer projects.
- Plan is basically to overcome challenges and to continue work on four relatively unrelated sub-projects.

Strengths and weaknesses

Strengths
- Transfer of fuel cell manufacturer's protocols. Hard to negotiate with them and very important for technology validation.
- Diversity of hydrogen production technology is a strength if the results can be compared and indicate best utilization of each one.
- Responsive to last year's peer review.
- Good natural renewable resources. Good collaboration with others. Central point for several organizations.
- Excellent initial data generation and dissemination.

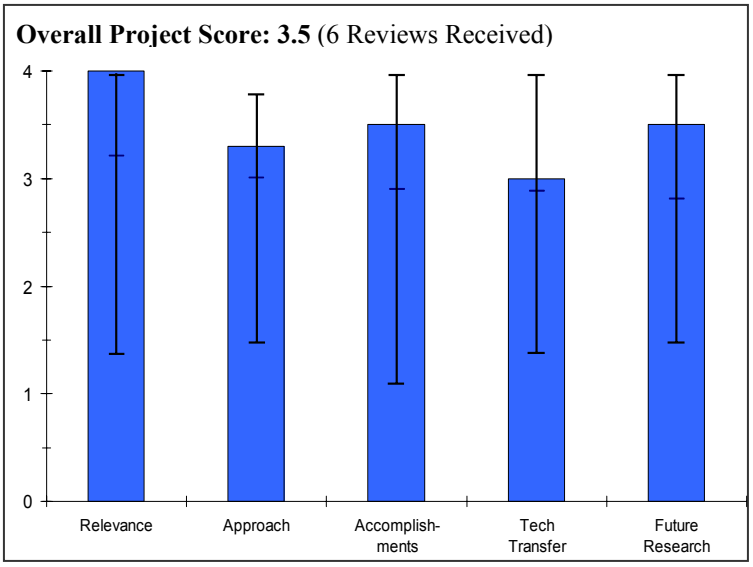
Weaknesses
- Diversity of hydrogen production technology demands significant project management resources and can delay activities.
- Requires a lot of management.
- Diverse set of projects without clear and quantified goals and objectives.
- Don't have a lot of fuel cell construction technology experience required for fuel quality evaluation.
- Overall project appears to be composite of four relatively separate sub-projects.

Specific recommendations and additions or deletions to the work scope

- Include cost analysis approach.
- Include approach comparison between different technologies.
- Continue project- good value.
- Do not fund this project.
- Suggest deletion of biomass effort. Suggest focus fuel quality effort on overall system not cell or cell element.
- Continue as planned and consider dropping a sub-project if funds are needed for completion of other sub-projects.

Project # TV-04: Hydrogen Power Park - Business Opportunities Concept Project
Hobbs, Raymond; Pinnacle West Capital Corporation

Brief Summary of Project



In this project, Pinnacle West Capital Corporation conducted field testing of several hydrogen power park components including electrolytic production systems, compressors, and dispensers; photovoltaic energy supply systems; hydrogen ICE engine generator sets; fuel cell generators; and hydrogen ICE vehicles. Results of these tests are being utilized to evaluate the economics of four hydrogen power park configurations. Aspects of these configurations include hydrogen production, renewable energy opportunities, integration of distributed generation and transportation fuel production, incorporation with existing energy assets, scalability, and integrated business opportunities. Analyses will identify technical barriers and market opportunities for each hydrogen power park configuration.

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- The project aims to validate different technologies, which is going to help DOE to decide between alternative approaches for hydrogen economy.
- The project aims to use local renewable energy sources.
- Find value proposition in the real world.
- Excellent technology validation with real-world operating conditions.
- Excellent facility with real results.
- Project addresses multiple HFCIT program objectives.
- Utility company involvement is highly appropriate.
- Program addresses key targets of RD&D objectives and supports the President's Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- Cost analysis was done.
- Analysis using utility techniques very comprehensive. APS demonstrated an ability to effectively engineer power park components and put the unedited info on the web.
- Size of equipment is small but a good first step.
- Some of the project components do not relate directly to the HFCIT program mission.
- Project work appears to be executed very carefully.
- Approach establishes models for a power park and then compares the models to real experience in operating a power park.
- It is not clear that models will be valid for other locations and a means to test models for different climates should be developed.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- Good cost analysis.
- Project results address DOE cost targets even if not achieving efficiency target.
- DOE has to analyze if previous result impacts global DOE targets.
- Successful operation.
- Good accomplishments relative to DOE funds consumed.
- Difficult to understand the energy cost to produce hydrogen. Only energy cost reported. No capital or installation cost included.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- PI has not shown partnerships.
- If there are partners, PI must show them and explain role and coordination.
- If there aren't PI must consider including FC and equipment manufacturers, and also other partners, like the community.
- Regulatory: the hydrogen station is in an historic building which shows great collaboration with local officials.
- Should expand promotion and exposure to public nationally.
- Valuable real-world data being generated.
- Perhaps raw performance data dissemination should be increased.
- Important to clearly state conditions/assumptions when announcing general conclusions from project.
- Collaborating with large group.
- Did not see references to publications.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.5** for proposed future work.

- Other hydrogen sources addressed.
- Continue because APS provides hydrogen fuel at posted prices to the public at gge economics- a powerful example for other utilities.
- Future plans are too broad if they expand to include biomass, bromine cells, and Mobile facility.
- Appears to be essentially "keep on doing the same".
- Expanding efforts to include other technologies appears valuable.

Strengths and weaknesses

Strengths
- Different hydrogen sources.
- Cost analysis.
- Safety. Availability, 26K hours of calendar operation. Great data that is very transparent.
- Useful demonstration project with real-world operating conditions.
- Good commercial experiences.
- Important power park technology validation project.
- Provides beneficial utility company involvement and perspective.

Weaknesses
- Lack of partnerships.
- Didn’t include all data analysis, i.e. didn’t include capital costs of fuel cells.
- Some project elements are not specific to HFCIT program mission.

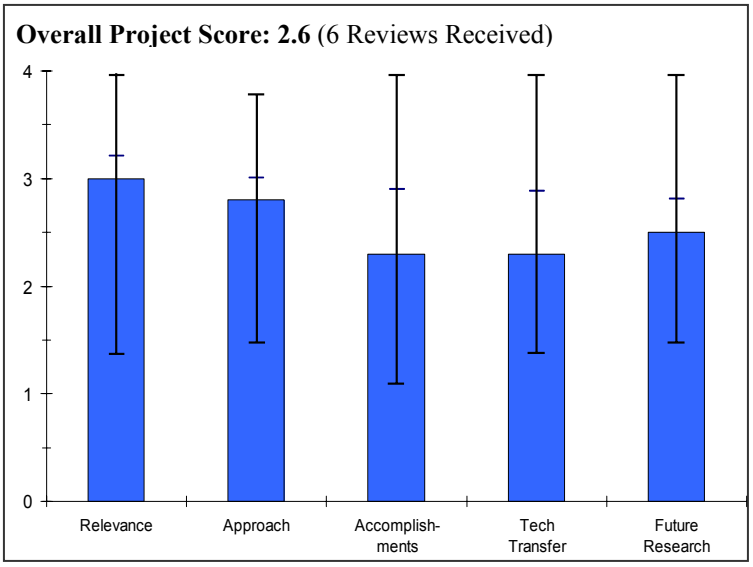
Specific recommendations and additions or deletions to the work scope

- Include cost reduction strategy, mainly for PV based hydrogen production.
- Must continue. Replace electrolyzer. Replace fuel cell.
- Continue as planned.
- Ensure that basic performance data as well as overall conclusions are broadly disseminated.
- Need a better explanation on energy cost to produce hydrogen using electrolysis from grid.
- Care should be taken with Bromine cell for future work; hazardous material.

Project # TV-05: Validation of an Integrated System for a Hydrogen-Fueled Power Park
Keenan, Greg; Air Products and Chemicals, Inc.

Brief Summary of Project

Air Products and Chemicals, Inc. (APCI) 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. They will optimize the system for lowest total energy cost, and develop a cost estimate to demonstrate a prototype natural gas based energy station at a suitable site. Safety will be the top priority in the system design and operation.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Using high temp fuel cell seems like political positioning. Copies the work of many fuel cell firms.
- Useful tech validation project using high-temperature fuel cell. Clear relevance to DOE targets.
- Goals seem marginal with only 10% hydrogen recovery. Low efficiency improvements.
- Producing low cost hydrogen may well entail the co-production of electricity, although there are many complexities in scheduling the co-products, regulation of electric supply, etc. This project addressed this important energy station option to future hydrogen supply.
- Program addresses operation of a molten carbonate fuel cell with off-gases going to hydrogen and utilizing heat for CHP type applications.
- Program seems better suited for Office of Fossil Energy and it is not clear why it is in this activity.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- Good and innovative solution for efficiency improvement.
- Simplistic. Relearned obvious information from past MCFC. Spent 2 years looking at PEM than switched to SOFC and /or MCFC using unclear criteria.
- Barriers are addressed in the presentation but the substance of the presentation doesn't flow from those barriers.
- This is a solid team.
- The molten carbonate fuel cell approach with the waste hydrogen it produces is an interesting option that deserves investigation.
- Approach does not supply details of how program would be applied. No indication of cost of installing CHP system, for example cost of chiller or ORD.
- Benefits described for program must be meant to justify the approach.
- Not clear that approach analyzes the balance of load demand, i.e. power, hydrogen or heat loads. The approach does not identify which load would be the primary design point.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- Most of demands made previous year were satisfactorily addressed.
- Lacks partnership detailing.
- Baloney! The numbers presented were theoretical and not bottoms up analytical- therefore simply repetitive of DOE publications. Two years to think about fuel cells is unacceptable.
- Phase 1 required. Too long for completion.
- Still early in the program.
- DOE cost goals are appropriately central to this project.
- If economically and operationally successful, this project will provide an important option for companies with electric and hydrogen co-production in their business plan.
- Results are presented but maybe not in a way that other companies and researchers can gain understandings that are likely to be useful to them. This is not unusual for a project still in the design stage.
- To operate with excess hydrogen at the anode, excess methane must be introduced to the MCFC. Because of internal reforming, the endothermic reforming of the excess hydrogen will cool the fuel cell and take it off of peak performance, possibly lowering the efficiency.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- Partnerships need to be detailed.
- Role of partners must be addressed.
- Molten carbonate is mature and overanalyzed: the chosen FuelCell Energy DFC-300 has been studied at length.
- There appears to have been little collaboration to date.
- This is not happening much yet, except for vendor involvement, but this technology transfer and collaboration is planned in Phase 3.
- Publication of interim results that would help others begin comparable projects should be considered.
- Presentations given but could not find whom collaboration was with.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- Activities seem reasonable, but PI needs to detail the targets and confidence level in achieving them.
- Desire more tax dollars to incrementally improve hardware that is protected by FCE. Not worthy of continuation.
- Seems like we need to focus on fuel cell output first, then a secondary issue such as hydrogen recovery.
- This project has a high probability of technical success.
- The results need to indicate whether this is an economically-viable approach to hydrogen production in the long term.
- Engineering analysis is critical for this project. This should be major function before design work is completed.

Strengths and weaknesses

Strengths

- Technical solution for co-production is innovative and can strengthen technology deployment.
- Was honest enough to admit this was not an original idea.
- Very useful technology validation project.
- Good expertise in gas processing.
- This is a strong project team and addresses the need to better understand the potential for this method (molten carbonate fuel cell) of co-production of electricity and hydrogen in distributed settings.

Weaknesses

- None of importance in context of the activities of competent fuel cell manufacturers and integrators. An empty project in terms of benefits to hydrogen community. Re-invention of molten carbonate fuel cell system that was fully described and analyzed using government funds over 10 years ago. This program is a great example of selling a used concept that belongs in the realm of industry. The program benefits no one but APCI and FCE, which own the patents to commercialize this system. (Arrogant position on sharing information.)
- Project has had a slow start, with an excessive length of time spent on phase 1.
- Marginal advantages for this process.
- Communication of more results to project developers, even at this early stage, would be useful.

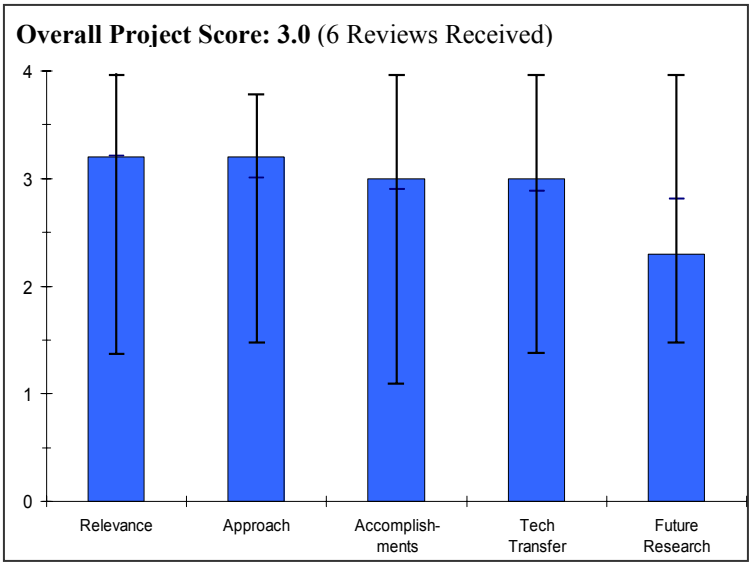
Specific recommendations and additions or deletions to the work scope

- Detailing of partnership and future work.
- Immediate cancellation is most appropriate. Public condemnation as waste and abuse of DOE funds. This is a blatant attempt to use government money for incremental product/system improvements to the sale benefit of APCI and FCE.
- Concentrate on each element i.e. FC and purifier.
- Since design and operation will need to evaluate the dispatch and role of the separate outputs (electricity and hydrogen, and heat), insight into this analysis and the results for future project developers will be important.
- Recommend independent team evaluate the energy and mass balance of this system and additional capital cost for CHP and hydrogen collection/storage prior to further design or construction of system.

Project # TV-06: Novel Compression and Fueling Apparatus to Meet Hydrogen Vehicle Range Requirements
Carlson, Todd; Air Products and Chemicals, Inc.

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. An isothermal compressor concept was designed, simulated and tested. High pressure automatic valves, 900 bar storage valves for cascade, flow meter, dispensing equipment and other instruments were also investigated for achieving this objective.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Time for fueling hydrogen into vehicles is an important barrier to be overcome.
- Improved compression efficiency and lower cost are important considerations in hydrogen systems.
- This narrowly defined project can potentially provide incremental benefits for a small portion of a "hydrogen economy."
- Hydrogen compression is important -- 1000 psig.
- Very relevant to hydrogen program, with significant potential benefits if successful.
- The advantages of this design are not obvious. While novel, alternatives are available.
- Improving the economics of hydrogen pumped into a vehicle tank is critical; the goal of this project is critical.
- This project is important for exploring better ways to produce high pressure hydrogen.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Barrier is very specific and the project is well focused.
- Design issues to be addressed, for compressor and fueling stations, have been identified.
- The approach is somewhat determined/limited by what the auto industry will accept, e.g., in range.
- In third year of project. Clear focus on high pressure. Tied to hydrogen fueling station business expansion.
- Focus on making a compressor that has large advantages over conventional compressor, i.e., cheaper and more efficient.
- The compressor approach is innovative and creative.
- This project recognizes the roles of codes and standards and their incomplete development, and possible codes and standards development too early for the results of this project.
- The relationships between the project approaches taken and cost goals are not articulated.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Problem analysis was adequately addressed.
- Pressure analysis indicates the need for hydrogen cooling, which can be a problem for fuel costs and safety.
- PI must indicate feasibility more clearly.
- New valves and pressure switches have been developed.

- Isothermal and single stage design objectives have mostly been achieved.
- Information on the cost baseline that progress is being measured against was not included in the PI's presentation.
- 700 bar target established by car firms. Competent in technology.
- The proof will be when the unit is assembled and tested.
- Technical accomplishments are well presented at the high level necessary in this meeting.
- Analysis and design details for future projects would be expected in a more detailed report.
- The expected efficiency and cost results should be provided.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Partners were not shown.
- If there aren't partners, PI must consider the need of them and explain if they are not needed.
- A number of collaborating companies on this project.
- Meaningless list of component suppliers that have all signed NDA's with APCI is misrepresentation of "collaborations."
- Excellent project team.
- Should get a compressor manufacturer involved.
- The presentation explained a lot of the decisions made and the technology selections.
- Conveying this initial analysis and design information in a way that others following in their tracks can understand will be important.
- The role of auto manufacturers in determining storage pressure presents interesting issues that were discussed.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Next steps are quite adequate.
- PI must detail tasks and indicate technical targets and solutions.
- Future work touched on only briefly.
- No indication of costs for future work.
- Assemble and test, case determination, marketing, and T&E for warranty all valid. Taxpayer support for their marketing effort is inappropriate. That cost element should be isolated and eliminated.
- Limited discussion of issues that might be encountered during remaining steps was presented.

Strengths and weaknesses

Strengths
- Focused project aiming to solve specific and important technical barrier.
- Technologically competent staff.
- Very relevant technology validation project with significant potential benefits.
- Broad experience with other fueling station activities. Broad experience with handling high pressure hydrogen.
- The safety considerations and experience are possibly the best of the projects.
- Air Products knows compressor technology very well and is using it in this project.
- This presentation was outstanding.

Weaknesses
- Part of presentation devoted to review of Air Products hydrogen history/experience. Not relevant for project review.
- In regards to compressors, there are several competent suppliers, most of which would not obviously agree to APCI's proprietary rules and market defense strategy. It is beyond reason for DOE to support this project to help APCI become vertically integrated to the detriment of many smaller businesses.

- No major technological advantages noted.
- There is not much to complain about.
- Tighter linkages between approaches taken and cost goals would help share understanding to future users of this information.

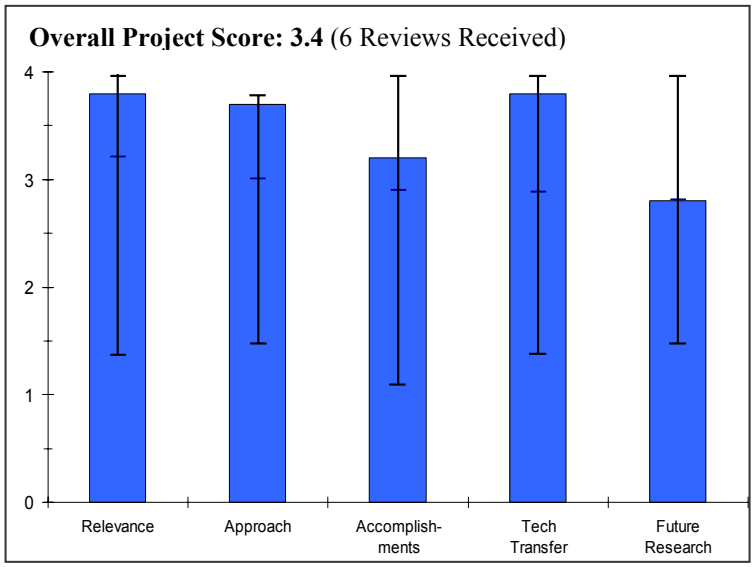
Specific recommendations and additions or deletions to the work scope

- Details and addressing of technical challenges are needed.
- More confidence on technical feasibility is needed. Suggest a risk analysis of target achievement be conducted.
- APCI should be clearly directed to share the vendor development info with other stakeholders.
- Suggest close evaluation of final test results before continuation.
- None.

Project # TV-07: Controlled Hydrogen Fleet & Infrastructure Analysis
Wipke, Keith; National Renewable Energy Laboratory

Brief Summary of Project

Under this multi-year validation project the National Renewable Energy Laboratory will assist DOE in demonstrating the use of fuel cell vehicles and H2 infrastructure under real-world conditions, using multiple sites, varying climates, and a variety of sources for hydrogen, including renewables. The primary activities over the last year were to develop data templates for fuel cell vehicle and infrastructure data, and then establish a Hydrogen Secure Data Center to store and analyze this highly sensitive data. Initial vehicle data has been received and analysis procedures are being developed. Future activities will include analyzing the data to obtain maximum value for DOE and industry from this "learning demonstration," feeding back key non-proprietary findings into the HFCIT program R&D activities.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Centralization of data from demonstration projects will be very helpful to DOE making go/no go decisions
- Project provides key support for, and credibility of, large validation projects that are vital to eventual commercialization.
- Hydrogen Secure Data Center seems a good idea to break through the sharing barrier.
- A cornerstone project.
- The value of providing 'real world' data analysis is of paramount importance as a feedback loop to DOE's programs.
- It is necessary for the government to perform validation of technologies to determine if the Program is on track to succeed.
- This validation in itself is not R&D, at best; it will verify that the targets have been met.

Question 2: Approach to performing the research and development

This project was rated **3.7** on its approach.

- Project will allow high level system analysis.
- Data management is well addressed.
- Note: Not R&D.
- Design and establish system for acquiring, analyzing and securing industry data.
- Approach has resulted in successfully negotiating agreements with validation contractors.
- Takes advantage of prior analytical tools and NREL working relations with industry.
- Comprehensive data collection. Template for providing data and analysis.
- All key players are involved.
- Range of topics and aspects well covered.
- The PI has a commendable appreciation of what data is needed and has successfully structured the means of getting it through cooperative participation.

- This project will not contribute to overcoming the barriers, but it appears that data is being gathered to track progress on most of the barriers and will provide feedback on technology shortfalls.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Explanation on timeline is not clear. PI has mentioned start at FY03 and project being 15% complete, but timeline starts Feb 7th 2005.
- For this timeline (Feb 2005) addressed accomplishments seem adequate, but for a project starting on 2003 results seem too modest.
- Created data templates and got approval by teams.
- Established Hydrogen Secure Data Center at NREL.
- Gained agreements from industry on data handling and security.
- Legalities of data transfer. Automation of statistical analysis. This project could move faster by using concurrent tests.
- Methodology is very good.
- Need to maximize data sharing.
- Getting the NREL Secure Hydrogen Data Center in place was and important achievement. Hopefully the data received will truly 'raw' and not in any way 'messaged'.
- Achieving agreement on data to be collected and reported is significant.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- Interaction with partners was well addressed.
- PI has demonstrated partner's role and cooperation on project.
- Must consider strategy to use data analysis to find solutions to development barriers faced by the partners.
- Commendable collaboration, interaction, negotiation with companies on validation teams.
- Project implementation included participation in California hydrogen activities.
- Very narrow. Seems non-applicable.
- Need effort to provide public relations story to public to sell hydrogen safety.
- Full participation has been ensured by this well planned venture.
- Certainly a good representation of industry partnering in this project.
- It is not clear that technology shortfalls will be communicated to other research institutions in a way that those institutions can assist is solving the problems.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- It is missing a mechanism to measure impact of data analysis on technology development (e.g.: how useful data analysis will be for reduction of hydrogen price per mile).
- Work to be done in years following FY 2005 is stated in general terms.
- Must stay independent and report the bad and the good.
- This project will be of outstanding interest and value over the next year.
- Go/no-go milestone criteria must be quantified.

Strengths and weaknesses

Strengths
- Ability to work constructively with validation teams to acquire data that will be important to future hydrogen program direction, and which is in the public interest.
- Ability to make agreements with team's lawyers. Answered last year's reviewer's comments.
- Well planned and operated.
- Great analysis capability planned.
- Great coordination, planning and implementation.
- Achieving agreement with manufacturers to make public composite data available to DOE and the public.

Weaknesses
- Lack of clarity of how the HSDC assures a meaningful data sharing with stakeholders.
- None.
- None evident.
- Lack of plan/process to provide technology shortfall information to research institutions.

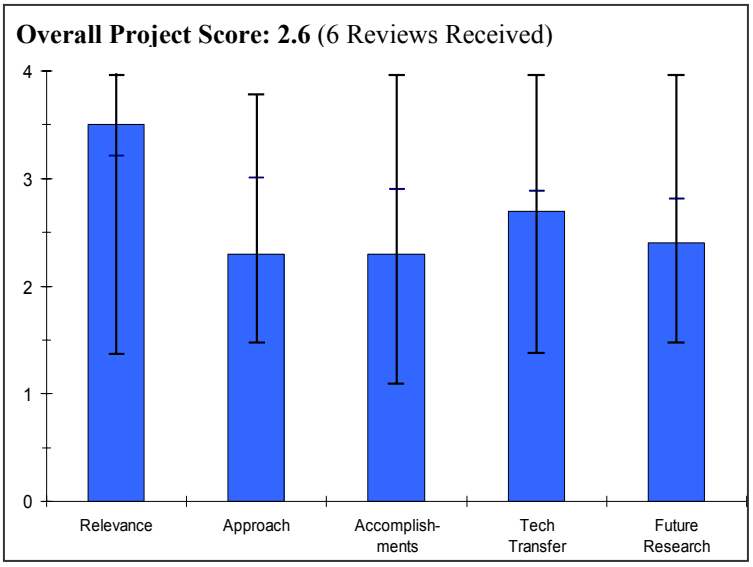
Specific recommendations and additions or deletions to the work scope

- To develop mechanisms for measuring impact of data compilation and analysis on technology development.
- To develop mechanisms for identification of solutions available in academia or national research centers.
- Safety confidence from aggregate experience should be prioritized as a communications product(s).
- Leave an entry opportunity for the small entrepreneur who may contribute new key technology unexpectedly.
- Ensure, to the extent possible, that both the data received, and its subsequent analysis remains 'spin-free' of stakeholder influence.
- Establish a plan for providing technology shortfall information to research institutions in a form that will permit them to propose projects that will lead to solutions.
- Establish quantified criteria for Go/No-Go decision milestones.

Project # TV-08: California Hydrogen Infrastructure Project
Pedersen, Mark; Air Products and Chemicals, Inc.

Brief Summary of Project

This project is focused on demonstrating a variety of flexible real world hydrogen fueling stations within California for use by fuel cell and internal combustion engine light-duty vehicles. The project will be performed with the support of state agencies, several automobile manufacturers and energy companies. The stations will offer various cost-effective infrastructure models that may lead to potential pathways in a fully developed hydrogen economy.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Better definition of hydrogen infrastructure costs is important.
- Project needs more planning to realize the potential benefits for DOE's goals and to get the full value from the significant expenditure involved.
- None.
- This project directly addresses the DOE cost goal of lowering fueling costs.
- The project explores a variety of fueling options which should yield cost results across the spectrum for different fueling options.
- Availability of affordable hydrogen is a critical factor in the commercialization of FCV's. This project should indicate what is possible.
- Project is focused on one of the important parts of the DOE program, which is hydrogen infrastructure costs and availability.
- Attacking delivered hydrogen cost is probably the most important activity an organization could undertake.

Question 2: Approach to performing the research and development

This project was rated **2.3** on its approach.

- Will document a number of infrastructure technologies, in terms of performance, but project evidently does not include efforts to improve or optimize alternative approaches to delivering hydrogen.
- Variety of fueling approaches will be covered, as will variable climate conditions.
- Total of 14 separate sub-projects. Work needs to be done on how best to compare results among them.
- A data acquisition program being established.
- Not clear just what is expected to be learned. More defined detail in expectations is needed.
- The project approach presents an array of items (14 projects) being explored without clear focus or explanation of the interrelationships.
- It is not clear how the project approaches will actually reduce costs.
- Good coverage of the different hydrogen supply options available for vehicle refueling stations.
- The objectives slide states "we will gather infrastructure and fueling experience data as input to an infrastructure recommendation," but the data to be collected is not defined and an approach to making the recommendation is not clear.

- Data should be provided to Secure Hydrogen Data Center to be compared to infrastructure from learning demonstration.
- Approach is focused more on data collection and tradeoff studies rather than research to reduce hydrogen delivery cost.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.3** based on accomplishments.

- Project work not yet underway.
- Milestones for FY 05 and 06 are very general. e.g., one says only "150 hydrogen fueling stations in Southern California."
- None.
- This project has not begun yet.
- Early days yet.
- New project, so accomplishments not expected yet.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- Air Products is lead. Many "big name" collaborators.
- It is anticipated that there will be extensive cooperation/involvement of collaborators, but that topic was not covered in PI's presentation.
- Cost reduction opportunities should be highlighted as a "tech transfer" item.
- Included is a strong list of collaborators which helps with the technology transfer.
- Collaboration with National Center for Fuel Cells at UC-Irvine is a plus for the analysis planned, although UC-Davis may have more expertise on fueling options.
- Most of the major players are signed up.
- Nature of collaboration and interaction with auto companies not clear.
- How is placement of stations relative to vehicles decided?
- How will placement of refueling stations be done so that they are not too close to learning demonstration stations?
- No stated plans.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- Information provided was so general that effectiveness of planning could not be determined.
- In response to question, PI said that more detail on objectives and milestones are in planning documents currently provided to DOE.
- Results expected are very general. Need details.
- Because the project has not yet begun, it is unfair to rank this item.
- Details of future work are somewhat sparse.
- Proposed Program schedule confusing. Rather than spanning 3 text slides, try doing it in a Gantt chart.
- Good plan for establishing many refueling stations with multiple delivery options, but it is not clear what new technology and research is entering into this project to reduce the barriers.

Strengths and weaknesses

Strengths
- Great plan. Will learn a lot.
- The company, team and collaborators are very strong.
- The variety of fueling approaches is a real plus and this lead contractor has the experience to do this work.
- Air Products knows this business.
- A strength of the project is that it focuses on an important DOE barrier.
- Strong emphasis on refueling.

Weaknesses
- Based on information provided, project planning to date is sketchy, limited.
- More clarity is needed on projected benefits/value added by this project.
- Expectation of learning to be gained not adequately defined in detail.
- Cost reduction opportunities identification should be first priority.
- The details and coordination among the 14 projects are not well documented, probably because the project hasn't started.
- Needs more focus.
- A weakness of the project is that it does not seem well planned or coordinated with either the four learning demonstration projects or the OEM vehicle rollouts in California.
- Not clear how project will continue to be able to collect/provide data once first year funding runs out.
- Very poorly defined plan.

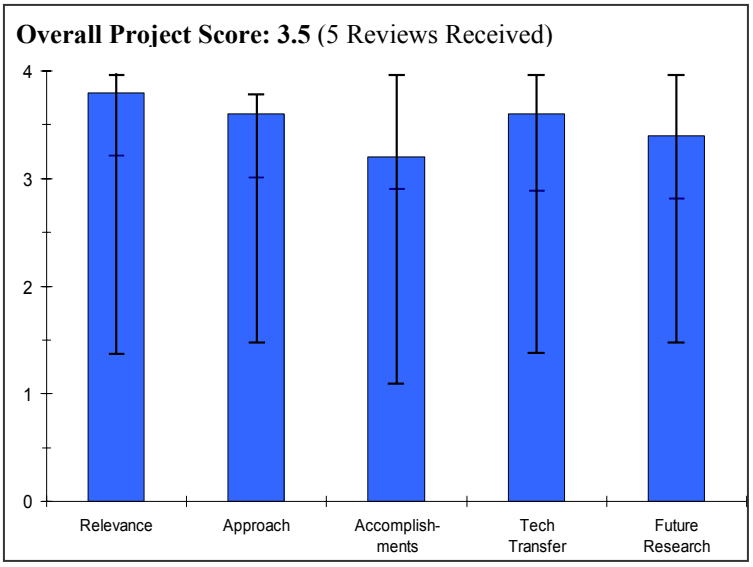
Specific recommendations and additions or deletions to the work scope

- Should add a project element that establishes communication, integration between this project and other California hydrogen initiatives, particularly those funded by DOE.
- None.
- Develop a work plan that links the multiple projects with goals and expected outcomes.
- For this large of a project ($10M) more emphasis should be placed on coordination with automotive OEMs and DOE's learning demonstration projects for the future.
- Unless a research, development and validation plan can be put together that fits with the DOE Program, this project should not be started.

Project # TV-09: Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project
BonHoff, Klaus; DaimlerChrysler

Brief Summary of Project

Through the DOE program, DaimlerChrysler will place more fuel cell vehicles in the hands of customers who can give valuable feedback about performance under different conditions. As the energy partner of the program, BP recently opened the first public hydrogen refueling station in Southfield, Michigan with DTE Energy Company. Another station was opened in a cooperative effort between BP and Praxair at the Los Angeles International Airport. DaimlerChrysler will test the vehicles in diverse environments in the United States. Varying weather, traffic conditions and driving style in each location will benefit the development of the fuel cell automobile and how it will be refueled.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Of paramount relevance to DOE's fuel cell vehicle programs.
- Program results should provide important database for operation of fuel cell vehicles, delivery of hydrogen, and production of hydrogen.
- Program should provide critical safety / hazards database.
- Database collection and dissemination will provide confidence level for public.
- It is critical to understand the performance of vehicles in real world conditions.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- Great plan- well thought out. Room for dealing with unexpected is excellent.
- The project team members are well experienced in designing and implementing vehicle demonstration programs.
- Well-structured approach.
- Multiple climates identified, but not clear if vehicles will be left out in sub-zero weather for several days before startup.
- Good/reasonable distribution of vehicles in three climates.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Good results were found on vehicle and systems performance.
- Customer acceptance study is a very good accomplishment for market development.
- Great start. Not clear what has been learned so far.
- Well planned and conducted beginning.
- Program in startup phase.

- Vehicles and early hydrogen delivery systems being put in place.
- Excellent collection of vehicles already in operation.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.6** for technology transfer and collaboration.

- A competent, strong team that will deliver.
- Good team formed for program.
- Commitment of data to DOE critical.
- Data provided to NREL.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- Cost analysis was not adequately addressed
- The next 4 years will be of immense value to the DOE programs.
- Program appears well structured.
- Proposed activities meet the DOE requirements, but future work would benefit from a more detailed technology transfer and technology improvement plan.

Strengths and weaknesses

Strengths
- Customer acceptance study.
- Automated on-board data collection.
- Outstanding plans, vehicles, and facilities.
- An extremely well planned project that brings together the major industry players.
- Great plan for technology validation and customer acceptance.

Weaknesses
- Not clear how results is presented or to whom. Are the detailed failure incidents reports available for the total DOE effort to use?
- None evident.
- Lack of information on how information from this activity will be used to improve the technology and overcome the technical and market barriers.

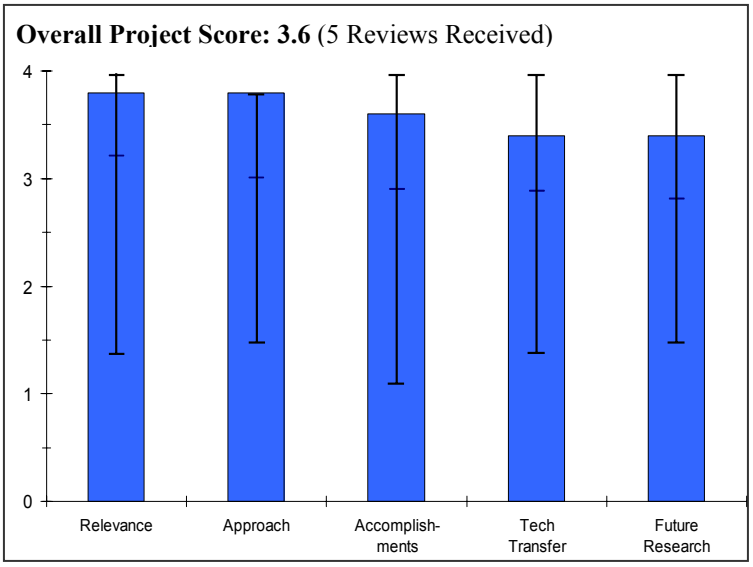
Specific recommendations and additions or deletions to the work scope

- To be more detailed on customer response. Not only are in-depth interviews with partner organizations necessary, but also structured interviews with drivers (level of information on vehicle and fuel technologies, evaluation of drivers feeling about driving experience, statistical analysis of troubleshooting, etc.).
- An important part of technology demonstration is the economic viability. The project must address the efforts to reduce technology costs (vehicle and fuel cell price reduction strategy, total cost per mile, etc.).
- Make as much failure data as possible available to as many people as possible.
- No changes needed at the moment.
- Prepare a more detailed technology transfer plan.

Project # TV-10: Hydrogen Fuel Cell Vehicle & Infrastructure Demonstration Program Review
Frenette, Greg; Ford

Brief Summary of Project

Hydrogen fueled vehicles, utilizing current fuel cell system concepts, will be placed 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 will be sited to establish an initial hydrogen infrastructure, demonstrate alternative production concepts, and evaluate production technologies for cost effectiveness. Emerging technologies in vehicle and hydrogen infrastructure will be validated in separate, advanced engineering vehicles 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 **3.8** for its relevance to DOE objectives.

- Gets at the realities of auto fuel cells very well.
- Of paramount relevance to DOE's fuel cell vehicle programs.
- Program results should provide important database for operation of fuel cell vehicles, delivery of hydrogen, and production of hydrogen.
- Program should provide critical safety / hazards database.
- Database collection and dissemination will provide confidence level for public.
- Addressing most of the critical barriers.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- Excellent.
- The stakeholders are very proficient in designing and implementing vehicle demonstration programs.
- Well structured approach.
- Multiple climates identified, but not clear if vehicles will be left out in sub-zero weather for several days before startup.
- Appears a second more advanced vehicle design is being held in-house for R&D.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.6** based on accomplishments.

- None.
- Well planned and conducted beginning to the project. The delivery of vehicles is well underway.
- Program in startup phase.
- Vehicles and early hydrogen delivery systems being put in place.

- Accomplishments appear to be good, but it is difficult to determine what was done prior to and since the beginning of this project.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Well-defined partner's roles.
- Need detailed data and failure mode information sharing.
- The collaboration between these project team members will ensure a very successful outcome.
- Good team formed for program.
- Commitment of data to DOE critical.
- Acknowledged the need to transfer data to NREL, but not the value of that activity.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.4** for proposed future work.

- Cost analysis was not adequately addressed.
- The next 4 years will be of immense value to the DOE programs.
- Program appears well structured.
- Place vehicles and fueling stations in three regions.

Strengths and weaknesses

Strengths
- On-board data collection system.
- Aggressive tests can be important to validate technologies.
- Everything is great.
- An extremely well planned project that brings together the major industry players.
- Overall, a great validation project.

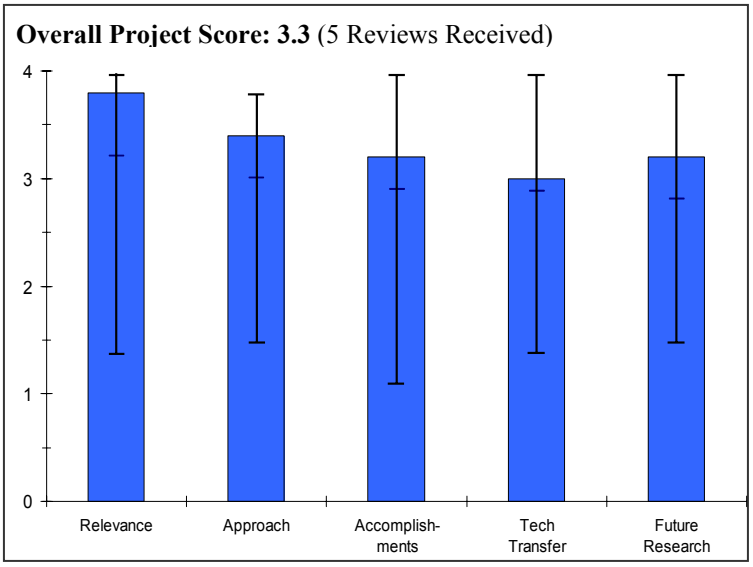
Weaknesses
- Sharing all information with everyone needs to be included in the program.
- None evident.
- Lack of recognition of value of technology transfer and reaching out to other organizations for help when technology shortfalls occur.

Specific recommendations and additions or deletions to the work scope

- An important part of technology demonstration is the economic viability. The project must address the efforts to reduce technology costs (vehicle and fuel cell price reduction strategy, total cost per mile, etc.).
- Detailed project results, failure modes, root cause, etc. data needs to be fully shared with all DOE participants.
- No changes needed at the moment.
- This project could be improved by developing a detailed plan describing how the 18 vehicles in this project will interface with the 12 additional vehicles being built.

Project # TV-11: Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project
Paulose, Rajesh; ChevronTexaco Corp.

Brief Summary of Project



ChevronTexaco Corp., Hyundai Motor Co. and UTC Fuel Cells will lead 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 public–private partnership 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 Fuel Cells' power plants. Hydrogen at the refueling stations will be generated using different types of natural gas reformer technologies and electrolysis. Some of the hydrogen stations will also include a stationary fuel cell to co-generate power. Other collaborators include Southern California Edison, Hyundai KIA America Technical Center, Inc. and Alameda Contra Costa Transit, 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 **3.8** for its relevance to DOE objectives.

- To understand how an average individual would react to fuel cell cars and hydrogen fueling stations is a very important objective of this project.
- Of paramount relevance to DOE's fuel cell vehicle programs.
- Program results should provide important database for operation of fuel cell vehicles, delivery of hydrogen, and production of hydrogen.
- Program should provide critical safety / hazards database.
- Database collection and dissemination will provide confidence level for public.
- Validation of the technologies is very relevant.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- Not enough emphasis on data evaluation. What are they collecting? Is it just a fuel and car system?
- The project members are very competent in planning and carrying out vehicle demo programs.
- Well structured approach.
- Multiple climates identified, but not clear if vehicles will be left out in sub-zero weather for several days before startup.
- Good approach, but could be improved with more detailed planning on what will be required in the infrastructure area.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Too early to tell.
- Well-planned and well-conducted beginning to the project.
- Program in startup phase.
- Vehicles and early hydrogen delivery systems being put in place.
- This team seems to be starting slowly.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- PI has not clearly identified the roles of partners and the information management.
- Need to share failure data with competitors, as least as much as possible.
- This strong team will make sure the project is successful.
- Good team formed for program.
- Commitment of data to DOE critical
- This team does not appear to understand the importance of technology transfer.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- Cost analysis was not adequately addressed.
- Not clear - beyond the obvious.
- The next 4 years will be of immense value to the DOE programs.
- Program appears well structured.
- Future plans are going in the right direction, but they seem to be uncertain about many aspects, such as the number of stations they plan to open.

Strengths and weaknesses

Strengths
- O.K.
- The major industry players are keen to see this project succeed.
- Planned/anticipate improvements to fuel cell vehicles.

Weaknesses
- Could be more detailed about what is expected to be learned and how the results will be used and shared.
- None evident.
- Project seems to be off to a slow start.

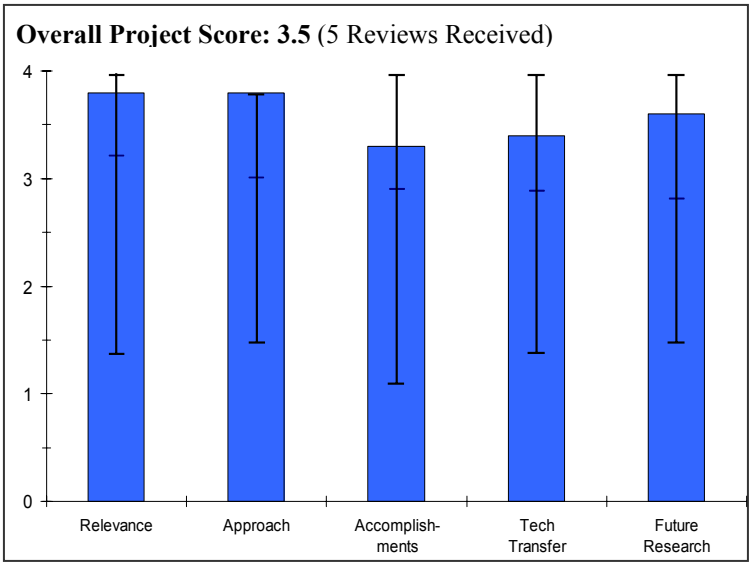
Specific recommendations and additions or deletions to the work scope

- An important part of technology demonstration is the economic viability. The project must address the efforts to reduce technology costs (vehicle and fuel cell price reduction strategy, total cost per mile, etc.).
- More specifics about what data is to be acquired and how it will be used.
- No change presently needed.
- Strengthen plan in the areas of infrastructure development and technology transfer.

Project # TV-12: Hydrogen Vehicle and Infrastructure Demonstration and Validation
Sell, Roz; General Motors

Brief Summary of Project

General Motors will conduct a demonstration project to validate forty Opel Zafira Generation 1 fuel cell vehicles fueled with hydrogen from stations in three locations chosen from the following: Washington, D.C.; Ft. Belvoir, Virginia; Southern and Northern California; Detroit, Michigan; and New York City, New York. The project will identify technology issues and address them in a second-generation vehicle.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Of paramount importance to DOE's fuel cell vehicle programs. Although the claim of several 'world's first' are not really relevant to the DOE's R&D plans.
- Program results should provide important database for operation of fuel cell vehicles, delivery of hydrogen, and production of hydrogen.
- Program should provide critical safety / hazards database.
- Database collection and dissemination will provide confidence level for public.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- Good emphasis on standard development.
- Great.
- The team assembled is very experienced in formulating and executing vehicle demo programs.
- Well-structured approach.
- Multiple climates identified, but not clear if vehicles will be left out in sub-zero weather for several days before startup.
- Plan for implementing two generations of fuel cells is great.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- New project.
- The project has gotten off to a great start.
- Program in startup phase.
- Vehicles and early hydrogen delivery systems being put in place.
- Great progress, probably indicative of work initiation prior to contract award.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Good arrangement, with roles distributed to the different partners.
- Plan for problem reporting and sharing not clear.
- Need detailed FMEA analysis to be shared with competitors.
- A successful outcome to this project can be assured.
- Good team formed for program.
- Commitment of data to DOE critical.
- Acknowledge the need to transfer data to NREL, but not the value of that activity.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- PI could give more details on near and long term future.
- Cost analysis was not adequately addressed.
- None.
- The next 4 years will be immense value to the DOE programs.
- Program appears well structured.

Strengths and weaknesses

Strengths
- Looks like a great plan and clearly is covered well by a great team.
- A very capable team has come together in this project.
- Overall, a great validation project with planned vehicle improvements.

Weaknesses
- Failure and mitigation reporting not defined or shared.
- None evident (or even allowed)!
- Lack of recognition of value of technology transfer and reaching out to other organizations for help when technology shortfalls occur.

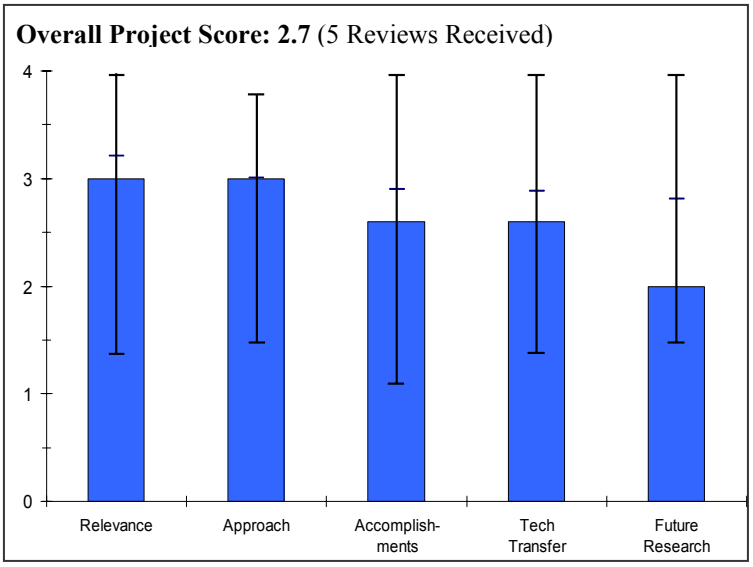
Specific recommendations and additions or deletions to the work scope

- An important part of technology demonstration is the economic viability. The project must address the efforts to reduce technology costs (vehicle and fuel cell price reduction strategy, total cost per mile, etc.).
- Report all incidents in detail for all DOE program participants to share.
- If it ain't broke, don't fix it!
- A more detailed technology transfer plan would be helpful.

Project # TVP-01: Hydrogen from Biomass for Urban Transportation
Bota, Kofi; Clark Atlanta University

Brief Summary of Project

Clark Atlanta University and its collaborators, focused on producing hydrogen from biomass, produced 25 kg/day of hydrogen from peanut shells for urban transportation. This process involved pyrolysis of the biomass followed by catalytic steam reforming of the gas and bio-oil products to produce hydrogen. Successful operation of 100 hours demonstrated the technical feasibility of the process, identified agricultural uses of the carbon product, and identified economical co-product options for the bio-oil. Current efforts are focused on completing a 1,000 hour demonstration of the process.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Design of the project did not include analyzing the process steps to reduce costs or energy inputs.
- Low cost. Realization of need for productivity. There are a lot of biomass pathways and this is a good example.
- Good bench/ prototype test demo of organic fueled generation of H_2 but nothing really unique.
- Hydrogen from biomass technology validation is consistent with HFCIT program objectives.
- The role of biomass in future hydrogen systems is very important, and a goal of DOE, because it is a renewable resource and it also eliminates waste products which themselves produce green gas gases when they decay.
- The project cost goal of $2.30/kg appears to be consistent with DOE's new goal of $2 to $3/gge.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- "Proof of principle" approach showed that peanut shells can provide a source of hydrogen.
- Principle demonstrated, but nothing done in this project to overcome barriers, define process constraints, and identify logical next steps.
- Process only operated continuously for a few hours.
- Seeking multiple co-products from biomass to hydrogen process. Two years of project - 200 hrs of runtime. The system design seems innovative.
- Need to analyze economic data to determine dependence on waste utilization.
- Prudent technical approach considering funding level.
- No hydrogen purification included due to limited funds/scope.
- The project was creative in developing a byproduct with agricultural value to help reduce the effective cost of the hydrogen.
- The ability to handle wet agricultural residues is a real plus.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- No process cost data or projections.
- No measuring of energy inputs - propane, electricity - required to produce hydrogen energy.
- Longest continuous period of operation was a few hours.
- Feedstock (peanut shells) input was intermittent.
- Is innovative. Fertilizer products from peanut shells would be useful.
- Good demonstration of process feasibility.
- Good accomplishments relative to funding.
- Produced useful data regarding hydrogen yield from one type of biomass.
- The project is a technical success.
- It is not clear from the presentation whether the project met its cost goals, or how the resulting costs relate to DOE cost goals.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- Two thirds of funds went to Eprida, a sub-contractor.
- Roles of the university unclear.
- Should expand publicity.
- No evidence that results are being transferred to other HFCIT projects.
- It is hard for a university to do technology transfer to industry but this project had a number of important partners including NREL, ORNL and Georgia Tech, which should help ensure that the results are shared more widely.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- PI would like to do more work, but nothing specific proposed.
- Hydrogen purification. Systemic characterization. Georgia Tech scale up future.
- Other than more and longer testing, no specific goals were mentioned.
- Project complete.
- Future plans not presented.
- No future research is recommended or planned for this completed project, although the presenter did indicate that the next step would be to scale it up.

Strengths and weaknesses

Strengths
- Skid mounted approach to mechanize prototype is smart.
- Good tech abilities. Good collaboration between academia and industry.
- Early demonstration of hydrogen from biomass.
- Generated useful quantitative data.
- Important message regarding benefits of producing biomass byproducts in addition to hydrogen.
- The project demonstrated good collaborative college, national laboratory and small industry work.
- The project was a technical success.
- The project was integrated into student work

Weaknesses

- Minimal useful data, results achieved for relatively large DOE resources expended.
- Durable catalyst still needed. It seems like a lot of partners are claimed but their role and contributions unclear.
- Carbon balances are issues.
- Lack of purification (due to limited scope/funds) made results seem less directly relevant to HFCIT goals.
- Technology transfer (utilization by others) not obvious.
- The person that did most of the talking was Danny Day who seemed to be representing Enviro-Tech Enterprises. Zach Wang and Kofi Bota were knowledgeable and helpful when given a chance to talk.

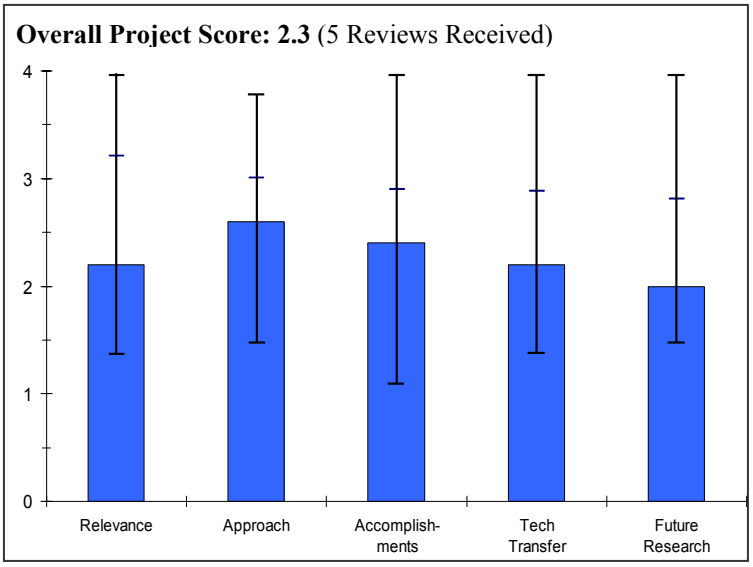
Specific recommendations and additions or deletions to the work scope

- No follow-on work is recommended for this project.
- It is important that this team focuses on their potential value added and doesn't try to re-invent catalysts, purification or devices that they can buy to incrementally improve the system.
- HFCIT Systems Analysis and Systems Integration activities should utilize results from this project.
- Possible follow-on work recommendations were not presented.
- Consider an assessment by ORNL and NREL to determine if there is merit to further work, including scale-up.

Project # TVP-02: Chattanooga Fuel Cell Demonstration Project
Ferguson, Joe; City of Chattanooga

Brief Summary of Project

Through The Enterprise Center and its Connect the Valley Initiative, the City of Chattanooga plans to facilitate 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 to be validated operates with high capacity factor even when the demand for hydrogen is relatively low.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.2** for its relevance to DOE objectives.

- Shows outstanding promise in terms of relevance.
- The question is will this flexibility to produce hydrogen or electricity become successful or not.
- This is an interesting project but it is not clear that it adequately addresses the hydrogen program cost goals.
- The co-production of hydrogen and electricity is a very important aspect for future hydrogen systems and it may be cost-effective.
- The program does not emphasize direct hydrogen production but only considers hydrogen production as a by-product.
- Program emphasis appears to be SOFC installation.
- The concept seems like an excellent transition technology to still operate at a high efficiency (through producing electricity) even while the number of vehicles that would likely be refueled would be small.
- There is no evidence of research to advance the technology, but rather a very expensive and risky build and test activity.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- Solid approach.
- Proving the approach with unproven solid oxide technology combines the risk of the approach with the risk of unnecessary fuel cell technology.
- The project is trying to do too many things.
- The project does not seem well designed.
- Approach may be flawed due to endothermic reforming of natural gas.
- To produce extra hydrogen for vehicle applications high amounts of natural gas must be introduced into SOFC and the endothermic reaction will cool the cell.
- No engineering analysis to determine if SOFC should be designed for power, heat production or hydrogen production.
- Cost analysis not in approach.
- Project seems well thought-out and designed.

- Performing demonstration/validation on a larger sized system (say 100 kW, rather than 5 kW) would provide more value to the program by removing questions of scale-up.
- This approach is very high risk and is not likely to achieve a 5 kW system as planned.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- Does not deal with CO_2 production issue.
- Maybe it is too early, but there is much being attempted here, with little clarity of focus.
- Good progress toward constructing installation.
- No activity to determine what the proper operating point should be and the cost of the integrated equipment.
- While project is well funded for FY05, progress to date seems slow. Seems questionable whether system can be built, tested, and validated within remaining 4 months of project.
- Chart #2 indicates the project is 50% complete; however, to date only 1kW has been demonstrated and the electrolyzer and electrolytes have not been selected for the 5 kW stacks.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.2** for technology transfer and collaboration.

- Some interface to auto industry should be included. (Presenter mentioned a desire to provide home hydrogen auto fueling).
- It is quite possible that the lack of industry and other outside collaboration that is this project's weakest aspect.
- The evidence for technology transfer is weak.
- Industry / university collaboration.
- Papers to be published.
- Interactions and collaborations with others seem limited.
- Not clear how many component suppliers are involved in project from a development perspective vs. simply selling off-the-shelf components.
- Project relevance to the Hydrogen Initiative and probability of success would be greatly enhanced if a national laboratory or other research institution with SOFC experience were added to the team to perform research addressing critical barriers.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- Should minimize CO_2 release into atmosphere.
- While the future work items are listed, there is little evidence of anticipating problems, or the experience of a proven research approach.
- Engineering analysis of thermal balance and cost analysis are needed to confirm viability.
- Does not seem likely that the large amount of work proposed (and needed) can be completed in next 4 months unless rate of progress on project is accelerated.
- All activities are built and tested with no evidence of research planned.

Strengths and weaknesses

Strengths
- Very specifically focused.
- Co-production of hydrogen and electricity is a concept that deserves research attention.
- Novel and promising approach to deal with both limited need for hydrogen production at the beginning, while still being able to run at high capacity by producing electricity when more hydrogen is not needed.
- Ion America claims to have demonstrated planer SOFC with no leaks.

Weaknesses

- Doesn't consider side effects sufficiently.
- Too much development of new equipment is attempted without fixing some of the variables with proven technology.
- Experienced industry collaboration, including the solid oxide fuel cell developer, is non-existent.
- While linear plot showing potential mixes between hydrogen production rate and electricity production was shown, trade-offs in terms of overall system efficiency not explored.
- This project is structured as if all barriers have been addressed and full commercialization is only a year away.
- Private sector cost share for this validation/demonstration project is inappropriate and excessively low.

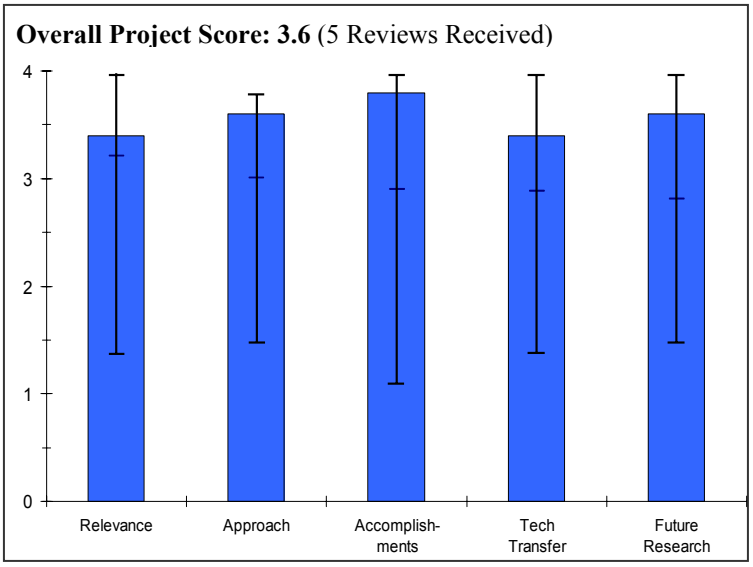
Specific recommendations and additions or deletions to the work scope

- CO_2 release concern.
- Automotive interface needed.
- Discontinuing this project should be considered unless the research scope can be adequately focused on achievable objectives that fit with the hydrogen program needs.
- Prior to further construction, the viability of the concept should be established with an engineering analysis. Possibly an outside team should conduct that analysis.
- Try to find private investor support to scale this system up to 100 kW size for more realistic validation.
- Explore tradeoffs in efficiency between H_2 and electricity production more fully.
- Restructure the project to address the barriers of cost and performance of SOFCs.

Project # TVP-04: Power Parks System Simulation
Lutz, Andy; Sandia National Laboratories

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 Laboratories (SNL) 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.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- System model.
- Looks like a good tool for analysis of power park elements as well as comparing parks.
- Appropriate to apply data from other technology validation projects to analyze and model technologies.
- Overlaps or complements HFCIT Systems Integration and Systems Analysis activities.
- This is a great project, at a relatively low cost that will address the unknowns that everyone is interested in... the potential for lower cost hydrogen production in a more complicated environment of co-production of electricity.
- This project will undoubtedly provide excellent simulation tools to DOE's technology validation activities.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- Within the constraint of using the designated software, this seems valid.
- PI is obviously very skilled in this area. Sound modeling approach.
- Consider approaches to enable broader dissemination of analytical models (e.g., to researchers without Simulink software).
- The approach relies on industry partners, real data and solid modeling experience.
- There are problems in a project like this but this project is robust in its use of existing experience and information.
- The PI has a comprehensive understanding of is mission and deliverables.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.8** based on accomplishments.

- Library of models is simulink. Should be expanded but has a good start.
- Working well with real inputs.
- Excellent work quality and productivity relative to funds consumed.
- Solid progress, in each of the power park examples, is evident.
- Improved performance of power parks, or possibly recognition that this is not the most economical approach to hydrogen production, will result. In either case, this work will pay for itself in reduced public dollars.
- The development of simulation models and their preliminary trials have progressed very well.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Collaborating with the 3 major power park projects i.e., DTE, APS, and Hawaii.
- PI is making obvious effort to disseminate results.
- This project has a most impressive list of collaborators, virtually anyone currently involved in building and demonstrating power parks.
- Good so far, but it is imperative that all of the simulation models within the DOE HFCIT program interface well with each other i.e., incompatibilities.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- Graphics user-interface to allow non-specialist to use model.
- GUI work excellent for broad public use.
- Project is carefully planned.
- The plans to make the results user-friendly and to validate the model with existing power parks will bring this effort a lot of credibility.
- The PI clearly knows what's needed to be done and will pursue it enthusiastically.

Strengths and weaknesses

Strengths
- Willingness to share with simulink users.
- Good central understanding of all elements.
- High quality technical work.
- Beneficial analysis of power park project data.
- Open availability of results.
- A sound research approach, all the important industry partners, and plans to make the results useful to others are important strengths of this project
- PI enthusiasm and realism is a big asset.

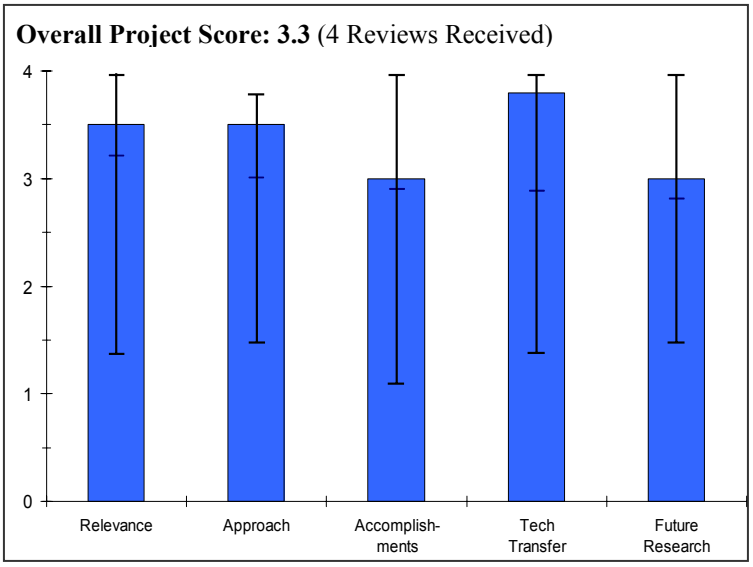
Weaknesses
- Seems to disregard the money modeling efforts which have come before. The rationale of starting over in modeling with a "black box functional building blocks" unclear.
- What is plan for interfacing results with HFCIT Systems Analysis and Systems Integration activities?
- Utilization of output models requires Simulink software.
- It is possible that this project is taking on too much with too little funding.
- Could benefit from more 'real world' system and sub system operational data from industry participants.

Specific recommendations and additions or deletions to the work scope

- Agree with creation of graphics interface (GUI) in the continuation of this project with the assurance that more stakeholders have eventual access to this tool for free.
- Keep analyzing more data.
- Continue high-quality work.
- Be more proactive with technology validation power park projects to ensure good quality data.
- Increase communication of "what the data is telling us" results as well as the models.
- Be sure that the project team reports to all organizations considering power parks, either as public decision makers, or as businesses or power park developers.
- No changes needed.

Project # TVP-05: Technology Validation: Fuel Cell Bus Evaluations
Eudy, Leslie; National Renewable Energy Laboratory

Brief Summary of Project



This National Renewable Energy Laboratory (NREL) project involves collecting operational and performance data on fuel cell buses in real-world service. The results will help assess the status of fuel cell systems for buses, determine issues that need further development, and provide the "lessons learned" to aid other fleets in implementing the next generation fuel cell systems into their operations. In coordination with industry partners, NREL is working to harmonize these data collection efforts with other fuel cell bus demonstration projects around the world. The team is also working to collect more detailed data on the vehicles and infrastructure similar to that of the Controlled Hydrogen Fleet and Infrastructure Demonstration and Validation Project.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Evaluates buses in service.
- Real world experience is a high priority.
- Evaluating the performance and maintenance of the hydrogen buses in operation today is critically important.
- This project is a very effective way to increase the learning from buses worldwide.
- Validation plans well conceived and implemented.

Question 2: Approach to performing the research and development

This project was rated **3.5** on its approach.

- Ballard Fuel Cell, Hydrogenics, UTC, ISE. Fuel cell buses treated like black boxes.
- Would be improved to be able to compare results between the 4 different approaches represented. Perhaps some "common route" testing.
- There is much to like about this project.
- It is cost-effectively evaluating existing buses, not deploying new buses with larger amounts of funding necessary.
- It is utilizing common data analysis approaches across a diverse set of bus operations and companies, including international programs like the CUTE program.
- The project is using their data, not forcing the creation of new data gathering efforts.
- The PI has a good appreciation of the issues and objectives.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.0** based on accomplishments.

- Fleet customers.
- The project seems on schedule, as buses are deployed and developing important operational and maintenance data.
- It is not clear whether cost data being developed as well.
- Early days yet for data analysis.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- This project is collaborating with all fuel cell bus manufacturers, all bus fuel cell manufacturers and all the programs, UC-Davis, as well as U.S. FTA and other organizations.
- This project goes beyond U.S. bus demos to European and other national programs.
- Excellent teamwork and coordination is paying off.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Desires to move into other fleets. The project should show it can work with these fleets before expanding.
- The project will finish up with bus projects just beginning.
- The results of the data analysis will be crucial in revealing the value of this effort.

Strengths and weaknesses

Strengths
- I appreciate her ability to get these fleets to cooperate with her.
- Clear mission and strong management focus.
- The project is appropriately using data that others are gathering, their own data, instead of gathering its own data. This gives the project credibility.
- The project is using all bus projects that are available.
- The operational experience of the fleet operators, bus manufacturers, and natural enthusiasm of the PI will bring success.

Weaknesses
- It is uncertain how much unedited reality will emerge from this work vs. a packaging of the hardware supplier's version of their own performance.
- Needs more ability to compare results for different alternative bus and fuel cell configurations.
- It is not clear that economic data is being gathered, although that may be difficult and not necessary at this early stage. It is more important to know how much hydrogen is used than what was paid for it, for example.
- None evident at this stage.

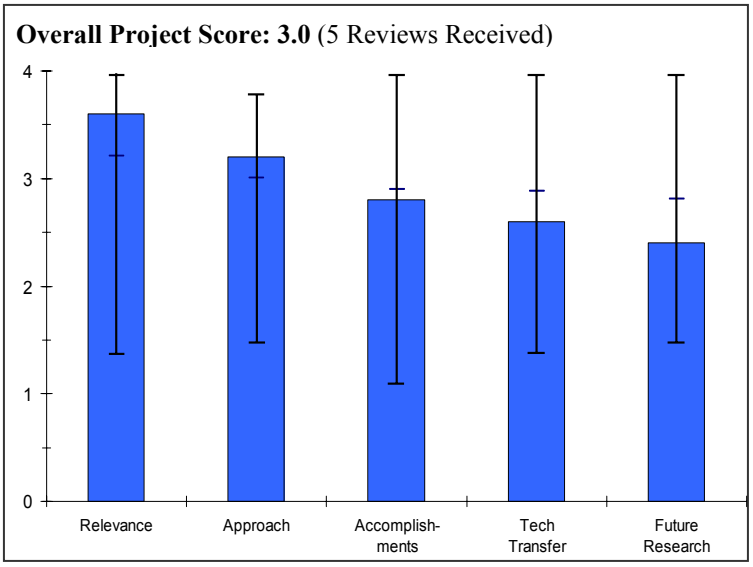
Specific recommendations and additions or deletions to the work scope

- A firm commitment to data integrity seems absent and an admission that access to these fleets could be jeopardized by supplier subjective reactions to her reporting was of concern.
- Add some degree of comparison of results for common routes.
- Document cost information that might be important as a baseline, like bus costs, fueling station costs, etc.
- Review justification for any further effort at the 4th 2006 workshop.

Project # TVP-06: Auto-Thermal Reforming Based Refueling Station at SunLine
Harness, John; HyRadix/SunLine

Brief Summary of Project

Hyradix and SunLine are working together to develop an on-site natural gas autothermal reforming system for vehicle refueling. This reformer will advance sulfur removal technology, purify the fuel stream through pressure swing adsorption, and be integrated with compression and storage systems designed by SunLine. The high-purity hydrogen will be compressed and stored at 3,500 psi for blending with CNG and at 6,000 psi for use in hydrogen ICE and fuel cell vehicles. The hydrogen and hydrogen enriched natural gas (HCNG) will refuel fuel cell and HCNG buses, street sweepers, and cars.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- A functional reformer is necessary.
- Good demo of actual use of ATR.
- This project supports the development of a fueling station-sized, lower-cost hydrogen production technique that utilizes lower temperatures, less fuel and lower cost materials.
- Hydrogen cost is one of the critical barriers to a hydrogen future. If it can produced at low cost nearer the point of use, distributed production, the transportation costs can be avoided, reducing the cost of the delivered fuel.
- This project has high-relevance to DOE's program because it provides a transition strategy before hydrogen infrastructure is widespread.
- Early experience from this project provided great value to SunLine, who is a leader in hydrogen transportation implementation for buses.
- Project provides an interim solution to hydrogen production and delivery, which can be used in the early phases of the transition to a hydrogen economy.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- In-house sulfur removal is cost effective for the flow delivered ($3.68 kg). The gas quality and cost performance are represented as useful to hydrogen energy.
- Good but utilization needs to be increased to verify durability and costs.
- The approach is sound to achieve the goals, and the technique has relatively fast start-up, 3 hours from cold start, faster than SMR technology.
- Approach was well laid out to accomplish objectives of the project.
- Approach resulted in reasonable accomplishments.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- It seems this field trial was useful to the contractor but no meaningful data was shown to this reviewer.
- It is not clear that the DOE cost goal for hydrogen can be achieved, although the lack of hydrogen transportation costs may be sufficient to support a higher cost.
- This appears to have been a very successful project.
- CO impurities were stated in % (0.03%), which is 3/10000, or 300 parts per million. There must be a PSA to clean this up for use in a fuel cell, but that is not discussed.
- Cost of $3.68/kg appears to be good progress toward $3.00 goal.
- Excellent that this resulted in a commercial product with at least one order for the product.
- Good documentation of problems encountered.
- Project identified engineering improvements that can be made to further improve the system and make it more viable as an interim production/delivery solution.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- SunLine is a demo site but has little value added and seems unwilling to comment on the project.
- This is a great showplace with lots of exposure.
- Collaboration with SCAQMD is a plus.
- It is not clear how the results will be useable by other companies, although it is clear that the contractor is moving ahead with commercialization of the technology and that the public will benefit from an alternative approach to hydrogen production at the fueling station.
- Good collaborations led to a successful project.
- There is no evidence that technology transfer to others performing research in this area is taking place.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- Close out. The contractor representing this hardware is commercial therefore this should be cleared out.
- Could do more run time and utilization but until demand rises what else needs to be demonstrated? It works.
- The contractor's plans are clear and there is great hope for the results of this project.
- Future work looks reasonable.
- If market introduction is not profitable, what is the plan to reduce costs, improve performance, etc. to move to profitability?
- Future plans will make incremental improvements to this system, but will not achieve long term initiative goals.

Strengths and weaknesses

Strengths

- Good equipment and capabilities. Good showcase facility.
- The contractor had a clear pathway to a potential commercial product and proved that it was feasible, an approach that will have public benefit.
- The project resulted in hydrogen production cost information.
- Project successfully completed and turned into commercial project. It doesn't get much better than that as far as a success story.
- Provides a hydrogen production and delivery system that is available today.

Weaknesses

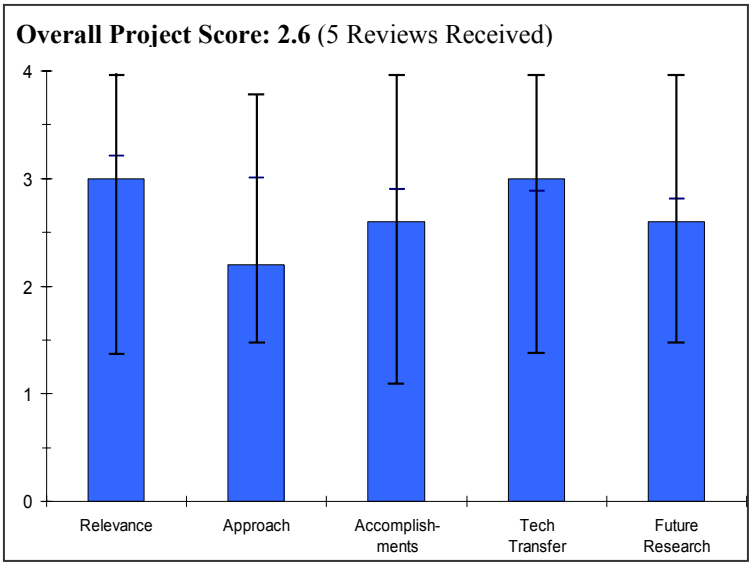
- A clear report, verified by a credible party, that the hardware functions as represented.
- Need more uses for the fuels.
- The DOE cost goal is not clearly met yet, but with the avoidance of transportation costs, this approach may provide a net economic benefit.
- While it may be proprietary, an idea of how Hyradix will approach the market: what niches, where does it make most sense from a consumer perspective, etc.?
- Reformation of natural gas is not a long-term solution to our energy problem.

Specific recommendations and additions or deletions to the work scope

- Contractor says the hardware can be bought and that it doesn't need government help.
- It would be interesting to develop experience data with the next generation of autothermal reformers from Hyradix.
- Project is completed.
- Identify market niches and go after them aggressively. The hydrogen investment community could use some examples of profitable companies to learn from.
- Project is completed and technology is being commercialized.

Project # TVP-08: Hydrogen Filling Station
Boehm, Robert; University of Nevada, Las Vegas

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 fueling system powered by solar energy. Objectives included development of the requirements for the fueling system, survey of potential sites and determining favorable/unfavorable characteristics of each, selection of the site with site plan and support to the site permitting process, design of the fueling system layout, installation of the fueling station in Las Vegas, monitoring operation of the fueling system, and characterizing performance. In the second step of the process, the refilling station is being supplemented with a high-pressure electrolyzer that is being developed for this project. In addition, 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 involves the conversion of a gasoline-fueled ICE system to a hydrogen-fueled ICE system. For the latter, we are attempting to use direct cylinder injection of the hydrogen. Finally, engineering of tandem solar cell systems is taking place as well as some basic science studies. Hydrogen is produced directly in these cells, and they promise high conversion efficiency. To accomplish this work, the first step is a CFD analysis of the flow and heat transfer within the cell. On-sun experiments are anticipated with a prototype unit during this next year. In the science studies, work on the separation membrane and coatings involved are being carried out.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Integrated with vehicle fleet operator. Scope includes vehicle conversions, a pickup truck for the water district, and a hydrogen fuel cell hybrid and ICE both renewably driven with a 15 kWPV 15 kW.
- Good solar demo.
- Project objectives are pertinent to HFCIT program goals.
- A field demonstration of high-pressure PEM electrolysis would be useful.
- This project has two important characteristics: renewable production of hydrogen to improve environmental characteristics; and high pressure electrolyzers to reduce costs.
- Other project elements, like conversions of vehicles do less to support the program. This is not important research when done in this context.
- Many of the projects covered by this project are relevant for DOE's program.

Question 2: Approach to performing the research and development

This project was rated **2.2** on its approach.

- Use two PEMs electrolyzer and two compressors. Use an APCI dispenser solar electrolyzer.
- Seems to include too much, i.e., includes vehicle work.
- Work plan does not appear to have been laid out with adequate care.
- Potential for missing technical, schedule, and/or budget objectives appears too significant.
- There are too many research experiments going on this project at the same time.

- Improved solar technology is good, but not necessary in a hydrogen program.
- Conversion of vehicles to use hydrogen is necessary but could be done by experienced retrofitters, without fuel cells.
- Use of solar to produce the hydrogen is an important aspect of this project but it is not necessary to use experimental solar technology.
- Development of a high-pressure electrolyzer is very important for fueling station economics.
- Approach seems very scattered.
- Project looks like a collection of small research projects without a cohesive tie together.
- For this large amount of funding, the project should be better organized and focused.
- Better to apply efforts in a few limited areas and have success, than spread too thin over large number of small projects that have limited chance of technical breakthroughs needed.
- No connection between PEC project and H_2 filling station project.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- In phase 2, a new PEM cell was used to support the membrane to provide output pressure of 2000 psi, a 99.999% hydrogen, and desiccant.
- Still early but seems on track.
- None of the major project elements appear to have been installed at the site yet.
- Presentation did not show originally proposed project schedule.
- The project appears to be behind schedule with results also behind.
- It not clear what benchmarks are being used to judge project success by the researchers.
- Some progress appears to be made on a few of the projects.
- Results not yet conclusive or quantitative.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Supply agreement with APCI. Like the use of local engine expert with fresh ideas.
- Working with a reasonable cross section.
- Collaboration appears to be limited to direct project participants so far.
- Proton Energy is a strong partner in the most important work to develop a high pressure electrolyzer.
- Other partners and collaboration do not measure up to the standards expected of this DOE Technology Validation Program.
- Good involvement of students on multiple projects.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- I think this project delivers a lot for a small amount of money.
- Scale back to focus on solar and let vehicle people do vehicles.
- FY 05 and FY 06 plans appear to be quite ambitious.
- The planned efforts appear to be designed to finish the project but do not appear to address possible problems or present contingencies, nor expand the knowledge base except by working through the tasks.
- Plans are very high level and a path to completion is not obvious.

Strengths and weaknesses

Strengths

- Will the 2K psi electrolyzer improve system efficiency? Uniquely innovative is the broad array of hydrogen supply options which are corporate in-context. Developing a local expertise.
- Good renewable resources (solar).
- High-pressure electrolysis field demonstration data would be useful.
- Project provides good student learning and hands-on experience.
- Solar energy to produce hydrogen at a fueling station is an important concept to evaluate.
- High pressure electrolysis is considered to be an important possibility to reduce compression costs and therefore reduce the cost of delivered hydrogen to a vehicle with a high pressure tank.
- Topics chosen are relevant for DOE program.

Weaknesses

- Slow because of codes and standards. Local contractors. Direct cylinder fuel injectors.
- Seems spread thin by being too inclusive.
- Project appears to be unlikely to meet all technical, schedule and budget objectives.
- Hydrogen program funding should not be used to develop solar equipment. Commercially viable PV, plus extrapolation to emerging solar technologies would allow the researchers to concentrate on the important aspects of this project.
- Converting hydrogen vehicles in this project is not a good use of hydrogen program funds.
- This project should concentrate on a solar fueling station with high pressure electrolyzers.
- Too many projects selected without a clear thread between them.
- For this amount of funding, a clearer focus and more significant results should be expected.

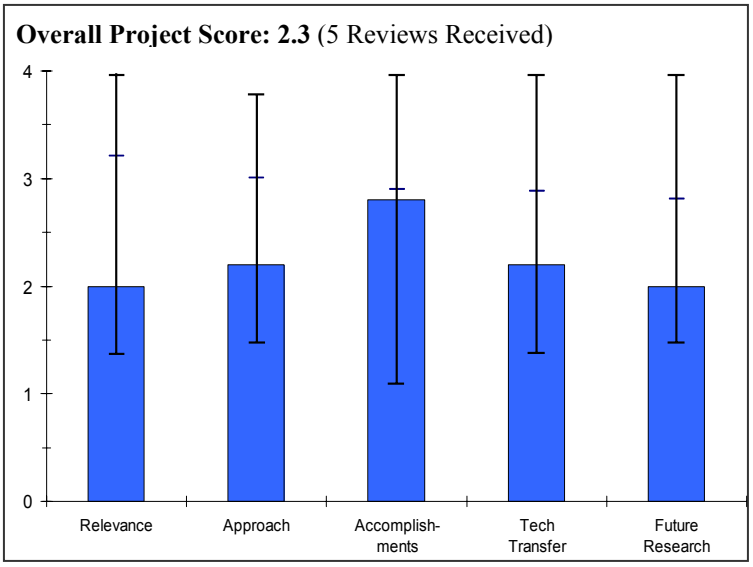
Specific recommendations and additions or deletions to the work scope

- Installation focus for the balance of 2005 field test for solar cell (tandem) - hydrogen solar scale up important.
- Delete vehicle adaptation work and concentrate on a solar fueling station.
- Narrow down the project scope, if needed, for a graceful project completion.
- Re-focus the research on the solar deployment and analysis and new reformer, and the analysis that should be done after the stations is built and utilized.
- Reduce number of projects.
- Focus on results.
- Continue to educate students through projects so that the H_2 community has a strong workforce.

Project # TVP-11: Hydrogen and Natural Gas Blends: Converting Light and Heavy Duty Vehicles
Mulligan, Neal; Collier Technologies Inc.

Brief Summary of Project

Collier Technologies Inc. is developing low-emissions, light- and heavy-duty vehicle engine and retrofit packages to seamlessly repower today's buses and trucks with existing natural gas and diesel engines that will exceed DOE's goal of reducing emissions and maintain or enhance vehicle drivability and reliability. This will be accomplished through the addition of hydrogen to the natural gas fuel mixture.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.0** for its relevance to DOE objectives.

- Useful technologies have been developed for the transition to a hydrogen economy.
- This approach has low confidence in satisfying practical needs for fuel storage on large vehicles.
- Supportive of hydrogen fuels, but broad-based use of blends is uncertain.
- Barriers and relevance to DOE program not clearly articulated.
- Looks like the project is developing a product to be sold (a kit), not a research or validation project.
- This is a product engineering/development activity.

Question 2: Approach to performing the research and development

This project was rated **2.2** on its approach.

- Marginal benefits at best coupled with inadequate on board storage do not look attractive.
- Approach is successfully based on earlier hydrogen/natural gas blend R&D projects.
- Approach is very focused on producing a commercial product for sale.
- This project seems to fall somewhere between a combustion research project and a technology validation.
- Project objectives of converting engines to operation on hythane will be accomplished, but the barriers to the Hydrogen Initiative are not being addressed.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- Two products for 30% HCNG have been successfully developed.
- The accomplishments are impressive to date.
- Sound accomplishments, which indicate that this project is ready for commercialization and no longer an R&D activity.
- Would be helpful if results of project were put in context, such as being numerically compared to the 2007 emissions standards.
- Problems encountered during the project were not discussed: need to provide lessons learned for others in industry.
- Performer states on chart #2 that they are 0 complete; however, they appear to have a product ready for the market.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.2** for technology transfer and collaboration.

- Is there a business case to be made by any collaborators?
- Collier Technologies has now established a commercial partnership with Daewoo Heavy Industries- few partners otherwise.
- City of Las Vegas and Daewoo Heavy Industries were listed as partners, but nature of collaboration not clear.
- No evidence of any collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- This niche application is now sufficiently evolved that DOE needs to let it go on its way.
- Looks like project has resulted in a successful product being developed.
- Future plans are for sale of the product devolved and no barriers are being addressed during or after this project.

Strengths and weaknesses

Strengths
- Developed a 30% HCNG conversion kit for light duty vehicles. Developed a dedicated 30% HCNG engine for the heavy duty engine market.
- Cleaner emissions are impressive.
- Collier Technologies have had a successful long-standing involvement in this technology.
- Focus on optimizing engine performance for emissions is good.
- Appears as though project resulted in product for market.
- Another hythane engine conversion will be available commercially.

Weaknesses
- Poster session was not presented.
- Getting enough fuel on board for adequate range will significantly reduce payload volume.
- Project did not compare progress to well defined metrics, such as emissions standards.
- Interaction with others outside Collier Technologies appears limited.
- Nothing about this project addressed barriers which must be overcome before the hydrogen economy can be realized.

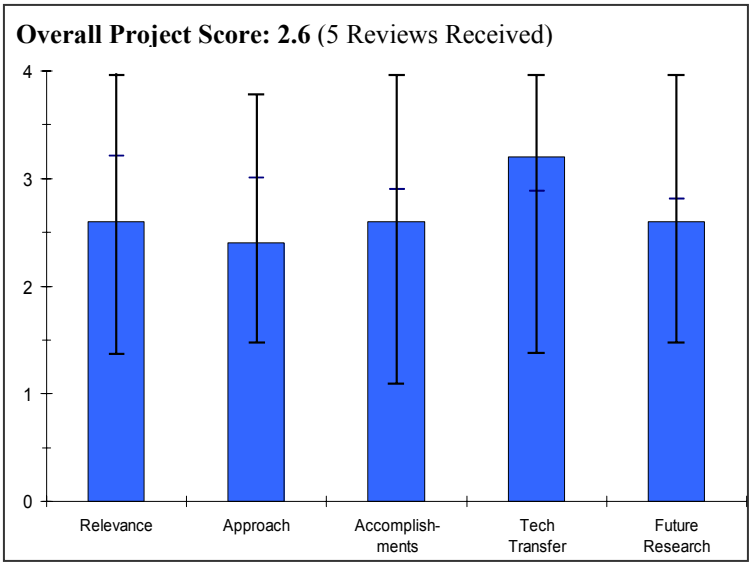
Specific recommendations and additions or deletions to the work scope

- Delete the project unless fuel storage is addressed with a credible proposal.
- Further funding is of questionable value for DOE's hydrogen and fuel cell R&D programs.
- Focus on perfecting product for marketplace.
- Demonstrate and document clear emission, efficiency, and other benefits (as compared to alternatives) so that they may be evaluated.
- Note: Poster was not "posted" so this review was performed from slides provided before conference. This did not allow the normal personal interaction that is objective of posters.
- Federal research dollars should be focused on critical technology barriers.

Project # TVP-12: Fuel Cell Powered Underground Mine Loader Vehicle
Barnes, David; Vehicle Projects LLC

Brief Summary of Project

Vehicle Projects LLC is developing 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 will evaluate the locomotive's safety and performance, primarily in surface tests, and evaluate its productivity in underground mines in Nevada and Ontario.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- Good application.
- Mine loader is a poor application choice relative to HFCIT technology validation goals.
- Integration of early-technology hydride storage and fuel cell into heavy-duty off-road vehicle could provide useful data.
- Although this project supports a niche-market application, it has high value in addressing challenges of hydrogen storage and operational safety.
- Project is doing a good job of exploring market niches for fuel cell vehicles now, rather than having to wait for the costs to come down.
- This build and demonstration project will highlight technology shortfalls; however, most of those shortfalls are already known and can be demonstrated through existing models and laboratory experiments.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- Source of hydrogen?
- The technical approach is reasonable considering the poor (relative to HFCIT goals) vehicle application choice.
- The systems integration experience with this prototype vehicle will provide valuable 'lessons learned' for subsequent generations of mining equipment.
- Project involves large number of participants, and appears to be well organized from a project management perspective.
- Very little is being done to overcome critical barriers required to have affordable fuel cells available to transportation applications.
- This is an excellent packaging exercise; however, the technologies being used are very immature.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- The project has not yet accomplished much relative to the fraction of total project funds consumed.
- Although some compromises were necessary, the integration of the fuel cell power source and hydrogen storage have been successfully achieved.
- More detail should be provided on technical accomplishments: lots of technical specifications were provided on the various components, but more detail on what has been accomplished should be provided.
- This team is making reasonable progress toward demonstrating that a fuel cell vehicle can be assembled.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Great collaboration.
- Collaboration does not appear to extend beyond project participants.
- Excellent relationships established with Caterpillar, Hera and other partners.
- Looks like good interactions with other companies (under subcontract), but not sure how much interaction is taking place with others outside of the project.
- This is an excellent example of qualified, well integrated manufacturing team; however, this team falls very short on research capability.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- Unlikely to finish project as planned within budget.
- The next generation vehicle should be an excellent example of the benefits of fuel cells and hydrogen as a safe, emission-free power train for critical environment operations i.e. mines.
- Plans for future work look reasonable, although more detail could be provided.
- Future activities will highlight that technical barriers do exist; however, these activities will do little to overcome those barriers.

Strengths and weaknesses

Strengths
- Well thought out plan and great collaboration.
- Ambitious integration of hydride storage and fuel cell on specialized off-road vehicle.
- Sound systems engineering expertise in supporting this project.
- Well organized project with clear responsibilities for all parties involved.
- Appears to be making significant progress within relatively short timeframe.
- Well integrated manufacturing team with reasonable cost share.

Weaknesses
- Source of hydrogen (and effects) not included.
- Application is poor fit with HFCIT program technology validation goals.
- Technical progress rate significantly lags project funds consumption rate.
- Can 'lessons learned' from this special application vehicle project be of use to conventional vehicles?
- Need to show how lessons learned from this very specific market niche could be applied to other applications (for example, light-duty automotive vehicles).
- Very little activity directed toward research to overcome technology barriers.

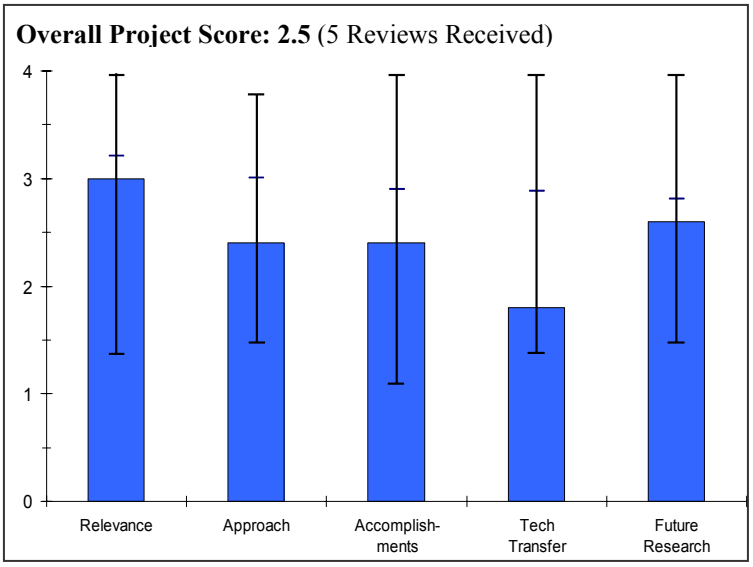
Specific recommendations and additions or deletions to the work scope

- Investigate on-site hydrogen sources.
- Re-plan project for graceful completion without additional funding.
- Only one more development generation of this vehicle is justified. Thereafter commercial viability should be achieved.
- Upon completion of project, write an NHA or SAE paper on the lessons learned and technical performance achieved from this project.
- Efforts need to be put in place to assure that performance, reliability, durability, operability, etc. data are collected and made available.

Project # TVP-14: Hydrogen Transition Infrastructure Analysis
Melendez, Margo; National Renewable Energy Laboratory

Brief Summary of Project

The National Renewable Energy Laboratory (NREL) infrastructure development analysis seeks to understand the benefits and drawbacks of various options for installing hardware for a developing hydrogen demand. Work focuses on the 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 **3.0** for its relevance to DOE objectives.

- Hydrogen as a transportation fuel in the context of GIS seems supportive of transportation orientation.
- Relevance high for actual hydrogen implementation but this is a little too academic and theoretical.
- This project addresses long-range HFCIT goals involving Systems Analysis and System Integration.
- This project is important if it was felt that DOE needed independent data on the build out of hydrogen fueling stations, but the industry might have provided enough of this information to have made educated estimates of the results without the expenditure of federal research funds. Look at the publicly created results for the California hydrogen highway.
- The results are important only if the federal government is going to direct or substantially pay for the fueling infrastructure. This is unlikely in this market environment.
- An industry analysis would be more credible.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- Correlation in GIS environment. Should consider including pipelines and commercial hydrogen resource locator.
- Focus is lacking. Too many mandated assumptions like driving patterns and distances.
- Alternative storage methods not considered.
- Focus is only on interstates- not adequate.
- Approach should give more emphasis to lessons learned from prior alternative fuel infrastructure experience.
- Approach should give more emphasis to hydrogen transition infrastructure strategy.
- Approach should give less emphasis to specific geographic locations.
- The approach is appropriately analytical with good data and assumptions.
- The regional approach, with fleet data, seems inconsistent unless the fleet data excludes the local fleets.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- 284 stations along major interstates minimum to achieve national coverage - $1 billion.
- Has just made some assumptions but hasn't validated anything yet.
- Seems superficial to date.
- Good progress relative to funds expended.
- Nice application of GIS.
- The results are clear and should be considered useful.
- The build-out of the fueling infrastructure is important but industry has said this is not a critical barrier, at least at this time.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- Analytical results. The communications with stakeholders not addressed.
- Doesn't look like many outsiders were consulted, i.e. vehicle teams, GM, Shell, etc.
- Auto companies should be included.
- Good H2A collaboration.
- The collaboration with industry is not evident.
- The data may guide DOE but it is unlikely to influence other decision makers.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- More detailed info at end position. Connect to feedstock sources.
- Needs to focus on barriers to establishing a network of refueling stations.
- Planned transition strategy development will be very important.
- The future research indicates important questions, but they are largely marketplace questions that require marketplace answers.

Strengths and weaknesses

Strengths
- Good presentation.
- Good access to lots of data.
- Starts to address very important issues associated with any hydrogen infrastructure initiation.
- Good collaboration with H2A.
- An analytical approach to estimating the magnitude of the build out of the infrastructure will be important to some decision makers.

Weaknesses
- Stakeholder usability was not addressed. Seem isolated from many state projects.
- Too theoretical and not realistic. Assumes government driven rather than industry and economics.
- Looks only at interstate highways.
- Looks only at compressed hydrogen.
- Needs collaboration with auto companies.
- Should have developed input from potential station owners/investors sooner.
- The study has credibility only if those that have been making decisions on building fueling stations are involved directly in this study.

Specific recommendations and additions or deletions to the work scope

- Address weaknesses.
- Collaborate with existing gasoline infrastructure participants. Collaborate with other refueling projects.
- Correct weaknesses.
- Continue as planned.
- Develop input from potential station owners/investors to support strategy development.
- Subject this study to an independent industry review team to provide credibility. Then the results will be more useful to decision makers.

Safety and Codes & Standards
Summary of Annual Merit Review Safety and Codes & Standards Subprogram

Summary of Reviewer Comments on Safety and Codes & Standards Subprogram:

Reviewers for the Safety and Codes & Standards Subprogram commented that these projects were very relevant to other DOE activities such as the Tech Validation subprograms, and necessary to furthering the development of national and international codes and standards for the hydrogen economy. Additional focused efforts in critical areas such as hydrogen fuel quality, set-back distances, research and demonstration on the gaps in hydrogen codes and standards, understanding risks, and materials research needs to continue in order to make progress in this area. Leveraging efforts and coordination among universities, standards organizations, national labs and industry was seen as valuable and effective to get a consensus on codes and standards.

Safety projects for this period included the HAMMER emergency response training activities. Comments on the project indicated that there is a need for safety training and that training for first responders is very important to the President's Hydrogen Fuel Initiative. The Safety Panel is continuing its activities this year and has become an important part of DOE activities to ensure the projects funded are operating with safety as a priority and that uniform safety protocols are being utilized. Comments received by some reviewers indicated that the Panel will experience a significantly increased workload in the near future and the Program should accommodate this need.

Evaluating hydrogen demonstration/refueling systems on a world-wide basis is a project that received relatively high scores. A database is being developed to document the International Energy Agency activities and will model and assess these stations.

Continuing concern was expressed over the low or reduced level of funding for the Safety and Codes and Standards subprograms, due to Congressionally-directed projects, and how the low funding could jeopardize maintaining overall DOE objectives.

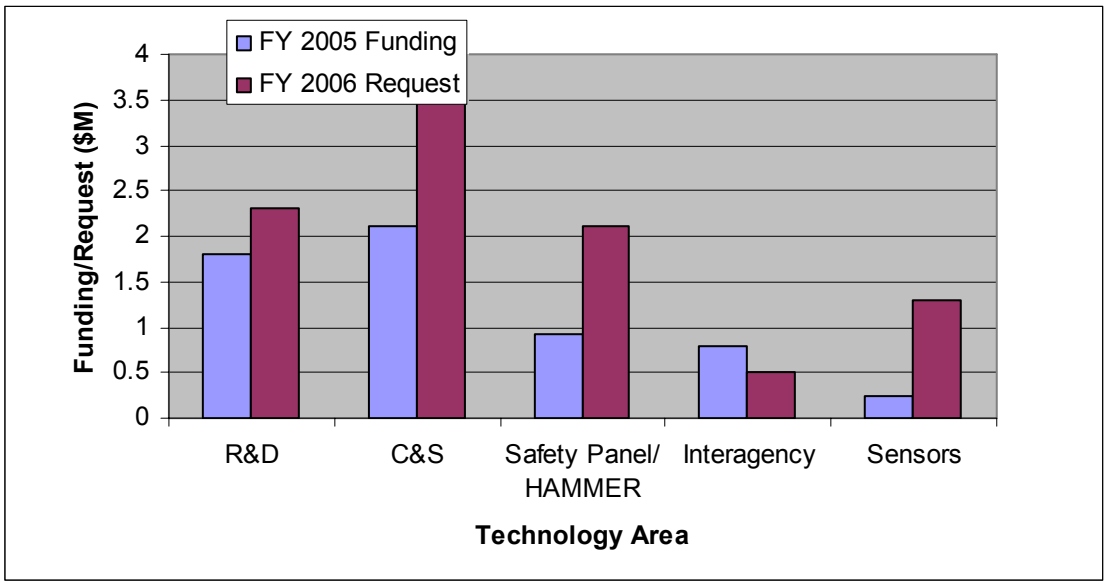
Recommendations by the reviewers included:
- More R&D is needed to fill in the gaps in hydrogen codes and standards – more focus should be made on following the roadmaps presented by the Codes and Standards Technology Team.
- Safety needs to be highlighted and tracked during the DOE demo program. Recommend DOE consider ways of focusing safety as an absolute.

Safety and Codes & Standards Funding:

The 2005 funding budget for Safety and Codes & Standards 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 the low level of funding could inhibit the level of activity that can take place.

The National Academies' report recently issued recommends additional funding for safety, codes & standards, for DOE to broaden its team of safety experts, and to increase public awareness of hydrogen safety issues to facilitate the transition of hydrogen vehicles into the marketplace.

Safety, Codes and Standards Budget:



Majority of Reviewer Comments and Recommendations:

Reviewer scores for the Safety and Codes & Standards Subprograms were average to high (average score for the projects was 3.18). The lower scores were related to the activities with Los Alamos National Laboratory technical support to the EPA/DOT/DOE development of global technical regulations (GTR) and to the International Partnership for the Hydrogen Economy (IPHE) on its efforts on regulations, codes and standards. Comments received pertained to the lack of technical R&D accomplishments, and additional coordination to be made with DOT/NHTSA. Recommendations to DOE are listed below. DOE will act on these reviewer recommendations as appropriate for the scope, focus and coordination of the safety and codes & standards effort.

- **Codes and Standards Development and International Coordination:** Focus on hydrogen fuel quality, and accelerate efforts on specifications. IEA projects should be coordinated with IPHE activities.

- **Safety Training:** Training over one million first responders requires some innovative planning. As part of this HAMMER effort, consider "training the trainer" at the facility and then develop a web-based training module for the masses with local practice. Need to capture "lessons learned."

- **Safety R&D**: Regarding the Panel activities: the checklist of items that the Panel uses to review projects should be described and include suggestion of alternatives to mitigate risks that may occur. More information should be provided on how projects will be selected for site visits.

Project # SA-01: Safety, Codes and Standards
Davis, Patrick; U.S. Department of Energy

Brief Summary of Sub-Program

The purpose of this Safety, Codes & Standards Sub-program Overview and introduction is to describe sub-program goals/objectives, budgets, barriers/targets, approach to R&D, technical accomplishments, interactions and collaborations, solicitations and awards, and future directions. As such, it sets the stage and puts into context the R&D and analysis projects, which will be presented in this sub-program area during the Annual Merit Review.

Degree to which the Sub-Program area was adequately covered and/or summarized

- Summary was adequate. The Team Lead was effective in articulating the scope and program requirements.
- Team Lead did an excellent job of describing the sub-program and all aspects of the research going on.
- Excellent sub-program coverage.
- Very comprehensive and concise presentation of the program. Key issues and priority issues were discussed appropriately.
- Team Lead did an excellent job of fully covering the sub-program in a twenty minute session.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- Problems and issues were identified to some extent. Improvement may be possible if greater attention is given to specific problems and issues, which would lead to a more focused program and result in better project solutions and a smaller number or issues.
- Several barriers for domestic codes & standards development as well as GTR were identified and several projects are underway to address them. However, the low level of funding for this sub-program inhibits how much activity can take place.
- Problems were identified; however, more work is needed on resolving setbacks.
- Yes, good overview of program barriers and plans for resolving within each area. Accomplishments and future work are progressive and logical.
- Team Lead focused his talk on the top issues and milestones and highlighted areas that were not proceeding as rapidly as desired.

Does the Sub-Program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Given the limited resources, the program should consider focusing (and directing funding) on topic areas that are not addressed or under the purview of other entities. For example, the GTR is better handled by DOT, and safety training by state and local fire departments.
- Performance-based standards are appropriate and preferred in some areas. In other areas, particularly fixed infrastructure, prescriptive standards may be more appropriate. These should not be excluded, and the Federal government should not dictate to the development organizations and local and state AHJs that may use them.
- The program is managed well. The program is effective in many areas; the areas for continued improvement are noted above.
- Management of this sub-program has improved in the last year and the team is becoming more focused. In order to be more effective, additional funding will be needed for these efforts.
- Sub-program adequately covers R&D needs.
- Program requires the management of many, many stakeholders and the sub-program has secured dedicated and expert managers. Communication and the ability to find compromise is a key need in this area and the sub-program managers have been very effective in this as evidenced by the evolution of program priorities.
- Funding status is unfortunate. The program requires full funding in 2006 or will jeopardize progress due to inter-related nature of this sub-program.
- Team Lead is doing his usual good management of the sub-program and making appropriate priority decisions.

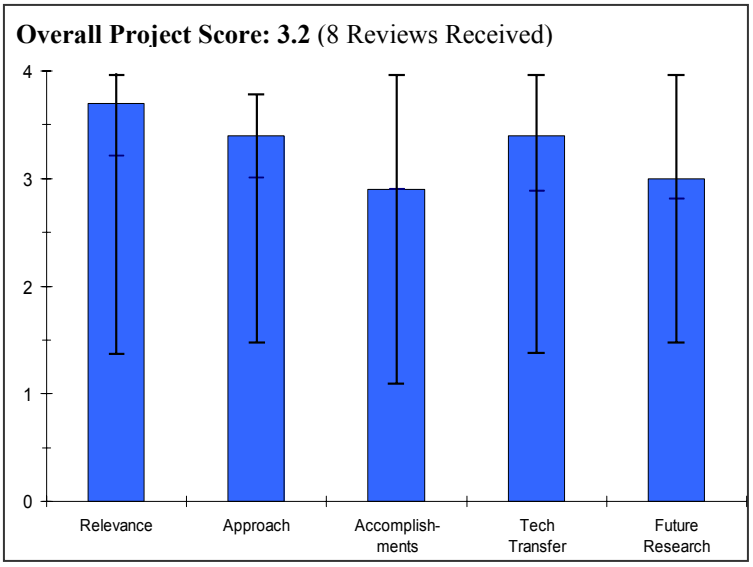
Other comments:

- Consideration should be made for the Hydrogen Safety Panel activities, as it appears to be a significant increase in workload in the near future.
- Team Lead has been very effective in managing the various industry stakeholders interests and recognizes when to compromise and when to pursue change. Current program is very well managed and structured. Funding for 2005 is disappointing and 2006 budget needs to be funded to provide best chance of meeting 2015 DOE targets…under-funding in 2006 will jeopardize maintaining DOE overall objectives.

Project # SA-02: Hydrogen Codes and Standards
Ohi, Jim; National Renewable Energy Laboratory

Brief Summary of Project

In this project, the National Renewable Energy Laboratory (NREL) works 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 is 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.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- 50% complete. Very relevant to all Technology Validation and other DOE projects. Good cooperation.
- Development of uniform and rational codes and standards are key enablers to the achievement of a hydrogen economy.
- National consensus on codes and standards and a path to timely regulation is critical to commercialization of the hydrogen economy.
- The project embraces all the necessary aspects to support the development of codes and standards for the hydrogen economy.
- The relevance of the subject is in line with what industry and government needs are regarding objectives.
- Coordination by DOE to ensure U.S. codes and standards development is completed effectively and completely is critical to meeting the 2015 goals. The efforts to date have brought a varied and diverse group of stakeholders together to work on a common effort with a common goal. Without this instrumental role from DOE, today's progress in U.S. codes and standards in hydrogen would not have been realized. Continued effort by DOE is needed and is valued by industry.

Question 2: Approach to performing the research and development

This project was rated **3.4** on its approach.

- Good – several slides on approach.
- The project and PI did a solid job of assessing the pathways and outlining a path forward. The project may benefit from a narrower focus. International standards and regulations should not be under this program.
- Reasonable approach for unified national agenda and National template.
- Transportation is receiving more attention than residential and commercial applications, which are not previewed in the objectives.
- Roadmap is an excellent document — consensus gained in response to entire industry.
- The approach by DOE has been appropriate and well planned. The effort in this arena cannot be accomplished over night and required a long-term view to ensure a successful working result. The National template has been extremely effective in moving development forward as has DOE's assistance to certain SDO/CDOs where

specific efforts are underway to resolve codes and standards issues ranging from setbacks to tank material usage.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.9** based on accomplishments.

- Good slide on time frame. Good progress from last year. Good progress in fuel quality.
- The pathways and roadmaps were detailed, but the presentation lacked information on specific accomplishments over the past year.
- To date most accomplishments appear to have been administrative.
- The presentation states that the project is 50% complete and ends in September 2005. There is little real world data to support codes and standards development in many areas, particularly performance based requirements. Continued support may be required for redrafting in those areas.
- Hydrogen fuel quality plan looks good.
- Progress was not shown very well during the presentation.
- DOE has maintained a broad scope yet implemented prioritized focus in specific critical areas such as; hydrogen quality, storage vessel materials, set-back distances, and material compatibility. DOE considers industry and other stakeholder inputs effectively to ensure priorities are tested.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Good leverage with other organizations.
- Work with some of the universities (Hawaii, Sandia, etc.) is valuable and it is clear how this transfers over and benefits a number of users. The project could benefit if the direction utilized greater input and direction from the code and standards organizations to meet their needs vs. the codes and standards organizations being led by DOE needs.
- Multiple interfaces with SDOs, international committees, etc. seem valuable. Key will be to see if interfaces prove able to make concrete progress.
- Good coordination.
- Getting better every day!
- Everyone involved in codes and standards should be talking to each other.
- Technology transfer/collaborations appear to be good between U.S. and international organizations.
- Outstanding integration and leverage across industry, academia, labs, and organizations has been achieved in this effort. DOE has been effective in keeping order and providing progress while working with numerous entities.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Good future work plan to test and validate.
- Future research for fuel quality is fine. Other areas, such as ISO TC 197 lack clarity. Work efforts need to be more than just coordination. They should focus on providing data vs. trying to control the process.
- Reasonable future plans for fuel quality and other enabling R&D are more in line with research-orientation of DOE program, but recognize that interfaces with code setting organizations are necessary.
- Future work covers most of the possibilities.
- Hydrogen fuel quality work well-planned. Execute the plan.
- Consider the next area in the roadmap and define the R&D effort.
- Develop an infrastructure R&D project.
- Future plans for research need to be made clearer.

- DOE's MYPP and internalization of the FreedomCAR CSTT Roadmap reflects DOE's future outlook and planning. Timeline planning with significant accomplishment targets are clearly defined and appropriate.
- Future efforts reflect past accomplishments, yet DOE continues to push itself to achieve further success as evidenced by its forward planning objectives.

Strengths and weaknesses

Strengths
- Able to link up NHA with DOE and USFCC to coordinate activities. Project draws on talent from many other good organizations - good mix of industry and university input.
- The interaction among participants seems to be strong.
- Hydrogen fuel quality plan has come together in a short amount of time.
- Leverage of earmark funds into useful work should be a model for the rest of the DOE hydrogen budget as a way of dealing with earmarks.
- Roadmap appears to be an excellent plan.
- Program Manager (Pat Davis) and Codes and Standards Lead (Jim Ohi) are "the" keys to the progress realized to date. Pat has been extremely effective in managing industry issues and ensuring production is achieved. Jim's ability to keep a herd of cats moving forward while being patient at the same time has allowed the National template to become reality and has also enabled the successful combination of the Codes and Standards Coordination Committee with the USFCC codes and standards work and the NHA codes and standards work; a feat that would have been unheard of only a few years ago. Strong focus by all involved and dedication to see results keeps this effort strong and purpose-driven on a daily basis.

Weaknesses
- Question whether there has been adequate progress considering the high budget of the program. The presentation was not clear regarding how the $1 to 2 million annual budget is being spent.
- Cooperation with other countries beyond Japan and EU can enlarge the results and can help to overcome barriers for the adoption of international codes and standards.
- R&D and template budget activities supposedly are separate subtasks now, but we didn't see any detail on that.
- Need detail on amount going to R&D.
- The progress and collaborations need to be detailed better.
- Funding. Codes and standards management requires the funding to ensure this critical bridge to commercialization is built. Without codes and standards, the entire technical R&D will not matter. Codes and standards funding, while small in relation to other R&D efforts, needs to be fully funded each year to avoid jeopardizing having new technologies unavailable to consumers.

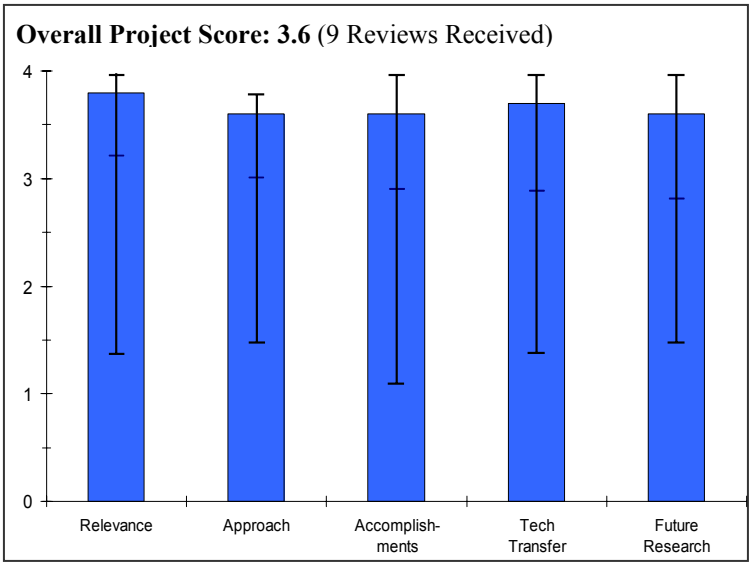
Specific recommendations and additions or deletions to the work scope

- Focus on fuel quality, data collection and underlying research. Eliminate GTR.
- More attention to residential and commercial systems is desirable.
- Move more budgets toward codes and standards R&D and decrease general contributions to trade associations.
- Accelerate efforts on hydrogen quality specifications.
- Correlate fuel cell vehicle performance in demonstration/validation program with respect to analyzed fuel quality provided by infrastructure partners. Would involve Keith Wipke/NREL as well. In the absence of budget detail between R&D and subcontracts to SDO's etc, here's my input based on guesses where the money is going: delete contract with CSA on HGV4, since this should be handled by CSA constituents, delete contract with NFPA, since NFPA 52 and 50A/B are being dealt with by industry, delete NHA support since they are not an SDO. Let other NHA members support it, unless there is a compelling reason for giving money to NHA that isn't obvious.
- Other subcontracts are vital to the program.
- More concentration research activities related to codes and standards and actual progress.
- Full annual funding.

- Consider how to include assisting NETL efforts for new products as this is an important step to placing hardware and systems into the marketplace. Once standards are established, the products typically require third-party testing. Not all manufacturers can fully fund these efforts during "validation" phase projects.

Project # SA-03: Research and Development for Hydrogen Safety, Codes and Standards
Keller, Jay; Sandia National Laboratories

Brief Summary of Project



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) will develop 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. It includes investigation of hydrogen ignition and combustion processes and subsequent heat transfer from hydrogen flames. Technical information is contained in simple engineering models that are used for rapid assessment of different scenarios and risk analysis. 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. SNL will compile existing data on hydrogen compatibility of materials from reports and journal publications. Material testing is being conducted to fill information gaps identified during the literature search. The focus is on materials data for applications that involve the storage, distribution, and consumption of high-pressure hydrogen gas. 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.

- Materials testing proposed is very critical to HFI. Hydrogen gas jet behavior understanding is very important to HFI.
- The program is addressing many fundamental knowledge needs necessary to facilitate codes, standards, and general technology advancement.
- Hydrogen safety, codes and standards are key enablers for a successful hydrogen economy.
- Key codes and standards gaps have been identified and data is being generated to close gaps.
- The relevance of the subject is in line with what industry and government needs are regarding objectives.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- The program is well thought out and organized.
- Approach seems reasonable.

- The speaker should not be so negative toward "old" data. Despite their age they have served a very large industry quite well for many decades.
- National labs, universities and knowledgeable contractors are being used to close key gaps in a collaborative way.
- Consider describing the setback work in different terms; the word setback has negative connotations.
- Why not hydrogen refueling station design optimization or something like that?
- The use of setback just entrenches in people's minds that hydrogen is different and dangerous.
- It is dangerous but so are other things that we manage all the time.
- Approach appears logical.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.6** based on accomplishments.

- The PI's clearly understand the barriers and challenges and have a clear plan to achieve their goals.
- Good progress so far on materials.
- Doing interesting and potentially valuable work in hydrogen embrittlement.
- Flame jet research is very valuable and should be shared widely with SDOs.
- More important to do materials research than participation in codes and standards activities.
- Setback distances are still unresolved and need more work. Models developed to date are robust and uncertainty has been quantified.
- Materials compatibility very useful.
- Good work in materials compatibility.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- Partnership is very comprehensive and each role clearly understood.
- PI enumerated several industries, SDO, and other entities that are benefiting from the results and contributing key input and materials. This needs to be maintained.
- Good collaboration with Miami, JPL, SRI, others on flame jet work.
- Unable to assess value of collaborations with other industry partners as PI declined to name them. Might have scored higher otherwise.
- The role of partnership alliances is very important to this effort.
- Leading universities and national labs have been used to generate data needed to close gaps.
- Continue interactions with other groups.
- Technology transfer regarding materials compatibility with hydrogen is excellent. However, Collaborations is lacking with other entities in the U.S. and internationally doing similar work.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- The proposed future work should provide valuable results in materials and hydrogen gas behavior understanding.
- Future research on materials compatibility and flame release is on target. There are other areas of needs but the program appears focused within its financial constraints.
- Future plans look good. Continue emphasis on materials properties; testing of low alloy and stainless steels.
- Future work compliment and build on work already completed.
- Move toward work on solutions to problems SNL has identified.
- Future Plans for research are not very clear other than future activities.

Strengths and weaknesses

Strengths

- Project well thought out.
- Investigators and the SNL provide a wide variety of capabilities related to hydrogen safety for fuel cell vehicles. The metallurgical aspects of the work are most valuable.
- Key codes and standards gaps have been identified and data is being generated to close gaps by leading universities and national labs. Future work compliment and build on work already completed.
- Good focus and organization.
- Some of the best capabilities and understanding of hydrogen behavior in the world.
- Analytical approach to assisting with codes and standards.

Weaknesses

- The flame behavior work should endeavor to tie in to "real-world" scenarios, which add considerable complexity to the behavior of a leak, hydrogen dispersion, and flames/deflagrations/detonations.
- Quite correctly the investigators are less focused on flame properties for establishing the separation distances utilized in natural gas and hydrogen fueling stations. They should continue to downplay connections between separation distances and fueling station dimensions.
- Set back distances are still unresolved and need more work.
- There are no weaknesses to be pointed out.
- Not clear who is doing the risk assessment work, NREL or SNL, or both. Most companies will do their own risk assessment.
- Unclear regarding future efforts in tank testing what the scope and makeup will be.

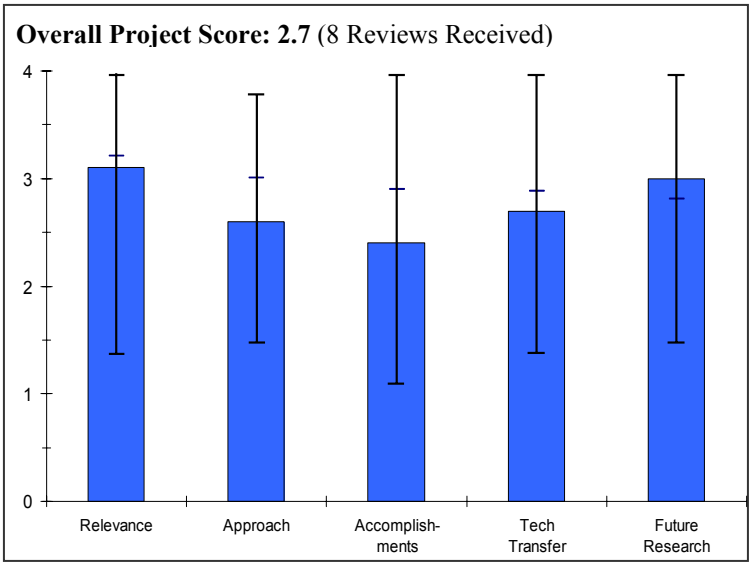
Specific recommendations and additions or deletions to the work scope

- Risk assessment should be narrowed or dropped. The project did not appear to fully incorporate many well established procedures already in use by government regulatory bodies.
- High-strength alloys will also need to be tested. This should be done in conjunction or in response to partner and collaborator requests and input.
- The flame studies are more of an academic interest than a practical interest. Interesting to know but what does one do with the results? The work on frequencies of releases is relevant and should be expanded.
- Financial and people resources to complete FY06 are tight, cut backs in these areas would have impact on delivery of hydrogen program objectives.
- On the setback work, we may not have the required 75 +/- feet, so add work to identify and validate an alternative solution to setbacks. Design optimization.
- Identify how to safely store hydrogen above ground with materials development to block heat from failures.
- More R&D is needed to fill the gaps in hydrogen codes and standards - more focus should be made to follow the roadmaps presented by Codes and Standards Technology Team.

Project # SA-04: International Standards and Regulations
Padro, Cathy; Los Alamos National Laboratory

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 **3.1** for its relevance to DOE objectives.

- Although international collaboration is not critical to the U.S. goal of reducing U.S. reliance on foreign oil, these kinds of programs are important for international collaboration and vehicle homologation.
- Supports DOE objectives for GTR.
- The development of international standards and regulations is a key point to the Hydrogen Fuel Initiative.
- There is a relevance of the subject to the RD&D plan objectives. The execution of this, however, has not assisted to the overall DOE objectives.
- International coordination by DOE to ensure U.S. codes and standards development is not only effective but consistent across international boundaries. It is a significantly important effort in a global economy. Ensuring international consideration is also helpful in leveraging limited resources and accelerating learning and development in this area.
- Providing the U.S. auto/fuel cell/energy industries with visibility into the morass of the EU regulatory process is a good thing. However, other than an information sharing exercise, this project provides little value to the charter of the Codes and Standards Tech Team, which is to identify and initiate research into the science that will enable a good basis for emerging hydrogen codes and standards.

Question 2: Approach to performing the research and development

This project was rated **2.6** on its approach.

- Seems to be working with the right international bodies.
- Consider how DOE or the U.S. can better influence the GTR process to accommodate our needs without jeopardizing other participant needs.
- The approach has not only failed to add to the progress of international codes and standards, but has in fact added to the compounding divide between U.S. and worldwide GTR progress.
- The international approach is being modeled, to some degree, off the U.S. National Codes and Standards Template, which has proven effective domestically. An International Codes and Standards Template has been put forth which can have the same aligning effect as it had in the U.S. Together with the U.S. TAG, IPHE, and integration with domestic codes and standards efforts, the approach was well rounded and comprehensive. The approach also recognizes the challenge ahead and provides for flexibility to ensure success.

- NHTSA, not DOE, sets vehicle regulations for the US and is the representative to those international forums that discuss global regulatory issues related to vehicles. LANL could perform a monitoring function at the request of DOT (NHTSA).

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.4** based on accomplishments.

- There are no technical or R&D accomplishments in this project; more of a status report on international collaborations.
- Collaborations have been slow, laborious and challenging.
- GTR roadmap development with strong input from DOE/U.S. is important work.
- Not clear if IPHE accomplishments are result of these project activities.
- This category tends to be N/A as the projects goals are not necessarily technical in nature.
- The progress is adequate in view of the difficulties related to the international character of the activities under way.
- A primary early test bed for leveraging international collaboration is being explored with the issue of hydrogen quality. In this area there has been good early work and progress that has brought the global stakeholders to the table. Other areas will include providing commercial opportunities for U.S. industry to have business internationally and effectively being managed via the U.S. TAG and the UN processes.
- LANL participation has increased the awareness of the Codes and Standards Tech Team into the progress (or lack thereof) of the regulatory process of the hydrogen economy within the EU.
- I do not necessarily see a limited DOE role in regulatory issues as a problem.
- How is LANL (DOE) seeking to increase U.S. industry support within this international framework? Has there been an increase in that support?

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.7** for technology transfer and collaboration.

- Seems to be working with the correct international bodies; many collaborations.
- Good collaboration with DOT, ISO, U.S. TAGs and others with respect to GTRs
- Coordination with other institutions clearly exists.
- It is difficult to evaluate how deep the cooperation among the participants is.
- There have been attempts at collaborations, but there appears to be a problem with gaining U.S. consensus and then representing this in outside forums.
- Expect that the review in 2006 will result in a "4" rating, but as this effort is relatively new, opportunities for improved coordination exist and are known by DOE for action. Not an easy challenge by any means, and the work to date has been impressive.
- It was not apparent that NHTSA provides much guidance to LANL for this effort.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- Continued support needed and planned, but not directly relevant to hydrogen R&D.
- Identify more details regarding future work including what the goals of that work will be.
- Future plans for research related to this subject are not defined.
- The long-term view of EU regulations and their impact to U.S. commerce are well understood by DOE and are a key future deliverable. This awareness provides DOE with critical insight as to how to manage relationships and know when and how to negotiate and compromise. A key value item of this effort is its "future" objectives.
- Not applicable as there is no research ongoing.

Strengths and weaknesses

Strengths

- Connections and interfaces with international codes and standards body already established.
- This effort is valuable to the overall effort. It is probably fitting that a national lab performs this effort.
- The problems are well identified and addressed.
- Relevant topic.
- Key players involved by DOE understand the international working of codes and standards and know who to engage outside the U.S. to be effective. Communication is a key tool in this effort and DOE is able to keep international stakeholders informed and allows DOE to have their "ear to the rail" should items of interest or concern becomes apparent. Committed personnel are very effective and appear to take personal pride in accomplishing this very challenging effort.
- Presentation provided visibility into the GTR process.

Weaknesses

- Overall coordination of codes and standards goals between the U.S. and Europe.
- It is not clear how the U.S. and EU can come together on regulations despite the efforts of the DOT. Will Pedro's work have any meaningful acceleration on this challenge?
- Most of the barriers are still active.
- Not effective in assisting the GTR process.
- Funding. DOE's requested annual budgets need to be provided in this most critical effort.
- Not sure what the Japanese "systems approach" is composed of. Usually, the Japanese auto industry follows the ISO process and adopts their documents. Up to this point, ISO is using a "component" approach building off the EIHP experience.

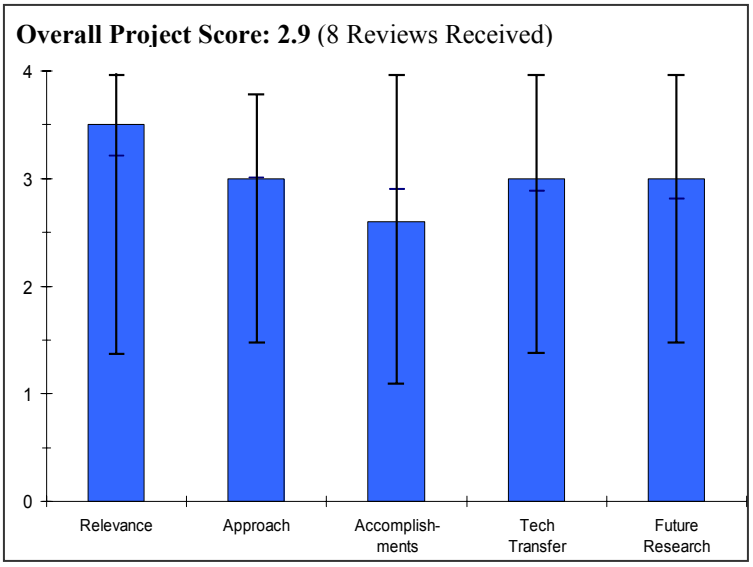
Specific recommendations and additions or deletions to the work scope

- This project would better serve the President's Hydrogen Initiative by assuming more of an input and advisory role to NHTSA. The U.S. effort would be better served by having the UN-recognized representative present at the international meetings, which is currently not happening.
- Develop methodology to more closely align U.S., European and Japanese bodies to refine GTR process.
- Stop activities and have OEMs discuss with North American industry representatives regarding direction of GTRs with NHTSA.
- Full annual funding.
- Consider how to further leverage IPHE to help maximize progress on key issues, such as hydrogen quality, test methods, and vehicle regulations. IPHE is a unique body that could perhaps do more in this area.
- Further LANL participation should be funded through the DOT portion of the Codes and Standards Tech Team budget and not directly through DOE. Allow DOT to decide if this effort is worth continuing.

Project # SA-05: HAMMER Emergency Response Training for the Hydrogen Economy
Kinzey, Bruce; Pacific Northwest National Laboratory

Brief Summary of Project

The DOE HAMMER training center in Richland, Washington is to be the location for a national hydrogen safety training facility. HAMMER will offer classroom and distance (satellite) learning, as well as live-burn and other hands-on props for training emergency responders, code officials, and others on the appropriate safety procedures corresponding to their respective responsibilities in the hydrogen economy.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Safety training is very important to the Hydrogen Fuel Initiative. The PI did not demonstrate that this is the most appropriate mechanism for developing this curriculum, and that it will result in fulfilling this national need.
- Training first responders to appropriately handle incidents will be important to the successful deployment of new technology and to minimize unnecessary public fear.
- Safety training for first responders and others involved in the codes and standards community is important for the success of the hydrogen economy.
- First responders need to be trained prior to public exposure to hydrogen through fueling and vehicle accidents
- This project addresses a show-stopper for the hydrogen economy.
- One spectacular accident and public opinion would not be favorable.
- This project directly attacks the two barriers identified and is a necessary component of the Hydrogen Fuel Initiative.
- Training of emergency responders is an essential element of the initiative.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- A training plan and timeline should be developed that is tied to the commercialization path for the various hydrogen related technologies, starting with those already in practice.
- A lot of work has already been done in predicting potential accident scenarios related to current commercial practices and ongoing codes and standards efforts. These scenarios should be analyzed and built on in order to set priorities for training.
- Funding and time is always a constraint; therefore, it is critical that up-front effort is placed on identifying highest probability scenarios, benchmarking current first-responder protocols, and setting priorities based on gaps that are identified.
- Reasonable approach.
- Addresses a comprehensive list of hazard scenarios.
- Training one million first responders is mind-boggling.
- Concept of HAMMER is great.
- Develop a web-based training module for the masses of first responders and physically "train the trainers" at the HAMMER facility.

- The approach enumerated is good. Involving both OSHA and NFPA is a beneficial strategy.
- Targeted audience will enable a smoother transition to the hydrogen economy.
- Training materials should stress fuel equivalence.
- The approach does not adequately address the need to train all first responders, emergency personnel, etc.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- The PI did not demonstrate that the project team understands the breadth of real-world hydrogen and fuel cell first responder training issues.
- Pilot course planning is good and will be important for setting the tone of future efforts.
- Project has not been well funded so far. Will need greater funding to be fully successful.
- Much accomplished in little time at little cost.
- No additional comments are necessary.
- Just really beginning.
- More relevant next year.
- The only suggestion here would be to have an additional training facility on the east coast to make the training more available to those first responders. Of course, this would entail a larger budget.
- The "training of the trainers" could be followed up with selected scenarios in various locales to give "newly trained" a feel for dealing with issues in their own backyards.
- Ahead of schedule, but the path chosen does not adequately address the real need.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Two of the partners listed are not as engaged as the presentation indicated. The project needs more technical input beyond that of the CaFCP to address the full scope of hydrogen infrastructure and potential incident scenarios.
- Good collaboration with multiple organizations.
- Good list of collaborations; all seem reasonable and able to contribute.
- Good coordination with state and federal agencies.
- This is going to be huge task. It will be difficult to come up with the answers.
- Need a plan to engage the 70% who are volunteers.
- Once program fully underway, develop modules for set-up at other sites around the country.
- Appropriate use of coordinating organizations. ICC and NFPA are the two predominant code-setting organizations. As the training progresses, perhaps lower-level officials could be included.
- Good collaboration with many of the organization that will be impacted by safety implications of the initiative.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The PI did not provide adequate detail to determine if the future work will accomplish the outlined objectives.
- Need to tie training schedule to commercialization pathways and current gaps in knowledge. When and where will technologies find their way into the marketplace?
- Although fueling stations are quite visible and are a high probability site for a potential accident, a great deal of up-front orientation of first responders has already occurred. Should these be the first target for training or are there other critical gaps related to commercial hydrogen uses that will have a greater negative impact?
- Good plans for the future. Need to expand training program and make it available to more students.
- Good. Project is fairly new and, to date, funded at a low level. Need to review again as program develops from paper exercise to hands-on training program.

- Presentation indicates clear focus on overcoming existing barriers.
- The harder issues of training the trainer and long distance training are being pushed off, but should be worked on now.

Strengths and weaknesses

Strengths
- The HAMMER facility is a world-class facility with extensive experience in training first responders. The success of this project can increase the probability of correct response to hydrogen accidents, which will be critical for achieving public acceptance of hydrogen technologies.
- Adequate planning.
- One common facility is a good idea.
- Excellent use of previous DOE investment in HazMat facility.

Weaknesses
- More emphasis should be placed on utilizing statistic-based probability scenarios and commercialization timelines for developing training curriculum content and schedules.
- No weaknesses were identified.
- How will you deal with the 70% volunteers?
- Who can pay for their training at PNNL?
- Not enough attention on the front end of the issues that must be addressed to train the million plus personnel.

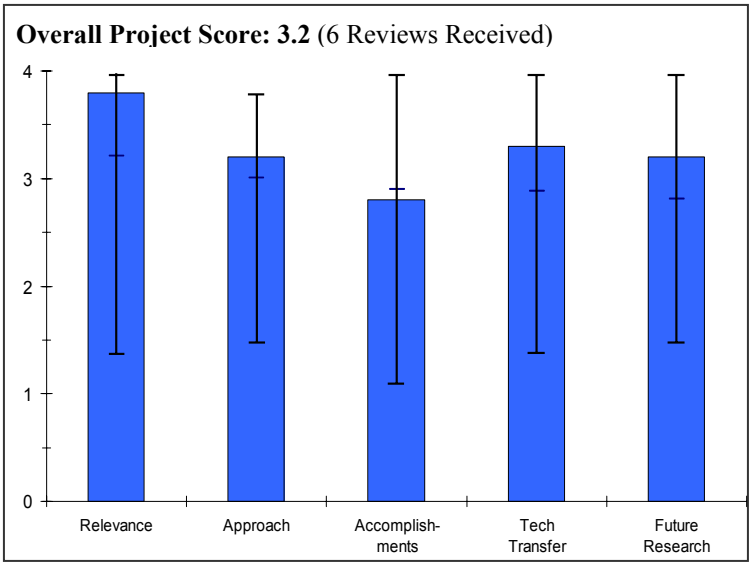
Specific recommendations and additions or deletions to the work scope

- Recommend that the project halt the development of curriculum until the fire safety community completes ongoing projects to adequately define its needs, requirements, and expectations.
- Closer coordination with related international efforts should be emphasized. Lessons learned from experiences in siting and permitting of the many hydrogen demonstrations should be incorporated into the planning.
- Training one million plus first responders requires some innovative planning. As part of this HAMMER effort consider "training the trainer" at the facility (i.e., one or two per jurisdiction) and then develop a web-based training module for the masses with local practice.
- Need to capture the "lessons learned" in a book or on the web.
- Detailed plan on training the trainer, distance learning, development of portable props, etc.

Project # SA-06: Hydrogen Safety Review Panel
Weiner, Steven; Pacific Northwest National Laboratory

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 helps capture best practices and lessons learned for the benefit of the program as a whole.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Project goals and objectives will go a long way to support the President's overall RD&D objectives.
- Will fill the gap of codes and standards needed for refueling hydrogen vehicles.
- Should ensure consistency between auto OEM and fuel providers for hydrogen codes and safety.
- The technical expertise of the panel members is good.
- Getting outside review of safety approaches is important for identifying potential issues and accident scenarios.
- Project strongly supports DOE goals and President's Initiative.
- Safety is an absolute requirement for the R&D plan.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- The approach does not seem to take into account the need to modify or develop codes and standards that make this an acceptable fuel of the future.
- More emphasis needs to be placed on information dissemination. 2007 is too late to communicate lessons learned and best practices. Need to seek more opportunities to, at least informally, communicate this information to the program participants.
- How do key issues that are identified get communicated to PI's working on similar projects? Who is responsible for doing this?
- Efforts to utilize as many methods of interacting with projects, rather than just relying on site visits (which will always be limited by schedules and cost) are commendable.
- Project well conceived and organized to meet current objectives
- Can the team handle a large ramp-up in workload in the future?
- Knowledgeable diverse members of Hydrogen Safety Panel insure key safety areas will be addressed.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.8** based on accomplishments.

- The presenter should have included the Checklist of Items they use for review.
- The checklist is the most important part of this presentation.
- The PI demonstrated an understanding of what is needed. Greater detail and information on what the accomplishments are would be beneficial. The information presented did not indicate if the project was successful in achieving safety or identifying safety incident precursors.
- Need to measure impact of Panel. What issues were identified? How did Panel's involvement improve safety? What trends should the program be concerned about? What best practices were identified and how can these be replicated throughout the program?
- Project has made very good progress this year, but as the need for more plan and site reviews increase, can the team keep up with the workload?
- Eleven site visits to date establishes solid base line.
- No additional comments are necessary.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- This project should have included some discussion on how the critical 'checklist' was developed.
- This project leader should make sure that participants are aware of the measures by which their projects will be reviewed and 'graded'.
- The TV Participants should be the first to have this information shared with them.
- Safety is critical. Greater information into how the efforts and lessons learned ties into the codes and standards development would be beneficial. The PI needs to clarify to whom and how the lessons learned will be conveyed.
- Efforts to communicate activities at national and international conferences are good.
- More emphasis needs to be placed on finding venues/opportunities to communicate lessons learned to all projects in the program.
- Strong collaborative effort with a diverse team.
- Cross section of relevant experience helps insure safety for entire hydrogen demo program.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- The checklist should include or suggest alternatives to mitigate risk that may occur to make hydrogen commercially viable.
- The project should focus on taking hydrogen from just an industrial setting to a consumer setting.
- Would have been useful to see more information on how projects will be selected for site visits in the future.
- A communication plan would help identify opportunities to have a greater impact on hydrogen safety practices.
- More detailed plans for future activities, including a timeline should be provided.
- "Guidance for safety aspects of proposed hydrogen project" has been and will continue to be updated.

Strengths and weaknesses

Strengths

- The panel consists of a very strong team of experts in a wide array of technology with practical field experience related to hydrogen safety, as well as safe practices in general. Incorporating safety plans right at the start of projects and reviewing these throughout the lifetime of the project is good.
- High level of expertise among team members.
- Diverse background of team members.

- Knowledgeable diverse members of Hydrogen Safety Panel insure key safety areas will be addressed. Guidance for safety aspects of proposed hydrogen project" has been and will continue to be updated.
- Site visits are essential to the success of the project.

Weaknesses

- The team appears short in the number of AEs and other operational safety officers and experts.
- It is unclear how much impact the panel is actually having. More emphasis should be placed on benchmarking progress towards improving safety and/or replicating best practices.
- Team could get overwhelmed with future workload.
- No weaknesses were identified.

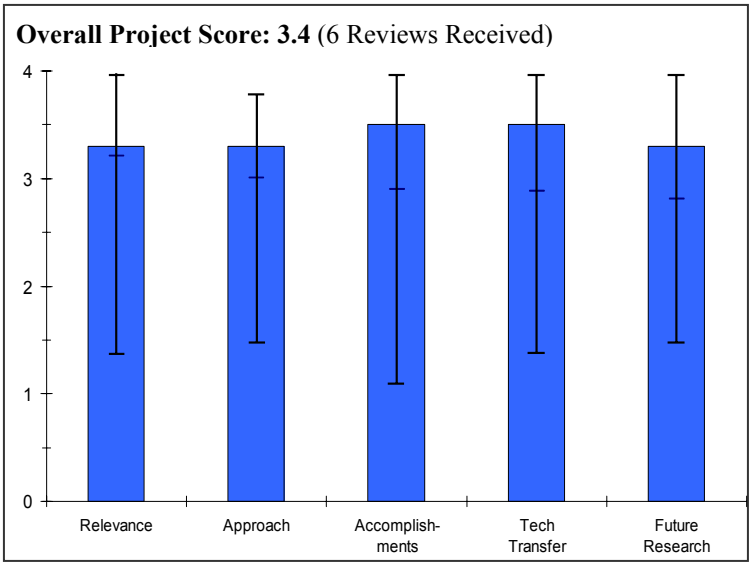
Specific recommendations and additions or deletions to the work scope

- Share the checklist with applicable DOE program participants, particularly the DOE Technology Validation Project participants.
- Focus on making 'hydrogen' a fuel of the future. This means implementing codes and standards that allow it to be a 'consumer friendly' fuel.
- Do not continue with existing 'mindset' of hydrogen as an industrial fuel.
- Consider a new or modified standard that moves it to a commercially viable fuel of choice.
- A requirement for regular reporting on the incident and near-miss checklist should be added.
- The scope is good. Would like to see a more detailed project plan with target dates for when reviews will be completed for major areas of RD&D. Out-year follow-up on projects that have been reviewed should also be addressed.
- Safety needs to be highlighted and tracked during DOE demo program. Energy companies typically open management review meetings with "safety scorecards." Recommend DOE consider ways of focusing/selling safety is an absolute. Safety stats from demo program need to analyzed, summarized, and marketed.
- Many of the safety aspects related to hydrogen can be used for other inert, toxic or flammable gases, e.g., ventilation, cylinder handling, etc. This extension can enlarge the project advantages.

Project # SAP-01: IEA Hydrogen Task 18: Evaluation of Integrated Demonstration Systems
Schoenung, Susan; Longitude 122 West, Inc.

Brief Summary of Project

Under the International Energy Agency Hydrogen Implementing Agreement, DOE is supporting leadership of a task to evaluate hydrogen demonstration systems around the world. Task members represent twelve countries and the European Commission. Detailed modeling and assessment of refueling stations and renewable hydrogen power systems is in progress. A documentation database is also under construction.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- This project is a good start in validating models and tools developed to date.
- This project is a good start in developing a database of hydrogen refueling station/system demonstrations on a global basis.
- The poster slides could have more clearly stated the true objectives of this Task 18, i.e.: (1) validating modeling tools already in-use, (2) documenting and comparing codes and standards used in real-world projects.
- This project will serve as an excellent facilitator of international information exchange for the Presidents Hydrogen Fuel Initiative and the International Partnership for a Hydrogen Economy.
- Project has good synergies with Technology Validation and Safety projects.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- A key approach for this project appears to be development of this database that allows for easy extraction of select data/information.
- This requires a searchable database.
- Presenter did not present any information on progress made to date in making this database usable for comparisons on key parameters between projects.
- It is not clear what is new about this evaluation.
- This effort will accelerate progress toward overcoming the barriers by making global research results available worldwide.
- Good ideas were presented.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- The goal as stated is to document performance of existing refueling stations.
- The scope appears to be focused more on electrolyzers.
- Value added for this project is to do more than one-of-a-kind station/system to truly understand performance and codes and standards used as it relates to safety.
- Examples of preliminary performance data identified with evaluated systems would have been great.

- Examples of consistency between codes and standards used with the various projects would have added value.
- Not clear if good or fair progress. The work appears to be soft technically for an evaluation project.
- Very good progress in development of the database and making information available on the website.
- Project has displayed good progress since last year.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.5** for technology transfer and collaboration.

- Contributions and sharing of information from the many partners in Task 18 appear to be fairly well coordinated.
- Member participation and support appears to be working fairly well.
- Twelve national collaborations are good, but it would seem that there are many other nations that could contribute.
- Project has displayed good areas of tech transfer.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- Not clear that future work will be forthcoming.
- Presenter noted that the project end date is year-end 2006.
- An assessment of achievements to date should be done to determine the need to extend this effort.
- No additional comments are necessary.
- Excellent plan.
- Project is 50% through. It looks promising. PI should stay on track.

Strengths and weaknesses

Strengths
- The project builds on other projects completed and could possibly provide valuable input for future projects or RD&D work.
- The global aspect and support from global entities provides a broader perspective of how the various technologies were implemented.
- Pulling the information together has some merit.
- Good interaction among participants.
- Models for evaluation and design guidance have good approach.
- Maximizing information availability to the world.

Weaknesses
- Refueling stations and stationary plants are too focused on electrolysis.
- Data analysis results appear to be slow and should be proceeding at a faster pace.
- Documentation of the codes and standards used in the various projects are needed now and not 6-months from now.
- It is not clear that the information, once gathered, will be highly useful. One problem is the moving target: Once a report is issued some of the information is already out of date.
- The projects present different sizes (powers) and different level of technology development, which can limit the validity of the results.
- Not apparent that this work is being coordinated with IPHE.

Specific recommendations and additions or deletions to the work scope

- Faster pace of documenting codes and standards used in the various projects in existence to date to identify: (1) best practices; (2) gaps.
- Broaden the scope of technologies to include SMR and other technologies.
- Attempt to include in the database more than one-of-a-kind systems (greater sample sizes) to allow for more conclusive data.
- The project is relevant to evaluate hydrogen demonstration projects under way.
- Safety aspects are not an issue for this project and each participant follows the regulations of its own country.
- Project should be coordinated with the International Partnership for a Hydrogen Economy.

Education
Summary of Annual Merit Review Education Subprogram

Summary of Reviewer Comments on Education Subprogram:

Reviewers considered the goals of the hydrogen education subprogram to be supportive of the President's Hydrogen Fuel Initiative. They noted that targeting public education in areas where technology validation projects are being developed is not premature and would have been helpful to recent hydrogen fueling station demonstration projects that were not fully supported by the local community. Reviewers praised the program's shift in hydrogen education priorities given zero funding for FY05, which dictated a tightening and refinement of the program focus.

Reviewers noted that education of safety and code officials is an important component of the education program. They judged the Hydrogen 101 project to be effective in educating state and local government officials. It should be noted that the education subprogram will target state and local government officials, safety and code officials, and communities where technology validation projects are located. Reviewers commented that all audiences may not be addressed given the lack of hydrogen education funds and noted that plans for proceeding with hydrogen education activities are unclear if there is no budget in FY06.

Hydrogen Education Funding:

There were no available funds in FY05. The budget shortfall was addressed by using limited carryover funds from FY04 efforts to sustain projects.

Majority of Reviewer Comments and Recommendations:

The number of education projects included in the 2005 merit review increased by 50% from the previous year and includes the baseline knowledge assessment, development of general educational materials to multiple target audiences, and activities targeted to middle school, high school, and college teachers and students, some of which are Congressionally-directed projects. Efforts related to state and local government and safety and code official education were included in the Education Subprogram overview presentation.

The reviewer scores for the education projects reviewed varied from as high as 3.7 to as low as 1.8. Some projects are new and had little progress to report, and reviewers commented on the need to focus Congressionally-directed projects more specifically on hydrogen and fuel cell technologies. Also, the Hydrogen Technology Learning Center projects awarded through the 2004 State Technologies Advancement Collaborative experienced significant funding delays that have affected performance. A few projects were commended for their exceptional management given federal financial constraints and the budget shortfall. To the extent possible, DOE will act on reviewer recommendations to support and refine the overall hydrogen education effort.

Baseline Survey: Excellent work and useful data. Results should be disseminated to a wider audience, through the DOE website, magazines, educational materials, and fact sheets in order to dispel hydrogen myths. Future surveys should dig deeper to understand about the effectiveness of education program tools. Need a contingency plan if no funding is available for the next round of surveys.

General Educational Materials: Projects address education barriers and provided needed tools. Develop metrics to measure effectiveness of educational tools. Ensure coordination with other education projects targeting similar audiences. Work with target audiences to define need, beta-test, and refine final products. Materials should be developed with an outreach plan in mind to disseminate efforts to the widest possible audience.

College/University Activities: Strong university collaboration and regional centers are a good idea. Training the next generation workforce is important to the success of hydrogen technologies. Collect and document data for demand of hydrogen education materials. Provide nationwide access to curriculum materials. Benchmark curriculum against previously produced hydrogen university-level curriculum, and collaborate with other universities. Collaborate with other DOE hydrogen education and safety activities. Participate with local industry to develop curriculum to provide knowledge and skills that students can immediately apply. Tighten focus to include more linkages to hydrogen technologies and the overall hydrogen initiative.

Middle and High School Activities: Excellent partnerships, inclusion of students and teachers in curriculum development, and actual hands-on teaching. Notable and measurable progress made in spite of lack of budget. Work with schools to obtain financing for hydrogen education materials. Focus more on long-term professional development for teachers.

Project # ED-01: Hydrogen Education
Cooper, Christy; U.S. Department of Energy

Brief Summary of Sub-Program

The Education Sub-program Overview presents DOE's hydrogen education strategy and provides context for the projects presented during the Annual Merit Review. The overview presentation describes hydrogen education goals and objectives, the budget, barriers/targets, approach, accomplishments, interactions and collaborations, solicitations and awards, and future directions.

Degree to which the Sub-Program area was adequately covered and/or summarized

- Excellent summary of sub-program needs, demonstrated links between this program and others. Also detailed ties between program goals and overall hydrogen initiative.
- Good overview of program efforts and accomplishments.
- Thorough overview of past projects and future.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- Yes. The budget's zero-balance was addressed with lessons learned from a year of functioning without funds.
- The problem of no 2005 funding was identified. Plans for addressing funding shortfall were mentioned; contractors must make do with FY04 funding. Plans for proceeding with hydrogen education activities are unclear if there is no budget in FY06.
- Yes, including FY05 budget and how FY04 money was carried over to sustain projects; perception remains that not all audiences are being addressed.

Does the Sub-Program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Focus has been tightened and refined. Budget dictated shift in priorities, which appear to have been well managed.
- Yes. The program area appears to be focused and managed well. The Hydrogen 101 sub-program seems to be effective at educating code officials and policy makers.

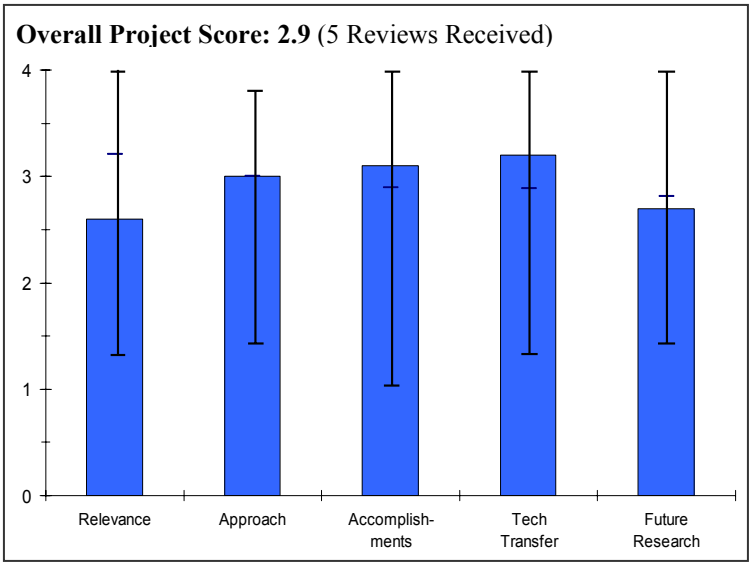
Other comments:

- Excellent, upbeat presentation in spite of gloomy budget reality. Commendable!
- It's good to see DOE targeting its public education towards folks in areas where technology validation projects are planned. Such efforts would have been useful to alleviate community concerns over recent hydrogen fueling station openings. Targeting the public in areas where technology validation is not premature. Education of code officials is also very good and a very important component of the education program.
- Remarkable management of a program that was not funded.

Project # ED-02: Hydrogen/Alternative Energy Center
Borger, Ruth; Lansing Community 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 will also build links between businesses and industry and educational institutions. Its goal is to become a resource center for educators, innovators, and policy makers.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- Good identification of barriers. Needs stronger connection to hydrogen – understands education/curriculum well, but the bridge to hydrogen needs development.
- The project supports the goals and objectives of the HFCIT MYRD&D Plan.
- Developing a 2-year degree program for community college students.
- Cross-cutting curriculum with HVAC and automotive.
- Includes transfer curriculum for 4-year degree.
- Development of hydrogen curriculum should increase number of informed students and faculty on hydrogen issues.
- If you are looking at this as a way to realize the President's Hydrogen Fuel Initiative with regards to vehicles, utilizing hydrogen ICE vehicles does seem to be a bit off course. However, attention to stationary fuel cells is very worthy, as these will likely be commercially available long before vehicles. I believe, that despite the attention to hydrogen ICE vehicles, the overall outcome of the project will align very closely to the objectives of the President's Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Looks like a standard education program. How will they address problems specific to hydrogen? Could be a great opportunity to work on areas of interest to the hydrogen community.
- The "Developing A Curriculum" or DACUM curriculum development model appears to provide a focused methodology to identify the appropriate curriculum needs and content.
- Curriculum development with faculty and students.
- Aware of the need for relating hydrogen curriculum to CURRENT jobs available in the area.
- Cross-cutting curriculum helps tie hydrogen to other areas such as HVAC and automotive.
- Overall approach is sound. Possibly spread too thinly over every area and track of studies within school.
- The approach seems to have been well thought out. Training of faculty is certainly the most logical first step, running concurrently with the development of the curriculum in collaboration with industry. Transfer of curriculum to other universities as it is completed is a good way to get accomplishments and successes spread as soon as possible.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.1** based on accomplishments.

- Good progress since last year.
- It appears that components of the curriculum development are being completed. Are there descriptions of the classes available? Fuel cell classes were mentioned several times.
- Curriculum for a 2-year degree has been developed.
- Continuing to work with community outreach and further develop curriculum for the 2-year degree.
- Focus on hydrogen ICE rather than just fuel cells.
- On-track progress is evident.
- Project seems to be moving at a good pace. Considering how much coordination has to be done with industry and other universities, Lansing seems to have implemented several pieces of its curriculum already and appears ready to launch another in the fall.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- Good sharing of data with other universities.
- The sharing of curriculum is a strong component of this project. More information on how this will be implemented would provide more value.
- Lansing Community College is working with General Motors and the American Association of Community Colleges to make this curriculum available to a larger audience – potentially to community colleges throughout the United States.
- Partnered with 4-year schools and industry – excellent leverage of resources.
- Collaborating with the University of Michigan sounds good and may raise awareness of "Learn Hydrogen Day" on the capitol lawn. Tech transfer partnership with colleges and universities seems to be working well. Bringing industry into curriculum development also sounds like a good idea.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- More ties with hydrogen technologies.
- It appears that significant work has been accomplished in the DACUM process. Future work in taking the DACUM process to the next step, actual curriculum development, does not appear to be identified in the future plans.
- Lansing Community College plans to continue to develop its curriculum and refine it based on student input, faculty input, and industry input.
- Plans for building include optimization of all available renewables.
- Not sure how the hydrogen storage project replacing the hydrogen fueling station project will work. The plans to address hydrogen safety seem to be in the works. However, it is unclear how they will be able to address these issues within the next year in order to accomplish what they want. Having a network of hydrogen storage demonstrations seems to be of much less value than a network of fueling stations for learning purposes.

Strengths and weaknesses

Strengths

- Good partnerships with state.
- Strong university collaboration within the state.
- Well-funded project; DOE's $981K cost shared with LCC's $1M.
- Curriculum development includes contributions from experts from industry, government, and academia.
- Dissemination to a wide audience, with potential to make a hydrogen technician curriculum available across the country.

- Curriculum is crosscutting with courses in HVAC and automotive technology.
- This is a very important area to be focused on. Training the future technicians who will repair fuel cell and hydrogen technology is most definitely a valid goal. The collaborations with industry in developing the curriculum, and the collaborations with other universities and colleges in the transfer of the curriculum, seem to be very solidly in place. The project approach seems to have been well thought out.

Weaknesses

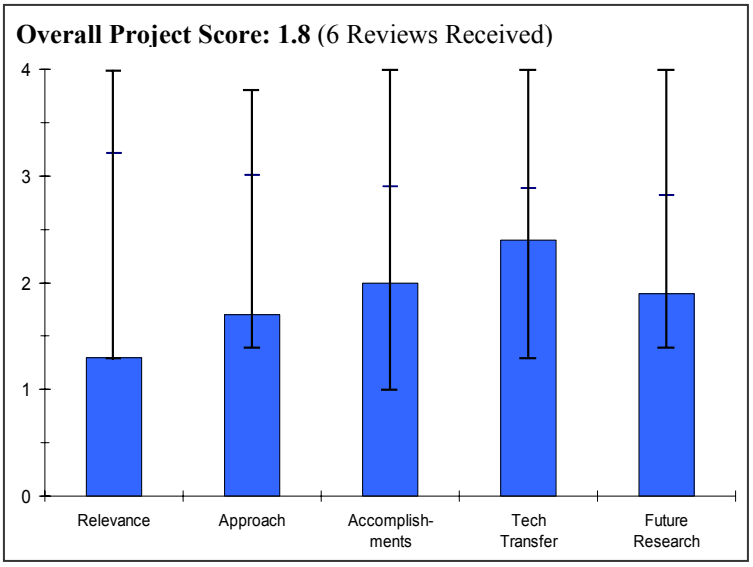
- Could use stronger linkage to hydrogen technologies. Appears to be focused on education in general; hydrogen not emphasized? Any participation by local auto industry?
- Implementation of the curriculum developed, and the collaboration within the community college and university system, should begin to be demonstrated.
- Not clear that an emphasis on hydrogen internal combustion engines will be as useful as fuel cells.
- At this point, it is not clear that there is a big demand for students who complete this degree program.
- As the field is still growing and standards are still being established and worked out, it isn't clear that the program is strong enough that graduates will be able to immediately use their knowledge and skills in working with the auto companies or fuel cell manufacturers.
- Possible dilution of HYDROGEN dollars as they are spread across all energy/science/environment/math areas.
- There were concerns expressed in the Q&A about the hydrogen ICE vehicle. I was not as concerned about the focus on hydrogen ICE vehicles and demonstrating to students hydrogen safety by using the hydrogen in an ICE. A larger concern is the focus on hydrogen in teaching students about the HVAC Hydrogen & Electrical Energy curriculum coming up in August 2005. The program needs to include other fuels that will be used in stationary fuel cell applications. Stationary fuel cells will most likely be dealing with issues surrounding natural gas and propane reforming. I believe there will be very little "stored" hydrogen.

Specific recommendations and additions or deletions to the work scope

- Training involves hydrogen ICE vehicles – any thought/direction to FCVs etc.?
- Recommend pursuing collaboration with the state's transportation industry.
- For the duration of the project, until 2006, the overall project scope is fine. If they are going to extend the program, recommend not pursuing hydrogen storage ideas if they cannot have hydrogen fueling stations. Training students in technology that will actually be in place when they graduate (i.e. small stationary fuel cells, backup/cheap fuel cells) would be more important for students who will be seeking jobs when they graduate.

Project # ED-03: Shared Technology Transfer Project
Griffin, John; Nicholls State University

Brief Summary of Project



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 **1.3** for its relevance to DOE objectives.

- Hard to follow relevance to hydrogen program. No strong linkage. Slides do not follow DOE template.
- There appears to be no relevance to the DOE objectives.
- Presentation related to research on rudder design and hull design is great for the Navy but not unique or valuable for the DOE.
- This project is not applicable to overcoming the barriers or achieving the objectives of DOE's Hydrogen Program.
- Some patents fit with HFCIT and some are beyond the scope.
- This program is not applicable to the education sub-program. It is unclear in its application to the MYRD&D Plan.

Question 2: Approach to performing the research and development

This project was rated **1.7** on its approach.

- Hard to follow – why is approach so unique?
- An interesting approach to evaluate a technology portfolio. Perhaps as the hydrogen technology portfolio is developed, this analysis process could be reviewed to determine applicability to use in the hydrogen program.
- Approach is very general – and would be very interesting for naval architects and navy designers. Not clear that this is useful information for the DOE Hydrogen Program.
- 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.
- Good plans for tracking inquiries and web hits so that relevancy and lessons learned can be captured.
- Catalog categories should tie directly to hydrogen, fuel cell, infrastructure categories and sub categories.
- Not R&D. Couldn't the Navy provide a database of Navy technologies?

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.0** based on accomplishments.

- Some progress made – but no relevance to hydrogen.
- The evaluation and portfolio analysis have advanced in the past year. What are the metrics? How many technologies have been reviewed and are they in the portfolio? How many technologies have actually been reviewed for adoption?
- Group has established a website and a ranking of technical level for recently unclassified NAVSEA material. Not clear if the materials will be relevant or useful to the hydrogen economy. Or, how much of the catalog of materials on the website will be helpful to DOE.
- 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. The TRL structure is good.
- Presenter stated that there were no barriers.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.4** for technology transfer and collaboration.

- Transfer with HARC/NAVSEA.
- The marketing activities were reported, but no information was provided on the metrics of the transfer of technologies.
- This project does have a high degree of coordination with NAVSEA and the ability to share information with the Navy, industry groups, universities, and other government projects.
- Good effort is being made to ensure that the database will be visible and to alert the technical community of its availability.
- Description of technology database – not certain why an RD&D program or education program are related to hydrogen. Not a hydrogen specific project.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.9** for proposed future work.

- Not good linkage with the DOE Hydrogen Program. Too close to DOD program. Does not promote hydrogen.
- Not clear what the benefit was for the HARC facility industry member tour.
- It is clear that this proposal has the potential to reach a large number of people. It is not clear that the data that is being used to establish this database and website is unique to the President's effort to establish a hydrogen economy or unique to hydrogen education.
- 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.
- Plans to continue with same?

Strengths and weaknesses

Strengths
- Interesting portfolio analysis process.
- Project has the potential to reach a wide audience.
- Project has a wide potential for technology transfer.
- As far as database development and understanding of the naval patents in their intended application, the team seems well qualified.

Weaknesses

- Couldn't see linkages with HARC, NAVSEA. Slide showing linkages would have been useful. Not a lot of new information or novel concepts.
- This project is not relevant to the DOE Hydrogen Program.
- Not clear that project is in the best interest of the DOE.
- Not clear that the data obtained from NAVSEA is applicable to hydrogen economy or hydrogen education.
- Little to no ties in advancing the goals of the DOE Hydrogen Program.
- Recommend tighter targeting.

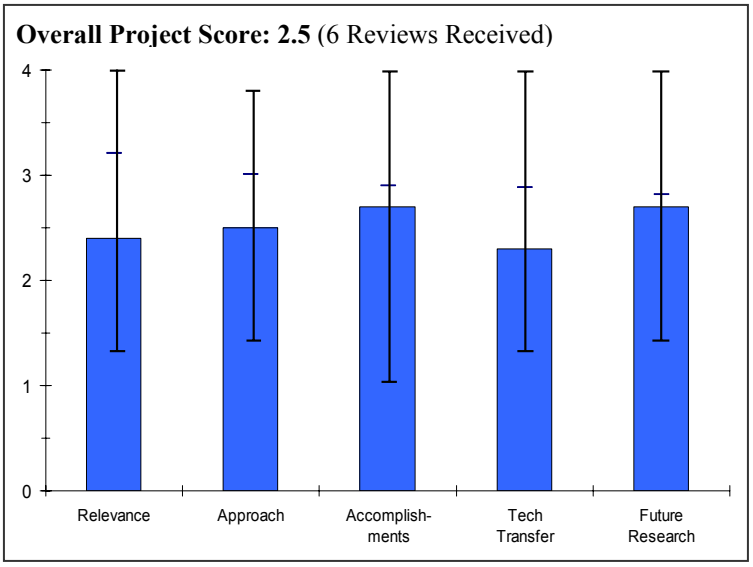
Specific recommendations and additions or deletions to the work scope

- Get closer to hydrogen education goals.
- Upon completion of the project, review portfolio analysis and classification process to determine if the evaluation process might be applicable to the hydrogen technology portfolio.

Project # ED-04: Montana Hydrogen Futures Project
Williamson, Paul; University of Montana

Brief Summary of Project

The Montana Hydrogen Futures Project includes a new web-based curriculum for hydrogen and alternative energy technician, a hydrogen education website for students and teachers of all ages, a hydrogen safety training center to provide safety training and certification, and Hydrogen Futures Park to provide a comprehensive, integrated learning environment in which to demonstrate, educate, and exhibit sustainable energy in Montana.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.4** for its relevance to DOE objectives.

- Good identification of barriers needs to continue in identifying better connections with hydrogen than education.
- Project description does have objectives related to goals in the MYRD&D Plan.
- Project is very narrowly focused, i.e. the state of Montana, and may have limited impact on the overall goals and objectives of the program.
- Clear understanding that there are a large number of U.S. citizens who need to learn about and become aware of issues related to hydrogen, fuel cells, and infrastructure to support the hydrogen economy initiative.
- Training the next generation workforce is important to the success of hydrogen technologies.
- Crosscuts multiple technologies.
- This seems to be primarily focused on Montana, with little relevance to DOE's national scope. The training programs are a good idea, as is the hydrogen safety center, but only if there are plans to provide nationwide access to developed curriculum.
- There's mention of generating hydrogen, but doesn't seem to be much thought into what the hydrogen would be used for.

Question 2: Approach to performing the research and development

This project was rated **2.5** on its approach.

- Good use of leveraging universities, 2-year colleges, and high schools.
- Technical barriers identified are very high-level barriers. It is not clear how the identified objectives will be accomplished.
- Objective 1: Develop a college curriculum for energy technicians. It is unclear what technologies the energy technicians will be trained to support. How will the curriculum content be identified?
- Objective 2: Establish a hydrogen safety training center at the University of Montana College of Technology. It is unclear as to who will be trained and what aspects of safety they will be trained on? Would coordination with the PNNL HAMMER Facility be more effective?
- Work with K-12 students and teachers, industry, and undergraduate students and faculty.
- Web-based curriculum to share information with a wide audience.
- Link with resources at federal, state, and local government agencies.
- Big ideas (such as making Montana a state that is based on hydrogen economy) linked with practical, simpler ideas (such as the informational web site) that can be implemented quickly and easily.

- Seems to be well planned, but this is a very complicated undertaking. Are the resources being stretched too thin?
- Need to benchmark against what has already been done with similar curriculum development (examples: College of the Desert, Humboldt State, and University of Victoria).
- Energy Technical Program sounds strong. Web-based curriculum access is good for allowing other universities to use curriculum.
- Hydrogen education web site sounds like a good idea. But not sure how it will differ from all the other hydrogen education web sites out there.
- Lots of attention to marketing and PR. That is important once the projects are completed and the results are worth promoting.
- The web based approach to curriculum is good.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.7** based on accomplishments.

- Good progress and good leveraging of funding for future activities.
- The accomplishments are limited as this is a new project.
- Accomplishments are difficult to review but it was reported that the project is 30% completed. It would seem there should be more definitive accomplishments identified with 30% of the project completed than were presented.
- Information being made available now.
- Establishing curriculum for a hydrogen /energy technician program.
- Business plans established and state and university approval.
- There is some concern that as the program grows, it clearly ties into available standards for safety in working with fuel cells and hydrogen as a fuel.
- Good progress has been made in the upfront planning and stakeholder development.
- Sounds like the web site portion of the project is progressing nicely.
- Doesn't sound like any parts of the course curriculum are completed yet, but perhaps they are waiting until the entire curriculum is complete before releasing portions.
- Hydrogen Safety Center seems to be least developed portion of this project. Why?
- Focus on coordination with DOE hydrogen safety program – a “must do” item.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.3** for technology transfer and collaboration.

- Progress slide - did not mention who partners were.
- The development of the web site appears to be a prominent technology transfer strategy. This would be an effective strategy for the state of Montana.
- Working with a wide range of university, K-12, and industry resources to acquire information share with others.
- It seems to me that the majority of resources are being used in Montana. It would be nice if the scope was broadened to support more of a national program.
- Although some thought has gone in to replicating the curriculum at other sites, stronger partnerships with other universities doing similar demonstrations and training development would improve the likelihood of success.
- Doesn't really sound like they are doing much in the way of collaborating with other universities or industry. At least it was not mentioned. Seems all focused on Montana's hydrogen economy and not dispersal throughout the U.S. University interest he mentions is all "intra" university.
- It is troubling that most of the "industry" participants on the curriculum development advisory board are non-hydrogen, non-fuel cell related. I would think those are the most important people to have on an advisory board for development of hydrogen related curriculum.
- Needs more coordination beyond MT.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.7** for proposed future work.

- Good list of future work in renewables, which the state can showcase.
- Future work identified appears to be focused primarily on renewable energy. Recommend developing stronger ties to hydrogen.
- Construct/coordinate a hydrogen sustainable campus among the University of Montana system.
- Business plans for future sustainability of hydrogen education
- Advisory board input.
- Leverages current efforts to expand capabilities.
- It sounds like a lot of proposed future work, once current projects are completed, is non-hydrogen, non-fuel cell related. There was some mention of wind-powered hydrogen generation. Lots of plans. Very ambitious project scope. Perhaps too broad? Funding from DOE may need to be more focused.
- Are hydrogen/fuel cells getting lost among all the other plans they have?
- Lots of vision, but needs more work fuel cell side.

Strengths and weaknesses

Strengths
- Good use of visuals and supplemental information on the board. Seems to have great potential for tech transfer to other applications. Good financial leverage. Good leverage with EPA on refueling station.
- PI is enthusiastic.
- Interest in a broad spectrum of hydrogen education issues and collaboration with many institutions in Montana.
- We need to start now to educate and train the individuals who will ultimately be responsible for realizing the hydrogen economy.
- Sounds like they are doing a great job of going to schools and to the public and talking about hydrogen and fuel cells. Is this part of their original project scope? They seem to do a very good job of public education, albeit only in their state.
- PR/marketing seems to be a big part of their project plans. This is definitely important. Make sure it is a nationwide promotion.

Weaknesses
- This project scope is very broad but targeted to a much focused audience. (Montana)
- Resources are too largely focused on one state (Montana). Would like to see resources and initiatives planned for reaching more of a national audience, both in terms of developing the materials and training students and educators (other than just making materials available online).
- Quite a bit planned for the limited resources available.
- The hydrogen education they do sounds primarily focused on Montana. Needs to be a more nationwide outreach. Not sure why the web site isn't being created specifically on the DOE's web site. There are so many hydrogen education web sites now. Why do we need another one?
- Lots of talk about hydrogen generation, little talk about demonstrating use of hydrogen. Perhaps that's not part of their scope. What will be done with the hydrogen?
- Little hydrogen/fuel cell industry involvement. Vague plans for disseminating resulting curriculum outside of the state.
- Hydrogen Safety Center does not seem to be progressing very quickly. Does not seem to be a "vision" for what they really want to do with this part of the project. Vague talk of working with NREL/Ohio on hydrogen safety issues. Is this misplaced in the Education Program?
- Need better safety coordination -visit the PNNL HAMMER facility. Need stronger hydrogen/ fuel cell partnerships.

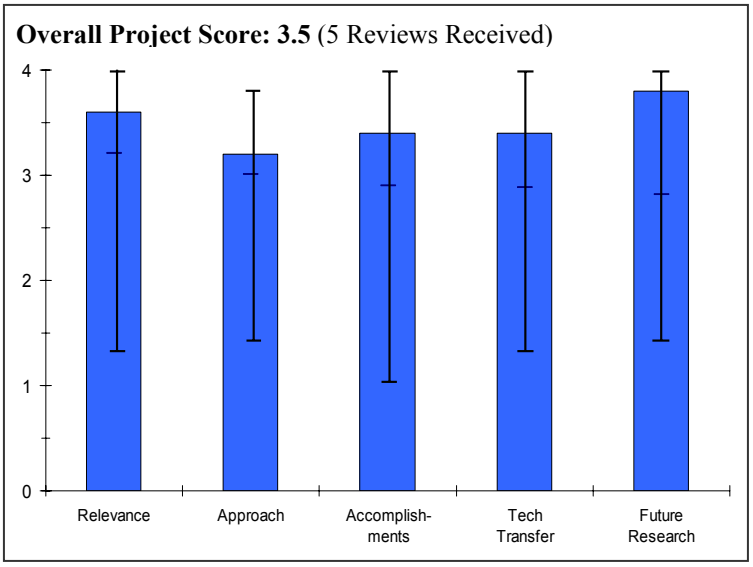
Specific recommendations and additions or deletions to the work scope

- This project could be strengthened with strong collaboration with the current activities ongoing in the HFCIT Education and Safety programs, i.e., other curriculum development projects and other safety training projects.
- More hydrogen/fuel cell industry involvement in curriculum development.
- Broader geographic focus.
- Perhaps they are trying to do too many things with their DOE funding and are unable to really focus on the hydrogen safety center, which seems to be the least advanced portion of their program.
- Rather than funding specific hydrogen education projects for K-12, perhaps Montana should focus on the more university level projects.

Project # ED-05: Hydrogen Technology and Energy Curriculum (HyTEC)
Nagle, Barbara; University of California, Berkeley

Brief Summary of Project

The Hydrogen Technology and Energy Curriculum (HyTEC) project, led by the Lawrence Hall of Science at the University of California at Berkeley, will develop, test, and disseminate hydrogen and fuel cell curricula for high school students. The project will create new materials, including student handouts, teachers' guides, kits for students' investigations and experiments, and professional development for teachers. The project will also incorporate hydrogen and fuel cell information into existing Lawrence Hall materials, which are used in more than one million classrooms nationwide. Project partners include Schatz Energy Research Center, AC Transit, and Lab Aids, Inc.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Emphasis on providing educational materials for teachers and students in grades 9-12.
- Emphasis is on curriculum and kit development rather than long-term sustainable professional development and evaluation of outcomes.
- Program as described should result in many new teachers exposing students to hydrogen basics.
- Directly addresses the goals and objectives outlined in the Multi-Year RD&D plan.
- Geared towards a wide impact approach to education.
- Excellent project that will reach target audience.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Bring at least 8 fuel cells to each classroom for student use.
- Have a larger "demo" fuel cell that can be disassembled for classes to look at and re-assemble.
- Kits can end up being relatively expensive ($200 to $500 per classroom), which makes the kits only available to school districts that are well funded.
- Making excellent use of previously developed expertise and relationships.
- Clearly a strong team knowledgeable in the area of education.
- Good benchmarking approach with strong stakeholder involvement.
- Hands-on training aids are a good approach to stimulating interest in the students.
- Ability of schools or others to finance the purchase of kits will be key to determining the impact of this effort.
- National approach is a strength.
- Outstanding sequential approach that addresses all target audiences. Impressive tools and training materials to prepare teachers and educate and motivate students.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.4** based on accomplishments.

- Completed outline of lessons.
- Prototype kits developed.
- Try outs after AP exams.
- Related to National Science Education Standards and California state science standards.
- Prototype of fuel cell with electrolyzer and multimeter to allow students to lift weights using the fuel cell.
- Excellent leveraging of resources.
- The team is commended for making strong progress given the funding constraints.
- Wish this were further along.
- Leveraging by also including hydrogen in existing science modules is a bonus.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.4** for technology transfer and collaboration.

- Lawrence Hall of Science has an established, large network for dissemination of curriculum materials to high-school teachers throughout the United States.
- Project is working to use existing kit material rather than go through an expensive development program for getting new materials to high school students and teachers.
- Significant involvement with range of appropriate groups. Should strengthen final product.
- Well-connected with teacher/education networks.
- Good approach.
- Could use more emphasis on working with schools to obtain financing for kits.
- Excellent partnerships.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.8** for proposed future work.

- Classroom testing of the curriculum.
- Online field trip to a hydrogen fueling station.
- Would like to extend to 3 modules and national field testing if funding is available.
- Working to collaborate with local science centers.
- Use with teacher workshops.
- On track to reach stated objectives.
- Strong plan for validating usefulness of kits and expanding into multiple science areas.
- Nice long-term sustainable financial basis.
- Planning revamped due to lack of DOE funding.

Strengths and weaknesses

Strengths

- Excellent collaboration with teachers and scientists to develop a grade 9-12 curriculum on hydrogen and fuel cells that is cross-curriculum (with physical science, chemistry, physics, and environmental science) and aligned with the National Science Education Standards and state science standards.
- Excellent presentation.
- Clearly a strong team with a clear understanding of educational needs and opportunities for inserting new curriculum into the school.
- Nice fit to broader high school science module series.

Weaknesses

- Too much focus on curriculum and not enough on teacher professional development that is long term and working to establish lasting ties between teachers and the research community. Not clear that DOE should be funding curriculum projects that do not use the unique resources of the DOE labs and DOE community of researchers and experts.
- Although getting hydrogen-related activities into schools is important and necessary, how much impact will a 2 to 3 day module make on accelerating the acceptance of hydrogen technologies? Modules that contain things that can be put into practice now, or that kids can associate with everyday, will be important.

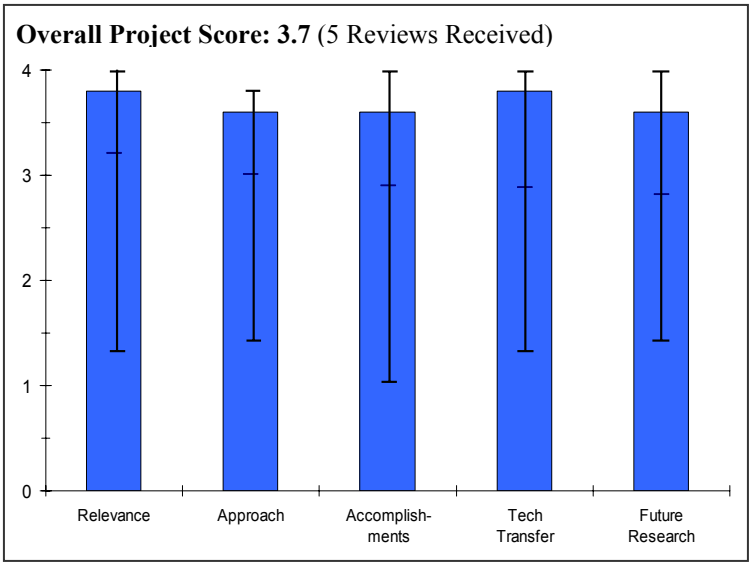
Specific recommendations and additions or deletions to the work scope

- None.

Project # ED-06: H2 Educate!
Spruill, Mary; National Energy Education Development Project

Brief Summary of Project

H2 Educate!, led by the National Energy Education Development (NEED) Project, will provide hands-on kits, classroom curriculum, and teacher training on hydrogen and fuel cells. The project, which targets middle schools, will reach a network of 45,000 classrooms with one-day workshops, week-long conferences, and strategic partnerships around the country. The program and materials were designed by teachers, for teachers, with technical support from SENTECH, Inc., the National Hydrogen Association (NHA), Los Alamos National Laboratory (LANL), and the U.S. Fuel Cell Council (USFCC).



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Developing curriculum units teachers and students in grades 6-8, with input from industry and LANL.
- Materials are provided free of charge in most cases to the middle school classroom.
- Excellent implementation plan for expanding exposure of large groups of teachers and students to hydrogen basics.
- Directly addresses the goals and objectives outlined in the Multi-Year RD&D Plan.
- Geared towards a wide impact approach to education.
- Meeting critical to educate teachers and students and elementary years.

Question 2: Approach to performing the research and development

This project was rated **3.6** on its approach.

- Resource materials for reading and learning about hydrogen and energy.
- Emphasis for teachers to use energy education as a theme or strand throughout the school year.
- Allows middle school teachers to work together to implement learning about hydrogen and energy across the curriculum.
- Addresses low levels of knowledge in population on hydrogen issues. Builds confidence for teachers new to technology.
- Demonstrates an exceptional understanding of identifying the need, establishing the stakeholders, and implementing the tools.
- Good tie to middle school teachers.
- Materials update annually - excellent. Ability to raise funds to cover cost of materials will help to reach larger audience - outstanding. Comprehensive planning.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.6** based on accomplishments.

- Curriculum pilot programs are completed.
- Training programs are completed.
- Starting professional development for teachers in New York.
- Making notable, measurable progress in spite of lack of budget.
- Project is ahead of schedule; the team should be commended for stepping up and moving the project forward given the federal financial constraints.
- Impressive, given the funding- bravo!
- 50% ahead of schedule in preparing module.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.8** for technology transfer and collaboration.

- Good focus on providing technically accurate hydrogen information to teachers.
- Excellent utilization of links to industry and other educational institutions.
- Strong collaborations. Great fundraising.
- Excellent partnerships and inclusion of students and teachers in curriculum development and actual hands-on teaching.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.6** for proposed future work.

- Pilot workshops with teachers leading to larger scale of professional development for middle school teachers.
- Work to assess how well students and teachers are learning about hydrogen and energy.
- Use of 8th grade students to teach 6th grade students about hydrogen and energy.
- Materials for teachers are updated annually -- keep material current and accurate.
- Program design allows expansion to meet other identified needs.
- Good strong plans for measuring progress and impact.
- Clear understanding of future challenges.
- Like the update approach.
- Continuing regardless of funding issues.

Strengths and weaknesses

Strengths

- Program is national in scope and NEED has an excellent way of providing middle school science curriculum related to energy and the hydrogen economy to a wide audience of middle school teachers and students nationally.
- Program has good input from teachers in the development of what they need for education resources and has worked to reduce the cost of curriculum to schools and school systems so that material will be widely available and not just to school systems that can afford it.
- Builds on expertise of organization. Able to deal with budget shortfall more easily than other organizations.
- Takes fundraising in stride. Excellent presentation skills provided clear information on project.
- Exceptional team with a long history of developing educational tools.
- Strong partnership development.
- Strong fit to broaden middle school effort.

Weaknesses

- Not clear that DOE should be in the business of funding science curriculum development, which can compete with funding from Education and National Science Foundation. DOE may be better off funding professional development and research experiences for students and teachers that use the unique resources of the DOE labs and community of researchers.
- None noted.

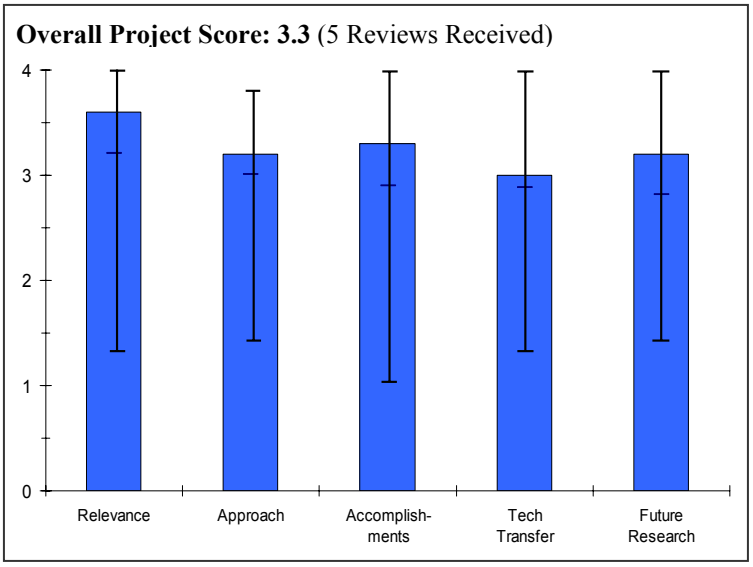
Specific recommendations and additions or deletions to the work scope

- None.

Project # EDP-01: Baseline Knowledge Assessment
Armstrong, Tim; Oak Ridge National Laboratory

Brief Summary of Project

The Baseline Knowledge Assessment, led by Oak Ridge National Laboratory (ORNL), with support from Opinion Research Corporation, measures the current level of knowledge and understanding of hydrogen and fuel cell technologies and the hydrogen economy in four target populations – the general public, students, state and local government agencies, and potential large-scale end users. It establishes a baseline for comparison of future evaluations of knowledge and awareness among these same four key populations. The surveys were completed in 2004 and an updated survey report will be available in 2005.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Great project – very relevant to assessing public knowledge.
- Project performed surveys of general public, students, government officials, and potential end users regarding their knowledge and fears about hydrogen and fuel cells.
- Data provides a baseline for DOE's hydrogen education program
- A major goal is to improve the knowledge and acceptance of hydrogen and fuel cell technology in order to facilitate the commercialization decision; establishing a baseline is a key first step.
- If you are going to measure the effectiveness of any educational program, having a survey of baseline knowledge to measure from is extremely important.
- Glad this work got done!

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Good approach. Well thought out – good data generated. Good analysis.
- Project seems to be statistically robust (n=1000).
- Actual survey questions were not available to reviewers.
- Need to ensure that population sampling – number of surveys, types of questions, and interpretation of answers – will lead to a statistically significant result.
- Should benchmark knowledge and acceptance of hydrogen against knowledge and acceptance of related and/or unrelated topics.
- Assuming acceptance improves, how will you gauge what tools have been the most effective?
- Very well thought out approach. Very methodical. Very relevant to other parts of DOE program.
- Mature and solid work.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Survey provides very good information – where to focus activities and additional research (i.e., students and general public). When the public goes to the government for information, the government needs to have the right information.
- Project has completed surveys in 2004.
- Project has plans to conduct surveys again in 2007.
- Report is about to be published and available on website. Great progress. Efficient work by ORNL in getting this accomplished.
- Needs to be on website.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Data needs to be disseminated to more audiences – very good insights on hydrogen myths.
- Good coordination with getting industry input on questions. Transfer of information to other DOE hydrogen education program projects will be important.
- Gives good guidance to education effort.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.2** for proposed future work.

- Continue work in out-years to assess knowledge of information.
- Surveys will be redone in 2007 and compared to baseline data from the 2004 survey.
- Activities seem to focus solely on repeating surveys. Why not dig deeper into the results of the first set of surveys to try and understand more?
- Plans in place for future surveys to measure effectiveness of education programs.
- Long term planning from the beginning.

Strengths and weaknesses

Strengths
- Excellent work – tells us at many levels what is going on. Great baseline work.
- Very useful data for EERE administration and policy decisions regarding hydrogen education programs.
- Important part of hydrogen education program. Timely completion. Will be effective tool to measure program efficiency.
- Best of the education posters.

Weaknesses
- Scope is limited. It would be nice to get information about more than just general knowledge but also to test knowledge about more specific areas such as how hydrogen will be produced, stored, and transported.
- Can't think of any weaknesses except to ask what contingency plans are there if no funding is available for next rounds of survey?

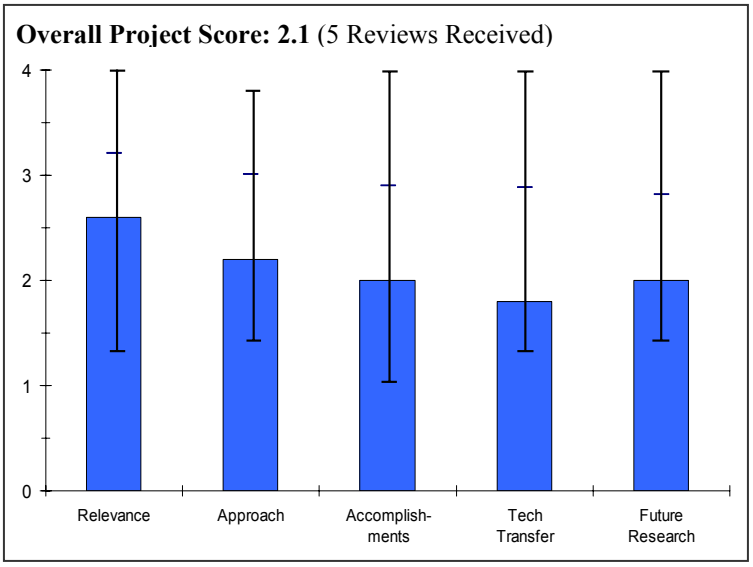
Specific recommendations and additions or deletions to the work scope

- This data really needs to get published to a wider audience – magazines, educational materials, fact sheets, etc.

Project # EDP-02: Hydrogen Technology Overview Publication and Program Information Kit
Andersen, Cindi; Andersen Creative

Brief Summary of Project

This project will develop hydrogen educational materials for multiple target audiences. Initial efforts focus on producing an information kit that contains fact sheets, a poster, a PowerPoint presentation, a CD containing current related documents published by DOE, and an interactive CD. Information will include an explanation of the President's Hydrogen Initiative and DOE Hydrogen Program; how hydrogen is produced, stored, distributed, and used; and the technical challenges to achieving a hydrogen economy. Pending future funding, this project will also include the development of a hydrogen technology overview brochure.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- Project is collecting data, quotes, and video relating to the hydrogen economy and fuel cells.
- Project is also producing animations.
- Collection of media will be made available to multiple audiences.
- This data is already available online and through trade organizations.
- Difficult to review without drafts.
- Very needed.
- Audience changed from secondary schools to general public. Project scope doesn't include dissemination. How can a product be developed without knowing how it will be disseminated?

Question 2: Approach to performing the research and development

This project was rated **2.2** on its approach.

- Project is producing fact sheets, CDs, and other media for distributing information to multiple audiences.
- Descriptions of materials, target audiences, and barriers are general and vague.
- Need to develop metrics to measure effectiveness of tools.
- General low-level information may not have sufficient impact.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.0** based on accomplishments.

- About 15% of work has been accomplished. Project seems to be basically on-hold pending additional funding.
- Difficult to evaluate progress without drafts of product. No evident plans for distribution.
- Changing the target audience seems to have slowed progress, but does changing the target audience really require a totally different approach?
- Not fair to rate accomplishments – funding problem hit squarely.
- Unclear since target audience changed.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **1.8** for technology transfer and collaboration.

- Project is collaborating with Argonne National Lab.
- Some of the delays have occurred due to a change in target audience focus.
- Appears to be work of one person/company with only DOE review.
- Need to develop a stronger collaboration with others doing similar work and targeting these same audiences.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.0** for proposed future work.

- Plan to complete an overview booklet.
- Development of a color brochure is also planned.
- Need to clearly identify the added value over other tools currently available.
- Are these the correct tools for the target audience?

Strengths and weaknesses

Strengths
- This is clearly a qualified firm to develop materials that can engage the audiences.

Weaknesses
- Limited scope; repeat of data and information that is already available in other forms and from other sources.
- Lack of defined distribution plan for finished products.
- Need to ensure that the materials being developed are the right tools to educate the target audiences. Short, visible material that catches one's attention, but does not require substantial investment of time is more likely to have an impact than long brochures or CDs of programmatic documents.
- Need to ensure good coordination with other efforts.
- This product is not the best format to reach the general public. A power point presentation is included as well as a CD.

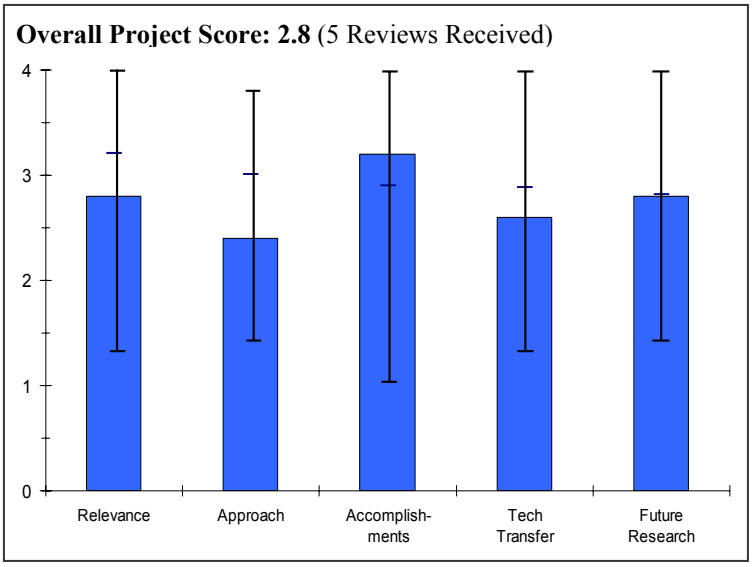
Specific recommendations and additions or deletions to the work scope

- Distribution or outreach plan needed.
- Hope this gets funding.

Project # EDP-03: Understanding the Hydrogen Economy
Tidball, Rick; Energy & Environmental Analysis

Brief Summary of Project

This project will produce hydrogen education materials that will help multiple target audiences understand the concept of a hydrogen economy and how it may affect them. Initial efforts include the development of a hydrogen technology overview brochure; pending the availability of funding, future efforts will include creating a hydrogen information kit. Materials will be developed in a graphics-rich format and will describe the status, challenges, technologies, and anticipated timeline to achieve a hydrogen economy.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- This project is the production of a ~40 page booklet on the hydrogen economy and hydrogen as a fuel.
- Booklet audience is public, policy makers, and industry leaders.
- Covers the basics. Individual sections of the booklet are being reviewed by appropriate DOE officials to verify alignment.
- We need this material.
- Very small target audience; people who are already interested in hydrogen. Only individuals who are very interested in hydrogen will read a 40-page document.

Question 2: Approach to performing the research and development

This project was rated **2.4** on its approach.

- Approach is to produce a booklet.
- Booklet may be out-of-date before it gets off the press.
- Booklet will be available as a PDF but not regularly updated.
- Approach is appropriate for identified audience.
- Need to develop metrics to measure the effectiveness of the tools developed.
- Seem to be relying heavily on DOE for content review and distribution of tools.
- Working with target audiences to better define need and for beta testing tools would result in a more useful product.
- Lack of access to audience not addressed well – targeting 20,000 subscribers to H2Nation magazine and web.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.2** based on accomplishments.

- Working on third draft currently.
- Graphics selection and publication are awaiting final DOE review and potential for additional funding.
- Progress is significant and commendable given the budget shortfall.
- Have made good progress towards accomplishing objective.

- Third draft round is nice progress on booklet.
- Two drafts provided to DOE in timely manner.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.6** for technology transfer and collaboration.

- Great opportunity for reaching a large audience.
- Some information already available from other sources and trade groups.
- Would be nice if information could be regularly updated as this field is still developing and changing often.
- Close coordination with DOE.
- Although H2Nation may not be the best mechanism for reaching the target audience, this is the correct approach for reaching broader audiences.
- Need to also target periodicals frequented by the less hydrogen familiar audience.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.8** for proposed future work.

- Work is still ongoing and will go to print.
- Still awaiting DOE reviews and funding for FY06.
- Reasonable goals stated, dependent on future funding.
- Need to ensure that efforts are not repetitive of similar activities.
- Project would benefit from an outreach/marketing plan; initial draft should be beta tested with target audiences and refined as needed.
- Fact sheets to be developed if FY06 funds available. Other education projects are also developing fact sheets.

Strengths and weaknesses

Strengths
- This project should provide a useful tool for those that already have a basic understanding of hydrogen and are interested in further information.

Weaknesses
- Perhaps need to strengthen ties to universities.
- Need to ensure that the content and scope of the tool is appropriate for the target audience. Booklet may be too long and detailed to sufficiently hold the audience's attention. Metrics should be established for measuring its effectiveness.
- Will need to coordinate with others doing similar work.
- Focused on a limited target audience. If this audience were already interested in hydrogen, a 40-page document compiled from other DOE hydrogen documents, which this audience would be aware of, would not be highly valued.

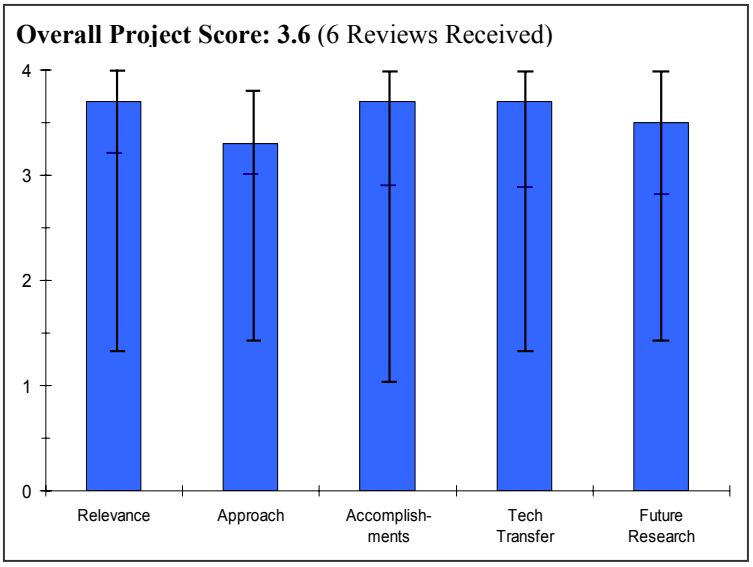
Specific recommendations and additions or deletions to the work scope

- Products need to be coordinated with those being developed in other education projects. Perhaps use this group to fill in other needed fact sheets rather than duplicate the same fact sheet topics being covered by others.

Project # EDP-04: Hydrogen Education and Outreach
Serfass, Patrick; National Hydrogen Association

Brief Summary of Project

The National Hydrogen Association (NHA) project will assess the information needs of fleet operators and managers, develop a series of hydrogen fact sheets on a variety of topics, and disseminate the fact sheets and other educational collateral using the media most requested to heighten knowledge regarding hydrogen energy systems. The tasks in this project will focus on key target audiences, such as early hydrogen adopters, and address three of the four main education barriers identified by DOE.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- The project supports the goals and objectives of the HFCIT MYRD&D Plan.
- Project has large-scale objectives – education for fleet operators and fact sheets on various hydrogen topics.
- Fact sheets are well-reviewed and targeted for different audiences.
- Providing needed documents for outreach to variety of audiences.
- Increasing awareness and providing understandable information to end-users and approving officials will be important for the success of hydrogen technologies.
- Well targeted.
- Target audiences align with DOE targets.

Question 2: Approach to performing the research and development

This project was rated **3.3** on its approach.

- Project activities are well defined and well aligned with addressing the barriers in the MYRD&D Plan.
- Project works with large group of users to collaborate on the education information provided and making that material available to a wide audience.
- Fact sheets can be downloaded from the web.
- Statistics are kept to see how often fact sheets are downloaded.
- Addresses needs of fleets, general public, and officials – all needed areas to raise level of understanding.
- Well-geared toward first users and approving officials.
- Could leverage more industry involvement in activities.
- Timeline to start required adjustment. Glad adjustment done.
- The products presented are professional and relevant.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.7** based on accomplishments.

- The fact sheets are a significant accomplishment and value to the program.
- Much has already been accomplished – fact sheets available online and under review for updates and new information.

- Excellent progress given low level of funding.
- Project appears on track to achieving its goals and objectives.
- Good progress. Waiting for next batch of fact sheets to go on web.
- Accomplishments are significant.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- The technology transfer is very effective through the use of the web sites and NHA.
- Educational programs and fact sheets available to a wide audience.
- Working with wide range of organizations in addition to appropriate DOE staff.
- Need to develop an outreach plan to ensure information is disseminated to the widest possible audience.
- Again, leveraging industrial, state, and federal partnerships more could increase the impact.
- Survey and broad review of materials.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.5** for proposed future work.

- The hydrogen fleet target will be an important accomplishment as they will be key to the transition and adoption of the hydrogen economy.
- Future work includes continued updates to fact sheets and workshops for fleet managers.
- On track to meet identified needs. Responsive to changing focus as dictated by DOE and market.
- Will current budget be sufficient for implementing future plans?
- How will effectiveness of outreach efforts be monitored?

Strengths and weaknesses

Strengths
- Strong knowledge in the hydrogen area.
- Excellent presentation. Drafts provided for review.
- Good focus on working with the target audience to baseline understanding and define need.
- Good useful materials. Nice to see the multi-year plan validated.
- Covers wide range of defined target audiences; design products relevant for each audience rather than a shotgun general public approach. Successful project.

Weaknesses
- None noted.

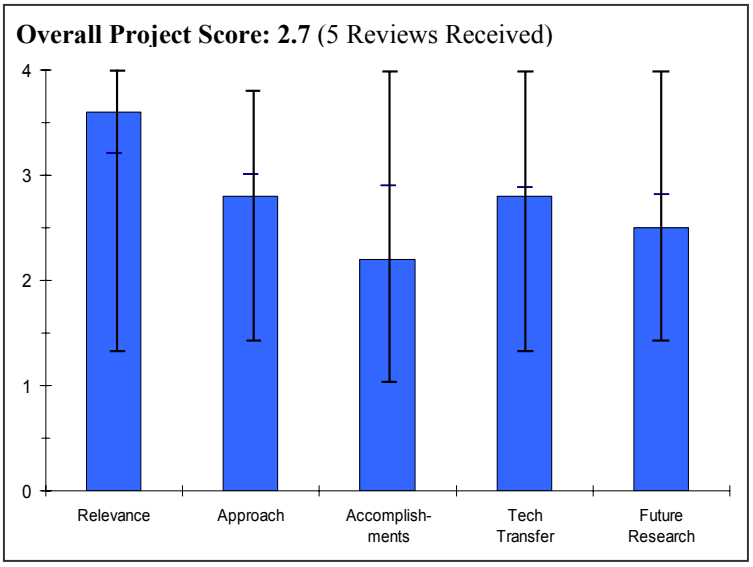
Specific recommendations and additions or deletions to the work scope

- Recommend pursuing DOE publications numbers for the fact sheets.
- Metrics should be established for measuring impact and effectiveness of tools.
- Develop a "waste hydrogen from industrial processes" fact sheet.

Project # EDP-05: Hydrogen Technology Learning Centers
Kripowicz, Bob; State Technologies Advancement Collaborative (STAC)

Brief Summary of Project

Under the State Technologies Advancement Collaborative – supported by the U.S. Department of Energy, National Association of State Energy Officials, and the Association of State Energy Research and Technology Transfer Institutions – three hydrogen technology learning centers are in development through partnerships among universities in FL, CA, and NY; MD and VA; and NC, SC, GA, and FL. Each center seeks to educate university students; potential end-users, such as fleets or building developers; local officials; and the public about the vision of a hydrogen economy, hydrogen technologies and applications, the safe use of hydrogen as an energy carrier, and the challenges to achieving a hydrogen economy. .



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- The project supports the goals and objectives of the HFCIT MYRD&D Plan.
- Project is seeking to work with federal and state agencies in multiple states and includes collaboration with state energy offices.
- Three projects are currently underway in (1) CA, FL, and NY; (2) VA and MD; (3) NC, SC, FL and GA.
- Regional centers very good idea.
- Opportunity to reach all audiences.

Question 2: Approach to performing the research and development

This project was rated **2.8** on its approach.

- Two of the three sub-projects have similar approaches and, although the projects have just begun and are not fully funded, they have sound approaches defined.
- The third sub-project has just begun and is as well defined.
- Project includes cooperation to develop curriculum for undergraduates, graduates, and some younger students
- Expectation of high demand for this educational material, but no data to show that demand has been high.
- Project sees funding as "seed money" to establish curriculum materials that will be eventually funded by other agencies as the curriculum matures.
- Approaches to funding three different projects around the country gives good education coverage. Each individual sub-program within this project seems to have a well-thought out approach.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.2** based on accomplishments.

- It is too early to evaluate this project's accomplishments.
- Work has started, but been delayed until future funding becomes clearer.
- Two of the centers seem to be progressing nicely despite lack of funding. The third seems to have waited until funding was in hand to start but is progressing well with curriculum development.
- A bit slow getting going.
- Too soon to tell. STAC process started late.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **2.8** for technology transfer and collaboration.

- It is too early to evaluate technology transfer, but the centers demonstrate strong collaboration efforts.
- High possibility that the information generated will be shared with many universities and colleges throughout the country.
- It is good that each center's information is online so that each can see and share the others ideas.
- Focus on working with rest of DOE's hydrogen education efforts.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- The continued work proposed for the rest of this year is appropriate.
- Future work seems to be largely "on hold" until future funding potential/availability is understood.
- Not really any contingency for what to do in absence of funding. Not focused on what comes afterward because they are concerned with getting money to actually do work on the project at hand.
- Not clear.

Strengths and weaknesses

Strengths
- Strong university collaboration.
- Important regional focus. Good progress despite lack of funding.
- Potential to be outstanding.

Weaknesses
- Funding levels – is there any cost share for these sub-projects?
- Lack of funding has inhibited progress, but that could not be blamed on PI or centers themselves.
- Loss of FY05 funding will impact. Did not see coping strategy.

Specific recommendations and additions or deletions to the work scope

- Recommend pursuing collaboration with other activities within DOE's hydrogen education program.
- Perhaps a meeting between the groups to discuss common issues would be useful – i.e. getting around funding problems or material development issues.

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 to be an essential component to the Hydrogen Program mission and critical to the President's Hydrogen Fuel Initiative. The projects are considered to be appropriately diverse and strongly focused on addressing technical barriers and meeting targets.

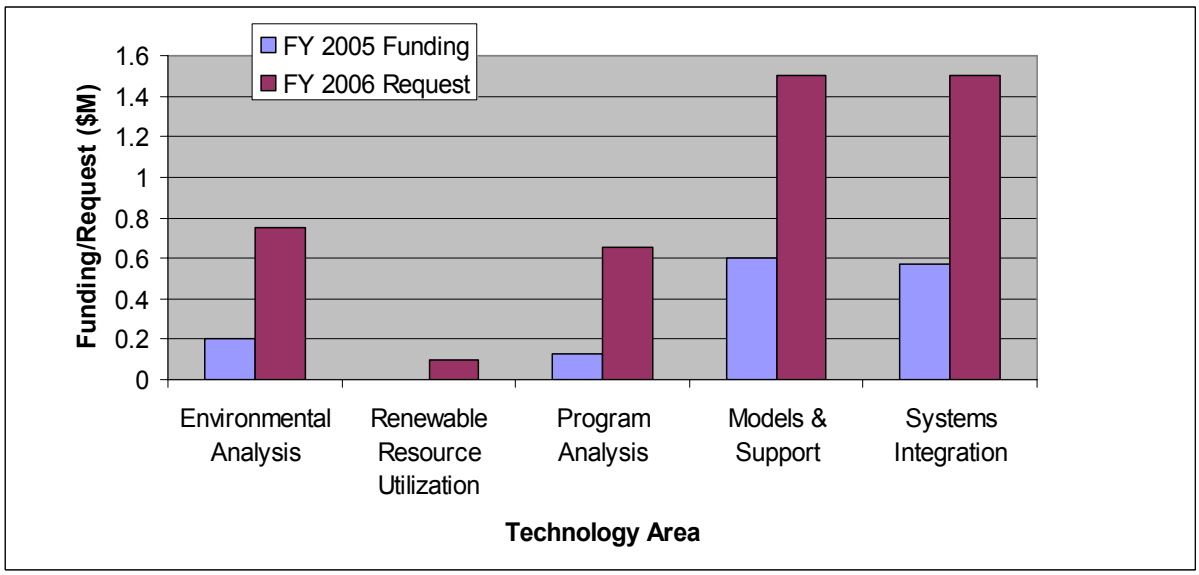
In general, the reviewers noted that Systems Analysis is a complex subprogram but is receiving the appropriate management attention. Some reviewers commented that the Program is covered finally by a "long needed" plan, which is consistent with addressing the comprehensive list of identified barriers.

The major concerns identified by the reviewers for Systems Analysis were: 1) coordination and cooperation with other parts of the Program (and other DOE offices, e.g., Fossil Energy and Nuclear Energy) is required; 2) the plan for how the analytical parts fit together should be defined; and 3) roles of the various models should be described. The Systems Analysis subprogram is in the first year of existence and these issues will be addressed in the Systems Analysis Plan which is in the development phase.

Finally, the reviewers commented on the significant funding planned for FY2006 and the need, if required, for cost reduction without sacrificing results. It should be noted that the 2004 National Academies' Report identified the requirement for the Systems Analysis function to address the complexity of the Hydrogen Program and recommended the funding for this subprogram should be ~$10 million/year. The subprogram is in the startup phase and DOE funding is focused on determining the requirements and the "analytical gaps" of the subprogram and resolving these issues. However, as suggested, DOE will continue to critically assess projects for redundancy and opportunities for cost reduction.

Systems Analysis Funding:

The funding portfolio for Systems Analysis addresses primarily the analysis model support and development to reach the 2015 Commercialization Decision. The requested FY2006 funding profile, subject to Congressional appropriation, addresses the National Academies' Report recommendations and provides greater emphasis on transition.



Majority of Reviewer Comments and Recommendations:

In general, the maximum, minimum and average scores of the reviewers for the Systems Analysis projects were 3.7, 2.7, and 3.2, 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 are summarized below. DOE will act on reviewer recommendations as appropriate for the overall Systems Analysis effort.

- H2A Modeling Tool: Focus on the model functionality and its role in the overall planned model architecture.

- Transition Analysis and Models: Develop an approach to utilize industry collaboration to validate the models and analysis. Automation and optimization in the model structure will be essential to understanding the model benefits. Emphasis on the ability to link the models, particularly with the H2A models and the future Macro System Model, is required.

- Resource Analysis: Focus on combining the resource analysis with other analysis projects. Ensure a common set of assumptions are used for the analysis basis.

- Hydrogen Supply and Infrastructure Analysis Model (WinDS/HyDS): Focus on linkage and integration with other models but minimizes redundancy. Introduce more hydrogen production technologies beyond wind in the model structure. Incorporate industry, academia and other national laboratory personnel in a peer review of the model.

Project # AN-01: Systems Analysis
Joseck, Fred; U.S. Department of Energy

Brief Summary of Sub-Program

The purpose of this Systems Analysis Overview and introduction is to describe analysis goals/objectives, budgets, barriers/targets, approach, technical accomplishments, interactions and collaborations, solicitations and awards, and future directions. As such, it sets the stage and puts into context the Systems Analysis projects which will be presented during the Annual Merit Review.

Degree to which the Sub-Program area was adequately covered and/or summarized

- A comprehensive, well-developed and understandable presentation was delivered by the sub-program manager.
- Issues, accomplishments, plans, future milestones were all discussed and clear.
- For next year: Show all the sub-program's analytical parts, how they do/do not fit together, and the plan for fitting them together better in the future.
- This was a coherent and complete summary.
- It was a thorough and detailed presentation that made clear the issues and concerns and provided methodologies for addressing them.
- The program was well described but would benefit from an overview of the respective roles of various models and any overlaps.

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- The challenges and issues associated with achieving a complete, coordinated analytical effort were identified objectively and accurately. Historically separate analytical activities are being brought together, integrated and used effectively.
- Yes, although more detail would have been useful (but would have required more time).
- Presentation was clear in identifying issues and ways to address, but achieving this is acknowledged to be a challenge given that there is a lot of 'herding of cats' involved.
- The forward plan is consistent with addressing barriers. Barriers identified appear to be comprehensive.

Does the Sub-Program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Discussion of systems analysis planning suggests that this complex sub-program is receiving appropriate management attention.
- The planning, accomplishments and budget indicate that the sub-program is a priority for management.
- A long needed plan has finally been developed, although the details of the plan were not discussed.
- The program appears focused on addressing identified needs. Some concern exists around trying to ensure coordination and cooperation with other parts of the program (and other DOE offices, e.g., Fossil Energy) and seeing the 'big picture'.
- As this sub-program is now covered by a plan, it can be expected that it will now be managed with greater focus and cohesion.

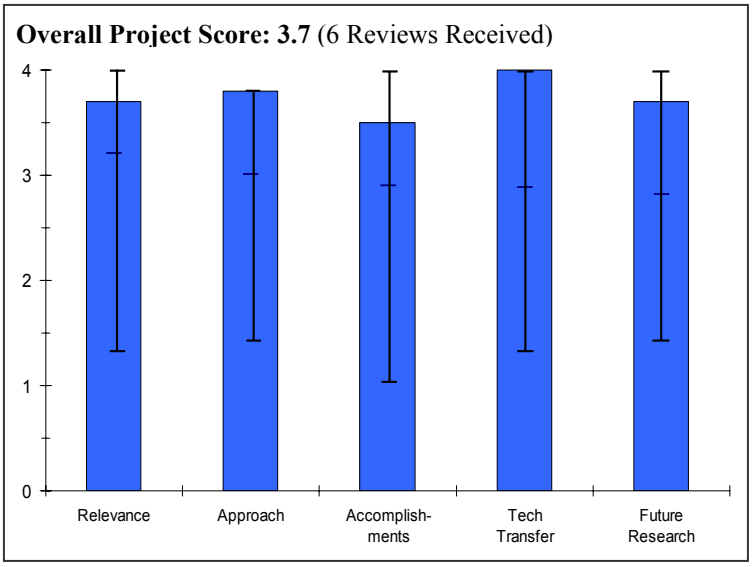
Other comments:

- $7.1 million annual budget (FY06) is a pretty steep cost.
- Need to assess, identify opportunities for reducing costs without sacrificing results.
- Not clear if completely "linked" Macro Model is necessary and funding may not be adequate.
- How will the program collate the various analysis tools and models such that there can be an 'official' view on a particular topic, e.g., transition, costs, policy requirements, etc.? Potential exists for multiple views on a particular topic creating division and confusion -- perhaps there is scope for an analysis project that collects all analysis on a particular topic and creates one official view?
- The overall architecture of the models should be designed so that greater direction is available for individual model development. This architecture should flow from the Macro Model. The Macro Model will need simulation, optimization and decision functions.

Project # AN-02: Moving Toward Consistent Analysis in the HFC&IT Program: H2A
Mann, Maggie; National Renewable Energy Laboratory

Brief Summary of Project

The H2A project team's goal is to bring consistency and transparency to hydrogen analysis. Phase I goals include production and delivery analysis, consistent cost methodology, critical cost analyses, and tool development for providing R&D direction.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- It would be better to write on the objectives of the program: "Tool for helping decision makers on R&D direction".
- This work is important for DOE and other decision-makers throughout the hydrogen community.
- This does not contribute directly to achieving hydrogen economy, but it should make getting there more efficient and cost-effective.
- The project's objectives are very clear and indeed crucial to an orderly and consistent analysis for the H_2 program.
- The goal of the project is to consolidate analytical capability and make data consistent, which would be essential in the realization of the President's Hydrogen Fuel Initiative.
- There was a frequent reference to H2A across the different elements of the hydrogen program, which provides a unifying role for the R&D effort.
- This is an important step towards standardizing hydrogen delivery cost analysis.

Question 2: Approach to performing the research and development

This project was rated **3.8** on its approach.

- There should be a higher focus to quantify uncertainties to demonstrate how much these uncertainties are involved in the results and the limits/tendencies of the results. To establish the curves, it is necessary to have a good effort and interaction among experts in order to contribute even more for the entire study. This is different from sensitivity analysis, especially in an 'if-then' type of analysis.
- The basic approach to production modeling is cash flow analysis tool.
- Accurate, useful results depend on quality of inputs, including those from parties with vested interests and possibly conflicting agendas. (See criterion 4).
- The approach is very thorough and effective, however, it may be a little slower than originally envisaged.
- The model presented is consistent and complete, yet allows for great deal of flexibility.
- The approach has already been very successful because it includes a comprehensive involvement of stakeholders.
- This project directly addresses targeted barriers and incorporates many other research organizations directly into the team.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.5** based on accomplishments.

- It is doing very well.
- Version 1 of H2A is an excellent start in achieving analysis sub-program objectives.
- Version 2 will be another critical step.
- The rate of progress may have been a little slower than anticipated, however, it is better for the result to be a complete job rather than a quick job.
- This project is steadily making progress to add more analytical capabilities to the tool.
- Technical accomplishment is commensurate with this stage of the project.
- There is good progress within the limitations of the available budget resources.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **4.0** for technology transfer and collaboration.

- Very good network, however, consider to include the car industry and the fuel companies.
- There are a number of partners on the team.
- There are also lots of industrial collaborators.
- This project effective exchanged information with and used results achieved by others doing similar modeling.
- The involvement of the DOE and external modeling community and industry feedback/peer review is exemplary.
- The involvement of H2A provides extensive collaboration.
- Collaboration is a great strength of this project.
- This presenter is also integrated with other researchers on a team.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.7** for proposed future work.

- This project has an impressive plan.
- Future work is needed to improve the identified H2A model capability.
- Complete the linkage to delivery module.
- The responsiveness to enhancements suggested from beta testing is good and benefits the program.
- The planned extension to other parts of the H_2 program is essential.
- The planned work is appropriate. An important issue, however, will be how this project interfaces with the Macro model.
- This team appears to recognize that much of the value of its work lies in using the model to put future analyses on a common basis, e.g., application to technology validation projects.

Strengths and weaknesses

Strengths

- HFC&IT management commitment to a strong, well-integrated analysis and modeling capability.
- This H2A team has the ability to work constructively with other organizations, particularly industry.
- Since this model is Excel based, its potential users can easily understand it.
- There is a strong involvement of multiple stakeholders, including industry.
- There are consistent data sets due to the development of the data book.
- Their intent is to enforce consistent data and assumptions throughout the H_2 Program.
- The clear and extensive collaboration on this project appears to be drawing together many of the R&D projects.
- This model serves as the repository for agreed data.
- Large numbers of collaborators are both incorporated into the team and provide input from the periphery.
- There is a good focus on targeted barriers.

Weaknesses

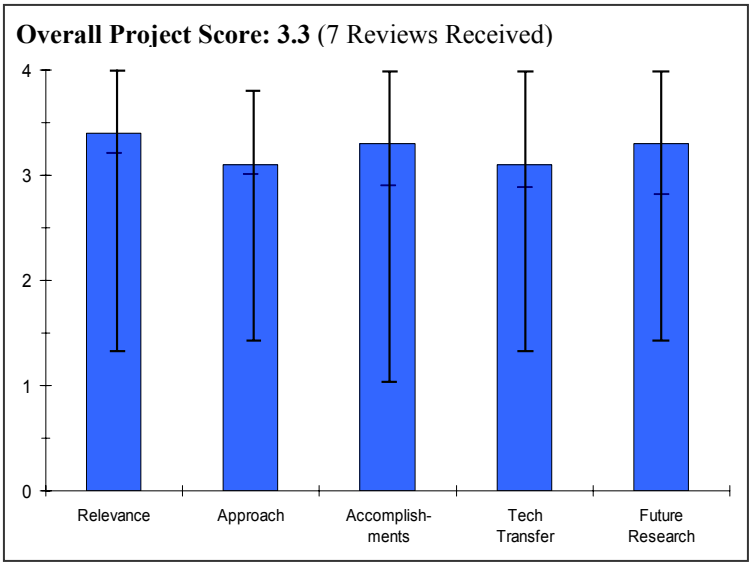
- This project does not start with a "clean sheet," so it must adjust models. Other "legacy" activities need to incorporate them and use them efficiently as part of this initiative.
- The core calculation as represented in the presentation appears to have some issues. The income calculation appears to be misrepresented: the income would normally be ($/kg x Kg/year) not ($/kg / kg/year). Cash flow would not normally include depreciation - perhaps this represents debt principal payments.
- Representation of capital cost changes over time and appears more arbitrary than in HyTrans, in which the economies of scale and learning curves are adopted.
- This is a big job with relatively limited resources.

Specific recommendations and additions or deletions to the work scope

- The EIA data do not consider the success of the hydrogen economy; it should be analyzed.
- The model only addresses a single production source - could multiple sources be incorporated?
- As with the other models, the interface between models and the planned Macro model are not yet clear. All modeling projects would benefit from an overall planned model architecture that outlines planned functionality of each model and its intended role.
- The H2A model appears to have the greatest potential to provide a common data set for hydrogen system components in a way that can then be used as a subroutine for a wider simulation and/or optimization model.

Project # AN-03: Hydrogen Transition Modeling and Analysis: HYTRANS v. 1.0
Greene, David; Oak Ridge National Laboratory

Brief Summary of Project



Oak Ridge National Laboratory has created a preliminary 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 hydrogen production and delivery; vehicle production, including technological progress, scale economies and learning-by-doing; and 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 MYPP goals and other technology forecasts, and publishing model documentation and scenario analyses.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- The project will be relevant especially when linked to a Macro model.
- The understanding the FCV buildup is an important input to many other analysis topics.
- The transition analysis is very important.
- This project scored a 3 because its transition analysis is only part of the overall System Analysis set of objectives.
- Based on the presentation, the analysis seems rather incomplete with respect to competition of various fuel options (hydrogen fuel cell, diesel, gasoline, and hybrid vehicles).
- The project addresses barriers C, D and E.
- The alignment of input assumptions with other modeling projects would assist with Barrier B.
- This is a useful analytical tool for addressing the transition.

Question 2: Approach to performing the research and development

This project was rated **3.1** on its approach.

- Improved work to establish the best representative scenarios should be completed
- FCV may bring other attributes to the market (120 V capabilities, noiseless, new designs related to wireless possibilities, etc.) that is not considered on existing correlations.
- Geographical specificities should be represented.
- Uncertainties in quantification should be considered.
- This approach may be too detailed for the level of accuracy expected from a consumer choice model.
- Is there a way to validate the results?
- Market equilibrium solutions are the ideal approach.
- Nonlinear optimization is very useful.
- This project has added H2A cost computation for consistency.
- The approach is very thorough and complex.
- Glad to see there were no equations in the presentation!

- Based on the presentation, the model developed seems to be rather complex. The presentation of the model should be improved to convey the message of the model. It was difficult to determine what the approach was, and particularly, what the assumptions were.
- The scenario analysis approach in the model along with an optimization process appears to be a very suitable framework for studying the transition to hydrogen use.
- Further description of the optimization process and the objective function would assist evaluation of the approach.
- Addresses targeted barrier directly. Good integration with other research, particularly H2A. Broad scope (vehicle technology choices, demand projections, infrastructure choices, infrastructure implementation - with optimization laid on top of all that), may have been a bit too ambitious.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- The non-linear optimization model as reported should become a target in the model development too.
- It is not clear if consumer preference is accurately represented.
- I am looking forward to more substantial results and conclusions in the next review period.
- Incremental work this year is on track - have working model to show feasibility.
- Delivery system model is preliminary - awaiting H2A delivery results.
- Preliminary results are interesting.
- This progress has been impressive and wealth of included detail.
- The extension of model capabilities is also impressive to include scale effects, learning effects, etc.
- The scenario results are presented in a concise and useful manner.
- It was interesting to note that the introduction of a hydrogen vehicle subsidy prevented the adoption of hybrids, eliminating early gains but creating long term gains. This would appear to force all efforts towards hydrogen, reducing diversity of solutions.
- This project has good progress vs. scope.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.1** for technology transfer and collaboration.

- The interaction and integration with H2A models should be intensified.
- Other experts should be considered to review and help the development.
- This could benefit from a more detailed review and feedback from other analysts/industry.
- Good collaborations with H2A group & UC Davis.
- This project relies on other models where appropriate.
- Consumer choice model needs to be fed to “macro-system model".
- There is a good inclusion of other critical models such as H2A, GREET, etc.
- It would be appropriate to see wider peer review.
- The extent of collaboration was not clear from the presentation.
- The modeling framework used in this project may well be useful in other projects.
- Good coordination.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.3** for proposed future work.

- It is well defined.
- The linkages to other models/analyses will provide significant benefits.
- Consideration of appropriate scenarios will be very important.
- Hooking this modeling effort to NEMS does not make sense to me.
- Future work should focus on relating this effort to the “macro-system” model.

- The work needs to interface into the Macro System model and work toward that.
- It is not clear what contingencies and optional analysis paths are being considered.
- Additional H_2 production paths should be incorporated at an early stage.
- Both the sensitivity analysis and linkage to the Macro Model will be useful steps.
- Plan appears sound.

Strengths and weaknesses

Strengths
- The market equilibrium concept using nonlinear optimization is an excellent approach.
- This approach will be a necessary component of the macro-system model in future work.
- The authors' experience in modeling consumer vehicle choice is valuable to macro-system model.
- The analytical approach used is quite powerful and has potential for other projects.
- The use of economies of scale, learning curves and technology change price reductions appear to be strength of the model.
- This project addresses a broad spectrum of topics over a wide transition window.

Weaknesses
- The first version of the model and its preliminary results are really for demonstration purposes only; nevertheless, the demonstration of the market analysis is a very important contribution to the overall H_2 system modeling program.
- The complexity of the modeling and associated mathematics/computing unfortunately detracts from ease of understanding of the detail and hence could reduce confidence in results.
- It is unclear how much model testing has taken place to validate results against, e.g., H2A.
- The presented results don't seem to reflect even the current mix of fuel types, especially hybrid gasoline and diesel.
- The use of common assumptions with other models would increase the comparability of the results of this project with other projects.
- The uncertainty as to whether the optimization process is finding the best solution is a concern.
- This project may be too ambitious, which could lead to modeling compromises that limit usefulness of results. The tendency of the model to make "either/or" technology choices (reality usually involves more of a technology mix) may be indicative of the effect of such compromises.

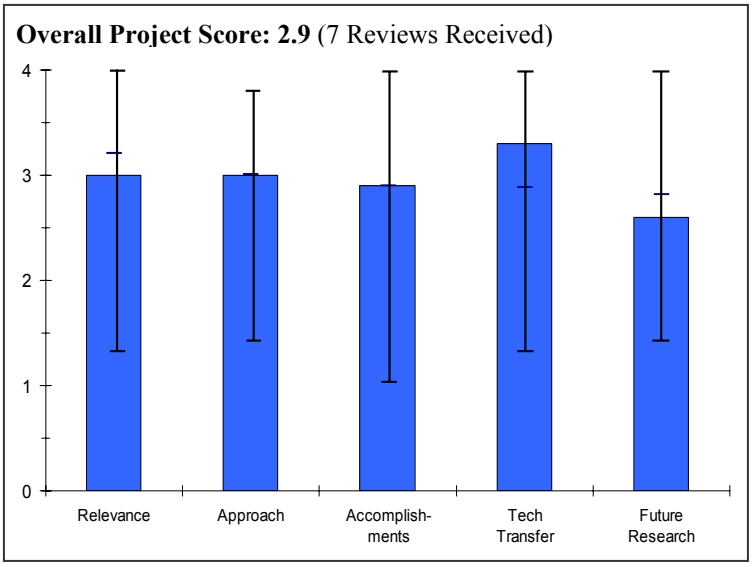
Specific recommendations and additions or deletions to the work scope

- Reaching the limits of the non linear optimization software leads to a concern over modeling for modeling's sake. To what extent are reasonable compromises possible in some areas to facilitate analysis, especially if it is desirable to incorporate geographical, i.e., greater detail?
- The use of assumptions that are common across analysis projects is important.
- An optimization process like this has potential in the macro model.
- The transition to hydrogen could be modeled using a decision based model where uncertainties in future market and technology outcomes can be considered in the model. This may assist in determining which R&D investments should be adopted given an assessment of probability of outcomes.
- The inclusion of hydrogen hybrid ICEs would be useful so that it can be assessed as a potential transition technology.

Project # AN-04: WinDS-H2 Model and Analysis
Short, Walter; National Renewable Energy Laboratory

Brief Summary of Project

By the end of FY 2004, the National Renewable Energy Laboratory completed the basic modeling structure of HyDS (renamed from WinDS-H2), implemented the most current data from the H2A effort, and used the HyDS model to analyze a base case. These results focused on the capability of wind to produce hydrogen for use as a transportation fuel and as a means of storage at the wind site. In FY 2005, NREL will refocus the model to encompass a larger range of production technologies and H_2 fuel distribution possibilities, as well as use the model to evaluate several scenarios.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This project should be integrated with a macro model.
- Part of its effort has been addressed in other projects.
- It is not clear if distributed electrolysis hydrogen production from central wind (i.e. renewable electricity distribution) will contribute to HFCIT goals. Focus should be on vehicle application (i.e. pipeline hydrogen delivery).
- There is strong program relevance of wind-to-H_2.
- I also see this as a small but interesting step in the direction toward the "macro-system" model.
- The fit and relevance of the Win-DS model to the H_2 program is not clear from the presentation; this is a relatively small subset activity that may not apply for a long time.
- The scope is narrowly focused on wind, whereas the President's Hydrogen Fuel Initiative supports a diversity of fuels and feedstocks. The project would be more relevant if additional fuels and feedstocks were at least used for comparison to the wind scenario(s).
- This appears to be a key element of modeling, bringing together electrical and hydrogen systems.
- The original intent of project appears to more oriented towards wind energy and finding a role for it in hydrogen than in promoting the hydrogen objectives.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Integration with a macro model is necessary.
- Uncertainties should be quantified.
- Wind should be competing with other renewable resources.
- It is not clear if low-volume SMR capital costs are used. H2A cases for SMR are for high production volume only.
- The regional model is good for evaluation wind production potential.
- It appears the H_2 production & transport maybe redundant with other efforts in the program (such as H2A-delivery).
- The linear minimization technique is not as powerful as the nonlinear method used in HyTrans.
- The use of H2A data gathering is commended to avoid duplication and ensure commonality of data.

- The future energy mix does not seem realistic or consistent with DOE's long-term objectives. The assumption is that coal, wind and (second half of the legend is unreadable) will grow, and other sources, such as nuclear, will steadily decline. This seems to be in conflict with DOE priorities (though perhaps not EIA projections).
- The treatment of the electrical system is quite sophisticated.
- This approach does not address some of the identified barriers: it is not really a macro system model nor does it contribute to eliminating "siloing" (it does take advantage of other programs that do). It does look at transition, but more in terms of how transition might drive this technology, rather than how the technology might drive the transition.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.9** based on accomplishments.

- The results do not necessarily add significantly to our understanding of the transition to hydrogen vehicles or the potential for using wind resources for this purpose.
- It started from existing WinDS model.
- Renaming the H_2 version of model is slightly misleading, because the focus & strength of the work is the renewable (wind) component.
- The comparison of distributed electrolysis, wind-electrolysis, & distributed SMR is interesting.
- It is hard to rate the new project development of existing project.
- Good progress on wind and SMR, addition of other technologies will be important.
- This group has accomplished what it set out to do.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.3** for technology transfer and collaboration.

- This is a very good team.
- They could use more non-wind industry feedback and collaboration with other analysis projects.
- Collaboration with H2A effort - using their cost database.
- The project uses multiple collaborations (Wind group, H2A) but could usefully be peer reviewed for overall fit in the H_2 program and alignment of goals, i.e., where does it fit in the big picture ?
- Extensive collaboration, including with H2A and wind community.
- Good to see collaboration with H2A etc.
- Appear to have coordinated with H2A.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- It is trying to replicate some of the work planned at the H2A.
- It should to integrate with a H2A macro model (HYTRANS at least).
- Proposed future work should have been part of the original scope.
- Any of the plans the author mentioned involve additions to the model that overlap with what will be needed in the “macro-system” model.
- Since this project is not designed to become the “macro-system” model, the future plans should be aimed at providing a wind sub-model for the larger macro-system effort.
- The project did not explain the future direction in a clearly understandable way with appropriate rationale.
- The projects proposed future research still does not include nuclear - though the larger DOE hydrogen program and other DOE initiatives do.
- There is the potential for duplication if this model remains isolated from other models.
- Project has served its purpose. Although it is now attempting to adapt model to scenarios outside of its scope, other projects may cover the same territory.

Strengths and weaknesses

Strengths
- The regional wind model is necessary for analyzing the renewable production of H_2.
- This model should be included in the macro-system modeling effort.
- Strong detailed assessment of the wind aspects.
- There is a representation of the electrical system in detail and the approach to transmission augmentation.
- This project provides a realistic analysis of the potential role of wind in the hydrogen program.

Weaknesses
- This model continues to build other H_2 production & transportation modeling which seems redundant with what the macro-system model will eventually need to do.
- The presentation was hurried and confusing without clear rationale for why the work is being done and how the results fit into an overall view of a future H_2 economy. It seemed that there were good reasons behind the results being presented but this was not coming through clearly.
- Use of a fixed cost ($2/kg) for H_2 seems an oversimplification.
- Focus on wind scenarios (though the basis is reforming of natural gas) sounds more like a biased sales pitch for wind than an independent, and hence legitimate, analysis.
- The examination of wind in detail without a defined interface with other models is limiting.
- Some of the model assumptions (H_2 produced in off peak) appear to be untested - why not test sensitivity to these assumptions. There may be a trade off between electricity price and equipment utilization. Perhaps shoulder price H_2 production would be more optimal.
- There is the potential for duplication if this model remains isolated from other models.
- Whilst there is a good representation of the electricity system, key barriers such as wheeling charges may not have been addressed.
- This project is more focused as a wind project than a hydrogen project.
- This project is not well-aimed at resolving identified barriers.

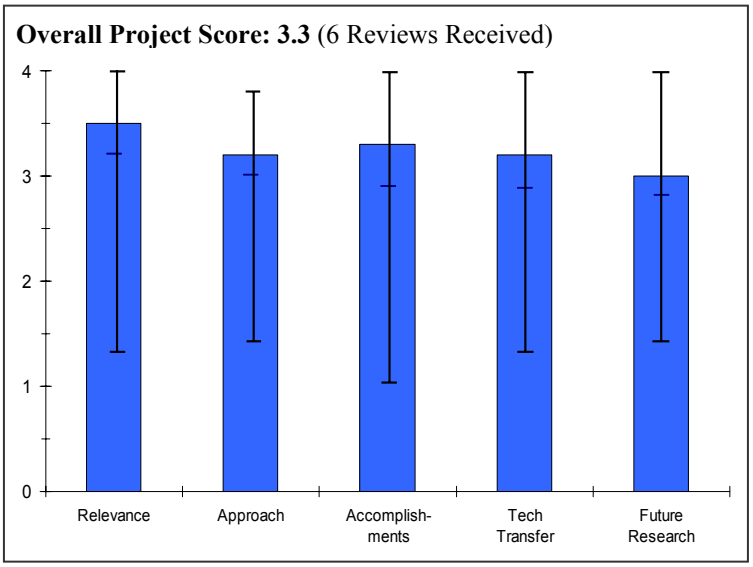
Specific recommendations and additions or deletions to the work scope

- This appears to be a relatively narrow topic, especially for the near term.
- The reduction in common power electronics cost for wind turbine co-located with the electrolyzer is not convincing.
- Inclusion of GHG effects would be valuable - electrolyzers using grid electricity.
- This project needs collaboration with other modeling activities to help show its relevance.
- Do not ignore nuclear energy specifically, without a stated justification or reason.
- This work should be integrated into a macro model so that the detailed wind analysis can be considered in comparison to other sources.
- Not clear whether transmission losses are considered.
- Proposed change in scope to model other technologies could salvage this as a hydrogen project, but there should be a careful look at the relevance of this project vs. other efforts.

Project # AN-05: Technical and Economic Studies of Regional Transition Strategies toward Widespread Use of Hydrogen Energy
Ogden, Joan; UC Davis

Brief Summary of Project

This University of California, Davis project will develop new simulation tools to evaluate alternative pathways toward widespread use of hydrogen under various demand scenarios and regional conditions. Tasks are to understand which factors are most important in finding viable transition strategies, to develop "rules of thumb" for future regional hydrogen infrastructure development, to conduct regional case studies of H_2 infrastructure transitions, and to work with the H2A core group to develop models of hydrogen delivery systems.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- It has been helping several other studies.
- It should help the integration of several H2A models into a single macro model (regionally based).
- Transportation issues are critical to understanding which hydrogen technologies should receive the most attention.
- The objectives were clear and focused on meeting transition analysis needs.
- Understanding of the phenomena associated with regional transitions to the hydrogen economy is essential in light of the fact that regional demand and availability of resources will define regional economics and therefore inform feedstock and technology choices.
- This approach can prepare an infrastructure plan that could be generally specified by a higher level optimization or simulation model.
- This is useful work for understanding the transition.

Question 2: Approach to performing the research and development

This project was rated **3.2** on its approach.

- Uncertainty analysis should be performed. It is different than sensitivity analysis because it quantifies the inherent uncertainties, the limits for them and the tendencies (possibility for a certain scenario to occur).
- Existing fuel station sites should be considered primarily.
- Approach seems to have evolved over time rather than following a set plan, but probably reasonable given DOEs evolving needs.
- The approach embodies a pragmatic 'rule of thumb' methodology that is easily understood.
- Tool developed is exclusively applicable regionally; however, it is generic enough to be applied to a variety or regions with specific case studies.
- The core approach is very useful for the examination of hydrogen infrastructure development.
- Inclusion of other sources will enhance its applicability.
- This approach addresses the targeted barriers.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Progress is not very significant this year. However, the project progress was impacted by the by the project funding.
- Progress is pragmatic, yet relatively simple; the approach is easy to validate and test and leads to credible and understandable results that are easy and relatively quick to prepare.
- The case study provided was developed to a point of completeness and a useful result.
- Good progress to date; the group has developed some useful tools.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.2** for technology transfer and collaboration.

- This group appears to have worked closely with the H2A team; however, more industry input is needed.
- Collaboration with other programs allows good interchange of data and review of methodology, e.g., Pathways, H2A.
- Extensive collaboration with H2A, Industry, other Universities, and NREL.
- Good collaboration is evident in this group.
- Good coordination with other efforts.

Question 5: Approach to and relevance of proposed future research

This project was rated **3.0** for proposed future work.

- The group needs to focus efforts and analysis on establishing the necessary assumptions.
- No need to estimate market penetration for consumer choice - ORNL is already doing this. Other proposed work that looks reasonable.
- Encourage the use of rules of thumb.
- Interaction with H2A and development of reasonable scenarios for transition are also very valuable.
- Continuation of pragmatic approach lends credibility to output.
- Work proposed would seem to lead to a general understanding of how various factors would affect a transition to a hydrogen economy.
- This project would benefit from the addition of an automation process to enable multi region infrastructure development.
- This group's plans are appropriate for remaining scope.

Strengths and weaknesses

Strengths
- Most of the other analysis projects are using the support of this project.
- This project uses a pragmatic approach, utilizing case studies, to ease understanding.
- Inclusion of GHG consideration and effects good.
- The project was presented very clearly and methodically.
- Strong GIS base, approach to routing of infrastructure.
- Very detailed.
- Useful tools.
- This project provides valuable learning's in terms of how to analyze specific transition cases.

Weaknesses

- The group should perform uncertainty analysis.
- Lack of automation.
- Lack of optimization.
- It is a static representation.
- There is a limited scope.

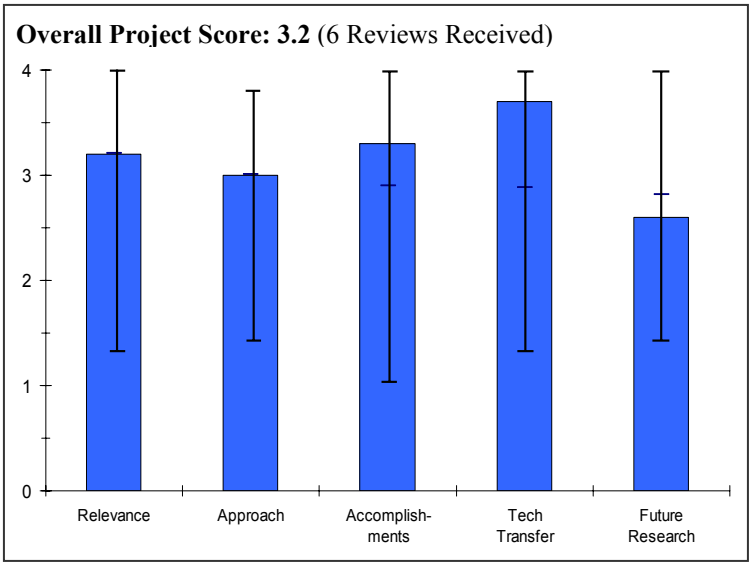
Specific recommendations and additions or deletions to the work scope

- Keep supporting it.
- Include as an algorithm in the macro model.
- If automated, has potential to be used to detail scenarios that are either directly developed or result from an optimization process.
- The ideal networks could be ground truthed against actual city layouts and then calibrated to key city characteristics.

Project # AN-06: Fuel Choice for FCVs: Hydrogen Infrastructure Costs
Lasher, Stephen; TIAX LLC

Brief Summary of Project

TIAX has previously evaluated the long-term benefits and costs for a wide range of vehicle/fuel pathway combinations. Based on the promising long-term benefits but uncertain near-term costs for compressed hydrogen as a fuel for fuel cell vehicles, TIAX is currently evaluating the transitional costs of various hydrogen fuel infrastructure pathways using a net-present-value-type analysis for the entire United States.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- This project is duplicating efforts with several H2A project.
- It should contribute to the development of a macro model integrating several analysis sub-projects.
- The group should focus is on capital costs and risks for infrastructure investors. Investor views of financial attractiveness of hydrogen investments will be a key factor in timing, pace of moving to a hydrogen economy.
- This project can provide valuable inputs for work that more directly impacts achievement of hydrogen program objectives.
- The objectives were well laid out and show clear linkage to DOE goals.
- The project is highly relevant because the reduction of capital cost is essential to the successful transition to the hydrogen economy.
- The analysis of the existing excess hydrogen capacity has particular potential to assist transition analysis.
- A very thorough and detailed analysis of transition scenarios.

Question 2: Approach to performing the research and development

This project was rated **3.0** on its approach.

- Uncertainties should be addressed and quantified.
- A better data treatment and scenario composition methodology should be developed.
- Work on this project preceded initiation of H2A activities.
- Approach utilizes separate TIAX assumptions to some extent.
- Approach being modified to use inputs, assumptions from other models.
- Use/involvement with H2A has been valuable to consistency between the program results and other DOE work.
- Steam reforming (both central and distributed) were addressed in the project, however electrolysis was not. The reason was not discussed.
- The deterministic approach to scenarios is informative but would be enhanced if the scenarios were as a result of an optimization process.
- Project addressing targeted barriers, and is well integrated.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **3.3** based on accomplishments.

- Work on this project is providing results and insights that are useful for government and private sector decision-makers.
- Some demand modeling is being done- a plus.
- Overall, good value for relatively small resources being spent.
- Results invite credibility due to clarity and presentation style.
- Derivation of older model a cost effective way to proceed with enhanced analysis.
- The case study on use of existing potential H_2 production was a good test.
- The meaningfulness of the scenarios is unclear but the excess hydrogen information is very interesting.
- Have completed a very thorough analysis of the scenarios addressed to date.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.7** for technology transfer and collaboration.

- Some coordination of work with H2A.
- Project team collaborates with a number of organizations.
- Collaboration seems to have been good with diverse participants.
- The project assumptions seem excessively conservative - to the point that if market penetration were that low the hydrogen economy would probably not be feasible.
- Collaboration and response to previous year review is evident.
- Very good integration.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.6** for proposed future work.

- Wind and electrolyzer model have been done on other projects.
- Proposal submitted to DOE on renewable pathways to hydrogen. No details provided.
- Need to clarify how proposed future work adds value, complements other analyses at national labs, universities.
- Project is nearing end so not much future work was discussed. Extension to renewable pathways makes sense.
- This is the last year of the project.
- Integration with other models is probably the key next issue.
- This project will significantly enhance the scope of the work.

Strengths and weaknesses

Strengths
- The assessment of excess existing hydrogen capacity.
- The use of an NPV approach is a useful tool for examining scenarios.
- Realistic scenarios.
- Very thorough.
- Credible results.
- Very good solicitation and use of outside sources.

Weaknesses

- Unable to determine how effectively this work on hydrogen transition modeling is being linked/ integrated with other program-sponsored transition modeling.
- The presentation was somewhat hurried and did not allow sufficient time to understand the assumptions made to generate the various scenario results - it sounded as though many were simply what-if type guesses.
- Given that making ammonia and methanol is not economic with high US gas prices it is suspect to assume that low cost H_2 can be obtained from these plants unless you assume that costs are all sunk already?
- The manner in which the scenarios have been established.

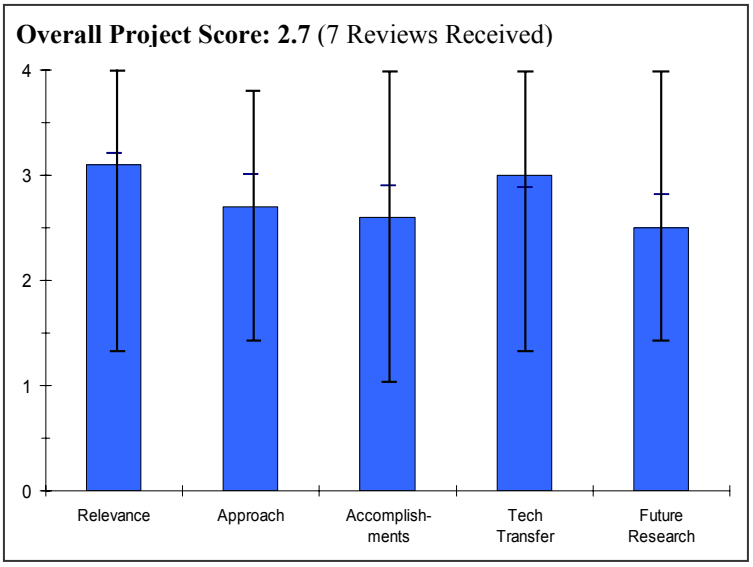
Specific recommendations and additions or deletions to the work scope

- When additional assumptions have been required in analysis projects, these should be documented and captured in the data book.
- Combining this work with other analysis results to generate a consensus view would be valuable.
- Follow on work (if any) should assume a shorter transition. It seems highly unlikely that there would ever be a hydrogen economy if there was only 30% market penetration in 2060.
- The inclusion of the excess existing hydrogen capacity in other models is necessary.
- The analysis of scenarios that emerge from other optimization models would validate both models.

Project # ANP-01: Hydrogen Production in a GHG-Constrained Situation: Major Results & Conclusions
Dougherty, Bill; Tellus Institute

Brief Summary of Project

Tellus Institute is examining in a detailed quantitative manner, plausible scenarios for a transition to a hydrogen economy. They will explicitly illustrate the staging and sequencing of major phases of the transition scenarios and their implications, quantify the greenhouse gas reduction benefits of each of the transition scenarios, explore the spatial characteristics of the transition scenarios based on GIS analyses for four greater metropolitan areas in the United States (Boston, Denver, Houston, and Seattle), and account for relevant techno-economic and policy factors.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Replicate efforts with H2A project.
- Stated objectives are reasonable and in line with DOE goals.
- The analysis of transition is important to program planning.
- This project was started prior to planning for the "macro-system" model - some aspects of the effort may be applicable to the larger effort.
- The project has clear objectives and was well presented.
- Limited focus - suggest including nuclear (and other non-GHG producing electricity) and not limit application to hydrogen fuel cell vehicles.
- The objective of this project is directly relevant to the hydrogen objectives and program.
- Useful analysis of a transition scenario.

Question 2: Approach to performing the research and development

This project was rated **2.7** on its approach.

- "If-then" scenario type is a poor approach.
- The location of existing fuel station should be considered.
- Description of the approach was not clear.
- Created a new model that reflects the NEMS model - I see limited value in this approach.
- Extending projections to 2050 based on other published projections is not very predictive.
- Assumption of a dual-fuel vehicle, while considered by an OEM (BMW) is not a good idea - experience with other AFV programs has shown that consumers don't use the alternate fuel, so these dual use vehicles don't help develop the fueling infrastructure.
- The basing of the demand on Annual Energy Outlook 2003 made a lot of sense and is very defensible.
- Broader focus would make conclusions on advantages of hydrogen with respect to GHG emissions more meaningful. Specifically -- change of mix of grid electricity and increase hydrogen applications.
- An interesting representation of geographic distribution of demand over time.
- Assessment of CO_2 emissions is helpful characterization of the results.
- The work itself was consistent with the objectives, but there appeared to be a possible disconnect when translating the work into conclusions.

Question 3: Technical accomplishments and progress toward project and DOE goals

This project was rated **2.6** based on accomplishments.

- Old conclusions using old approaches.
- Conclusions are very reasonable, but it is not clear how they were determined.
- The project shows progress with plenty of results.
- Projections are subject to the assumptions regarding vehicle penetration.
- There is a lack of a market feedback mechanism limits the strength of the conclusions.
- This presentation had inadequately documented graphs.
- While not unexpected the results confirm other work. Conclusions were clear and logically presented.
- It is not clear how the conclusions have been drawn from results from the project to date.
- Good work, strange-appearing conclusions.

Question 4: Technology transfer/collaborations with industry, universities and other laboratories

This project was rated **3.0** for technology transfer and collaboration.

- Car industry and fuel companies should be involved.
- This project would benefit from a deep dive into the model assumptions.
- Writing their tool in VBA would allow for transfer to other users, including possibly the macro-system modeling effort in the future.
- A number of collaborators were listed but it would have been good to see more industry participation.
- Not assessed.
- Good interaction.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.5** for proposed future work.

- The benefit of doing a localized study is to bring the specificities of the region into consideration.
- It is not offering too much contribution on the methodology or on the political discussion.
- It is not clear that future work is necessary.
- Many of the next steps seem redundant with other modeling efforts in the analysis program.
- While the authors are thinking about the macro-system modeling needs, the future planning does not seem to suggest how they would contribute to that developing effort.
- The project is nearing completion; discussions on any future work have not yet taken place.
- Proposed future research includes additional use of non-GHG emitting technologies, however work should be broaden further to cover more "realistic" future energy mixes and more innovative/extensive hydrogen applications.
- Not assessed.
- Project appears to be finished.

Strengths and weaknesses

Strengths
- Thorough analysis, well presented and understandable results.
- GIS analysis system.
- Good, detailed work.
- Useful learning's on how to do analysis that can be applied to future work.

Weaknesses

- It was not clear that H2A assumptions are being used though this project may pre-date H2A.
- Presentation used excessive (and mostly undefined) acronyms and abbreviations.
- Conclusions are not demonstrated in presented results.
- There is a lack of optimization process to define the scenarios used.
- Conclusions based on observations that are inconsistent with other work (e.g., "Hydrogen FCVs are unlikely to be cost competitive with gasoline ICEs").

Specific recommendations and additions or deletions to the work scope

- The project was concise and easy to understand unlike several others which often show excessive detail and confuse the message, i.e., modeling for modelings sake not for the analysis of the results.
- Adoption of scenarios produced by optimization models would assist in validating modeling.
- Modeling of the same region as is being studied in other models would assist validation.

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APPENDIX B: PROJECTS NOT REVIEWED or NOT SCORED ON CRITERION 3

Projects Presented but Not Reviewed

Proj #	Title	PI Name	Organization
ANALYSIS			
ANP-2	Hydrogen Production Infrastructure Options Analysis	Brian D. James	Directed Techs.
ANP-3	Impact of Hydrogen Production on U.S. Energy Markets	Harry Vidas	Energy & Envr. Analysis
ANP-4	Analysis of the Hydrogen Production and Delivery Infrastructure as a Complex Adaptive System	George Tolley	RCF, Inc.
ANP-5	Hydrogen Analysis Support	Marylynn Placet	Pacific Northwest National Laboratory
ANP-6	Macro-System Model	Mark Ruth	National Renewable Energy Laboratory
ANP-7	Well-to-Wheels Analysis with the GREET Model	Michael Wang	Argonne National Laboratory
FUEL CELLS			
FC-51	Fuel Cells Basic Research	Dr. Paul Maupin	U.S. Department of Energy/Basic Energy Sciences
FCP-6	Low Cost, High Performance PPSA-based PEM Fuel Cell Membranes	Junqing Ma	T/J Technologies
FCP-9	Novel, Combinatorial Method for Developing Cathode Catalysts for Fuel Cells	Keith Kepler	Farasis Energy
FCP-10	Improved Fuel Cell Cathode Catalysts Using Combinatorial Methods	Junhua Jiang	NuVant Systems
FCP-13	Development of Low-Cost, Clad Metal Bipolar Plates for PEM Fuel Cells	Scott Weil	Pacific Northwest National Laboratory
FCP-21	Complex Coolant Fluid for PEM Fuel Cell Systems	Satish Mohapatra	Advanced Fluids Tech.
FCP-22	Advanced Catalysts for Fuel Cells	S. Narayanan	JPL
PRODUCTION AND DELIVERY			
PD-8	Integrated Hydrogen Production, Purification & Compression System	Satish Tamhankar	BOC Group, Inc.
PD-9	Low-Cost Hydrogen Distributed Production Systems	Frank Lomax, Jr.	H2Gen Inno. Inc.
PD-10	Integrated Short Contact Time Hydrogen Generator	Ke Liu	GE Global Research
PD-22	Photoelectrochemical Hydrogen Production Using New Combinatorial Chemistry Derived Materials	Eric McFarland	University of California, Santa Barbara
PD-36	Hydrogen Production Using Nuclear Energy	A. David Henderson	U.S. Department of Energy
PD-37	Basic Research Needs for Hydrogen Production	Prof. Tom Mallock	Pennsylvania State University
PDP-11	Zeolite Membrane Reactor for Water-Gas-Shift Reaction for Hydrogen Production	Jerry Y.S. Lin	University of Cincinatti
PDP-12	Large Area Cell for Hybrid Solid Oxide Fuel Cell Hydrogen Co-Generation Process	Joseph Hartvigsen	Ceramatec Inc.
PDP-18	Inorganic Membrane Porous Support Tube Fabrication	Brian Bischoff	Oak Ridge National Laboratory

Proj #	Title	PI Name	Organization
PDP-21	Ion Transport Membranes for Hydrogen Separation	Andrew Payzant	Oak Ridge National Laboratory
PDP-26	High-Performance, Durable, Palladium-Alloy Membrane for Hydrogen Separation & Purification	Scott Hopkins	Pall Corp.
PDP-29	Maximizing Light Utilization Efficiency & Hydrogen Production in Microalgal Cultures	Tasios Melis	University of California, Berkeley
PDP-30	Novel Two-Stage Process for Photobiological Hydrogen Production	Juergen Polle	Advanced Bionutrition Corp.
PDP-31	Hydrogen from Water in a Novel Recombinant Oxygen-Tolerant Cyanobacteria System	Hamilton Smith	J. Craig Venter Inst.
PDP-34	Solar Water Splitting: Photocatalyst Materials Discovery & Systems Development	Thomas McNulty	GE Global Research
PDP-35	Critical Research for Cost-Effective Photoelectrochemical Production of Hydrogen	Liwei Xu	Midwest Optoelectronics
PDP-36	Bioinspired Composite Nanomaterials for Photocatalytic Hydrogen Production	John Peters	Montana State University
PDP-37	Photoelectrochemical Hydrogen Production	Arun Madan	MVSystems, Inc.
PDP-38	Development of Water Splitting Catalysts Using a Novel Molecular Evolution Approach	Neal Woodbury	Arizona State University
PDP-43	Modular System for Hydrogen Generation & Oxygen Recovery	Iouri Balachov	SRI International
PDP-44	Fundamentals of a Solar-thermal Mn2O3/MnO Thermochemical Cycle to Split Water	Alan Weimer	University of Colorado
PDP-50	Hydrogen Delivery Infrastructure Options Analysis	Tan-Ping Chen	Nexant Inc.
PDP-51	New Materials for Hydrogen Pipelines	Barton Smith	Oak Ridge National Laboratory
PDP-52	Evaluation of Natural Gas Pipeline Materials and Infrastructure for Hydrogen/Hythane Service	Thad Adams	Savannah River National Laboratory
PDP-53	Novel Hydrogen Screw Compressor	David Livengood	Argonne National Laboratory
PDP-54	Inexpensive Delivery of Compressed Hydrogen with Advanced Vessel Technology	Salvador Aceves	Lawrence Livermore National Laboratory
SAFETY, CODES & STANDARDS			
SAP-2	IEA Hydrogen Implementing Agreement Secretariat	Mary-Rose de Valladares	M.R.S. Enterprises, LLC
STORAGE			
ST-8	A Synergistic Approach to the Development of New Hydrogen Storage Materials	Samuel Mao	University of California
ST-11	Center of Excellence for Chemical Hydrogen Storage	Bill Tumas	Los Alamos National Laboratory
ST-20	System Level Analysis of Hydrogen Storage Options	Rajesh Ahluwalia	Argonne National Laboratory
STP-1	Next Generation Hydrogen Storage Containers	Andrew Weisberg	Lawrence Livermore National Laboratory
STP-2	Fuel Cell and Hydrogen Research	Lee Stefanakos	University of South Florida

Proj #	Title	PI Name	Organization
STP-3	Center of Excellence for Chemical Hydrogen Storage: PNNL Tasks and Collaborations	Chris Aardahl	Pacific Northwest National Laboratory
STP-4	Electrochemical Hydrogen Storage Systems	Digby MacDonald	Pennsylvania State University
STP-5	Main Group Element Chemistry for Hydrogen Storage and Activation	Anthony Arduengo	University of Alabama
STP-6	Chemical Hydrogen Storage Using Ultra-High Surface Area Main Group Elements	Philip Power	University of California, Davis
STP-7	Chemical Hydrogen Storage Using Polyhedral Borane Anion Salts	Fred Hawthorne	University of California, Los Angeles
STP-8	Amineborane Hydrogen Storage	Larry Sneddon	University of Pennsylvania
STP-9	Kinetic and Mechanistic Studies of B-N Hydrogenation/Dehydrogenation	Mike Heinekey	University of Washington
STP-10	Development of Advanced Chemical Hydrogen Storage and Generation System	Ying Wu	Millennium Cell
STP-11	Novel Approaches to Hydrogen Storage: Conversion of Borates to Boron Hydrides	Susan Linehan	Rohm and Haas
STP-12	Combinatorial Synthesis and High Throughput Screening of Effective Catalysts for Chemical Hydrides	Xiao-Dong Xiang	Intematix Corp.
STP-13	Development of Regenerable, High-Capacity Boron Nitrogen Hydrides for Hydrogen Storage	Ashok Damle	Research Triangle Institute
STP-14	Safety Analysis and Applied Research on the Use of Borane-Amines for Hydrogen Storage	Clinton Lane	Northern Arizona University
STP-15	Overview of DOE Metal Hydride Center of Excellence (MHCoE)	Jim Wang	Sandia National Laboratory
STP-16	Synthesis and Properties of Aluminum Hydride as a Hydrogen Storage Material	Jim Wegrzyn	Brookhaven National Laboratory
STP-17	Development and Evaluation of Advanced Hydride Systems for Reversible Hydrogen Storage	Robert Bowman	Jet Propulsion Laboratory
STP-18	Neutron Scattering Characterization and Thermodynamic Modeling of Advanced Metal Hydrides for Reversible Hydrogen Storage	Terry Udovic	National Institute of Standards and Technology
STP-19	Novel Synthetic Approaches for the Preparation of Complex Hydrides for Hydrogen Storage	Gilbert Brown	Oak Ridge National Laboratory
STP-20	Development of Reversible Hydrogen Storage Alane	Ragaiy Zidan	Savannah River National Laboratory
STP-21	Synthesis of Nanophase Materials for Thermodynamically Tuned Reversible Hydrogen Storage	Channing Ahn	California Institute of Technology
STP-22	Thermodynamically Tuned Nanophase Materials for Reversible Hydrogen Storage: Structure and Kinetics of Nanoparticle and Model System Materials	Bruce Clemens	Stanford University
STP-23	Reversible Hydrogen Storage Materials – Structure, Chemistry and Electronic Structure	Ian Robertson	University of Ilinois – U-C

Proj #	Title	PI Name	Organization
STP-24	Effect of Gaseous Impurities on Long-Term Thermal Cycling and Aging Properties of Complex Hydrides for Hydrogen Storage	Dhanesh Chandra	University of Nevada - Reno
STP-25	First-Principles Modeling of Hydrogen Storage in Metal Hydride Systems	Karl Johnson	University of Pittsburgh
STP-26	Synthesis and Discovery of Nanocrystalline Reversible Hydrides by Vapor Phase Reactions	Zak Fang	University of Utah
STP-27	Lightweight Intermetallics for Hydrogen Storage	J.-C. Zhao	GE
STP-28	Thermodynamically Tuned Nanophase Materials	Greg Olson	Hughes Research Labs
STP-29	High Throughput Combinatorial Chemistry Development of Complex Metal Hydrides	Xiao-Dong Xiang	Intematix Corp.
STP-30	Overview of DOE Carbon-based Materials Center of Excellence	Mike Heben	National Renewable Energy Laboratory
STP-31	Metal-doped Carbon Aerogels for Hydrogen Storage	Joe Satcher	Lawrence Livermore National Laboratory
STP-32	Synthesis and Processing of Single-Walled Carbon Nanohorns for Hydrogen Storage and Catalyst Supports	David Geohegan	Oak Ridge National Laboratory
STP-33	Neutron Characterization of Carbon-Based Materials	Dan Neumann	National Institute of Standards and Technology
STP-34	Enhanced Hydrogen Dipole Physisorption	Channing Ahn	California Institute of Technology
STP-35	Controlling the Diameter of Single Walled Carbon Nanotubes for Hydrogen Storage	Jie Liu	Duke University
STP-36	Study of Hydrogen Storage in Advanced Boron and Metal Loaded High Porosity Carbons	Peter Eklund	Pennsylvania State University
STP-37	Cloning Single Wall Carbon Nanotubes for Hydrogen Storage	Richard Smalley	Rice University
STP-38	Optimization of SWNT Production and Theoretical Models of H_2-SWNT Systems for Hydrogen Storage	Boris Yakobson	Rice University
STP-39	Examination of the Physical Aspects of Hydrogen Storage in MOFs	Omar Yaghi	University of Michigan
STP-40	Hydrogen Storage in Graphite Nanofibers and the Spillover Mechanism	Ralph Yang	University of Michigan
STP-41	Characterization of Hydrogen Adsorption by NMR	Yue Wu	University of North Carolina
STP-42	Conducting Polymers as New Materials for Hydrogen Storage	Alan MacDiarmid	University of Pennsylvania
STP-43	Designing Microporous Carbons for Hydrogen Storage Systems	Guido Pez	Air Products
STP-44	Carbide-Derived Carbons with Tunable Porosity Optimized for Hydrogen Storage	Yury Gogotsi	University of Pennsylvania
STP-45	Electron-Charged Graphite-Based Hydrogen Storage Material	Chinbay Fan	GTI
STP-46	Nanostructured Activated Carbon for Hydrogen Storage	Israel Cabasso	SUNY - Syracuse
STP-47	Glass Microspheres for Hydrogen Storage	Matt Hall	Alfred University

Proj #	Title	PI Name	Organization
STP-48	Inorganic Clathrates for Hydrogen Storage	Viktor Struzhkin	Carnegie Institute
STP-49	Metal Perhydrides for Hydrogen Storage	Jim Hwang	Michigan Technology University
STP-50	Hydrogen Storage Materials with Binding Intermediate Between Chemisorption and Physisorption	Juergen Eckert	University of California, Santa Barbara
STP-51	Lithium Nitride-Based Materials for Hydrogen Storage	Leon Shaw	University of Connecticut
STP-52	New Concepts for Optimized Hydrogen Storage in MOFs	Omar Yaghi	University of Michigan
STP-53	Hydrogen Storage in Novel Organic Clathrates	Jerry Atwood	University of Missouri - St. Louis
STP-54	Hydrogen Absorption on Irradiated Carbon and Other Materials	Luis Muga	TOFTEC, Inc.
STP-55	Development of Complex Metal Hydride Hydrogen Storage Materials	Jim Ritter	University of South Carolina
STP-56	Clean Energy Research Project III: Hydrogen Storage Using Chemical Hydrides	Michael Matthews	University of South Carolina
STP-57	Underground LH2 Off-Board Hydrogen Storage Technology	Mark Richards	GTI
STP-59	Fundamental Studies of Advanced High-Capacity, Reversible Metal Hydrides	Craig Jensen	University of Hawaii
STP-60	Center of Excellence for Chemical Hydrogen Storage: LANL Tasks and Collaborations	Bill Tumas	Los Alamos National Laboratory
STP-61	Center of Excellence for Chemical Hydrogen Storage: LANL Tasks and Collaborations	Bill Tumas	Los Alamos National Laboratory
STP-62	Development of Metal Hydrides at Sandia National Laboratories	Jim Wang	Sandia National Laboratory
STP-63	NREL Activities in DOE Carbon-based Materials Center of Excellence	Mike Heben	National Renewable Energy Laboratory
TECHNOLOGY VALIDATION			
TVP-3	NextEnergy Microgrid and Hydrogen Fueling Facility	Dave McLean	NextEnergy
TVP-7	R&D of a PEM Fuel Cell, Hydrogen Reformer, and Vehicle Refueling Facility (Las Vegas Energy Park)	Mark Wait	Air Products
TVP-10	Hydrogen Fuel Project - H2Fuel	Dereck Morse	RTC of Washoe County
TVP-13	Global Assessment of Hydrogen Based Technologies	Robert Peters	University of Alabama
BRAZIL GUEST POSTERS			
-	Brazilian Fuel Cell Program (ProCaC)		Brazilian Ministry of Science and Technology
-	Brazilian Roadmap for a Hydrogen Economy		Brazilian Ministry of Mines and Energy

Reviewed Projects Not Scored on Technical Accomplishments (Criterion 3)

Proj. #	Title	Name	Organization
PD-7	Hydrogen Generation from Biomass-Derived Carbohydrates via the Aqueous-Phase Reforming (APR) Process	Randy Cortright	Virent Energy Systems
PD-13	Low Cost Hydrogen Production from Biomass Using Novel Membrane Gasification Reactor	Francis Lau	GTI
PD-14	A Novel Slurry-Based Biomass Reforming Process	Tom Vanderspurt	United Technology Research Center
PD-34	Reversible Liquid Carriers for an Integrated Production, Storage & Delivery of Hydrogen	Guido Pez	Air Products
PD-35	Materials Solutions for Hydrogen Delivery in Pipelines	Subodh K. Das	Secat, Inc.
PDP-24	A Novel Membrane Reactor for Hydrogen Production from Coal	Francis Lau	GTI
PDP-42	System Design and New Materials for Reversible Solid-Oxide, High-Temperature Steam Electrolysis	James Ruud	GE Global Research
PDP-46	Combined Reverse-Brayton Joule-Thompson Hydrogen Liquefaction Cycle	Martin Shimko	Gas Equipment Engineering Corporation
PDP-47	Active Magnetic Regenerative Liquefier (AMRL) Development	Robert Thompson	New Concepts Research Corporation
PDP-48	Hydrogen Embrittlement of Pipeline Steels: Causes & Remediation	Petros Sofronis	University of Illinois

APPENDIX C: FEEDBACK AND RECOMMENDATIONS

These notes summarize the comments received from various participants at the May 23-26, 2005 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: Inputs from interviews/meetings with DOE Technology Development Managers and Research Managers during the week following the Review.

Section 3: Scores and summarized answers to questions from the Review Questionnaire, filled out by approximately 110 of the participants.

Section 1 – Peer Reviewer Comments

Review Structure

- The Review could benefit from being divided up into smaller gatherings instead of a huge gathering.
- Utilize a panel of presenters and allow dialogue among them— similar to a Q&A; address key questions across the program.

Peer Review Process

- It would be helpful not to have to review two presentations in a row—could finish up notes during following presentation.
- Electronic form— the updated version froze when saved; or would not save. Did not like the impact of the macros.
- Liked the advanced web access to PowerPoint presentations—allowed Reviewers to bring a binder with particular presentations to review and review forms.
- Computers for Reviewers are not attractive right in front of screen, and you cannot take the computer with you afterward to finish comments. Maybe have more computers in the Peer Reviewer Room and be able to check them out. Or be able to check out and use anywhere in the session room. Have the Peer Reviewer Room open more hours.
- The Reviewer presentation load --10 presentations (5 were posters) -- was okay and scattered among groups/subprogram was not a bad thing.
- Some Reviewers did not receive pre-Review coordination e-mails from H2review@sentech.org because of spam filters.
- It was very helpful to have PDF versions of presentations available before the AMR. Much better than being given huge binders of hardcopies.
- It was great to have power strips available for laptop users.
- Electronic Evaluation forms – idea is good, but:
  - Forms were cumbersome and should be without the macros. Electronic evaluation forms should be in Excel.

  - They lost everything when you tried to save without a project number entered.
  - Should have a spell-check function.
  - The questions do not fit/apply to all types of projects. Some reviewers did not realize they could enter N/A.
  - Need a finer scale than whole numbers from 1 to 4.
- Should have detailed written descriptions of each project available to Reviewers.
- Difficult to get everything done – especially the posters. A lot of work for reviewers to do.
- Posters take a lot of time to review. First you need to spend an appropriate amount of time at the posters and with the PIs and then, later on, you need to spend time putting your evaluations down on hardcopy or softcopy.
- It was unclear why some reviewers were assigned to some areas.

Presentation Content

- Some PIs recycled the exact same slides from 2004. This makes it difficult to evaluate progress that has been made.
- PI Presenters – Not always clear what they do exactly and how it relates to the goals & aspects being presented.
- Acknowledgements should be at the back of the presentation, not at the front. PIs spend too much time on this fluff at the beginning, causing them to run out of time.
- PIs need to practice/rehearse their presentations beforehand so they do not run out of time leading to a low evaluation score. Presenters from companies do a much better job of being prepared/rehearsed than academic presenters.
- All so well done that it is difficult to give constructive criticism. PIs did a good job adhering to the template.
- Sometimes the tie of the project to program goals was unclear.

Poster Presentations

- PIs were not always with their posters; one reviewer went to a particular poster twice and PI was not there either time.
- Posters better this year, enjoyed experience more—there was more physical space between posters.
- Posters were put up slightly before and after the oral presentations; wish posters were up longer.
- Should be more time for reviewers to talk to PIs presenting posters. Posters should be available for a longer period of time. Difficult having them during the oral sessions.
- Some were disappointing in terms of what they were doing and accomplishments. These were primarily the Congressional/earmark posters.

Production/Delivery

- Context of Production projects should be on volume [i.e. capacity, kg per day] of hydrogen generated. How much hydrogen can we really expect from the particular projects? Would like results/impacts quantified. PIs should relate their project to the DOE/HFCIT goals. What do they want/expect this project to contribute to? Focus on the real goal—not just produce at a particular price but at a particular volume and price. Everyone in the project should be focused on the overall goal.

- Volume of hydrogen produced is important. A lot of the presentations were geared at the price. Need a different emphasis.
- Concerning the quality of projects— it appeared to be the same for some, improved for others, but none have gone down though. Number of projects has gone up though.
- Seems like a significant number of projects that were presented orally had not even started. How can reviewers review the progress of projects that have not started yet? What kind of information are we looking for? Maybe differentiate between reviews of current projects and upcoming project -- either not review or have different questions.
- Graphs are often hard to follow with all the different variants—maybe show what it all means, rather than simply presenting results in graphs.
- One project was confusing because it got funding from outside the DOE program. But, it appears to support the program, which is okay. It is a project in progress and won 5 awards; so how do we review it?
- Why can't we strip hydrocarbons (carbon hydrogen bonds) off coal on the way to power plant? Heat it up and it gives off more hydrogen/methane.
- Felt like there were too many projects -- the difference between last year and this year was noticeable. There was more discussion after presentations because presenters were cut off during their presentations.

Systems Analysis

- Difficult to follow the format with analysis; the evaluation form and the project template does not lend itself to the subject.
- Need to be able to connect the analysis projects with validation activities.
- Concern of overlapping of analysis projects—need to compare, what overlaps, and the conclusions.

Technology Validation

- During the Fleet projects it may be difficult to get the feedback/data on why fuel cells/systems go down and what part of the plan was affected. This area needs more emphasis – little detail on this was provided in presentations.
- The vehicle demos are great – the data collection they are doing is very important. One project was disappointing because details were sparse.
- It was surprising how many stationary power park projects there are. People need to be more aware of these projects – the program is often viewed as being transportation heavy.
- NREL data center –
  - There is skepticism about getting data that is "spin-free."
  - Also a concern that universities not involved in the program may have the solution to a problem, but not realize it because they are not involved in the program/unaware of a vehicle fleet problem.
  - Further concern that other programs that could benefit from the information will not be able to because of the proprietary information protection concerns of the data center.

Safety/Codes & Standards

- SC&S Reviewers felt that most details, from the papers presented, the project contents, the quality of the research, and the pre-planning activities, were very well done.

- Reviewers cited a few problems with the electronic review forms and the built-in macros. Several reviewers identified other possible approaches that could be used to steer away from the use of the macros and the software compatibility problems that resulted.
- The SC&S Reviewers agreed with the effort to capture the comments electronically.

Education

- Many of the presentations were related and/or overlapping. This was confusing, and some of the presentations needed to do a better job relating to the hydrogen program.
- The presentation template and review forms seemed to be designed for R&D rather than education. Maybe have different forms. Format for presentations should be different for Education. (i.e. the safety slide does not fit the topic very well; maybe have a different slide instead).
- Would like to see a slide geared more toward a curriculum chart with dates/timeline.
- Maybe break up the Review (as a whole) so can have more slots for categories like Education.

Section 2 – Comments from DOE Personnel

General

- Some people [the attendees?] felt they lost perspective of the meeting. They liked the meeting but felt they were at a conference versus a peer review.
- Turn off the sleep function on the presenter's PC, especially when a speaker is not using the PC.
- One of my PIs was a little taken aback to get a random phone call about his presentation being available on the web (to Reviewers) before he even got to the Review. [Check security of website for presentations that are available to the reviewers prior to the meeting.]
- Make the agenda less confusing -- organize it by days and have overviews of each day.
- Cut down on the concurrent side bar meetings:
  - No corporate or association meetings.
  - Tech Team meetings must be after hours or a time to not interfere with the Review.

Peer Reviewers

- It is critical to "test-drive" the electronic version of the Reviewer form before sending out. Also, the e-form need not "look-like" the paper copy. Comments were cut off because there was not room in the boxes. This should not happen in a proper e-form.
- The Reviewers found the format of trying to evaluate and listen difficult. Some of the reviewers felt they did not give the projects a fair evaluation since they were trying to type and listen at the same time.
- There was also some complaining about the number of evaluations that some Reviewers were assigned.
- When scheduling Reviewers, try not to have the same person review back-to-back projects. It was difficult for some Reviewers to finish writing out their comments before the next PI stepped up to the plate.

- The process for assigning reviewers left much to be desired.
- Look at trying to include a couple other review criteria:
  - Cost – what it would cost to utilize the technology of the project?
  - Impact – how the project's result would impact the hydrogen infrastructure (i.e., if this biomass-to-hydrogen technology were developed, how many million tons of hydrogen could it produce per year?).
- The electronic evaluation form does not have to look like the hard copy – i.e., use a spreadsheet or something to make macros easier.

Presentations

- I got a lot of comments on the length of the presentations and not enough time to ask questions. Even though we stayed within the time allotments, we had to cut off some of the questions. Also, the Reviewers mentioned they did not have enough time for all their questions.
- It was suggested to have everyone present at one time and then have a panel discussion with written questions and verbal questions from the audience.
- Storage had the speakers come up afterwards for additional informal Q&A time with Reviewers [not always possible and cuts into the breaks].
- Consider tweaking the presentation template to better reflect the nature of non-R&D projects; also consider tailoring the review forms for non-R&D projects.
- The circular logic used in the project presentations is a problem. For example, "Fl loss causes membrane degradation. I found Fl loss; therefore the membrane must have degraded." It was more prevalent in the lab presentations. I think this circular logic is an artifact of the presentation format. We do not ask them to explain their experimental results.
- Industry does not even try to state conclusions.
- I find most presentations are truly lacking in stimulation. We were taught in our very first science lab classes that experimentation requires going past reporting results -- the "what happened" -- and taking a guess at answering the "why."

Posters

- Do not have the poster sessions at the same time as the oral presentations. I ended up missing both posters and orals that I wanted to see. In the past, the poster session has been held during the evening reception.
- People wanted to see the posters but could not because they were attending the session. Also, people would move in and out of the meeting to see the posters; this was disruptive to some of the speakers.
- I heard complaints about having the poster sessions at the same time as the oral sessions, since it required some of the Reviewers (of posters) to miss some of the oral presentations in the same technical areas.
- Have a separate room/ballroom, and not at same time as Orals, for that Program Element.

Logistics

- In the Systems Analysis and Storage sessions, there was no one to assist. It would be good to have either someone from the hotel, QSS, and/or another contractor to assist with microphones, questions, etc.

- The meetings/sessions were very organized from the standpoint of having all the presentations loaded and available, which helped to keep the meeting continuity.
- We need a better time-warning system for speakers; it will be helpful if it is more obvious to the speakers. It was in the middle of the room, while most of the time the speaker looks toward the direction of the screen.

Hotel/Food

- The reception, the continental breakfast, breaks and lunch were very good.
- The hotel was great -- very adequate for our needs; but, it may be too small as the meeting gets larger.
- The people I talked with said it was well run and they benefited by attending.
- I thought the facilities at the hotel were great and that everything ran smoothly.

Fees

- The one-day registration fee is a good idea. Could there be a student rate as well?
- Maybe we should have separate tickets/fees for receptions? That might help keep the overall registration cost down.

Section 3 – Review Feedback Questionnaire Responses

DEMOGRAPHIC QUESTIONS

1a. What was your role in the review?

28 Peer Reviewer (please answer questions in Sections A. and B.)
44 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.)
30 Attendee, neither Reviewer nor Presenter (please answer questions in Section A. only)
12 Reviewer and Presenter

1b. What is your affiliation?

6 Government agency directly sponsoring the program under review
34 National/government lab, private-sector or university researcher whose project is under review
30 In an industry directly involved in the program under review
12 In an industry with interest in the work under review
3 Government agency with interest in the work
11 National/government lab, private-sector or university researcher not being reviewed, but who has an interest in the work
11 Other (please specify, e.g., consultant, retired employee, public, etc.):
(Answers: Oversight & Advisory, Retired National Lab, Consulting Firm, Consultant, Consultant, Foreign Institution, Contractor)

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*
				4.2		
4.	Sub-program overviews were helpful to understanding the research objectives.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				4.2		
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*
				3.9		
	b. Question & Answer periods	*1*	*2*	*3*	*4*	*5*
				3.3		
	c. Answers provided concerning programmatic questions	*1*	*2*	*3*	*4*	*5*
				3.5		
	d. Answers provided concerning technical questions	*1*	*2*	*3*	*4*	*5*
				3.6		
6.	Enough time was allocated for presentations.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				3.8		
7.	Time allowed for the Question & Answer period following the presentations was adequate for a rigorous exchange.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				3.0		
8.	The questions asked by reviewers were sufficiently rigorous and detailed.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				3.0		
9.	There were no problems with:	*disagree*				*agree*
	Groupings of projects by technical area	*1*	*2*	*3*	*4*	*5*
				4.3		
	Proprietary data (should not be any at this Review)	*1*	*2*	*3*	*4*	*5*
				4.4		
	Quantity/level of the information presented	*1*	*2*	*3*	*4*	*5*
				3.7		
10.	The review was conducted smoothly.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				4.3		
11.	The frequency (once per year) of this formal review process for this Program is:	**102** - about right **9** - too frequent **0** - not often enough **2** - do not know				

12.	Logistics and amenities were satisfactory.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				4.3		
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*
				4.1		
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 following bullets summarize the numerous responses received for Question 17:

- **Interactions** – opportunity to meet and talk to members of the hydrogen community, DOE program managers, principal investigators, etc
- **Information Exchange** – the wealth of information that is exchanged across the community, from top-level plans down to the details of individual projects
- **Subprogram Structure**– arranging the agenda and tracks by subprogram, allowing the attendees to see the depth and breadth of technology focus areas in a structured way
- **Overviews** – having the Team Leads provide an overview/kickoff briefing at the start of each track, to put the entire subprogram into context
- **Formatted Presentations** – requiring the PIs to adhere to a fixed format for the oral and poster presentations made the review more productive and easier to follow
- **Questions & Answers** – ability to ask questions of the PIs during the oral presentations, and to interact one-on-one with them during poster sessions
- **Posters** – these give even the small and start-up projects a chance to present their status and plans
- **CD/Attendees/Information** – receiving softcopies of all the presentations, lists of attendees, and other information

18. What could have been done better?

The following bullets summarize the numerous responses received for Question 18:

- **Agenda** – was very confusing and not organized in a way to make it easy to understand what was ahead and how to locate it; abstracts of the projects would be helpful in determining attendance at orals/posters

- **Plenary** – did not add much to the overall review; if retained, it should be more focused on the peer and merit review purpose of the meeting

- **Advance Information** – although the CD provided at registration was good, having more information about the projects, logistics, etc. prior to the Review (e.g., through the website) is needed

- **Posters** – do not have them at the same time as that subprogram's oral session, not outside the doors of the orals rooms (noise/disturbance issues), and allow more time for review and discussion with the poster PIs

- **Questions & Answers** – there was not enough good, quality time for Q&A

- **Presentation Content** – force the Reviewers to stick to the topics they are supposed to address; do not allow "advertising" of their products or organizations; make every project show a chart that compares their plans coming into the year with their actual accomplishments

- **Time** – there is just not enough time to adequately cover a couple hundred projects; do not review every project every year – not necessary; individual projects need more time for good presentations and adequate Q&A

- **Schedule** – there needs to be enforcement of the schedule; in some sessions PIs ran over their time and no one tried to manage or correct the situation

- **Reviewers/Process** – the Review is too large to do an adequate peer review; need to re-think the size, content, and structure of the review process; the automated forms are a good idea, but the macros caused numerous problems

- **Slides/Graphics** – for the oral presentations, some standards and QA need to be enforced to ensure that slides are not too busy, can be read throughout the room, etc

- **Logistics** – session rooms need to be closer together; need better signs to lead attendees to various sessions/activities; should have a map/floorplan in the registration packet; audio could be improved; screens need to be raised to allow everyone to see the bottom portion

19. Overall, how satisfied are you with the review process?

very unsatisfied				very satisfied
1	*2*	*3*	*4*	*5*
		4.0		

20. Would you recommend this review process to others and should it be applied to other DOE programs? *Yes* - ***86*** *No* - ***8***

21. Please provide comments and recommendations on the overall review process.

The following bullets summarize the numerous responses received for Question 21:

- **Overall** – the review has grown too large and needs to be reduced or divided into two; do not review/present small projects every year; have earmark projects only do posters, not orals
- **Time** – need more time for individual projects, both for the Reviewers and for the general audience
- **Presentation Content** – do not require the programmatic "boiler plate" as it is not critical to a good technical review; concentrate on accomplishments; eliminate the barriers-addressed slide; only list/discuss the important/key collaborations
- **Posters** – move away from the oral session rooms and/or hold at a different time (e.g., in the evenings); have better lighting and more physical space around the poster boards
- **Review Process** – use a panel of 8-10 Reviewers for each subprogram area; take more care in selecting quality Reviewers; do not have back-to-back reviews, so a Reviewer has time to write up his/her comments after one finishes; come up with a better way to review the posters; the review process is too "casual" since Reviewers do not get paid and must review too many projects
- **Agenda** – provide a clear, day-by-day agenda
- **Evaluation Form –** dry-run the automated form more thoroughly before the Review

B. QUESTIONS 22 THROUGH 32 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*
		3.5		

23. Review instructions were provided in a timely manner.

disagree				agree
1	*2*	*3*	*4*	*5*
		4.0		

24. The information provided in the presentations was adequate for a meaningful review of the projects.

disagree				agree
1	*2*	*3*	*4*	*5*
		3.5		

25.	The evaluation criteria upon which the review was organized were clear and used appropriately.	*disagree*				*agree*
	1. Relevance	*1*	*2*	*3*	*4*	*5*
				3.8		
	2. Approach	*1*	*2*	*3*	*4*	*5*
				4.1		
	3. Technical Accomplishments and Progress	*1*	*2*	*3*	*4*	*5*
				4.2		
	4. Technology Transfer/Collaboration	*1*	*2*	*3*	*4*	*5*
				3.7		
	5. Proposed Future Research	*1*	*2*	*3*	*4*	*5*
				4.1		
26.	Explanation of the questions within the criteria was clear and sufficient.	*disagree*				*agree*
	1. Relevance	*1*	*2*	*3*	*4*	*5*
				4.0		
	2. Approach	*1*	*2*	*3*	*4*	*5*
				4.1		
	3. Technical Accomplishments and Progress	*1*	*2*	*3*	*4*	*5*
				4.2		
	4. Technology Transfer/Collaboration	*1*	*2*	*3*	*4*	*5*
				4.0		
	5. Proposed Future Research	*1*	*2*	*3*	*4*	*5*
				4.1		
27.	The right criteria and weightings were used to evaluate the project(s)/program.	*disagree*				*agree*
	1. Relevance	*1*	*2*	*3*	*4*	*5*
				3.9		
	2. Approach	*1*	*2*	*3*	*4*	*5*
				4.1		
	3. Technical Accomplishments and Progress	*1*	*2*	*3*	*4*	*5*
				4.1		
	4. Technology Transfer/Collaboration	*1*	*2*	*3*	*4*	*5*
				3.9		
	5. Proposed Future Research	*1*	*2*	*3*	*4*	*5*
				4.1		
28.	During the review, reviewers had adequate access to the Principal Investigators.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				3.8		
29.	Information on the location and timing of the projects was adequate and easy to find.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				4.0		
30.	The number of projects I was expected to review was	**6 --** Too many				
		2 -- Too few				
		29 -- About right				

31.	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* ***4.3*** *Don't know their Credentials -* ***24***
32.	Altogether, the preparatory materials, presentations, and the Question & Answer period provided sufficient depth for a meaningful review.	*disagree* *agree* *1 2 3 4 5* ***3.6***

C. QUESTIONS 33 THROUGH 43 FOR PRESENTERS ONLY

33.	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* ***4.3***
34.	Instructions for preparing the presentation were sufficient.	*disagree* *agree* *1 2 3 4 5* ***4.5***
35.	The template for the presentation was helpful.	*disagree* *agree* *1 2 3 4 5* ***4.3***
36.	The PDF format provided adequate functionality for my presentation.	*disagree* *agree* *1 2 3 4 5* ***4.2***
37.	The time limit for my presentation was adequate to present the information needed by reviewers.	*disagree* *agree* *1 2 3 4 5* ***4.2***
38.	The audio and visual equipment worked properly and were adequate.	*disagree* *agree* *1 2 3 4 5* ***4.3***
39.	The evaluation criteria upon which the review was organized were clearly defined and used appropriately	*disagree* *agree*
	1. Relevance	*1 2 3 4 5* ***4.2***
	2. Approach	*1 2 3 4 5* ***4.2***
	3. Technical Accomplishments and Progress	*1 2 3 4 5* ***4.3***
	4. Technology Transfer/Collaboration	*1 2 3 4 5* ***3.9***
	5. *Proposed Future Research*	*1 2 3 4 5* ***4.2***

40.	Explanation of the questions within the criteria was clear and sufficient.	*disagree*				*agree*
	1. Relevance	*1*	*2*	*3*	*4*	*5*
				4.2		
	2. Approach	*1*	*2*	*3*	*4*	*5*
				4.2		
	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*
				4.3		
41.	The right criteria and weightings were used to evaluate the project(s)/program.	*disagree*				*agree*
	1. Relevance	*1*	*2*	*3*	*4*	*5*
				3.9		
	2. Approach	*1*	*2*	*3*	*4*	*5*
				4.1		
	3. Technical Accomplishments and Progress	*1*	*2*	*3*	*4*	*5*
				4.1		
	4. Technology Transfer/Collaboration	*1*	*2*	*3*	*4*	*5*
				3.8		
	5. *Proposed Future Research*	*1*	*2*	*3*	*4*	*5*
				4.0		
42.	During the review, reviewers had adequate access to the Principal Investigators.	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				3.9		
43.	Altogether, the preparatory materials, presentations, and the Question & Answer period provided sufficient depth of review	*disagree*				*agree*
		1	*2*	*3*	*4*	*5*
				3.9		

May, 2005

DOE Hydrogen Program
2005 Annual Program Review
Project Evaluation Form

Project No.

Reviewer Name: _____

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 –degree to which the project supports the President's Hydrogen Fuel Initiative and the goals and objectives of the DOE Multi-Year RD&D plans. Weight **(20%)**

4-Outstanding. The project is critical to realization of the President's Hydrogen Fuel Initiative and fully supports the RD&D plan objectives		Specific Comments:
3-Good. Most aspects of the project align with the President's vision the RD&D plan objectives.		
2-Fair. The project partially supports the President's hydrogen vision the RD&D plan objectives.		
1.-Poor. The project provides little support to the President's vision 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%)**

4-Outstanding. The project is sharply focused on one or more key technical barriers to development of the hydrogen or fuel cell technologies. Difficult for the approach to be improved significantly.		Specific Comments:
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%)**

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.		Specific Comments:
3-Good. The project has shown significant progress toward against its objectives and to overcoming one or more technical barriers.		
2-Fair. The project has shown modest progress in overcoming barriers, and 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%)**

4-Outstanding. Close coordination with other institutions is in place and approporiate; partners are full participants.		Specific Comments:
3-Good. Some coordination exists; full and needed coordination could be accomplished fairly easily.		
2-Fair. A little coordination exists; full and needed coordinatiion would take significant time and effort to initiate.		
1.-Poor. Most all 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%)**

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.		Specific Comments:
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

May, 2005

DOE Hydrogen Program
2005 Annual Program Review

Team Lead Briefing Evaluation Form

Session: Mon ☐ Tue ☐ Wed ☐ Thu ☐ a.m. ☐ p.m. ☐ **Reviewer**: _____

Title of Sub-Program: _____ **Project No.** ☐

Team Lead's 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. --

6. **Degree to which the Sub-Program area was adequately covered and/or summarized:**

7. **Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?:**

8. **Does the Sub-Program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?**:

9. **Other comments:**

Page 1 of 1

REPORT DOCUMENTATION PAGE

Form Approved
OMB No. 0704-0188

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13. SUPPLEMENTARY NOTES

14. ABSTRACT *(Maximum 200 Words)*

This report summarizes comments from the Peer Review Panel at the FY 2005 DOE Hydrogen Program Annual Merit Review, held on May 23-26, 2005, 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

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