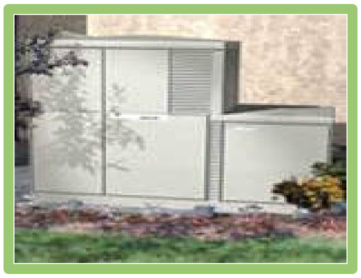
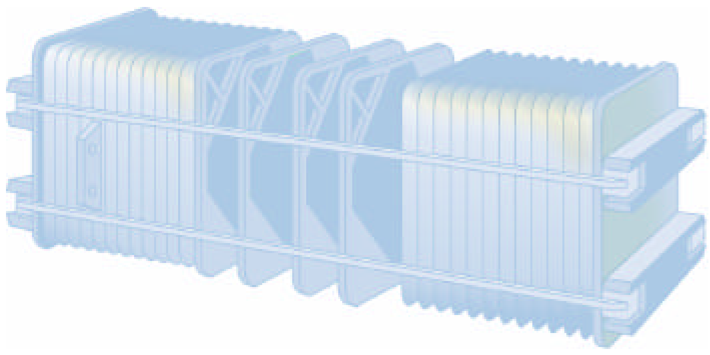


Hydrogen, Fuel Cells & Infrastructure Technologies Program

FY2003 Merit Review & Peer Evaluation Report

May 19-22, 2003
Berkeley, California

October 2003



U.S. Department of Energy
Energy Efficiency and Renewable Energy
Bringing you a prosperous future where energy is clean, abundant, reliable and affordable

Dear Colleague:

This document summarizes the comments provided by the Merit Review Panel at the U.S. DOE Hydrogen, Fuel Cells & Infrastructure Technologies Program (HFCIT) FY 2003 Merit Review and Peer Evaluation, held on May 19-22, 2003 in Berkeley, California. The recommendations of the panel have been taken into consideration by DOE technology development managers in the development of work plans for fiscal year (FY) 2004.

The tables below list the projects discussed at the review and the major actions to be taken during the upcoming fiscal year. The projects have been grouped according to which activity area in the HFCIT Multi-Year Research, Development and Demonstration Plan they support. The average scores are on a 4-point scale. To furnish all principal investigators (PIs) with direct feedback, raw evaluations and comments were provided to each presenter. However, the authors of the individual comments will remain anonymous. The PIs of each project are instructed to fully consider these summary evaluation comments, as appropriate, into their FY 2004 plans.

Hydrogen Production & Delivery: **FUNDING**

Project Number	Project, Performing Organization	Avg. Score	Con-tinued	Discon-tinued	Project Completed	Summary Comment
1	*H_2 from Biomass: Catalytic Reforming of Pyrolysis Vapors*, NREL	3.28	v			Focus on fast fluidized bed process.
2	*H_2 from Post-Consumer Residues*, NREL	3.16	v			Focus on circulating bed system.
3	*Fluidizable Reforming Catalysts*, NREL	3.40	v			Accelerate screening and testing of catalyst compositions.
4	*Biohydrogen Production from Renewable Organic Wastes*, ISU	2.80	v			
5	*Biological Water Gas Shift*, NREL	3.23		v		Project funding discontinued based on unfavorable economic analysis.
6	*Thermocatalytic CO_2-free Production of H_2 from HC Fuels*, Florida Solar Energy Center	2.83			v	
7	*Novel Catalytic Fuel Reforming Using Micro-Technology with Advanced Separations Technology*, InnovaTek	2.72	v			Emphasize development of beta prototype. Project will be completed in FY04.
8	*Engineering Development of Ceramic Membrane Reactor Systems for Converting Natural Gas to Hydrogen & Syn Gas for Liquid Transportation Fuels*, Air Products & Chemicals Inc.	3.40	v			Emphasize higher pressure and higher temperature testing.
9	*Integrated Ceramic Membrane System for H_2 Production*, Praxair	2.67	v			Focus on developing better modeling.

10	*Low Cost H_2 Production Platform*, Praxair	2.95	v			Emphasize collaboration.
11	*Defect-free Thin Film Membranes for H_2 Separation & Isolation*, SNL	2.87	v			
12	*Maximizing Photosynthetic Efficiencies and H_2 Production in Microalgal Cultures*, UC Berkeley	3.33	v			Focus on program RD&D goals for 2005.
13	*Reformer Model Development for Hydrogen Production*, JPL	2.27		v		Model analysis in this area is no longer a program requirement.
14	*Photoelectrochemical H_2 Production*, University of Hawaii	3.30	v			Emphasize further development of multi-junction photoelectrodes to meet program RD&D goals for 2005.
15	*Photoelectrochemical Water Splitting*, NREL	3.23	v			Focus on candidate light absorbing materials.
16	*Encapsulated Metal Hydride for H_2 Separation*, SRTC	2.83			v	
17	*Economic Comparison of Renewable Sources for Vehicular Hydrogen in 2040*, DTI	2.90			v	
18	*Biomass-Derived H_2 from a Thermally Ballasted Gasifier*, Iowa State University	2.70	v			
20	*Evaluation of Protected Metal Hydride Slurries in a H_2 Mini-Grid*, TIAX	3.20			v	
22	*Novel Compression and Fueling Apparatus to Meet Hydrogen Vehicle Range Requirements*, Air Products & Chemicals Inc.	3.20	v			
30	*Techno-Economic Analysis of H_2 Production by Gasification of Biomass*, GTI	2.60			v	Project completed.
31	*Supercritical Water Partial Oxidation*, GA	2.57		v		Unlikely that cost barrier can be overcome.
32	*Development of Efficient and Robust Algal Hydrogen Production Systems*, ORNL	3.47	v			Focus on designing new DNA sequence coding for proton channel.
34	*Water-Gas Shift Membrane Reactor Studies*, University of Pittsburgh	2.90	v			Emphasize feasibility of hi-temp water-gas shift under realistic operating conditions.
38	*Low Cost, High Efficiency Reversible FC Systems*, Technology Management Inc.	2.80		v		High electrical input requirement prevents overcoming energy efficiency barrier.
39	*High-Efficiency Steam Electrolyzer*, LLNL	2.37		v		Carbon deposition at anode is a recurring problem.

40	*High Temperature Solid Oxide Electrolyzer System*, INEEL	2.80	v			Emphasize collaboration with industry.
41	*Photoelectrochemical H_2 Production Using New Combinatorial Chemistry Derived Materials*, UC Santa Barbara	3.13	v			Emphasize combinatorial system development, library design and photocatalyst analysis.
42	*Algal Hydrogen Photoproduction*, NREL	3.23	v			Emphasize engineering oxygen tolerant hydrogenase and temporally separated system.
43	*Discovery of Photocatalysts for H_2 Production*, SRI International	3.13	v			Emphasize improving material testing systems, synthesizing materials and processing materials data.
49	*Advanced Thermal Hydrogen Compression*, Ergenics Inc.	3.04			v	Project was fully funded and will be completed in FY04
59	*Development of a Turnkey H_2 Fueling Station,* Air Products & Chemicals Inc.	3.27	v			Emphasize technical feasibility and cost reduction.
60	*Autothermal Cyclic Reforming-Based H_2 Generating and Dispensing System,* GE Energy	3.07	v			Emphasize collaboration.
63	*H_2 Reformer, FC Power Plant, & Vehicle Refueling System,* Air Products & Chemicals Inc.	3.40	v			Install 2^{nd} generation fuel cell and operate facility.
64	*Candidate Fuels for Vehicular Fuel Cell Power Systems: Stakeholder Risk Analysis,* TIAX	3.27	v			Emphasize coordinating with H2A Group.
111	*Fuel Cell Distributed Power Package Unit: Fuel Processing Based on Autothermal Cyclic Reforming*, GE	2.67			v	

Hydrogen Storage:

Project Number	Project, Performing Organization	Avg. Score	Con-tinued	Discon-tinued	Project Completed	Summary Comment
44	*DOE H_2 Composite Tank Program,* Quantum Technologies Inc.	3.10	v			Focus on reducing cost and meeting DOE performance targets.
45	*Development of a Compressed H_2 Gas Integrated Storage System,* Johns Hopkins University/APL	2.81			v	Project completed.
46	*H_2 Storage Using Lightweight Tanks,* LLNL	2.52	v			Focus on conformable tanks.
47	*Insulated Pressure Vessels for Vehicular Hydrogen Storage*, LLNL	2.60	v			Focus on meeting DOE targets.

48	*Low Permeation Liner for H_2 Gas Storage Tanks*, INEEL	2.75		v		Project funding terminated pending further review of approach.
50	*Catalytically Enhanced H_2 Storage System*, University of Hawaii	3.53	v			Focus on understanding and improving $NaAlH_4$-based material.
51	*Hydride Development for Hydrogen Storage*, SNL	3.30	v			Project funding increased. Extend research beyond $NaAlH_4$.
52	*Hydrogen Storage Using Complex Hydrides*, Florida Solar Energy Center	2.20		v		Project funding terminated due to poor review.
53	*High-Density H_2 Storage Demonstration Using $NaAlH_4$-Based Complex Compound Hydrides*, UTRC	3.37	v			Focus on engineering of prototype subscale system.
54	*Standardized Testing Program for Emergent Chemical Hydride & Carbon Storage Technologies*, SwRI	2.70	v			Project funding accelerated to provide testing capability by end of 2004.
55	*H_2 Storage in Carbon Nanotubes*, NREL	2.90	v			Project funding increased. Address reproducibility issue and planned 2005 go/no-go decision.
56	*Doped Carbon Nanotubes for H_2 Storage*, Westinghouse Savannah River Tech Center	2.43			v	
57	*H_2 Storage in Metal-Modified Single-Wall Carbon Nanotubes*, Caltech	2.71			v	
58	*Hydrogen Storage via Ammonia and Aminoborane*, Florida Solar Energy Center	2.48			v	

Fuel Cells:

Project Number	Project, Performing Organization	Avg. Score	Con-tinued	Discon-tinued	Project Completed	Summary Comment
67	*Study of Fuel Cell Water Transport with Neutron Imaging*, NIST	3.35	v			Project funding increased. Increase dissemination of results to community.
70	*Integrated Manufacturing for Advanced Membrane Electrode Assemblies*, DeNora	3.37	v			
71	*Development Of High-Temperature Membranes & Improved Cathode Catalysts*, UTC	3.57	v			
72	*Advanced MEA's for Enhanced Operating Conditions*, 3M	3.28	v			

73	*R&D on an Ultra-Thin Composite Membranes for High Temperature Operation in PEMFCs*, Fuel Cell Energy	2.40			v	
74	*Development Of High-Performance, Low-Pt Cathodes Containing New Catalysts & Layer Structure*, Superior MicroPowders	2.93	v			
75	*Design & Installation of a Pilot Plant for High-Volume Electrode Production*, SwRI	2.60			v	
76	*Scale-Up of Carbon/Carbon Composite Bipolar Plates*, Porvair Corp.	3.47	v			
77	*High Temperature Polymer Membranes for Fuel Cells*, Case Western Reserve University	2.83	v			Focus on 120°C operation.
78	*Electrodes for PEMFC Operation on H_2 & Reformate*, LANL	2.84	v			Emphasize new catalyst materials and impurity tolerance.
79	*New Electrocatalysts for FCs*, LBNL	3.36	v			Project funding increased. Continue catalyst research, begin screening studies of non-PGM catalyst.
80	*Low-Platinum Hydrous Metal Oxides for PEMFC Cathodes*, NRL	2.91	v			Project funding increased. Focus on doped metal oxide catalyst supports.
81	*Low-Platinum Loading Electrocatalysts*, Brookhaven National Laboratory	3.20	v			Project funding increased. Focus on cathode catalysts.
82	*Microstructural Characterization of PEM Fuel Cells*, ORNL	2.36	v			Develop correlation between performance degradation and structural changes.
83	*Bipolar Plate-Supported SOFC "TuffCell"*, ANL	2.60	v			Developing short stack.
84	*Coatings for Fuel Cell Air Compressors*, ANL	2.78	v			
85	*Carbon Composite Bipolar Plate for PEM Fuel Cells*, ORNL	2.89			v	
86	*Cost-Effective Surface Modification for Metallic Bipolar Plates*, ORNL	2.56	v			Project funding increased. Initiate technology transfer to fuel cell suppliers.
87	*Carbon Foam for Fuel Cell Humidification*, ORNL	1.93		v		Project terminated since other approaches to fuel cell humidification appear to be more cost effective.
89	*Sulfur Removal from Reformate*, ANL	2.70		v		Project halted pending Go/No-go decision.

90	*Assessment of FCs as Auxiliary Power Systems for Transportation Vehicles*, TIAX	2.52			v	
91	*Fuel Cell Reformer Emissions*, TIAX	3.07			v	Project fully funded and to be completed in FY04.
92	*Fuel Processing of Diesel Fuel For APUs*, NETL	2.60			v	
94	*Fiber-Optic Temperature Sensor for PEM Fuel Cells Monitoring*, ORNL	2.95	v			Increase collaboration with fuel cell developers.
95	*Selective Catalytic Oxidation of Hydrogen Sulfide*, ORNL	2.87	v			Develop a continuous desulfurization process.
96	*SOFC Auxiliary Power Units for Long-Haul Trucks: Modeling and Control*, PNNL	2.48	v			Increase collaboration with SECA Program.
99	*Diesel Reforming*, ANL	2.33		v		Project funding terminated in favor of higher priority R&D.
100	*Fast Start Reformer Components*, LANL, ORNL, and PNNL	2.60	v			Project funding decreased in anticipation of completion.
101	*Fuel Cell Systems Analysis*, ANL	3.30	v			Explore hybridization and direct hydrogen fuel system cycle.
102	*Fuel Cell Vehicle Systems Analysis*, NREL	3.17	v			Project funding continued. Address thermal and water management issues.
103	*Cost Analyses of Fuel Cell Stacks/Systems*, TIAX	3.30	v			Project funding continued. Analyze cost of direct hydrogen FCV.
104	*Precious Metal Availability & Cost Analysis for PEMFC Commercialization*, TIAX	2.93			v	Final Report due.
105	*DFMA Cost Estimate of FC/Reformer System at Low, Medium, & High Production Rates*, DTI	3.00			v	
106	*Water-Gas Shift Catalysis*, ANL	2.90	v			Project funding decreased pending Go/No-go decision. Continue work on Pt-Ce and Cu catalysts.

107	*Catalysts for Autothermal Reforming*, ANL	3.00	v			Project funding decreased pending Go/No-go decision. Work to decrease precious metal loading while improving catalyst activity and sulfur tolerance.
108	*Development of WGS Membrane Reactor*, Ohio State University	3.00	v			Develop prototype membrane reactor. Efficiency analysis initiated to guide decision for program continuation.
109	*OnBoard Vehicle, Cost Effective Hydrogen Enhancement Technology for Transportation PEMFCs*, UTRC	1.90	v			Develop prototype membrane reactor. Efficiency analysis initiated to determine whether to continue funding.
110	*Advanced High Efficiency Quick Start Fuel Processors for Transportation Applications*, Nuvera	3.00	v			
112	*Plate-Based Fuel Processing System*, Catalytica	2.97	v			
113	*Quick-Starting Fuel Processors*, ANL	3.26	v			
114	*Progress in Microchannel Steam Reformation of Hydrocarbon Fuels*, PNNL	2.79	v			Project funding decreased pending Go/No-go decision. Incorporate improved, less expensive catalyst, initiate testing of fast-start, low dP system.
115	*Reformate Clean-Up: The Case for Microchannel Architecture*, PNNL	2.58	v			Project funding decreased pending Go/No-go decision. Evaluate new formulations, continue scale-up of reactors to 2kW level.
116	*Fuel Processors for PEM FCs*, University of Michigan	2.80	v			
117	*Direct Methanol Fuel Cells*, LANL	3.33	v			Project funding decreased. Technology for portable power applications is near commercialization.
118	*Development of Advanced Catalysts for DMFCs*, JPL	2.60	v			
119	*Fuel Cell Power System for Transportation - Gasoline Reformer*, UTCFC	2.87	v			

120	*PEMFC Power System on Ethanol*, Caterpillar, Inc.	2.80		v		Project concluding.
121	*New Solid Sulfide Thio-acid Membranes for High Temperature PEMFCs*, Iowa State University	2.30			v	
122	*Effects of Fuel Composition on Fuel Processing*, ANL	2.80	v			Go/No-go decision will affect future project directions.
123	*Testing of Fuels in Fuel Cell Reformers*, LANL	3.04		v		Fuels effects project funding terminated, funding for critical durability studies continued.
124	*Carbon Monoxide Sensors for Reformate-Powered FCs*, LANL	2.83	v			Test and initiate technology transfer.
125	*Electrochemical Sensors for PEMFC Vehicles*, LLNL	2.83	v			Project transferred to Safety, Codes & Standards sub-program.
127	*Development of Sensors for Automotive PEM-Based Fuel Cells*, UTCFC	3.25	v			Review and deliver sensors for testing.
129	*Sensor Development for PEMFC Systems*, Honeywell Sensing & Controls	3.15	v			Finalize sensor requirements and begin sensor development.
130	*Fuel Cell Turbocompressor*, Honeywell	3.30	v			
131	*Development of a Torroidal Intersecting Vane Machine Air Management System for Automotive Fuel Cell Systems*, Mechanology, LLC	3.10	v			
132	*PEM Fuel Cell Air Blowers*, UTCFC	3.60			v	
133	*DOE Compressor/Expander Module Development Program*, TIAX	1.80		v		Project terminated since technology is unable to meet technical targets.

Technology Validation:

Project Number	Project, Performing Organization	Avg. Score	Con-tinued	Discon-tinued	Project Completed	Summary Comment
19	*Technical Analysis: Integrating a H_2 Energy Station Into a Federal Building*, TIAX	3.20			v	
21	*Validation of an Integrated System for a H_2-Fueled Power Park*, Air Products	2.90	v			
23	*Hawaii Hydrogen Power Park*, University of Hawaii	3.77	v			
24	*Power Park*, Pinnacle West	2.20	v			Project started late.
25	*DTE Hydrogen Power Park*, DTE Energy	3.00	v			Procure equipment, install and commission system, and establish safety framework.

26	*Filling Up With Hydrogen 2000*, Stuart Energy	3.13			v	
27	*Mixtures of H_2 & Natural Gas (HCNG) for Heavy-Duty Applications*, Collier Technologies	3.27	v			Complete conversion of 6 buses and test.
33	*Hydrogen from Biomass for Urban Transportation*, Clark Atlanta University	3.13	v			Install unit at Georgia University and test.
36	*Power Parks System Simulation*, SNL	3.55	v			Project funding increased. Expand from Power Parks to additionally include external infrastructure analyses.
37	*On-Site Hydrogen Generation & Refueling Station*, Hyradix/ SunLine	3.60	v			Demonstrate storage tanks and compressors.
61	*Development of a Natural Gas to H_2 Fueling System*, GTI	3.33	v			Proceed with installation and tests.
65	*Renewable Energy Transportation System*, SunLine	3.56	v			Emphasize maintenance and operation of natural gas and renewable hydrogen production systems.
66	*H_2 Storage and Compression: LAX*, Praxair	3.36	v			Install and operate system at LAX.
88	*Advanced Underground Vehicle Power & Control FC Mine Locomotive*, Vehicle Projects LLC	2.70	v			

Safety, Codes and Standards:

Project Number	Project, Performing Organization	Avg. Score	Con-tinued	Discon-tinued	Project Completed	Summary Comment
93	*Gallium Nitride Integrated Gas/Temp Sensors for FC Sys Monitoring for H_2 & Carbon Monoxide*, Peterson Ridge LLC	2.53	v			
126	*Interfacial Stability of Thin Film H_2 Sensors*, NREL	2.97	v			
128	*Micro-Machined Thin Film H_2 Gas Sensors*, Adv Tech Materials Inc.	3.07	v			
68	*Codes & Standards Analysis*, University of Miami	3.63	v			
69	*Hydrogen Codes and Standards*, NREL	3.68	v			

Other:

Project Number	Project, Performing Organization	Avg. Score	Con-tinued	Discon-tinued	Project Completed	Summary Comment
28	*Toward the Development of a Thermodynamic Fuel Cell*, SNL	3.00			v	
29	*Reduced Turbine Emission Using H_2-Enriched Fuels*, SNL	3.90			v	

The next Merit Review and Peer Evaluation is scheduled for May 24 – 27, 2004 in Philadelphia, PA. We would like to express our sincere appreciation to the members of the Merit 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 2003 Hydrogen, Fuel Cells & Infrastructure Technologies Program Merit Review and Peer Evaluation meeting. We look forward to your participation in the FY 2004 review.

S. Chalk

Steven G. Chalk, Program Manager
Hydrogen, Fuel Cells & Infrastructure Technologies Program
Office of Energy Efficiency and Renewable Energy

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INTRODUCTION

This report is a summary of comments from the Merit Review Panel at the FY 2003 Hydrogen, Fuel Cells & Infrastructure Technologies Merit Review and Peer Evaluation, held on May 19-22, 2003, at the Claremont Resort in Berkeley, California. The work evaluated in this document supports the Office of Energy Efficiency and Renewable Energy and the results of this merit review and peer evaluation are major inputs utilized by DOE in making its funding decisions for the next fiscal year. The objectives of this meeting were to:

- Review and evaluate FY 2003 accomplishments and FY 2004 plans for DOE laboratory programs and industry/university cooperative agreements in HFCIT (Hydrogen Fuel Cells and Infrastructure Technologies) and R&D that supports HFCIT 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.

The Merit 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 HFCIT-related backgrounds including national laboratories, hydrogen production manufacturers, hydrogen storage manufacturers, fuel cell manufacturers, universities, and other U.S. Government agencies. A complete list of the meeting participants is presented as Appendix A.

Table 1: Merit Review Panel Members

Member Name	Affiliation
Salvador Aceves	Lawrence Livermore National Laboratory
Radoslav Adzic	Brookhaven National Laboratory
Shabbir Ahmed	Argonne National Laboratory
Wade Amos	National Renewable Energy Laboratory
Raymond Anderson	Idaho National Eng & Environmental Lab
Michele Anderson	Office of Naval Research
Donald Anton	United Technologies Research Center
Timothy Armstrong	Oak Ridge National Laboratory
Paolina Atanassova	Superior MicroPowders
Carol Bailey	ChevronTexaco
Richard Bechtold	QSS Group, Inc.
Josette Bellan	Jet Propulsion Laboratory
Thomas Benjamin	Argonne National Laboratory
Theodore Besmann	Oak Ridge National Laboratory
Douglas Blom	Oak Ridge National Laboratory
Alexander Bogicevic	Ford Motor Company
Rod Borup	Los Alamos National Laboratory
Ken Butcher	Porvair Fuel Cell Technology
Richard Carlin	Office of Naval Research
Eric Carlson	TIAX, LLC
Gerard Ceasar	NIST Advanced Technology Program
William Chernicoff	US DOT- Volpe Center

Prashant Chintawar	Nuvera Fuel Cells, Inc.
William Clapper	SunLine Transit Agency
Simon Cleghorn	W.L. Gore and Associates
Hongli Dai	DuPont
Zissis Dardas	United Technologies Research Center
Emory DeCastro	De Nora N. A., E-TEK Division
Mary-Rose de Valladeres	Virent Energy Systems
Mark Debe	3M Company
Gunther Dieckmann	ChevronTexaco
Gary Dixon	South Coast Air Quality Management District
Glenn Eisman	Plug Power, Inc.
Erich Erdle	DaimlerChrysler
William Ernst	Plug Power, Inc.
Edward Feinberg	Decision Support/Energy Consulting
Karl Fiegenschuh	Ford Motor Company
Allison Fisher	Motorola Labs
Thomas Gibson	General Motors Corp.
Karl Gross	Sandia National Laboratories
James Hill	TechScope, LLC
Shinichi Hirano	Ford Motor Company
Susan Hock	National Renewable Energy Laboratory
Larry Hudson	NIST
Michael Inbody	Los Alamos National Laboratory
Craig Jensen	University of Hawaii, Dept. of Chemistry
Gilbert Jersey	ExxonMobil Research and Engineering Company
Scott Jorgensen	General Motors Corp.
Andrew Kaldor	ExxonMobil Research and Engineering Company
John Kerr	Lawrence Berkeley National Laboratory
David King	Battelle-Pacific Northwest National Laboratory
John Kopasz	Argonne National Laboratory
Dennis Kountz	Dupont
Theodore Krause	Argonne National Laboratory
Ravi Kumar	General Electric
Romesh Kumar	Argonne National Laboratory
Daniel Loffler	IdaTech, LLC
Andrew Lutz	Sandia National Laboratories
Leonard Marianowski	Gas Technology Institute
Tony Markel	National Renewable Energy Laboratory
Jennifer Mawdsley	Argonne National Laboratory
James Miller	Argonne National Laboratory
Theodore Motyka	Savannah River Technology Center, Westinghouse
Deborah Myers	Argonne National Laboratory
S. R. Narayanan	Jet Propulsion Laboratory
Jim Ohi	National Renewable Energy Laboratory
John O'Sullivan	Private Consultant
George Parks	ConocoPhillips

Larry Pederson	Pacific Northwest National Laboratory
Guido Pez	Air Products and Chemicals Inc.
Walter Podolski	Argonne National Laboratory
Roger Prince	ExxonMobil Research and Engineering Company
Michael Quah	US Army CECOM Fuel Cell Tech Team
Jerry Rogers	General Motors Corp.
Phillip Ross	Lawrence Berkeley National Laboratory
Tecle Rufael	ChevronTexaco
James Seaba	ConocoPhillips
Richard Silver	Los Alamos National Laboratory
Ron Sims	Private Consultant
Wayne Smith	Los Alamos National Laboratory
Ken Stroh	Los Alamos National Laboratory
Robert Sutton	Argonne National Laboratory
Scott Swartz	NexTech Materials, Ltd.
William Swift	Argonne National Laboratory
Morse Taxon	DaimlerChrysler
Levi Thompson	University of Michigan, College of Engineering
Pete Tortorelli	Oak Ridge National Laboratory
Susan Townsend	General Electric
Francisco Uribe	Los Alamos National Laboratory
Nicholas Vanderborgh	Private Consultant
Gerald Voecks	General Motors Corp.
Frederick Wagner	General Motors Corp.
Robert Wegeng	Pacific Northwest National Laboratory
Andrew Weisberg	Lawrence Livermore National Laboratory
Douglas Wheeler	UTC Fuel Cells
Keith Wipke	National Renewable Energy Laboratory
Chris Wolverton	Ford Motor Company
Chao-Yi Yuh	Fuel Cell Energy, Inc.
Piotr Zelenay	Los Alamos National Laboratory

SUMMARY OF MERIT REVIEW PANEL'S CROSS CUTTING COMMENTS/RECOMMENDATIONS

The Merit Review Panel members provided a number of comments and recommendations that apply to several of the projects presented or to DOE's overall management of the HFCIT Program. These comments are provided in Appendix B of this report. DOE will utilize these comments to improve both the program and future review meetings.

ANALYSIS METHODOLOGY

As shown in Table 1, a total of 99 panel members participated in the merit review process. A total of 133 project presentations were given at the meeting and a total of 507 review sheets were received from the advisory panel members (not every panel member reviewed every project). These members were asked to provide numeric scores (on a scale of one to four, with four being the highest) for five aspects of the research on their review form, a sample of which can be found as an appendix to this report (Appendix C). The five aspects were:

- Relevance to overall DOE objectives;

- Approach to performing the research and development;
- Technical accomplishments and progress toward achieving the project and DOE goals;
- Technology transfer and collaborations with industries, universities, and other laboratories; and
- Approach to and relevance of proposed future research.

The numeric scores given to each project by the reviewers were averaged to provide the overall score for that project for each of the five criteria. An average score for the five criteria was also calculated within each of the project categories. In this manner, a project's overall score can be compared to other projects in that category.

Reviewers were also asked to provide qualitative comments on the five research aspects, as well as the specific strengths and weaknesses of the project and any recommendations for additions or deletions to the work scope. These comments, along with the quantitative scores, were placed into a database for easy retrieval and analysis. These comments are summarized in the following sections.

ORGANIZATION OF THE REPORT

This report is organized in six sections, in an effort to group projects according the sub-program in which they fall in the HFCIT Multi-Year Program Plan. Some sub-programs have been grouped together due to the recent restructuring of the Program and overlapping project goals. A brief description of the general type of research being performed in each category is presented.

The remaining pages of each section present the results of the analysis for each of the projects discussed at the merit review. Graphs showing how the particular project compared with all other projects presented, as well as a discussion of these results. A summary of the qualitative comments is also provided.

This category includes projects that explore various pathways for the efficient production of low-cost hydrogen from diverse domestic resources, including fossil, nuclear, and renewable sources, while minimizing environmental impacts. It also includes projects that explore means of distributing hydrogen from centralized or distributed sites of production to enable the introduction of hydrogen as an energy carrier for transportation and stationary power as well as ensuring its long-term viability. These efforts seek to achieve the following overarching milestones established in the Hydrogen, Fuel Cells and Infrastructure Technologies Program Multi-Year Research, Development and Demonstration Plan:

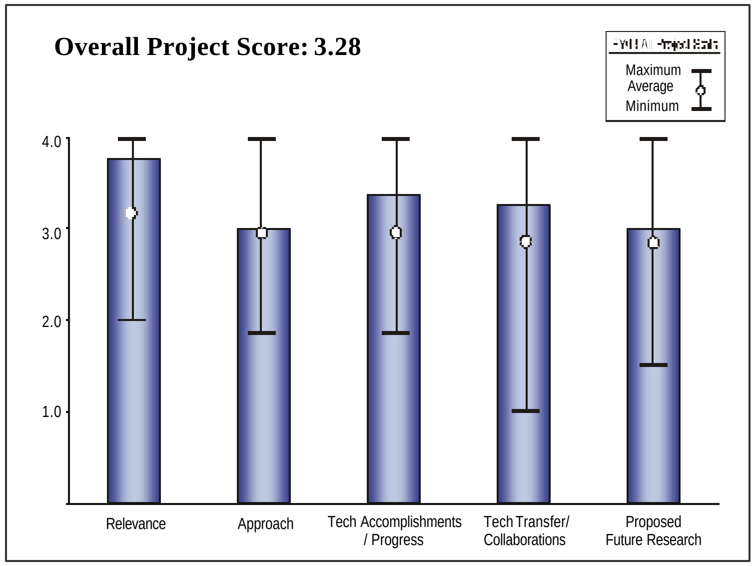
Hydrogen Production

- Reduce the cost hydrogen production from natural gas or liquid fuels to a price equivalent to $1.50 per gallon of gas at the pump by 2010 from the current price equivalent of $5.00 per gallon.
- Develop and demonstrate the production of hydrogen from biomass, currently costing $3.60-$3.80 per kilogram, at a price of $2.60 per kilogram by 2010 and is competitive with gasoline by 2015.
- By 2015, demonstrate direct water-splitting production of hydrogen at a plant gate cost of $5.00 per kilogram photoelectrolytically and $10 per kilogram photobiologically from a current cost of more than $200 per kilogram.
- By 2010, verify large-scale central electrolysis at $2.00 per kilogram of hydrogen from the current $2.60 per kilogram.

Hydrogen Delivery

- By 2010, reduce the cost of delivering hydrogen fuel from central production sites to refueling stations to less than $0.70 per kilogram, and the cost of on-site movement and handling to less than $0.60 per kilogram, and
- By 2015, decrease the cost of hydrogen fuel delivery and on-site movement and handling to a combined cost of below $1.00 per kilogram.

Project #1: H_2 from Biomass: Catalytic Reforming of Pyrolysis Vapors
Evans, Bob, NREL



Brief Summary of Project

In this project, NREL evaluated the production of hydrogen from biomass by pyrolysis - steam reforming for $2.90/kg by 2010 and its barriers. Their milestone is to verify advanced catalysts and reactor configuration for fluid bed reforming of biomass pyrolysis liquid at pilot scale with catalyst attrition rates of < 0.01%/day by the fourth quarter of 2009.

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

This project earned a score of 3.75 for its relevance to DOE objectives.

- Project supports goals and objectives in EERE Hydrogen R&D Plan.
- This could become one of most economic processes at 500-1000 kg H_2/day for production from biomass, depending on the contribution of by-products (char and adhesive).
- By-products can be sold into existing markets (energy, centralized adhesives).
- There is no evidence that H_2 total cost status is $3.80/kg in 2003 - this needs a preliminary economic evaluation that supports that achievement.
- Not responsive to 2005 goals.
- This project is well thought out and fully addresses the production of hydrogen from biomass. Biomass is a cheap resource which in many cases goes to waste. Instead of having to spend resources to dispose of biomass, the situation is reversed and the biomass is used instead as a resource. Because CO_2 sequestration is not a problem for biomass, the biomass-related projects are environmentally friendly.

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

This project was rated 3.00 on its approach.

- Market barriers or lack thereof are addressed.
- Reviewer does not believe technical feasibility of integrated process has been demonstrated.
- Is thermal instability of pyrolysis oil a potentially fatal flaw? Reviewer believes that for continuous integrated operation (months), pyrolysis oil will have to be more stable to avoid vaporizer, reformer feed distribution plate, fluid bed, etc. from fouling.
- Need to characterize oil vs. pyrolyzer operating conditions: Engler Question (Q) 2, Bullet (4): distillation, olefin/aromatic analysis; functional group analysis (-OH, - CO_2H).
- Will stabilization with MeOH improve operability?
- Peanut shell orientation needs to be explored - reliance on one crop makes this a niche effort. Suggest a multi-feedstock approach.
- The approach is excellent. It focuses on the feasibility of demonstration.
- The interaction with the modeling effort, and with the development of improved catalysts, addresses the important issues of optimization and scaling, as well as that of issues associated with longtime operation.
- Sound.
- Real world.
- May contribute to distributed reforming.

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

This project was rated 3.38 based on accomplishments.

- Scale-up of fluid bed processes in bubbling bed range must be carefully planned (data needed; how to acquire them).
- Reviewer recommends minimum reactor diameter of 3" to avoid s/v ratio of reactor phenomena as well as fluidization gas velocity/residence time problems.
- Catalyst testing can be done in smaller reactors.
- The PI has shown feasibility for operation up to 84 hours. Foreseen problems associated with long time operation (desired 1000 hours run) are being addressed.
- Good progress reformer unit; improved H_2 yields.
- Pyrolysis unit - flow rate seems low.
- Good publications.

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

This project was rated 3.25 for technology transfer and collaboration.

- Recommend consultants with industrial experience in: 1) experimental design and scale-up of fluid bed reactors (R.W. Pfieffer is still active) and 2) development of new fluid bed catalysts (J.R. Ebner).
- Recommend conversations with United Catalysts and Davison companies with lots of experience in developing fluid bed catalysts.
- Should coordinate with other pilots using similar methods but different feedstocks.
- This is a built-in part of the project through the collaboration with the Clark Atlanta University, which runs a reactor based on the utilization of peanut shells.
- Good. Timing of transition to Clark Atlanta University seems appropriate.
- Project enjoys goodwill in community - need outreach plan for students and community.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Circulating fluid bed (CFB) should be put on back burner until problems with bubbling bed systems are resolved. If process won't work in bubbling bed, it most likely will not work in CFB, which is a more difficult process to develop.
- Should have a mid CFB project review - possible acceleration of scale-up with BB.
- The future activities are well outlined, with a decision to be made at the end of 2006. If the decision is to go ahead with the concept, the scaling of 10x would be greatly helped by the companion modeling effort at JPL.
- Focus on PI-identified pyrolysis design challenges in order to meet \$2.90/kg H_2 goal by 2010.
- Pursue fast fluidized bed, (rather than bubbling bed), recognizing that this is a less conservative approach.

Strengths and weaknesses

Strengths
- Has a good start on by-product utilization.
- Strengths in concept, association with modeling, and technology transfer.
- Good fit with other 2 NREL projects.

Weaknesses
- Pyrolysis stream, unless characterized and stabilized, will be a difficult stream to reform in commercial fluid bed processes.
- Given the complexity of the concept, one cannot assign weaknesses at this point.
- More information on coproducts, even in Phase 2, would be beneficial.

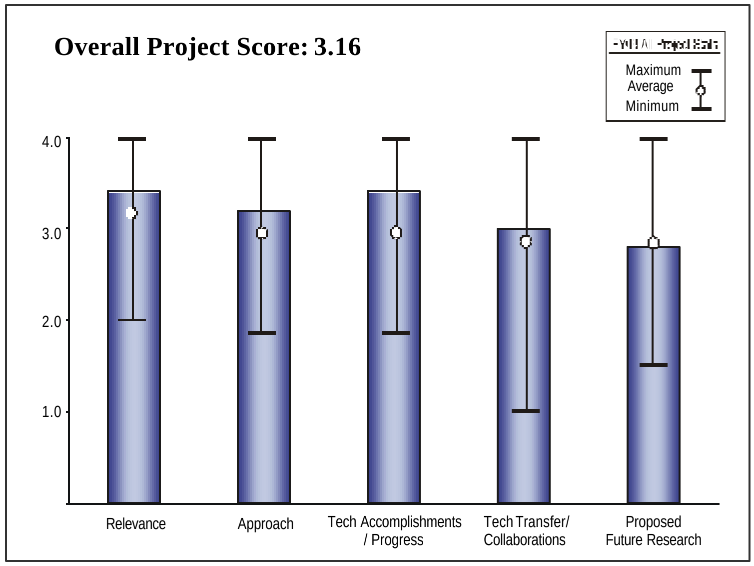
Specific recommendations and additions or deletions to the work scope

- Recommend continued funding.
- Additions to work: Pyrolysis oil characterization, pyrolysis oil stabilization, experimental design for fluid bed scale-up/operation, more traditional fluid bed catalyst development, use outside expertise to assist in above.
- Delete: CFB until above items are well in hand, scale-up of fluid beds until above items are well in hand.
- Continue as planned. Keep in close association with the modeling effort without which optimization and scaling will be very problematic.
- Add outreach component.
- This story (George Washington Carver, peanuts, science, economic development potential) has educational value beyond the worthy "Train the Trainers" approach.
- Capitalize on synergy with other NREL biomass projects.

Project #2: H_2 from Post-Consumer Residues
Czernik, Stefan, NREL

Brief Summary of Project

This project explores the feasibility of producing hydrogen from renewable feedstock to increase flexibility and improve economics of biomass to hydrogen process. The goal is to develop and demonstrate technology for producing hydrogen at $2.90/kg by 2010. The approach being used is pyrolysis or partial oxidation and steam reforming of biomass, plastics, and other solid organic residues.



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

This project earned a score of 3.40 for its relevance to DOE objectives.

- This project addresses the production of H_2 from plastic wastes and trap grease, which are currently considered waste materials. Therefore, a liability (plastic waste and trap grease) will be turned into all asset if the project is successful.
- Not responsive to 2005 goals.
- Needs to discuss emissions and possible toxic by-products of processing plastic and grease.
- Project offers higher hydrogen yield for given feed rate to NREL's pyrolysis/reforming process. Could reduce H_2 total cost when waste steams are co-fed with biomass as compared with biomass only.
- The ease of reformation and transport could facilitate disturbed reforming.
- Certainly relates as a "renewable" source of H_2.

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

This project was rated 3.20 on its approach.

- The project relies on well-known chemical path (steam reforming and water-gas shift) to transform hydrogen-rich materials into hydrogen and CO_2. The transformation is proposed to be performed in a fluidized bed where pyrolysis and reforming will occur concurrently.
- Doesn't address barriers except as milestones. What might keep the project fro m achieving the milestones?
- No discussion of cost breakdown of $2.90/kg goal.
- Needs to be compared to other processes such as conversion to diesel, or other feedstocks such as agricultural biomass.
- Market barriers do not appear to be issue.
- What constitutes "demonstration of technical feasibility"?: fixed bed NG reforming catalysts last 10+ years, commercial fluid bed catalysts last 1-2 years, 1000 hr stable run with no loss in fluidization properties activity, sensitivity, attraction resistance would be desirable, and characterization of pyrolysis oils is essential.
- Good targets.
- Reasonable to seek opportunities to co-process feedstocks.
- It's a reasonable approach to obtain H_2 by pyrolysis and reforming of waste polymeric materials that are relatively clean hydrocarbon sources, which makes it more technically feasible than using natural biomass sources. That is, providing that the waste plastic materials can be collected at sufficiently low cost to function as economical feedstocks for this process.

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

This project was rated 3.40 based on accomplishments.

- This project was initiated in FY 02.
- For the generic polypropylene, runs of up to 10 hrs have been performed showing 80% of stoichimetric recovery of H_2. For trap grease, runs of 180 hours show between 40% and 60% yield.
- This seems to be a start-up project, with the first results coming later this year.
- Preliminary works of calculation of possible market size, feedstocks, etc.
- Has demonstrated concept of pyrolysis/reforming of polypropylene (PP) and reforming of trap greases using an alpha-version of NREL fluid bed reforming catalyst.
- Large potential global market opportunity - needs clarification, quantify PP within universe of plastics.
- Good results to date.
- The work has provided good results for the pyrolysis/reforming of polypropylene, a "clean" fuel. But there appears to be a diminishing H_2 yield with time from reforming trap grease which contains N, S and very likely also trace elements which can affect the Ni catalyst.
- This is the real challenge in any H_2 from biomass process which only one speaker (P. Irving of Innova Tec) seems to have seriously addressed.

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

This project was rated 3.00 for technology transfer and collaboration.

- There are publications and other collaborative interactions.
- Would recommend getting help from consultants with industrial experience in scale-up/commercialization of fluid bed thermal and catalytic processes.
- Pursue collaboration for future sector entity.
- Collaborated with Pacific Biodiesel – a trap grease material source.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.80 for proposed future work.

- The work is early in its lifeline.
- The proposed future research is reasonable and should also incorporate the understanding of the physics leading to a derivative of long-term performance.
- Needs to exp lore possible exit points.
- Currently organized in parallel tracks.
- Chlorine, which would form in pyrolysis of PVC, is a good gas phase, non-selective, chain-carrying catalyst. Could lower process temperature by 50 – 100^{o}C. May want to dilute chlorine by co-processing PVC with PP. Also check reactors and process lines for corrosion when using PVC. Watch out for effect of chlorine on the reforming catalyst.
- Objectives fine.
- Ensure readiness for tech transfer.
- Determine cause of catalyst deactivation.
- The intended future development of this technology for the pyrolysis of more complex (and hence "dirtier") feedstocks will require the development of "heteroatom", S and P resistant catalysts or better techniques for the removal of these species prior to the catalytic reforming step.

Strengths and weaknesses

Strengths
- Well thought out project; very knowledgeable PI.
- Excellent presentation.

Weaknesses

- Although not a weakness at this point, the PI should seek to understand the progress of the deterioration of the long-term performance.
- How to deal with S, P and other naturally occurring contaminants is clearly a generic challenge for any biomass based H_2 process and should be the key barrier to attack with continued research.

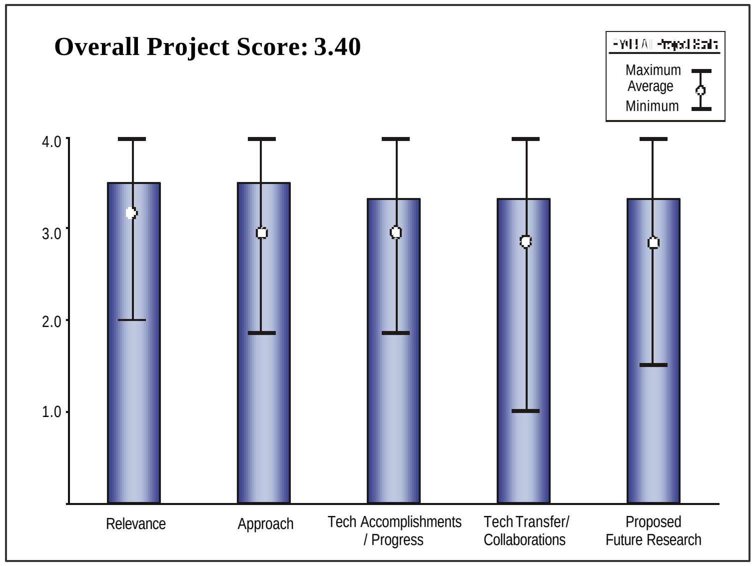
Specific recommendations and additions or deletions to the work scope

- The work will be enhanced by the addition of a modeling component.
- Recommended continued funding.
- Co-processing biomass and PVC could form dioxins - need to test char and waste water streams. Also need to check for PAHs and chlorinated PAHs in waste stream runs. Also don't forget about polymer additives/modifiers such as plasticizers, flame retardants, processing aids, etc.
- Make sure you don't create an environmental problem.
- Investigate cause of catalyst deactivation (phosphorous theory,etc).
- Techno-economic analysis needed.
- Look to closer cooperation with industry.
- Capitalize on synergy with other NREL biomass programs.
- This work should be complemented by an economic analysis of the process including the cost of gathering and collecting the feedstocks. The analysis should include an estimate of the energy efficiency of the process calculated on the basis of the calorimetrically determined heating value of the feedstock and the yield of H_2 product.
- Recommendations on how this overall energy efficiency could be improved by an integration of unit operation in the process.

Project #3: Fluidizable Reforming Catalysts
Magrini, Kim, NREL

Brief Summary of Project

The objective of this project conducted by K. Magrini and colleagues from NREL is to develop and demonstrate the technology to produce hydrogen from biomass at $2.90/kg based on 750t/day by 2010 and to make it competitive with gasoline by 2015. Their approach is to identify the best industrial reforming catalyst; identify the aluminas for use as catalyst support: and formulate, evaluate and optimize multifunctional, multicomponent catalyst. To date they have developed novel fluidizable reforming catalysts with CoorsTek ceramics and improved reforming activity.



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

This project earned a score of 3.50 for its relevance to DOE objectives.

- This work is crucial to the success of project 1 and 2, through the development of improved catalysts for utilization in converting biomass, plastics, and trap grease to H_2 via fluidized bed reforming.
- No discussion of 2005 objectives.
- No quantification of contribution to achieving 2005 goals.
- Quantification of saving over conventional technology in terms of $/kg H_2.
- Discovery of catalysts system that supports multiple fuels is a worthy goal as it is a necessary enabler for biomass feeds supply reliability.
- Project is to develop catalyst for a key step in the process targeted to meet goals and objectives in EERE H_2 R&D plan.
- High.

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

This project was rated 3.50 on its approach.

- An assertive approach is in progress to develop improved catalysts.
- Might benefit from collaboration with combinatorial discovery projects.
- Market barriers do not appear to be an issue.
- Development of a new-to-the-world commercial grade fluid bed catalyst for a new high temperature process (fluidized bed reforming) using a not very well characterized thermally unstable variable composition feedstock is a very difficult technical challenge.
- Ms. Magrini has made a significant advance in getting to candidate no. 1.
- I believe significant work remains before a "commercial grade" catalyst can be developed. Even then, some conditioning of the feed (pyrolysis oil) will most likely be required.
- Solid approach.
- Search for new catalysts for lower reforming temperatures and evaluation of renewable feedstocks are important industry concepts.
- Stable catalyst system needed in fluidized bed system.

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

This project was rated 3.33 based on accomplishments.

- Operation with catalyst have been performed for up to 25 hrs.
- Results consistent with expectations.
- Further success would enable goals to be achieved.
- Need performance indicators on yield, selectivity, activity, rate of overall activity loss, rate of activity loss for specific reactions, fluidization data, PSD optimization, attraction resistance change with OST, regenerations, etc.
- Progress in reforming activity.
- Longevity of catalysts is an issue.

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

This project was rated 3.33 for technology transfer and collaboration.

- The interaction with projects 1 and 2 ensure that the PI addresses the right problems.
- Other collaborations and tech transfers are listed.
- Coordination with other materials research projects would be useful.
- Reviewer recommends using outside consultants and catalyst companies (United Catalyst, Davidson, etc.) with experience in developing and commercializing fluid bed catalysts.
- Investigative assistance from industrial catalyst developers and universities needed.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.33 for proposed future work.

- All future milestones seem reasonable.
- Quantification of future goals would be useful.
- Rapid screening and accelerated testing of catalyst compositions are critical capabilities.
- Accelerate screening approach work for deactivation analysis.

Strengths and weaknesses

Strengths
- Client-oriented research with well defined goals.
- Dynamic and knowledgeable PI.
- Excellent presentation.

Weaknesses
- Needs to address the issue of long term operation.
- Include techno-economic analysis with feedstock evaluation.

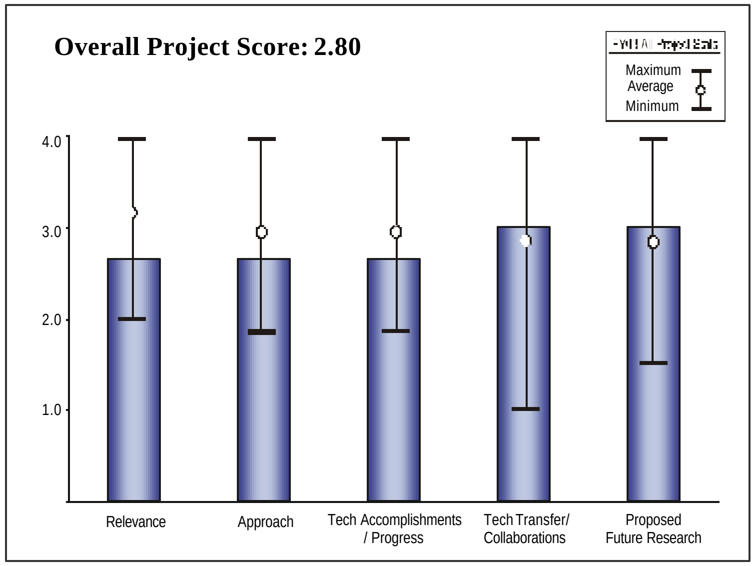
Specific recommendations and additions or deletions to the work scope

- Continue as is, with the addition of the long-term operation as an important goal.
- Recommend continued funding.
- Recommend using diphenols (e.g. resorcinol) as model compound.
- Probably should look at the combinations of model compounds to more closely simulate reactive groups in pyrolysis oil.
- Seek industry (user) input and cooperation on low temperature catalyst development.

Project #4: Biohydrogen Production from Renewable Organic Wastes
Sung, Shih-wu, ISU

Brief Summary of Project

Iowa State University studied the Biohydrogen production from renewable organic wastes by identifying and quantifying the hydrogen-producing bacterial population in a complex microbial community background, developing different strategies for selective growth of hydrogen producing bacteria (e.g. heat selection and pH control) in a mixed culture environment and optimizing the process to achieve sustainable hydrogen production in continuous flow bioreactors.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.67 for its relevance to DOE objectives.

- No indication that process can come close to goals and objectives of EERE hydrogen R&D plan.
- An important component.
- Nearest bio approach to commercialization. But not very 'new' - a clinical approach with similar work in other countries.

Question 2: Approach to performing the research and development

This project was rated 2.67 on its approach.

- It's the reviewers experience that negative value streams become more valuable when there is a use for them that makes money.
- Perhaps hydrogenases would be useful in some other more efficient systems.
- Generally good program.
- "Naturally occurring" approach.
- Very reasonable; addresses key barrier.
- Good feedstock selection.

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

This project was rated 2.67 based on accomplishments.

- Terminal Restriction Fragment Length Polymorphism (T-RFLP) is a dated approach.
- The program in molecular biology should allow more direct approaches at least occasionally.
- Although far from goal, good results and progress to date (e.g. purification process, identification producer, consumer organisms).

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

This project was rated 3.00 for technology transfer and collaboration.

- Would hydrogenases present in this system be useful in another biosystem?
- The team has plenty of experience with commercializations.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Project results do not warrant going to pilot scale design and installation.

Strengths and weaknesses

Strengths
- Naturally occurring approach.

Weaknesses
- Under funded?
- What is the theoretical maximum yield for hydrogen from system? It was indicated that the yield of H_2 from sucrose is 2 - 3 moles/mole sucrose which corresponds to 0.66 - 1.0 kg H_2 per 60 g sucrose.
- Even at best this will be an inefficient way of making H_2 - most of the H_2 in the waste does not get liberated as H_2 gas.

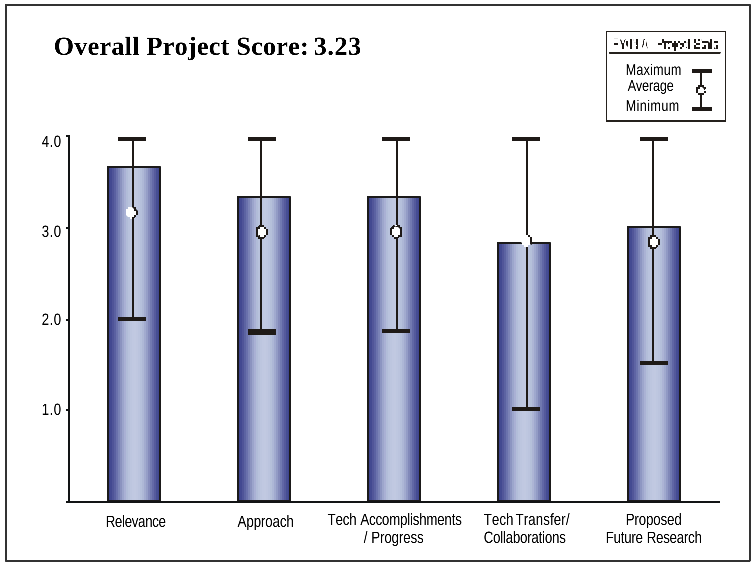
Specific recommendations and additions or deletions to the work scope

- If the expected yield is only less than or equal to 1.0 kg H_2/60 kg sucrose, I recommend not funding the project unless the hydrogenases would be useful in a biophotolysis system or a more efficient bacterial system.
- Needs creative thoughts to go from $2H_2$ per glucose to near the 12 available.
- Add techno-economic analysis.

Project #5: Biological Water Gas Shift
Maness, Pin-Cing and Wolfrum, Education, NREL

Brief Summary of Project

In this project, NREL studied the overall biological CO shift pathway, improved the rate and durability of H_2 production from CO, developed and operated a bioreactor in order to develop a biological water gas shift pathway.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.67 for its relevance to DOE objectives.

- A good niche project that should allow a bio commercialization.
- No competition to my knowledge.
- Project shows economic potential as a processing step that could help meet goals and objectives of EERE hydrogen R&D plans.

Question 2: Approach to performing the research and development

This project was rated 3.33 on its approach.

- Good to see real goals including higher preview operation.
- No market barriers.
- Success story.
- Both engineering and biological approaches sound.

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

This project was rated 3.33 based on accomplishments.

- Good solid program.
- Addressed suggestions/comments of past reviews.
- Organism with great potential.
- Excellent lab and scale up work. Significant accomplishments.

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

This project was rated 2.83 for technology transfer and collaboration.

- Apparently a good opportunity in refinery.
- Transfer to other organizations for H_2 production should be pursued.
- Is transfer of hydrogenize enzyme system from R. gel worth trying?
- Industry could have growing role opportunities?

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Looks as though this is coming to an end.
- The two stage fermentation/photo fermentation concept should be explored.
- Focus on hydrogenase from clostridium into blue green algae as planned.

Strengths and weaknesses

Strengths
- This has been a long-running project. Good to see the current investigations doing such a good job of collaborating.
- This is a one stage process and low temperature.

Weaknesses
- None specified.

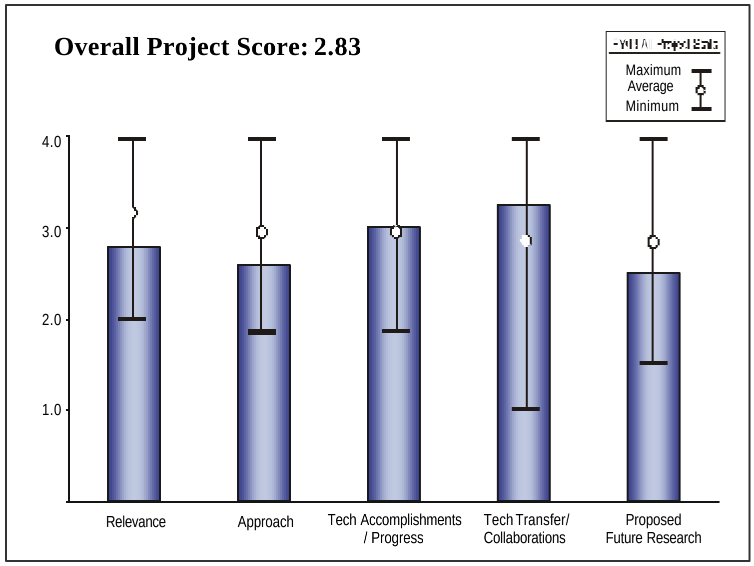
Specific recommendations and additions or deletions to the work scope

- Should be planning a quite different project for 2004. This should end soon!
- Recommend funding.
- Outreach on success story.

Project #6: Thermocatalytic CO_2-free Production of H_2 from HC Fuels
Muradov, Nazim, Florida Solar Energy Center

Brief Summary of Project

In this project, the Florida Solar Energy Center is working on developing a thermocatalytic process for the production of CO_2-free hydrogen from hydrocarbon fuels and improving its efficiency while reducing the cost of production. The steps taken in this project is to design, fabricate and test a bench-scale thermocatalytic reactor for CO/CO_2-free production of hydrogen-rich gas and carbon products, determine the effect of heavy hydrocarbons, moisture and sulfur compounds present in commercial hydrocarbon fuels on the process efficiency and the catalyst activity/stability and characterize carbon products of the process and evaluate their application areas and market value.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.80 for its relevance to DOE objectives.

- Project addresses CO_2 emissions, which is critical in the success of any fossil based reforming.
- Needs to evaluate the utilization of CH_4 vs. reforming.
- Appears to solve one problem at the expense of another (CH_4 availability). Particularly problematic if renewable (carbon neutral) sources are used.
- Good project.

Question 2: Approach to performing the research and development

This project was rated 2.60 on its approach.

- This project relies heavily on favorable commercial use of the carbon byproduct. This makes it not suitable for distributed H_2 product as a result.
- Could benefit by analyzing (techno economic or other) how in a highly distributed generation the carbon will be recovered and used.
- Project has not fully analyzed or demonstrated life cycle benefit vs. centralized steam reformation and sequestration or CO_2 conversion.
- No external catalyst may be required. Several forms of carbon may be produced as a byproduct that could be sold to reduce the price of production.
- A very interesting self-catalyzed pyrolytic decomposition of methane to carbon and hydrogen has been developed and the process has been tested in a 1kW pilot plant. Its economic value – the ultimately attainable cost of H_2, critically depends on the assigned worth for the very significant carbon by-product.

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

This project was rated 3.00 based on accomplishments.

- Good progress made on effect on contaminants (H_2O, S) and carbon catalyst activation.
- The PI addressed renewal issues related with the catalyst activity: activation, moisture, and sulfur presence. Operation of up to 4 hours has been performed.
- Need to compare against H_2 production benchmarks and milestones.

- Excellent technical accomplishments, resulting in an H_2 producing process that really works. But the arguments with what to do with the carbon by-product were not at all convincing. To just state that the carbon from a local H_2 generating site can be trucked and that the cost of this is "negligible" is simply not realistic.

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

This project was rated 3.25 for technology transfer and collaboration.

- Excellent collaboration and effort.
- A good mix of expertise.
- There are two publications, Nuvera's conference presentations and 3 patent applications.
- Not clear if there are external collaborations.
- Needs "cooperative integration" with large scale carbon uses e.g. aluminum manufacturers.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Use of biomass feed may be a diversion. Aim for a centralized H_2 plant, not distributed.
- The proposed research seems somewhat overly optimistic given the product results.
- Not clear as to what barriers are and how they were being addressed.
- There should be not only technical but also economic analysis milestones which envisage a use scenario for the technology.

Strengths and weaknesses

Strengths
- Inexpensive catalyst.
- Proved easy activation of the carbon catalyst.
- Good concept and dedicated PI.
- Nice approach to CO_2-free production of H_2 without gas sequestration.
- A clean production of H_2.

Weaknesses
- Reliance on favorable carbon usage.
- The PI seems to have contracted the modeling component and presented results whose relevance he did not understand.
- Some of the modeling results did not make much sense.
- No realistic convincing scenario of what would be done with the by-product carbon, discerning the real cost of C disposal and use.

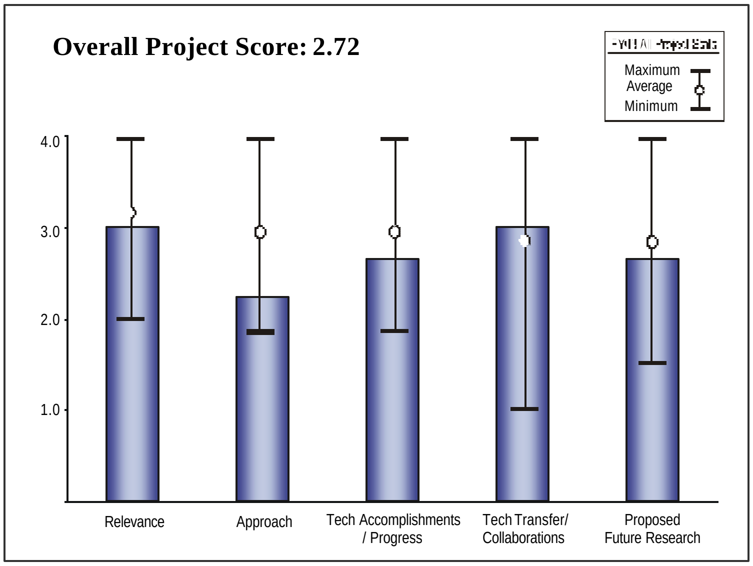
Specific recommendations and additions or deletions to the work scope

- Consider integration with gasification process.
- Need to understand the physics of the situation and be more critical towards his own results.
- Need to be more realistic in plans and expectations.
- Develop specific realistic scenarios of where and how this technology would be used and its potential cost. For example: Its integration with carbon black manufacturers (mostly a C product for rubber tires) and for anodes for the aluminum industry.

Project #7: Novel Catalytic Fuel Reforming Using Micro-Technology with Advanced Separations Technology
Irving, Patricia, InnovaTek

Brief Summary of Project

The goal of this project by InnovaTek, Inc. is to produce pure hydrogen from infrastructure fuels using cost-competitive, highly efficient catalytic steam reforming and membrane separation technology by optimizing InnovaTek's proprietary steam reforming catalyst composition, optimizing the hydrogen-permeable membrane composition and operating procedures, developing efficient thermal management using microchannel heat exchangers and an internal bumer and integrating processes and components to achieve smallest size and most efficient thermal management.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Prevention of coking is the best value of R&D.
- Project attempts to address feed pre-treatment, reforming, separation which are all key areas in the EERE plan.
- Excellent concept. Well thought out.
- Efficiency targets are too low.
- Fuel sulfur targets do not need to be so high, given market fuel content in 2007.

Question 2: Approach to performing the research and development

This project was rated 2.25 on its approach.

- Volume and weight coupled with low (1 kW) power may be lagging other efforts and are prohibitive for many applications.
- Program needs to demonstrate a roadmap to achieve substantial size and weight reduction.
- Objectives too broad.
- Should drop NG reforming objectives and stick to diesel reforming.
- System is more suitable for liquid fuel reforming due to the small power design.
- Good approach. The important issues seem to be addressed.
- It is difficult to check the details which are not provided (e.g. modeling of processes in microchannels).
- It should be integrated with other fuel processing activities. Little novelty.
- Should use market fuel; not doped fuel and you will see different results.

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

This project was rated 2.67 based on accomplishments.

- System is too complex for a portable APU.
- The PI is on track and goes assertively after the goals.
- Long term natural gas reforming tests have been performed for up to 1100 hours.
- Efficiency targets are too low.

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

This project was rated 3.00 for technology transfer and collaboration.

- Associations with universities and national material laboratories are noted, as well as two presentations.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- Decision points are not well-defined.
- A beta prototype is proposed for testing in FY 04.
- Needs more realistic efficiency targets.

Strengths and weaknesses

Strengths
- Progress in system integration.
- Well thought-out project.
- Knowledgeable PI.

Weaknesses
- Objectives are too broad.
- No details on the models are provided, particularly publications from the university collaborations. Their impact to the project is unclear.
- Little novelty; technical approach should be modified as indicated.

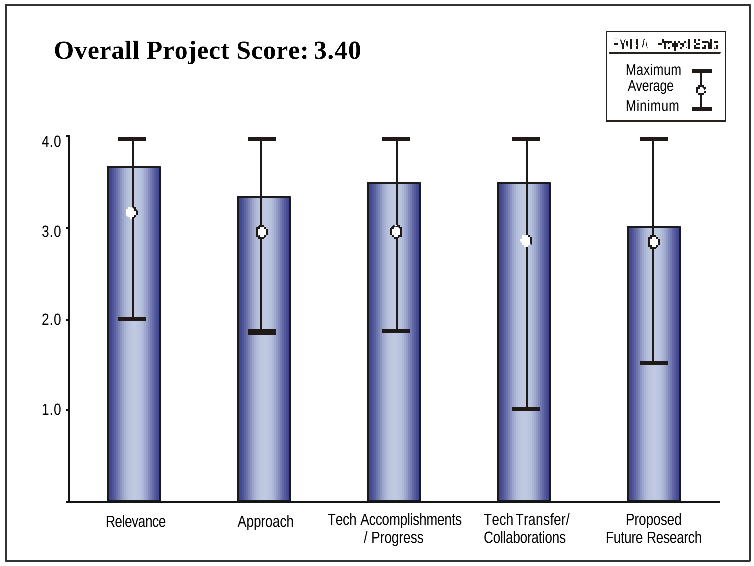
Specific recommendations and additions or deletions to the work scope

- Limit goals to diesel reforming and smaller power.
- Address the potential plugging of the microchannel pores to the performance of the reformer.

Project #8: Engineering Development of Ceramic Membrane Reactor Systems for Converting Natural Gas to Hydrogen & Syn Gas for Liquid Transportation Fuels
Chen, Christopher, Air Products & Chemicals Inc.

Brief Summary of Project

In this project, Air Products and Chemicals, Inc is working towards developing a ceramic membrane reactor system for converting natural gas to hydrogen and synthesis gas for liquid transportation fuels. The reactor is intended to be scaled up through pilot-scale testing and precommercial demonstration. Technical, engineering, operational and economic data necessary for full commercialization are being collected and analyzed.



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

This project earned a score of 3.67 for its relevance to DOE objectives.

- This project is very critical to realizing the H_2 economy.

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

This project was rated 3.33 on its approach.

- Needs to address natural gas pre-treatment, performance, and wafer stability. There are issues with contaminants.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.50 based on accomplishments.

- This project has shown important progress in many areas, especially the critical ceramic-to-metal seal issues.

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

This project was rated 3.50 for technology transfer and collaboration.

- Excellent mix of partners in place.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- Should include impurity tolerance tests for the wafers and catalysts.

<u>Strengths and weaknesses</u>

Strengths
- Well thought out approach.

- Excellent collaboration with partners.
- Novel approach and has made good progress toward targets.

Weaknesses
- None specified.

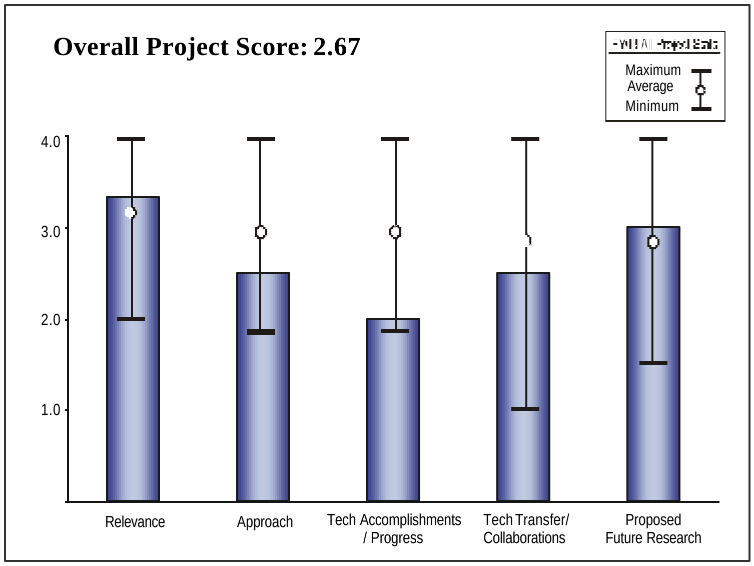
<u>Specific recommendations and additions or deletions to the work scope</u>

- Consider testing the ceramic-to-metal seal under more drastic conditions (e.g. larger change in pressure or change in temperature).

Project #9: Integrated Ceramic Membrane System for H_2 Production
Schwartz, Joseph, Praxair

Brief Summary of Project

In this project, Praxair, Inc. is working on developing an integrated ceramic membrane system for hydrogen production. The objective of this project was to perform technoeconomic feasibility analysis for the system and define the development needed to prepare the concept for pilot testing and demonstration.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.33 for its relevance to DOE objectives.

- The project addresses important steps towards DOE objectives.

Question 2: Approach to performing the research and development

This project was rated 2.50 on its approach.

- Have not made much progress.
- Program would benefit by partnering with competent and experienced ceramic processing and or materials engineers.
- Having the OTM & HTM reactors separate is good in that the system is simplified and easily understood. It is not clear whether the OTM reactor development is part of this scope.

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

This project was rated 2.00 based on accomplishments.

- No significant data.
- Deliverables not aggressive enough (however, the amount of resources/budget was not provided).

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

This project was rated 2.50 for technology transfer and collaboration.

- May need to extend partnership.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Not clear what phase 1 and phase 2 are in terms of milestone deficiencies.
- The deliverables, with respect to the HTM alloy or substrate, should include specific material properties (physical and chemical).

Strengths and weaknesses

Strengths

- Less complicated approach.

Weaknesses

- Lack of hard data.
- Objectives may be a bit soft.

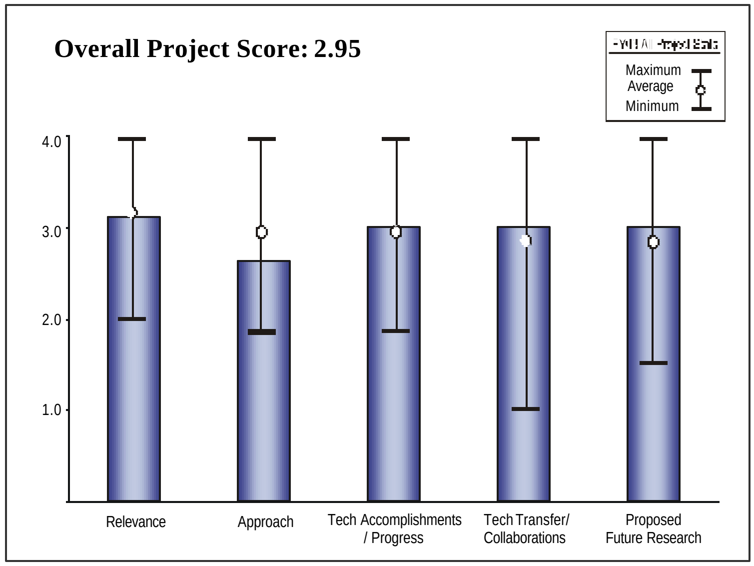
Specific recommendations and additions or deletions to the work scope

- Consider adding partners to help the remaining areas (pretreatment and reforming).

Project #10: Low Cost H_2 Production Platform
Aaron, Tim, Praxair

Brief Summary of Project

In this project, Praxair, Inc. is working towards the development of a low cost hydrogen production platform. Their efforts include defining process/equipment concepts and developing preliminary designs suitable for mass production of a small on-site hydrogen system, performing a technoeconomic study and developing business cases regarding the viability of the development project. They are using steam methane reformer and purification process technologies as the basis to evaluate different systems and identify the system most likely to be commercially viable when mass produced.



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

This project earned a score of 3.13 for its relevance to DOE objectives.

- This project should provide important feed-back to other projects that also consider individual sub-systems (catalyst, reformer, integration, infrastructure, auto industry, etc.).

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

This project was rated 2.63 on its approach.

- Work would benefit if it involved the compression and storage in its analysis and potentially as part of reintegration system.
- Does not address codes and standard citing issues.
- It is difficult to understand why Phase 1, as described, should precede Phase 2.
- Before assessing economics, it seems that a model is necessary. Techno-economical analysis should be done only after it is proved that the concept really works.
- Very good overall system and sub-systems breakdown.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- Good understanding of component and system design options.

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

This project was rated 3.00 for technology transfer and collaboration.

- Not presented in a specific manner.
- Three different organizations participate.
- Should identify catalyst supplier soon.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- It seems that modeling and techno-economical analysis activities should have been inserted into timeline.

Strengths and weaknesses

Strengths
- Good cost and system breakdown of components.

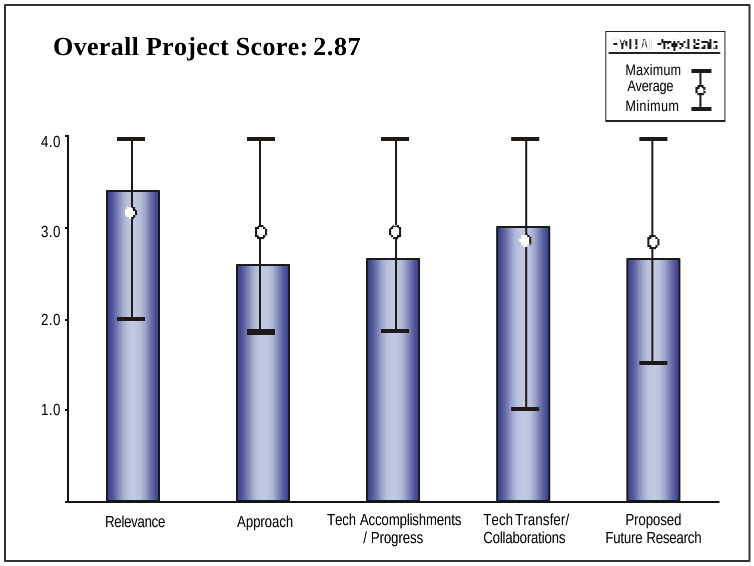
Weaknesses
- None specified.

Specific recommendations and additions or deletions to the work scope

- Consider partnership with energy company or infrastructure based company.

Project #11: Defect-free Thin Film Membranes for H_2 Separation & Isolation
Nenoff, Tina, SNL

Brief Summary of Project



In this project, SNL is working towards synthesizing defect-free thin film membranes for H_2 separation and isolation which can be replace existing expensive and fragile Pt catalysts. This work includes testing the separation of light gases through the membranes and demonstrating effective light gas separations and commercialization potential of zeolite membranes. SNL is modeling the permeation of light gases through various frameworks/pores for optimized performance and validating the model with actual permeation data obtained through tests on unique in-house permeation unit.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.40 for its relevance to DOE objectives.

- The relevance is very high, if a highly efficient and selective membrane separation can be found to purify hydrogen.
- H_2 separation effort is critical in light of the fact that most feasible H_2 production methods today are from fossil based fuels.
- Membrane development and fabrication for H_2 separation is potentially a future-enabling technology that can be used in H_2 manufacturing processes.

Question 2: Approach to performing the research and development

This project was rated 2.60 on its approach.

- Could benefit from analysis or demonstration of scale-up.
- The intended approach is clear, but not how zeolites will do the job.
- Size exclusion does not seem possible with the materials they are using. This is evident in the separation data provided. If the separation is via a chemical interaction, an explanation should be provided.
- The "defect-free" aspect of the membranes is not clear. How important is it for the materials to be defect-free? How will this affect manufacturing cost?
- Targets are not clearly defined.
- Inorganic permselective membranes in the form of zeolite thin films have been prepared which generally show some as yet very limited separation selectivity for H_2 vis-à-vis most heavier gases. Permeation data for SF_6 shows that there are no pin holes (defects) in the test membranes.This is in itself is an important accomplishment but for the systems to be practically useful, the synthetic approach has to be further improved to enable the realization of the much needed higher selectivities.

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

This project was rated 2.67 based on accomplishments.

- It appears that good progress is being made in developing the films of zeolites. This is a difficult task.

- Crystalline alignment is critical. However, the presentation glossed over details and should have referenced work done by others where relevant.
- Good work in physical property study of the membranes.
- There has been good progress in synthesizing the test membranes, which in experiments with single gases, show potential selectivity for H_2 separations. As the PI also noted it's important to also perform the testing with mixed gas feeds.
- A key finding is the generally higher permeance of CO_2 versus H_2, the opposite of what would be expected on the basis of these molecules' relative sizes and is based on a surface flow phenomena on the more strongly adsorbed CO_2.
- It's important in terms of practical applications since it is fundamentally more efficient to remove the minor component (CO_2) from a H_2-rich feed stream, as in fact is done industrially using acid gas sorbents.

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

This project was rated 3.00 for technology transfer and collaboration.

- Too early on in the project.
- Some linkages with potential manufacturers e.g. Pall.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- Needs a better definition of their future approach and how current results can be improved. Too many different materials were suggested to coherently follow what is the most promising and why.
- Should focus on improving existing membranes.
- Reasonable plans for the synthesis of more selective membranes.
- Suggest exploiting the discovered selectivity of CO_2 vs. H_2 or at least understanding it better from a knowledge of the CO_2 adsorption isotherm in the membrane material and modeling using the well-known methods of R. Barrer for "surface flow" membranes.

Strengths and weaknesses

Strengths
- Very relevant area, novel work, not many others pursuing this approach.
- Good membrane materials screening plan.
- Outstanding presentation and enthusiasm.
- Good technology to continue research.
- The strength of the program is in its ability to deliver inorganic membranes which should be stable at severe process conditions where most polymers would not survive.

Weaknesses
- Membranes have not been tested under any realistic condition. Testing needed with reformatted steam-content, CO, and H_2S.
- The plausibility that this approach has a good chance of success has not been provided.
- Possible side-tracking. Too many parallel tasks may lead to lack of focus on the main goal.
- The challenge (but not a fundamental weakness) lies in "honing" the synthetic techniques to yield membranes which display much higher selectivities – at least greater than 10 and preferably 100+ which will be required for their industrial application.

Specific recommendations and additions or deletions to the work scope

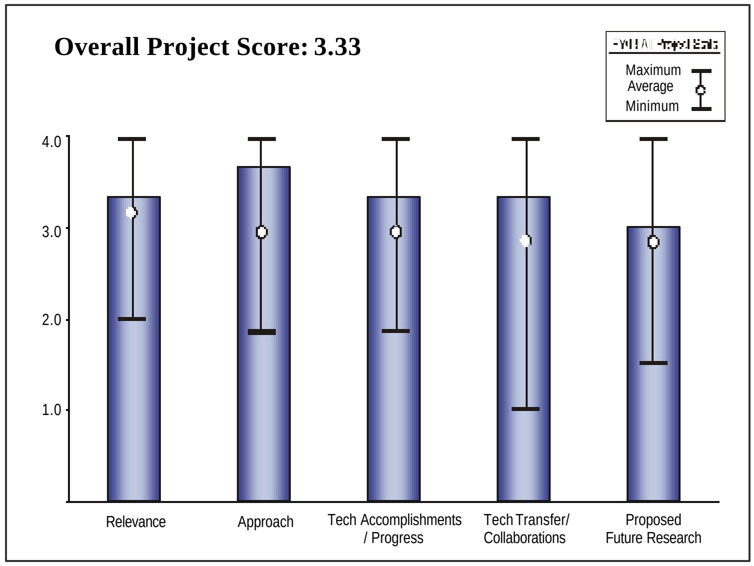
- These membranes need to be tested with realistic steam content, sulfur, etc. and at temperatures reasonable for systems. Until testing under realistic conditions, hard to know whether anything is being accomplished.

- Define specific materials and methodology to separate CO_2 from H_2.
- Consider looking into membrane performance effects in presence of nitrogen (may be N_2/H_2 mixture) since most reformate gas streams will have N_2 in them.
- Consider improving permeation rates to match H_2 delivery demands.
- Needs more focus on targets.

Project #12: Maximizing Photosynthetic Efficiencies and H_2 Production in Microalgal Cultures
Melis, Tasios, UC Berkeley

Brief Summary of Project

In this project, in order to maximize the photosynthetic efficiencies and H_2 production in Microalgal cultures, the University of California at Berkeley developed genetically engineered microalgae with enhanced photosynthetic solar conversion efficiencies and biomass/hydrogen production capabilities under mass culture conditions. The approach adopted was to apply DNA insertional mutagenesis and screening in the model green alga Chlamydomonas reinhardtii for the isolation of 'truncated Chl antenna' transformants and apply biochemical, genetic and molecular analyses of the transformant cells, followed by DNA sequencing to identify genes that confer a 'truncated Chl antenna size'.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.33 for its relevance to DOE objectives.

- Project, in collaboration with NREL, has taken a giant stride toward making biophotolysis of water an attractive alternative for large scale hydrogen production.
- Reducing antenna size is needed for any effective use of microalgae, so the success of this project will impact not only H_2 products, but also other alternative uses of the organism, including sequestration, waste treatment, and other biofuels.
- This supports ongoing efforts in photobiologic H_2 production. This is a long term effort and therefore does not correlate well with individual targets and goals but is consistent with the overall program plan.

Question 2: Approach to performing the research and development

This project was rated 3.67 on its approach.

- Project focused on major technical barrier of wasteful absorption of light by a surface algae which is dissipated as heat. Light could not penetrate to subsurface algae.
- This is an appropriate approach and the new mutant indicates there is still progress to be made.
- The approach is well thought out and appears to address most of the immediate and near-term barriers in this area.

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

This project was rated 3.33 based on accomplishments.

- Effective penetration of light is now up to 15 cm, with potential for even deeper penetration in algae bed. Modified algae are much more amendable to photobioreactor or shallow pond systems.
- Good progress.
- Future direction and objectives appear to be well thought out.

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

This project was rated 3.33 for technology transfer and collaboration.

- Professor Melis has a close collaboration with NREL and, if project proposed by ORNL is approved, then collaboration with ORNL would also be appropriate.
- More work is going on in Japan than is indicated in 4B.
- PI appears to be making a good effort to collaborate with NREL and others in the field and this is to be encouraged.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Is automated laboratory equipment available that would be of significant help in moving project forward?
- Is cost and effectiveness easily justified?
- Well integrated scientific approach.
- This is a long-term program in which specific goals have been set for future work that need to be maintained otherwise work may go on indefinitely.

Strengths and weaknesses

Strengths
- The three biohydrogen (microalgae) projects are very synergistic.
- This one has the broadest applicability.
- The identification of a regulatory gene is potentially very exciting scientifically.
- Very good approach but this is long-term high risk R&D and even if successful it may not affect near term H_2 production goals (2015).
- This work might be a better match for office of science.

Weaknesses
- None specified.

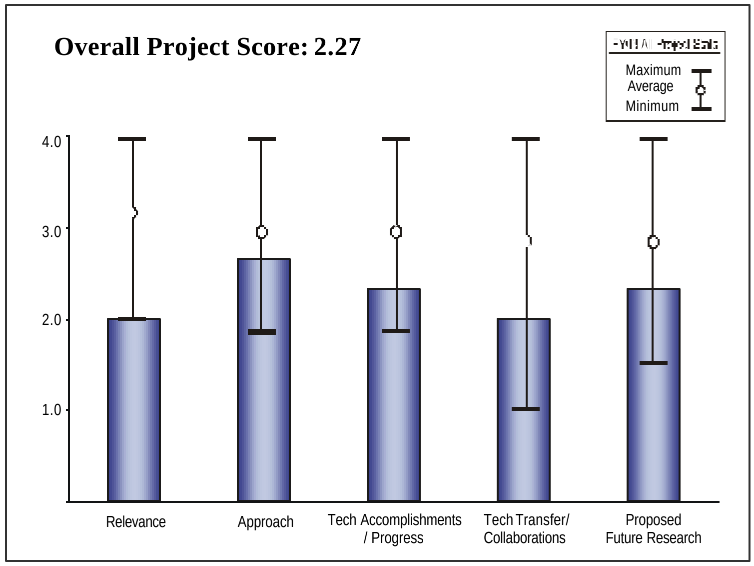
Specific recommendations and additions or deletions to the work scope

- Project should be funded at optimum level for timely progress.
- As long as there remains hope that microalgae can offer a realistic amount of H_2, work like this should be pursued.
- Encourage continued collaboration with others (NREL) in this area.

Project #13: Reformer Model Development for Hydrogen Production
Bellan, Josette, JPL

Brief Summary of Project

In this project, the California Institute of Technology is developing a reformer model for hydrogen production. The model is intended to be free of empiricism, as much as possible. The approach of the project is to resolve all scales of the flow in a small domain to understand the physics of the drop/flow interaction, develop small-scale and large-scale models and finally validate the models with experimental data.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.00 for its relevance to DOE objectives.

- Work needs to be more directly tied to actual reformer design and data.
- This project has the potential to solve a critical problem in the development of low cost hydrocarbon reformers.
- In order to improve fuel economy, the fuel must be vaporized and mixed in a heated (up to 500^{o}C) steam/air stream in a minimum amount of space and time.
- Development of a model for full boiling range fuels could be very helpful in designing an optimum system. The one drawback is that the model may not arrive in time, and the simple empirical solutions may already be well established.
- The goal is overly ambitious and the probability of success is low.

Question 2: Approach to performing the research and development

This project was rated 2.67 on its approach.

- The approach does address some barriers with respect to reformer efficiencies but the approach is very fundamental and its impact on actual reformer effects is still a question.
- Too much reliance on computational methods without empirical verification.

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

This project was rated 2.33 based on accomplishments.

- Good progress made on model but the program needs to be applied to real systems.
- Project appears to be on track.
- It is difficult to see how their project will contribute to reformer design.

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

This project was rated 2.00 for technology transfer and collaboration.

- Needs more collaboration with equipment designers and manufacturers.
- To be outstanding, the researchers need to coordinate with companies (industry) developing fuel reformers.
- No collaboration noted with companies that might make reformers.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.33 for proposed future work.

- Needs more collaboration with equipment designers and manufacturers.
- No defined decision point.

Strengths and weaknesses

Strengths
- This project has the potential to be very useful, if it arrives in time and is linked to real fuel reformer designs.
- Computational methodologies.

Weaknesses
- No end point in sight that would lead to development of more efficient/lower cost reformers.

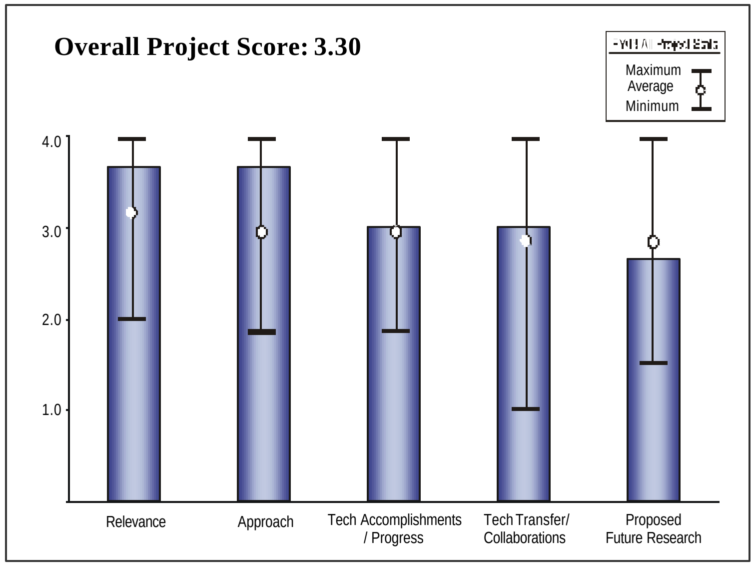
Specific recommendations and additions or deletions to the work scope

- Needs more interaction with designer and equipment manufacturers to see if the developed models are truly adding to real improvements or just advancing the science.
- Consider modeling other full range boiling fuels such as gasoline and jet.
- Recommended no further funding.

Project #14: Photoelectrochemical H_2 Production
Miller, Eric, University of Hawaii

Brief Summary of Project

In this project, the University of Hawaii worked towards developing high efficiency, cost-effective photoelectrochemical processes for the production of hydrogen by engineering stable multi-junction photoelectrodes based on low-cost materials and designing, fabricating and testing optimized photoelectrodes suitable for eventual commercial-scale use.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.67 for its relevance to DOE objectives.

- Nice presentation of connection. The reviewer thinks photoelectrochemical technology is an important long-term goal of the program.
- Long-term R&D is needed on this area - 2010 goals are very aggressive.
- Miller's broad approach to hydrogen production with photoelectrochemical (PEC) technology is the best service to the President's goal/DOE objectives since it is cheap, renewable, and potentially massive in scale.

Question 2: Approach to performing the research and development

This project was rated 3.67 on its approach.

- Approach seems reasoned and clear. However, better integration with other materials research should be considered.
- Good progress is being made in evaluating new material candidates but significantly more progress will be needed to meet 2010 goal.
- Miller has a broad grasp of the key barriers including efficiency both in semiconductor performance, catalyst/electrolysis efficiency, and cost involved in a successful PEC program.

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

This project was rated 3.00 based on accomplishments.

- Connection to overall goals is weak.
- H_2 production is measured as electric current. Conversion to standard units (kW, SCFs) would be useful.
- Good progress is being made.
- They have tested more new materials such as Fe, W, and Ti and contributed more to the knowledge of hydrogen and oxygen electrodes than anyone recently and are making good progress toward a feasible system. This should continue.

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

This project was rated 3.50 for technology transfer and collaboration.

- This project would benefit from better coordination or task sharing with other material research and maybe combinatorial discovery.

- Appears to be coordinated well with others in the area (NREL). More coordination is encouraged.
- Many important collaborations going simultaneously.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- No details provided.
- Meeting 2005 goals should require more careful roadmapping.
- I see continued improvements in this area being made but I do not see the type of improvements needed to meet 2010 goals and beyond.
- Very good future plans.

Strengths and weaknesses

Strengths
- Good work.

Weaknesses
- More emphasis is needed on scaling-up to a complete PEC system as well as finding better materials.

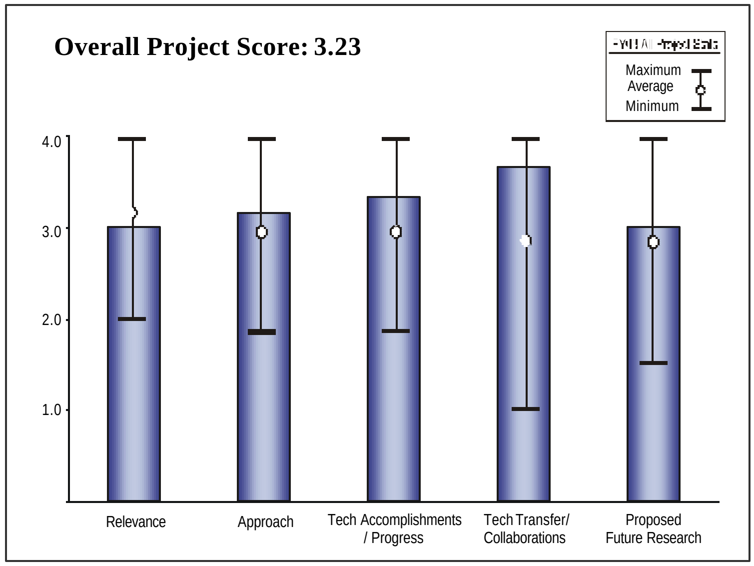
Specific recommendations and additions or deletions to the work scope

- Keep up the coordination and collaborations with others in field.
- Work may need a more specific Go/No-go point prior to 2010 goal.

Project #15: Photoelectrochemical Water Splitting
Turner, John, NREL

Brief Summary of Project

In this project, NREL worked on the development of photoelectrochemical systems for hydrogen production by identifying and characterizing semiconductor systems for higher efficiency water splitting. Identifying new semiconductor materials with bandgaps in the ideal range, catalyzing the surfaces and engineering the band edges of the identified material and determining if the existing PV device structure could be easily modified to affect the direct splitting of water was the approach adopted for this project.



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

This project earned a score of 3.00 for its relevance to DOE objectives.

- No time-frame linkage to production goals.
- No discussion on how it supported PEC project goals.
- Relevant but this work may not arrive in time to impact current H_2 fuel initiative.
- The photoelectrochemical approach for hydrogen generation is relevant to the highest degree by potentially supplying the fuel for vehicles and stationary fuel cell power renewability and with a price near any competitive technology. It is more GHG neutral that any fossil fuel dependent method as well.
- This project has a correct vision for future goals.

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

This project was rated 3.17 on its approach.

- Project may benefit from collaboration with other materials research efforts.
- Their approach is generally well planned short term but it could use some longer range planning.
- Turner's approach involves finding better semiconductors for the very specific requirements of water electrolysis in the photoelectrochemical device and shows the clearest possible understanding of the needs and limitations of the technology and the electrochemistry of the system.
- Concentrating on the semiconductor and PEC device has left him less time for catalyst questions - developing the electrodes for the O_2 and H_2 generation.
- The key technical barriers are addressed in this work.
- Cost has not been addressed as much but is part of the future plans.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.33 based on accomplishments.

- Technical presentation of results excellent. Presentation would have benefited by a summary of accomplishments and progress with a discussion of reaction to overall goals.
- Lifetime goals are a real challenge.
- There is not much progress being made in the area. May need to set up more specific intermediate goals.

- Very good and significant progress has been made in testing and starting to develop new types of semiconductor PEC materials for hydrogen generation. Testing along these lines must continue.

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

This project was rated 3.67 for technology transfer and collaboration.

- A wide array of participants is shown. Further participation with industry is suggested. Gain the interest of more solar manufacturers if possible.
- Coordination appears to be very good.
- Turner is one of the best resources for collaboration and gives industry and other labs a clearer and convincing explanation of his own as well as all areas of renewable hydrogen/electrical energy research and development. A great apostle of the program to a skeptical industrial world.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- This project eventually needs to cull some paths to concentrate on most promising ones.
- While I see good progress being made and specific 2005 and 2010 targets, I don't see a real off-ramp identified here.
- Future plans are very good.
- Efforts should continue to include amorphous silicon photoelectrodes as well as other newer materials.

Strengths and weaknesses

Strengths
- Good R&D.

Weaknesses
- Work can be more focused for the long-term (off-ramp or Go/No-go point).
- More emphasis on the whole PEC system may seem premature but could also be addressed. This would involve more study of catalyst and counter electrodes.

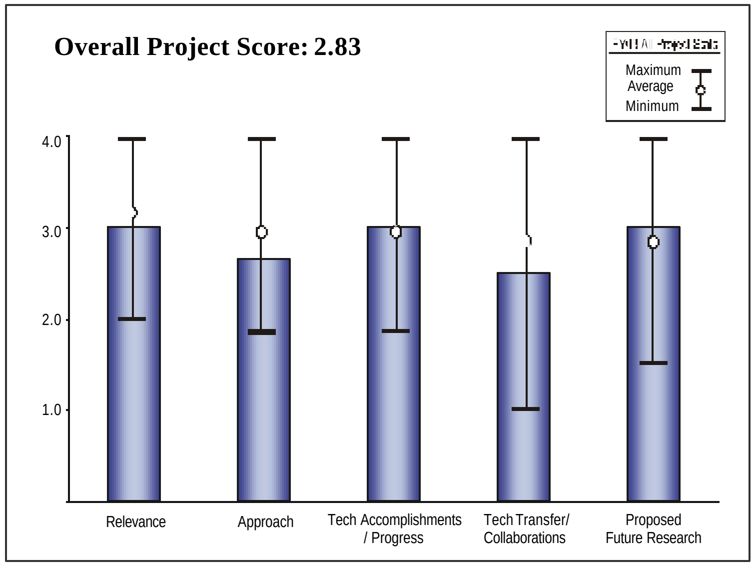
Specific recommendations and additions or deletions to the work scope

- Possible participation in materials research.
- Database proposed for combinatorial projects.
- Some real targets need to be developed in this area.
- The differences between 2005 and 2010 targets are substantial.
- Without some intermediate goals, the work will go on for 5 years without an off-ramp strategy.

Project #16: Encapsulated Metal Hydride for H_2 Separation
Heung, Leung, SRTC

Brief Summary of Project

In this project, Westinghouse Savannah River Technology Center is developing a separation membrane made of sol-gel encapsulated metal hydride packing material that will adsorb hydrogen selectively, not break down to fines, and tolerate reactive impurities. It is also evaluating selected packing material for hydrogen separation in a small-scale column. The approach adopted is to develop formulations and procedures to make silica encapsulated metal hydride composite material and test selected samples for hydrogen separation from feed streams of different compositions.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- H_2 separation from reforming streams is a critical part for DOE objectives.
- More background on specific applications may help focus future development efforts.

Question 2: Approach to performing the research and development

This project was rated 2.67 on its approach.

- Metal hydrides have the potential to play bigger roles in H_2-purification, especially in low concentration streams.
- The research should look to answer a more involved analysis to tolerance from impurities in low H_2 concentration feed gas. How will the hybrid system connect with and interact with a gas purifier? Will a purge of the system hood re-occur before the hydrogen is used as fuel?

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

This project was rated 3.00 based on accomplishments.

- Need to increase gas pressure tests.
- Effect of encapsulation on adsorption/desorption rates needs to be conducted.

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

This project was rated 2.50 for technology transfer and collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- A more detailed plan should be developed.

Strengths and weaknesses

Strengths

- Good methodical approach of synthesizing the membranes.

Weaknesses

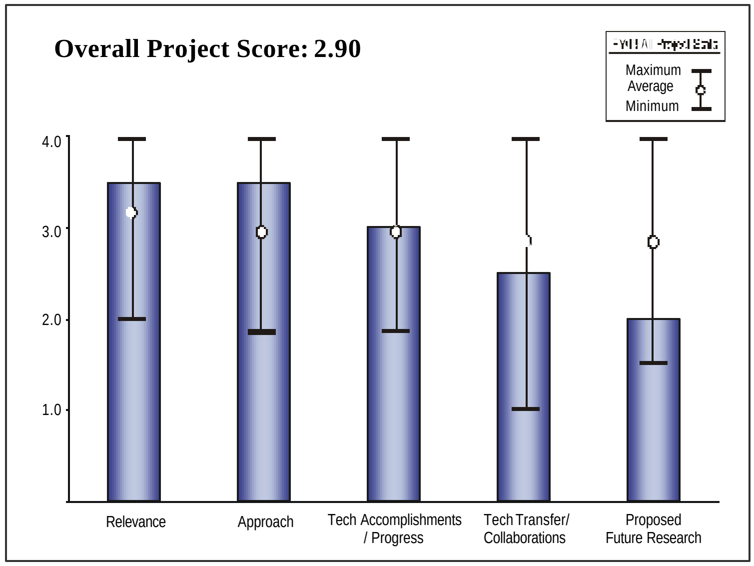
- There is no clear characterization plan for the life cycle, durability, and effect of H_2O etc.

Specific recommendations and additions or deletions to the work scope

- Consider using a higher plateau and low-cost metal hydride base such as Mischmetal based alloys.
- Increase gas feed pressure.
- Consider a flow-through reactor design for the separation process.

Project #17: Economic Comparison of Renewable Sources for Vehicular Hydrogen in 2040
Myers, Duane, DTI

Brief Summary of Project



DTI conducted a series of analyses addressing the delivery of 10 quads of H_2 from renewable sources in 2030-2050 for the U.S. transportation sector, with respect to resource availability, demand, cost, and distribution pathways. The project provided insight into a hypothetical hydrogen infrastructure for vehicles, with the hydrogen supplied from predominantly domestic resources as well as identifying cost barriers that must be overcome to achieve high utilization of renewable resources for hydrogen production.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.50 for its relevance to DOE objectives.

- Great state by state analysis on the cost of delivering hydrogen from renewable sources. Provides extremely valuable insight on the economic penalties of moving hydrogen around and the cost of hydrogen (renewable) in the future.
- Logical analysis.

Question 2: Approach to performing the research and development

This project was rated 3.50 on its approach.

- The state by state analysis is extremely valuable in developing new research programs. It provides guidance on relative costs.

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

This project was rated 3.00 based on accomplishments.

- Report demonstrates that there are serious economic barriers to current status of renewable hydrogen in states with large populations.
- Results not very useful in overcoming barriers.

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

This project was rated 2.50 for technology transfer and collaboration.

- Minor effort for coordination.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.00 for proposed future work.

- No proposed future work plans.

- Nothing new was said.

<u>Strengths and weaknesses</u>

Strengths

- Excellent study.
- Shows that lower cost hydrogen transportation needs to be funded or that low cost solar (thermal?) routes to hydrogen need to developed. Otherwise in 2040, non-renewable sources for hydrogen will dominate.
- Comprehensive analysis.

Weaknesses

- No new information provided.

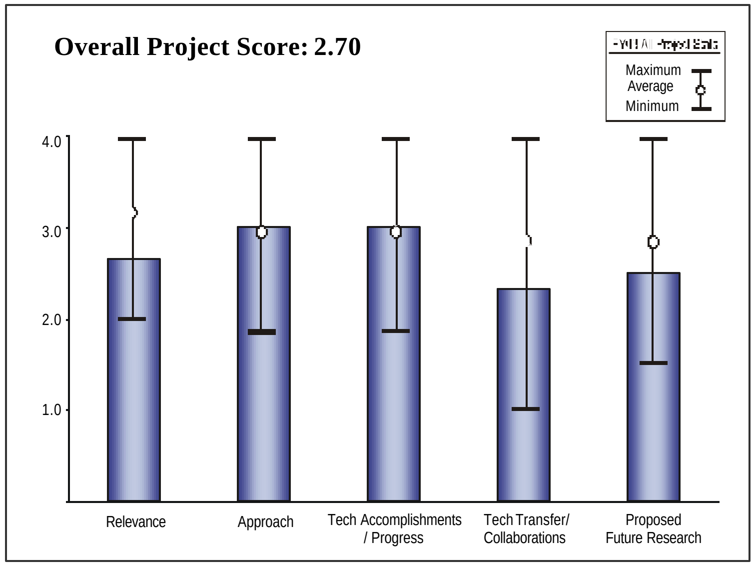
<u>Specific recommendations and additions or deletions to the work scope</u>

- Such projects should be discontinued unless there are sufficient new developments to warrant more work.

Project #18: Biomass-Derived H_2 from a Thermally Ballasted Gasifier
Brown, Robert, Iowa State University

Brief Summary of Project

This project was carried out by Iowa State University to determine the feasibility of switchgrass as a suitable fuel for the ballasted gasifier and ways to maximize hydrogen production, remove contaminants from producer gas, mediating the water-gas shift reaction in the product gas and evaluate its economics. This was achieved by preparing switchgrass fuel, feeder, gas sampling, a 5-ton per day bubbling fluidized bed gasifier and a latent heat ballasting system and performing gasification trials.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.67 for its relevance to DOE objectives.

- Overall process does not seem to offer economic or technical advantages over current processes. Individual processing steps could replace inferior steps in other gasification processes.
- The objective of the project is to use the thermal gasification of switch gas to develop hydrogen from biomass.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Gas cleaning process and gas conditioning system could be useful in other gasification processes.
- Is there a preliminary economic evaluation that supports development of the gasifier or overall process for blower feeds?
- The approach is the empirical determination of the efficiency of H_2 production from a technically balanced gasifier.
- Indirect heating approach valuable. Value - optimal use of thermal energy and chemical energy in switchgrass - may provide replicable gasification model for other biomass feedstocks.

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

This project was rated 3.00 based on accomplishments.

- Progress has been made on certain processing steps and analytical technologies.
- An answer to the question, " why are you doing this work", would provide clarity to the program.
- Several runs have been performed using tar cracking, steam reforming, and the water gas shift reaction. The results showed that all metal catalysts used in tar cracking were effective; however, creating pore blocking may be a problem and could deactivate the catalyst.
- Some parametric studies were performed. It seems that the low temperature gas shift favors H_2 production.
- Reasonable H_2 production but the costs are high in relation to the economic targets. How do flow rates compare with similar and/or combining technologies?

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

This project was rated 2.33 for technology transfer and collaboration.

- Doing the experimental work is obviously good training and expertise for students and post doctorates.
- The project is not yet in the technology transfer stage. Several collaborations are listed.
- Look for further industrial collaborators.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Future plans are processing step improvements. Clear performance targets for the processing steps are needed.
- The projected future activities are reasonable in light of what was accomplished.

Strengths and weaknesses

Strengths
- Good idea as well as a very qualified PI.
- This project shows simplicity and has a low temperature.

Weaknesses
- Highly emp irical.
- No provisions made for future scaling up and demonstration.
- Cost is a weakness.

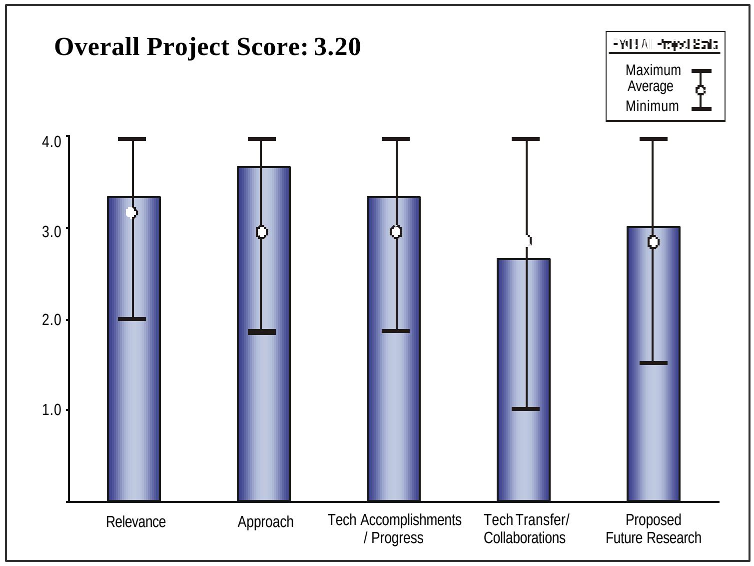
Specific recommendations and additions or deletions to the work scope

- I believe Professor Brown should be funded but a refocusing of goals for the project is needed.
- Continue the work but with a modeling component that will allow the understanding of the system for eventual scale-up and optimization.

Project #20: Evaluation of Protected Metal Hydride Slurries in a H_2 Mini-Grid
Lasher, Steven, TIAX

Brief Summary of Project

TIAX is conducting a study of promising hydrogen purification technologies that DOE does not currently fund. TIAX will determine the technical maturity of such promising technologies, the risks associated with them, and complete a detailed comparison of the performance and cost of these technologies integrated into a hydrogen fueling station concept relative to baseline technologies.



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

This project earned a score of 3.33 for its relevance to DOE objectives.

- Natural gas based H_2 filling stations will be built first.
- We need to explore novel hydrogen distribution options and this is an inexpensive way to do so.
- Comprehensive.

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

This project was rated 3.67 on its approach.

- Good concept of incorporating a small scale power plant.
- The project is laid out logically. It appears that it is being conducted efficiently.
- It draws a similar work for input data.
- Excellent theoretical research.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.33 based on accomplishments.

- Addresses novel distribution schemes for H_2.
- Excellent work on sensitivities.
- Photos of actual equipment would improve project.

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

This project was rated 2.67 for technology transfer and collaboration.

- Coordination with industry for costs (not much required).
- Need heavy duty vehicle collaboration for fuel cell work.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- Work on developing larger scale stations i.e. 1000 kg/day is very important.

- Should include some investigation into possible 'show-stoppers'.

<u>Strengths and weaknesses</u>

Strengths

- The energy station concept helps reduce capital expenditures.
- Explores novel distribution techniques.

Weaknesses

- Needs to build on the concept of providing power to local businesses. Local businesses could realize a cost saving over grid power.
- Does not appear to take into account relative energy needed for distribution of hydrogen.

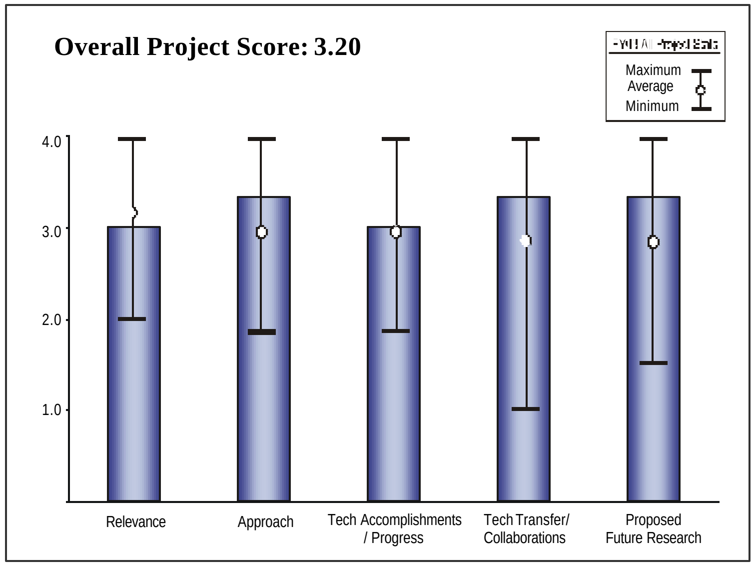
<u>Specific recommendations and additions or deletions to the work scope</u>

- Larger stations need to be considered.
- Investigate potential pipeline corrosion from hydrogen and potential for degradation due to thermal cycling.

Project #22: Novel Compression and Fueling Apparatus to Meet Hydrogen Vehicle Range Requirements
Carlson, Todd, Air Products & Chemicals Inc.

Brief Summary of Project

The objective of this project by Air Products, Inc. was 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 barg storage valves for cascade, flowmeter, 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 3.00 for its relevance to DOE objectives.

- Improved compression and filling will benefit the infrastructure development, but the impact of this technology is not revolutionary. Nevertheless, the concept is novel and should be pursued.
- Air Products is one of the leading developers of H_2 refueling systems.
- The project is exploring an innovative approach to compressing H_2 from conventional 2200 psi on a tube trailer to about 7500 psi for refueling station.
- This is a potentially simpler and lower cost process.
- The identification and validation of existing high pressure components is the most needed part of project. Identifications of deficiencies and correction is second. Development of a new pump is much lower.

Question 2: Approach to performing the research and development

This project was rated 3.33 on its approach.

- The novel idea of using liquid compression is interesting. The design method describes modeling of the dynamics of the process, but shows no results to suggest how the process might approach the isothermal limit for the p-dv work on the H_2 gas.
- Focus is on developing the novel compression process, but issues of cost and safety and reliability are not being overlooked.
- Until the weakness of current compression technology is identified and how a new compressor would solve, work on a new compressor is not needed. The rest of the effort related to the valves, in statements etc., is needed as is testing to determine many limitations in life and reliability.

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

This project was rated 3.00 based on accomplishments.

- The project is just getting underway, so it's a bit premature to judge progress. The critical testing and a Go/No-go decision occurs this summer.
- A recent project start data, but progress is already evident. Better assessment of progress can be made at later review meetings.
- Appears to have just started and a clear path to new compressor seemed very uncertain. They have changed their approach to the original design and are now looking for another.

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

This project was rated 3.33 for technology transfer and collaboration.

- A number of collaborations are listed, with the CaFCP an important link to OEMs. A couple of the collaborations are not specific, so they are yet to be established.
- This is a new start, and collaborations could be established after technical and economic feasibility are well demonstrated.
- Seems to have appropriate players on manufacturing team.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.33 for proposed future work.

- Most of project is future work at this point in time.

Strengths and weaknesses

Strengths
- A leader in hydrogen production and developer of hydrogen refueling facilities.
- A novel and potentially lower cost method for compressing hydrogen to the levels needed for vehicle refueling.
- No significant weaknesses.

Weaknesses
- None specified.

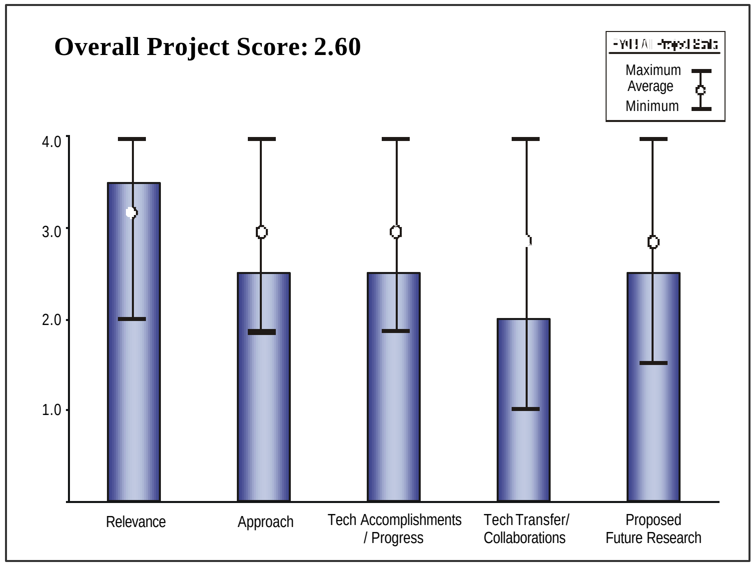
Specific recommendations and additions or deletions to the work scope

- There should be a collection of 'lessons learned' from all the filling stations. This can be used to scope-out future compressor related R&D.

Project #30: Techno-Economic Analysis of H_2 Production by Gasification of Biomass
Bowen, David, GTI

Brief Summary of Project

In this project, Gas Technology Institute analyzed the production of hydrogen by gasification of biomass, identified the economic and technical barriers associated with it. A simulation to quantify hydrogen production was developed, various cases were tested for optimizing hydrogen production with the simulation and the results were analyzed to determine its technical and economic feasibility.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.50 for its relevance to DOE objectives.

- Biomass could be a useful source of hydrogen in the future.
- Good information but little new work.

Question 2: Approach to performing the research and development

This project was rated 2.50 on its approach.

- Need to consider cost of cleaning reformer gas. The cost of produced H_2 appears to be lower than the estimates made by other groups.
- Identifies problems but does little to identify solutions.

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

This project was rated 2.50 based on accomplishments.

- Needs to build a working pilot plant to understand potential show stoppers.
- Also needs to consider sources of biomass that could be provided year round.
- Contributes little not already known.

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

This project was rated 2.00 for technology transfer and collaboration.

- Need to collaborate with groups working on biomass gasification.
- Minor coordination.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- The most important step is to get into the lab.
- No decision points are evident.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- None specified.

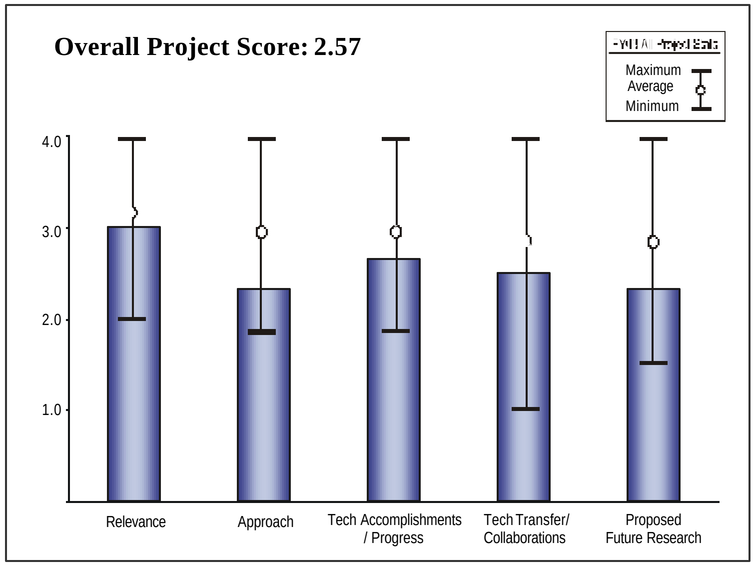
Specific recommendations and additions or deletions to the work scope

- A full scale pilot plant needs to be built. This will help in identifying potential show stoppers such as the formation of dioxins in the gasifier or reformer. The gas clean up cost may be a lot higher than anticipated. Finally, should consider running a series of crop wastes that can be provided year round.
- Recommendation: discontinue funding.

Project #31: Supercritical Water Partial Oxidation
Spritzer, Mike, General Atomics

Brief Summary of Project

In this project, General Atomics presented the feasibility of supercritical water partial oxidation (SWPO) in pilot-scale preliminary testing. Other objectives of this project include developing improvements to SWPO hardware, optimizing SWPO operating parameters and hydrogen yields and demonstrating and integrating SWPO pilot-scale system including hydrogen separation. To achieve these objectives, various feedstocks and reactor designs were tested and improvements were identified.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Potential economic data do not appear attractive. Feeding oxygen, fuel, suspending agent to reactor, separation of product methane, and steam reforming product methane are economic burdens to the process. Can the economic burden be overcome?
- This is a very good project and certainly very relevant to the H_2 program.

Question 2: Approach to performing the research and development

This project was rated 2.33 on its approach.

- Wouldn't you expect the price of the proposed feedstocks to decrease once a use is identified?
- The approach is empirical but it does have, as a basis, the PI's experience of many years.

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

This project was rated 2.67 based on accomplishments.

- Process operation in 650-800^oC range appears to make only slightly more H_2 than CH_4 (molar basis). Would you separate H_2 and CH_4 before reforming of feed H_2 and CH_4 both to reformer? The product gas shows significant amounts of O_2. Is the excess O_2 necessary and, if so, is that a problem?
- What do you see as the technical and economic barriers?
- Low percentage makes for a high economic hurdle.
- Does the business plan quantify municipal waste opportunity?
- Given the restricted amount of funds, the progress has been significant. The PIs have also leveraged the funds against other projects.

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

This project was rated 2.50 for technology transfer and collaboration.

- Although collaboration will take effort, it is not just worthwhile but essential.
- This is an applied project that is in need of technology transfer.

- There do not appear to be any outside-of-the-company collaborations.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 2.33 for proposed future work.

- Reviewer does not see key technical and economic barriers identified based on past results. Those barriers need to be identified and addressed.
- Does business plan quantify municipal biomass opportunity? A scale-up is needed to take advantage of market potential, accompanied by techno-economic analysis.
- The next stage of research seems reasonable.

<u>Strengths and weaknesses</u>

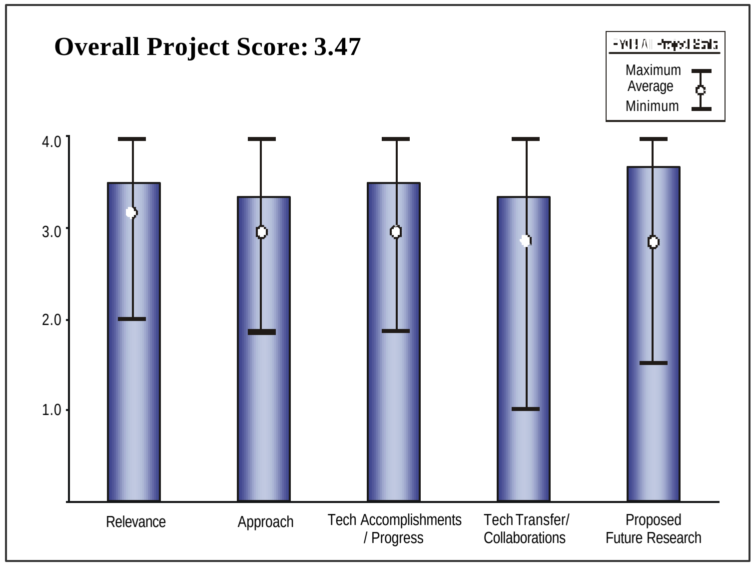
Strengths
- This project has market potential because of the PI's previous experience; their commitment to success; a very favorable economic analysis; and the feed used is thought to be processed under different conditions.

Weaknesses
- Cost seems to be a barrier.
- Completely empirical approach.

<u>Specific recommendations and additions or deletions to the work scope</u>

- Recommend not funding.
- Pursue industry cooperation on demos.
- The project should be continued, but with an added modeling component to strengthen the physical understanding and enhance the chances of success for 1) long term outreach 2) optimization and 3) scale-up.

Project #32: Development of Efficient and Robust Algal Hydrogen Production Systems
Lee, James, ORNL



Brief Summary of Project

In this project, ORNL studied the production of algal photosynthetic hydrogen and its barriers. The technical issues are discussed and potential solutions, based on a novel approach developed by ORNL, to overcome the barriers are also presented. Significant interactions and collaborations with other laboratories and universities have been set up to achieve this common goal.

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

This project earned a score of 3.50 for its relevance to DOE objectives.

- While the approach is only likely applicable to two-stage biophotolysis, it promises to be an enabling technology that solves several problems at once.
- Potentially, this project is very important.
- The project, if successful, would be a major breakthrough toward making biophotolysis an economically viable route to fuel hydrogen.
- Supports long-term H_2 production goals but this is very much a longer term R&D activity.

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

This project was rated 3.33 on its approach.

- Uneasy about designing a new polypeptide because there may be additional hurdles. But the basic approach is novel, sound, and even exciting.
- The project is closely integrated with the projects at NREL and UCB.
- Very well planned to meet initial objectives.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.50 based on accomplishments.

- Nothing done yet which is not the PI's fault.
- Project, if successful, would eliminate the proton gradient by genetic insertion of proton channels in the thylakoid membranes.
- New project but good progress appears to be made already.

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

This project was rated 3.33 for technology transfer and collaboration.

- Good interactions with NREL, potentially with Berkeley.
- Will work closely with NREL and UCB utilizing advanced organisms already developed by those groups.
- Appears to be aware of other work in area which encourages further collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.67 for proposed future work.

- If successful, this approach will overcome hurdles in biophotolytic production of H_2.
- The team should work on natural isophores, such as the brown adipose tissue peptide and perhaps melittin in addition to their synthesis approach.
- Project would reach Go/No-go decision point at the two year mark.
- Good future plans but this again appears to have a very long timeline toward commercialization.

Strengths and weaknesses

Strengths
- Good basic R&D.

Weaknesses
- A new program - good that PI is becoming an independent PI, but this seems to be outside his expertise.
- Needs to keep a close eye to ensure the goals are really addressed.
- No one else is doing this and it is very appropriate to DOE biohydrogen research.

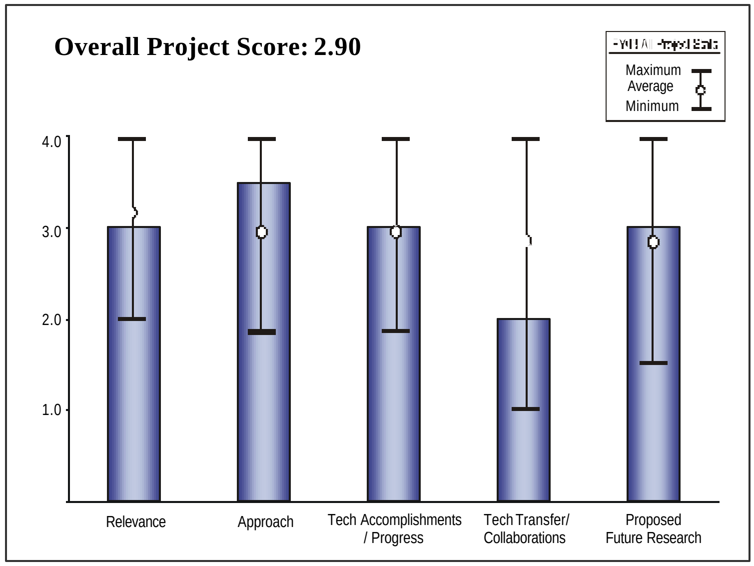
Specific recommendations and additions or deletions to the work scope

- Needs a milestone by next review.
- Project must be funded.
- This may need to be funded by basic R&D rather than H_2 programs.
- Timeline may be longer than President's goal.

Project #34: Water-Gas Shift Membrane Reactor Studies
Enick, Robert, University of Pittsburgh

Brief Summary of Project

In this project, University of Pittsburgh is developing a water gas shift (WGS) membrane reactor to produce and separate hydrogen from fossil fuels for downstream uses. The WGS kinetics and membrane flux was evaluated using industrial gas mixtures and conditions.



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

This project earned a score of 3.00 for its relevance to DOE objectives.

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

This project was rated 3.50 on its approach.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

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

This project was rated 2.00 for technology transfer and collaboration.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

<u>Strengths and weaknesses</u>
- None specified.

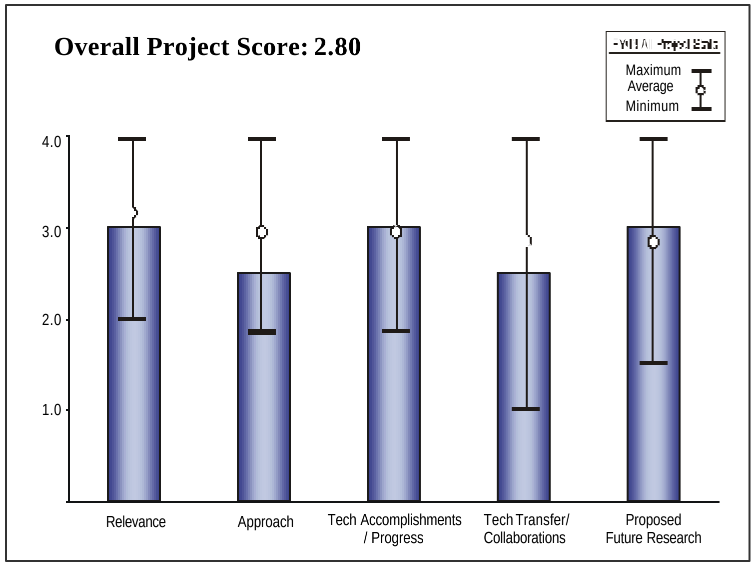
<u>Specific recommendations and additions or deletions to the work scope</u>

- None specified.

Project #38: Low Cost, High Efficiency Reversible FC Systems
Ruhl, Robert, Technology Management Inc.

Brief Summary of Project

In this project, Technology Management, Inc. worked on improving the performance of reversible stacks by reducing area specific resistance, rate of degradation, and seal leakage. Alternate materials and geometric factors were evaluated to reduce cell resistance and reduction on operating temperature was considered to improved life and performance. An integrated fuel cell hot subassembly operating with a stack of about 50 reversible type cells were to be tested and its economic impact and potential applications were analyzed.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Reversible system is interesting but not essential.
- Some compromises may be necessary for dual mode operation.

Question 2: Approach to performing the research and development

This project was rated 2.50 on its approach.

- Not sure if areal resistance hurdle has a clear plan to overcome.
- This mode of operation in electrolysis mode requires a high electrical input to internally generate the necessary temperatures for operation of the solid oxide system. This defeats a potential advantage of high temperature operation; reduction in electrical requirements.

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

This project was rated 3.00 based on accomplishments.

- Not clear how much of a hurdle it will be to get from 20 to 50 cells/stacks.
- Appears to have reasonable progress.
- Decline on the 5%/1000 hour is still substantial and it is not clear how this would be addressed
- The cost estimates provided for cheap hydrogen are not credible.

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

This project was rated 2.50 for technology transfer and collaboration.

- Appears to rely highly on government sponsorship.
- Some interactions but not with commercial companies.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Would help to know more specifics on how they will address the technical issues.

Strengths and weaknesses

Strengths
- Cutting edge work.

Weaknesses
- Not convinced they can meet targets.

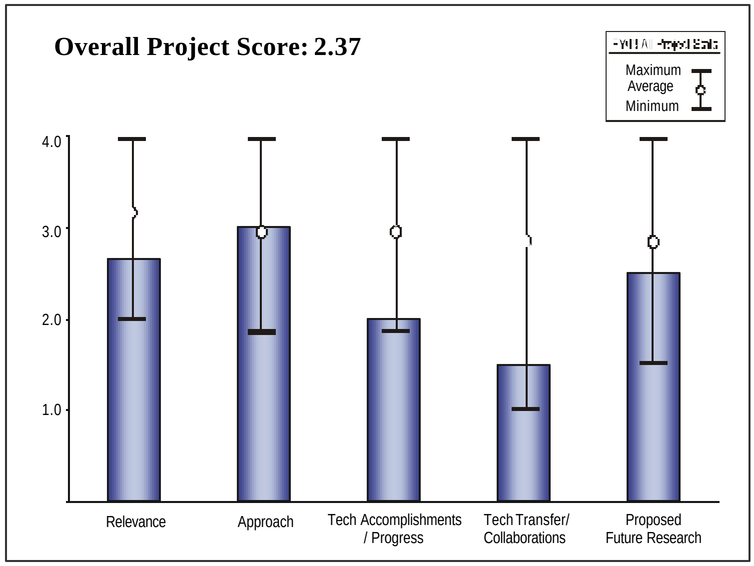
Specific recommendations and additions or deletions to the work scope

- None specified.

Project #39: High-Efficiency Steam Electrolyzer
Vance, Andrew, LLNL

Brief Summary of Project

The objectives of this project by LLNL were to build a prototype steam electrolyzer with high hydrogen production efficiency, using the concept of natural gas as anode depolarizer to reduce the electrical energy required. The approach adopted was to evaluate new materials for the electrodes, develop low cost electrolyzer tube fabrication processes, develop ceramic-to-metal seal for easy gas manifolding and design tubular electrolyzer stack for high-pressure operation.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.67 for its relevance to DOE objectives.

- The program is primarily driven by the premise that by decreasing O_2 concentration at the anode by combustion, the productivity of the system is increased. This then dictates that the electrolyzer is tied in intimately with a natural gas supply.
- Relevant as a process for generating H_2 by a combination of water electrolysis and methane combustion.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Difficulties are in funding adequately but project is very ambitious.
- The approach is novel and has good potential for perhaps limited opportunities due to the need to compile fossil and renewable resources.
- It's an interesting approach for the generation of H_2 by an electrolysis of water, the thermodynamic potential of which is reduced by a concomitant combustion of methane. It could alternatively be described as an electrically assisted reforming of methane with steam. Though fundamentally a good concept, it was unfortunately not well conveyed.

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

This project was rated 2.00 based on accomplishments.

- Progress has been retarded by funding issues and the loss of a PI so project has struggled a little.
- More should be shown about the catalytic combustion/partial oxidation aspect and catalysis development.
- I am not convinced that the CH_4 oxidation at the anode is sufficient to maintain the high temperature regulated preparation.
- Progress was disappointing in that no actual performance data was reported although some of the problems encountered in its pursuit (e.g. carbon deposition at the anode) were discussed.
- It was also disappointing that the PI was unable to state at least the potential energy efficiency of their system. i.e. ideally what would the electrolysis voltage be for their CH_4 oxidation assisted process vis-à-vis conventional steam electrolysis at the same conditions?

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

This project was rated 1.67 for technology transfer and collaboration.

- Appears to be just a feasibility study at LLNL.
- The system needs a high-temperature ceramic that is both an oxide ion and an electronic conductor. Would it not be expedient to collaborate with others who are investigating high temperature ionic transport membranes that have these properties?

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Forward plan is reasonable.
- The problem of C deposition arises fundamentally from an inadequate O ion transport, to mitigate this will require much improved ceramics. Little attention seems to be given to this. Collaboration with parties that could supply such ceramics is highly recommended.

Strengths and weaknesses

Strengths
- Novelty of approach; potential to reduce electrolysis cost.

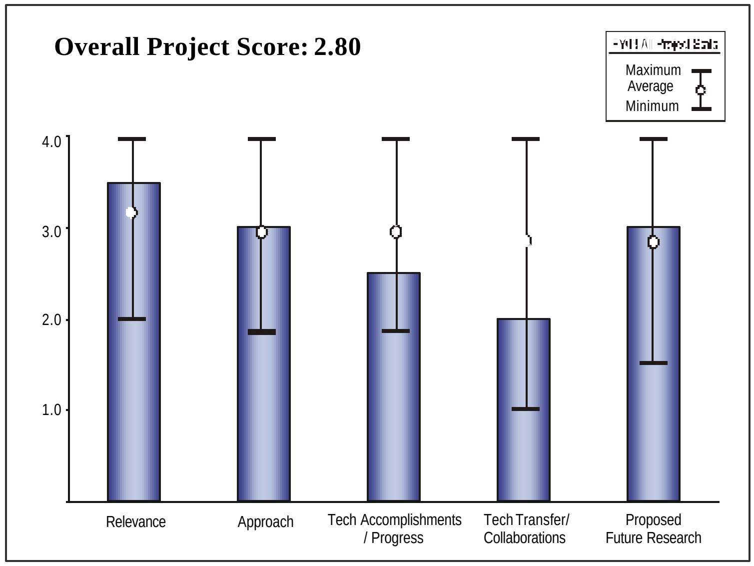
Weaknesses
- Project is tied to availability of natural gas.

Specific recommendations and additions or deletions to the work scope

- Focus on heat integration of entire system to verify CH_4 requirements for temperature maintenance and the ability to cycle the system on/off and start up time.
- Clearly define the potential and actually realized energy consumption advantage of this process vis-à-vis conventional steam electrolysis.

Project #40: High Temperature Solid Oxide Electrolyzer System
Herring, Steve, INEEL

Brief Summary of Project



Idaho National Engineering & Environmental Laboratory (INEEL) 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 electrolyser 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 3.50 for its relevance to DOE objectives.

- The project is aligned with the need to reduce electrical requirements for electrolysis and is also relevant to DOE-NE strategies.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- The approach is well thought out. However INEEL does not have the background with solid oxide-based materials and must rely on Ceramatec to provide high-efficiency anode and cathode materials (low polarization losses) as well as the electrolyte. It is not clear how much development effort will be provided by Ceramatec.

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

This project was rated 2.50 based on accomplishments.

- Progress is just getting underway. Too soon to evaluate fully but good plan in place.
- Early in the project, mostly concept development at this point.

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

This project was rated 2.00 for technology transfer and collaboration.

- Too early to tell.
- I'm not sure that SOFC key company such as a Siemens is a potential path to market.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Forward plan appears to be reasonable.

Strengths and weaknesses

Strengths
- Well aligned with mission.
- Key data can be provided to assess the true viability of the technology.

Weaknesses
- Need to assure a strong interaction takes place between the test systems and the component suppliers.

Specific recommendations and additions or deletions to the work scope

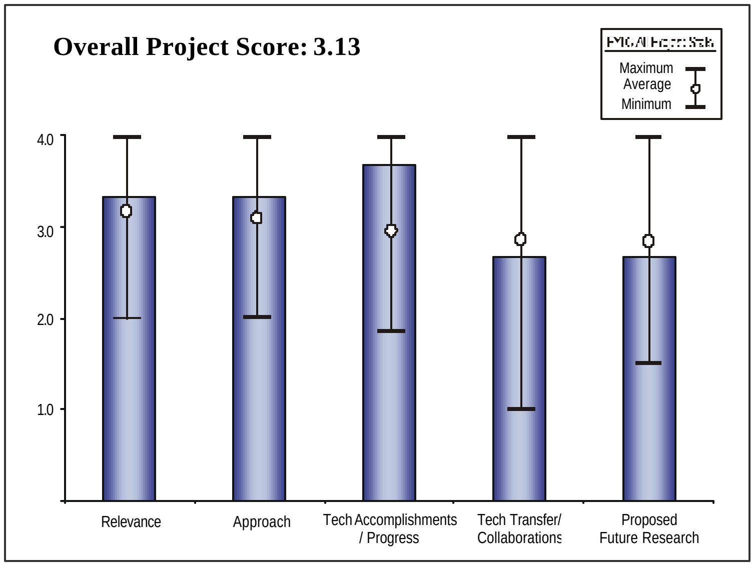
- Once the system is in place and operational, continue to push for advancing the state of the art in electrolyte and anode/cathode catalysts. Keep abreast of the field of solid oxides. The best materials are likely to be required to provide a competitive system.

Project #41: Photoelectrochemical H_2 Production Using New Combinatorial Chemistry Derived Materials

McFarland, Eric, UC Santa Barbara

Brief Summary of Project

In this project, the University of California, Santa Barbara, CA designed and built a system for automated electrochemical synthesis of combinatorial libraries of mixed metal oxides. This project also included creating and screening libraries of potential patterned metal oxides using diverse types of structure directing agents under a variety of deposition conditions.



Question 1: Relevance to overall DOE objectives

This project eamed a score of 3.33 for its relevance to DOE objectives.

- Helps meet overall objectives and should help others in this area (dual use).
- Indirect support of goals.
- Basic research efforts aimed at H_2 from renewables.
- High relevance to all DOE goals.

Question 2: Approach to performing the research and development

This project was rated 3.33 on its approach.

- Well planned, with good results to date.
- Integration with other research can be improved.
- Barriers to scale-up, analysis and searching results needs more emphasis.
- Combinatorial approach is a cost-effective way to get quickly to new materials that may break key barriers to a practical photoelectrochemical solar hydrogen system.
- New, efficient metal oxides are an important need.

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

This project was rated 3.67 based on accomplishments.

- This work is more supportive than aimed at meeting specific goals.
- Very good progress on specific goals and objectives.
- Impressive track record of building and finding candidate materials.
- Highly effective approach is making good progress in materials for photo-catalysis.
- Tungsten-molybdenum oxides very interesting.

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

This project was rated 2.67 for technology transfer and collaboration.

- I encourage even more coordination with researchers to help guide the work in this area.
- Needs shareable results library to assist analysis and prevent undesirable duplication.
- Some good collaboration mainly helping this project.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- Off-ramps for this work should be tied to other experimental work in this area.
- Needs more focused future direction. Should this be a rapid turnaround service or a dedicated research process.
- Building on current efforts to add to early progress.

Strengths and weaknesses

Strengths
- Very good, supportive work.

Weaknesses
- It is not clear that the best materials are found by a combinatorial approach as expected instead of other clues.

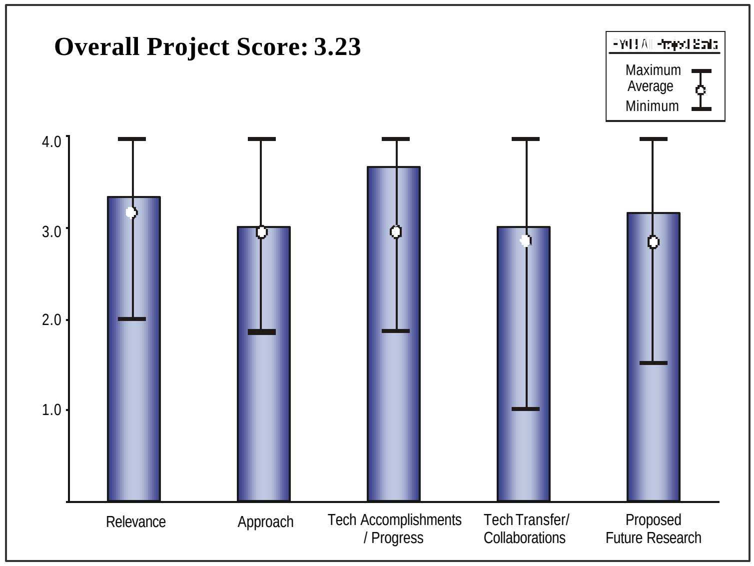
Specific recommendations and additions or deletions to the work scope

- Continue coordination with others in the field.
- Work on recording a publication of results should be funded.

Project #42: Algal Hydrogen Photoproduction
Ghirardi, Maria and Seibert, Michael, NREL

Brief Summary of Project

In this project, NREL studied the photobiological algal H_2 production using two approaches: (i) molecular engineering of algal H_2 production and (ii) cyclic photobiological algal H_2 production. In the molecular engineering of algal H_2 production approach, two cloned hydrogenases were studied and that information was used to design mutated genes with higher tolerance to O_2. In the cyclic photobiological algal H_2 production approach, a new photobioreactor was to be built in order to determine the effects of chosen parameters.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.33 for its relevance to DOE objectives.

- Curiously schizophrenic - the H_2 production being measured is "indirect biophotolysis" yet the engineering is all aimed at "direct…" Are the researchers clear on the distinction? Is the economist who gave the remarkable $ value?
- Preliminary economics, assuming continued success of NREL and UCB programs along with funding and success of ORNL project, show hydrogen cost by biophotolysis as low as $2.34/kg (land based) and < $1.40/kg ocean based.
- Supports long-term goal, though perhaps not in time to meet President's 2010 goal.
- Very long term R&D and very basic.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Increasing tolerance from 0% to 1% O_2 is a major break-through, but by no means technologically significant - it will be exciting to show synergistic benefit of a second mutation this year.
- Key technical barriers have been identified and major breakthroughs made on engineering O_2-tolerant hydrogenase (NREL) and ChI antenna modifications to increase efficiency and organism utilization.
- Work has changed over the years to more of a genetic engineering project - may need to be funded out of basic R&D.

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

This project was rated 3.67 based on accomplishments.

- At last some real progress toward the "O_2-tolerant hydrogenase". Let us hope that it can be substantially built upon.
- Good progress was made earlier in continuous versus batch production system.

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

This project was rated 3.00 for technology transfer and collaboration.

- Good interaction with Berkeley - need to make closer ones with ORNL.
- Numerous collaborations.
- Collaboration appears to be very good.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.17 for proposed future work.

- Definitely progress, but need to clearly define the economics of direct and indirect biophotolysis -- it seems muddled.
- Encourage priority on immobilized algae cultures to go from SO_4 to non-SO_4 without centrifugation. Also, immobilized enzymes would give options in design of photoreactor systems.
- Future plans are to genetically engineer new systems. Will this meet the President's goal?

Strengths and weaknesses

Strengths

- No one else is doing this and it should be part of the DOE program. This is the first success at the attempt to improve oxygen tolerance of the hydrogenase - a goal for the last several years.
- Good past work but new focus and engineering new algae needs to be reexamined in light of President's goals.

Weaknesses

- None specified.

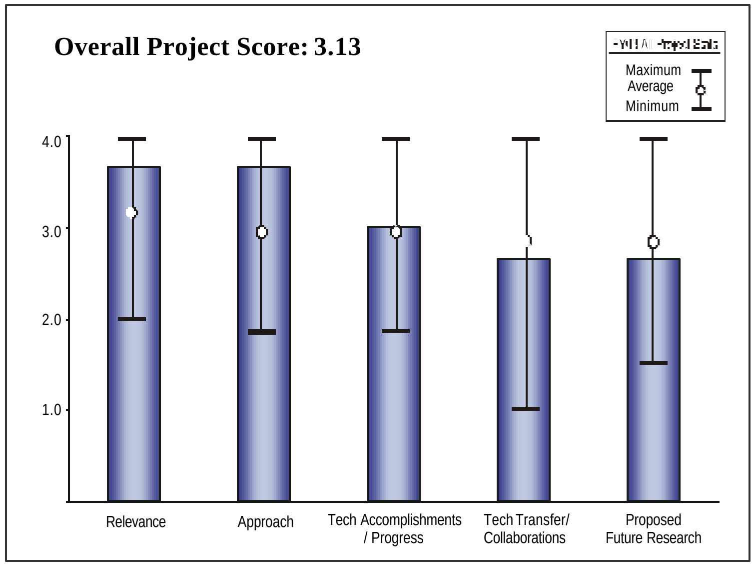
Specific recommendations and additions or deletions to the work scope

- Need to ensure that economic analyses are clear as to whether the goal is simultaneous production of O_2 and H_2 (direct biophotolysis) or segmented production (indirect - O_2 evolved and carbohydrate or something stored - then later converted to H_2). They will have very different costs.
- This work and similar work may need to be funded out of the Office of Science as longer term objectives.

Project #43: Discovery of Photocatalysts for H_2 Production
MacQueen, Brent, SRI International

Brief Summary of Project

In this project, SRI International developed a high throughput system to evaluate physical properties, measure solar hydrogen production of photochemical systems, and produce a database of photocatalytic materials. The effectiveness of nanosized particles for hydrogen generation from water using visible light was also evaluated.



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

This project earned a score of 3.67 for its relevance to DOE objectives.

- More of a supportive task than one aimed at meeting specific DOE H_2 goals.
- Good presentation of connection to goals.
- Highly relevant to the program goals.
- PEC is a superior approach.

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

This project was rated 3.67 on its approach.

- Need to spend more time making sure results can be communicated to others (database).
- Automated high throughput materials production and analysis are nicely addressed.
- Barriers are not specifically addressed, but data collection and analysis are mentioned as weak points.
- Very effective approaches to new designs for test equipment are potentially finding new materials for semiconductor electrodes.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- Good progress but appears to have had funding issues.
- Higher rate of analysis needed to support throughout.
- The project is making great strides, still in early stages.

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

This project was rated 2.67 for technology transfer and collaboration.

- Encourage coordinating national database activities with others in the field.
- Needs to work directly with other materials research projects.
- Some outreach and partnering. Could use more collaborations.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- Needs a little more focus on objectives for later years.
- Needs a longer range plan - either for scaling up or branching out.
- Work needs to continue on the lines of progress being made.

Strengths and weaknesses

Strengths
- Good work on autosamples.

Weaknesses
- Results need to be better communicated. (Improve database access.)

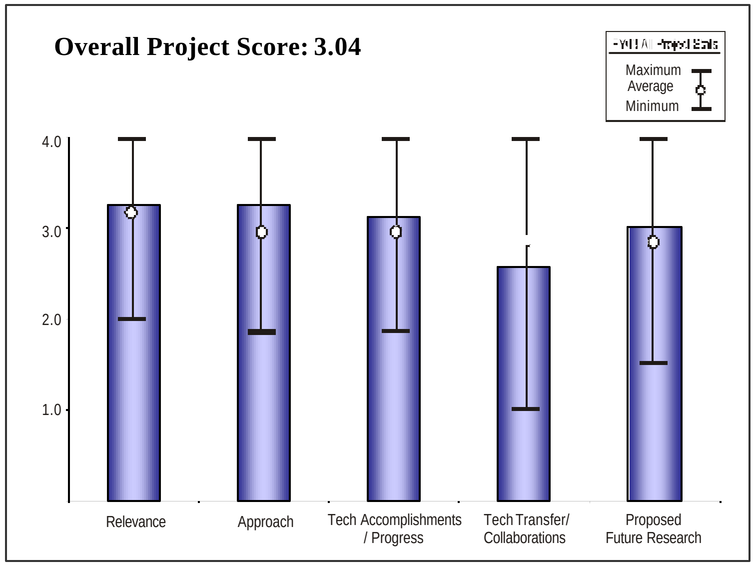
Specific recommendations and additions or deletions to the work scope

- Needs more specific material selection and planning for future work.
- Data library for rapid analysis, scaling data, and avoiding duplication is a serious need and should be addressed in year two.

Project #49: Advanced Thermal Hydrogen Compression
DaCosta, David, Ergenics Inc.

Brief Summary of Project

Through this contract, Ergenics plans to develop a hydride thermal hydrogen compressor that operates in conjunction with advanced hydrogen production technologies and improves the efficiency and economics of the compression process. They will also seek to determine to what extent the hydride compression process can perform the dual function of compression with purification, to remove impurities that adversely affect fuel cells.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.25 for its relevance to DOE objectives.

- Efficient hydrogen compression will be important for all forms of hydrogen storage and transport.
- Very relevant by potentially allowing quiet operation, convenience and miniaturization.
- Not strictly necessary, not highly efficient, but very useful in near and mid-term (cost reduction). Cycle life of hydrides an important, under-studied topic.
- Very original and creative concept. Not too many competing ideas on this objective.
- Useful for compressed gas.
- Although the envisioned system is based on a metal hydride material, this project is much more relevant to the infrastructure development than the hydrogen storage goals of the R&D plan.
- The metal hydride compressor has “good” relevance to the infrastructure aspect of the program as it could lead to a significant reduction in the cost of hydrogen.

Question 2: Approach to performing the research and development

This project was rated 3.25 on its approach.

- Well defined approach that included both efficiency and economic analysis.
- Market rationale is not entirely clear, based on the metrics of slide 9. These do not appear directly focused on the overall economics of hydride compression.
- Fighting hard to control capital costs.
- Very small pilot hardware.
- Good focus on critical path issues.
- Good work on impurity sensitivity
- Utilize the temperature-dependent "plateau" storage capacity of metal hydrides in a thermal energy driven H_2 compressor.
- Investigating alternatives to mechanical compression using hybrids to store H_2, heating, then desorbing compressed H_2.
- Cost to compress H_2 thermally seems promising.
- Assessing the effects of impurities is a valuable goal to storage. The other aspects support infrastructure of compressed storage but are not really on topic for this group.
- The PI's industrial experience and know-how are readily apparent from his well thought-out, realistic plan for the development of an advanced, thermal hydrogen compressor within a reasonable time frame.

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

This project was rated 3.13 based on accomplishments.

- Solid demonstration of technical progress. Good results on efficiencies.
- We would have felt a lot better if his accomplishments had been put in the context of the technical barriers that hydride compression has to overcome to be market viable.
- Beginning to show the crucial tolerance to impurities.
- Initial scale too small to validate capital cost estimated.
- Discovery helps CO tolerance.
- CO conversion is very important result!
- Good program orientation toward targets, though somewhat slower progress than we would have expected.
- Did not see progress or capital cost reduction in commercial product design
- Outstanding results: (1) H_2 compression demonstrated to 5000 psi with a far lower energy cost than conventional mechanical compression. (2) Development of a CO-resistant metal hydride alloy which works in the "compressor."
- Concerned about large amount of CH_4 produced at outlet.
- Suitable progress on correct topics.
- Making good improvements on mechanical compressors, especially in terms of cost of operation.
- Nice work to protect alloy from CO.
- Project has made steady progress and is on schedule.
- The 55% cost reduction compared to an electrical compressor and 9% cost reduction compared to a natural gas compressor is quite impressive.

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

This project was rated 2.57 for technology transfer and collaboration.

- Mentioned that partnerships were being pursued, however not many details.
- Coordinating with universities or pushing the frontiers of hydride science may be useful.
- Full scale demo in 2004 and at fueling station is soon enough. Averse to collaboration (by admission).
- Puzzled why there is no commitment to a compressor demo already in place. Sounds like very significant technology!!
- Seeking commercial development/implementation partners.
- No mention of other collaborators currently being involved, but discussion of potential future partners.
- No real collaboration, some participation in regulation/ trade groups.
- Partners for full-scale hydrogen compressor demonstration are being actively pursued.
- PI as been participating in the IEA hydrogen storage expert's group.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Very unspecific on future research. Nearly generic.
- The research has been successfully completed.
- Seeking partner for a full scale demonstration of the technology.
- Will want to see how CH_4 will be removed from H_2 outlet stream.
- Demo program a good idea to see how well it really works.
- Making smaller and cheaper is the right direction.
- Partners for full scale hydrogen compressor demonstration are being actively pursued.

Strengths and weaknesses

Strengths

- Unique H_2 storage infrastructure.
- Very impressed by accomplishment and potential value of this work!!
- Ergenics Inc.'s vast experience in metal hydride based systems greatly enhance this project's chances for success.

Weaknesses

- Overall balance between benefits and barriers was not communicated. Detailed systems analysis should be conducted to show advantages.
- Need to work on minimizing HC bleed through.

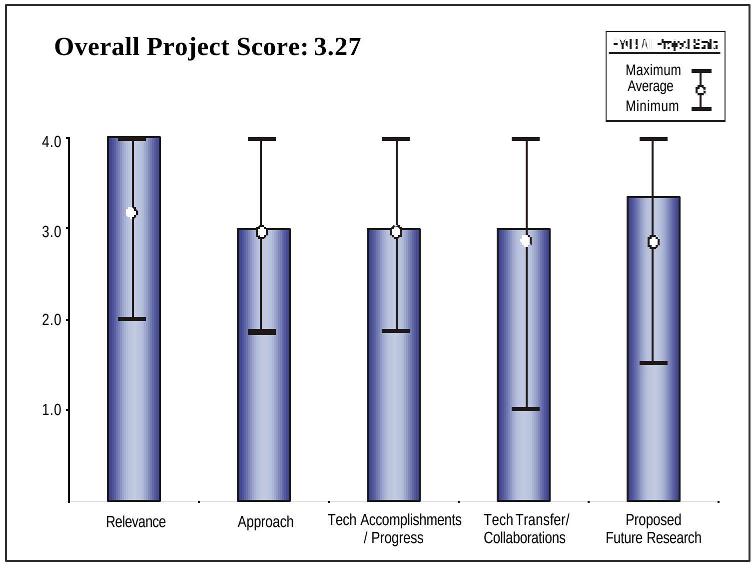
Specific recommendations and additions or deletions to the work scope

- The energy efficiency versus conventional reforming is apparently excellent but what are the relative capital requirements of the two systems?
- Mechanical compressors often require a back-up for reliability, how do hydride-based compressors fare in this respect?

Project #59: Development of a Turnkey H_2 Fueling Station
Guro, David, Air Products & 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 H_2 fueling station based on the reforming of natural gas. Building on the learnings from the Las Vegas H_2 Fueling Energy Station program, the project seeks to optimize the system, advance the technology, and lower the cost of H_2. 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 the maintenance of its safety standards in its design and operation.



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

This project earned a score of 4.00 for its relevance to DOE objectives.

- Good demo of H_2 fueling station - not gas reforming.
- Addresses all aspects of H_2 refueling facility design issues.

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

This project was rated 3.00 on its approach.

- Assessing best combination of components - special emphasis on PSA unit.
- Advanced PSA is a good addition, thought it is unclear how extensively they looked for the best product.
- Great use of existing facilities to test components. Good use of Vegas info.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- The advanced PSA is the only significant technical advancement noted.

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

This project was rated 3.00 for technology transfer and collaboration.

- Need more emphasis on education - and safety.
- Some collaborations but could be wider.
- Make detailed reports available on DOE web site.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.33 for proposed future work.

- Learning from Las Vegas station.
- Innovative idea to expand the project to include H_2/CNG
- Good work so far. Plans point to successful completion.

Strengths and weaknesses

Strengths
- Very comprehensive design with promising economies.

Weaknesses
- Is there a potential conflict-of-interest in the selection of the PSA unit?
- Should do more to incorporate non- Air Products Equipment.

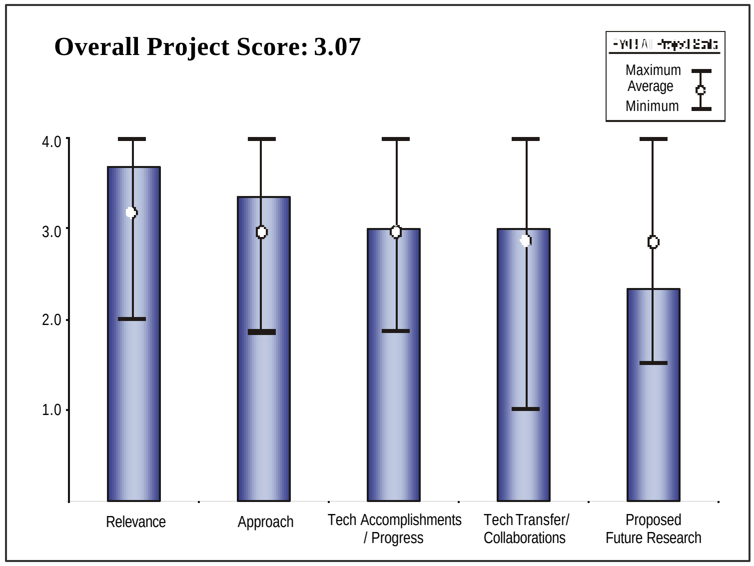
Specific recommendations and additions or deletions to the work scope

- Expand the education possibilities using the fueling station to demonstrate safety issues.
- Incorporate new components for testing as they become available.

Project #60: Autothermal Cyclic Reforming-Based H_2 Generating and Dispensing System
Kumar, Ravi, GE Energy

Brief Summary of Project

GE Energy and Environmental Research Corporation is designing, fabricating, and installing a reliable and safe H_2 refueling system based on autothermal cycling reforming. This system will be capable of producing at least 40 kg/day of H_2, sufficient for the refueling of at least 1 bus or 8 cars daily. GE hopes to achieve a current target cost of $19.2/GJ of hydrogen (LHV) for a 900kg/day of H_2 system.



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

This project earned a score of 3.67 for its relevance to DOE objectives.

- Demonstrations of working distributed H_2 production and refueling facilities are very important.

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

This project was rated 3.33 on its approach.

- Excellent component developed and test plans.
- No discussion of installation and use/market.
- Is there a Go/No-go decision?
- Little innovation outside of GE reformer evident.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- The only tech advancement is a high-pressure version of their system.

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

This project was rated 3.00 for technology transfer and collaboration.

- Need more discussions for future deployment.
- Would like to see detailed reports on DOE web site!
- Obvious input from BP on refueling station requirements and economics.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 2.33 for proposed future work.

- Not addressed.
- Future plans are weak and do not appear to be widely applicable.

Strengths and weaknesses

Strengths
- Excellent implementation of refueling system and analysis of economics.

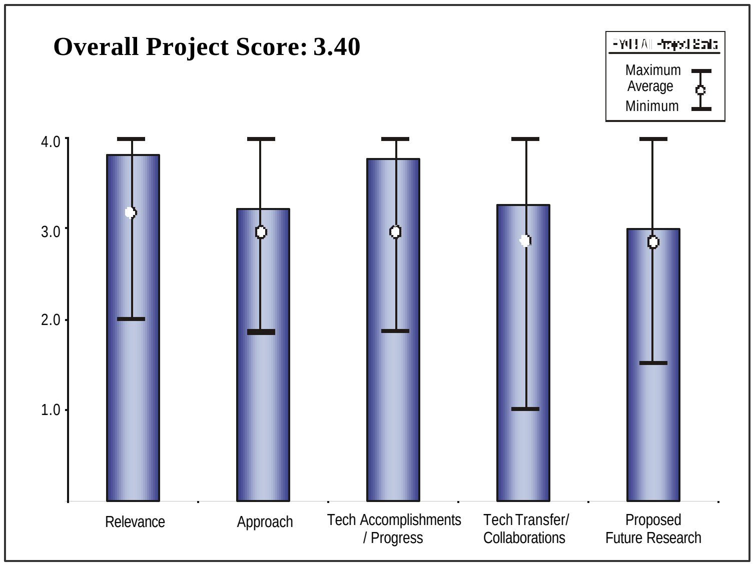
Weaknesses
- Focused on GE system exclusively.

Specific recommendations and additions or deletions to the work scope

- Economic analysis should include reformers from other manufacturers.

Project #63: H_2 Reformer, FC Power Plant, & Vehicle Refueling System
Raman, Venki, Air Products & Chemicals Inc.

Brief Summary of Project



This project is designed to accomplish the following: resolve design issues and demonstrate small, on-site hydrogen production for fuel cells and hydrogen fuel stations; design, construct, and operate a multipurpose refueling station; dispense compressed natural gas (CNG) H_2/CNG blends, and pure H_2 to up to 27 vehicles; design, construct, and operate a stationary 50 kW fuel cell on pure H_2; evaluate operability, reliability, and economic feasibility of integrated power generation and vehicle refueling designs; obtain adequate operational data on the fuel station to provide a basis for future commercial fueling station designs; develop appropriate "standard" designs for commercial applications; and expand the current facility to serve as the first commercial facility when sufficient hydrogen demand develops.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.80 for its relevance to DOE objectives.

- Nothing better than trying to run a real system with remote operation.
- Addresses system integration for both fuel and electric products.
- Similar systems could be used to expedite the development of a hydrogen infrastructure with a fuel cell for power generator while vehicle demand of H_2 is still developing.

Question 2: Approach to performing the research and development

This project was rated 3.20 on its approach.

- Needs to work on reducing the footprint.
- Land costs money in urban areas.
- Needs more analysis of economics.
- Needs to integrate the components in an optimized manner (i.e. water management).
- Include the cost analysis for next year.
- Supports on site H_2 generation, application of fuel cells for distributed power generation, hydrogen refueling infrastructure development, institutional barriers addressed in siting facility.

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

This project was rated 3.75 based on accomplishments.

- Having a pilot system work for one week is a wonderful accomplishment.
- System has been fully operational.
- Technical problems with fuel cell, will require close monitoring. This project has encountered several technical issues during early stages which have delayed extended durability testing/demonstration of the facilities.
- Significant contribution to the move toward a H_2 economy.

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

This project was rated 3.25 for technology transfer and collaboration.

- There are many manufacturers of 50 kW systems.
- The educational benefits should be enhanced.
- Seems to be a good working relationship between the industrial partners in the project as well as with the city of Las Vegas.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Needs to consider how to build 100 to 1000 kg H_2/day stations that could be placed in current gasoline filling station sized lots. Learning will be passed on to future projects.
- Not clear that demand for vehicular hydrogen will be developed as planned (outside projects control).
- Start up of FC has been delayed due to a variety of technical issues that have delayed demonstration of an integrated system.
- Should have operating experience data next year.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- None specified.

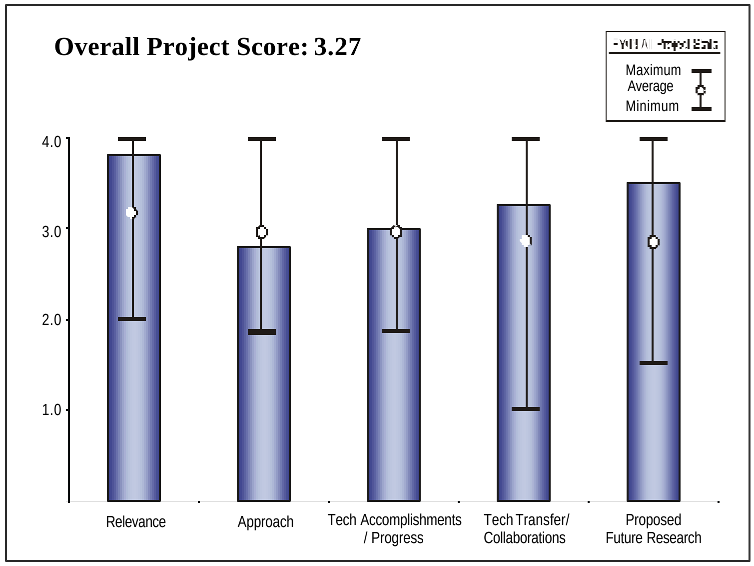
Specific recommendations and additions or deletions to the work scope

- Need to consider how to build 1000 to 1000 kg H_2/day stations that could be placed in current gasoline filling station sized lots.
- Need to increase the economic analysis to understand the sensitivity of life cycle costs to electricity buyback rate, O & M costs, natural gas cost, etc.
- Collection of data and assessment should include information on cost of engineering and cost of H_2 delivered.
- Need to establish baseline performance against DOE targets.

Project #64: Candidate Fuels for Vehicular Fuel Cell Power Systems: Stakeholder Risk Analysis
Lasher, Steven, TIAX

Brief Summary of Project

TIAX is conducting an assessment of the opportunities and risks for various fuel choices for FCVs –in particular comparing hydrogen to onboard reforming of gasoline. The assessment is also being used as a support to the refined R&D targets for direct-hydrogen FCVs based on an analysis of well-to-wheel energy use, GHG emissions, cost, and safety of direct-hydrogen FCVs and competing vehicle technologies.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.80 for its relevance to DOE objectives.

- Ultimate goals are well tied to energy providers and onto manufacturing concerns. This analysis will be critical to understanding program benefits.
- This is a supporting function for stakeholders and collaborators.
- There's only indirect relation to program goals.

Question 2: Approach to performing the research and development

This project was rated 2.80 on its approach.

- Needs to include analysis of foot print effects as well as cost of land (urban vs. rural) and where the demand will be greatest.
- The underlying assumptions need to be stated very explicitly - it is dangerous to present results without clarifying all assumptions.
- Analysis inappropriately compares technology-projected assumptions of FC vs. current and undervalued data for diesel HEV (for example). All data and assumptions are transparent. This needs to be presented to add credibility.
- Study lacks system and socioeconomic perspective particularly in use and market penetration.
- Study does not evaluate as much as it creates an artificial and unfounded pathway to the ultimate goal.
- Fueling station coverage is very out of line with even the lowest penetration estimates of DOE and even more out of line with other less optimistic estimates. This should at least be explained and substantiated.
- Project will be providing a standard modeling discipline for a variety of transportation areas.
- Putting the models on the Internet is recommended.

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

This project was rated 3.00 based on accomplishments.

- Needs to apply consistent case parameters.
- Needs to watch for improvements in DOE technology.

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

This project was rated 3.25 for technology transfer and collaboration.

- Have interactions with appropriate partners.
- Needs to include participation of manufacturing industry.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.50 for proposed future work.

- Should include discussion of information technology platform for models.
- This material needs to be shared and shareable across a wide band.
- Should make strong effort to review with stakeholders.
- Sort out pathways (CH_2, LH_2).

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- None specified.

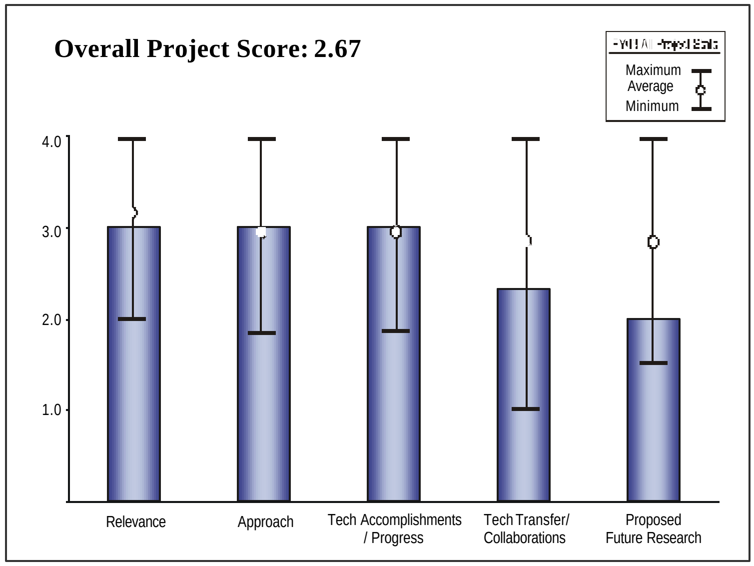
Specific recommendations and additions or deletions to the work scope

- Need to include analysis of foot print effects as well as cost of land (urban vs. rural) and where the demand will be greatest.
- Formal plan for shared data and models to facilitate stakeholder participation.

Project #111: Fuel Cell Distributed Power Package Unit: Fuel Processing Based on Autothermal Cyclic Reforming
Zamansky, Vladimir, GE

Brief Summary of Project

GE is developing a fuel cell distributed power package unit for fuel processing based on autothermal cyclic reforming. GE is designing a precommercial integrated fuel processor, and assessing the performance and economics of the design. The fuel processor will convert natural gas to a hydrogen-rich stream, operate for 100 hours, achieve fully automatic operation during normal, start-up, shut-down, and stand-by modes, 75% efficiency on higher heating value basis, and achieve cost target less than $1000/kW and electricity cost that is less than $0.06/kWh, when the system is mass produced.



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

This project earned a score of 3.00 for its relevance to DOE objectives.

- System has potential for high efficiency for production of H_2.
- Only reason this is not outstanding is that reforming is not absolutely critical to vision as H_2 can be supplied in other ways, however, I think we need to go this way.

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

This project was rated 3.00 on its approach.

- Question the use of nickel catalysts: Nickel has been shown by many to be poisoned by sulfur and have high propensity to form carbon. The potential for the quickest way to degrade base metal catalysts is to have them undergo 50 oxidation-reduction cycles. Durability is questionable.
- For transportation, start-up time is critical. This barrier needs to be addressed.
- Integration is a real strength.
- Project is mature so feasibility is fairly well established.
- Design and integration appear quite innovative.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- The specified catalyst life of 2000 hrs of catalyst (before maintenance) seems low.
- It appears they are projecting electrical costs of about 14 cents/kW; this is extremely high. Eight cents at 500 kW_e + 10000 units. I don't think this is commercially viable.
- It is unlikely the nickel catalyst is truly sulfur tolerant. The fact that it gets regenerated every 5 minutes prevents it from being noticeably poisoned by sulfur.
- Three hundred hours of testing - not much of a durability test for the reformer.
- Good cost modeling.
- While not meeting all targets (e.g. CO level), the important aspect is that this is an integrated system and such barriers can be overcome by plugging in an improved module.

- Impressive amount of work for the funding level.

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

This project was rated 2.33 for technology transfer and collaboration.

- Appears to have no current interactions.
- No collaborations in this fiscal year but one is planned for next year.
- Transfer of this technology to demo that integrates with fuel cell under auspices of California Energy Commission and Air Resources Board is a good indicator of R&D success and an appropriate graduation.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.00 for proposed future work.

- I think future work should concentrate on catalyst durability/stability.
- Future work does not address start-up time.
- How can this project "improve reliability"?
- Decision points are not defined.

Strengths and weaknesses

Strengths
- Three kW-scale units built and tested.
- They seem to work pretty well.

Weaknesses
- The stability, poisoning, and issues with carbon formation have been documented. A plan is needed to overcome these issues.
- The cyclic valve potentially will have durability issues especially with carbon being formed with the nickel catalyst.
- The cost projections appear to be too high.

Specific recommendations and additions or deletions to the work scope

- This project really needs to demonstrate the suitability of nickel catalysts.
- Interactions with a national lab or university to do some fundamental catalyst evaluation could help.
- They should explore: NiS, NiC, and physical stability of Ni i.e. dusting. All could be done at relatively small bench-scale level.
- Start-up time is not addressed. Is this project for land-based reforms? If this project is being funded for transportation, start-up time should be added to the scope of the work.

This section includes projects that explore various pathways for storing hydrogen or its precursors for both vehicular and stationary applications. The focus of this program element is primarily on on-board vehicular storage systems, but off-board applications such as hydrogen delivery and refueling infrastructure, Power Parks, and vehicle interface technologies for the refueling of hydrogen storage system on vehicles are also explored. The aim of these efforts is to achieve the following milestones established in the Hydrogen, Fuel Cells and Infrastructure Technologies Program Multi-Year Research, Development and Demonstration Plan:

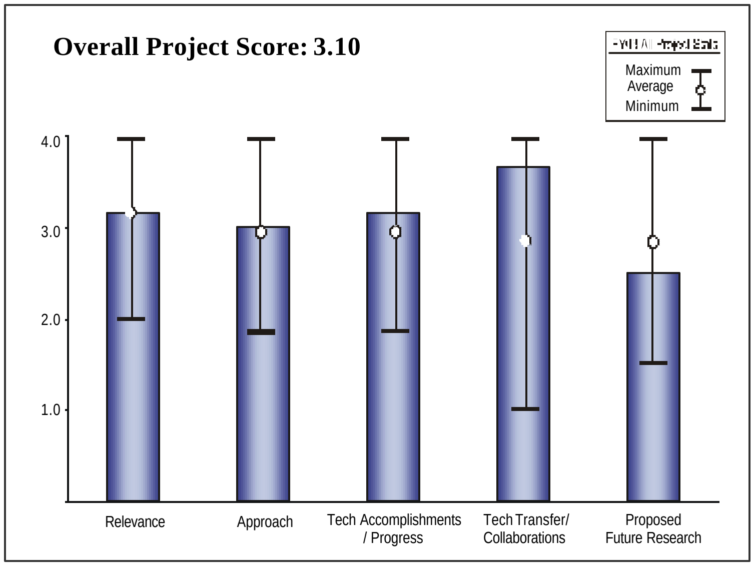
Hydrogen Storage

- Demonstrate on-board hydrogen storage systems with a 6% capacity by weight by 2010 and a 9% capacity by 2015.

Project #44: DOE H_2 Composite Tank Program
Sirosh, Neel, Quantum Technologies Inc.

Brief Summary of Project

In this project, Quantum Technologies is working to further their "TriShield" composite fuel storage tank technology. They will develop, demonstrate, validate and deliver 5,000 psi tanks (7.5 wt% and 8.5 wt% Type IV) and 5,000 psi in-tank-regulators to the DOE Future Truck and Nevada hydrogen bus programs. They are also working to demonstrate 10,000 psi storage tanks.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.17 for its relevance to DOE objectives.

- Without progress being made by this vendor, President's vision couldn't happen. Lots of learning on crucial ancillary components.
- Compressed hydrogen is a "second-best" solution and to be more relevant, it would need to show significant technology advances.
- With all of the compressed work, there is the "hard upper limit" for energy densities that calls into question whether these technologies ever (even theoretically) can achieve 2015 targets. But, they are the currently most viable technology, and this work seems well-aligned with DOE objectives. (Already beyond many of the 2005 targets!)
- Tanks are likely to be a short term solution to H_2 storage. 10,000 psi is critical for volume and mass efficiency. Cost reduction is needed to meet DOE targets and to make technology affordable in high volume automobile production.
- The targets have moved, so their original 10 kpsi system only meets the new '05 goal. This is not a criticism, as it appears that the '10 goals might be reachable.
- High pressure tanks are currently the prime candidate due to product maturity, so this is quite relevant.
- Valuable contribution to the mid-term goals, incapable of meeting ultimate goal.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Great expertise applied, but some lack of ambition on mass efficiency.
- Nice integration work (not under DOE contract).
- Progress on fast fill.
- Approach is comprehensive and focused on cost reduction and BOP components.
- Progress well described, but very difficult to assess the technical approach of this work. The approach wasn't really described. So, ranking in this category is a bit of guesswork based somewhat on their past progress.
- The approach seems fairly conservative; it would be nice to see this group pushing the edge of the envelope a bit more, especially with their DOE funding – conformable tanks, new materials, cheaper fiber production, etc.
- Report did not provide a lot of information about research process, plans, etc. Was more of a progress report or a "sales/corporate" story.
- The approach was to work first on weight, then on capacity, but these are not mutually exclusive! There is nothing in the approach to minimize material, and thus to maximize volumetric efficiency.

- The approach is very conservative, and does not indicate any "out-of-box" thinking. Why don't they look at fiber materials other than carbon? How about aramids, synthetics, spider silk, other oddball ideas? What about lower cost methods of production?
- Parallel approach to multiple barriers is suitable to meeting all goals. Greater emphasis on reducing weight of regulator, hangers, tubes needed. Cost needs to drop a lot.

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

This project was rated 3.17 based on accomplishments.

- Not ambitious, but solid proof of feasibility. Understanding of issues gained at 10 kpsi confirms results at national lab.
- Presenter did not specify what the tasks were for this year. Many of the objectives were accomplished in previous years.
- Already beyond many of the 2005 goals!
- It is difficult to assess how much is specifically due to DOE's contract vs. the in-house work that would have been done anyway
- Have made progress from 350 bar to 700 bar. Hope to see significant progress toward regulatory approval/certification of 700 bar tanks in the U.S.
- Fairly good progress towards original goals, and they recognize that cost is an issue. However, they do need to do more in the areas of design and manufacturing optimization.
- Quite impressive advancement over the years, though it is unclear to what extent this contract has driven that.
- Fast fill in 3 minutes is an important demonstration.

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

This project was rated 3.67 for technology transfer and collaboration.

- No transfer needed, Quantum is a partner of the car companies (GM equity).
- Pro-active on safety regulations.
- INEEL CRADA on liners is an important collaboration.
- Good collaboration with all OEMs.
- More of this technology has been "transferred" than any of the other storage options.
- Good mixture except that universities seem to be missing.
- Already in industry and selling product. Working with suppliers of key materials. Obtained certifications. These are all good efforts to integrate other information and access ideas in national labs.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Future work not tasked or funded - DOE's fault, not Quantum's. Much of what Quantum is working on would happen without further DOE funding.
- Presenter did not show enough detail to be able to evaluate future plans.
- Thermal management and burst factor reduction are key barriers that should be pursued.
- Report not specific enough about funded vs. unfunded future plans. Unclear which plans are funded and planned.
- No timeline is identified to certification of 700 bar tanks in the U.S. or to finding lower cost solutions.
- The list of future areas to investigate is good, but no actual plans were presented. They should focus a lot on costs – materials and manufacturing.
- Right ideas, unclear if the exact plans are going to get them there. Need to focus on reducing volume via smart tanks (less tank volume). How will they reduce cost of fiber and mass of balance of plant?
- Durability (cycling) in fast fill will need to be proven.

Strengths and weaknesses

Strengths

- None specified.

Weaknesses

- Unclear on future plans.
- Very conservative approach yielded safe, good results. However, their thinking is not “out-of-box” enough with respect to materials, manufacturing, and statistical design.
- How much of this work was performed with DOE funding? What was the level of DOE funding?
- System unlikely to reach 2015 goals so this needs to aim at the 2010 target and then close out.
- This is an important program for meeting the intermediate goals on time.

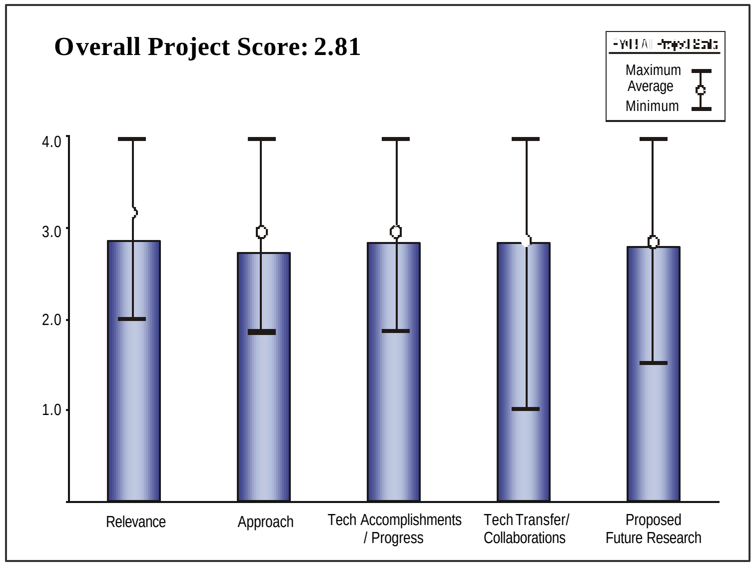
Specific recommendations and additions or deletions to the work scope

- Too much emphasis on weight reduction. Safety, cost, and refueling are most important.
- It seems that the issue of low-cost carbon fiber is of central importance to the future of this technology. It's not clear if this type of work will be done or if this will be entirely relegated to suppliers. Also, the description of "significant cost opportunities" was extremely vague -- what opportunities? How accomplished?
- Investigate other materials, thinner walls, lower cost manufacturing conformable tanks.

Project #45: Development of a Compressed H_2 Gas Integrated Storage System
Wozniak, John, Johns Hopkins University/APL

Brief Summary of Project

In this project, the Johns Hopkins University Applied Physics Laboratory will advance the technology elements required to develop a semi-conformal compressed hydrogen gas integrated storage system for fuel cell vehicles. As part of this research, Johns Hopkins will attempt to develop materials and treatments to reduce hydrogen gas permeation through tank liners, an optimized carbon fiber/epoxy resin tank overwrap, and alternative designs and materials for constructing the unifying elements of the integrated storage system.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.86 for its relevance to DOE objectives.

- Better DOE H_2 storage mission statement, obtained from program manager, that matches program rationale due to what DOE solicited in FY01.
- A new approach moves some functions to shell outside tank.
- Relevance is in question due to low volumetric efficiency. Otherwise, the concept is creative, good, and focused.
- The issue of conformable tanks is an important one, and thus this work is most welcome. However, as with all compressed programs, the energy density needs to be addressed. In this case, the system will obviously decrease the energy density compared to a single tank. What is the "penalty" for this design?
- Tanks are likely to be short term solution to H_2 storage. 10,000 psi is critical for volume and mass efficiency. Cost reduction is needed to meet target and to make technology affordable.
- The focus on semi-conformable high pressure tanks is very relevant to the eventual design of hydrogen powered vehicles.
- Valuable contribution to the mid term goals, incapable of meeting ultimate goal.
- Semiconformable is definitely the right direction.

Question 2: Approach to performing the research and development

This project was rated 2.71 on its approach.

- Coupling to vehicle (first analysis I know of) is important and unprecedented integration issue.
- This new approach may be fundamental to improved safety.
- Well focused project. Well defined mission. Good possibility of success.
- PI should strongly consider the value of working with OEMs (safety) personnel, SAE and DOT.
- PI should consider the existing SAE standards and other issues related to actual use and integration of the design.
- Consideration is needed for the design of a high reliability outer shell that will not be penetrated by road debris, or to inform the operator if the shell has been compromised.
- Concept has merit, but is currently nothing more than an academic exercise that cannot be fully applied. Bonfire test should be conducted with the cylinder(s) in the case.
- The program should NOT assume safety margins will be lowered in any code or standard.
- The idea of dividing tasks of tank function is clever.

- Could do less on packaging and more on tanks. OEMs can concentrate on packaging once technology has made more progress.
- Novel approach to conformable storage, but I have concerns about test results where safety/engineering design margins were in some cases barely met.
- The idea of separating the functions of containing pressure and protecting the tanks is very intriguing. However, they are still constrained by the use of cylindrical tanks.
- Although their sequence of development is logical, this approach may not yield optimum energy density. That said, it is a good start.
- No work indicated on cheaper/stronger fibers or statistical design.
- The focus on vehicle design was an unnecessary distraction and drain on resources. They should focus on their technical goals, which are truly pre-competitive, and leave vehicle design to the OEMs.
- Semi-conformable is a better route if it can be achieved.
- Clever approach – dividing tasks the tank must do, but the implementation is not looking promising, especially on volume!
- Despite certain weaknesses, the idea of separating the functions into separate structures is worth pursuing - for example, an internal structure like tie-lines to take load, and the skin to give protection.

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

This project was rated 2.83 based on accomplishments.

- Moderate ambition on integration tasks; not ambitious on core tank technology, competent but not progressive.
- Good experiments on permeation.
- Seems to be making good progress toward achieving the goals.
- Making progress but I am concerned about slim margins to meeting key test requirements (such as gunfire/fire).
- Need to work more on manufacturing processes. Work on in-vehicle packaging ought to be left to the manufacturer, and work in this project ought to focus on addressing safety and manufacturing issues.
- They methodically addressed failures, improved permeation, and increased volumetric and gravimetric storage efficiencies. They also focused heavily on manufacturing and system costs and on safety, indicating a good grasp of automotive requirements. However, results still fall short of the newest storage targets.
- How much of this was done with DOE funding, and what was that funding level?
- Good safety progress.
- Not achieving the real strength of conformables, good density.
- Not making quite the progress one might want on certification.
- Work on crash system is good, implementation needs help (see part 5).

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

This project was rated 2.83 for technology transfer and collaboration.

- DaimlerChrysler cooperation in 14 demo vehicles. Good cooperation and learning from academic center in Ohio. No obvious technology transfer partner for future.
- Good collaboration with General Dynamics.
- Because the system is sealed, this is likely to lead to difficulties in inspection for damage, etc. Should address this issue in the future.
- Unclear from presentation how much collaboration there is.
- No national lab involvement was reported.
- Working with industry. Working with other universities. Selling product. Working with industry suppliers to address goals and quality.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.80 for proposed future work.

- Will make demo vehicle integration easier.
- Some cost reduction, slightly sloppy with volume lost.
- Pursuing several innovations with high cost leverage.
- Good detailed plans with collaborations and milestones.
- Agree with future plan and need for improved toughness, to find industrial partner, etc.
- They did have good plans to both increase storage efficiency and reduce cost. Addressing the factor of safety is a good start toward statistical design. However, the basic design still includes a lot of volume in the balance of plant.
- Looking at lower factor of safety is the right thing if the reliability is there, and only if it is there. Need to put more emphasis on volume!
- This may never be suitable even for near term goal. Need to address safety of single solenoid and sensor in accident.

Strengths and weaknesses

Strengths
- Solid mechanical engineering. Hardware focused.
- The focus on costs and vehicle performance requirements is good.

Weaknesses
- No collaboration with car companies.
- Low volumetric efficiency.
- Probably want to concentrate on storage and let auto industry do the crash packaging.
- Crashworthiness is an area car companies do well.

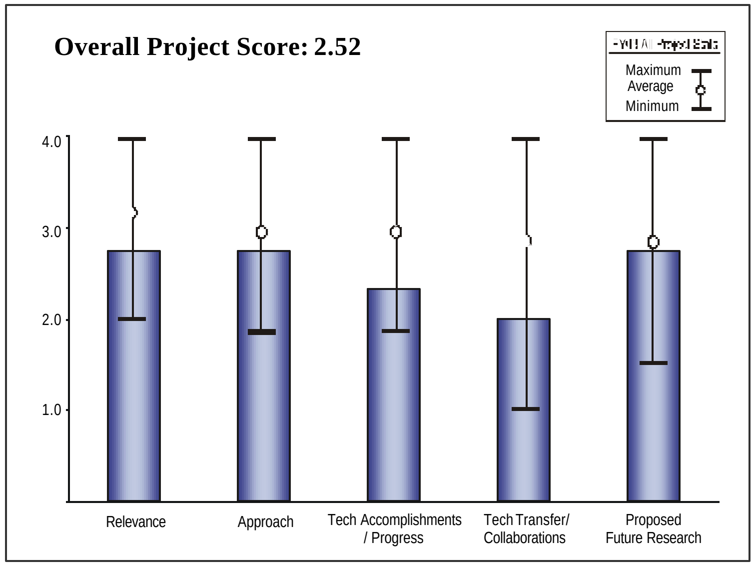
Specific recommendations and additions or deletions to the work scope

- I question the value in researchers wanting to address issues of changing regulatory requirements – ought to expend efforts at addressing design issues and manufacturability.
- There should be some effort to find cheaper/better fibers.
- Statistical design should be incorporated to optimize design.
- They should not do any more vehicle packaging work.

Project #46: H_2 Storage Using Lightweight Tanks
Weisberg, Andrew, LLNL

Brief Summary of Project

Lawrence Livermore National Laboratory is concentrating on hydrogen storage using lightweight tanks. They plan to build and operate the best hydrogen container technologies, explore the performance of recently discovered non-pressure-vessel structural containment options for compressed hydrogen, and demonstrate fundamental improvements in hydrogen containment safety by quantifying optimality, fixing computer aided engineering models, adopting process research and through continued experimentation.



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

This project earned a score of 2.75 for its relevance to DOE objectives.

- The goals and objectives of this program are somewhat obscured by the funding and facilities irregularities. Nevertheless, one does wonder if the work presented is actually what this program was funded to do?
- Novel approach and thought process to address tank issues. Statistical experimentation and research could prove valuable.
- Of course hydrogen storage is relevant to the eventual implementation of a hydrogen economy, and high pressure tanks are clearly in the lead with respect to product maturity. The ideas presented go to the heart of high pressure storage issues – cost, energy density, and vehicle implementation.
- Hard to assess as they have not really gotten going.

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

This project was rated 2.75 on its approach.

- Parts of the presentation were unclear and it was quite difficult to assess the technical approach. For instance, slides contained too much text, too many equations, unfamiliar symbols, and unfamiliar dimensionless units. This work might be very important and have a significant impact on DOE's objectives; however, if the presentation is not clear, the review panel cannot make an informed judgement.
- I applaud the desire to get reliable empirical datasets of statistics for these failure rates. However, in this sort of analysis, naturally, the observed rates will be in the tails of the distributions. Thus the form (e.g. Weibull vs. Gaussian) is critical, and small data sets will not adequately account for these tails.
- The idea of the ultra-conformable zoo structures is intriguing. These ideas should be put into practice.
- It appears that a lot of innovative thought has been given to the challenge of finding volume- and mass-efficient storage designs. Unfortunately it is unclear where the research is going.
- It is not clear from the presentation just what the original plans were, and how they intended to approach the problem. However, the three directions which have evolved despite (or because of) program funding, facilities, and personnel glitches, are each unique and intriguing.
- Statistical design should be rigorously applied to high pressure storage systems, as the application of industrial and aerospace technology to mass produced automobiles demands that safety factors and usage assumptions be revisited.
- Pre-stressed macroscopic internal lattice structures present an excellent opportunity to develop a truly conformable high-pressure tank (which is a Holy Grail).

- Developing a mathematical “unified hydrogen storage theory” to assess tradeoffs associated with various hydrogen storage systems is a brilliant, if ambitious idea.
- Interesting and needed look at how reliable these systems are.
- Inside structures are an interesting idea, more work should be done for confirmation.

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

This project was rated 2.33 based on accomplishments.

- The accomplishments are not impressive, but due to the funding irregularities (and poor presentation), it is difficult to assess this yet.
- Although funding limits appear to have precluded work planned, it appears that a lot of innovative thought has been given to the challenge of finding volume- and mass-efficient storage designs.
- It is unclear where the research is going.
- The project has resulted in little more than excuses for why things weren’t done. To be fair, funding, people and facilities were not available.
- The new directions presented indicate that a lot of original thought has gone into how to optimize high pressure storage systems. It is a case of 98% inspiration and 2% perspiration.

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

This project was rated 2.00 for technology transfer and collaboration.

- No discussion of collaboration.
- Minimal, but then actual program work has not really begun. Some collaborations are suggested for future work.
- No obvious collaborations.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.75 for proposed future work.

- With the presentation conveying little useful information, it is difficult to assess.
- Author lays out hope for new approaches, but is non-specific about future plans or how optimistic vision would be achieved.
- This presentation was all about proposed future research. It is very intriguing, and represents three areas of unique original thought.
- Definitely needs to be communicated better.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- Lots of ideas, new concepts, but no concrete plans or roadmap to specific/deliverable goal.
- No progress to report, but great ideas. However, reviewers don’t want to hear excuses. Also, don’t assume that everyone can think in the abstract – it took quite a lot of time and discussion to understand what the PI was trying to communicate. Understanding could have been enhanced through the use of simple language and standard notations.
- Until this is put in a language that others can understand, it will be hard to fully evaluate.
- I like that he has come up with cool ideas when he did not get funding.
- I question how well he will stay on topic if/when he is funded.

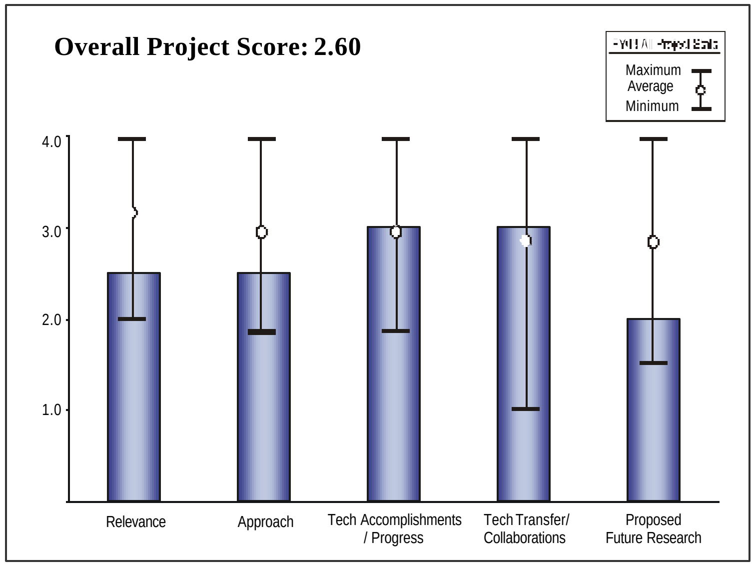
Specific recommendations and additions or deletions to the work scope

- Aggressively move forward with the proposed areas of research.
- Include funding level for this program.
- Three good ideas to follow in some way: 1) Statistical effects on safety and cost and so on. 2) Prestressed and internal support members. 3) Analytical approaches to tradeoffs between mass, volume, etc.

Project #47: Insulated Pressure Vessels for Vehicular Hydrogen Storage
Aceves, Salvador, LLNL

Brief Summary of Project

In this project, Lawrence Livermore National Laboratory will demonstrate the advantages of insulated pressure vessels for hydrogen storage in terms of improved packaging, reduced evaporative losses, and reduced costs. Through their work on this storage technology, Lawrence Livermore will also develop standards for assuring the safety of insulated pressure vessels.



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

This project earned a score of 2.50 for its relevance to DOE objectives.

- The speaker did not give gravimetric and volumetric system numbers for this concept. This is a crucial (and perhaps, most important) aspect of this project, and it wasn't mentioned! (When asked, the gravimetric number of ~5% was given.)
- Liquid H_2 storage is an alternative short term storage possibility. Conformable approach directly aligned with OEM needs.
- This is another attempt to improve hydrogen storage efficiency, so from that standpoint, it is relevant. However, one must question a design which combines the negative volumetric attributes of two storage techniques, and then requires two infrastructures (or at least 1.5) to realize its full potential.
- Good for interim goals. Not for 2015.

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

This project was rated 2.50 on its approach.

- It apppears that a methodical approach was taken.
- Photos seem to indicate that the tank evaluated is too large for automo tive use.
- Statement regarding feasibility of having both liquid and compressed hydrogen at same refueling station is unpersuasive.
- The approach is strange. Although it is an attempt to provide the benefits of both high pressure and cryogenic hydrogen storage, a conservative approach was used, so a full pressure containment system was included inside a complete vacuum “thermos-like” insulation system. Naturally, this can be expected to result in low gravimetric and volumetric energy capacities. That said, the future concept for a rectangular tank with internal structure is exciting, and bears further development.
- Interesting concept with chance of getting the best of both worlds. This needs gate review to make sure that it's not headed for the worst of both worlds.
- Cost is a real question, one that will be resolved only with proper, inexpensive materials.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- It is an interesting and unusual idea, and the progress reported is encouraging.
- Testing of LH_2 tank under various conditions, but no details about whether design evaluated meets DOE targets.
- There was good progress toward the goals, but I question whether the goals were logical.

- Testing has been positive.
- Gravimetric and volumetric energy capacities should have been specifically reported.
- Hard to believe that the incremental infrastructure and fuel costs for the liquefied fuel are as low as they say – more detail is required in that analysis.
- Progress on certification is needed.

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

This project was rated 3.00 for technology transfer and collaboration.

- Collaboration with SunLine demonstration vehicle: the connection of these vehicles to the insulated pressure vessels is somewhat unclear. More detailed discussion of these prototypes would have been appreciated.
- Partially funded by SCQAMP.
- No mention of universities.
- Working with customer and standards setting organizations and outside funding.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.00 for proposed future work.

- There is little indication of the vision, "where is this work headed"?
- Will be interested to see results of computer modeling to see if a non-cylindrical conformable tank can be made. But report does not lay out specific plans or roadmap to design, develop or test plans proposed.
- The idea to pursue a form of semi-conformable tank is good, as is the intent to apply more rigorous 3-D analysis of complex stress states. However, no specific plans defining work in these areas were presented.
- Need to establish early on if internal stiffeners are likely to work at this temperature in a vibration environment.
- Actual plans not too clear.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- Isn't there a way to combine the positive attributes of the systems without bringing along the negative baggage? An ideal would be a conformable tank which can also insulate, but which does not include all of the weight and volume associated with both high pressure containment and cryogenic insulation.
- Cost argument is not very sound. Infrastructure costs likely to be much higher than stated.

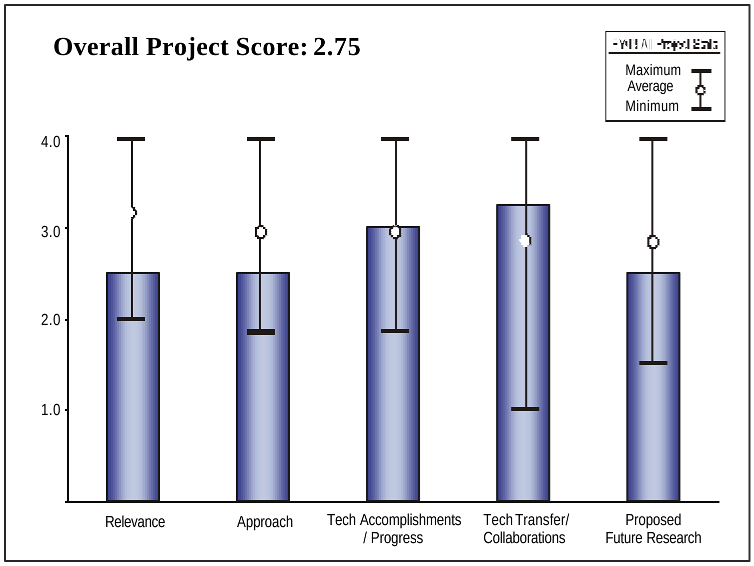
Specific recommendations and additions or deletions to the work scope

- Revisit infrastructure costing analysis and effects of liquefaction on fuel cost. Measure hydrogen capacity of the system as a function of temperature – are there benefits to cold gas which is not cryogenic?
- Specify level of DOE funding on this project.
- Confirm the 5% system storage. If true, this is good.

Project #48: Low Permeation Liner for H_2 Gas Storage Tanks
Lessing, Paul, INEEL

Brief Summary of Project

Idaho National Engineering & Environmental Laboratory (INEEL) is currently working to develop a polymer liner that greatly limits hydrogen losses from commercial, light-weight, composite, high-pressure hydrogen tanks. In order to complete this objective, INEEL will select and fabricate polymeric materials with the necessary electron and proton conducting properties, fabricate a “bench-top” model of tri-layer, electrochemically-active protection device that reduces hydrogen permeation through polymer “substrates,” and deliver a prototype system.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.50 for its relevance to DOE objectives.

- Existing Type IV tanks already meet permeability requirements, so the justification for this added complexity should be clearly articulated.
- Could, if successful, support goals of low cost, size and mass.
- I’m not sure how relevant this is, given the good permeation performance of existing tanks. It seems to be an answer to a question which has not been asked. However, any focus on reducing the cost, weight, and volume of a hydrogen storage system does hold some interest.
- Useful for intermediate steps, may serve final goal too if polymer tanks serve solid storage.

Question 2: Approach to performing the research and development

This project was rated 2.50 on its approach.

- The justification for this project as a whole must be addressed. The tanks are already expensive and meet permeation standards; why add complexity and cost to this system?
- Patent application, CRADA and university sub-contract are some indicators of progress.
- Design approach is intriguing but I question if it will be a practical approach since constant electric current is need to the barrier located inside the tank.
- Assuming permeation is an issue, the idea of using an electrochemical barrier is unique. However, the cost of applying a multi-layer internal coating, the potential leak paths caused by terminations, and the need for constant current (which costs some efficiency) limit the usefulness of this approach in this application.
- Clever idea to make a voltage barrier to protons trying to diffuse through metal liner films. The magnitude of the problem solved is not clear to me.

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

This project was rated 3.00 based on accomplishments.

- More information about the catalyst would be useful in assessing this technology.
- The cost of this material/process should be discussed, particularly in light of the justification (cost benefit analysis).
- Sounds like an interesting technical approach that warrants additional investigation.

- This is the first year of the program, but they have IP and contractual documents in place. In addition, work has begun, materials have been selected, and test plans are in place.
- Suitable progress given short time on task.

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

This project was rated 3.25 for technology transfer and collaboration.

- Doing CRADA work with known/respected manufacturer.
- Linking with appropriate university and industry people.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Appears to have good plan for future research that builds on past work.
- Given the goals, these plans make sense. However, I'm not sure that the cost savings or improved storage efficiency resulting from thinner linings will justify the cost and complexity of this system.
- Good plans but order is likely to be wrong. Need to find leak rate expected and see if current material can handle it.
- Need to appraise cost reduction because that is the real value to this, cost versus other liners.
- If current is low, may want to look at some sort of passive system.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- Cost may become an issue.
- This is mildly interesting technology, but it focuses on a problem that doesn't exist. Can it be applied in another way? How about using it for in-tank pressure or temperature measurement? Would that be cheaper or more reliable than existing sensors?
- Need to see if this is needed given the existence of tanks with known success.
- They really do not know if this is needed or how badly it is needed. Continued funding should hinge on showing a problem exists and how much power will be eaten.

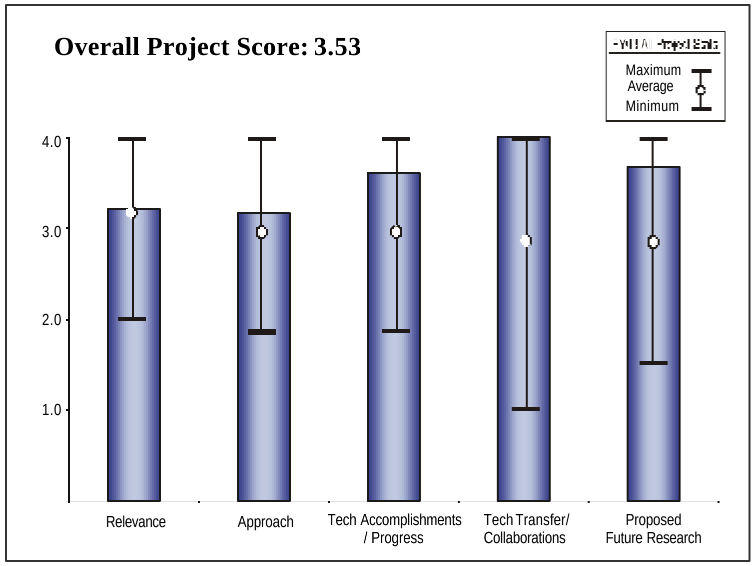
Specific recommendations and additions or deletions to the work scope

- They should investigate the effects of liner compression on permeability. If current tanks meet permeation specifications (and they do), why add 3 layers, electrical terminations, and high pressure seals to make it better?
- How much current must be used to keep the hydrogen in? It will have to figure into the overall efficiency of the system.
- Really needs to get data on current permeation rates and what the costs are.
- Need to evaluate power draw.

Project #50: Catalytically Enhanced H_2 Storage Systems
Jensen, C., 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 materials will have greater than 6 wt% hydrogen storage capabilities, and can undergo dehydriding and rehydriding at temperatures below 100ºC, and maintaining more than 95% of their hydrogen capacity through more than 200 dehydriding/rehydriding cycles. To accomplish this, they are studying kinetics and mechanisms of the fundamental dehydriding and rehydriding reactions.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.20 for its relevance to DOE objectives.

- Most work highly relevant. Some characterization work questionable.
- Very nice work, good understanding, BUT - even if successful this will only deliver intermediate targets. There is a fundamental material limit and it is not clear where this can go.
- Can fundamentals learned here help to guide where we need to go? I see limited prospects!!
- The "conventional" sodium alanate ($NaAlH_4$) obviously will not achieve the 2010 or 2015 system performance targets; however, understanding the role of Ti is crucial towards opening up new classes of materials that could conceivably achieve these goals.
- Although alanates cannot actually meet the new DOE goals for hydrogen storage, the work to determine the effects of dopants and the mechanisms of how they change hydrogen storage capacity, reversibility, and adsorption kinetics can potentially lead to other solutions.
- Obviously only suitable for early goals, but may show the way to help other solids.

Question 2: Approach to performing the research and development

This project was rated 3.17 on its approach.

- Good progress but the approach is rather ill-defined, as described through the objective.
- Needs more focus.
- Fundamentally, this is a materials project.
- Approval searching for fundamentals is very good.
- Good progress.
- Global collaboration is impressive, but probably not very effective in terms of efficiency.
- Need preliminary economic analysis. How much does alanate cost now? Target price? Economics for forecourt where wt% doesn't matter?
- The hypothesized mechanisms of Ti substitution are fascinating, and I applaud the PI for at least proposing a hypothesis about the role of Ti; however, several questions arise with regard to this: 1) The x-ray diffraction data looks extremely noisy; is there a reason that one only can get 0.01 Angstrom resolution? (This is nearly the size of the entire effect that you're looking for. I understand that the samples for the neutron diffraction data are much higher purity, but you give two orders of magnitude better resolution in this work – down to 0.01 pm.) This diffraction data also seems to be key in the leap to the idea of bulk substitution; to that end, it is critical to be able to explain the unusual shape of this data. (Is this unusual shape also reproduced by Gross, for instance?)

2) Another key component is the idea that Ti substitutes on the Na site, thereby creating two H vacancies (and a possible avenue for enhanced kinetics?) as well as leaving the Al available for H binding. This idea of the 3+/1+ substitution seems quite surprising, particularly when there is a 3+ cation (Al) in the structure. So, it is critical to understand whether or not the Ti^{3+} goes on the Na or Al sites? Also, where is Ti in the dehydrided state? This is just as important as understanding the mechanism for Ti in the hydrided state, and it seems as though there's even less information about this. The proposed test of 20% doping should help to shed light on this most interesting problem (i.e. in a dehydrided sample with 20% Ti, surely one should be able to determine whether there is TiH_2, Al_3Ti, etc., and how much?).

- This appears to be a sound technical approach to understanding the effects of dopants.
- Methods are quite good, wide range of measurements to understand the system.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.60 based on accomplishments.

- Very good progression on mechanism definition.
- Within the limitations of the inherent materials problem, great science.
- "Bulk" model seems well established.
- The hypothesis for site substitution is quite interesting, if not yet completely compelling in my mind. However, the most interesting result of this hypothesis is that it provides, as you point out, an entirely testable idea for improving the storage density of the alanates. Whether or not one can get 6 wt% out of a highly doped (but NaCl-free) sample is a clear test of this idea. It will be interesting to see the progress along these lines.
- Following the idea a bit further: why not just remove Na all together and form a Ti alanate or Al_3Ti hydride. Does the hypothesis predict anything about the storage properties of these materials?
- Ti was shown to be the best and lightest dopant. $NaAlH_4$ data was promising, but kinetics were shown to be too slow.
- They learned that doping is a bulk effect, and used experimental methods to determine the effects of doping on the lattice structure of the material. They also verified changes in plateau pressures resulting from doping, and developed a theory regarding sodium and a method for removing it, and thus potentially improving storage capacity.
- Making good progress in improving kinetics and understanding the bulk phenomena.
- Work de-salting is in the direction of meeting goals in solid phase.

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

This project was rated 4.00 for technology transfer and collaboration.

- Many collaborations. Great coordination.
- Great collaboration. Probably too early for industrial engagements and little is done (appropriately). No criticism of work or approach. Need to find fundamental limits. Great basic work.
- Excellent collaborations with a wide variety of researchers.
- They've got everybody involved.
- Set up work with needed people (university, national labs, etc.) to advance the work.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.67 for proposed future work.

- Future work is ill-defined. This is OK for university work.
- Recommend focusing resources on more promising materials.
- Only continue if learning is fundamental and transferable to other systems. Recommend moving to other system.
- Again, the testable hypothesis should be pursued, not only to obtain a higher storage material, but to confirm or deny the role of Ti in $NaAlH_4$.

- Some work needs to be done in the role of Ti in the rehydriding process. There is very little information in the community about this important piece of the puzzle.
- The plans for further characterizations of materials flow logically.
- Plans well directed and on important targets.
- Cycling of desalted material and production of higher storage materials are key and should be given precedence over others planned.

Strengths and weaknesses

Strengths
- This is good science which may lead to other potentials for hydrogen storage. It also improves the fundamental understanding of the chemistry and physics associated with these materials.
- Good understanding of the process.
- All his hypotheses are testable. It is important he test them and if the answer is he’s wrong, be brave enough to accept it rather than fight the data (By the way, I think he has that courage).

Weaknesses
- Need clean statement of approach. This program is a little too free-form. Not a lot of new results presented. Too much emphasis on patents.
- Only real weakness is the ultimate alanate specific mass.

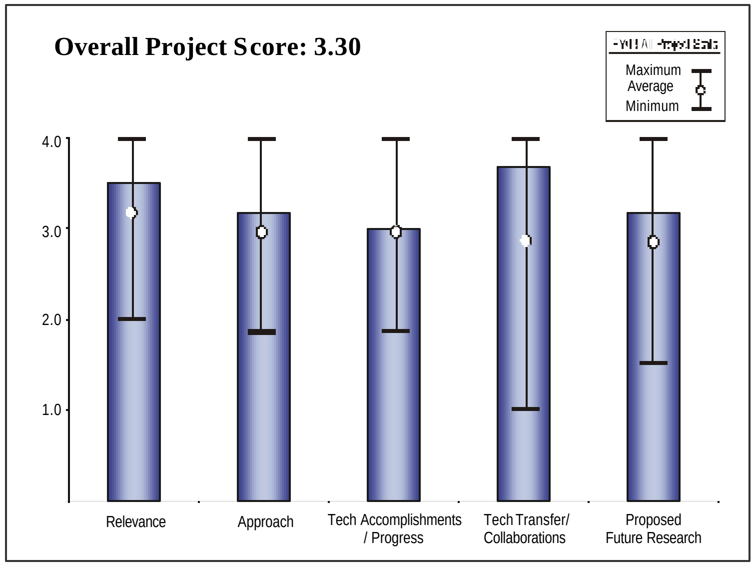
Specific recommendations and additions or deletions to the work scope

- Keep going!
- Would be good to understand the molecular mechanism (how does the large island of metal dissolve in the NaH so fast, what is the actual process of hydrogen getting in and out of the solid).
- It would be good to try to get smaller error bars on lattice work.
- Find out where the Ti goes in the dehydrided form!

Project #51: Hydride Development for Hydrogen Storage
Gross, K., SNL

Brief Summary of Project

Sandia National Laboratories is seeking to develop new complex hydride materials. They aim to develop materials capable of achieving 6 weight percent system hydrogen capacity, to improve kinetics of adsorption and desorption and thermodynamic plateau pressures of Ti-doped sodium alanate metal hydrides, and to improve processing and doping techniques of Ti-doped sodium alanate which will lower cost.



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

This project earned a score of 3.50 for its relevance to DOE objectives.

- Working on $NaAlH_4$ and "other" alanates right on target.
- Materials search is focusing on materials that have limited potential, would be better to expand material search through collaborations and high throughput approaches.
- "Conventional" alanate ($NaAlH_4$) will not completely reach DOE's goals, but this program is well thought out – to ascertain the empirical data and mechanisms behind the Na material, and extend this to other materials.
- It's nice to see a bit more effort on some of the engineering properties – this is good, we need this in addition to the fundamental understanding!
- The focus here is on OEM requirements. To develop practical materials for onboard hydrogen storage, basic science needs to be done. In particular, understanding the tradeoffs between kinetics and hydrogen capacity is fundamental, and using that knowledge to improve kinetics, storage capacity, thermodynamics and cycle life is absolutely relevant.
- Obviously only suitable for early goals, but may show the way to help other solids.

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

This project was rated 3.17 on its approach.

- Both engineering and scientific investigations are required. A road-map of the program would be helpful in determining progress and approach.
- Focus on materials search is much slower than I would expect.
- Measured thermodynamics (ΔH) of reactions. Why not do this as a function of dopant composition, and then you could settle this debate about whether or not the thermodynamics is affected by Ti?
- Although the specific approach was not spelled out, it could be derived from what was accomplished. A logical progression was used to determine the effects of dopants on hydrogen storage of alanates.
- Solid research program.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- Good progress.

- Good progress, but limited hard edge focus. Several folks working together, sharing with Univ. of Hawaii program would expect some element of program forming or approaching synthesis and surveying of new materials.
- The work is clearly carefully performed and of high quality.
- Ti was demonstrated to be important as a dopant. They also determined that it functioned by destabilizing the system, and that both kinetics and thermodynamics are in play. And they figured out how to directly synthesize some hydrides.
- Good understanding of the evolution of the system in bulk.
- Mg_2FeH_6 is an interesting sidelight.
- Materials properties are a critical step in implementation. Absorption work is important in improving charging conditions.

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

This project was rated 3.67 for technology transfer and collaboration.

- Many international collaborations.
- Too early to transfer good "academic" type collaboration, but potential for an extended team!!
- Good collaboration so learn from and support other programs.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.17 for proposed future work.

- Trajectory of work on track.
- Troubled by slow pace planned to explore more attractive material composition space!
- Perform preliminary economic analysis. Work on mechanical and electrical properties. Good addition to program!
- Plans are excellent and complement other DOE programs.
- They plan to investigate other complex hydrides, such as sodium alanates. They are also going to develop methods to determine engineering properties of the materials (thermal expansion, thermal conductivity, electrical resistance, etc.) These are all necessary for future system engineering.

<u>Strengths and weaknesses</u>

Strengths

- Much of this is good, basic research to determine how things work. Although this program may not lead directly to the true solution, the techniques and results will probably lead to developments elsewhere. The focus on eventual system engineering is very good.

Weaknesses

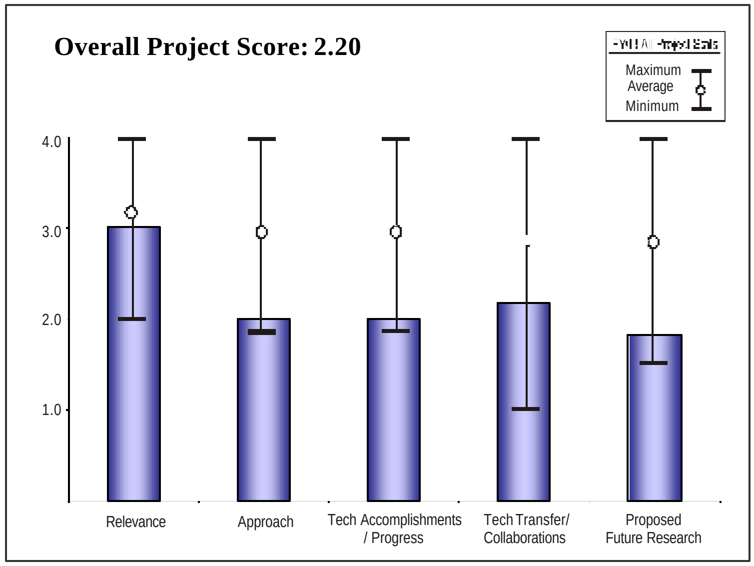
- Weakness of hydride effort (not just this work) is the lack of a HTE/coordinating materials program and of an integrated computational/modeling effort to look at the experimental work. Strongly recommend that such an approach be considered.
- Frankly, more work needs to be done before I can begin to poke holes in it.

<u>Specific recommendations and additions or deletions to the work scope</u>

- Keep going!
- For the next program review, generate a slide specifically defining the technical approach.
- Keep funding these guys. Engineering properties are not easy, I am glad they are doing it because this team is good and careful.

Project #52: Hydrogen Storage Using Complex Hydrides
Slattery, D., Florida Solar Energy Center

Brief Summary of Project



In this project, the Florida Solar Energy Center (FSEC) is working to identify and prepare a complex hydride that reversibly stores at least 6.0 wt% hydrogen and that optimizes kinetics by the addition of a catalyst. To do this, FSEC will determine hydrogen uptake and release characteristics of each compound in pure and catalyzed form and will study mechanisms of catalyst action using physical methods such as x-ray photoelectron spectroscopy, Auger electron spectroscopy, and x-ray diffraction.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- These materials need to be evaluated in both charge and discharge forms!
- New materials are critical to the President's Hydrogen Fuel Initiative.
- It is nice to see more work on (theoretically) higher capacity materials; however, the technical merit of this work is not very impressive.
- This program was focused on the original DOE targets, and did not take into account the balance of plant hardware requirements.
- The original scope was quickly determined to be too aggressive, indicating that they really didn't understand the magnitude of the problem. They therefore limited the scope to lithium aluminum hydride, which is relevant only because its light weight is directionally correct.
- Lighter complex hydrides are a good thing to look at, can meet near term goal and mid term goals.
- The development of improved materials for hydrogen storage is of key importance to the R, D, & D plan.

Question 2: Approach to performing the research and development

This project was rated 2.00 on its approach.

- A thermodynamic analysis should be performed on as many compounds as possible.
- The approach was not very clear with regards to synthesis of materials.
- Consider screening more promoters. Don't limit consideration to what others have done.
- The approach is sound, although fairly unimaginative.
- The approach was to look at the effects of material processing (ball milling), doping with elemental titanium, and doping with titanium chloride on kinetics and reversibility. The process flows fairly well, but it is not clear that they looked at other forms of processing or attempted to determine the causes of their effects. They also did not provide much rationale for their choice of dopants.
- Adequate to gain data with some work to understand the process as well.
- Alanates are a very promising class of hydrogen storage materials. However, this effort has been restricted to only the duplication of studies of Li and Mg alanates that have been published by other research groups. Progress will require innovation.

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

This project was rated 2.00 based on accomplishments.

- Little progress on stated funding. Not the PI's fault.
- The technical accomplishments of this work do not inspire me with confidence for the future. I do not recommend continued funding for this effort.
- Dehydriding of the base material and the effects of ball milling and doping on kinematics and hydrogen capacity were measured. They did allow results to change program direction. However, no theories or conclusions were developed to describe what was happening.
- The results were against highly modified goals.
- Progress on $LiAlH_4$ has not been very meaningful.
- Elemental Ti work with $NaAlH_4$ is nice.
- Little progress in the laboratory can be expected as no funding was received in the current fiscal year. However, some progress should have been made in the development of a strategy for the development of improved alanates.

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

This project was rated 2.17 for technology transfer and collaboration.

- A few collaborations but not extensive.
- No mention of universities or industrial partners.
- Some collaboration with Sandia.
- The envisioned collaboration with SNL has not been actualized. I understood the presenter to say that this is due to SNL requiring payment for their collaboration. The program director should intervene if SNL attempting to “double dip” and true collaboration should be initiated.

Question 5: Approach to and relevance of proposed future research

This project was rated 1.83 for proposed future work.

- The compounds are a good selection, but intensive work on each is required.
- Very unimaginative. Taking $NaAlH_4$ and replacing Na and Al with the next lighter elements ($LiAlH_4$ and borohydrides) in the periodic table is an obvious avenue for future work. There is no doubt that many researchers will take up this task. The real question is: what do you bring to the table?
- Their identification of $Mg(AlH_4)_2$ and borohydride powder as subjects for future study is good. However, synthesis of the former may be a distraction, and reversibility of the latter is questionable. Frankly, these should be investigated, but I’m not sure FSEC is the place to do it.
- Suitable but not very clear or ambitious.
- The future plans are not directed toward the elimination of barriers.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- Limited resources curtailed research. However, an apparent lack of initial understanding resulted in too broad of an initial scope.
- More explanation of the history and the program decisions made (and why they were made), and more focus on understanding the observed effects would be useful.
- Focusing on materials which could meet targets in systems would be more useful.
- Note: comments need to be tempered by the low cash they got.

- The project suffers from lack of originality.

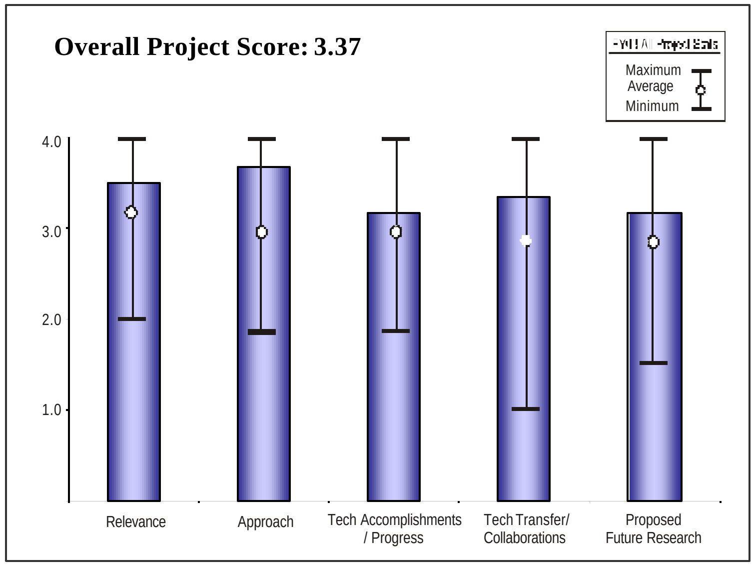
Specific recommendations and additions or deletions to the work scope

- The effect of heating rate on dehydriding of the compounds should be investigated.
- Future presentations should include a slide detailing the technical approach.
- Topic is a good one, however it is not at all clear they are the best or among the best to do this work.
- The focus of the project should shift to alanates and borohydrides with novel formulations.

Project #53: High-Density H_2 Storage Demonstration Using $NaAlH_4$-Based Complex Compound Hydrides
Anton, D., UTRC

Brief Summary of Project

United Technologies Research Center (UTRC) is working to design, develop, and evaluate a hydrogen $NaAlH_4$-based complex compound hydride storage system, having a 5kg hydrogen capacity and installation capability in a mid-sized fuel cell vehicle (FCV). In order to do this UTC will attempt to improve the charging and discharging rates of the $NaAlH_4$-based complex compound hydride by increasing the reversible weight fraction of hydrogen stored to 7.5%, enhancing the hydrogen evolution rate to meet steady-state demand, and increasing the regeneration rates to achieve the five-minute refill requirement. They will also seek to determine the safety and risk factors associated with the enhanced compositions.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.50 for its relevance to DOE objectives.

- A working demonstration of an alanate storage system is important for the development of on-board storage systems using hydrides.
- First real full-scale test of alanates!
- This work is crucial in that no one else is considering the system-level issues of hydrogen storage in complex hydride materials.
- Although $NaAlH_4$ will not meet all of DOE's goals, it is clear that the lessons learned in this effort will likely be applicable to future (higher density) hydrides as well.
- As a demonstration system for vehicular H_2 storage with fuel cell integration it is certainly consistent with DOE's objectives.
- Good broad approach to system.
- The development and evaluation of a hydrogen storage system based on advanced storage materials such as alanates is of key importance to the R, D, & D plan.

Question 2: Approach to performing the research and development

This project was rated 3.67 on its approach.

- While very nice work has been done on materials characterization and modeling, it is not clear that much work has been done on the design and testing of the storage system.
- Excellent ab initio work. Excellent safety analysis.
- After studying the slides, I now see the link between modeling and system building, and how it is an interesting use of thermocalc data to feed into heat management modeling! I would have liked to see this connection more clearly articulated in the presentation.
- Nicely focused on the demonstration of a specific system, $NaAlH_4$ with the option of using the developed methodology for future improved H_2 storage systems.
- The approach is well thought out in all aspects from material performance to safety testing protocols.
- It was refreshing and encouraging that good use was made of quantum mechanics-based computational methods to guide the effort, predicting the material's thermodynamic properties etc.

- Addresses most of the questions that need to be addressed to build a system. Nice mix of theory, engineering and materials science.
- The design of this project is excellent, given the time frame of the R, D, & D plan.

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

This project was rated 3.17 based on accomplishments.

- Good progress was made on modeling efforts and materials safety evaluation. More work needed on materials/system testing to reach goals.
- This project will define parameter/models for future complex hydride systems.
- The progress is quite encouraging.
- An excellent job thus far on performing basic safety tests according to UN/DOT protocols, basic kinetics and system modeling, etc.
- Making progress on many fronts.
- Interesting idea that the Ti may replace aluminum – needs verification.
- Tank model to estimate high savings is good work.
- Excellent progress has been made in system design and safety studies. However, there has been some inappropriate drift towards fundamental topics such as quantum mechanical calculations.

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

This project was rated 3.33 for technology transfer and collaboration.

- Several, if not many development partners.
- Nice consortium of university, industry and national labs.
- Good interaction with QuesTek.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.17 for proposed future work.

- The project has well defined Go/No-go points. Plans to reach those milestones were not clearly defined.
- Very high quality materials understanding and comprehensive engineering development work.
- Good plan directions, would like to see them aim higher though.
- Would like to see verification of the idea that Ti may replace Al.
- The future plans appear to be on target but no specifics were given.

Strengths and weaknesses

Strengths
- Very high quality comprehensive engineering development work.
- Strength is that they are looking at engineering and that needs doing even if they don't meet goal.
- This project brings the industrial experience and know-how that will be required for the timely development of hydrogen storage systems based on novel, high hydrogen capacity materials.

Weaknesses
- Main weakness is that this will not meet even our low goal.

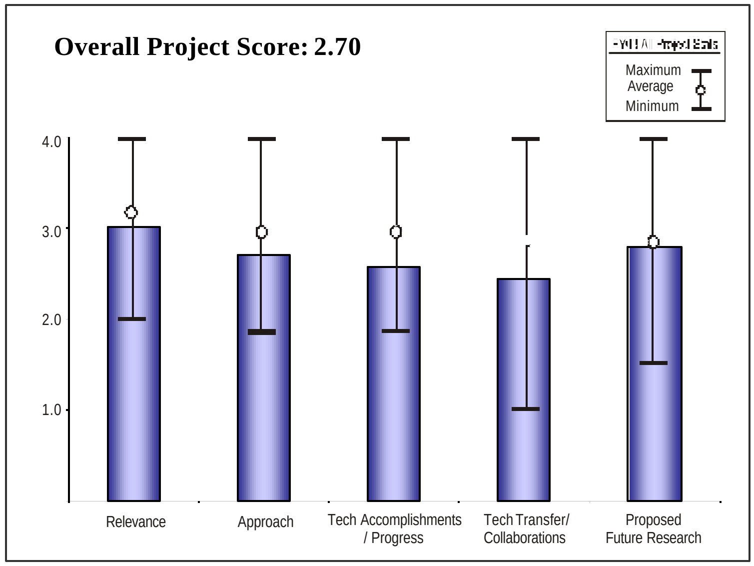
Specific recommendations and additions or deletions to the work scope

- Maintain sufficient latitude in the design to accommodate other reversible H_2 sorbents, preferably specifying what might be the ideal characteristics of such future sorbents.
- In the coming year, the PI should focus on scale-up rather than fundamental studies.

Project #54: Standardized Testing Program for Emergent Chemical Hydride & Carbon Storage Technologies
Page, R., SwRI

Brief Summary of Project

In this project, Southwest Research Institute will develop and operate a standard testing and certification program aimed at assessing the performance, safety, and life cycle of emergent chemical hydride and carbon adsorption/desorption hydrogen storage materials and systems. As part of this project, SwRI will work with industry and the US government to develop an accepted set of performance and safety evaluation standards.



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

This project earned a score of 3.00 for its relevance to DOE objectives.

- With the new, large programs to come, and with virtual centers of excellence this model for centralized testing makes less sense than it originally did. This will be an inefficient way to work the storage areas.
- Can act as an independent arbitrator in disputes but will not make key breakthroughs.
- An unbiased standardized testing facility is critical to keeping the hydrogen program on the right course.
- In its goal of providing a standardized evaluation of sorbents for hydrogen storage, the project is totally consistent with DOE's objectives.
- This will serve a very useful, though not necessarily nationally critical, function since its intended capabilities already exist in a few laboratories.
- Critical project to support all new materials work.
- The controversy that has arisen about the hydrogen storage properties of novel, carbon-based materials does not extend to other novel materials such as the alanates. Thus the envisioned center will be of value only to the evaluation of one class of hydrogen storage materials.
- Should one round of experiments on this now very suspect class of materials prove negative, the 3 million-dollar center will be of no further value to the program. Furthermore, it is questionable if even the initial round of results would result in a consensus opinion about the storage properties of advanced carbons.

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

This project was rated 2.71 on its approach.

- Having a broad range of test apparatuses is useful, but will "they" come?
- Soliciting input from all projects and experts in the field is the correct approach.
- Project is on track by focusing on the objective of developing a standard testing and certification program.
- The investigators have been very diligent in reviewing and carefully evaluating the various options for the needed instrumentation. Their intended plans seen reasonable but there is a concern that (as far as this reviewer is aware) they themselves do not have the personal specific experience with the various methodologies, and should in the course of their development and use, seek and utilize outside expert input.
- Being new to the field, they need to concentrate hard on making the basic measurements in a very dependable way, every time. Multiple test methods to measure hydrogen content is appropriate.
- Far too few labs were consulted about the facility design. Moreover, GMR was the only lab consulted that enjoys a generally well-respected reputation in the measurement of hydrogen absorbing-desorbing properties. Leading experts at JPL and BNL were not even contacted by email. As a result, one-of-kind, controversial

approaches, such as those employed at Air Products have received too much consideration while basic problems such as the inherent problem of buoyancy in high pressure TGA measurements have not even been considered.
- More input from the “hydrider” community that has made a living measuring hydrogen absorbing-desorbing properties for the past 30 years should be obtained BEFORE a facility is established that produces yet more questionable measurements on carbon-based materials.

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

This project was rated 2.57 based on accomplishments.

- Accomplishments limited due to limited funding.
- Made good progress, though it seems agonizingly slow. My experience is that this kind of decision/selection of a design and construction should take 12 to 18 months!! Is the government funding process slowing it?
- State-of-the-art review and visits to leading labs were performed.
- While they have made good progress towards the design of the required laboratory, there appear to be some important remaining issues: (1) The need for a method to determine the density of small samples of porous materials. This data is required for performing adequate buoyancy corrections when using the gravimetric analyzer at high H_2 pressures. (2) Measurement of heats of sorption. This data is just as critical for evaluating H_2 sorbents as capacity and there should be the means to acquire it – at least as isosteric heats from H_2 sorption isotherms run at different temperatures.
- Basically building the facility right now.
- The consulting of experts the measurement of hydrogen absorbing-desorbing properties has been far too limited thus insufficient progress has been made in the design of the test facility.

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

This project was rated 2.43 for technology transfer and collaboration.

- Limited imput. Should be much broader due to scope of program.
- Recommend continued efforts to visit other labs and solicit expert help and advice.
- In view of this it's suggested that the PI consult closely on an on-going basis during the project with people who have demonstrated expertise in the respective analytical methodologies.
- Did do lab visits, good, but seems largely an isolated effort without ongoing community input on a large scale. This may also affect buy-in at the end.
- If the envisioned facility is to be accepted as the premier, last-word testing facility of hydrogen absorbing-desorbing properties, then a much wider base of input and contact among university, government, and industrial labs throughout the world must be established.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.80 for proposed future work.

- Future work on track with program.
- Future plans were clearly delineated.
- This standardized testing laboratory will have to "earn" its reputation by being able to consistently get data that agrees well with preferably undisclosed data from well-established techniques in other laboratories.
- Plan to do multiple tests is a good one. Speciation is a good idea.
- Current plans to proceed with the purchase of equipment without seeking further input will lead to the creation of an irrelevant facility.

Strengths and weaknesses

Strengths
- Strong on facilities.

Weaknesses

- Weak on proposed application.
- This is going to be an overhead burden. May have greater value in looking at large scale storage materials later in the program. Better value would be to agree on test procedures and round-robin testing by all program participants as a condition for participating.
- The PI has been prematurely swayed by the opinion of a few high-stake holders in the "carbon controversy" and those with vested interests in selling a specific brand of equipment.

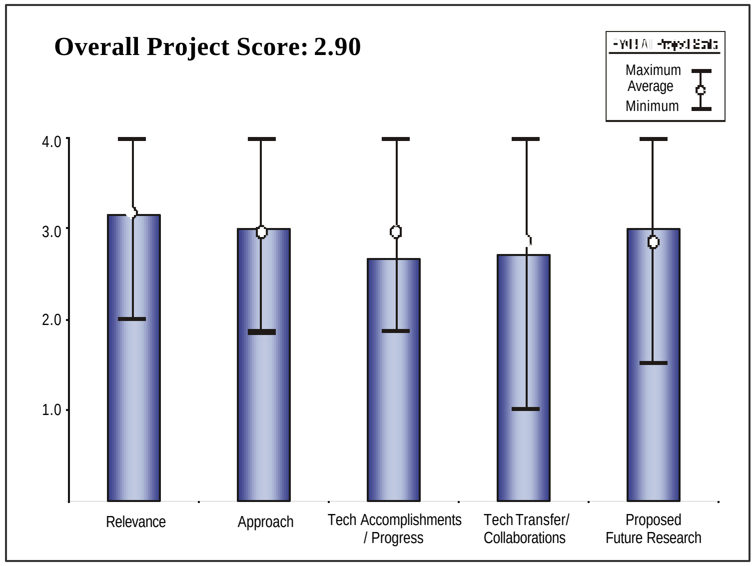
Specific recommendations and additions or deletions to the work scope

- Develop technical contacts and program content.
- Need to also provide heat of sorption measurements both for the small and large scale samples which are ultimately necessary for the design of any sorption-based H_2 storage system. The heat data could be derived from H_2 sorption isotherms taken at different temperatures but could desirably also be obtained from direct calorimetry.
- It is critical that the results from this lab be trustworthy AND trusted. A double blind round robin with the best labs in the country, or the world, is needed.
- Very little was said about protocol, this can not be ignored. The multiple test method is a good idea but it needs to be codified.
- The PI should consult with a broader base of experts as to the methods and techniques to be used in measurement of hydrogen absorbing-desorbing properties.

Project #55: H_2 Storage in Carbon Nanotubes
Heben, M., NREL

Brief Summary of Project

The National Renewable Energy Laboratory is currently working on hydrogen storage technology in carbon single-wall nanotubes (SWNTs) to determine the extent to which SWNTs can reversibly store hydrogen, to discover the mechanism of hydrogen storage in SWNTs, and to develop simple, reproducible, and potentially scalable processes for producing SWNTs.



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

This project earned a score of 3.14 for its relevance to DOE objectives.

- This family of materials has the highest potential to meet targets. It is also full of potential difficulty. There is a tough road ahead, but it is worth the trip! I like the concept of setting a deadline/gate!
- It is difficult to assess the relevance of this project until the results are confirmed and reproduced by an outside group.
- A high capacity, near ambient temperature, reversible H_2 adsorbent – the stated promise of this work - would if realized be a much needed and significant advance towards the DOE's Hydrogen Fuel Initiative.
- Nanotubes are a technically intriguing opportunity that need to be evaluated quickly to determine whether they are truly a viable storage alternative.
- If this is real, and if it can be verified, then it has the potential to be very relevant. Any hydrogen storage research which has the potential to meet the goal is relevant to an eventual hydrogen infrastructure.
- It is unclear if this technology can meet the standards, but if it does cycle 6% at moderate pressure and temperatures then it is well directed to intermediate goals.

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

This project was rated 3.00 on its approach.

- Would love to see a much broader materials-based collaboration, and characterization of materials by advanced tools. Also would like to see a computational chemistry component to integrate experimental results into a coherent model.
- Reproducibility is the key. This has to be addressed immediately. It must be the first thing to be done; there are lots of creative ideas for how this might be accomplished, but no matter; it must be. Until this is accomplished, additional work in this area is of little value to the community as a whole. The researchers at NREL have clearly made many efforts to answer criticisms and perform additional experiments; however, the highest priority should be verification of the results by someone outside of the NREL group.
- The approach of using carbon nanotubes for H_2 storage was initiated and pioneered by the PI (M. Heben) and it has captivated the interest and attention of scientific investigators world-wide who have provided both strong support and criticism for its validity. The consensus opinion is that the approach does have promise and will provide value if it's pursued as very careful scientific work at a basic science level, particularly in close collaboration with other laboratories.
- Good technical approach to trying to understand (and defend) new technology. Still need independent evaluation/confirmation of results.

- Perhaps because of previous skepticism, much of the direction seems to have been driven by the need to explain the history or this work and to justify results. Thus, it seems less focused, and more "shotgun."
- The scientific approach is broad and comprehensive. Many experiments done to try to answer questions raised outside – that's good. Need to understand how temperatures might be lowered or only about 3% can be accessed.

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

This project was rated 2.67 based on accomplishments.

- Improve work to address critical issues, but progress is much slower than I would like for such a critical program. Transport isues need to be addressed!
- As the PI admitted, little significant new knowledge was gained in the past year of the project because most of the teams' efforts were directed at trying to substantiate and reconfirm the previously claimed 7-8 wt% H_2 uptake by a carbon nanotube/metal alloy combination. But this now seems to be behind them and we look forward to seeing once again the creative and productive research of the previous years.
- Much time has been spent defending against criticisms. This isn't necessarily bad, as it goes to the core of the issue. They seem to have been drawn off target by the need to defend their work, but this has led to more suspicion as they apparently have not welcomed, allowed, or helped any independent verification.
- On the plus side, a carbon materials working group has been established, and they are working to separate the effects of coupling and metal incorporation.
- Interesting work to date.

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

This project was rated 2.71 for technology transfer and collaboration.

- Lot of room for improvement. But also need some creative solutions to IP concerns that must abound. Too early for industrial collaboration, but not for guidance!!
- The rating is due to the lack of urgency on the NREL group's part to see these results reproduced. This should be their top priority.
- The PI has been very open and communicative on an informal level with other laboratories and many of us have learned and benefited a great deal from these interactions.
- Needed is more formally established actual collaborative work with material sampling and current information exchanges. The PI's organization of the incipient "Carbon Group" is a good first step in this direction.
- Good cooperation/dialogue with other labs, universities, but little industry collaboration – however at this stage of the technology, this is not an issue.
- There is a negative with respect to independent verification of results. This must be resolved if the results are to be accepted as valid!
- Appropriate group of partners, industry is not appropriate at this point.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Future plan is not aggressive enough!
- Large samples (less than or equal to 100 mg) must be measured to gain credibility in larger community!
- Hopefully, with the Carbon Materials Working Group (or other mechanisms?) the verification and reproducibility of these results can be addressed in a short amount of time (< 1 year).
- As a combination of carbon nanotube synthesis, structural characterization and H sorption/desorption measurements with computational chemistry guidance, it's in an overall sense a good plan. However, too few details of these plans were presented to permit an adequate evaluation of the path forward.
- Certainly, the PI and his group have the needed synthetic and material characterization capabilities and there are some in-house computational chemistry colleagues, but it will be critically important to involve outside

laboratories where there are complementary skills and facilities to confirm H_2 storage data and provide external input and guidance.

- Need more work to prove out concept. Recommend exchange of materials with other laboratories to see if storage % can be attained elsewhere.
- The plans include a fairly good list of fundamental work required and a logical progression for those efforts. This might lead to improved storage densities in other types of materials. However, there should be an assessment as to what the potential is to meet future system targets.
- It would be beneficial to organize the work to answer the most pressing questions (is this a reproducible 7 or 8% storage method) first and then go on to the in-depth understanding.
- The ability to control types and avoid adding metal (or adding it in a controlled fashion) are important next steps.
- Plan is really very complete, the order is the only question.

Strengths and weaknesses

Strengths

- The strength of the program is in the technical capabilities and the quality of the NREL carbon group efforts with the personal determination of the PI to make it all work.

Weaknesses

- I wonder why a combinatory/HTE approach is not considered.
- The lack of detail in the proposed future research suggests a need for more focus and a clearer definition of the H_2 storage immediate and ultimate objectives.
- This is intriguing work, but more effort at independent verification of results needs to occur. If the goal is truly to establish a hydrogen economy, then parochialism and defensiveness must be eliminated.
- Reproducibility must be established to really move forward. Otherwise there will always be doubt.

Specific recommendations and additions or deletions to the work scope

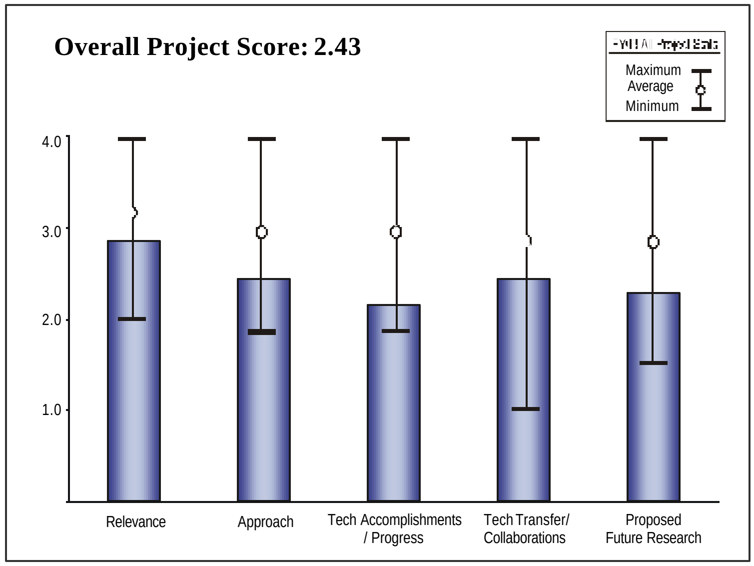
- The project should strive for the stated 2005 capacity goals, preferably targeted at some specified temperature range which should ideally be of the order of room temperature to ~80^{o}C for a mostly pressure reversible compressed gas storage material, where the H_2 is held by the intended "strong physisorption" interactions with the sorbent.
- Need independent verification of storage claims.
- Adsorption rates should be reported. Also, the presentation comes off as too defensive. If the work is truly good, it will stand up on its own merits to independent verification.
- Once proven to be reproducible, the primary goals need to be to access individual types of tubes, controllably add metal, and alter/tune temperature while maintaining capacity. Without control, the spectroscopy measurements will have much less value because it will be unclear which type of tubes are actually doing the work.

Project #56: Doped Carbon Nanotubes for H_2 Storage

Zidan, R., Westinghouse Savannah River Tech Center

Brief Summary of Project

The Westinghouse Savannah River Technology Center, in this project, is working on doped carbon nanotubes for hydrogen storage to develop reversible high-capacity hydrogen storage material that has greater than 6 wt% hydrogen capacity, favorable thermodynamics and kinetics suitable for transportation applications, stability with hydriding/dehydriding cycling, and resistance to trace contaminates.



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

This project earned a score of 2.86 for its relevance to DOE objectives.

- Class of materials form a goal.
- Could be of importance, but only if nanotube storage is reproduced and confirmed. Very little information given here to judge approach or future.
- Probably not worth funding unless nanotube area becomes more credible.
- This investigation on the preparation of doped carbon nanotubes, potentially with H_2 adsorption energies that are intermediate between physical adsorption and chemisorption, is certainly consistent with the H_2 Fuel Initiative.
- Nanotubes are a technically intriguing opportunity that need to be evaluated very soon to determine if they can be a viable storage alternative.
- Of course hydrogen storage is relevant, but 6% is too low. What is the theoretical maximum?
- It is unclear if this technology can meet the standards, but if it does cycle 6% at moderate pressure and temperatures then it is well directed to intermediate goals.

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

This project was rated 2.43 on its approach.

- Approach is creative - like model.
- Not very impressive progress.
- Characterization and cooperation present in other papers are missing.
- I'm bothered by lack of reference to Tony Baden's earlier work in this area!
- The approach of seeking ways to bind H_2 as a "weak covalent bond" to a site is practically what's needed but the example given of a sideways-bound H_2 to a transition metal is not illustrative since it requires an electrophilic center which a carbon atom in a nanotube is not. Just hoping to achieve this medium strength binding of H_2 may turn out to be experimentally realizable but providing a theoretical basis for it in terms of the function of the metal or N dopant would render the approach more credible.
- Appears to have taken care to ensure measurements of H_2 storage capability are repeatable and accurate.
- Insufficient information to judge.
- The focus was to generate large quantities of nanotubes. This is good. The path (using dopants) is OK, but they did not include anything about mechanical processing.
- General plan is good, specifics are not clear.

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

This project was rated 2.14 based on accomplishments.

- Not a lot of progress.
- The PI reported only on work done to the end of FY'02; a lack of funding apparently precluded its continuing pursuit this year. This explains why only sketchy data is reported here, namely the growth of C tubes with the inclusion of different metals – none really well characterized – and the preparation of some N doped tubes. The PI reported on an intriguing result of a net 1% reversible uptake of H_2 by a temperature-swing process from 50 to 250^{o}C. Unfortunately no further details could be presented since the data was said to be the subject of a patent application, which makes it impossible to adequately evaluate this finding.
- PI claims the ability to manufacture nanotubes and claims the ability of the nanotubes to uptake and release small quantities of H_2.
- Funding ended for '03. However, they did develop a method for growing tubes with different configurations and dopants. They also created nitrogen-doped nanotubes with stable electronic structures. Although hydrogen uptake and release were demonstrated, it was at a mere 1% level.
- Made some new materials. Cycling 1 percent or so at 250^{o}C. Progress is not fully clear..

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

This project was rated 2.43 for technology transfer and collaboration.

- Room for improvement.
- Collaborations with staff at Clemson and with J. Ritter of USC who is well known in the area of gas adsorption.
- Cooperate and exc hange of information with local universities.
- Probably need to be integrated into a more coordinated effort.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.29 for proposed future work.

- The PI did not in his talk make clear the proposed future research, except to say that further investigation of H_2 sorption in doped C-nanotubes would be pursued, a very wide field indeed. Some specific approaches with a sound theoretical backing should have been provided.
- Claims ability to manufacture nanotubes and claims ability of the nanotubes to uptake and release small quantities of H_2. Future work may be warranted only if nanotubes are found to be a viable storage opportunity; otherwise, there is little value in being able to manufacture them in quantity.
- The future direction looks good, and the sequence seems fine, but can they do it all? It seems rather ambitious.
- Probably appropriate future work if nanotubes pan out. Could perhaps be integrated with Heben work.

Strengths and weaknesses

Strengths

- None specified.

Weaknesses

- It seems that because of a break in funding, the project never really got off the ground. Also, no details of the reported interesting (but far from the DOE goal) H_2 "capture" data could be provided because these were said to be part of a patent application. Which makes it all very difficult to evaluate this work.
- Was willing to say carbon could not hold high amounts of hydrogen when all others were claiming great amounts stored.
- The goal is good, but the storage results aren't.

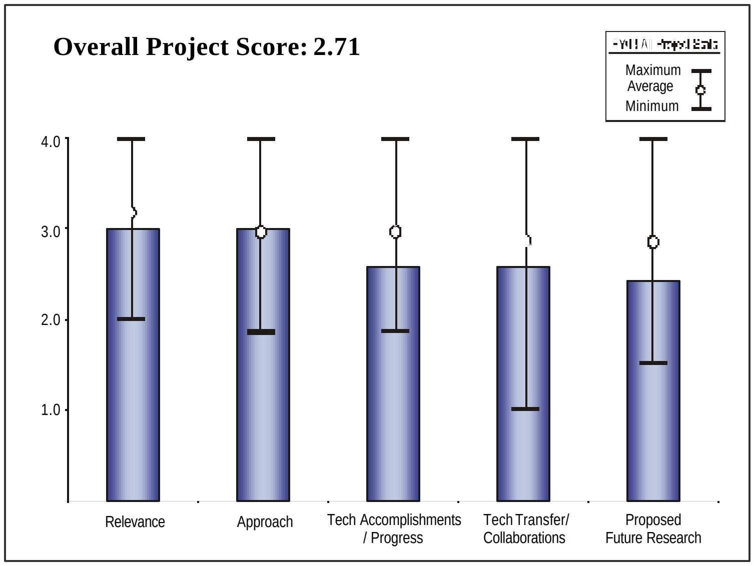
Specific recommendations and additions or deletions to the work scope

- The project would be greatly aided by collaborative accompanying computational chemistry or theoretical work that provides a substantiation and guidance for the effort. For example, should doping of C nanotubes with N fundamentally alter their potential interaction with H_2 and in what direction, to lower or higher heats? A guiding computational analysis of this should be possible at least using a simplistic model.
- Need to exchange material samples with other labs for independent confirmation of results.
- Include theoretical maximum wt%.
- Hold this work until the basic question as to whether nanotubes actually work is answered. If they are proven to store hydrogen, then this should be pursued.
- I would hold this one till some proof of nanotubes function is produced.

Project #57: H_2 Storage in Metal-Modified Single-Wall Carbon Nanotubes
Ahn, C., Caltech

Brief Summary of Project

The California Institute of Technology is working on hydrogen storage in metal modified single-walled nanotubes to alter adsorption enthalpy of hydrogen on single walled nanotubes (SWNTs) by use of a potassium intercalant, increase the number of adsorption sites in SWNTs by increasing the surface area through the use of intercalant, and improve the hydrogen storage capacity of SWNTs.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- At this point this is early, basic materials characterization work. I assume the project will be heading for evaluation and testing.
- The materials target is where potential action is!
- Doesn't seem particularly well aligned with DOE objectives.
- This fundamental investigation on metal-modified nanotubes with its promise of realizing an effective H_2 storage material is certainly consistent with DOE's objectives.
- Nanotubes are a technically intriguing opportunity to meet targets for H_2 storage. Viability needs to be assessed very soon to determine if work should continue.
- This is relevant in terms of what might be applied both to SWCNTs and to other storage systems. Increasing active surface area and understanding SWCNT physisorption are both necessary for these technologies to develop to their full potentials.
- It is unclear if this technology can meet the standards, but if it does cycle 6% at moderate pressure and temperatures then it is well directed to intermediate goals.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Working on a well-conceived plan to bring to closure or national potential, but this work is at very early stages.
- Good to have someone to actually show "negative" results for storage of certain materials. Without this type of talk, it is impossible to judge the field as a whole.
- It has been well established that alkali-metal intercalated graphite compositions adsorb H_2 at cryogenic temperatures more strongly than does graphite alone. The approach here is then one of trying to improve the affinity of H_2 for single wall nanotubes that have been similarly modified with metals. It's a perfectly reasonable endeavor.
- Procedures to purify nanotubes appear to be comprehensive. Evaluated materials from a number of sources.
- The approach was to use K-intercalated graphite as a guide. Although this might be logical, how do we know that this is analogous to hydrogen storage in carbon nanotubes? More background is required.
- Simple but focused. Unclear if graphite really tells us about nanotubes.

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

This project was rated 2.57 based on accomplishments.

- Progress less than I would have expected. Not clear about level of effort or other issues!
- Careful studies were done on the preparation of potassium intercalated carbon nanotube (SWNT) samples, the characterization of these materials by x-ray diffraction followed by an evaluation of their H_2 sorption properties. While no "dramatic" results of H_2 sorption were obtained, the data acquired is fundamentally important. It was interesting to see that K intercalates of SWNTs are stable while Li "unzips" the tubes.
- Making reasonable progress in evaluation of materials.
- Much of the work was distracted by the need to develop a way to purify purchased materials.
- Storage results were poor (< 1%), but they did verify that no "unzipping" was occurring -- and this is good because we need to see more honest reports of projects which have not been very successful. We can learn from problems as well as successes.
- Intercalated potassium. Found 1% storage. Progress is a bit slow but probably OK.

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

This project was rated 2.57 for technology transfer and collaboration.

- Surprised by how limited interactions are with others such as Carbon Nanotechnologies, etc.
- Seemingly good collaboration with HRL labs and JPL, but it would be worth while for the PI to also work closely with other labs that are doing research on H_2 sorption and carbon.
- HRL and JPL collaborations, but could be more broadly connected.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.43 for proposed future work.

- Does not overwhelm me. Don't see focus, hard data or key decision points.
- The future work doesn't seem particularly creative or imaginative.
- Reasonable plans for further work mostly on improving the intercalation methods with SWNT and other carbons, accompanied by various in-depth physical characterization methods and H_2 sorption studies.
- Need to continue work to determine if there is value in nanotubes.
- Although the progression is logical, it seems like "more of the same." A plan which leads to a real solution should be developed and implemented.
- Needs a clearer plan.

Strengths and weaknesses

Strengths

- Nice basic work driven mostly by PI's curiosity.
- The strength of this work lies in the focused approach towards evaluating the effects of alkali metal intercalation on the H_2 adsorptive properties of nanostructured carbons, particularly SWNTs, combined with an excellent utilization of physical measurement techniques including a new Sievert's apparatus.
- Was willing to say carbon could not hold high amounts of hydrogen when all others were claiming great amounts stored.

Weaknesses

- Need to collaborate with other labs that work on carbons for H_2 storage.
- The program was limited. However, the results, though not good, many lead to better understanding. The question is, "Will it lead to materials capable of meeting the goals?"

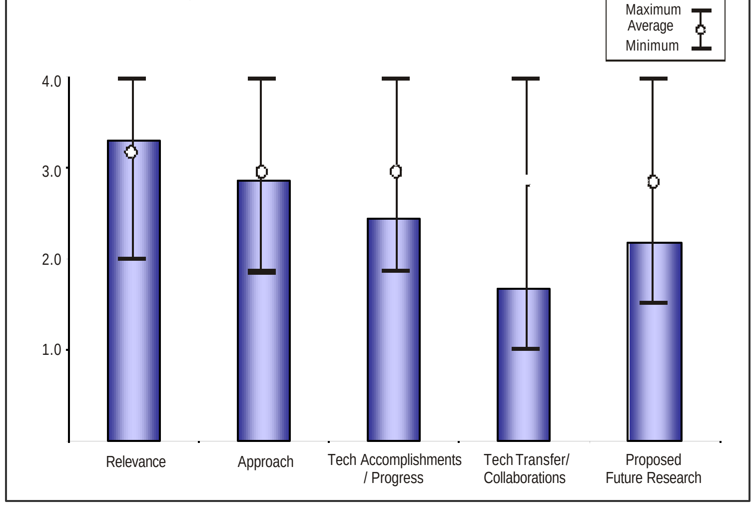
Specific recommendations and additions or deletions to the work scope

- Would consider seting hard targets and making hard decisions based on those.
- The H_2 sorption studies should be complemented by heat of sorption data which can be derived from adsorption isotherms. It's important because it essentially "deconvolutes" the two properties that principally govern H_2 capacity: the heat of sorption (i.e. the energy of interaction of H_2 with the sorbent) and the density of H_2 adsorption sites in the material.
- Need labs to exchange materials for independent confirmation of results.
- A clear set of goals and a clear approach as to how to achieve them should be defined.
- Channing does good work and probably should be funded, but needs a clearer plan of what exactly DOE will get for their investment.

Project #58: Hydrogen Storage Via Ammonia and Aminoborane
Raissi, A., Florida Solar Energy Center

Brief Summary of Project

This project analyzes the cost, safety, and performance of ammonia-based chemical hydrides as hydrogen (H_2) storage compounds for fuel cell applications. Principal activities involve indentifying the pros and cons of using ammonia (NH_3) as a chemical carrier for H_2, evaluating the viability of autothermal NH_3 reformation on-board fuel cell vehicles, analyzing the viability of using ammonia-borane complex (H_3BNH_3) as a chemical hydrogen storage medium on-board fuel cell vehicles, and identifying the technoeconomic barriers to the implementation and use of amine borane complexes, in general, and H_3BNH_3, in particular, as prospective chemical hydrogen storage media on-board fuel cell vehicles.



Overall Project Score: 2.48

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

This project earned a score of 3.29 for its relevance to DOE objectives.

- I have mixed views on this. The focus on identifying potential high H content candidate molecules is very good. Most of the materials identified are not interesting - especially the borazine family. This approach is promising, but I would recommend moving to other topics.
- It is good to see some chemical storage in the DOE portfolio.
- Chemical storage is an opportunity for on-board H_2 storage. This work can help to determine if NH_3 storage will be viable.
- This is unique and intriguing background work. Chemical hydrides do have potential to meet the goals, and this was the only work in that area reported on this year.
- Could meet all goals if cost and engineering goals are met.
- The development of improved materials for hydrogen storage is of key importance to the R & D plan.

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

This project was rated 2.86 on its approach.

- Good approach, identified some, but not all the key issues. Would not continue!
- Due to the small amount of funding and the lack of "competition" in the area of chemical storage this year, the expectations of this program are likely to rise significantly in the coming year.
- Evaluated various materials and identified issues with many of them.
- The approach was logical. The attempt to identify a complex based on capacity, safety (including toxicity), and usability indicates an understanding of OEM requirements.
- Use of literature search is efficient.
- Focus on cheaper production is important.
- Fine for limited funding. Would want to see more if he got real money.
- Materials with high hydrogen content have been identified that have not previously been studied as hydrogen storage materials. They are clearly not economically viable candidates for use as “chemical hydrides” (hydrogen obtained via irreversible hydrolysis).

- The viability of borozenes as reversible hydrogen storage media is questionable as equilibrium plateau pressure with the hydrogenated product would be less than 1 atm below 200^{o}C and catalysts to overcome the appreciable kinetic barrier are known.

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

This project was rated 2.43 based on accomplishments.

- Good study - did not identify attractive candidates.
- For small amount of funds, the progress was good; if funded more fully, expectations will be considerably higher.
- Identified ammonia carriers – some hold as much as 17 NH_3 molecules! Also investigated metallic salt ammonia complexes. Cyclotriborazane is interesting – it has 2 known synthesis routes, and is relatively safe. Going to borazane releases about 6.47% hydrogen, and further decomposition yields another 6.5%. These results arevery promising, as they can conceivably meet the aggressive storage goals. This work was done with very little financial support.
- Given low funding, the literature review and limited lab work is OK.
- Basic checks of the viability of the system requiring tables of bond enthalpies and a few hours of effort have not been carried out.
- Also literature work relating to the tendency for some borazines to detonate and well as their toxic properties has not been carried out.

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

This project was rated 1.67 for technology transfer and collaboration.

- No mention of collaboration.
- At this early stage – during a paper study – other organizations need not be included. So this is not necessarily bad.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.17 for proposed future work.

- Would stop this at this stage.
- Cyclotriborazine is good approach to consider -- liquid H_2 carrier! Would $MCl+NH_3$ materials meet most of NH_3 safety concerns?
- A very interesting and creative idea.
- Insufficient time for presenter to make convincing argument for future research. No information in presentation about % wt. storage of H_2 or energy to release.
- No specific future plans were listed. However, the compounds identified offer unique possibilities, and should be fully investigated.
- Unclear plans, not sure they are poor plans. Need to review with time for questions.
- No plans beyond a few simple tests on a couple of compounds were presented.

Strengths and weaknesses

Strengths
- Low-cost scoping studies are valuable.
- This was a paper study, but it led to really intriguing possibilities.
- Good: Looking at real requirements like safety and cost.

Weaknesses
- Not so clear they are the best people to do this work. But as they are interested I would none the less let them break the ground.

- Research project is quite sketchy. More direction and detailed planning are required (i.e. outline of catalyst development effort).

<u>Specific recommendations and additions or deletions to the work scope</u>

- Pyrolysis hydride reactions which are difficult to stop prove a real concern. Presenter should have planned presentation to permit questions from reviewers and from the floor.
- A program to formulate these compounds and test them should be proposed and funded.
- More literature work needs to be carried out on the compounds being proposed as candidates for hydrogen storage materials.

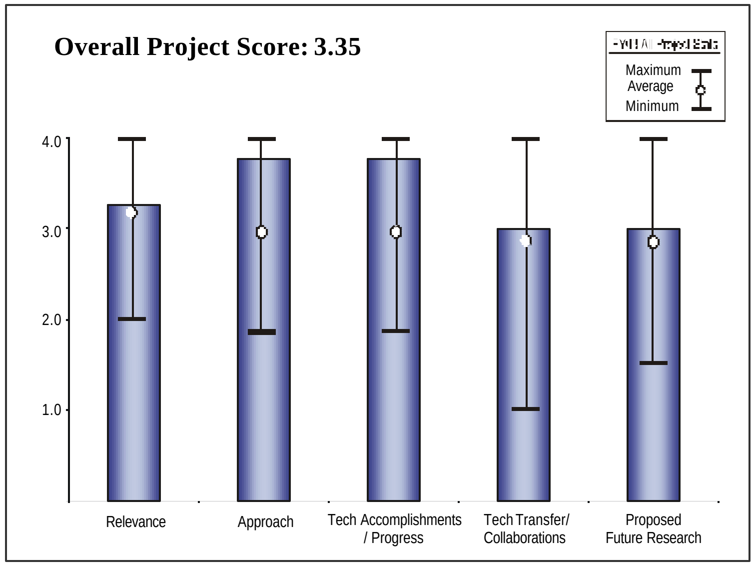
This category includes projects that are developing fuel cell technologies for the production of electrical power for use in vehicular applications as either a primary propulsion system or an auxiliary power unit or in stationary and portable applications. The aim of these efforts is to achieve the following milestones established in the Hydrogen, Fuel Cells and Infrastructure Technologies Program Multi-Year Research, Development and Demonstration Plan:

Fuel Cells

- Develop polymer electrolyte membrane automotive fuel cells that cost $45 per kilowatt by 2010 and $30 per kilowatt by 2015.
- Develop a distributed generation PEM fuel cell system with 40% electrical efficiency and 40,000 hours of durability at $750 per kilowatt by 2010, from the current 30% efficiency and 20,000 hours of durability.

Project #67: Study of Fuel Cell Water Transport with Neutron Imaging
Arif, Muhammad, NIST

Brief Summary of Project



NIST is addressing the issue of neutron imaging for fuel cell water management. The goal is to transfer neutron imaging technology into a safe and user friendly facility (user is a fuel cell developer) for general public use. This will allow one to look inside a fuel cell during operation, ultimately enabling a process to facilitate efficient water removal formed during operation.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.25 for its relevance to DOE objectives.

- This project has two sides, on one side the technique is uniquely suited for imaging H_2O. On the other hand, I think its ultimate usefulness in FC will be very limited for reasons of resolution, sample size limitation, etc.
- More importantly, H_2O imaging is important, but is not critical to FC performance.
- More than once, visualization technologies have advanced mankind's understanding - I rank this work as one of those seminal visualization methods.
- Purpose is admirable and very necessary.
- This project is strongly relevant to the FreedomCAR fuel cell development objectives.
- This in-situ measurement technology is unique and very beneficial for good water management of fuel cell.

Question 2: Approach to performing the research and development

This project was rated 3.75 on its approach.

- To image H_2O, you can not beat this approach.
- It appears the rush to service industrial clients has minimized the more widespread dissemination of what has been discovered.
- Excellent approach to achieve fundamental advantages.
- Outstanding!! This is what OEMs and fuel cell developers want.

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

This project was rated 3.75 based on accomplishments.

- The project team did a good job at bringing this imaging technology to the new application. The results are as good as one can expect of this technology.
- I had last viewed only a poster on this topic - the progress from a blurry ill-resolved image to sharp, real-time water images is impressive!
- Improved stack diagnostics are critical. The results are impressive.
- Adequate resolution and speed achieved for Gas Diffusion Layers evaluation. Needs finer resolution for MEA evaluation?
- They should continue to work to develop better technique for finer resolution, e.g. development to apply finer scintillation screen.

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

This project was rated 3.00 for technology transfer and collaboration.

- I hope the presenter is more transparent in disclosing this.
- Strongly endorse more university collaborations so information is more widespread.
- Important to get fuel cell models as part of this activity.
- This project needs fuel cell developers who want to use the technology.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- I am skeptical the technique can be improved much more than what it is today. There are fundamental limitations and under a reasonable budget.
- Not really a future plan other than a good list of technical challenges (spatial resolution). Should there have been a list of key transport problems for either plate or MEA and how this technique will address this issue?
- Keep focus on method development.
- Membrane imaging is less critical than water removal, at least for now.
- Adequate resolution and speed achieved for Gas Diffusion Layer evaluation.
- Wider area evaluation is also necessary.

Strengths and weaknesses

Strengths
- Good technique. Good Results.
- Unsurpassed ability to visualize water in 9-layer assembly (bipolar plates) or stacks.
- Research emphasis is excellent.
- Very unique and powerful tool for fuel cell development.
- Good optimization has done for fuel cell application.

Weaknesses
- Nice image, but relevance is not clear.
- I suspect this team has learned that water movement is not how we currently model it - and would push for mechanisms to get this information out into a wider audience.
- Progress is slowed by IP issues.
- Should have sufficient resources to continue technology development.

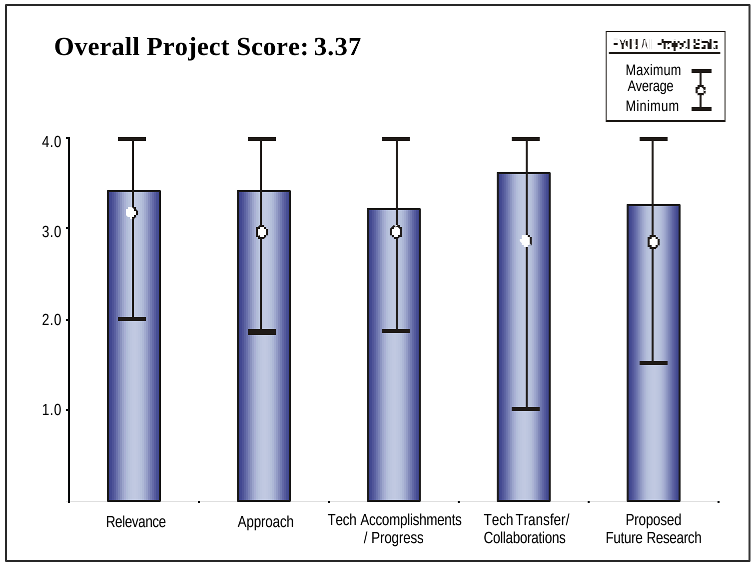
Specific recommendations and additions or deletions to the work scope

- I think we should realize this technique is well developed and may be near a wall. Needs to be realistic in potential of future development.
- Other techniques to look at H_2O (modeling, NMR imaging) should give better resolution at a lower cost.
- Strongly recommend increasing academic collaboration - especially with the major modeling groups. Can DOE start a solicitation to augment 3-way collaborations using this state-of-the-art tool? (NIST-Industry-University)
- Need to go after details about non-uniform current density (across fuel cell area).
- Recommend that temperature measurements (in plane of cell) be made concurrently. Should also vary the gas flows to get information with essentially uniform feed gas concentrations.
- Program needs to focus on verifying fuel cell models.
- This project should be continued to accumulate technology for the application.
- They should continue to work to develop better technique for finer resolution, e.g. development to apply finer scintillation screen. Wider area evaluation is also necessary.
- This is high priority.

Project #70: Integrated Manufacturing for Advanced Membrane Electrode Assemblies
DeCastro, Emory, DeNora

Brief Summary of Project

De Nora, in this project will work on integrated manufacturing for advanced membrane electrode assemblies. De Nora will create improved cathode structures and catalysts for polymer electrolyte membrane fuel cells (PEMFCs) without loss in performance at temperatures <100^{o}C that allow a significant reduction of precious metal. De Nora will develop PEMs capable of extended fuel cell operation at higher temperatures of 120 to 150^{o}C, at low relative humidity (RH), and without leachable components. De Nora will incorporate all these efforts with advanced MEA fabrication processes that are amenable to mass production.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.40 for its relevance to DOE objectives.

- The project directly addresses two of the critical features for fuel cells - better catalysts and better membranes.
- The focus is appropriate.
- MEA (ionomer, electrocatalysts, electrode structure, GDL) is the key component that must be improved for cost and durability while maintaining or improving performance.
- MEA architectures (and cost projections) are critical for overall success as the MEA is the "heart" of the stack.
- Use of "paper-type" processing leads to lowering manufacturing costs.
- Developments in high temperature membranes is good advancement (but needs more fabrication improvements).
- DeNora, DuPont and N.E. University bring a very unique expertise to this program.

Question 2: Approach to performing the research and development

This project was rated 3.40 on its approach.

- Approach to the catalysts and membranes involves separate projects - The effect of one on the other is crucial.
- The MEA performance of the new membranes with the ELAT electrodes is an important experiment to perform-need to identify controlling features of polymer and electrode interaction.
- Approach is comprehensive and appears well integrated.
- Solid approach to reducing manufacturing costs (new catalyst chemistry reducing loading, "old" paper-making approaches to improve graded density structure in the GDL, coating techniques).
- Systematic approach to higher temperature membrane for 120^{o}C (by partner).
- Kinetic T&A approach is "old" but aptly applicable.
- They, together, understand the issues and problems and their past accomplishments are the best available.

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

This project was rated 3.20 based on accomplishments.

- Significant progress has been made in both areas. Plans to continue them are not clear. Would be good to map this out for the future.
- Good progress - improvements in key areas are greater than two times demonstrated
- Graded density GDL represents solid advancement.
- Need more information on how this innovation can be sustained (and is this structure metastable?).
- Need more information on alloy structures and performance.
- Need more data on "ion-beam deposition" approaches.
- This team has a deep fundamental knowledge of the mass transfer and water management. They know that hydro-philicity/hydro-phobicity is the KEY at the 3-phase boundaries.
- Improved or better methods for catalysts characterization seem necessary to complement the electrochemical characterizations.

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

This project was rated 3.60 for technology transfer and collaboration.

- Excellent.
- Strong team coordinated and engaged - progress will be incorporated in commercial products.
- Strong university collaboration with publications.
- Not clear about role of DuPont in partnership (except in high temperature membranes).
- Clear integration with stack and system developers.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.25 for proposed future work.

- Plan seems appropriate.
- Work on new electrolytes is systematic.
- Not clear relative to polymer/inorganic future research.
- Need to understand the GDL/catalyst interactions better.
- The key issue preventing PEMFC is the cost of an MEA. DeNora & DuPont are in a very good position to formulate a low cost and high performance MEA.

Strengths and weaknesses

Strengths
- Teaming and individual team member contributions.
- Strength - very systematic approaches in GDL, new membranes (and understanding of polymer degradations).
- Solid (but minimal) data.
- Possibility for increased quantity of catalysts production is a strength of this project.

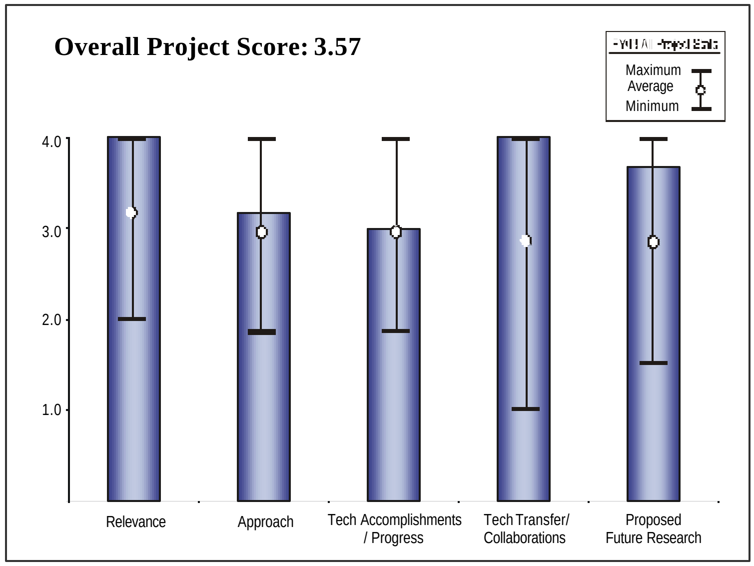
Weaknesses
- Need more information on possible advacements in catalyst (and catalyst stability).
- Need clearer explanations of innovations in GDL.

Specific recommendations and additions or deletions to the work scope

- Continuation of this project is recommended.
- Stay on focus: new improved GDLs, membrane innovations.
- Broaden the catalyst effort (or get improved catalysts from other sources).

- Drop the composite approaches (inorganic fillers) because such systems (under load cycling) are prone to defect formation.
- Expand the program and provide them with more funds.

Project #71: Development of High-Temperature Membranes & Improved Cathode Catalysts
Perry, Mike, UTC



Brief Summary of Project

UTC Fuel Cells will develop high temperature membranes and improved cathode catalysts in this project. The advanced polymeric membrane will be developed to operate at near ambient (1-1.5 atmospheres) pressure in the temperature range of 120~150^{o}C and able to meet DOE's program goals performance. In addition, UTC will develop and demonstrate improved high-concentration platinum (Pt) cathode catalysts that will enable the reduction of Pt loading to 0.05 milligrams per centimeter squared and meet DOE's goals for performance.

Question 1: Relevance to overall DOE objectives

This project earned a score of 4.00 for its relevance to DOE objectives.

- The objectives of this project are aiming to satisfy the specific DOE targets. The scope of this project is very wide and covers key aspects of R & D in fuel cells.
- Program shows good focus towards high temperature membranes and oxygen reduction catalysts and the key technical barriers for enhancing the cell & system level performance of H_2-air fuel cells.
- High-temperature membranes and advanced cathode catalysts.

Question 2: Approach to performing the research and development

This project was rated 3.17 on its approach.

- The project has followed the right track by measuring the protonic conductivities of membranes at high temperatures (120-150^{o}C) and at RH lower than 50%.
- Stability tests under FC operational conditions with the best membranes are necessary. This also applies to catalyst stability.
- Definition of specific approaches to raise the current values of membrane conductivity from 0.01 S cm^{-1} to 0.1 S cm^{-1} (which is the DOE goal) needs to be better defined. Also sensitivity of membrane performance to RH in the range of 25-50% requires that these conditions are clearly defined for membrane downselect.

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

This project was rated 3.00 based on accomplishments.

- Good progress demonstrated by achieving 10 mS cm^{-1} with RH less than or equal to 50% at 120^{o}C (with various membranes). Good progress in modeling and predicting catalyst alloy activity. Catalyst stability should be tested in a FC under operational conditions.
- Substantial improvement over Nafion 117 has been demonstrated in the area of membrane conductivity.
- Modeling results on Pt-skin catalysts seems to have predictive value. However, plans do not show which alloys will be synthesized in the future and how the enhancements set forth in the goals will be achieved. Not clear as to how Pt pulse electrode position will lead to the "core-shell" catalyst architectures.

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

This project was rated 4.00 for technology transfer and collaboration.

- The progress demonstrated is a result of a well coordinated team. All the partners are working in their respective tasks very efficiently.
- Project shows excellent focus and coordination by UTC.
- Good team with respect for universities. Other "industrial" collaborations are with a sister company (UTRC) and IONOMEM.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.67 for proposed future work.

- Very clear and consistent time schedule.
- Result of durability at least in limited testing (500-1000 hrs) experiments would be key to downselection of membranes. Definition of such downselect criteria must be established. Better definition of performance and stability criteria would be key for downselect.
- Catalyst work appears open-ended.

Strengths and weaknesses

Strengths
- Good technical team and coordination.

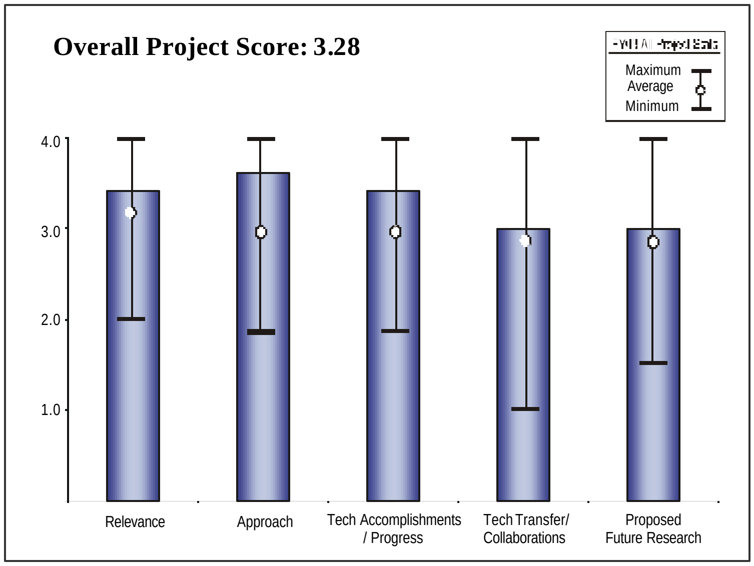
Weaknesses
- None that is significant.

Specific recommendations and additions or deletions to the work scope
-
- Recommend that all tasks be continued with better definitions of measurable targets for early major task.

Project #72: Advanced MEAs for Enhanced Operating Conditions
Debe, Mark, 3M

Brief Summary of Project

3M is working to develop high performance, lower cost PFSA based ionomer MEAs amenable to high volume manufacture. These membranes will be qualified to meet demanding system operating conditions, such as a temperature range of 85°C to 120°C with little or no humidification, will contain less precious metal catalyst, and higher durability than state-of-the-art constructions. 3M is also developing MEAs based on non-aqueous proton conduction mechanisms that will be capable of operating in temperatures of 120°C to 150°C.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.40 for its relevance to DOE objectives.

- Outstanding. DOE's support of projects like this is an excellent investment.
- The work on the higher temperature membranes would improve system design, but is not critical for the stack performance itself. Their efforts on lower cost materials and catalysts would be very worthwhile.
- Comprehensive program that addresses the important issues at MEA assembly: catalyst development and membrane development. It's a good plan to integrate and scale-up.
- Right on the money in many areas. It will impact the longer-term goals significantly if successful.
- Approach to roll-goods manufacturing processes is laudable. Non-carbon catalysts are innovative; but more data needs to be generated. Costs of new perfluorinated structures not provided.

Question 2: Approach to performing the research and development

This project was rated 3.60 on its approach.

- They are non-Nafion, but Nafion-like (i.e. perfluorinated sulfonic acid ionomer) could offer low temperature conductivity as well as high temperature operation.
- Complex program, but its approach brings effort to apply on crucial issues.
- MEA formation and operation with new catalysts and membranes is central to critical points.
- Excellent! Could do with more information on what are the critical properties to measure MEA structure.
- Solid! Addressing the right issues; understands which issues need to be brought to the top of the priority list.
- A bifurcated program with two temperature regimes is bold! Road map/path which stresses critical property analysis is excellent. Builds on 3M fluorine chemistry strengths.
- Excellent idea to drop C in MEAs - very innovative and may lead to new findings.

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

This project was rated 3.40 based on accomplishments.

- Results of cathode catalyst development work have shown promising results. The results shown for the higher temperature membranes are inconclusive. They are still far from desired target volumes.
- Progress is good but have advantage of prior contacts to start from.
- Excellent progress; may yield a new electrode design; could be the first in years.

- I have mixed reactions to 3M's claims re: less fluoride generation by their new membranes. Are these results because of mismanufacturing of Nafion-based MEAs? Advanced catalysts results are impressive (and unexpected) - surface area, activity.

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

This project was rated 3.00 for technology transfer and collaboration.

- The presentation listed several university, national lab, and industry interactions and collaborations.
- Catalyst development good but based on prior relationship. New relationships are slow to form. 3M should do better.
- Good.
- Stray collaborations with universities; but 3M appears to be very "possessive" relative to industrial collaboratives. 3M needs to open up its innovations (re: polymers and catalysts) to neutral 3rd parties.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Little detail was offered other than continuing on the present development approach.
- Proposed work is based on solid planning and is results oriented.
- Not much detail given; because the future builds on existing programs without some focusing.

Strengths and weaknesses

Strengths
- The 3M work is making outstanding progress toward overcoming the most important technical obstacles in high temperature PEM fuel cell operation and commercialization. The work makes me optimistic that HFCIT program goals can be achieved. I only wish they could have revealed more of the proprietary details.
- The perfluorosulfonic acid membranes have the potential to offer reasonably high proton conductivities at room temperature (for start up), but the shown conductivities are still too low.
- Solid approach. Logic and know-how is good.
- Many innovations (two temperature regimes; builds on 3M's fluorine chemistry strengths; great innovation to drop C in catalyst structure).

Weaknesses
- Needs to "open up" on the technical innovations to third party industrial firms.

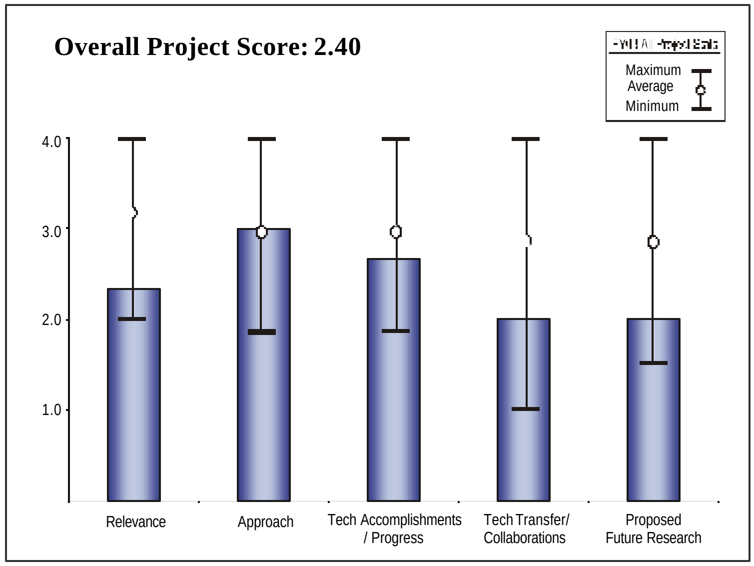
Specific recommendations and additions or deletions to the work scope

- Keep up the approach. Right on target.
- Conductivity and fuel cell polarization measurements should be extended down to + 20^{o}C, perhaps even down to -20^{o}C.
- Keep it up!
- Stay the course; but do accelerate the outside collaborations with industry.
- 3M's innovations should be independently verified because, if proven out, their developments could have major impact.

Project #73: R&D on an Ultra-Thin Composite Membrane for High Temperature Operation in PEMFCs
Yuh, Chao-Yi, Fuel Cell Energy

Brief Summary of Project

In this project, FuelCell Energy, Inc. is researching and developing an ultra-thin composite membrane for high-temperature operation in polymer electrolyte membrane (PEM) fuel cells. This ultra thin membrane will be capable of operation at 100-140°C, with less than 0.2 Ohm-centimeter-squared membrane ohmic resistance and less than 1% crossover, with less than 0.6 V performance at 400 mA/cm squared under ambient reformate/air operation at 120°C. In addition, this PEM will have high ionic conductance with negligible electronic conductivity and high mechanical strength.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.33 for its relevance to DOE objectives.

- Originally funded as part of stationary systems; only looked at atmospheric system (transportation will almost certainly be pressurized); higher temperature operation is important.
- The successful development of a higher temperature membrane would lead to an increase in CHP efficiency for stationary applications and would assist in deploying distributed generation systems.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Showed good understanding of sources of cell performance loss at higher temperatures, however methods for improving cathode performance (while moderately successful) were not well explained. Adding high temperature proton-conductivities into membrane electrode assembly (MEA) will not ultimately achieve the DOE program goals.
- Good explanation of why you have focused your effort on the cathode.
- Project initially started out to develop a high temperature membrane. Need for better performing cathode is recognized, but is not adequately addressed. Approach to high-temperature membrane by adding high temperature proton conducting compounds is being tried elsewhere.

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

This project was rated 2.67 based on accomplishments.

- Improved 120°C operation as achieved, but still far short of DOE goal. 500 hour endurance test is insufficient!
- Good to see that you have done testing on the larger 300 square cm test cell. Looks like reasonable results are being obtained.
- Some success was achieved however further work is required to improve performance and understand performance degradation mechanisms. The project is over with considerable work yet to be done to have a viable membrane.

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

This project was rated 2.00 for technology transfer and collaboration.

- Sub-contracted out large portions of the work.
- I would encourage more collaboration (if possible) outside of direct subcontractors (i.e. national labs, universities).
- FCE does not have history in PEM fuel cells and appears to rely on Ion Power for membrane expertise. Who would manufacture this membrane if it were successfully developed?

Question 5: Approach to and relevance of proposed future research

This project was rated 2.00 for proposed future work.

- N/A - project ends in July 2003.
- Future plans beyond this contract (or this year) were not clearly presented.
- Project is over. Not much detail provided on proposed future work.

Strengths and weaknesses

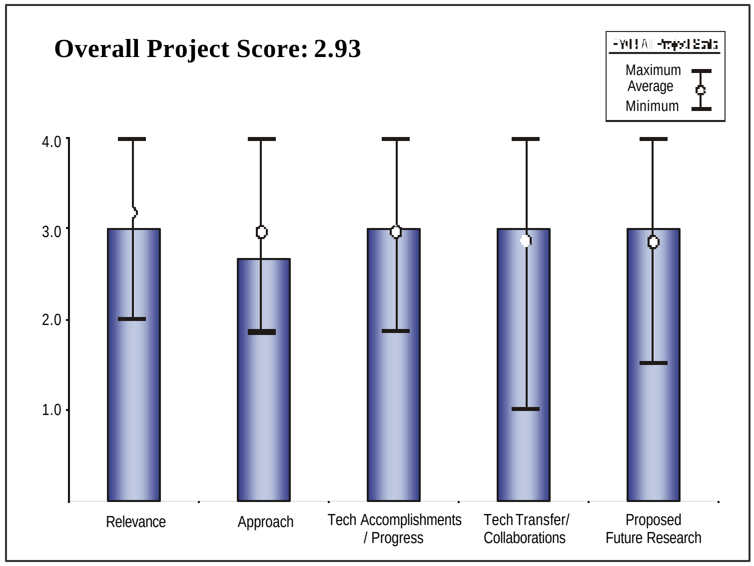
Strengths
- Achieved 120^{o}C performance over Nafion.

Weaknesses
- Did not discuss decision-making procedure for selection of proton-conducting additives.

Specific recommendations and additions or deletions to the work scope

- DOE should be funding research which is higher risk/reward. High temperature generation of Nafion-based PEMFCs is a non-starter. New, unique polymer chemistries are where DOE should spend its money.
- Don't extend this effort unless a clear path to a viable product is shown.

Project #74: Development of High-Performance, Low-Pt Cathodes Containing New Catalysts & Layer Structure
Atanassova, Paolina, Superior MicroPowders



Brief Summary of Project

Superior MicroPowders is working on the development of high-performance, low-Pt cathodes containing new catalysts and layer structure. Superior MicroPowders will develop a combinatorial powder synthesis platform based on spray pyrolysis for discovery of high-performance low-Pt cathode electrocatalysts along with engineered cathode layer structures containing the new electrocatalysts. They will demonstrate enhanced performance of membrane electrode assemblies (MEAs) with low Pt content towards the DOE goals of 0.6 g Pt/kW in automotive applications for the year 2005.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Working on higher cell (cathode) performance and advanced manufacturing techniques.
- The objectives as stated address critical issues. The approach, however, may produce little that is of interest.

Question 2: Approach to performing the research and development

This project was rated 2.67 on its approach.

- Presentation was hard to follow and not enough detail was presented to determine degree of novelty. No evidence that spray pyrolysis produces new Pt microstructures as claimed. Combinatorial synthesis rediscovering same Pt M1 M2 combinations already reported by others.
- Both, the catalyst and the membrane development approaches seem promising. The effort to develop catalyst nanoparticles with preferentially oriented facets may not produce desired results since the atoms at the edges make a major contribution to the activity.

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

This project was rated 3.00 based on accomplishments.

- Pt reduction is good.
- Appears only to be catching up to where others already are or were a few years ago. Not clear that processing costs are being reduced.

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

This project was rated 3.00 for technology transfer and collaboration.

- DuPont Fuel Cells does testing. GM consults.
- Not clear what DuPont is doing except providing membrane and doing some testing.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- Logical progression.
- Not clear where this project is going or who is going to benefit.

<u>Strengths and weaknesses</u>

Strengths
- None specified.

Weaknesses
- None specified.

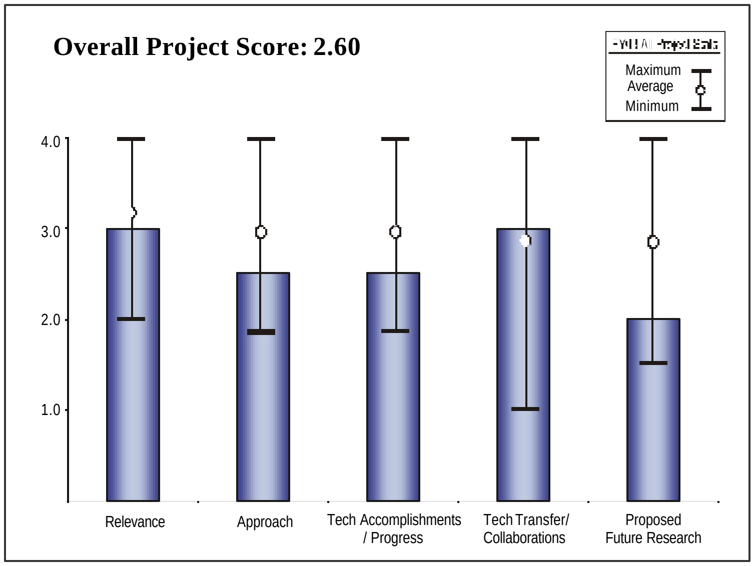
<u>Specific recommendations and additions or deletions to the work scope</u>

- DuPont needs to step forward and identify their role and where they see this project going. SMP is not going to produce MEAs or fuel cells.
- A coordinated development of the catalyst and the membrane should be maintained.

Project #75: Design & Installation of a Pilot Plant for High-Volume Electrode Production
Arps, James, SwRI

Brief Summary of Project

In this project, Southwest Research Institute designed and installed a pilot plant for high-volume electrode production. SwRI demonstrated proofs of concept and assessed electrode and membrane electrode assemblies (MEAs) architecture against FreedomCAR cost targets. Also, SwRI designed, built and installed equipment for a high-volume pilot plant capable of catalyzing tens of thousands of square meters of electrode material per year. They completed process development and qualification of the pilot program and benchmark MEAs against commercially available products. Finally, they incorporated MEAs into two "short stack" fuel cells built by General Motors and delivered to Argonne National Laboratory for testing and evaluation.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- MEA is the "tall pole" in the tent for cost.
- Program goals very relevant.
- Addresses key barrier (cost) for fuel cells, but has not done much to look at durability.

Question 2: Approach to performing the research and development

This project was rated 2.50 on its approach.

- Processing membrane in vacuum may add additional complications through membrane dimensions changing with hydration-dehydration.
- There was no information on MEA cost or durability, so it is not clear whether or not the approach (vacuum web coating) is reasonable. These market barriers (cost and durability) were simply not addressed, a key deficiency in the presentation.
- No information on uniformity of the coating was provided. No comparison with existing alternative processes.

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

This project was rated 2.50 based on accomplishments.

- MEA performance good, but not stellar.
- No presentation of mass production statistics (uniformity, etc.). Catalyst losses in system are too high.
- This project has been underway for nearly 3.5 years, and still no information on durability and no realistic stack test yet.
- The major objective of this project is to address electrode manufacturing costs, yet no cost projections were presented.

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

This project was rated 3.00 for technology transfer and collaboration.

- GM, Gore testing. ANL stack testing. No evidence of collaboration in process development.
- Gore is engaged as a supplier of membrane and for testing performance and durability of SwRI's MEAs.
- GM is planning to do short stack evaluation.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.00 for proposed future work.

- This project nearly complete.
- Project is nearing completion, and does not build on progress to date. Seems to be focused on wrapping things up, rather than moving forward. The topics for proposed future research (i.e. Pd membranes, non-precious metal catalysts of reversible fuel cells) seem unrelated and do not build on current project.

Strengths and weaknesses

Strengths

- This is probably a much better project than what was presented.

Weaknesses

- None specified.

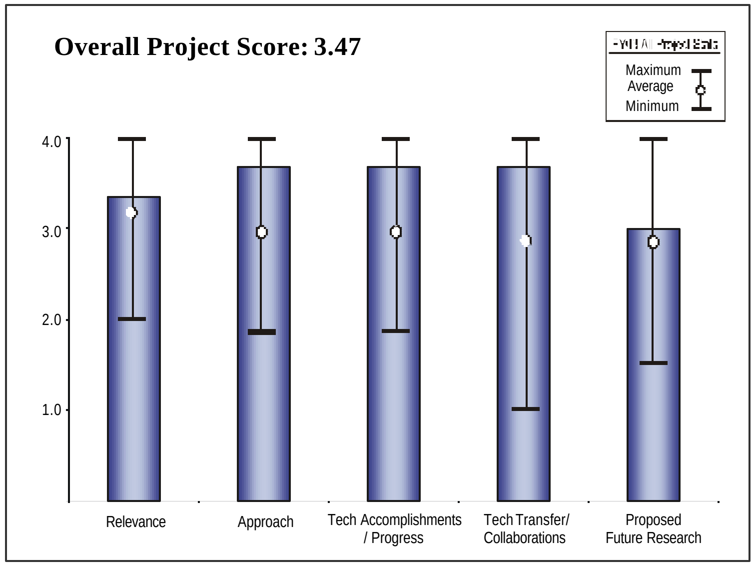
Specific recommendations and additions or deletions to the work scope

- Evaluate independent test results and examine process attributes (uniformity, speed, catalyst loss, etc.) to determine if follow-on is appropriate.
- Recommend closer link between Pt loadings used here (0.1-0.2 mg/cm^2) and costing studies by TIAX and DTI.
- Recommend that this project make realistic cost projections using information on equipment costs, production/labor rates, scrap rates, platinum losses, etc.

Project #76: Scale-Up of Carbon/Carbon Composite Bipolar Plates
Haack, David, Porvair Corp.

Brief Summary of Project

Porvair is building and demonstrating a pilot facility capable of producing 300 carbon/carbon bipolar plates per hour. Plates will possess properties that meet or exceed performance requirements over an extended period of use. Porvair has begun investigating processes for high-speed material forming, pattern forming and thermal treatment.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.33 for its relevance to DOE objectives.

- Not ranked "4" because there are other approaches that can be used.
- Outstanding example of taking concept development at National Laboratory (ORNL) to commercial scale. Lab and industry clearly worked together closely.
- Bipolar plate is a critical part of fuel cell system.
- Scale-up of fabrication is a necessary step in fuel cell communication.
- Address key barrier (cost) for fuel cells, but much of effort is directed to establishing production capacity rather than performance and cost improvements.

Question 2: Approach to performing the research and development

This project was rated 3.67 on its approach.

- Approach ranked "4" because it appears all barriers are overcome.
- Porvair identified and solved a key issue required for manufacture of bipolar plates to a scale of 1000's of parts.
- No information on cost, so can't determine how well this market barrier is being addressed. Showed data only on bulk conductivity, but need to focus on surface conditions and overall resistance (i.e. bulk and surface resistance).

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

This project was rated 3.67 based on accomplishments.

- Exceeding their targets ahead of schedule; indicates that the $10/kW goal will be exceeded, thus meeting customer needs.
- Progress in scale-up has been outstanding. Parts have been manufactured at a rate far beyond that originally anticipated for phase I.
- Good progress toward establishing production capability. No information presented on progress towards cost targets.

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

This project was rated 3.67 for technology transfer and collaboration.

- Have taken ORNL process and shown that large scale manufacture communications are feasible.

- Porvair and ORNL clearly collaborated very closely in this scale-up activity. Excellent example of communication of a technology developed at a national laboratory.
- Good coordination with stack developers. Should lead to good evaluation of plates under realistic conditions.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- What happens after Phase II line is complete?
- Time to get DOE support and sell to the customers?
- Project appears to be complete.
- Issues remaining to be solved for larger scale manufacturing probably should now be the responsibility of Porvair.

<u>Strengths and weaknesses</u>

Strengths
- Outstanding success in Phase I.
- Good scale-up from lab to pilot plant.

Weaknesses
- Outside of the scope of this project, but I would have liked to have heard something about how these are performing in actual stacks.
- Sealing successfully addressed?
- Scientific principles/technical barriers for Phase II scale up not as clearly defined.
- No cost projections.
- No data on interfacial resistance of plates.
- No mention of scrap rates.

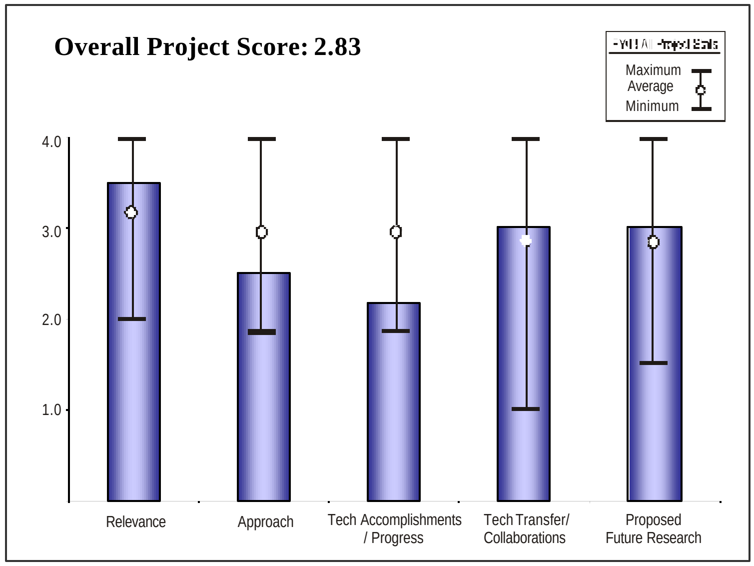
<u>Specific recommendations and additions or deletions to the work scope</u>

- Phase II line scheduled to be completed in December 2003.
- Strong customer demand.
- Not a research program anymore. Time for phasing out of DOE R, D, & D funding?
- Goals of DOE program appear to have been met.
- Further development should probably be the responsibility of Porvair.
- Recommend that cost projections be specifically included in work scope. If not already done.

Project #77: High Temperature Polymer Membranes for Fuel Cells
Zawodzinski, Tom, Case Western Reserve University

Brief Summary of Project

In this project, Case Western Reserve University and universities, national labs, and industry are developing new membranes and membrane electrode assemblies (MEAs) which can operate at 120º - 150ºC.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.50 for its relevance to DOE objectives.

- Agree with presenter's comment that program is finally "launched." Excellent mix of projects and PI's with fundamental expertise in materials synthesis & properties rather than fuel cell researchers trying to tweak the materials. The variety of approaches mitigates risk and will likely lead to several long-term solutions for high T membranes.
- High temperature membranes are holy grail of fuel cells. The success of this and other HT work will have dramatic implications on power system simplicity.
- This is among one of the most important also most challenging technologies to enable the H_2 fuel cells in transportation. It's important to support this research with realistic view of the high risk and long term nature.
- There is no question that the development of superior membranes for PEMFCs-DMFC, RHFC, and H_2/Air over a wide temperature range, is critical for the commercial realization of FC based power systems. However, I have severe reservations about how well this particular program will meet this objective so much so that I would rate it a 1 on your scale.
- Program addresses key issues assisting, resolving system problems associated with low temperature of PEM.
- This program did not quantify the benefits for raising the temperature, i.e. how much will a HTM simplify the reformate system?
- Program needs to be coordinated with system study.
- High emphasis on one aspect critical to EERE R&D plan, high temperature membranes. It alone will not solve the issues of high temperature MEAs.
- Appropriate catalysts, 3-phase interfaces, stable catalyst supports are all part of the issues that need to be solved.

Question 2: Approach to performing the research and development

This project was rated 2.50 on its approach.

- Projects within program address several distinct, potentially viable approaches not limited to one "pet" project.
- Degree of participation in HTM working group not clear. Are these meetings required for funded projects? (If not, they should be). Some apparent overlap of R&D areas between groups (i.e. some liquids @ AZ state & LANL). Are these coordinated or independent projects?
- If (commercial) results are expected in short term, it may not or will not happen. Nonetheless, the team that Tom Z. has put together is, without a shadow of doubt, world-class and extremely capable.
- The project lined up the right technical teams from synthetic chemistry to FC engineering. Each team seems to be working on the right problems/barriers. The focus can and should be improved. I'm surprised that the project picked up some "tried and failed" routes, e.g. the SiO_2/ Nafion composite. Be careful not to spread too thin.

- Fuel cell membranes are high performance materials for specific applications. Macromolecular engineering is essential for achieving a break-through in FC membranes. Comprehensive structure-property correlations must be established for new materials under development.
- The cycle for carrying out well-designed material development can be viewed as a four-step process. First there is Controlled Synthesis (ATRP, RAFT, etc.) Second, there is Controlled Processing which includes characterization and simulation/modeling. Third, is Determination of Properties which are dependent on structure, size-seal of the structure, and heterogeneity (difference in structure from one phase to another). Fourth, is the Determination of Material Properties which depend on dynamics, time-scale and response rate. This is not a one-way process. If target materials properties can be defined clearly first (such as in the case of fuel cell membranes), then by working backward we can design the polymers needed to achieve the target materials, specific to this project.
- Working group is too big and not balanced to cover the 4 steps above. I counted 10 participating members, 9 of whom are focused on polymer synthesis (and not using controlled methods).
- Rationale should be quantified in measurable terms such as system efficiency, power density, cost, and BOP rather than the rogue "substantial system benefits"
- The description of this effort is also vague - maybe we can use hydrated polymers as one example. When will they decide whether they can or can't? What is the plan in either event?
- The description of the barriers is ill-defined. What is "adequate conductitivity and polymer stability?" We know enough to define these performance specs very clearly and there is no excuse not to do so.
- A standard protocol for MEA fabrication for all materials under development by the working group must be established to ensure apple-to-apple comparisons. For example, catalyzed ETEK electrodes could be placed against the membrane with hot-pressing in order to get a relative screening-type comparison of performance. MEA fabrication then only needs to be optimized for the best candidate.
- I question why 1.5 teams/10 are working on electrodes in this project. The electrode work is as presented more fundamental rather than geared towards MEA fabrication. Consequently, it really should be in its own, separate project.
- Finally, it seems to me that the majority of the materials under development by this group have been under development for many years. In fact, they have been reviewed by several authors. What is it now about this project that will lead to something other than incremental improvements? There is a fabulous world of new polymers that can be prepared using recently developed controlled polymerization processes. Why are none of these materials the target of this group? Why do I not see block copolymers, grafts, gradients, star polymers and interpenetrating networks? Why is film formation, morphology determination and control not a prime focus? I see no leaders in macromolecular synthesis and engineering in this working group.
- Speaker did not address market barriers. He did discuss targets but not well defined. What is "adequate" polymer stability? Is it five-thousand hours or fifty-thousand hours?
- Approach appears to be a mix of ideas but rational for choices of materials not given.
- For academic team, the program does not appear to be based on fundamentals.
- The approach to the high temperature membranes is very broad and covers many of the currently known potential approaches, but the efforts appear to be individual efforts. That is, it is not clear there is any synergy of the multiple groups working toward a common goal.
- The other components critical to developing high temperature MEAs, such as catalysts and their supports and interfaces with the membranes, processing methods for those membranes, all have to be addressed as well, and can be as difficult as the membrane material itself, but in different ways.
- Many of these issues appear to be included in the Roadmap:Research Topics slide, but it doesn’t appear projects have been identified or initiated in them yet.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 2.17 based on accomplishments.

- Difficult to assess progress since state of various projects at last review is not provided for perspective. Impression was that most of these are relatively new & data is current status. Would be helpful to see goals for each project (including interim goals) rather than just the approximate time-frame. Perhaps could select one or two projects with most progress to focus on the next presentation.
- Nice link between concept and concrete results which either reinforce or refute the concept.

- The project has now shown signs of great future progress to the credit of Tom's coordination and insights. The ideas and concepts are well founded. I hope the execution by various collaborators will realize the potentials efficiently in the next year.
- It's hard to measure research progress against performance indicators when no clear goals are stated. No timelines or milestones were presented, only a generic roadmap in-progress with approximate length of time to complete a program. The one-slide-per-subgroup progress did little to convey a clear sense of what's going on and whether or not the group is on target to achieve what goals.
- Presentation did not quantify targets.
- Performance of membranes relative to those targets was not presented.
- This program needs a focus.
- This poor rating is not to be taken as a criticism, rather just as an assessment of the position of the project along the learning curve it must be on to be successful. Some of the potential paths have been identified, and work is just beginning on them.

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

This project was rated 3.00 for technology transfer and collaboration.

- Active participation by industry in bi-annual meetings.
- It's not clear if CCM/MEA samples have been sent out to industry for evaluation yet or not. Maybe project has not advanced to this stage yet.
- This is the strength of the project. But I hope the divergence phase will soon pass and the top three projects are selected to focus on.
- Coordinated primarily with universities; industrial collaboration is tenuous; a clear path for commercialization of developmental material needs to be incorporated now, so that show stoppers such as cost and scale up can be identified early.
- Biannual meetings are inadequate, particularly for a working group of this size. Once a month meetings would be better.
- Good team formed. Program needs stronger directions or coordination between teams.
- Currently the effort appears to be a collection of primarily individual academic research projects taking different approaches. Perhaps it is still too early to expect significant interactions and certainly too early to expect technology transfer.
- To expect meaningful technology transfer to occur in even the 2008 time frame will require planning for that now, with a valid appreciation of what is involved in scaling up any of the membrane material approaches identified.
- For a company to make the capital investment required, the understanding of the materials should be advanced enough to understand the major benefits and risks.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- More specific decision points should be defined in particular. Multiple approaches are being pursued and it's not clear if down-selects will be done to focus more resources on the most promising efforts prior to the timeframe end.
- Not clear from a scientific standpoint what other 120^{o}C approaches/solutions exist to justify increased emphasis on this area.
- Focus on science and technology of your research. Let some one else worry about money!
- The project has lots of good work planned. The presentation did not mention who will down-select the various approaches and by what time and criteria.
- The future directions were so poorly defined that its impossible to grade it using this scale. Also, much of their future direction appears to be funding-related, which is inappropriate.
- Program should specify conductivity targets at temperature. Each membrane needs a target. Program needs to quantify targets relative to fuel cell power plant improvements that will be attained.

- Agree that more emphasis on the T<120^{o}C approaches may be good, and more achievable in the near term, and require fewer breakthroughs in the areas they are not addressing, like new catalysts, supports, etc.

Strengths and weaknesses

Strengths

- Excellent mix of projects with distinct approaches.
- Projects/PIs are strong in materials fundamentals.
- Strength of program is the strong team of researchers.
- Strengths include good solid research efforts by highly reputable laboratories and individuals.

Weaknesses

- Not clear that all projects participate in HTM working group, or that there is adequate distinction and/or coordination between similar projects at different labs.

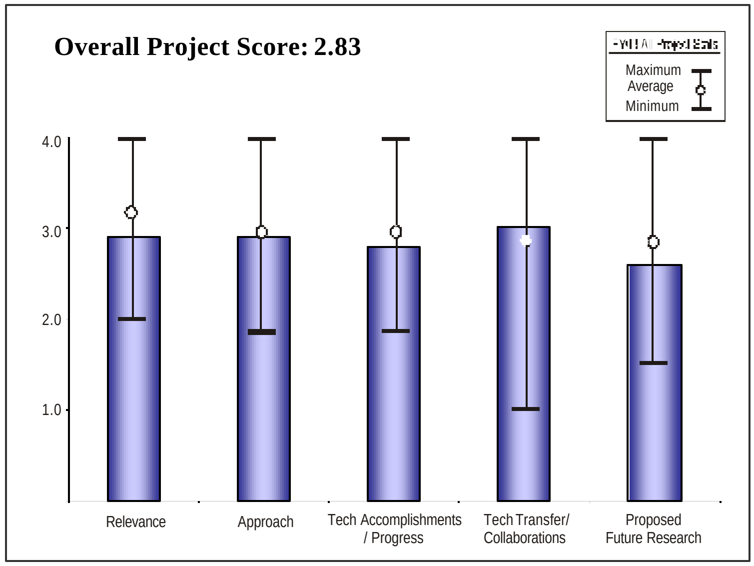
Specific recommendations and additions or deletions to the work scope

- It is this reviewer's opinion fundamental R&D work carried out must be continued. The economic benefits and impact of such work is hard to quantify. However, it is clear that this work creates a strong foundation for industry efforts which are usually on one application and market oriented.
- Take off at least three things from the list of potential materials. Cut the ionic liquid work, there is little reason to go back.
- Have team focus on goals for membrane. Correlate the goals with the system requirements. Have the National Lab or industry supply technical imputs on system requirements and work with researchers to establish benefits that will simplify the system.
- I would recommend that this effort could be enhanced and funded at a higher level, but only with significantly stronger program definition and management. This might be difficult to do in the current mode with traditionally very independent academic departments participating. It would involve a significant amount of fundamental work to be done on more aspects of the problem, sooner.
- I raise the question of whether some of these important activites that are not being covered yet, e.g. addressing the issue of stable high temperature catalysts and supports, catalyst/membrane interfaces (with the membranes from the academic labs), could be focused in some of the DOE National Labs, where a stronger management overseeing could be implemented.

Project #78: Electrodes for PEMFC Operation on H_2 & Reformate
Uribe, Francisco, LANL

Brief Summary of Project

Los Alamos National Laboratory is developing electrodes for polymer electrolyte membrane fuel cell operation on hydrogen/air and reformate/air. In this project, Los Alamos will optimize existing PEM fuel cell technology for use with H_2 and reformed hydrocarbons, improve overall PEM fuel cell operating efficiency, improve efficiency of the cathode while lowering the dependence on precious metal loading, and achieve 0.4 A/cm^2 at 0.8 V on H_2 with < 0.25 mg Pt/cm^2 and reformate with < 0.40 mg Pt/cm^2 on cathode. In addition, they will initiate a Cooperative Research & Development Agreement with Donaldson for studying the effect of ambient air impurities on fuel cell performance and establish the effect of 50 parts per billion hydrogen sulfide on anode performance.



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

This project earned a score of 2.90 for its relevance to DOE objectives.

- Excellent fundamental studies and performance correlations and recognized focus areas for improving performance.
- Addressing several barriers - anode, cathode, impurities and long term stability.
- Specific HFCIT targets not referenced in presentation, but assumed to be reduced catalyst loading and increased life and/or fundamental understanding of mechanisms that will lead to their accomplishment. Responses to 2002 comments are not consistent with these objectives. Disparate data is not beneficial. We need analysis and conclusions from data that can be used to advance technology, i.e. single cell test protocol does not " develop fundamental understanding of catalyst and electrode operation."
- More emphasis needed on long-term (2000 hrs.) durability effects of reformate on FC performance than just confirming partial remedial use of air bleed for short-term passivation effects.
- Focusing on both anode and cathode catalysts, impact of impurities, important new diagnostic tools, and numerous outside interactions.

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

This project was rated 2.90 on its approach.

- Excellent correlation of fundamental science and fuel cell performance. Strong recommendations on approaches to improve performance based on the results that are reflected in future work.
- Expanding on the type of impurities that might affect long-term stability was positive. From many other presentations seems that LANL is providing lots of support for the overall DOE program - ORNL, BNL, NRL collaborations.
- Segmented cell not used to study downstream, CO or water management issues under realistic conditions (T_a=105^{o}C); also data shows no effects contrary to all models with no explanation. Is data meaningful in real world conditions; explanation required. RCA with air bleed is no better than other approaches and can cause more problems than it solves. Constant flow rather than constant stoich data not realistic. Two of more interesting items; Pt/Cr mass ratio changes and CV effects on SO_2 not explored/explained. Do not conclude that

porosimetry data correlates with Nafion content. Do not agree that data shows cell fully recovered after NO_2 off and operated on clean air. Work should address “why” after “what” is determined.
- Somewhat a “smorgasbord” of investigations and the activities/results reported do not provide comprehensive coverage of the goals and approaches stated.
- It appears the LANL group is evaluating a number of different problems and serving as an evaluation house for a number of others. For the latter they are not developing the materials so can’t expect be expected to “overcome the barriers”.

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

This project was rated 2.80 based on accomplishments.

- Excellent advancement of new diagnostic techniques (XRF & porosimetry).
- Good progress in analyzing impact of poisons/contaminants on cell performance.
- Studies are thorough and dwell-defined.
- Segmented cell technology valuable tool for accelerating studies and increasing information.
- Good findings and conclusions on the importance of impurities an their effect on long term MEA performance.
- Results in general not useful as actionable conclusions or add to overall understanding. Stated goals not attained. Quantification of problems with contaminants not that useful. MEA manufacturers not using results. Work seems behind schedule. Need to relate results to polarization curves. Where is NaCl data referenced?
- Modest progress reported – not yet engendering high confidence in addressing reformate usage. Not quite sure what the “contaminant ions effect” of K, NH_4, & Cu findings really told me? Need to find solution or mitigating means to SO_2 problem – not good enough to state “exposure to SO_2 must be avoided”.
- The results are very useful and establish some baselines, particularly the impact of various impurities.

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

This project was rated 3.00 for technology transfer and collaboration.

- Demonstrated interaction with industry and other lab researchers and technology transfer specific highlight this year is MEA preparation training for NRL researchers.
- LANL provides validation testing and guidance for a large number of DOE funded projects.
- Not enough data to evaluate to conclude.
- While collaborations reported are encouraging, we need to understand more fully how these support the project goals.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.60 for proposed future work.

- Goal of re-activating catalysts under standard operating conditions would provide most impact of planned future work.
- May be more focused research in one area could be beneficial, of example this could be a study of the relative importance of each of the different factors that affect MEA long term performance -e.g. which ones could be more easily eliminated (impurities vs. catalyst agglomeration; vs. printing approval for MEAs).
- Work does not have measurable metrics specific enough to define completion, i.e. see Goals and Approach charts. Work just continues without tying up with a conclusion that unifies all pieces at the same time. Goals are filled with words like Study, Investigate, Initiate, etc.
- Rate of progress needs to increase and be more focused on quantifying long-term effects of reformate versus H_2. Is there a durability/reliability price to be paid for reformate use?
- The list of proposed future topics is too long for the amount of resources and the time for any new results to have a good impact. Suggest that some serious prioritization and group focusing be carried out.

Strengths and weaknesses

Strengths

- Clear project timeline/accomplishment goals excellent fundamental studies and performance evaluation.

Weaknesses

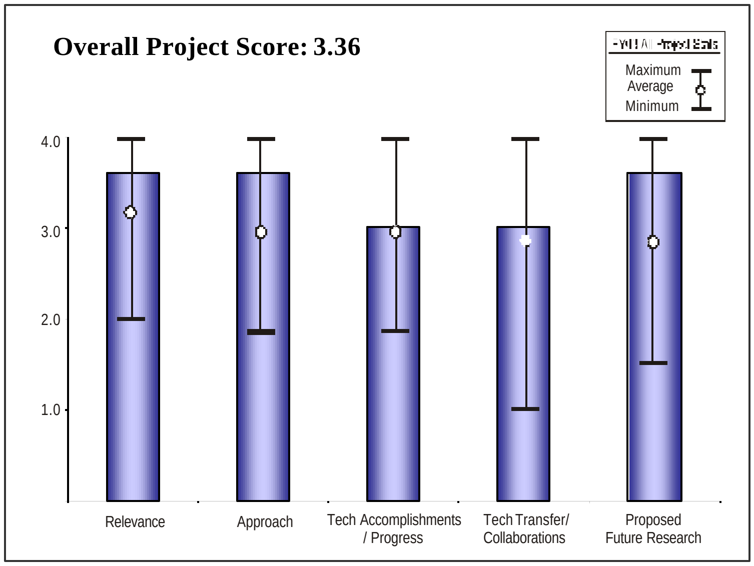
- It is clear that a lot of test data has been taken and man-hours applied, but I cannot see specific conclusions that have been reached in simple declarative sentences.
- Some of the statements are not justified by the data shown. We need to know what (mechanisms) things are happening at a more intimate level and why, i.e. degradation over time, H_2O movement thru GDL catalyst and membrane, etc.
- Overall impression is that the project appears more to be confirming the known problems of reformate operation rather than finding solutions.

Specific recommendations and additions or deletions to the work scope

- Develop more specific goals that cover all features simultaneously.
- Publish more comprehensive and in depth analysis of results in peer review journals like Journal of Electrochemistry, etc.
- Possibly needs a Go/No-go review (2004) - possibly linked to the Go/No-go decision on on-board quick start reformer work.

Project #79: New Electrocatalysts for FCs
Ross, Phil, LBNL

Brief Summary of Project



In this project, Lawrence Berkeley National Laboratory is developing new electrocatalysts for fuel cells. They will conduct research on the kinetics and mechanisms of electrode reactions in low temperature fuel cells and develop new electrocatalysts using a materials-by-design approach.

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

This project earned a score of 3.60 for its relevance to DOE objectives.

- These types of fundamental studies are crucial to developing advanced fuel cell materials.
- Main criticism is that impact of collaborations on this research is not clear. Appears to be a stand-alone project not adequately tied to the rest of the program.
- Excellent basic modeling of ORR catalysts which is used as guidance by other researchers in the field.
- To improve the objectives in efficiency, durability and cost, basic understanding of cathode catalyst reaction mechanisms and limitations is critical. DOE support of these activities is necessary. This work addresses these issues and offers a basis from which catalyst system modifications and alternatives can provide.
- Critical to the needs of the fuel cell program is a fundamental understanding of catalysts. This program offers that understanding.
- Focuses just on catalysts, and understanding basic mechanisms, so clearly some work will appear to be “off the track.” But this is necessary for this kind of research.

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

This project was rated 3.60 on its approach.

- More effort should be applied to synthesizing the proposed "grape" structures if the fundamental predictions are borne out. PI indicated that this is being pursued at a university, but didn't provide a sense of the current status and anticipated timeline for materials availability for testing.
- Excellent project workflow and consistent build up on previous findings and experience
- The tools for this research are available and applied to advance this work. By combining experimental work with a theoretical description to help describe the observed results, additional insight into cathode catalysts performance is provided to all. The information is presented in a concise and clear message.
- Program is logically and structurally focused with a fundamental approach that should lead to a greater undertanding of the catalyst. This understanding should yield benefits in the development of new catalysts.
- This is the only program where an attempt is being made to understand how to design catalysts from a fundamental basis.

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

This project was rated 3.00 based on accomplishments.

- Additional fundamental understanding of materials provides guidance to preparing relevant materials -by-design to meet DOE goals. Main concern is adequate materials synthesis program to complement the fundamental studies.
- Interesting findings and important observations, excellent modeling work.
- Progress has been very good in an area of research that is difficult. Some theoretical work has been tied with the experimental work which is quite appropriate. The modeling effort provides the opportunity to propose and examine new catalyst systems. However, the theoretical modeling effort seems to be limited and based on atomic, rather than metal cluster and surface phenomena.
- Great progress and understanding compared to what was reported last year.
- Work at this basic level is critical, but still far removed from practical systems that might be scaleable. Progress is slow because the questions being studied are very difficult.

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

This project was rated 3.00 for technology transfer and collaboration.

- Collaborations listed, but impacts of interactions on this project not clear.
- Extensive publications list.
- There could be additional effort applied to expanding cooperation with other laboratories to expedite the investigation into the proposed catalyst mechanism and generating more experimental data.
- Works well with industry groups.
- There appears to be a lot of collaborative research, mainly academic in nature.
- Technology transfer is less clear, but probably could be carried out fairly quickly is a good materials system is identified.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.60 for proposed future work.

- Synthetic aspects are critical to relevance of this project and to providing future decisions. Leveraging of BES funding for theoretical modeling will benefit project.
- Continue expanding on the materials choice for cathode catalysts.
- A good mix of experimental work comb ined with theoretical basis is planned. It would be very helpful to predict alternatives that may offer similar activity. Assistance in carrying out some of the experimental work through outside collaborators would be very helpful to program.
- Future research following logical approach that combines theory and experimental methods.
- Some concern that consideration for how the catalyst fabrication process affects the structure, and hence activity, is under appreciated. It is not shown as a specific part of the LBNL Materials by Design Approach slide, but is critical to manufacture of real materials.

Strengths and weaknesses

Strengths

- Strength is high quality of scientists participating in the research.
- Clearly the chance to obtain a fundamental understanding of ORR and H_2 reduction, relating materials structure to materials activity.

Weaknesses

- More activity and faster progress is needed.
- Relating materials process to materials structure (which then determines activity).

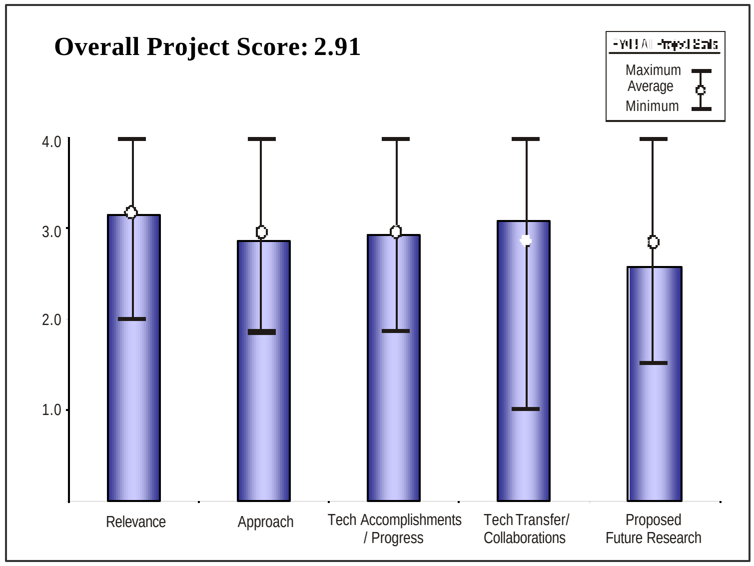
Specific recommendations and additions or deletions to the work scope

- Focus should be on synthetic methods to produce grape structures. This project should be scaled back or terminated if the synthetic effort is not ramped up.
- Collaboration with more universities and laboratories to expand both the experimental work and theoretical description and modeling should be encouraged, both by LBNL and DOE.
- Group should compare their emerging concept of a "grape" catalyst with previous theories of catalysts.
- Evidence outside the LBNL work suggests that ternary catalysts will be much improved over binarys. They should start to try to understand those systems. Also, with their research plan “to pursue new synthetic chemistry to synthesize nanoparticles with the “grape” structure,” care should be given to processes that can be cost effectively scaled-up in the end, i.e. don’t settle on just the first route that may work and stop, because again the process is so important for determining the catalyst structure they are trying to achieve.

Project #80: Low-Platinum Hydrous Metal Oxides for PEMFC Cathodes
Swider-Lyons, Karen, NRL

Brief Summary of Project

In this project, the Naval Research Laboratory is developing low-platinum catalysts for oxygen reduction at PEMFC cathodes. The Naval Research Laboratory will decrease the platinum content of the oxygen reduction reaction catalysts in fuel cell cathodes to meet the DOE 2010 precious-metal-loading goals of 0.2 g/kW, in addition the new MEAs must cost less than $10/kW and be stable for at least 5000 hours.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.14 for its relevance to DOE objectives.

- "Out of the box" thinking for a project that is high risk, but potentially has a very high payoff.
- Viability of oxide-based materials would address cost, precious metal loading and performance issues and could greatly expand the potential catalyst materials base for fuel cells.
- Very relevant in long-term aspect.
- Addressed FC cost. Key problem is electrode costs - must drive down materials cost.
- Lowering precious metal content is definitely a step in the right direction.
- Work is relevant if it yields results.
- Novel cathode catalyst materials chosen on a rational basis -- just about the highest priority-type project that could be imagined.
- Addresses only one issue, trying to reduce Pt levels. It is not clear there is any economic benefit of attempting to reduce levels to the 20x lower limits that the presenter claims.

Question 2: Approach to performing the research and development

This project was rated 2.86 on its approach.

- Focus is on fundamental materials development and understanding, with excellent use of relevant fuel cell performance indicator experiments to demonstrate viability.
- Good progress compared to the report last year. New systems studied, stability issues addressed.
- Outstanding program for investment dollars.
- Focused on key FC problem to reduce loading. Use chemical understanding to design and understand electrocatalysis. Tied to lab and industry.
- Good theoretical approach.
- Would like to see a more detailed strategy/plan with respect to where this program is going technically. The understanding of the needs is there.
- Choice of materials is good. Testing needs improvement, and interpretation of electrochemical test data, as shown in the presentation, has to improve very significantly or it will completely destroy the value of the project.
- The slide "RDE results for acid-stable Pt-FeOx" purports to demonstrate that the novel catalyst has 610% of the activity, per gram of Pt, of a 20% Pt/carbon reference catalyst. But the data are compared not in the kinetically-controlled potential region, but rather in the limiting-current region, where the currents are determined by mass transfer effects and normalization to the mass of Pt makes no sense. Comparing limiting currents (which should be independent of catalyst loading as long a minimum activity is achieved) and normalizing to the mass of Pt,

the lower-loaded catalyst will always look better. This is not a measurement of intrinsic catalyst activity at all. Another slide later in the presentation shows the same error, so this was not just a one-off slipup. Measurements below 0.6V don't mean much.

- It still is very unclear why this approach will allow reduction of Pt levels. The justification for using metal oxide as supports is to enhance proton conduction to active sites, but there is little evidence this is the rate limiting step in ORR. The initial claims that the PtFePOx showed 20x the activity of Pt is very suspect. The data presented this time, of 6x higher activity is also very confusingly presented and suspect.

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

This project was rated 2.93 based on accomplishments.

- Significant progress since last year's review. Transfer of LANL MEA preparation procedures and implementation of GM boiling test are key improvements in evaluating the viability of the propsoed materials. Addressed DOE cost barrier concern and is pursuing durability and performance concerns.
- Expand on testing of the new materials in MEA configuration and on optimization of the MEA structure - this would reveal significant performance advantages.
- Developing good understanding of how system works. Lots of new, good ideas to pursue.
- Hard to quantify progress. What was the status last year between what it is now?
- The applied science and methodologies are not close. Did not see significant developments from last year. What was communicated seemed tenuous at best.
- Good, provided that the errors in test interpretation discussed above are freely acknowledged and immediately corrected.
- Great care should be taken to baseline the results against the absolute activity of Pt in a state-of-the-art MEA. After this has been done, it should become possible to assess whether the new materials really represent an advance over Pt/C.
- This year's work apparently showed that finer grinding of the new catalysts solved the very bad performance of the materials in MEAs at LANL last year.
- Further refinement of the catalyst and electrode preparation is needed.
- Rigorous baselining against Pt showing true industrial performance levels must be shown.
- The amount of funding is very modest, but progress has been made in terms of getting outside expertise to help with the assessment. However, the rate of progress towards the ultimate goal is more questionable, due to again, the way in which the data presentation appears to be camouflaging the true comparative results.

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

This project was rated 3.07 for technology transfer and collaboration.

- Excellent. See above re: LANL & GM, and continued strong collaboration with the University of Pennsylvania.
- Project was focused in a better way after collaborations with established/experienced FC manufacturers/researchers' advice was sought and followed.
- Working to increase industrial collaborations. Establishing work relationship with labs.
- Good collaboration is beginning to take place.
- Group is not interfacing enough with others. Increase such discussions like the ONL-CU talks.
- Useful lines of communication have been opened. Full advantage needs to be taken of these opportunities to bring the work to its highest possible level.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.57 for proposed future work.

- Based on level of funding should focus on one or two key areas for 2003 - 2004. Particle size effect and approaches to small particle synthetic methods should be pursued. Optimal MEA development should probably await identification of one or two promising small particle catalysts since the LANL procedure cannot be followed exactly and optimization will likely require signficant time and effort.

- Very good plan for future work. Focus on MEA testing needed in addition to structural characterization.
- No off-ramps, but good directions in future research plans.
- Fairly comprehensive list of future plans. Should focus on one or two aspects.
- What is the ultimate commercialization plan? Work with Gore/DuPont/OMG, etc..? If this is the plan, such discussions should be initiated now.
- This group is good at screening systems - more is needed in fundamentals. Should report more in-depth technical results/data.
- Plans are good and can move forward once testing and interpretation of test results are brought up to acceptable levels.
- Most of these goals are necessary to study problems that the metal oxide supports brought with them. The use of metal oxide supports for enhanced proton conduction is suspect fundamentally, but when it requires additional methods to try and improve electronic transport and studies of carbon/metal oxide supports, it is only making the problem more complicated without any convincing win at the end of the game.

Strengths and weaknesses

Strengths
- New catalyst discovery through scientific understanding highly innovative.
- Good organization.
- Interesting materials.

Weaknesses
- Low level of effort given potential payoffs.
- Communication of more scientific oriented approaches/results.
- Interpretation of test results and, to a lesser but still serious extent, testing methodologies.

Specific recommendations and additions or deletions to the work scope

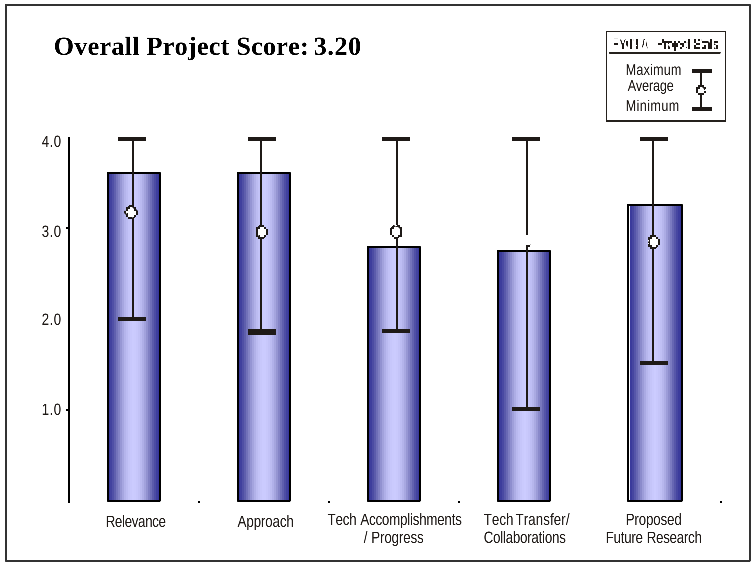
- Increase funding to speed-up analysis and testing of materials.
- Interface with electrochemical and solid state labs.
- Take a deeper dive into the fundamentals.
- Correct the data interpretation. Add explicit baselining (absolute performance numbers, not just relative curves) against activity of state-of-the-art Pt both in MEAs and in the form of the catalyst actually tested (e.g. RDE).
- This program continues to leave significant doubt as to the scientific credibility of the conclusions from the data presented.
- The justification for continued work is the evidence for enhanced activity of the metal oxide supported Pt materials. But the Tafel plots and polarization curves are not presented in the conventional manner that shows this, and critical facts that could be used to substantiate the claims are specifically omitted, such as the exact Pt loadings used in the Pt-MO_x case versus the 20 wt% Pt/VC case. The wt% values are irrelevant, it is the exact mg/cm^2 of Pt that is critical. In slide 5, of the $PtFePO_x$ case, the claim is that the latter has 6x times the activity of the 20 wt% -VC appears to be deduced from the limiting current density value of the Tafel plot presented. The rotating disc limiting currents should not be dependent on the catalyst activity, since they are transport limited currents. So unless the measurements are done wrong, it is difficult to understand how this data plot supports the conclusions claimed. In the Pt-SnO_x cathode performance slide, the same data plotting approach is taken, but relative loadings are given there.
- It is possible then to replot the data in a more conventional manner, of voltage versus A/cm^2. Doing so shows that the $PtSnO_x$ sample is about 400 mV below the 20 wt% Pt/C in the 0.2 to 0.3 A/cm^2 range. This is far more than the 70mV that would be expected for a one decade lower Pt loading (0.2 versus 0.02 mg/cm^2 as claimed), which strongly suggests the $PtSnO_x$ on carbon is performing much much worse in a PEMFC than 0.02 mg/cm^2 of pure Pt/VC would. Hence, again, the data appears to be presented in a fashion that confuses the issue rather than clarifying it. The same problem occurs with the RDE data from the Pt-$NbPO_x$ data. Limiting currents cannot in principle be used to compare catalyst kinetic activities.
- These latest results should be plotted in the conventionally accepted manner and taken to a recognized fuel cell electrochemist for conclusions.

- The amount of funding on this project is not much, but there is still the option to use that funding to evaluate some other new or radically different approach to solving another critical problem.
- Suggest that next year this work be presented in a poster so more discussion and clarification can take place.

Project #81: Low-Platinum Loading Electrocatalysts
Adzic, Radoslav, Brookhaven National Laboratory

Brief Summary of Project

Brookhaven National Laboratory is developing low platinum loading catalysts for fuel cells to reach the 2010 DOE target of 100 $\mu g/cm^2$ total noble metal loading and improved carbon monoxide tolerance compared with commercial platinum ruthenium (Pt-Ru) catalysts. They are also characterizing Pt/Ru electrocatalysts prepared by a new method involving a spontaneous deposition of Pt on Ru nanoparticles.



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

This project earned a score of 3.60 for its relevance to DOE objectives.

- Based on current SOA, Pt and Pt-alloy catalysts are most likely to meet DOE targets if loadings are decreased. Therefore, this project supports DOE's goals by combining fundamental studies and performance-based testing of these materials to optimize activity and minimize loading.
- There is still a question as to whether DOE's long-range goals can be met.
- Directly addressing the cost of components - novel approach with materials that have the potential to minimize use of platinum.
- This program would positively impact the goals because it would lead to lower cost and more reliable catalysts therefore leading to success of technology introduction.
- Project develops a fundamental understanding of carbon monoxide poisoning of fuel cell anodes which may be critical to resolving CO poisoning.
- Although focused only on basic catalysts, the work and approaches are unique and should lead to fundamental improvements in ORR catalysts.

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

This project was rated 3.60 on its approach.

- This project leverages previous approaches to making low Pt-loading catalysts (spontaneous deposition), but would benefit from interaction with researchers outside of BNL.
- Relationship to/distinction from catalyst work by others (i.e. Wieckowski) is not clear. It is also not clear how the structure/phase behavior is being exploited to design and demonstrate practical catalysts.
- Work on cathode materials is of higher importance and needs to be expanded further.
- Novel and excellent approach shown building on past work.
- The project focuses on critical issues - especially durability and life of the electrodes.
- Continued evaluation of platinum on ruthenium is not beneficial. The instability of ruthenium at the anode during the "off" state of the fuel cell invalidates this material as a catalyst.
- Their ability to study catalyst activities and surface structure in-situ is tremendously important and potentially will lead to the fundamental understanding necessary for the improvements that need to be made.

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

This project was rated 2.80 based on accomplishments.

- The project demonstrated improved life for 1% Pt/ 10% Ru/ C catalyst at loadings that exceed DOE 2004 targets for anode.
- They reported on initial low loadings last year, so it is not clear how much of improvement is due to processing vs. fundamental materials improvements.
- Excellent idea on alloying Au as core material on top of which Pt can be deposited as monolayer.
- Needs to be validated in MEA. Testing done previously in collaboration with LANL on the anode catalysts.
- This work is expanding the knowledge base. New concepts and/or materials will result.
- Very good work.
- Program needs some system guidance to focus on catalyst research to develop understandings for new catalysts.
- Good, fundamental results being implied, but they are not yet close to helping overcome the barriers to commercialization.

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

This project was rated 2.75 for technology transfer and collaboration.

- Benefits of listed collaborations to the program were not evident.
- This is a basic research project that is of significant importance and the coordination with other institutions is adequate.
- Collaboration is apparent and in place.
- Working with laboratories and industry which is the target.
- Not really clear to what extent the interactions with outside companies or laboratories is proceeding.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.25 for proposed future work.

- This is a simple extension of current work.
- Better correlation between the fundamental scientific studies of surfaces and the insight they provide for catalyst development is needed.
- Milestones for demonstrating progress are indicated, but targets for the performance at those milestones are not.
- It is important to keep focus on cathode electrocatalysts and expanding on compositional variables.
- This project exhibited a high degree of thought as to what comes next.
- The project continues to build on past accomplishments.
- Still need to focus more on cathode catalysts.
- The approach of applying Pt monolayer to other metal particles supported on carbon needs to be evaluated as well from a process/cost perspective. Is this the best way to try and obtain monolayers of Pt. If it can’t be manufactured cost effectively or reproducibly, then the advantage of low precious metal loadings will be compromised.

Strengths and weaknesses

Strengths
- Solid work shown.
- The knowledge is there and the 'know-how' is in place.
- The science is very strong and research demonstrates a good understanding of the topic area.
- Ability to fundamentally relate activity to structure.

Weaknesses
- Very difficult to follow presentation. Better organization is needed.

- Close contact needed with large scale catalyst manufacturer to help evaluate ultimate feasibility of catalyst manufacturability and effect of process on structure.

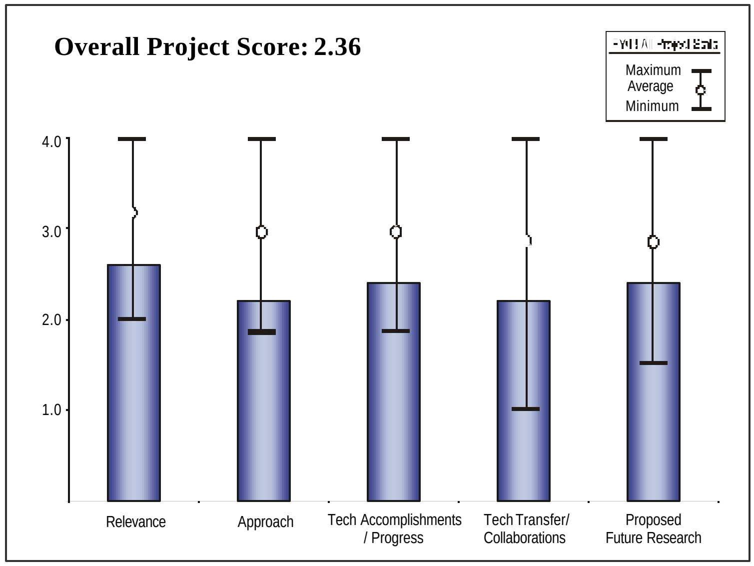
Specific recommendations and additions or deletions to the work scope

- Last year significant accomplishment was reported on the anode catalyst - it seems that no further significant improvement can be made. Similar extended study on the cathode catalysts may deliver significant advances.
- A little more funding could accelerate this work. It appears this program has low funding.
- More effort on cathode catalysts and less on anode, and also catalysts for high temperature MEA operation. Since high temperature membranes and MEAs are a critical barrier being addressed by other DOE projects, aligning this one with those goals would make sense.

Project #82: Microstructural Characterization of PEM Fuel Cells
Blom, Doug, ORNL

Brief Summary of Project

ORNL is using TEM characterization techniques to provide information necessary to optimize the distribution of precious metal catalyst for increased efficiency and reduced catalyst loading, leading to cost reductions in PEMFCs. ORNL is also characterizing/quantifying microstructural changes and relation to the performance loss in PEMFCs to understand issues relating to durability and lifetime.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.60 for its relevance to DOE objectives.

- Very useful analytical tool for studying the long term stability issues for PEM MEAs.
- This tool will be and is very valuable in understanding the special arrangement of components in the MEA. This will be very important to understand degradation mechanisms.
- An important analytical tool that should be developed to support the material research. But it is not a critical technology.
- While the first objective, the distribution of precious metal catalyst is relevant, this technique may have too high resolution to really relate to production parameters. However the second objective seems highly relevant to durability.
- Thorough analyses of PEMFC Beginning of Life (BOL) and End of Life (EOL) is essential for following component changes and correlating these to operating time.

Question 2: Approach to performing the research and development

This project was rated 2.20 on its approach.

- Good progress towards improving resolution including the acquisition of new equipment.
- I do not perceive enough focus in this activity.
- Leadership and direction to this work could be provided by a better relationship with LANL or other MEA experts.
- ORNL obviously has very powerful tools and capability, but I am not sure they are being challenged.
- Maybe the project is in the early stage, but it needs a clearer and more direct link to problem solving. This is best achieved by work on durability study and even focusing on one or two selected loss mechanisms among the many possible losses.
- More correlation with either single cell testing and/or a 2^{nd} characterization such as porosimetry or capillary flow porometry, or even high frequency resistance is needed.
- This program has been underway 3 years and no concrete progress has been shown.

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

This project was rated 2.40 based on accomplishments.

- More focused and targeted study of factors influencing loss of performance and more quantitative analysis of data needed.

- Technical accomplishment was the demonstration of making thinner sections. The data derived from this new technique did not really show anything that was not shown in previous work.
- A focused program with LANL in pursuit of specific mechanism etc… might be more valuable.
- Improved resolution is good but we have not seen any real impact yet.
- Compared to previous work, appears that more has been accomplished.
- The < 100 nm ultra microtomy thickness barrier has been broken with impressive results.
- Some progress but not enough to truly advance technology.

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

This project was rated 2.20 for technology transfer and collaboration.

- Good collaboration started with LANL - would be good to expand on it.
- Are there other collaborations with industry which can not be discussed? Have all industry partners developed their own methods with their own instruments? If so, is ORNL providing information on their sample preparation techniques?
- There are many small areas where the technique can help. I urge the team to link with a couple more FC teams in the front line to really understand their needs and initiate some new applications.
- While LANL makes a good partner, I would have liked to see more industry interaction.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.40 for proposed future work.

- Needs to have a more specific plan of what parameters should be studied.
- New instrument to do elemental analysis is critical.
- Proposed future work does not address durability. It is still not clear if this methodology can image the "soft" components such as Nafion ionomer or Teflon.
- No evidence of useful data has been shown. Data on how the cell microstructure changes are needed. Does porosity change? Does Pt sinter (re-crystallize)? Are there potential corrosion products in the MEA?

Strengths and weaknesses

Strengths
- Appears great for following catalyst changes or distribution.

Weaknesses
- There are too few industrial connections and a lack of fuel cell insights.
- Can the method image soft components? Is there any correlation between visual images and other physical properties/performance?

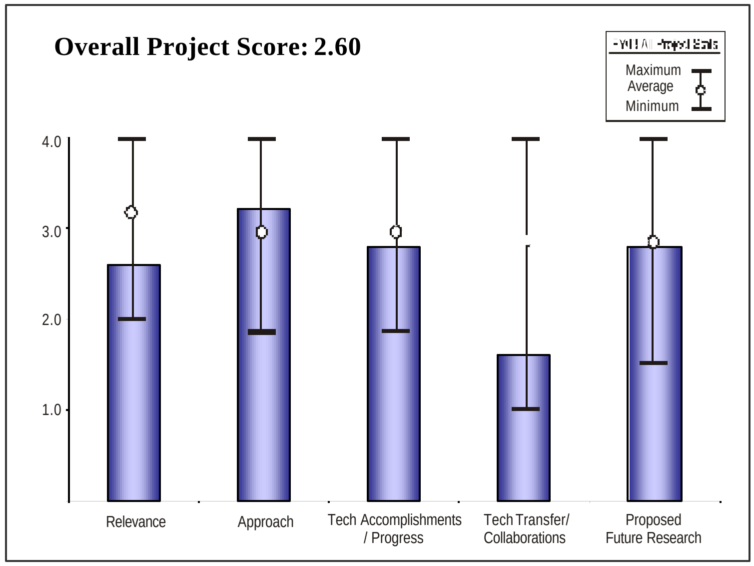
Specific recommendations and additions or deletions to the work scope

- Stronger teaming with, for example, LANL, combining LANL's existing analytical tools (XRF, EDS, etc…) with the powerful tools that ORNL has, is needed.
- Show the technique's relevance or run the risk of being labeled non-essential in a year or two.
- Correlation between image and performance and/or characterization from another method.

Project #83: Bipolar Plate-Supported SOFC "TuffCell"
Myers, Deborah, ANL

Brief Summary of Project

ANL will be working with a bipolar plate-supported solid oxide fuel cell (TuffCell) to achieve better characteristics for use in APUs. This will be accomplished by tape-casting oxide and metal slurries into films which are then laminated, sintering metallic bipolar plate, anode, and electrolyte together, and slurry-coating the cathode onto the electrolyte in situ.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.60 for its relevance to DOE objectives.

- Innovative approach to addressing some of the critical material issues of SOFCs.
- This project addresses durability, cost, and practical systems development.
- Provides reliable SOFC design at expense of weight. May trade off on program goals.
- Good approach as an alternate to anode-supported electrolyte.
- This is good work on a solid oxide fuel cell for APU applications, but it is not relevant to FreedomCAR objectives.
- Project should be moved to a more appropriate funding area within DOE.
- Program is too small to advance technology.
- Program could be coupled with SECA funding to reach funding level to advance.

Question 2: Approach to performing the research and development

This project was rated 3.20 on its approach.

- Mitigates brittle ceramic material issues and uses straightforward fabrication techniques to develop a prototype system.
- Very good project, given investment.
- Provides unique solid approach to make rugged SOFC, possibly at expense of some program goals… e.g. specific power.
- Good uniformity in expansion coefficients should allow real life operation and success in thermal cycling.
- TuffCell is making progress.
- Market barriers could be addressed by getting some more collaboration going with an industrial partner.
- Approach is reasonable but also done by others. Why not coordinate?
- Program is not addressing chromium migration, which is a critical failure mode.
- Program should include solving failure problem for SOFC.

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

This project was rated 2.80 based on accomplishments.

- Significant improvements in power density demonstrated.
- Order of magnitude improvement in current density at OCV. Cyclability and material improvements also demonstrated.

- Primarily addressed reliability and cost of manufacturing.
- Good approach but not necessarily optimized by performance (efficiency, power, density). Excellent progress this past year in getting performance up.
- Some progress but not enough to truly advance technology.
- Approach and understanding of electrochemical and mechanical interaction is evident.
- A good deal of progress has been shown in the past year.
- The status (2-3 hrs startup and 100 hr life) is a very long way from the DOE goals (15-30 min startup and 5000 hrs lifetime). Also, have the DOE goals for such an APU really been thought through?
- It wasn't clear why 5000 hrs was chosen, other than it seemed to align with the mobile application. Don't see 15-30 min startup as an acceptable goal. (By the way, this "fair" rating isn't a criticism of the work, but a recognition of the status compared to the goals.)

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

This project was rated 1.60 for technology transfer and collaboration.

- No indication of external collaboration on this project.
- Laminate process unique, but project could benefit from interaction with materials developers and collaborators with tape-cast expertise to improve characterization and processing without re-inventing the wheel.
- Not clear.
- Perhaps the program is too new for much collaboration to be in place.
- It is time for ANL to identify an industrial partner for this work and execute a collaborative agreement.
- Program should coordinate with SECA funded groups and/or with PNNL.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.80 for proposed future work.

- Two-cell stack demo important. Some concern about multi-cell stack performance since adjacent cells will be intimately bonded at this point.
- No off ramps. Progressing to stacks is very important future plan.
- This program should be continued and has a chance to solve some very significant problems with SOFC technology. Flat plate technology using this concept should be cost-effective and provide a means to solve the sealing issues. Program should operate 5-10 cell stacks.
- Claim is made that TuffCell has lower manufacturing and material costs, but no data were presented to substantiate this claim (other than an estimate of >$2000/kW$_e$).
- ANL should focus on getting a handle on cost estimates for TuffCell and where the opportunities are relative to the $400/kW$_e$ target.

Strengths and weaknesses

Strengths
- Solid program providing potentially highly reliable SOFC.
- Argonne Labs has high quality R&D capability which could be utilized for program.

Weaknesses
- May trade some performance for reliability.
- Not fully funded and need closer associations with industry.

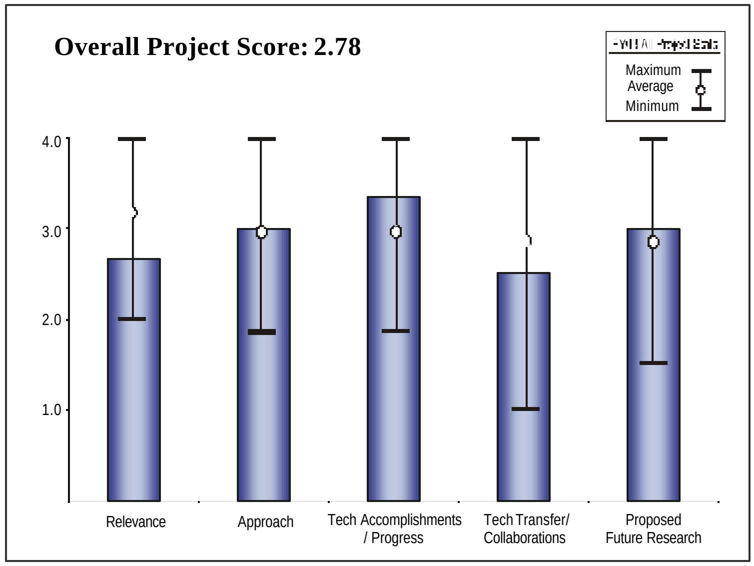
Specific recommendations and additions or deletions to the work scope

- May need to increase support to allow more rapid progression to stack. Stack testing may provide project end and transition.
- Transfer program to fossil or form alliances between EERE and fossil to move funding to a level that will lead to success.

Project #84: Coatings for Fuel Cell Air Compressors
Fenske, George, ANL

Brief Summary of Project

ANL is developing and evaluating low friction and wear-resistant coatings and/or materials for critical components of air compressor/expanders being developed for fuel cells. Furthermore, ANL will develop a material selection methodology applicable to all DOE compressor contractor's tribological conditions and requirements.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.50 for its relevance to DOE objectives.

- Although subject may be considered obscure, highly relevant and impacts products/progress.
- Good solid evaluation work.
- Concerns may have relevance to a section of the fuel cell market place - does not appear to be a topic of broad appeal.
- Clearly a strong bent towards mechanical analysis; but no clear understanding of polymers, PEEK, Teflon, nylon/delrin - no understanding that variations in MW and MWD can affect results.
- Program is not on critical path for fuel cell success. It could contribute to fuel cell success, but in a minor way.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- The use of the T-maps to anticipate frictional heating is a good extension of the work.
- Seems to make good sense although I do not have much experience in this area.
- Solid but pedestrian - tests are standard materials characterization tests.
- Approach does not distinguish differences among polymer properties within each of the families: PEEK without a clear source of the polymer gives minimal info.
- Addressing specifications that are important for identifying low friction coating.

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

This project was rated 3.00 based on accomplishments.

- It appears that they have achieved/extended the goals at least on an experimental basis. The next step is to create working units with the preferred material.
- Okay as a first round of work.
- Results solid but limited because presenters did not fully identify the polymers used, e.g. Teflon or Delrin or PEEK. (What MW? What product ID?) Without correlation to molecular properties, finding may be limited.
- Reported developing coatings but not obvious the coatings solve friction problems to the extent they would be incorporated in equipment.

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

This project was rated 2.67 for technology transfer and collaboration.

- While many companies are listed, it is time to "start building" devices for the next level of evaluation, granted some of these activities have already begun.
- Not sure.
- Needs connections to polymer experts. Needs connections to companies who actually use the materials in their devices and are dissatisfied with materials at hand.
- Several contacts. Appear to be expanding to fuel cell manufacturers.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.75 for proposed future work.

- Approach looks good except for corrosion measurements under either forced or "operating" environmental conditions.
- Plan to improve program is good but subject matter experts in polymer structure and synthesis are missing.
- Actual customer demands must be articulated and qualified.
- Do not identify specific pathways for low friction coating.
- "Work with" does not identify specific objectives.

Strengths and weaknesses

Strengths

- Great pragmatic approach.
- Solid understanding of mechanical wear due to tribology.
- Good understanding of testing procedures.

Weaknesses

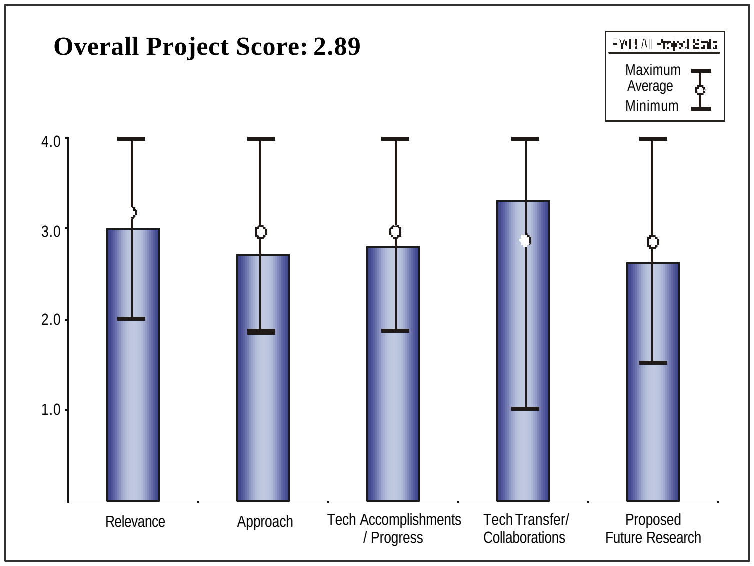
- Needs to continue working on devices to anticipate "real" material issues.
- Is cost a factor? No cost or relative cost was discussed.
- Must improve depth of polymer understanding so that practical suggestions for new materials may be effected.
- Weakness is no pathway identified that would lead to low friction coating.

Specific recommendations and additions or deletions to the work scope

- Corrosion testing. Continue working on real devices.
- Add polymer experts.
- Evaluate need for program.
- Focus effort on solving specific friction problems for a compressor/blower.
- Answer questions regarding value of compressor coating technology.

Project #85: Carbon Composite Bipolar Plate for PEM Fuel Cells
Besmann, Ted, ORNL

Brief Summary of Project



In this project, ORNL's objective is to develop a slurry-molded, carbon fiber material with a carbon chemical vapor infiltrated (CVI) sealed surface as a bipolar plate that would meet DOE cost and property goals. The preform is slurry-molded carbon fibers similar to paper or felt production, with fibers ~100µm plus filler particles, and features stamped/embossed into perform. They will also use CVI with carbon to seal and make hermetic high-density surfaces, while providing a continuous high – conductivity material.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Good work.
- Specific DOE HFCIT objectives not indicated but assumed to be high performance low cost stack plates
- This project is good to make high power density fuel cell stack and may provide better cold start-up capability if the thermal mass of the fuel cell stack can be reduced.

Question 2: Approach to performing the research and development

This project was rated 2.70 on its approach.

- In the 5 years since project initiated, data has not developed indicating comparative life (5000 hours) of CVI based and alternate materials in real full size stack with reformate or H_2. Stack data is needed!
- It is necessary to identify the defect modes of the surface modification for real world conditions, and set criteria for technical success.
- Targets regarding mechanical property, e.g. for vibration or physical shock, are unclear.
- Unclear why this method has been down-selected. Is this method competitive?
- Manufacturability (size precision and plate flatness) is needed to evaluate.

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

This project was rated 2.80 based on accomplishments.

- Problems to be addressed are: surface quality - contact resistance; testing (incl. long term) of bipolar plate under different conditions (e.g. cycling of power, frequent start-up/shut-down); and cost analysis via Porvair.
- Progress made in reducing thickness but not clear if it is with real bipolar plates or sample coupons.
- Work still very preliminary on details of manufacture.
- Conductivity is good but cost must be less than $0.50 per plate (is it?).

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

This project was rated 3.30 for technology transfer and collaboration.

- Good example of transfer to industry.
- Interaction with licensee is positive but need further interactions with stack builders and dissemination of stack test results to assist FC community.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.63 for proposed future work.

- Time to move on to other projects.
- Surface properties, stack performance, cost, and testing in stacks to be more intensely addressed.
- Proposed tasks are vague and emphasize properties only.
- Needs to work on stack performance and durability.
- Needs competitiveness analysis whether or not this technology could be competitive with other carbon composite technology from industry.

Strengths and weaknesses

Strengths

- Not only a nice poster, but nice work too! Sound approach and remarkable progress.
- Program has been transferred to Porvair.

Weaknesses

- No data presented comparing performance and life of full size CVI plates with SOA commercial and development technology under real operating conditions. Plate work is only meaningful if it is realized in a stack.
- Needs substantial additional data on effects of stack construction and operation.
- What about loadings and flatness of plates, etc?

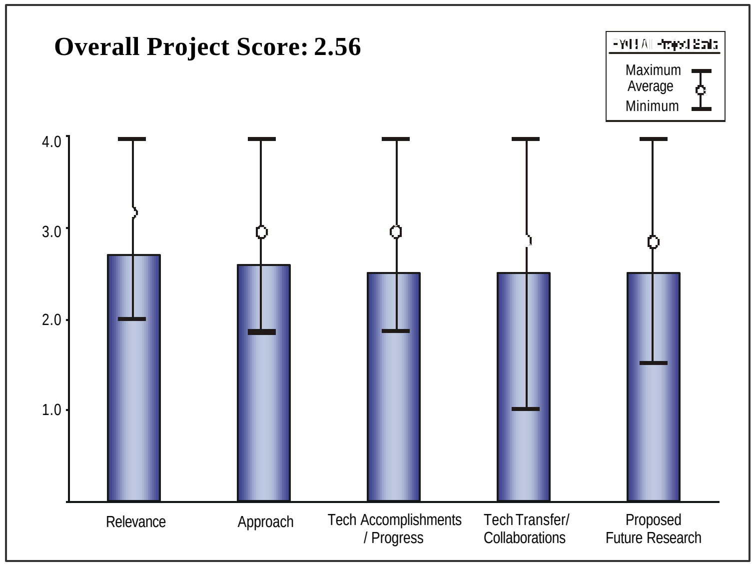
Specific recommendations and additions or deletions to the work scope

- Cancel ORNL and fund-scale up at PORVAIR.
- Add comparative back to back endurance and performance test of CVI plates with SOA commercial plates in full stack under real operating conditions.
- Obtain real full size stack operating information.
- Obtain data at higher temperature operating conditions; i.e. 120ºC to 180ºC.
- It is unclear whether or not this approach would be competitive.
- Presently, several carbon composite bipolar plates are proposed other than this project. Some technologies, such as Nisshinbo, SGL showed good performance. It is necessary to identify significant advantages of this project (including Porvair's project) against existing technologies. Otherwise, discontinue.
- This technical area is necessary for the automotive fuel cell. However, we couldn't see significant advantages in this project when we compare with other available carbon composite bipolar plate technologies.

Project #86: Cost-Effective Surface Modification for Metallic Bipolar Plates
Brady, Mike, ORNL

Brief Summary of Project

In this project, the cost effective surface modification of metallic bipolar plates will be explored with the aim of developing an alloy which will form an electrical-conductive and corrosion-resistant nitride surface layer during thermal (gas) nitriding. Control of gas-metal reactions by environmental and compositional manipulation will also be pursued.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.70 for its relevance to DOE objectives.

- Metal bipolar plates if thin (0.005") are essential for vehicular PEMFC.
- Thus far after 3 years no evidence that this process can be used on sheet metal has been shown.
- Supports low cost bipolar plate production - aligns with the needs for automotive fuel cells.
- Assuming this does turn out to be less expensive and effective.
- Addresses specific DOE HFCIT objectives important to FC commercialization.
- Plate specifications are correct.
- This project can lead to high power density fuel cell stacks and may provide better cold start-up capability if the thermal mass of the fuel cell stack can be reduced.

Question 2: Approach to performing the research and development

This project was rated 2.60 on its approach.

- Approach must be changed to use sheet metal and materials that cost less than 316 stainless steel.
- The researchers appear to have addressed or are currently focused on the key technical uncertainties: formability, contact resistance, corrosion resistance, fuel cell testing and low cost.
- It will take a lot to overcome the leachable ions. Coating must be perfect.
- Project team has pragmatic approach addressing obstacles as they appear step by step.
- Have combined material and stack testing in logical fashion.
- Basically good approach to focus on the nitride by surface modification.
- It is necessary to identify the defect modes of the surface modification for this real world conditions, and set criteria for technical success.
- Basic performance (electrical conductivity), durability for real-world usage (voltage range, fuel or oxygen starvations, thermal cycles, etc.), thickness to meet targets of power density and heat mass of stack, and cost.

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

This project was rated 2.50 based on accomplishments.

- Cost barriers have not been adequately addressed.
- Hopefully this process can be used on sheet metal and produce flat fuel cell plates.
- It appears that to reduce cost, different substrate alloys are necessary - therefore the team must repeat the characterization cycle.
- Some progress but issues remain the same.

- Very good progress shown although they have not yet reached the holy grail.
- Decrease in conductivity, it is questionable that the testing condition is well representing real-world usage profile.

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

This project was rated 2.50 for technology transfer and collaboration.

- No need to have technology transfer with this costly and cumbersome process using thick plates.
- The collaboration and LANL appears to have been fruitful in establishing the performance characteristics of the material. However I would like to know whether the automotive developers are interested in further developing this technology.
- Excellent capable team in place that covers all bases.
- I recommend making more close communication with OEMs to get real-world usage profile and requirements. FreedomCAR fuel cell tech team may be helpful.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Is this technology ready to be transferred to industry once more cost effective substrate alloys have been demonstrated?
- Targets are on track with objectives.
- Schedule should be tightened and include decision points.

Strengths and weaknesses

Strengths
- Initial results extremely promising for automotive use.
- Good mix of material and stack work.

Weaknesses
- None specified.

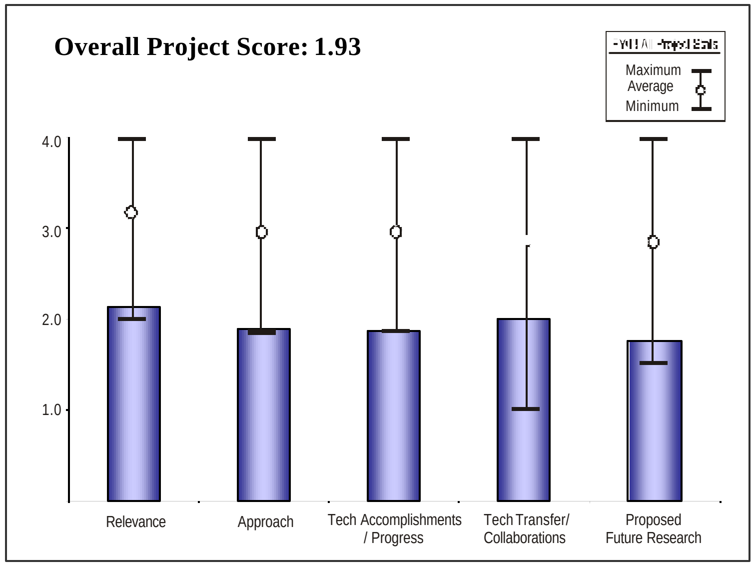
Specific recommendations and additions or deletions to the work scope

- Consider life longer than 5000 hours as a target so technology is appropriate to stationary applications as well as automotive.
- Cost target should be less than $0.50 per plate in mass production.
- Check the progress of other developers so that results are not duplicative; some companies are currently testing coated metal plates.
- Identify the defect modes of the surface modification for real world conditions, and set criteria for technical success.
  - Basic performance (electrical conductivity)
  - Increase durability for real-world use (voltage range, fuel or oxygen starvation, thermal cycles, cost, etc.)
  - Decrease thickness to meet targets of power density and thermal mass of stack.
- I recommend making closer communication with OEMs to get real-world usage profile and requirements.
- FreedomCAR fuel cell tech team may be helpful.
- This project is high priority.

Project #87: Carbon Foam for Fuel Cell Humidification
Ott, Ron, ORNL

Brief Summary of Project

ORNL is currently working to develop carbon/graphite foam for PEMFC humidification. ORNL will attempt to maximize the recovery of water from the outlet side of the FC by condensation, wick the water in order to evaporate it on the inlet side, and transfer adequate heat from exhaust side to inlet side.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.13 for its relevance to DOE objectives.

- A piece of technology looking for an application.
- There is no clear evidence that this is a unique material.
- Humidification for fuel cell applications is certainly important, and I don't think we want to be forced to add water.
- Specific DOE HFCIT objectives not indicated but assumed to be high performance heat exchangers/humidifiers.

Question 2: Approach to performing the research and development

This project was rated 1.88 on its approach.

- There seems to be no understanding of the basic issues.
- Progress is apparently non-existent.
- I'm a bit uncomfortable that this is a case of a new material looking for an application, rather than the other way around. How do the properties of this material compare with others? ORNL showed a cost model, but what targets have to be achieved to make this material acceptable for the humidification application? Presenters should always include targets and status, especially here.
- Time schedule is long and FC manufacturer involvement is too late to influence results.
- Go/No-go decision point is not timely and resources may be misspent.

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

This project was rated 1.88 based on accomplishments.

- During the past year very little progress was shown.
- ORNL is making progress using carbon foams for humidification. ORNL states they need saturated air at 80^{o}C; so how close are they? Not clear how the model is really going to help. Seems like experiments with several samples of different pore sizes would quickly establish the foam's ability to wick water.
- Analytical model is incomplete and not convincing of benefits.

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

This project was rated 2.00 for technology transfer and collaboration.

- Working with Porvair is not sufficient.
- Porvair is primarily interested in the ORNL separator plate.
- Good to see they are working with Porvair, which should bring some relevance to the work.
- Too much emphasis on manufacturability at this stage of development until technical feasibility is shown.

Question 5: Approach to and relevance of proposed future research

This project was rated 1.75 for proposed future work.

- I see no evidence that the porous carbon (reticulated) is the path to a cost effective solution.
- ORNL should think about speeding up the 2005 involvement with a fuel cell manufacturer to test the graphite foam recovery unit on a PEM fuel cell. If the technology has a chance of success, ORNL should have an easy time convincing a manufacturer to partner with them.
- I'm not sure we want to continue funding through 2004 before it's clear that the technology will succeed without an expression of interest on the fuel cell manufacturing side.
- Effort expended is excessive for promise of results shown to date.
- The Go/No-go decision is not indicated and resources may be misspent.

Strengths and weaknesses

Strengths
- Concept is interesting and deserves study.

Weaknesses
- FC system developers must be included early to set direction.
- No indication that they are involved in development of specification for heat exchanger or humidifier.

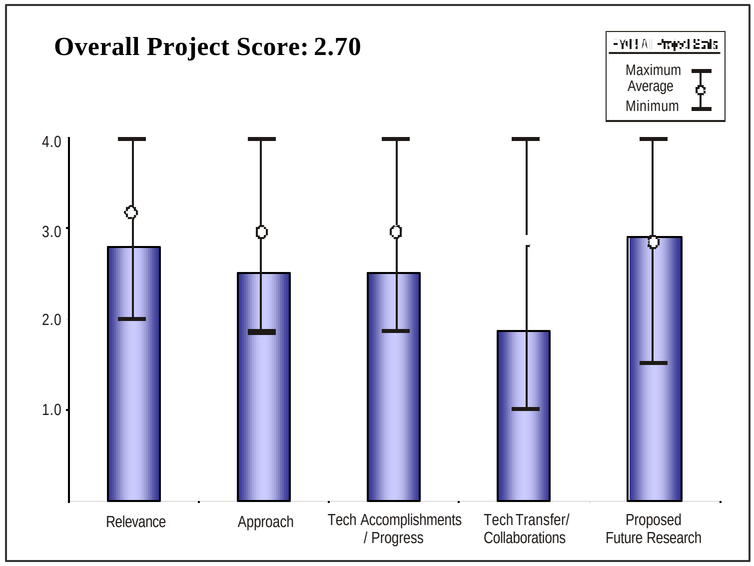
Specific recommendations and additions or deletions to the work scope

- Cancel program for lack of concrete progress.
- Very little prospects for cost reduction and no justification it is superior to reticulated metals.
- This work should be presented to the Systems Engineering & Analysis TT when they visit ANL (June time-frame, I believe).
- Development approach is not cost effective. Hold off expensive manufacturing development until technical feasibility is demonstrated.
- Involve FC system manufacturer earlier in process.
- Do more system analysis after specification finalized.

Project #89: Sulfur Removal from Reformate
Krause, Theodore, ANL

Brief Summary of Project

Argonne National Laboratory is working on sulfur removal from reformate. In this project they will be adapting proven technologies for on-board fuel processing to reduce the H_2S concentration to less than 1 ppm in reformate. Also, Argonne is developing new or improved existing technologies to meet the DOE targets for H_2S removal, such as, H_2S concentration of <10 ppb in reformate (FY 2010), a reactor size of <0.06 L/kW$_e$, and GHSV of 50,000 h^{-1}.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.80 for its relevance to DOE objectives.

- Sulfur removal is key.
- Removing S from the reformate is key to the performance of fuel processors. CuO is an inexpensive and efficient S scrubber.
- The original onboard reforming focus is not consistent with the new hydrogen-focused emphasis of the FreedomCAR program.
- Decentralized offboard reforming would be better served by upfront desulfurization rather than post-reformer, through a small S-polishing bed (or bed of copper-based WGS catalyst, which serves the S-polishing function). Upfront desulfurizer can bridge startup by storing desulfurized fuel. Post-reformer desulfurizers requiring operation at elevated temperatures cannot bridge startup. Central offboard H_2 production does not need this kind of technology.
- Gasoline reformation, on-board or off-board, could be an important, interim step in the conversion to a hydrogen economy.

Question 2: Approach to performing the research and development

This project was rated 2.50 on its approach.

- It is not clear what "breakthrough" discovery/invention has been made since 1998 (project start date). Desulfurization using ZnO or Cu/ZnO is a mature technology.
- Need additional tests on the pyrophoric nature of the material. Can the material be regenerated?
- The experimental approach is adequate; yet, the results should be more thoroughly interpreted using a gas-solid reaction model. This model would help to understand the effect of process variables and it would be a useful tool for comparing different adsorbents and for designing S removal beds.
- The project has never given adequate attention to upfront S removal, despite repeated strong recommendations to follow that path.
- Copper oxides will reduce to copper metal in the relevant feed gas starting at ~160^{o}C, reaching completion by 230^{o}C. While the copper-containing mixed metal oxides might be more resistant to reduction than CuO alone, the similar behavior of Cu/alumina (with 36.3% utilization) and the best material (Cu+B/alumina, with 57.9% utilization) suggest that the mechanism of operation is the same (i.e. surface adsorption of S on metallic copper). Capacity is improved vs. simple Cu/alumina probably through higher Cu dispersion. These materials are therefore not fundamentally different than commercial Cu/ZnO WGS catalysts used as S-polishers, such as C18-7 (or better, T2650) [examples from Süd-Chemie]. The new materials should have been directly compared to these commercial benchmark materials.

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

This project was rated 2.50 based on accomplishments.

- Good progress.
- The project has identified an adsorbent that significantly reduces the level of S in reformate while utilizing a large fraction of the Cu atoms available.
- The copper utilizations achieved for the experimental materials seem to be considerably higher than one would expect from commercial Cu/ZnO WGS catalysts. Does one get spillover of S to the other oxide, as is proposed for Cu/ZnO? So the materials from this project do represent a successful reoptimization of Cu just for the purpose of sulfur removal. However, similarly low H_2S-out levels can be obtained with the commercial Cu/ZnO catalysts, albeit probably with lower capacity. This project has therefore achieved over two years an incremental improvement in an area where major breakthroughs are needed.
- For an on-board system, it is essential that the sulfur adsorber has a long life-time. EPA is not going to be satisfied with a change-out interval of 30K miles, since there's no guarantee that the change-out is done. A change-out target of 60K miles should be added to the work.
- Is H_2S the only sulfur species that should be considered?
- Are organic sulfur compounds a problem?

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

This project was rated 1.88 for technology transfer and collaboration.

- Would be nice to compare new material with conventional Cu/ZnO material available commercially.
- Need collaboration with university and industry on different sulfur removal applications.
- No outside collaboration was identified.
- Only outside interaction discussed was that several outside parties are interested in getting samples to test.
- Inadequate attention has been given to reviewer comments from prior years.
- I would urge a much more aggressive collaboration effort with industry, perhaps one of the energy companies.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.90 for proposed future work.

- Long term testing is critical.
- The forward plan is well thought through and it addresses key issues.
- Plans to test shutdown/startup effects, including air exposure, on the Cu-based materials are appropriate.
- Plans should be extended to include the effect of the desulfurization material on cold start energies and times. Otherwise, just more futzing around with a chemistry that is likely to remain inadequate, rather than taking on the direct challenge of upfront desulfurization of fuels.
- Determination of the long-term stability of these adsorbents should be speeded up; if they don't have a long life-time, this adsorbent system won't be used.

Strengths and weaknesses

Strengths
- Lots of data and results.
- Highly relevant topic and well planned research.
- Seem to have obtained improved capacities.

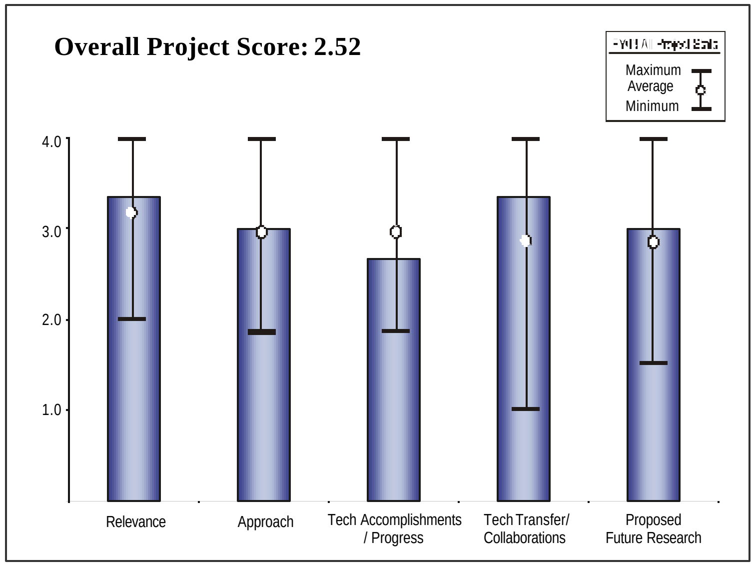
Weaknesses
- No out-of-the-box thinking. Maybe there is no easy solution to this problem and refiners must remove sulfur before it gets to the fuel cell vehicle.
- Work limited to H_2S, could be expanded to removing COS and mercaptans in natural gas.

- Stuck on a chemistry that is doomed to be inadequate (no way to bridge startup).

<u>Specific recommendations and additions or deletions to the work scope</u>

- Expand scope of work to investigate sulfur removal from natural gas streams.
- Publish in refereed journals.
- Establish collaborations with industrial partners.
- Switch to upfront sulfur removal from natural gas or abandon the project.
- There will be a Go/No-go decision on on-board reforming in the future. ANL should consider how to adapt this project to off-board reforming, if the decision is No-go.

Project #90: Assessment of FCs as Auxiliary Power Systems for Transportation Vehicles
Lasher, Steven, TIAX



Brief Summary of Project

TIAX, in this study, will determine the viability of using PEMFCs and electrode supported SOFCs as auxiliary power units (APUs) for on-road vehicles. This assessment will consider fuel flexibility, start-up time, power level, duty cycle, vehicle efficiency, weight, volume, O&M costs, and reliability and maintenance. This will help identify R&D needs and possible DOE roles.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.33 for its relevance to DOE objectives.

- Another paper study!
- Reduction if fuel consumed during idling is substantial and is important to minimize.
- A nice study; needed to be done.
- Good approach to introducing fuel cells to the market.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- The types of systems were decided at the outset based on "conventional" wisdom which may or may not be supported by the results of the Portable Power solicitation.
- Good summary of situation adding cost element might be helpful.
- Identifying barriers is not the scope of this project.

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

This project was rated 2.67 based on accomplishments.

- Another paper study!
- Good progress in identifying barriers.

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

This project was rated 3.33 for technology transfer and collaboration.

- Similar analysis has been done and is in progress under 21st Century Truck. Should consider participation in 21st Century Truck.
- Good partnering.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Future work will bring the project to its planned conclusion.

- Work beyond the current scope may not be needed at this time in light of the Portable Power solicitation.
- Next steps not clear.
- Future plans not specifically addressed.
- PI not present, presenter didn't appear to be fully aware of what's going on.

Strengths and weaknesses

Strengths
- Adequate team.
- Concise study.
- Easy to understand.

Weaknesses
- Consider collaboration with 21st Century Truck partners.

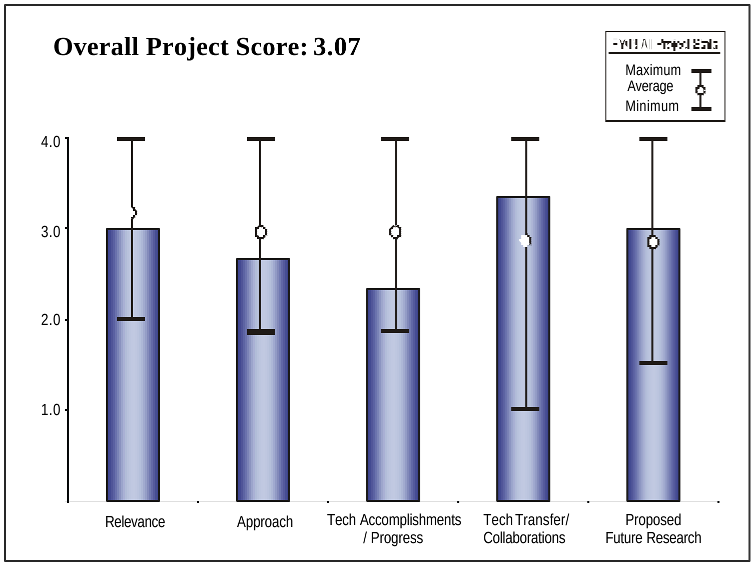
Specific recommendations and additions or deletions to the work scope

- Continue with project.
- Good study.
- Wrap it up.

Project #91: Fuel Cell Reformer Emissions
Unnasch, Stefan, TIAX

Brief Summary of Project

In this project, TIAX, LLC, will do an evaluation of fuel cell reformer emissions. TIAX will measure the emissions from autothermal fuel processors for a PEM fuel cell system under cold-start and normal operating conditions. In addition, TIAX will assess the feasibility of meeting emissions standards for automobiles and light-duty trucks through the use of a fuel cell vehicle with a gasoline reformer.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- When combined with other efforts, this project becomes critical.
- Many people assume zero emissions from a system which clearly is not the case. Emissions need to be measured.
- Important work which will show whether the claims of lower emissions for fuel cell vehicles with on-board reforming are true. Emissions measurements from current systems will let us know where they need to be improved.

Question 2: Approach to performing the research and development

This project was rated 2.67 on its approach.

- Evaluation of emissions is an important aspect of bringing the fuel cell technology to application. Would like to see some evaluation of particle emissions.
- The authors should incorporate PM (particulate matter) into the testing and address S emissions as well.
- Suggest that more careful examination of start-up occur; this is probably the majority of the emissions.
- This project could also potentially benefit by working with the ANL-led Fuel Processor fast-start project.
- They claim to have measured NO_x coming out of the fuel processor in the presence of large amounts of H_2. This is surprising, since catalysts in ATR & WGS should actually reduce NO_x. Determination of where NO_x are formed would be helpful (NO_x formed before AGB).
- Integration of the amount of NO_x formed during start-up would be helpful (may need a different analytical technique).
- Modeling studies need to know how and where NO_x is formed to be able to have predictive capability.

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

This project was rated 2.33 based on accomplishments.

- Why aren't particle emissions included? Need more actual measurements with the fuel cell in place.
- It appeared that the HCs were off scale during the light-off of the tail-gas combustor. This needs to be quantified.
- The NO_x levels shown seem high, usually measured at or below detectable levels in most work.
- The schedule calls for three systems to have been tested by now, but TIAX only showed results for one (May be due to circumstances outside their control).

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

This project was rated 3.33 for technology transfer and collaboration.

- Good coordination is necessary for project success and this appears to be happening.
- Working with a couple of fuel processor developers. It would be easier if this were conducted by an organization with their own fuel processor, but since TIAX does not have that program, they could potentially use the deliverables from other programs (ANL) to carry this out.
- Coordination between TIAX and fuel processor manufacturers Nuvera and McDermott should be better. TIAX test members did not appear to have a good knowledge of the configurations of the systems they were testing when asked about them. There should be feedback from TIAX to Nuvera/McDermott so they have a chance to make changes to reduce emissions.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Focus appears to be reducing emissions by reducing energy requirement needed for startup, which is where most emissions will come from - but emissions come mainly from unreformed hydrocarbons and NO_x, mainly from afterburner. Looking at different afterburner technology would be beneficial.

Strengths and weaknesses

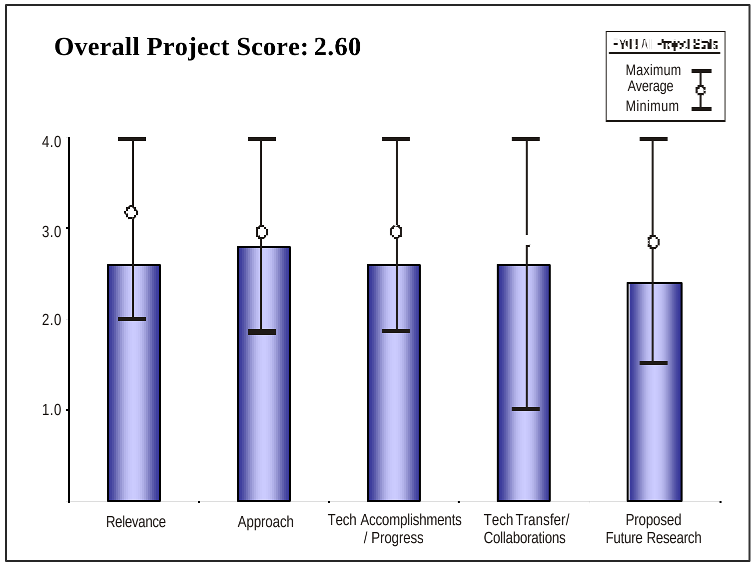
Strengths
- Emission characterization study conducted by "neutral" party for fair characterization.
- These measurements need to be made - very relevant work.

Weaknesses
- No particulate measurements.
- Limited by reliance on Nuvera or MTI.
- Measurements didn't show total NO_x during start-up (integrated value).

Specific recommendations and additions or deletions to the work scope

- Incorporate particulate matter measurements, SO_x measurements, and H_2S measurements.
- Concentrate delineation of start-up emissions during tail-gas combustor light-off.
- Try to understand $NO_x > 0$ (detectable limits). The number shown may not be real.

Project #92: Fuel Processing of Diesel Fuel for APUs
Berry, David, NETL



Brief Summary of Project

The Department of Energy's National Energy Technology Laboratory (NETL) is developing fuel processing of diesel fuel for auxiliary power units (APUs) and providing necessary tools and information to fuel cell/fuel process developers and system integrators for performance optimization and system control.

Question 1: Relevance to overall DOE objectives

This project earned a score of 2.60 for its relevance to DOE objectives.

- Diesel processing is important, but lower importance to H_2 initiative.
- It is not clear how the model will ultimately enhance reformer performance.
- Diesel or heavy hydrocarbon reforming is critical for many commercial and military applications. This is truly important work that most industrial players are likely to shy away from.
- Diesel is not a domestic fuel, hence is not part of the President's H_2 Fuel Initiative.
- This reviewer considers the development of APUs to be a sideshow that draws resources away from the socially-important task of developing fuel cell powertrains. Since APU requirements and technology are so different from those for propulsion, the argument that APUs represent a higher-cost-point opportunity to introduce fuel cells on the path to propulsion is specious. Nevertheless, the development of APUs is clearly stated as one of the high level goals of FreedomCAR, so this review will proceed with the assumption that the work is relevant to program goals.
- Diesel reforming needs to be done at very high efficiencies to apply. ATR will not yield high efficiencies. Total efficiency of diesel ATR and fuel cell may be less than diesel combustion engine.

Question 2: Approach to performing the research and development

This project was rated 2.80 on its approach.

- Models appear empirical in nature. How big of an advance is this? Will models provide realistic information for complex fuels?
- It seems that this project might be a duplication of other work (i.e. ANL work on diesel reforming).
- Please compare your model prediction with literature data. Also compare your expected results to the results of other researchers in the field.
- Good.
- This project attempts the laudable goal of developing a complete, sophisticated kinetic description of autothermal reforming of a complete diesel fuel.
- Seems to have grown out of last year's optimism that the relevant kinetic parameters could be obtained from catalyst suppliers for the asking.
- Presentation did not indicate an understanding that reformer design and operation are controlled by the allowable levels of output hydrocarbons besides methane. Essentially, one must totally convert nonmethane hydrocarbons (unless SOFC tolerance of higher hydrocarbons is demonstrated). Kinetic expressions based on data in the experimentally-accessible region of partial conversion may not be relevant getting to 99.96% conversion. This reviewer is afraid that ATR description will have to stay for a long time at the level of demonstrated maximum space velocity at which a given very high conversion can be achieved. The

investigators have taken the reasonable intermediate step of generating an empirical power-law rate expression, but that expression seems highly counterintuitive.
- Approach analyzes individual components in diesel with over 100 components and by analyzing 3 components per year, this will be a slow program. How will program address mixing of compounds?

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 2.60 based on accomplishments.

- Okay progress, but empirical nature of effort will limit value to complex fuel prediction.
- Scatter in data points to potential uncertainty in model predictability.
- Excellent fit between real data and model projection, indicating the level of sophistication of model.
- Good methodology.
- Need to study axial variation in temperature.
- The power law model generated looks strange in that it is negative first order in water, the species that provides most of the hydrogen in the products. The proferred explanation that this is due to site blocking by adsorbed water ignores the very weak bonding of water to Pt, Rh, and Ni surfaces -- the residence time of water on the surface at reforming temperatures would be very short. This data was obtained in a small reactor. Even in large reactors, it is difficult to obtain a temperature reading representative of the state of the reactor (because the front of an ATR is much hotter than the back). This reviewer wonders if water appears to the -1 power in the rate expression because addition of more water cooled a critical section of the reactor. It is hard to accurately decouple temperature and gas mole fraction effects in a highly nonisothermal system.
- The work on effects of fuel components is a good start but only if they progress quickly to binary mixtures. There are synergistic effects between different classes of fuels that end up damping out many of the distinctions seen between different discrete components.
- Project appears to be setting up procedures for analyses. Next year will be more important for this question.

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

This project was rated 2.60 for technology transfer and collaboration.

- There appears to be some collaboration with other DOE labs and transfers through information dissemination...but there is not direct work with industry.
- Switch to real dirty diesel ASAP!
- Need more industry collaboration.
- Seem to be in touch with the major players in the APU game. This reviewer cannot really judge the extent and quality of those interactions.
- Giving papers is not really tech transfer. Researchers should go out and meet with industry representatives.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 2.40 for proposed future work.

- It is not clear what the final goal is - there are no off-ramps - and what decides "final model?"
- Can this study be extended to more fundamental studies?
- Work in close collaboration with a catalyst development company.
- It will probably be necessary to back away from attempts to generate a detailed molecular-step kinetic model for a process as complicated as ATR of diesel.
- Not specific enough. Need to set targets and goals.
- What other fuel compounds will be evaluated?

<u>Strengths and weaknesses</u>

Strengths
- Understanding the reforming process is important - the team has good expertise.

- A well thought out research project with solid results.
- Starting to develop some sophistication on reforming despite the handicap of starting with a difficult fuel.

Weaknesses

- Models are primarily empirical in nature and so may not provide the best information for complex systems.
- Should NETL be asked to work with industrial organizations (which has stronger fuel processor hardware development) for diesel fuel processor fabrications and evaluation?
- It is not clear that the experimental power law expression has been passed through a reasonableness filter.
- Program needs future focus and needs to work closely with industry.
- Program should predict maximum efficiency or diesel fuel processing and assess its value.

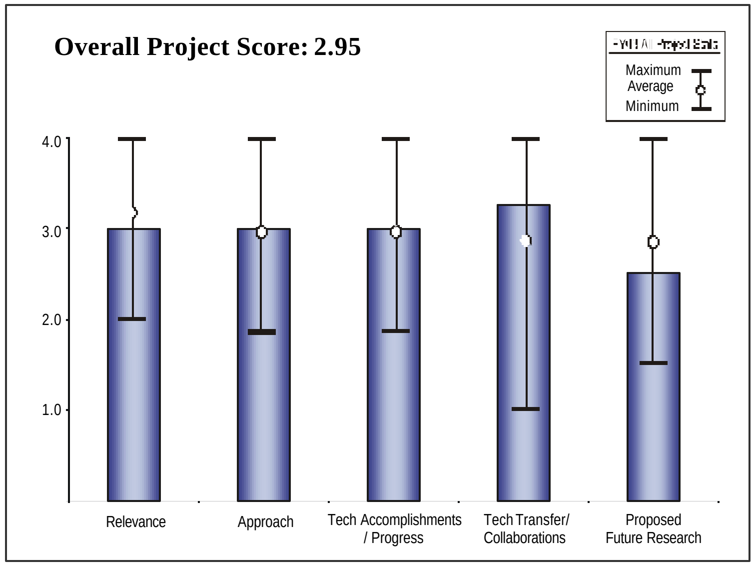
<u>Specific recommendations and additions or deletions to the work scope</u>

- Need to decide if data from simple fuels really provides predictive capability for complex fuels. Try some combinatorial mixtures and look at the results. If the scatter is bad, then need to reconsider the project.
- What about adding Universities?
- Include logistic fuels such as JP-8 and others.
- Good work.
- Program should team with diesel fuel processing company and be the analytical arm of the organization.

Project #94: Fiber-Optic Temperature Sensor for PEM Fuel Cells Monitoring
McIntyre, Timothy, ORNL

Brief Summary of Project

ORNL is developing a low-cost, fiber optic temperature-sensing probe that could provide intra-cell thermal information for thermal mapping and location of hot spots. Part of this project will involve the development of a low-cost architecture for integrating the fiber optic temperature sensor into the fuel cell control electronics – a plug-and-play modular design.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Accurate reliable instrumentation is essential for proper fuel cell diagnosis and operation.
- This project aligns closely with overall EERE programs R, D, & D plan.
- Accurate and in-situ temperature monitoring is critical for MEA and cell development. Conventional temperature sensors detect temperatures of the coolant and/or the bipolar plate. This technology could detect temperature of MEA itself and locations of hot spots.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Market barriers are system miniaturization, low BOP, and very low cost.
- The approach seems technically feasible and versatile if other sensing applications can be demonstrated.
- Endurance testing in a PEMFC environment is needed to determine if candidate probe materials are stable in the strong acidic environment of a PEMFC.
- This temperature sensor technology is expected for the development tool rather than products application. This tool should be critical for robust fuel cell stack technology.
- We are trying to get rid of a complex sensing system. For this temperature sensing system, we won't try to use in the products of the fuel cell system.
- For development tool, thermal mapping, accuracy, direct MEA temperature detection is necessary, however, low cost may not be necessary. High cost laser diode could be used if it increases the performance.

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

This project was rated 3.00 based on accomplishments.

- Progress on technical issues looks good.
- It would be more beneficial if sensing could provide concrete data regarding thermal gradients across the MEA.
- This is a new project with only a few months of research. It's too early to state the rate of progress.
- Fundamental data supports this sensing system for the development tool.

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

This project was rated 3.25 for technology transfer and collaboration.

- I believe there is a group at Penn State that is working hard on temperature sensing- collaboration there would be very helpful.
- Industrial collaboration that would provide a path for miniaturization (all on a chip?) as well as commercialization is needed.
- Closer collaboration with fuel cell developers will be needed.
- Good communications with FreedomCAR industry group.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Need to get input from end users more proactively, rather than waiting for their input. Need to decide if temperature monitoring is something absolutely critical in each product or if it is best suited for stack development and basic understanding of MEA performance.
- The decision points are not defined clearly in the project schedule.
- The project plan does not include validation by fuel cell developers.
- Good to work with the vehicular fuel cell developers for prototype design and technology demonstration.

Strengths and weaknesses

Strengths

- Robust, inexpensive, non-amperometric sensors for temperature, CO, H_2, and MeOH fuel concentration (e.g. DMFCs) are needed for a broad range of FC based power systems.
- Sound technical approach.
- This project team is experienced on optical temperature sensors but is not experienced in fuel cell testing.
- This is a unique technology to detect MEA temperature and for its thermal mapping. This technology should be critical for robust MEA and fuel cell stack development.

Weaknesses

- Needs more industry interaction, however project just started.
- Technology has immediate impact on R & D of stack to evaluate thermal and water management.
- More collaboration with fuel cell developers is recommended.

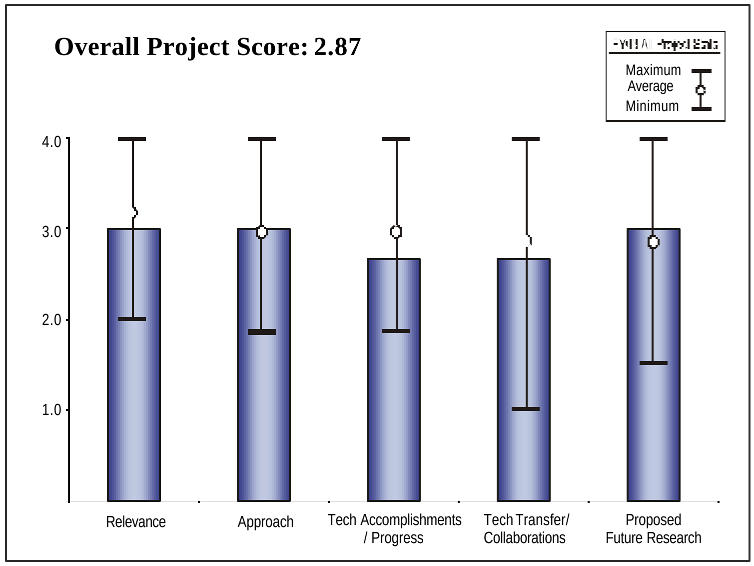
Specific recommendations and additions or deletions to the work scope

- The operating range should be extended up to 1000^{o}C for SOFC APU.
- The objectives of this project should be focused on the developmental tool. The idea of product system application would be unlikely.
- OEMs try to develop robust fuel cell systems with less sensitivity to operating conditions. OEMs are trying to get rid of complex sensing systems from the products.
- In this project, lower cost should be the focus.
- Low cost LED is not necessary. High cost device could be used if the required sensing performance as a development tool would be provided.
- This project is high priority.

Project #95: Selective Catalytic Oxidation of Hydrogen Sulfide
Armstrong, Tim, ORNL

Brief Summary of Project

In this project, ORNL is developing direct sulfur removal technology (gaseous sulfur to solid sulfur) with parts per billion separation efficiency.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Fits domestic fuels well- coal and natural gas.
- The removal of S is a critical barrier to hydrocarbon fuel processing.
- Sulfur control is critical for all DOE hydrogen programs.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Good summary of H_2S removal methods.
- The approach is innovative and it is based on solid science.
- Process is similar to commercial designs.
- Oxidation of S^{--} to S^0 is an excellent approach.
- Understanding the influences of CO and other contaminants is important.

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

This project was rated 2.67 based on accomplishments.

- New project.
- Need comparison between experimental data and equilibrium calculations.
- The project seems to be on track; however, no significant results have been achieved yet.
- Project is just getting underway and is not possible to rate "progress." Poster was well done and this mark is based on that.

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

This project was rated 2.67 for technology transfer and collaboration.

- Only one collaboration has been identified (NETL), others are under development (SGL, ChevronTexaco, CNP).
- Vendor relationships are in place.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- Continuous process should be developed early to identify possible barriers and to facilitate field tests.
- The direction of this activity is focused well.

<u>Strengths and weaknesses</u>

Strengths
- Highly relevant topic.
- Well planned research.

Weaknesses
- Lacks outside collaboration.

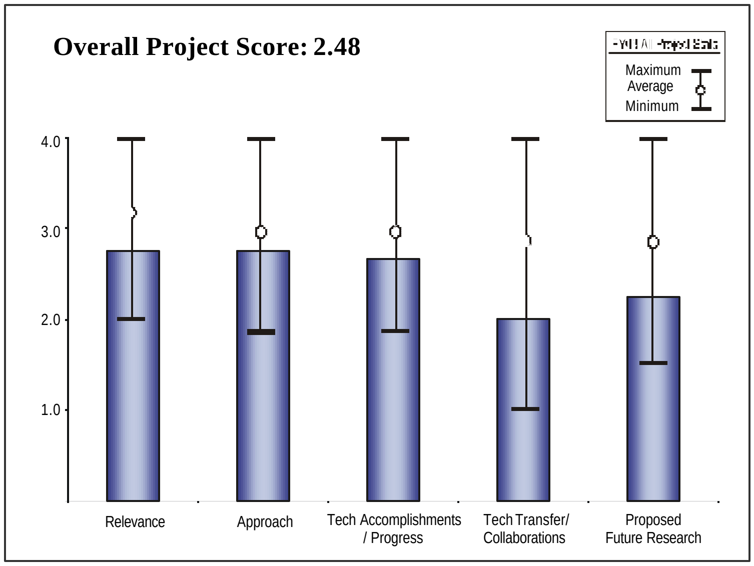
<u>Specific recommendations and additions or deletions to the work scope</u>

- Accelerate development of a continuous desulfurization process.
- Have potential customers test the process, and then publish in refereed journals.

Project #96: SOFC Auxiliary Power Units for Long-Haul Trucks: Modeling and Control
Khaleel, Mo, PNNL

Brief Summary of Project

PNNL is working to develop a dynamic model of APU systems including SOFC, diesel POX reformer, power conversion electronic and electrical hotel loads. They will design a system controller to minimize diesel fuel consumption while satisfying electrical load requirements, investigate different system configurations and their impact on overall efficiency, determine the response of the APU system to the vibration environment of a Class VIII truck, develop a model to determine stresses to a planar SOFC stack in an APU, and finally define vibrational limits for SOFC materials.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.75 for its relevance to DOE objectives.

- Valuable program to provide reliable SOFCs for transport. In particular, this project could help guide sealing research. Performance predictions are also important.
- Could be an interesting, early SOFC application.
- The emission aspects are probably more significant than energy usage attributes.
- While modeling of components and systems is important, the direct link between specific needs and identified problems is necessary to make modeling most effective.
- Very weak rationale for this work was provided.

Question 2: Approach to performing the research and development

This project was rated 2.75 on its approach.

- Program addressing lots of issues.
- Reliability important and can make important contribution, though it is not clear that performance modeling is unique compared to other work.
- Need to show connections to SECA.
- Combination of vibrational, thermal (partial in situ reforming in stack) and dynamic load stress should be addressed in future once the model is finalized within the present scope. Experimental validation would be "nice".
- Good review of user requirements.
- Tough problem of getting diesel reformer into the small volume budget.
- While structural intensity of SOFCs is important, adequate understanding of the issues involved is necessary to be of value to understanding system issues.
- Several vibration damping devices are available and should be included in evaluation.

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

This project was rated 2.67 based on accomplishments.

- Project started in December. Good progress considering start time.

- Hard to judge from poster.
- Somewhat difficult to evaluate. Requires successful components from vendors.
- Limited modeling has been achieved with respect to applying actual conditions of operation that would be relevant to this type of modeling.

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

This project was rated 2.00 for technology transfer and collaboration.

- Need to show connection with SECA.
- Have identified sources of valuable input in terms of technical aspects and application concerns, but no input.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.25 for proposed future work.

- Early in project. No off-ramps set, but should be able to decide tentative project length -- 2 years?
- Hard to judge from poster.
- Not much progress has been made in relating the need for this model and the model that has been developed.
- Should industry be more involved -- after all, it is their product that will have to survive the market application?
- If industry needs this model, its involvement is not adequate.

Strengths and weaknesses

Strengths
- Predictive capability for reliability very valuable…mechanical strain important to understand, especially for sealing.
- Project is addressing an important issue that is often neglected.

Weaknesses
- No real weakness, although performance modeling may not be unique.

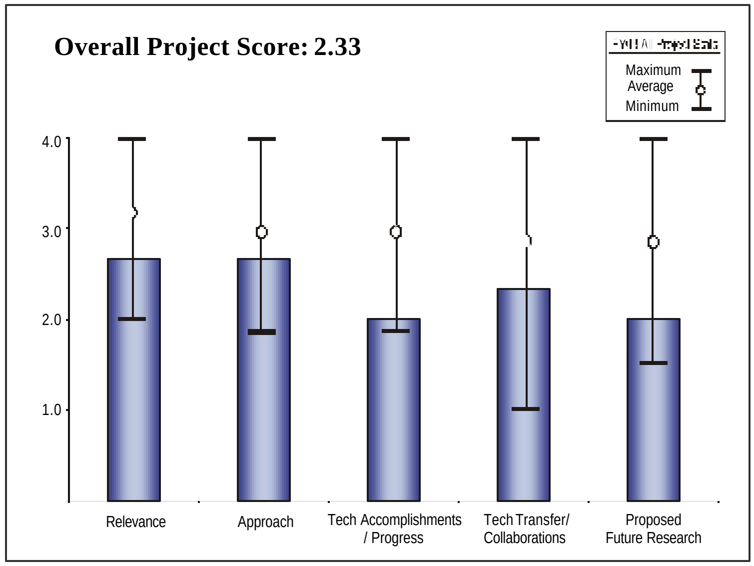
Specific recommendations and additions or deletions to the work scope

- Emphasize efforts on reliability and insure strong integration with SECA program.
- Experimental validation (which will be costly) is desirable.
- If industry has not expressed a need for this model, should this project continue?

Project #99: Diesel Reforming
Kopasz, John, ANL

Brief Summary of Project

ANL is using computation fluid dynamics modeling to study mixing of reactants and guide improvements in a liquid injector system. ANL is also testing model compounds of different types of hydrocarbon species found in diesel fuel under autothermal reforming conditions in an engineering-scale reactor.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.67 for its relevance to DOE objectives.

- Diesel is not part of the H_2 Fuel Initiative.
- This is a highly relevant project. Diesel fuel is readily available and inexpensive. Diesel fuel reforming is critical to the development of fuel cells for transportation.
- This project is supposed to support PEM or SOFC APUs and schemes to reduce diesel NO_x (reformate to the engine) or reformate as the selective catalytic reducing agent.
- APU work diverts resources that should be used for a direct attempt to apply fuel cells to vehicular propulsion. But APUs have been stated as a high-level deliverable by DOE management, so this review will proceed on the assumption that APU work is somewhat relevant.
- POx for pistons work has been carried out by various OEMs at various times and does not look promising, in part because of the derating of an engine using a gaseous, rather than liquid, fuel. Reformate for SCR may possibly be more promising, but it is getting pretty far afield from the FreedomCAR fuel cell and hydrogen mission.
- The project contributes to an educated Go/No-go decision on fuel processing through its design and characterization of liquid fuel injectors.

Question 2: Approach to performing the research and development

This project was rated 2.67 on its approach.

- Need additional leverage of ATR work in industry.
- The approach has not been presented properly. Three general areas are mentioned (CFD modeling, reaction engineering, reforming chemistry) without further indication of what is intended to do on each of those areas).
- The liquid injector work appears to be well thought out -- the proof of the pudding will be when the injector becomes a critical component of the ANL-led fast-start fuel processor.
- Mixing issues are critical to any reforming -- the laser doppler imaging could be critical to any ongoing attempts at processing of liquid fuels to hydrogen.
- The catalytic work seems to continue a couple of defects that have been pointed out in the past in reviews of ANL reforming projects: first, looking at different fuels under constant O/C ratios and steam/C ratios. Different fuels have different heating values. Fuel processor control strategies have to deal with this variation in fuel heat content. The most practical approach for a full-size ATR (that operates nearly adiabatically) is to control O/C to generate a constant reformer outlet temperature. This also gives control of gas composition -- partial oxidation goes to completion, then the steam reforming and WGS processes go virtually to equilibrium, giving a gas of defined composition if outlet temperature is maintained. For small-tube reactors the greater proportional heat losses cannot be accurately compensated for even by a surrounding furnace -- the better

control in this case is to calculate the O/C ratios needed to produce the same calculated equilibrium adiabatic outlet temperature for each of the fuels, then compare results at these different O/C ratios, using the furnace to get as close to the calculated adiabatic outlet temperature as possible. To do this accurately, it will probably be necessary to preheat the steam and air to a temperature higher than 200^{o}C, as one would also do in any practical integrated fuel processor. Temperature is by far the most important parameter in reforming of any type; unless the reactor is controlled to maintain at least the outlet temperature fixed, other variables will give false apparent results by coupling indirectly through temperature. Yes, conversion efficiency will vary a bit with fuel type. The more important point is whether variations in fuel composition will greatly affect the maximum space velocity that can be maintained without getting measurable non-methane hydrocarbon slippage at a constant reformer outlet temperature. By the time that the % H_2 is measurably affected by any process variable, so much hydrocarbon has slipped that the stack is dead. A second problem is that for fuel effects one must get beyond single components to at least binary mixtures. Synergistic effects between different classes of fuels can greatly damp out the apparent big differences seen in single-component work (particularly when outlet temperature is not maintained).
- Tech transfer seems to have been weakly stated. Major contribution may be injector for ANL-led fast-start fuel processor.

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

This project was rated 2.00 based on accomplishments.

- The conclusions listed do not present new insights into diesel reforming.

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

This project was rated 2.33 for technology transfer and collaboration.

- There seems to be no outside collaboration at this time.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.00 for proposed future work.

- Future plans are vaguely described.
- Plans for durability studies need to be fleshed out more for decent review --- valid durability work is hard, even before it is coupled to fuels effects.
- Other plans are perhaps reasonable but not particularly well-specified or compelling.

Strengths and weaknesses

Strengths
- The topic is highly relevant to both transportation and stationary applications.
- Coupled calculational and experimental approach to injector design.

Weaknesses
- The work plan needs to be revised. Specific tasks should be described with more detail.
- Questionable focus (three different possible applications, none of which are for propulsion fuel cells).
- Improper testing methodology for comparing different fuels, testing of individual components without checking for synergisms between components.

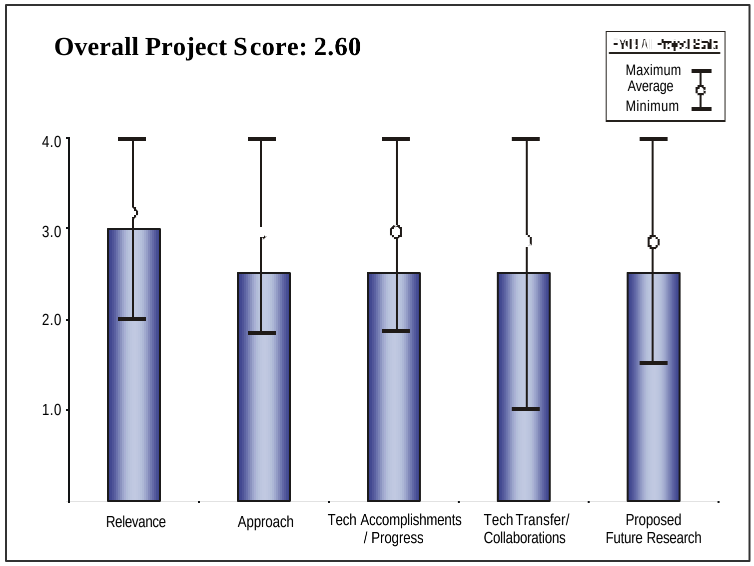
Specific recommendations and additions or deletions to the work scope

- Indicate type of reactor modeled.
- Show how the injector works.
- Indicate on what catalyst the reforming chemistry is being studied.
- Publish in refereed journals.

Project #100: Fast Start Reformer Components

Inbody, Michael; McMillan, April and Whyatt, Greg, LANL, ORNL, and PNNL

Brief Summary of Project



In this project, PNNL is building an assembly designed to fit between the outlet of the ATR and the entrance to the WGS reactor that will fit the FASTER (Feasibility of Acceptable Start Time Experimental Reactor) geometry. The assembly contains two main components – the first is a recuperator in which heat is extracted from the ATR reformate to preheat the air and stream feed to the ATR allowing for energy efficient operation. The second component is a microchannel mixer which will mix air with the reformate and allow oxidation of reformate to occur on the WGS catalyst, allowing the catalyst to be brought to temperature rapidly. ORNL is providing lightweight annular graphite foam heat exchangers to the FASTER Project while LANL is providing a multistage PrOx reactor for CO removal.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- On-board reforming is critical until 1) hydrogen storage is solved from a customer perspective, or 2) on-board reforming is realized.

Question 2: Approach to performing the research and development

This project was rated 2.50 on its approach.

- Project aims at quick startup using a proprietary Engelhard catalyst.
- Four labs are working on one device -- overburden on management -- need to focus on research.

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

This project was rated 2.50 based on accomplishments.

- Validation of quick start-up was demonstrated on the LANL reformer.
- Three separate devices shown: 1) Recip HEX, PNNL, Good, 2) Carbon HEX, ORNL, Good, 3) PROX, LANL, Poor -- no advances seen.

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

This project was rated 2.50 for technology transfer and collaboration.

- Interaction and leverage from Engelhard work on catalysts is a plus.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- This facility could be important in validating other promising approaches to quick start-up.

<u>Strengths and weaknesses</u>

Strengths
- Important validation of usefulness of Engelhard catalyst for quick reformer start-up.

Weaknesses
- Technology is interesting, how components work in the system is unclear.
- Project not well defined, no barriers or cost shown.

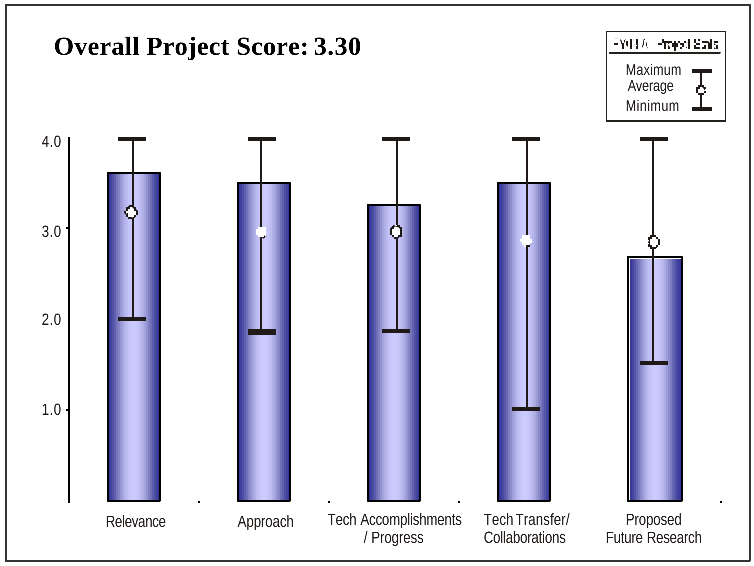
<u>Specific recommendations and additions or deletions to the work scope</u>

- I would like to see a consolidated effort at one lab. Too diffuse to be successful.

Project #101: Fuel Cell Systems Analysis
Ahluwalia, Rajesh, ANL

Brief Summary of Project

Argonne National Laboratory is developing fuel cell systems analysis tools. Argonne will identify key design parameters and operating efficiencies, as well as assess design-point, part-load, and dynamic performance of automotive fuel cell systems.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.60 for its relevance to DOE objectives.

- Important tools to support development of R&D goals and research directions. Establishes metrics for gauging progress of R&D plans. Provides support to other, e.g. cost analysis, areas.
- This program continues to provide information in the public domain that is critical to the targeting of other DOE fuel cell programs. Even if the OEMs were to become more open about fuel cell systems calculations, there probably would be a perceived need for such independent, publicly-funded analysis.
- Good work.
- This project is critical to set FreedomCAR fuel cell direction and targets of the fuel cell sub-system and components.
- Project is very relevant since FC system architectures are still evolving. Therefore, advanced design/analysis tools are essential.

Question 2: Approach to performing the research and development

This project was rated 3.50 on its approach.

- Analysis focuses on key target areas.
- Needs more evidence of integration with research teams and validation of models with data. Have insights from analysis been incorporated into developer plans?
- Good responsiveness to changing program needs.
- I like the project milestone focus on setting targets, including H_2 storage targets and CEMM targets.
- Dynamic modeling is highly demanded.
- Good approach to minimize utilizing "look-up tables".
- Hydrogen system should be focused.
- The approach is both thorough and comprehensive and the models continue to extend the range of system components supported.

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

This project was rated 3.25 based on accomplishments.

- Analyses of scenarios completed.
- Unlike development programs, modeling does not have benchmarks other than validation by data.
- CEM target proposals provided a reasonable starting point for new targets. Thermal analysis puts the CEM work into context.

- The CEM targets analysis was needed to direct CEM work.
- Good dynamic simulation.
- Radiator sizing study is very valuable for optimizing in-vehicle FC system architecture. Needs to extend thermal management to include hydrogen storage – particularly during rapid refueling and high power operation, e.g. high hydrogen flow rates in/out of metal hydride storage.

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

This project was rated 3.50 for technology transfer and collaboration.

- ANL modeling works with industry but this interaction is not evident in the presentation. Try to get input from system integrators rather than component developers like Nuvera.
- Work appears to be useful to a number of lab projects and to some of the industrial contractors. It also appears to be incorporated in a number of wider-ranging studies.
- Provides technical input to the NREL vehicle systems study.
- Seems to be quite a bit of industrial collaboration.
- ANL should keep in mind that a Go/No-go decision point on fuel reforming looms ahead in the program, and the amount of work in this area could substantially change.
- Good communications with FreedomCAR fuel cell and vehicle teams to incorporate vehicle level requirements for fuel cell system.
- Very appropriate relationships already established.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- Future research and development was not discussed.
- Didn't see much on future plans in talk (or at least in the printed presentation material).
- This is the one area that was weak in the presentation.
- Future work really was not discussed other than the analysis of Nuvera data by July 2003 and evaluation of FC systems for combined heat and power. Neither of these deliverables/milestones were discussed as part of the presentation.
- Need to plan verification with subsystem and components data by FreedomCAR contractors.
- Proposed future work has good diversity. In the long-term, they may need to model the effects of impurities and contaminants in fuel and coolant media on reliability and durability.

Strengths and weaknesses

Strengths
- ANL is to be commended for their collaborative work with several FreedomCAR tech teams to assist in defining battery requirements for start-up requirements for hybrid vehicles.
- Compared to the NREL fuel cell modeling work, this presentation has much more quantitative results. This should be the preferred style.
- Excellent modeling capabilities coupled with understanding of underlying technical drivers.
- Competent analysis, supporting DOE program on an ad hoc basis.
- This modeling is good for dynamic behaviors and those are critical for automotive application.
- Overall, a significant contribution to the achievement of the DOE and FreedomCAR goals.

Weaknesses
- It's not clear to me that ANL and NREL ever talk to each other on what they are doing on fuel cell modeling. They should be encouraged to have more interactions; it would strengthen both programs.
- Challenges of getting data from developers to calibrate and validate models.
- Not clear that there is a coherent vision for the future of their work.

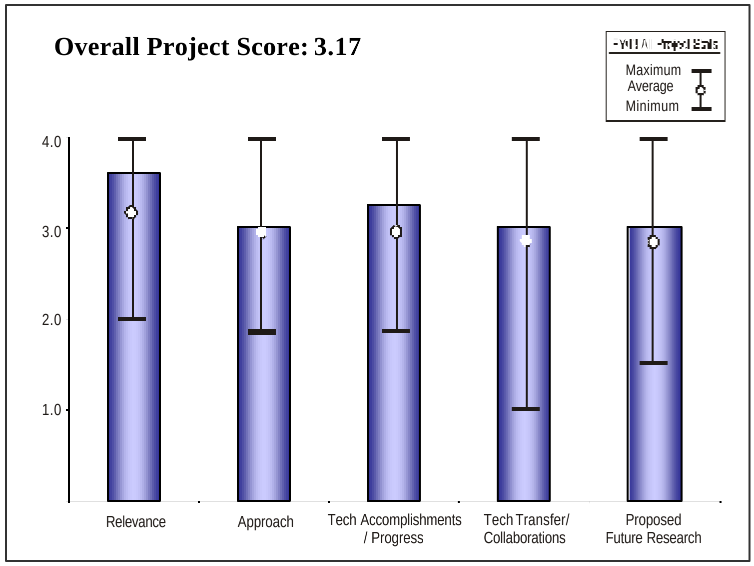
Specific recommendations and additions or deletions to the work scope

- It is assumed that the team will focus more on hydrogen storage options and benefits/issues of high temperature membranes on system efficiency and performance and BOP.
- Keep communication with FreedomCAR tech teams to align with the requirements of the real world vehicles.
- This project is expected to propose the subsystem and components level targets for the fuel cell system to meet the vehicle level requirement.
- This project is high priority.
- Seek OEM validation of the model's hydrogen storage targets and fuel economies – results appear optimistic if based on current state-of-the-art.

Project #102: Fuel Cell Vehicle Systems Analysis
Markel, Tony, NREL

Brief Summary of Project

National Renewable Energy Laboratory is developing fuel cell vehicle systems analysis capabilities, which will validate models and simulation programs to predict fuel economy and emissions. NREL will provide DOE and industry with modeling tools and early design insight in addition to quantifying benefits and impacts of DOE fuel cell technology development efforts at the vehicle level.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.60 for its relevance to DOE objectives.

- Provides tools to developers and DOE to assess overall system performance and viability of program targets/goals.
- In principle, a much needed systems model for projecting tradeoffs needed to realize DOE goals.
- Provides public-domain analyses, referenced to automotive driving cycles, that are critical to proper DOE program guidance.
- Some of the detailed design work on individual components with individual industrial customers may not be fully in the spirit of the role of the National Labs.
- Certainly it is important to understand the vehicle-level implications of the component R & D that FreedomCAR is focused on.
- This project provides the "bridge" between FreedomCAR subsystem targets and overall vehicle attributes.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Project addresses key system issues.
- A lot of words and activity in the project write-up but the approach and central accomplishments of what has been achieved are not clear from the detail provided.
- Overall approach is very good, with several small points where the relevance to real automotive conditions could be improved.
- The fuel processor startup calculations are still a bit too close to "cycle beating"; i.e. determination of the absolute minimum performance that would meet the requirements of a given driving cycle.
- A critical factor which does not seem to enter into these analyses is how much power would actually be demanded how soon after the startup by, say, the most severe 2% of the driving population. Something like this would fix the actual maximum power required during startup.
- The present calculations show that the actual total stored energy needed for a plausible startup is not that large and thus seems to require a relatively small battery in terms of energy (kWh). But the size of a battery for an electric-electric hybrid is set not by the energy consumed during start-up, but rather by the maximum power that could be called for during that time, which generally results in a significantly larger battery.
- I would like to see much greater involvement of the OEM members on the fuel cell tech team and the systems engineering and analysis tech team in this work. On the positive side, some of this is beginning to happen, and I hope next year to rate this category a "good".
- The approach is based on well-proven vehicle modeling algorithms and CAE tools, and has already shown its high value as a vehicle systems analysis tool.

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

This project was rated 3.25 based on accomplishments.

- What uniquely has been learned about how to improve fuel cell systems performance for auto applications is not clear.
- Tools have been developed but usefulness of the tools is questionable based on detail provided. Maybe this project needs to better communicate the message.
- Progress is being made on the broad analytical tools that provide guidance for public policy decisions. However, much of the progress that was presented was in what appeared to be small, tightly focussed design optimization projects with individual fuel cell developers - it is not clear to this reviewer that this detailed design work is really appropriate for a national lab.
- The component target repository and technical targets tool that calculates the system impacts of a particular component target should prove broadly useful to the entire developing fuel cell industry, as well as within the DOE program planning context.
- The water balance work gives proper attention to a major issue that often gets ignored.
- A lot of the presentation focused on the technical targets tool (T3) development.
- There are a lot of assumptions behind this T3 work, which were not covered. Are the assumptions correct? How sensitive are the results to the assumptions? This part of the work should be reviewed by the FreedomCAR Systems Engineering and Analysis tech team. Has it been? Which team is responsible for guiding this work, fuel cell or Systems Engineering? If the fuel cell tech team blesses the technical targets tool work, does DOE assume that the Systems Engineering and Analysis tech team is satisfied as well?
- The accomplishments presented showed excellent diversity of applications and very valuable progress made in improving the capability of the Advisor model.

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

This project was rated 3.00 for technology transfer and collaboration.

- They are working with several developer teams but did not get a good sense of the extent. Collaborative effort appears on narrowly defined issues, similar collaboration on broader system performance metrics.
- Interacting with Ballard and Nuvera is a good start but more needs to be done.
- NREL interactions with ANL have been useful and should be further strengthened. NREL design work has clearly tied in a number of industrial partners closely, though it is unclear to this reviewer that detailed design-specific work of this type is appropriate from a national laboratory (though at least some of this has been openly published). NREL has aggressively pursued programs tied to individual customers.
- A somewhat more fundamental and broadly-based set of projects would seem more appropriate.
- I'm not sure that VulcanWorks/Nuvera is the best choice for designing and optimizing fuel cell vehicle packaging solutions. Why is this not something done with the OEMs through the Systems Engineering & Analysis tech team?
- Numerous collaborative projects reported, but packaging studies need to address the effects of weight distribution (front-to-rear) on fuel economy, i.e. single axle regenerative braking utilization.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Future workplan is clear and details how this modeling tool could result in understanding fuel cell performance under simulated driving conditions.
- Startup energy calculations should be extended to the fuel cell itself and to real and hypothetical hydrogen storage systems.
- Hybrid work should pay adequate attention to real power requirements and real operating temperatures, should be very tightly coordinated with the quasi-experimental hybrid work being done at ANL (real battery, model fuel cell).

- "Robust design process transferred to industry to address fuel cell stack cost and durability technical barriers" needs further definition and to be kept within the somewhat fundamental realm appropriate to a national lab.
- I don't understand the plan to transfer the robust design process to industry to address fuel cell stack cost and durability technical barriers. Does NREL have expertise in this area? This project's focus doesn't seem to cover that area, and it isn't clear how the model could be used. Who are the industry collaborative partners? Do they have expertise in this area? Plan to validate fuel cell models with industry partners by 2004—who will this be done with, the OEMs or the fuel cell people like Ballard, Plug Power, and Nuvera??
- Explore trade-offs (weight, size and cost vs fuel efficiency & performance) for different hybrid powertrain architectures.
- Validate subsystem weights with OEMs and suppliers to confirm FCV GVW estimates that appear optimistic based on current state-of-the-art.

Strengths and weaknesses

Strengths
- Excellent modeling skills.
- Work plan for future is clear and describes areas where this technique may be useful.
- Global systems calculations of wide applicability.
- Strength in propagating models of fuel cell vehicles through established drive cycles.

Weaknesses
- Breadth and range of topics considered.
- Detailed MEA design/analysis tools and manufacturing analysis have to be closely coupled to developers to be of value.
- Uniqueness of approach or of insights gained not clear. If something useful has been realized, it has to be better communicated.
- Contract-house-like design work on small component issues should be done on a competitive basis, not by a national laboratory.
- Weakness lies in not always recognizing the system requirements that go beyond the mere drive cycles (e.g. high power soon after startup.
- Maintain priority of effort at the vehicle systems level rather than subsystem components, e.g. end plates.

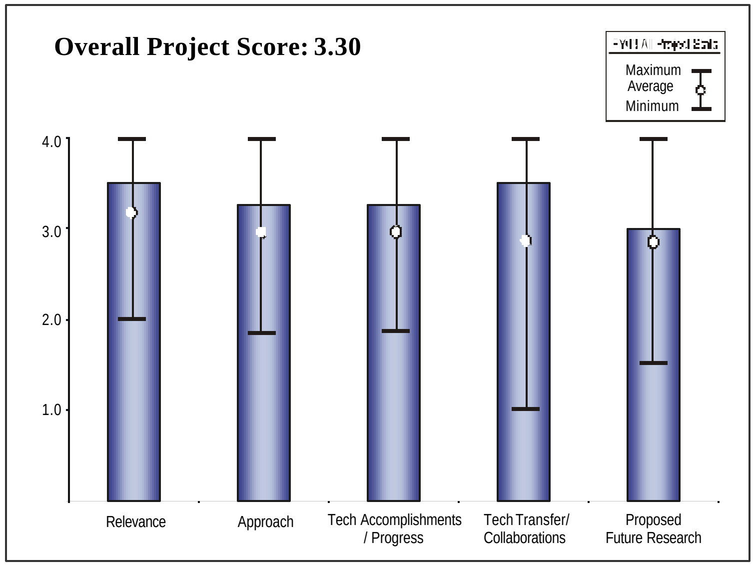
Specific recommendations and additions or deletions to the work scope

- If manufacturing and design modeling efforts not closely coupled to developer teams for validation, shift these resources to "higher" level issues.
- Work as a national lab, not a contract engineering house. In hybrid calculations, run them through at -20ºC as well as at room temperature -- batteries generally suffer a lot at low temperatures, even those that constitute a fair proportion of total driving time in populous sections of the US.
- This project should also be reviewed by the Systems Engineering and Analysis FreedomCAR technical team.
- NREL and ANL are to be commended for improving communications across the FreedomCAR tech teams and assisting in the cross-tech team project to scope out battery requirements for start-up in hybrids.
- In general, next year I would like to see more results presented at the expense of slides showing pictures of the input screens to Advisor.
- Extend model to include thermal management of high speed refueling (< 5 min.) of the different hydrogen storage systems, e.g. compressed 10 kpsi compressed gas, metal hydrides, and alanates.

Project #103: Cost Analyses of Fuel Cell Stacks/Systems
Carlson, Eric, TIAX

Brief Summary of Project

TIAX is developing an independent cost estimate for PEMFC systems including a sensitivity analysis of operating parameters, materials of construction, and manufacturing processes. TIAX will identify opportunities for system cost reduction through breakthroughs in component and manufacturing technology and will provide annual updates to the cost estimate for the duration of the project.



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

This project earned a score of 3.50 for its relevance to DOE objectives.

- Outstanding project.
- The cost estimates are critical to setting HFCIT objectives and priorities.
- Public cost estimates very necessary to guide R & D.
- Program goals clearly in line with DOE. Plan objectives as costs are bottom-line critical.
- Over the course of 5 years, emphasis has been shifted from on-board reforming to on-board hydrogen storage, hence changing emphasis. Activity-based costs being made public and shared - that is useful.

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

This project was rated 3.25 on its approach.

- Good project.
- Not clear at this point what a complete fuel cell system will look like and the cost estimates have to assume a particular design. Sensitivity analysis was a wise choice.
- Excellent methodical approach. Good consultation with others but should do more statistical analyses so that specific assumptions less influential.
- Systematic and methodical; but lacks critical analysis and push-back to challenge assumptions and inputs.
- Activity-based costing misses the synergistic, holistic views, which are necessary for overall system costs (trade-offs not thorough enough).

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.25 based on accomplishments.

- Outstanding.
- Successfully produced cost estimate and sensitivity analysis.
- Address barrier of limited publicly available cost information.
- Results are not earth-shattering but realistic (almost common sense).
- Sensitivity analyses were more statistically-based versus technology-based.
- Underlying cost estimates by 'suppliers' were not challenged.
- Insights gained were not spectacular - insights were obtained linearly not multivariable and interdependent.

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

This project was rated 3.50 for technology transfer and collaboration.

- Outstanding.
- Good collaboration with others in place.
- DOE needs to continue to integrate all analysis activities for consistent assumptions.
- Clearly, broad sweeps with many company inputs.
- Value in guiding DOE program is not major. Ultimate focus on Pt loading as major variable is expected (so is ohmic resistance and costs).

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- This project appears to be winding down at a time when a continuing and growing need exists for cost estimates in the program. Suggest consideration of project continuation indefinitely as long as need for cost estimates exist in program.
- End of this specific study seems clear. Many more similar cost studies needed, i.e. hydrogen storage, energy storage for fuel cell vehicles, & H_2 production/distribution.
- Program is winding down so future research is limited.
- No linkages to real programs.

Strengths and weaknesses

Strengths

- Highly relevant to HFCIT program.
- Very competent and skilled PI.
- Critical information to program planning. Overall outstanding work.
- Well organized and executed.
- Systematic, methodical and pedestrian.

Weaknesses

- Lack of information about uncertainties.
- Technology revolutions make results from models such as this obsolete (eds. note we hope!).
- A change in catalyst or catalyst utilization would greatly change the outcome of the model. Keep the limitations of this kind of model in mind.
- Are the models integrated, parametric, and linked? (i.e. Are analyses easy to automate or is this very manual?)
- Costs must incorporate holistic system views, and show synergistic, interdependency, multi-variable correlations.

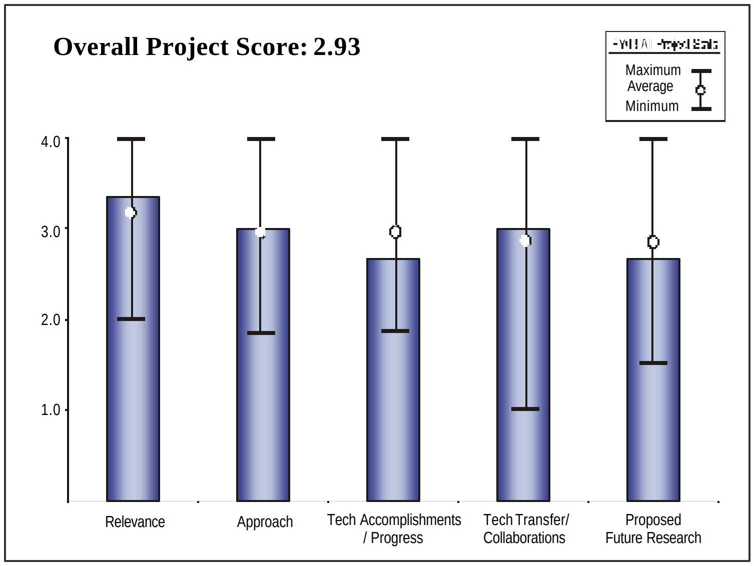
Specific recommendations and additions or deletions to the work scope

- I would like to see the addition of uncertainty and sensitivity analysis in the presentation of final results. For example, to what probability is an 0.2 mg cm^{-2} Pt loading optimal?
- In as much as the cost curves are relatively flat at higher loadings, could not DOE targets be given as a range of values rather than single numbers? More generally, can a probabilistic approach be developed?
- Consider automation if not already in place - leads to optimization.
- Consider applying methodology to other areas.
- Program should be terminated because of limited utility.

Project #104: Precious Metal Availability & Cost Analysis for PEMFC Commercialization
Carlson, Eric, TIAX

Brief Summary of Project

TIAX is conducting an assessment of the precious metal availability and a cost analysis for PEMFC commercialization. TIAX will assess current and projected demand for Pt group metals (PGM), estimate the relationships between supply capacity/reserves and long-term growth in demand, develop an econometric model to simulate the impact of a FC market on PGM supply and pricing, perform a sensitivity analysis, and develop a cost impact projection for the recycling of PGMs.



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

This project earned a score of 3.33 for its relevance to DOE objectives.

- With today's technologies, realization of President's hydrogen vision depends on the price and availability of platinum group metals.
- Excellent information.

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

This project was rated 3.00 on its approach.

- Good interactions with platinum companies.
- The forecast of 30 grams of platinum per vehicle seems extremely optimistic. If it doesn't go down that far, how does that affect your forecast of price changes?
- Approach clear and well organized.
- Visit to South Africa was important and may provide only new information on Pt resources.
- Presentation, however, was disappointing. Most of the information was available on the web from USGS and J-M Pt 2001 paper in Pt Metals Review.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 2.67 based on accomplishments.

- Need feedback from OEMs on how a 30% increase in the price of platinum would affect their ability to sell without raising prices to the point that consumers are no longer interested.
- Did not address geopolitical problem of exclusive reliance of a technology on a single natural resource. Would like to see more interviews in auto industry executives on this issue.

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

This project was rated 3.00 for technology transfer and collaboration.

- Good coordination with platinum companies.
- Coordination with car companies seems to be more difficult.

- Necessary collaborations seem to be in place.
- Not enough input from other industries on reliance of a technology on a single natural resource.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 2.67 for proposed future work.

- Future plans are clear and necessary to validate and improve confidence in study conclusions.

<u>Strengths and weaknesses</u>

Strengths
- Study reviews important supply and demand analysis that may have a direct impact on fuel cell introduction.

Weaknesses
- None specified.

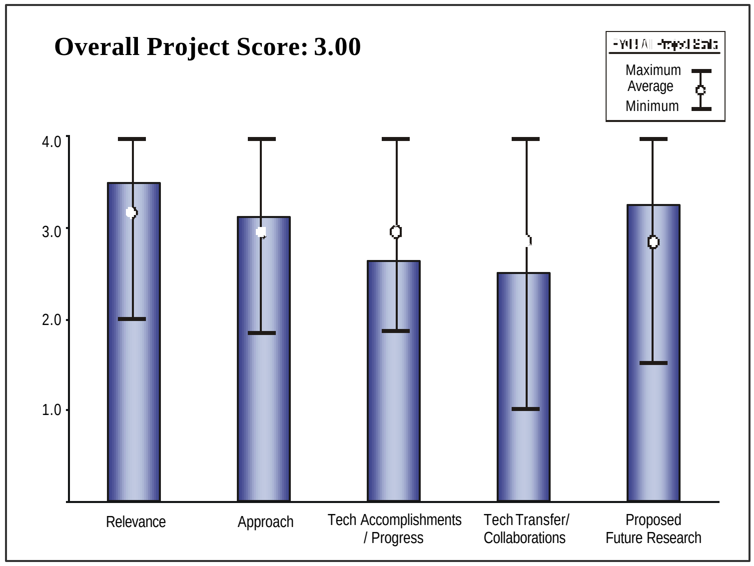
<u>Specific recommendations and additions or deletions to the work scope</u>

- None specified.

Project #105: DFMA Cost Estimate of FC/Reformer System at Low, Medium, & High Production Rates
James, Brian, DTI

Brief Summary of Project

Directed Technologies is preparing technology-based cost estimates of complete fuel cell-reformer systems at low/medium/high manufacturing rates to assess the current status and identify the most pressing cost barriers.



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

This project earned a score of 3.50 for its relevance to DOE objectives.

- Clear statement of technology barriers.
- Interesting and relevant analysis of the direct hydrogen/reformer system cost as a function of production volume. Insightful analysis of individual components cost.
- A very worthwhile program.
- Absolutely necessary to assess the most promising fuel processing technologies and compare to direct hydrogen approaches on basis of cost as well as technical barriers.
- Focus on on-board reforming and direct hydrogen is appropriate. Also focus on current year technology is more real but may be correlated to realistic expected improvements.
- Battery & Traction motor being left out is not appropriate because of interrelated improvements.

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

This project was rated 3.13 on its approach.

- Approach is reasonable to satisfy project objectives.
- The method of cost analysis somewhat unclear, thus difficult to evaluate. Source of the data for the analysis should be better identified.
- Project is well focused on major fuel cell system options.
- Solid, sound conclusions; but interdependence among variables are not accounted for.
- System size not mentioned (50 kW_e?) early in talk. 50 kW_e fuel cells must be coupled to batteries - therefore battery costs must be included.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 2.63 based on accomplishments.

- Good job of identifying existing percentage of cost spread between the costs of the reformer and stack.
- Sensitivity analysis impact on cost was good.
- There seems to be quite a lot of work to be done in this project.
- Good progress in identifying fuel cell system components that are most cost sensitive.
- Good assessment of applicability of certain fuel cell technology options such as microchannel technology.
- Some assumed operating parameters used in assessment may not be realistic e.g. 0.6 V cell potential, instead of 0.7 V.
- Fair, because focus is on stack and reformers and materials only.
- Simplistic accomplishments and conclusions (from costs views because of lack of coupling to technical reality).
- Focus on microchannel is good but technical/engineering analysis is weak.

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

This project was rated 2.50 for technology transfer and collaboration.

- Could use stronger fuel cell industry collaboration (e.g. Nuvera, UTC FC, PlugPower,etc.)
- Little outside interaction, other than reliance on patent resources.
- Closer collaborations with technology developers would be helpful to better understand operating parameters/details. Good beginning effort, however.
- Partnerships not clarified during talk or paper (minimal information).
- Costs must be correlated to system size and weight because these parameters affect fuel efficiency.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.25 for proposed future work.

- The future PSA work is very important and should be completed as soon as possible. The roadmap to lower cost should also be of high value to DOE.
- Proposed future work is well defined and should be of value to the fuel cell community.
- Needs a more realistic technical underpinning to cost models, especially because of the complexity of reformer systems.

Strengths and weaknesses

Strengths
- Clear approach and presentation of results.
- Valuable independent assessment of applicability/cost of fuel cell system options.
- Systematic and methodical.

Weaknesses
- The assumed temperature of 1100ºC reforming temperature is probably too high. ANL reactor for example never reached even 900ºC. Assumption of high temperature may have led to incorrect conclusions about applicability of metal reactors.
- Closer collaboration with technology developers essential in developing an accurate assessment of various options.
- Lack of linkages to technology reality and complexity, especially in reformer area.

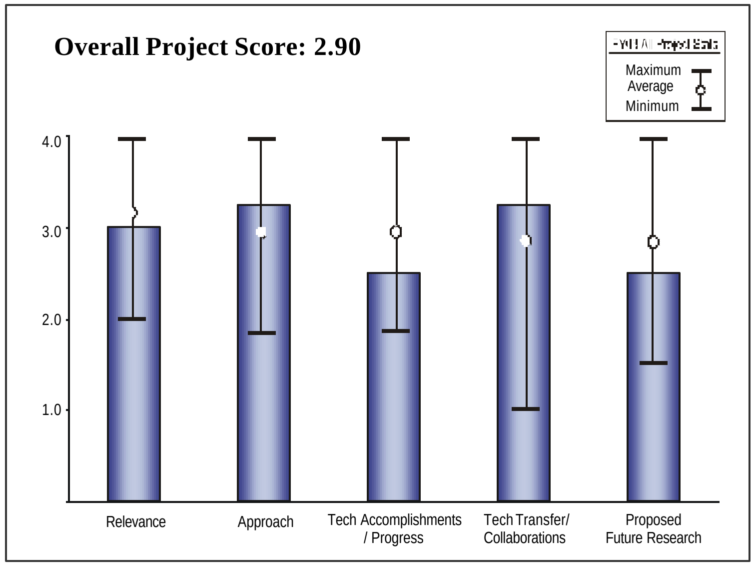
Specific recommendations and additions or deletions to the work scope

- Have closer collaboration with TIAX on input assumptions. Being separate and independent is useful, but using different assumptions clouds the usefulness of the results.
- Investment cost required for mass production should be included in the analysis.
- Roadmap to lower cost badly needed.
- Place technology experts who add more technical input into future projections of manufacturing schemes and costs.

Project #106: Water-Gas Shift Catalysis
Choung, Sara Yu, ANL

Brief Summary of Project

In this project, Argonne National Laboratory is developing water-gas shift catalysts that will meet the DOE goals of 90% CO conversion. The catalysts targets are 99% gas hourly space velocity (GHSV), <$1/kilowatt electric ($kW_e$), as well as elimination of the need for careful in situ catalyst activation. In addition, Argonne is developing water-gas shift catalysts that are tolerant to temperature excursions and have operating lifetimes of >5000 hours.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Low-temperature WGS catalysts are needed for atmospheric-pressure fuel processors
- Enhancing WGS catalytical activity is critical to realization of the President's hydrogen vision.
- Although the project is addressing DOE targets, the results so far indicate poor prospects for significant cost reductions and demonstrable durability.

Question 2: Approach to performing the research and development

This project was rated 3.25 on its approach.

- Good mix of literature search, modeling and experimental data and seems to be a sound approach.
- The approach should focus on: catalyst activity (it must be improved); catalyst stability (current catalysts decay rapidly); cost.
- The technical approach should include cost-benefit analysis.
- Good diversity of approaches, but still lacks the innovation needed to achieve a breakthrough in challenges.

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

This project was rated 2.50 based on accomplishments.

- Work on both precious metal and non-precious metal catalysts is good.
- Put equal emphasis on formulation development and wash-coating techniques - important for monoliths.
- Good progress towards improved catalyst activity. Further work is needed to improve durability.
- All catalyst tests have been relatively short-term (< 60 h.). Longer-term endurance testing and life projection should be performed.
- The cost benefit was not described.
- The modest progress achieved has, nonetheless, provided better understanding of the activity/stability challenge.

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

This project was rated 3.25 for technology transfer and collaboration.

- A commercial contract with Toyota is excellent!

- Good interactions with potential customers; collaboration with catalyst manufacturers would be desirable.
- Test results and conclusions from industrial collaborators should be presented.
- Very appropriate industry partnerships have been established for the evaluation of catalysts under real-world conditions.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 2.50 for proposed future work.

- Monolith work should be given priority. Powder and monolith catalysts may show different behavior.
- Forward plan addresses key issues
- Future work plan does not describe how to retard Pt sintering and how to improve Cu catalyst sulfur tolerance.
- Use of SEM/TEM should greatly help with the understanding of the sintering mechanisms in PtCe and Cu.
- Also examine different methods of preparing Pt bimetallics since different morphologies could affect activity/durability.

<u>Strengths and weaknesses</u>

Strengths
- Good work combining literature and modeling.
- Reasonable progress toward better catalyst activity

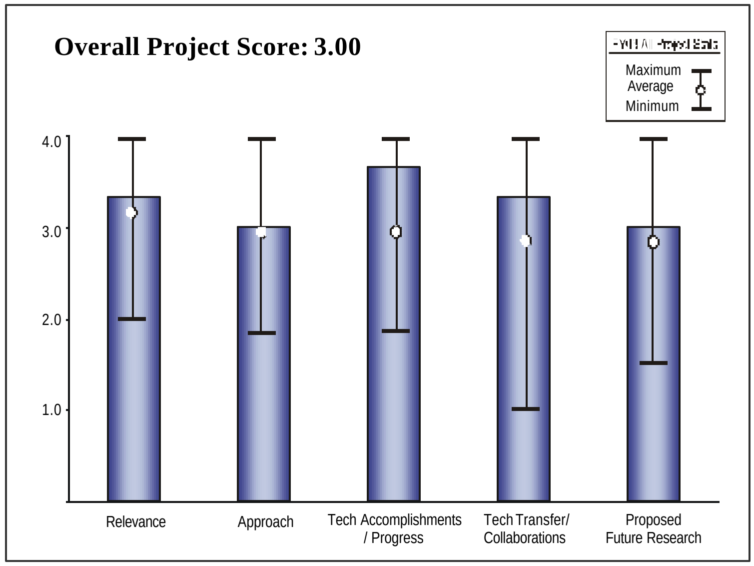
Weaknesses
- Catalyst stability not yet satisfactory.
- Comments from industrial collaborators should be reported.

<u>Specific recommendations and additions or deletions to the work scope</u>

- Be more aggressive in seeking industrial collaboration. Can this technology be licensed to Engelhard or another catalyst company for manufacturing?
- The search for a low temperature WGS catalyst that is stable under FC fuel processor conditions has been going on for several years now. This project should assess the feasibility for such a catalyst to be developed. Are the requirements for activity, durability and cost ever to be met?
- Should publish in refereed journals.
- The contractor needs to report to DOE how much activity of life improvements are required to achieve the program objectives.
- Project needs critical review soon (2004) since progress appears limited when measured against targets.

Project #107: Catalysts for Autothermal Reforming
Mawdsley, Jennifer, ANL

Brief Summary of Project



Argonne National Laboratory is working on improving catalytic activity, reducing the cost of autothermal reforming (ATR) catalysts, decreasing the size of the fuel processor, and reducing start-up-time, while developing a better understanding of reaction mechanisms in order to optimize catalytic activity, minimize deactivation, and improve sulfur tolerance.

Question 1: Relevance to overall DOE objectives

This project earned a score of 2.90 for its relevance to DOE objectives.

- New catalysts are needed for reforming, so this project has high value to reduce costs and increase performance/stability.
- How does this work integrate with reforming work presented in the hydrogen session?
- If on-board reforming is not economically viable then GHSV target can be relaxed.
- Highly relevant topic - autothermal reformer is key to transportation applications.
- Supports Fast Start Reformer Project as well as general ATR catalyst development.
- Project supports needed efforts to overcome technical barriers.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Needs to put emphasis on S-tolerance.
- Needs science-based choices for new combinations of metals and metal oxides.
- Needs further studies on sulfur tolerance.
- Needs more tests on reduced temperature in furnace to better simulate actual reactor conditions. Perhaps reduce focus on GHSV as on-board may not be economically viable
- Well-planned work, should include kinetic model for catalyst deactivation.
- Long-term endurance testing will be needed to establish durability.
- Effort builds on previous ANL work.
- Objectives are appropriate but approach seems somewhat narrow and does not seem to be weighting efficiency and durability enough compared to cost.
- Needs more work on understanding of reaction mechanisms.

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

This project was rated 3.00 based on accomplishments.

- Appears to be making steady progress, but will this just "dead end" due to deactivation issues?
- They need a fundamental understanding to see if deactivation has been controlled.
- Good work on understanding sintering.
- While '03 milestones are significant, longer duration tests (5,000-10,000 hours) should be scheduled soon.
- How far is the catalyst activity from the goal and how much improvement is still needed?
- Good progress on GHSV improvement and reduction in methane slip.

- Rh catalyst at 55% is promising but not a world-beater.

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

This project was rated 3.40 for technology transfer and collaboration.

- Work with Süd-Chemie is good.
- Interactions with other reforming work in the hydrogen session needed.
- Good collaboration.
- Well balanced collaboration with industry and academia.
- Collaboration with Süd-Chemie and two universities is underway.
- Specific results therefrom not indicated.
- Work with H_2 Fuel not mentioned.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.70 for proposed future work.

- Not clear when one decides a catalyst is "good enough".
- Appears to be a "continuous" project.
- Turn-down is also important.
- Well planned future work. Needs to include field tests at potential customers' sites.
- The future work plan does not detail how to decrease precious metal loading while improving catalyst stability and life. The same comments apply for the perovskite catalysts.
- Specific tasks identified as Future Work appropriate for accomplishing objectives. 1-5 kW adiabatic reactors are logical next step in evaluating GHSV status.
- Consider catalyst loading secondary at this stage of development.

Strengths and weaknesses

Strengths
- Good innovation.
- Important to reforming development.
- Well planned research, experienced team, significant outside collaboration.

Weaknesses
- Still struggling with S-tolerance.
- Needs more fundamental understanding of deactivation.
- Should include independent catalyst testing at potential customers' sites.
- Schedule is slipping and may impact Fast Start FP. Backup plan not identified.
- Needs to publish results in peer reviewed journal.

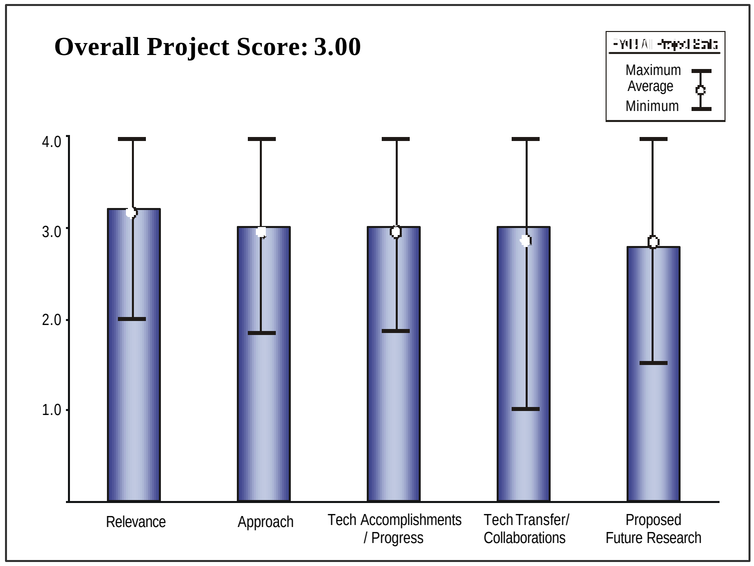
Specific recommendations and additions or deletions to the work scope

- Recommend enhancing work to understand deactivation.
- Needs to tackle S-tolerance issues as higher priority.
- Have potential customers test catalyst. Develop catalyst deactivation model to quantify rate of deactivation. Publish in refereed journals.
- Endurance testing (with or without sulfur) should be performed to project catalyst lifetime.
- Broaden work beyond previous efforts. Consider dropping pervoskites if GHSV cannot be increased unless S-tolerance cannot be attained since cost targets can be reached with PM. Studies indicate FP catalyst cost is not a major system cost element.
- Needs more work on understanding of reaction mechanisms.
- Consider system approach to design the best component operation to remove sulfur (not necessary in ATR). Consider concentrating more on S poisoning, deactivation and sintering with PM.

Project #108: Development of WGS Membrane Reactor
Ho, Winston, Ohio State University

Brief Summary of Project

The Ohio State University is working on development of a novel water-gas-shift membrane reactor. In this project, Ohio State is developing a mathematical model for the novel water-gas-shift (WGS) membrane reactor with a synthesized carbon dioxide (CO_2)-selective membrane to elucidate the effects of system parameters on the reactor and to show the feasibility of achieving hydrogen (H_2) enhancement via CO_2 removal and carbon monoxide (CO) to 10 parts per million (ppm) or lower.



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

This project earned a score of 3.20 for its relevance to DOE objectives.

- This approach is based on a low T (140ºC) CO_2 separation membrane. The membrane cannot be integrated into a WGS reactor (very low kinetics), so it only eliminates the PrOx subsystem.
- Current predicted component size is bigger than current PrOx→ No volume, weight or cost reduction.
- Interesting new approach.
- Very relevant to administration goals even though it is a small piece of the puzzle.
- This effort not only addresses H_2 production improvements in system operation simplicity and efficiency for on-board, it is also applicable to off-board applications as well.

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

This project was rated 3.00 on its approach.

- In addition these membranes (polymer-based) are prone to defects; low selectivity. Needs a high temperature membrane, so that you can perform WGS. Errors in estimating reactor size, etc…
- Need to move forward to the proof-of-concept on experimental level vs. modeling
- Believe this project would benefit if it remained with others. Not comfortable with effort with respect to the different gas phase species at the various accessible points, can you quantify?
- Data for only one polymer has been shown for higher temperature operation and no suggestions are provided for alternatives or modifications to materials that may improve performance.
- H_2 permeability and humidity dependence are two technical issues to be addressed.
- The approach is innovative and technically sound.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- Difficult system to scale up for mass production
- Good start but needs to present data in more common units such as space velocity (h^{-1}) vs. (mol/s) and selectivity defined as % of change of certain components such as CO vs. initial concentration in the feed.
- Would like to see effort focus on specific transport properties and characteristics of the device.
- Good progress has been made.

- Would like to see more candidate materials tested and effect of alternative, transient, start-up type conditions on performance.

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

This project was rated 3.00 for technology transfer and collaboration.

- Time will be needed to further develop the concept.
- This area could be enhanced by working with real system operators or potential device fabricators.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 2.80 for proposed future work.

- Did not present a 2003 plan.
- Very ambitious plan for prototypes, excellent if the modeling work can be demonstrated and configured on (a) prototype equipment.
- Not really clear as to what is next based on the presentation.
- Better understanding of potential failure modes.
- Could increase rate of materials development for investigating limits early to determine material property faults later.
- The membrane durability (mechanical, lifetime and cycling) needs to be addressed.
- Cost analysis should be performed to assure cost benefit for both transportation and stationary systems.

<u>Strengths and weaknesses</u>

Strengths
- Novel concepts being applied-solid know-how.
- This project is technically sound and has demonstrated the feasibility of the novel approach.

Weaknesses
- More weaknesses than strengths due to the low operating T of the membrane.

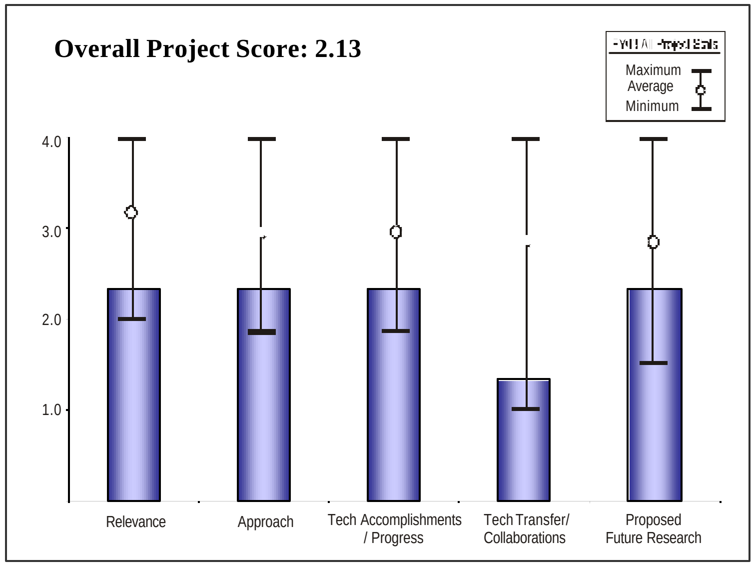
<u>Specific recommendations and additions or deletions to the work scope</u>

- Recommend to identify a high temperature (=300°C) membrane system, if it exists.
- Add an investigation task on how to scale up the design of the reactor.
- Add system trade-off studies.
- A detailed model (experimental) be generated so as to mate the potential characteristics with fuel cell "systems."
- This project has a lot of potential, but the materials development and the reactor design and testing should be addressed by two different organizations.
- The author should focus on materials development and material alternatives.
- The effect of fuel and air impurities on membrane life will need to be addressed.

Project #109: OnBoard Vehicle, Cost Effective Hydrogen Enhancement Technology for Transportation PEMFCs
Dardas, Zissis, UTRC

Brief Summary of Project

In this project, UTRC is developing on-board vehicle, cost effective hydrogen enhancement technology for transportation PEMFCs. Based on FPS and membrane reactor modelling and analysis, maximum FPS efficiency does not necessarily imply maximum FC efficiency and the the overall FC system efficiency rather than FPS efficiency should be maximized.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.33 for its relevance to DOE objectives.

- This project offers one approach to provide pure hydrogen to the fuel cell stack from gasoline as the onboard fuel.
- No explicit discussion of relevance (relevance slide missing).
- Did not provide project timeline.
- PI needs to show how project will contribute to illuminating Go/No-go decision in '04 on on-board fuel processing.
- H_2 separation key to PEM viability.

Question 2: Approach to performing the research and development

This project was rated 2.33 on its approach.

- The overall fuel cell systems efficiency is projected to be only on the 22 to 33% range, which is rather low, particularly at high stack operating pressures.
- Integration of WGS reactors within a membrane-based subsystem is innovative but need more details on how the subsystem will function in overall reformer system.
- Market barriers not addressed.
- Choice of Pd membranes questionable. Classical thermodynamic analysis indicates Pd-W should not be S tolerant. Long-term testing in S necessary to validate approach.

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

This project was rated 2.33 based on accomplishments.

- Most of the accomplishments to date appear to have been in modeling and simulating system performance.
- It is not clear if alloy membranes can be made with their electroless plating process.
- Little connection between Pd synthesis and system integration and benefits.

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

This project was rated 1.33 for technology transfer and collaboration.

- No external interactions were identified or discussed.

- No partners identified - one with an industrial gas company would help project, esp. in view of other UTC projects with strong partnerships, e.g. UTC/Shell on H_2 reforming.
- Little interaction with others outside UTRC. Why is UTRC developing supports when they are commercially available? Partner!!!

Question 5: Approach to and relevance of proposed future research

This project was rated 2.33 for proposed future work.

- Much of the future work seems focused on identifying suitable porous stainless steel substrates - makes one wonder if they are on the right track.
- During Q&A someone in the audience mentioned Pall Corp. had such substrates. The presenter was non-committal.
- Proposed future research addresses Pd membrane synthesis but not system issues - simplicity, thermal integration, H_2 purity with reformate feed stream.

Strengths and weaknesses

Strengths
- Project has innovative potential.

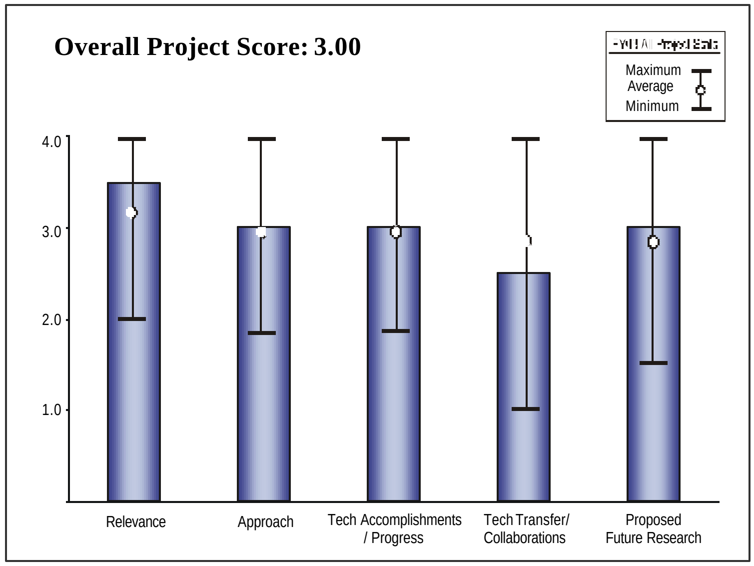
Weaknesses
- Many of the slides were very busy and hard to read, on screen and in the hand-outs.
- It was not clear what the projected costs of this approach would be.
- Lacks timeline.
- Rely on literature to prove S tolerance of membranes.
- Carry out long term tests to prove tolerance.
- Needs classical thermodynamic analysis.
- CO tolerance needs to be proven.

Specific recommendations and additions or deletions to the work scope

- With such projected low system-level efficiencies, they should identify where the losses are and develop means to reduce such losses. Without significant improvements in efficiency, is this approach viable?
- PI needs to show how project fits into overall DOE reformer development effort and what value the project adds to meeting the DOE reformer targets.
- Need economic analysis to prove cost viability.
- Need comparison to other membrane approaches.

Project #110: Advanced High Efficiency Quick Start Fuel Processors for Transportation Applications
Chintawar, Prashant and O'Brien, Christopher, Nuvera

Brief Summary of Project



In this project, Nuvera's technical approach consists of replacing conventional pelleted catalysts and heat exchangers with compact and low thermal mass substrate (monolith, foam, reticulate) based media. As a result, two products will be produced: STAR (Substrate Based Transportation Application Autothermal Reformer) and HiQ (High Efficiency Quick Start Transportation Fuel Processor). The STAR fuel processor is an autothermal reformer capable of 200 kWh input, automotive volume of approximately 75 liters, design focus on gasoline, under-vehicle "flat" aspect ratio, and modular, serviceable design. The HiQ fuel processor will incorporate the high power density STAR technology with a turbocompressor/integrated motor-generator that is compatible with 50kW elevated temperature stack.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.50 for its relevance to DOE objectives.

- Fuel processors will be needed to generate hydrogen. These projects allow technology growth for multiple applications.
- The development of an integrated fuel processor with low thermal mass substrates (STAR) is very relevant to the DOE goal of developing reformers with a fast start capability.
- This fuel processor development also is relevant, but to a lesser extent, to the development of fuel processors for off-board or stationary production of hydrogen.
- The HiQ fuel processor is less relevant to the DOE goals as they are defined in that the focus has shifted from reducing fuel processor startup time to reducing total system startup time.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- They are aggressive in conceptual designs but tend to exaggerate novelty. Technical merits should be explained with a little more data/numbers.
- The approach taken in the STAR fuel processor program is good in that it focused on developing an integrated fuel processor with catalysts on substrates.
- The approach taken in the HiQ fuel processor program is good and innovative in addressing the issue of total system startup. Also, the approach to testing the turbo-generator concept, particularly with the stack simulator, is well thought out.
- The approach to improving the fuel cell processor system and its startup time is not identified.
- Also, the overall approach adds significant complexity to the fuel cell system by adding the need for additional power conditioning electronics and expensive components (turbo-generator).

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

This project was rated 3.00 based on accomplishments.

- Good effort and success in many directions.
- Disagree with PI's low prioritization of materials and catalysts.
- The STAR fuel processor development is a significant technical accomplishment in reducing fuel cell processor size and moving toward meeting the requirements for an automotive fuel processor.
- Catalyst durability will need to be improved.
- The HiQ program is moving toward the goal of a quick start fuel cell system and seem to be on the track with the set milestones.
- The project goals and timing appear to fall short of the DOE targets, perhaps these are considered as stepping stones to the DOE targets. The DOE target for stand up is full rated power in less than 30 seconds while the Nuvera goal in the HiQ program is 25% of rated power.

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

This project was rated 2.50 for technology transfer and collaboration.

- A catalyst company and a university are partnering with them.
- Partnering with organizations that can offer technical advantages may be more attractive. That is, select technology first then agree on partnership/collaboration.
- There is some collaboration between Nuvera and catalyst and substrate companies. There is also some university collaboration. Beyond those, there is little technology transfer, probably because the technology developed is considered proprietary.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Nuvera receives significant government funding. Yet technical data in public domain remains scarce. They are systematic in laying out plans.
- The proposed future research is a well thought out continuation of the program.
- The project end point is clearly defined, but there are no obvious plan off-ramps.
- There probably should be an off-ramp based whether the proposed combined cycle is successful in terms of projected cost and system efficiencies and whether an appropriate turbo-generator can be developed.

Strengths and weaknesses

Strengths
- The module serviceable design of the STAR fuel processor is a good approach to an R&D fuel processor.
- Also, the stack simulator is a good idea to test both the turbo-generator concept and fuel processor/fuel cell stack integration issues.

Weaknesses
- The addition of a turbo-generator will be a significant cost to the system.
- The overall efficiency gain, if any, should be balanced against the additional cost.

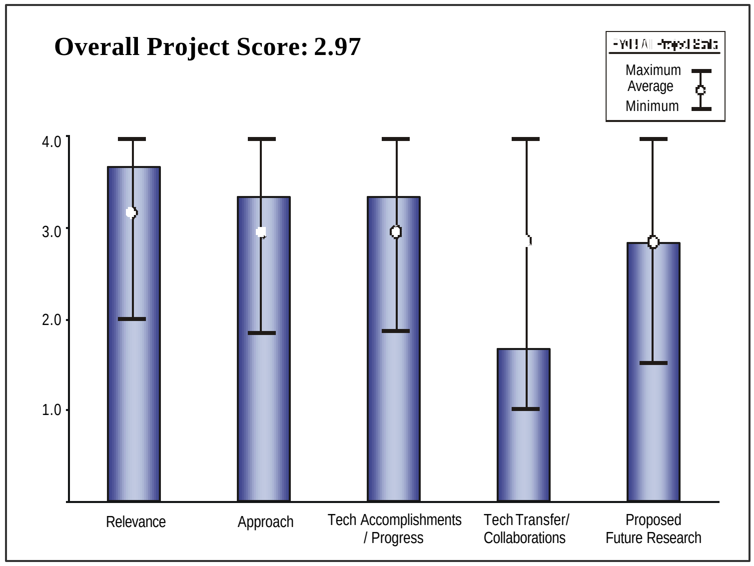
Specific recommendations and additions or deletions to the work scope

- None specified.

Project #112: Plate-Based Fuel Processing System
Dalla Betta, Ralph, Catalytica

Brief Summary of Project

Catalytica Energy Systems will design, build and demonstrate a plate-based fuel processing system that will convert EPA Tier 2 gasoline into a hydrogen-rich gas for direct use in polymer electrolyte membrane (PEM) fuel cell systems for vehicular applications.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.67 for its relevance to DOE objectives.

- This project will help test out a nice reactor concept.
- Research aimed at developing a compact on-board fuel reformer that would yield a high hydrogen concentration in the reformate.
- Flat plate reactor design allows heat to be provided to adjacent sections sufficient to drive a very endothermic reaction.
- The development of a plate-based reactor system is relevant to the DOE objectives of reducing cost and improving performance, particularly start-up and transient performance.
- Projections for meeting the fuel cell processor size criteria look good.
- Projections for meeting cost criteria or reducing cost were not presented.

Question 2: Approach to performing the research and development

This project was rated 3.33 on its approach.

- Approach appears to be weighted to their strength - catalyst development.
- Reactor modeling is leading to early detection of favorable options.
- Approach in general appears to be very promising. High conversions at impressive velocities were achieved.
- Thermal cycling tests are unique and useful - not being done by others so extensively. Somewhat difficult to understand complete approach since reactor details were not given.
- FeCr alloy construction very durable, but cost may be an issue.
- The approach taken to develop a plate-based reformer system is well thought out.
- The prime considerations (or possible roadblocks) are the compatibility of the catalyst with the plates and whether the catalyst activity and kinetics will be sufficient to keep the fuel processor size reasonable. It appears they are addressing these issues through a combination of experimental testing simulating the plate reactor configuration and simulation and modeling.

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

This project was rated 3.33 based on accomplishments.

- Progress on catalyst development, reactor designs are good.
- Results/data provided are incomplete.
- Difficult to interpret value if input conditions are not provided.
- Impressive achievements with regard to fuel processing at high space velocities.
- Thermal cycling tests are important.

- The project to date has made reasonable progress towards achieving their objectives.
- The project as defined would not demonstrate whether the key technical barriers would be overcome until later in the program.
- They have made significant progress in modeling the plate reactor and in design of the plate reactor.
- Experimental measurements of durability show improvements but are still short of the DOE targets.

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

This project was rated 1.67 for technology transfer and collaboration.

- No partners or collaborators.
- Could enhance interactions with labs.
- Appears to have similarities with R & D performed elsewhere on plate-type reformers.
- No technology transfers or collaborations with other industrial partners, universities, or national laboratories were identified.
- The work appears to be all done within Catalytica.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.83 for proposed future work.

- Completing only a plate reformer design study after 3 years is not very promising.
- Future research goals well defined.
- Assessment of energy costs of thermal cycling are important.
- Catalyst stability important - does catalyst spill off walls during operation/thermal cycling?
- The proposed future research follows their project plan and should show whether the plate reformer design will be able to overcome the technical barriers.
- The project plan, as presented, does not have off-ramps or decision points where the project could be ended.

Strengths and weaknesses

Strengths
- Approach shows significant promise for providing a compact reformer that yields a reformate stream with a high hydrogen concentration.
- Achieved high reactor productivity in prototype.
- Combustion catalysts will be an important part of the plate reformer section to supply heat input.

Weaknesses
- Few details provided on specifics of reactor design.
- The presenter made no mention of work on the combustion catalysts.
- The plate-based reactor design has a large part count, and although there are many repeat parts, the parts count could make the cost targets difficult to achieve.
- Other concerns about the plate reactor system which were not addressed are the approach to sealing the reactor and manifolding and flow distribution within the reactor.
- The results presented on the air exposure of the WGS catalyst showed only one cycle. Multiple cycles will be necessary to prove durability.

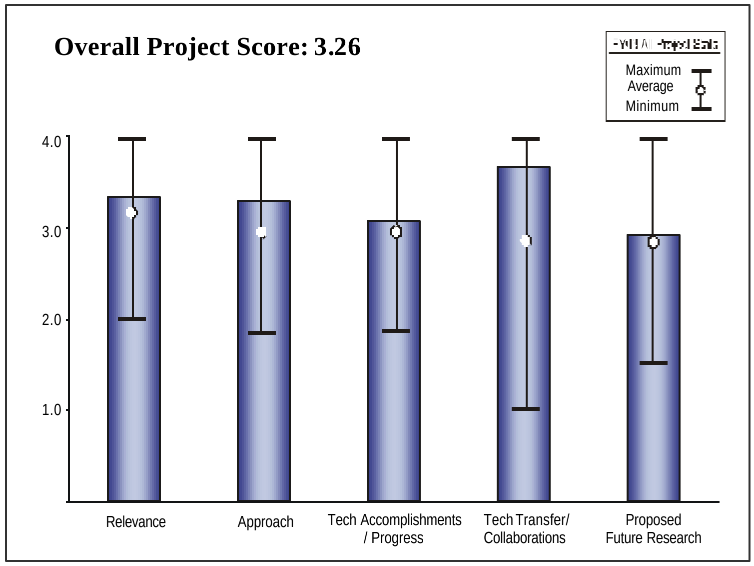
Specific recommendations and additions or deletions to the work scope

- Energy costs of thermal cycling and starting need to be addressed.
- Large size of WGS suggests that this should be a focus.
- S management critical to all fuel processing options.

Project #113: Quick-Starting Fuel Processors
Ahmed, Shabbir, ANL

Brief Summary of Project

In this project, Argonne National Laboratory is developing fast-starting of fuel processors, in which strategies will be developed to meet the rapid-start targets for on-board fuel processors in gasoline-fueled fuel cell vehicles.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.33 for its relevance to DOE objectives.

- Good approach for rapid start-up, however cost targets on the current system design (4 stage WGS, 3 stage PrOx) and sophisticated HXs is dramatically higher (at least 4-5x) than DOE target.
- Well-focused and clear understanding of program needs.
- Well-coordinated and very clear presentation (methodological with strong understanding of fundamentals, i.e. inputs and chemical interactions and trade-offs.
- This project is essential for DOE to make Go/No-go decision on on-board fuel processing.
- Start up time is one of the critical parameters for the 2004 Go/No-go decision on onboard reforming.
- Project is still a bit too new for a critical evaluation.
- This project is important to generation of a public-domain modeling and experimental database towards the 2004 Go/No-go decision on continued fuel processing work.
- In the long term, it is not clear if the project would be consistent with the hydrogen-centered goals of the FreedomCAR program.
- This review will cover both the calculational part of the project and the ANL oversight and integration project for the fast-start fuel processor demonstrator unit.
- Target in 2010 of 30 seconds from 20ºC does not align with DOE 2004 Go/No-go decision. Good otherwise.

Question 2: Approach to performing the research and development

This project was rated 3.30 on its approach.

- Cost targets on the current system design (4 stage WGS, 3 stage PrOx) and sophisticated HXs are dramatically higher (at least 4-5x) than DOE target.
- Questioning the predicted start-up time, though; It is too optimistic with current system design (multi-staged).
- Controls will be very sophisticated thus adding to system complexity and cost.
- Solid "ATR/Shift/PrOx" approach.
- Clear timeline.
- Good understanding of thermal management and interactions due to O/C and S/C ratios.
- Needs to correlate excellent model set-up with experiments and real-life materials/components
- The general approach is good. It appears that modeling might be useful for optimizing the four-stage WGS reactor. Efficiency needs to be addressed, as does cost.
- The modelling part of the project is appropriate for determining whether the fast start of a fuel processor is within the realm of thermodynamic possibility by comparing the total heat capacity with the amount of fuel available without imposing an unacceptable fuel economy penalty (in coordination with NREL analysis project).

- The experimental part of the project will contain many good ideas, but warm-up of desulfurization materials appears to have been ignored and it is not clear that enough attention has been given to ensuring a clean initial lightoff of the ATR.
- Bringing this multiorganizational project to fruition within its limited timeframe will require tremendous achievements both technical and organizational in nature.
- Some simplification of the design of individual components might provide more and better information within the alloted timeframe.
- Experienced team established consisting of national labs, university and industrial members. Integrated approach progressing thru analysis, experiments with subcomponents to complete systems.
- Strong analytical system approach.
- Focus on major obstacles.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.08 based on accomplishments.

- Possibility to achieve both cost and start-up time targets with this design not evident. It remains to be seen with real experimental data.
- The durability of the reformer and especially WGS 1 and 2 catalyst will be affected.
- Model is very robust and recognizes compromises and trade-offs.
- Zonal approach is good (theoretically) but how does it translate into practice?
- Clear understanding of penalties for quick start.
- When are the practical experiments scheduled?
- Can hardware materials handle the thermal transients?
- Size and cost analysis need to be performed also (in addition to fast start-up time).
- Project is off to a good start.
- The modeling aspect of this project has worked backward from the 2 MJ/50 kW_e startup energy tech team target to a conceivable pathway by which that goal could possibly be achieved. It has abandoned the parallel heating approach proposed last year. In doing so, it is not clear that it has fully addressed the issue of local overheating of catalysts that was addressed last year through the use of very high air flows to keep the temperature rise of the gas flows below that at which damage to the catalysts would be probable.
- A major challenge for the proposed approach will be maintaining adequate catalyst durability-the demonstration of 50 shutdown/quick startup cycles would be necessary to provide any confidence that adequate durability might be within reach.
- Relevant aspects of catalyst sintering and other degradation mechanisms can be fundamental processes not clearly amenable to engineering solutions.
- Program is at midpoint with excellent progress shown to date.
- As a hardware performance demonstration project, proof of accomplishment occurs in final demonstration phase.
- Analysis is solid and on track.
- Accomplishments for individual components indicated (kinetics, P&ID, component designs and tests, etc).

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

This project was rated 3.67 for technology transfer and collaboration.

- Excellent, well integrated effort with many pioneers in each of the technical fields (catalysts, microdevices, catalyst supports).
- Excellent collaboration across national labs and "interesting" industrial company (one emerging).
- Needs to involve "bigger" more traditional oil and gas companies.
- Collaboration with other national labs is excellent.
- Collaboration with developers should also be included.
- Good collaboration between national labs, but it was unclear whether it extended beyond that.

- If this complex multiorganizational project can be pulled off on schedule, it will be a triumph of collaboration between at least three laboratories and two industrial organizations, one of which has relevant mass-production capabilities.
- It appears that at least the appropriate groundwork for this collaborative tour de force is in place.
- Time will tell whether the organizational, as well as technical, challenges can be fully surmounted.
- Large multi-disciplined team in place and functioning well.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.92 for proposed future work.

- Project needs to be more specific. This is moving forward because the start of this program is so impressive.
- Needs better parallel processing of hardware-type experiments alongside modeling efforts
- How to assure that the experimental results can be projected to actual system.
- Future research program appears well targeted.
- Generally well thought-out plans to a very ambitious timescale.
- All might be possible if nothing goes wrong, but this is a hard area in which to advance without learning from multiple failures.
- Are test rigs available or being built to allow all downstream components to be proven out before assembly to the upstream components (this would seem to be the only way to meet the timeframe)?
- Bringing forward separate tests of ATR light-off (with adequate spare parts for repeated rebuilds and perhaps redesigns) would seem essential to the attainment of full system experimental data within the planned timeframe.
- Accurate measurements of total power consumption to both first full electric power and to steady-state operation are as important as startup times.
- Analysis performed yielding time history of critical subcomponents needs to be verified at subcomponent level before integration and after integration. Could have off-ramp after subcomponent verification.
- Not enough time allotted until final testing.

Strengths and weaknesses

Strengths

- Smart approach for rapid start-up.
- Great, comprehensive, modeling efforts.
- Very systematic approach with clear understanding of the chemistries and mass flow issues and kinetics.
- The team is strong in performing the system analysis and is well qualified to perform the tasks.
- Thoughtful calculational work, presumably based on hard kinetic data for each of the individual reactors.
- The tapping of capabilities from an impressive collection of organizations.
- A lot of chutzpah to propose such a complex project.
- Excellent program.

Weaknesses

- The system design is still complicated, though (many reactors and HXs) even if small in size.
- Why not have a parallel effort in materials and hardware?
- Not clear that the means of ATR ignition has been adequately considered.
- WGS reactor volume production over optimistic.
- The system will cost too much.
- Has enough durability work been done to generate even an educated guess as to how many times this thing could quick-start before losing 25% of its rated throughput?
- Annular design is good on paper but complicates reconstruction after a problem develops.
- Conflict in development approach between complex compact (low mass) highly integrated design and ease of use of sub-components arranged for sub-optimization resolved in favor of meeting overall project goal of quick startup. This is essential to understanding/optimization of results of integrated system to have complete experimental performance data of subcomponents.

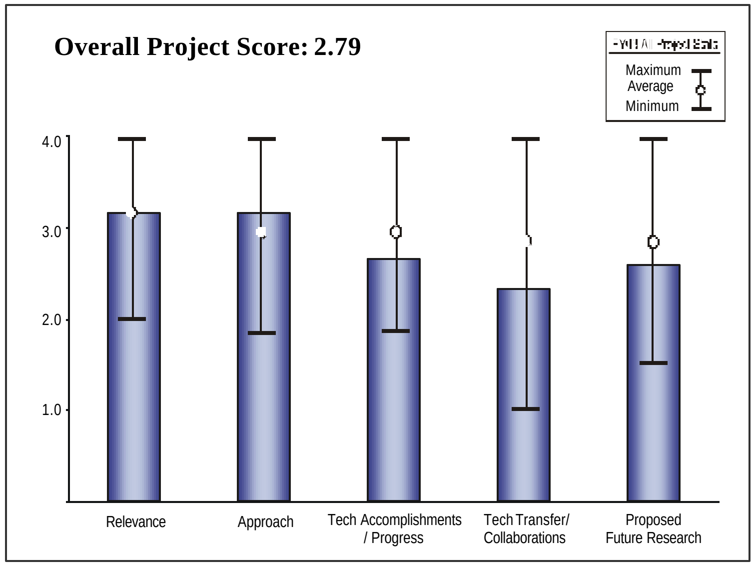
Specific recommendations and additions or deletions to the work scope

- Find ways to reduce system cost.
- Better access system controllability, especially under start-up and transients.
- More detailed control strategies should investigated.
- In areas where the modeling efforts are strong, make sure that the experimentalists work alongside so that practical feedbacks and compromises can be incorporated. This effort would be greatly enhanced with real-world experiments.
- Should non-national lab technologies be included in the analysis?
- Some means of sulfur control needs to be factored into the startup calculations and experiments. Is "the best" the worst enemy of "the good" here, and might not "the good" be good enough?
- Consider a bit of simplification in light of need for public domain experimental data in time for the decision point.
- Add more schedule time for system optimization after controls testing and total system testing.
- Publish design approach and results for individual subcomponents, integrated system, and experimental verification of design.

Project #114: Progress in Microchannel Steam Reformation of Hydrocarbon Fuels
Whyatt, Greg, PNNL

Brief Summary of Project

In the microchannel fuel processor development project Pacific Northwest National Laboratory is developing a compact, steam reformation-based fuel processing system for onboard reformation of hydrocarbon fuels that meets FreedomCAR performance targets. In addition PNNL is developing highly effective reactors, fuel and water vaporizers, recuperative heat exchangers, and condensers broadly applicable to fuel processing and fuel cell systems.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.17 for its relevance to DOE objectives.

- Why stop at 2 kW_e? The goal should be to design 50 kW_e system.
- High relevance, includes important efficiency analyses.
- The higher productivity and start-up benefits gained from higher temperature steam reforming are definitely needed to verify the credibility of microchannel designs.

Question 2: Approach to performing the research and development

This project was rated 3.17 on its approach.

- Was not articulated properly.
- Weight reduction issues by applying materials other than stainless steel should be addressed.
- Needs industry support to manufacture devices
- Needs computational work.
- Needs manufacturing assessment.
- Microchannel components need to be considered.
- Heat exchange applications could be very useful.
- Addressed energy/efficiency penalty due to cycling, which is very important.
- Needs to expand that study to include exhaust heat losses, which would make the analysis more realistic. Furthermore, need to do longer H_2S studies (esp. at 800^{o}C); 1-3 hours is too short to make statements about long-term durability.
- The approach is appropriate for supporting rapid start-up, but the use of Inconel 625 materials raise cost concerns.

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

This project was rated 2.67 based on accomplishments.

- Test device was not manufactured properly leading to poor flow distributions.
- Takes several months to get manufactured, too slow.
- Needs numerical modeling to support work. (ANL can help).
- $1.8M is a lot of money. Need to show more output, (leverage with industry).
- Weight remains a concern. 3" insulation still results in a large package.

- System complex to maintain -- change catalyst, etc.
- Sulfur degradation is disappointing.
- Good progress since last presentation.
- Early days yet, but the reformer productivity benefits of higher temperature operation have been demonstrated.
- The use of moving flow path baffles in air side of low dP reactor is a reliability/durability concern.

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

This project was rated 2.33 for technology transfer and collaboration.

- Good interaction and efforts to commercialize technology.
- Should be intensified.
- Needs automotive partner with freedom to operate.
- Manufacturing required.
- Catalyst partner can help.
- No meaningful interactions with auto industry.
- Stronger interactions with external partners beyond GTO & McDermott, especially with ANL, would be advised.
- Difficult to judge – few details given.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.60 for proposed future work.

- Only approach to enable on-board reforming. In view of the difficulties of H_2 storage on-board - this approach needs to be fully tested.
- Future research program addresses important technology barriers, but a better efficiency penalty analysis needs to be undertaken.
- This project desperately needs to demonstrate the claimed benefits of microchannel designs.

Strengths and weaknesses

Strengths
- Nice high potential technology, good progress.
- Microchannel design integrates a set of difficult reactors.

Weaknesses
- Presentation conclusions did not acknowledge the durability trade-off between higher temperature operation and catalyst activity and structural materials survivability.
- Need manufacturer (auto-related) as current manufacturing process not good.
- More catalyst development required due to this new environment, where heat and mass transfer limitations are no longer a problem. Can partner in this area.

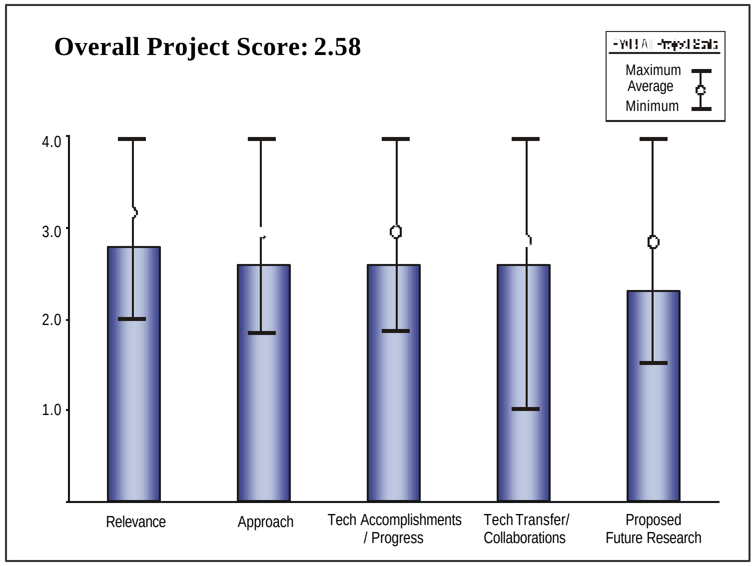
Specific recommendations and additions or deletions to the work scope

- Carbon formation is a major challenge with heavy hydrocarbon fuels such as gasoline and diesel. This will become even more prominent with microchannels ultimately leading to pressure drop increase.
- Manufacturing (high volume) methods should be assessed (What is the ultimate cost in mass production and what are appropriate manufacturing techniques?)
- Generate an automotive focus lead at PNNL (not ANL) or whereever one sets up manufacturing process and design.
- Focus on system generation (Fuel and H_2O in, H_2 out). ANL and others can support this effort as well as ANL modeling.
- ORNL has carbon material opportunities in design.

- The fuel processor engineering is system engineering.
- Microchannels are clearly more compact reactors, but using them requires tubing, valves, flow controllers.
- PNNL should think of approaches which make the whole system smaller, not just the reactors or HEXs.
- Must start to address cost concerns arising from the fabrication of microchannel devices using nickel-based high temperature materials (Inconels).

Project #115: Reformate Clean-Up: The Case for Microchannel Architecture
TeGrotenhuis, Ward, PNNL

Brief Summary of Project



PNNL is applying microchannel architectures, where appropriate, in fuel processing for transportation, stationary and portable applications to reduce size and weight, improve fuel efficiency, and enhance operation.

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

This project earned a score of 2.80 for its relevance to DOE objectives.

- Fuel processor development is an integration art. It's not clear the usefulness of component R & D which will have to be integrated by system integrator.
- If successful, could reduce size and catalyst costs.
- This technology has good potential for this application, but the specific relevance needs better definition.
- Relevance not in question, but the case for microchannel architecture is still yet to be convincingly demonstrated.

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

This project was rated 2.60 on its approach.

- The approach is good for the chosen objective of component development.
- There doesn't appear to be much fluid dynamic CFD modeling accompanying this work to correlate theory and tests. How will the flow maldistribution be controlled?
- Are the inlet/exit manifolds correctly designed?
- Approach is well reasoned, but differential WGS reactor results are disappointing.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 2.60 based on accomplishments.

- Lots of data!
- Good combination of non-precious metal and PM catalysts for PrOx.
- WGS results are discouraging. Kinetics are less than projected.
- PrOx designs are useful, but show no unique benefits over earlier approaches.
- There appears to be too many different applications for the basic understanding and level of effort - too ambitious.
- So far, BOP (condenser and separator), rather than WGS reactor, have shown notable benefits – need a better reactor!

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

This project was rated 2.60 for technology transfer and collaboration.

- Why repeat sulfur kinetic data, already collected by ANL? Seems redundant.

- More collaboration with fuel cell industry would be nice.
- Difficult to evaluate - listed interactions seem casual.
- No apparent connections to fuel cell developers.
- There needs to be a broader extent of discussion with people who have done work in these areas. Much work has been done in WGS & PrOx.
- Useful contacts established.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.30 for proposed future work.

- Hard to assess from presentation.
- Proposes a continuation of existing work.
- Does Ti material meet cost goals?
- Demonstrate progress in meeting performance/durability with proven understanding of all parameters in one area before embarking on new ventures.
- Need to make up for lost ground on WSG reactor concept.

Strengths and weaknesses

Strengths
- Good approach towards an interesting option for size and weight reduction.

Weaknesses
- Low pressure drop leads to poorly controlled flow distributions - may need to add some other ideas to achieve uniform cooling flows.
- "Durability" is still missing from list of technical challenges.
- Need evidence or pathway for realization of 2,000 hr target.

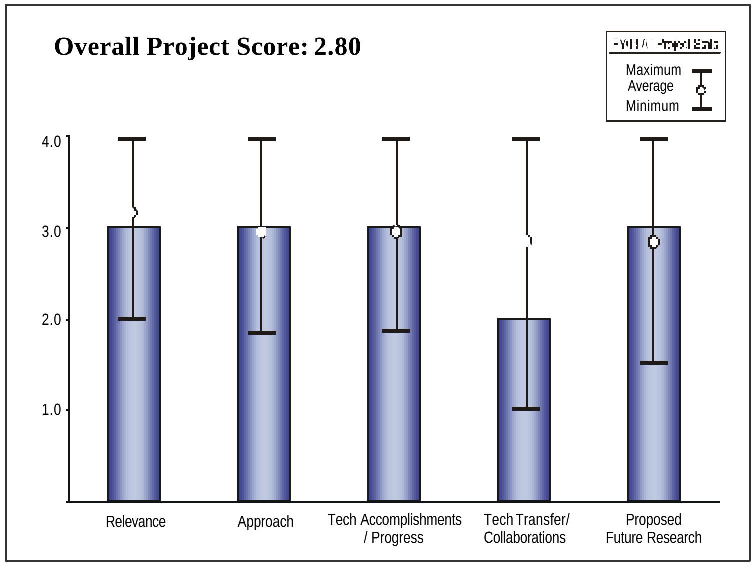
Specific recommendations and additions or deletions to the work scope

- Manufacturing/cost issues to be addressed/estimated.
- PNNL needs to focus on basis of computational fluid dynamics.
- This technology is very demanding and a thorough understanding of the heat transfer within these devices is mandatory.

Project #116: Fuel Processors for PEM FCs
Thompson, Levi, University of Michigan

Brief Summary of Project

The University of Michigan will develop fuel processors for PEM fuel cells. In this project, UM will demonstrate high performance desulfurizer, catalyst, microreactor and microcombuster/microvaporizer concepts that will enable production of compact fuel processors. In addition, UM will design, fabricate, and evaluate a 1 kW fuel-flexible fuel processor in the first 36 months and a fuel-flexible fuel processor capable of producing a hydrogen-rich stream for up to a 10 kW PEM fuel cell following a Go/No-go decision to be completed within 48 months.



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

This project earned a score of 3.00 for its relevance to DOE objectives.

- The work in developing microreactor components with improved catalyst for fuel processing is directly relevant to the goals of reducing size and cost of fuel processors.

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

This project was rated 3.00 on its approach.

- Desulfurization approach of using Cu(I) in zeolite has been tried. The zeolites structure/framework adds a lot of weight which is ineffective in absorbing S, so loading cannot be as high as in other absorbent systems.
- Absorbent size (5 kg) appears to be large.
- Claims for WGS suggested good progress but data supporting claim not shown.
- Data shown for Ni ATR catalyst appears to show good progress, still concerned about coking with a Ni catalyst especially in a microreactor environment.
- The proposed technical approach appears to be a bottoms-up approach to developing a fuel processor.
- The focus of the work is on the components and primarily on the developing improved catalysts.
- There seems to be a combinational approach to developing catalysts. Certainly, improved catalysts would be beneficial, but they need to be pursued within an overall view of how they will work within the fuel processor system. The micro system approach does not seem to be focused as a range of technologies are being investigated.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- Activity of WGS which can be worked at a GHSV of 78,000 h^{-1} is a major accomplishment if verified.
- Component design/development on schedule.
- The program has made good progress toward achieving some of the project goals given their funding situation.
- The catalyst and sorbent work appears to be making progress towards increasing activity and GHSV needed for size reductions.
- Progress in developing microreactors appears to be behind schedule.

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

This project was rated 2.00 for technology transfer and collaboration.

- Appears as if majority of the work is being done at Michigan with minimal involvement by companies at this point.
- Some collaboration is done with the two outside companies and an additional collaboration is being pursued.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Work builds on development of microprocessors at PNNL and catalyst development at Michigan.
- The proposed future research is a good continuation of the current program.
- The program does not have a decision point where the project would be ended.

Strengths and weaknesses

Strengths
- WGS work is a strength. Other catalyst development work is also good.

Weaknesses
- Microprocessor work appears to be similar to work at PNNL.
- Not clear that microchannel architecture will help in areas such as WGS where reactions are kinetically controlled.

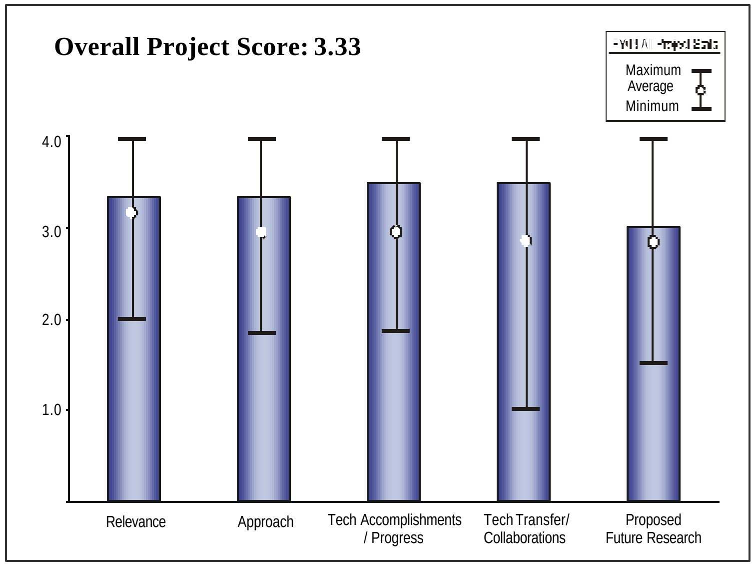
Specific recommendations and additions or deletions to the work scope

- The program should also examine sulfur-tolerant ATR catalysts and a hot gas sulfur removal sorbent. While the researchers have shown good results with sulfur removal upstream of the fuel processor, the concern is that a failure to replace the sorbent would lead to a complete failure of the fuel processor.

Project #117: Direct Methanol Fuel Cells
Zelenay, Piotr, LANL

Brief Summary of Project

Los Alamos National Laboratory will develop direct methanol fuel cells. In this project, Los Alamos will develop materials, components and operating conditions of direct methanol fuel cells (DMFCs) for portable applications optimizing power density, overall fuel conversion efficiency and cost. In particular, they will design and optimize MEA performance, improve electrocatalysis of methanol oxidation and oxygen reduction, thus allowing lower total precious metal loading and/or better cell/stack performance, and demonstrate viability of cell components in short- and long-term operation of single cells and stacks.



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

This project earned a score of 3.33 for its relevance to DOE objectives.

- This program remains very relevant as long as the work continues to focus on APU and portable fuel cell applications. I believe in their collaborations with industry, the researchers understand very well portable applications - however are they knowledgeable about APU fuel cell system requirements?
- The project is well designed to support the President's goal. It's longer term than H_2 FCs for transportation, which is well worth funding by the government.
- Repeat of last year's accolades. Astonishing productivity on all key areas of DMFC.
- Most of the effort is relevant and does apply to higher level goals.
- DMFC may not be appropriate for high wattage transportation systems but will play a role in portable systems. Stayed focused on lower power systems.
- Need better discussion on costs.

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

This project was rated 3.33 on its approach.

- Workers appear to have made significant progress in many areas of the technology. I commend the team for their focus on durability - an area which is poorly addressed in many of the DOE programs.
- As a National Lab, its more important to perform material research than make stacks - especially when the stack work may take away the resources needed for new material research.
- Good integration approach to working on the problem areas as well as performance targets.
- Approach is sound but a few areas could be modified or in some cases enhanced.
- Durability is critical.
- Solid and systematic - focus on durability is appropriate.
- Experiments led to very interesting theoretical revelations.
- Focus on durability and new membranes must be maintained.

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

This project was rated 3.50 based on accomplishments.

- There appears to have been significant learning in: cathode design; durability - in both single cells and stacks; and stack hardware design.
- Work is first class. I especially like the durability study results.
- Replace the word automotive with portable.
- Outstanding progress in reducing PM loadings, and introducing new membranes.
- Accomplishments are solid.
- Approach is well thought out, especially along the lines of endurance testing. Could impact overall goals significantly.
- Further studies in membrane-specific degradation to methanol fuel would be useful.
- Listened to past reviewer' comments.
- Met some milestones.
- In lifetime tests, need load cycling program.
- Ru crossover - a major finding.
- Good collaboration with VA Tech on polyarylene ether sulfones.

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

This project was rated 3.50 for technology transfer and collaboration.

- The team shows a high degree of credibility in attracting both academic as well as industrial collaborations. It would be most valuable to the program to see a stack developer license the technology.
- The interaction with Ball seems to be a national lab as a supplier of stacks. Maybe this should change.
- Great interactions with both the basic (i.e. catalyst and membranes) and the "Customer" (Ball Aerospace).
- Excellent coordination underway - also doing it with the appropriate groups.
- Strong collaborations with Ball Aerospace and Motorola (appropriate choice of partners, including SMP, JM, OM Group, TKK, etc…). Continue this breadth of partnerships up and down the value chain.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- I would like to see this team focus more on the fundamental understanding of the technology - especially in the areas of cell stability and materials durability.
- I hope the team can license the stack technology and avoid becoming a stack manufacturing house.
- I'd like this project to tackle more difficult problems such as anode electrocatalysis.
- Since the PI is now also including durability in his studies, what is the customer's lifetime requirement?
- Future plans will build on solid start. Not sure why stack design, etc… is a key element. Stack design should be left to others - should be working with stack folks in other companies.
- Focusing on life limiting mechanisms now rather than later is good.
- Very clear understanding (and attacks) on key issues.
- Good balance of theoretical understanding and practical experiments. But why move to higher power? What can you learn from it?

Strengths and weaknesses

Strengths
- Focused program on key areas.
- Setting up relationships and building on them.
- Knows what to focus on.
- Solid balance of theory and practice.

- Strong focus on cathode-side and membrane research.

Weaknesses
- It is hard to appreciate the gains without a unifying metric.
- Is it time to start reporting a system-specific power density and specific power? What is the target system power density?
- Wants to design and build stacks in a national lab.
- No clear defense on why the move to higher power applications for DMFC.

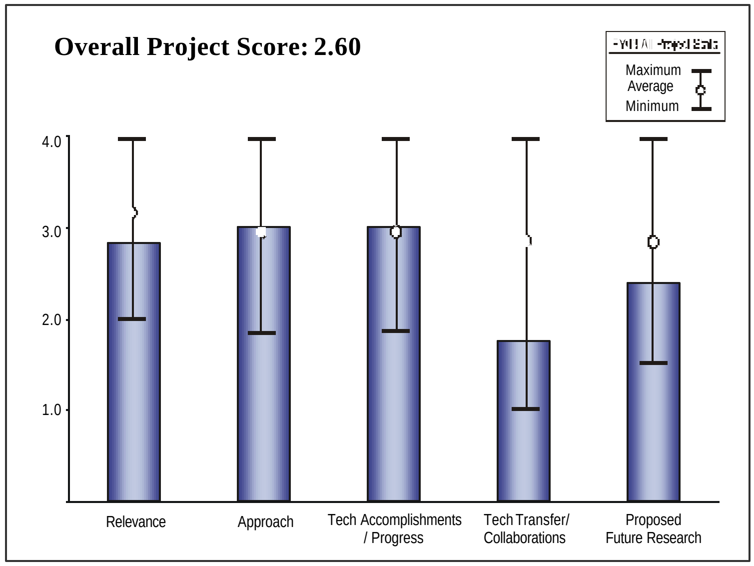
Specific recommendations and additions or deletions to the work scope

- In understanding materials durability, it would be valuable for the team to get a greater understanding of the systems operational needs, i.e. for a portable or APU application, the stack is likely to spend most of its time idle - how would this affect the understanding of important degradation mechanisms - i.e. Ru migration.
- Transfer stack making technology to an industrial company and add new anode electrocatalysis R&D.
- Redirect goals to relate to portable electronics applications (lower temperature applications).
- Continue funding.
- Is this a pure MeOH system? Why not?
- Will the cell be turned off and on: What happens to durability with off/on cycles (depolarized to OCV)? Similarly, what is the design for fuel introduction? Will the cell run until MeOH is exhausted? What happens at the point of low fuel?
- 2003 research plans solid - should do it in a collaborative fashion with team members. Continue to work the fundamentals.
- Stay with your strengths - focus on improving performance and on fundamental understanding.
- No need to go to high power to understand fundamentals because costs remain an issue.

Project #118: Development of Advanced Catalysts for DMFCs
Narayanan, S., JPL

Brief Summary of Project

Jet Propulsion Laboratory is working on the development of advanced catalysts for direct methanol fuel cells. JPL will reduce catalyst cost for direct methanol fuel cells (DMFCs), demonstrate feasibility of reducing Pt-Ru catalyst loading to 0.5 mg/cm^2 using thin film deposition techniques, develop low-cost technique for MEA fabrication, and prepare low-cost alternatives to Pt-Ru based on Ni, Zr, and Ti catalysts.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.83 for its relevance to DOE objectives.

- This project is aligned with reducing cost in direct methanol fuel cells. I believe DMFC technology projects should be aligned to both APU and portable power applications.
- Focus is on catalyst: either metal loading or non-noble metal catalysts.
- This program could impact the higher level goals if it yields.
- The topic is critical and the timing is excellent as any potential use will or should be incorporated into more sophisticated tests.
- Strong focus (and understanding) of catalyst costs.
- Interesting focus on anode-side catalysts, including non-noble metals.
- Focus on catalyst utilization is appropriate.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- The discovery of non-precious metal catalysts for DMFCs would be of enormous value. However, the rationale of focusing work on Ni/Zr alloys is not clear to me. The researcher gave no reason for choosing this material. Indeed the only electrochemical data shown indicated very little catalytic activity.
- Appears most of the efforts were on obtaining low corrosion loss for the new anode material - but not too many MeOH oxidation tests.
- Approach is good - would like to see it enhanced to include structure-property relationships. Would like to see a detailed logic trail as to what metals should be tested - especially before going to ternary and quaternary systems.
- Quick focus on to high utilization schemes (sputtering and thin films).
- Focus on non-noble catalysts is bold (Ni-Zr, etc.).
- Co-sputtering approach - innovative for this area.

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

This project was rated 3.00 based on accomplishments.

- With limited funding, researchers have demonstrated their ability to produce low loading electrodes.
- Would have been nice to see absolute fuel cell performance and not only specific mass activity.
- Corrosion resistance alloys were demonstrated - but with little apparent catalytic activity.

- Good progress in the past on reducing Pt:Ru loading, however only modest progress on alternative catalysts.
- Good progress has been made. With continued progress, the results will impact critical cost and performance issues.
- Explore issues around contamination of FC from spattering process and its effect on membrane durability.
- Sound results (interesting) on co-sputtering experiments.
- Extrapolation of Ni/Zr structure to being an underlayer is innovative.
- Need more scientific explanations for results and material choices.

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

This project was rated 1.75 for technology transfer and collaboration.

- None were reported - too early in the work?
- Did not list much, except for informal collaboration with Northeastern University.
- Not aware of any - it may be time to think about teaming with an MEA manufacturer or at least a DMFC company for initial testing purposes.
- More partnership with industry leaders would be good.
- No mention of collaborations in talk - only mention is Northeastern University.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.40 for proposed future work.

- I am not sure we should focus too much activity on the Zr/Ni materials until we establish there is any catalytic activity.
- What other potential non-precious metal catalysts could be evaluated?
- I am not clear if the approach is "combinatorial" or "theory guided structure by design." If combinatorial, a wider scope of new materials is suggested; if by design, greater collaboration with theoreticians is suggested.
- Would encourage investigator to expand the effort to develop an understanding at the "electronic" level. Overall approach is good - expect it will lead to a new series of catalyst systems.
- Plan simply extrapolates on co-sputtering experiments into quaternary compositions. Needs more theoretical underpinning to their "wish list" of future projects.

Strengths and weaknesses

Strengths
- The methodology of sputter deposition is a powerful tool for not only new catalyst development, but electrode fabrication.
- Good solid depth of know-how and resources and the path to get such work quantified.
- Bold new push into new approaches (co-sputtering of Ni/Zr).

Weaknesses
- Would like to see group branch out to work with others.
- No clear scientific defense of bold approach (somewhat Edisonian).

Specific recommendations and additions or deletions to the work scope

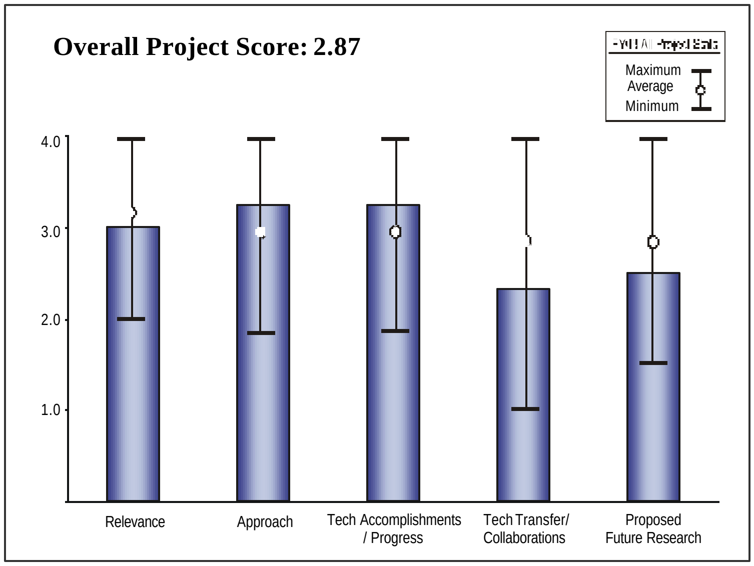
- This is a high risk approach.
- In looking for new precious metal catalysts, this team should not dwell too long on a single material, until there is evidence of catalytic activity.
- Decide if the approach is combinatorial or structure by design - pursue a collaboration with an appropriate partner.
- Presentation appears focused on corrosion over MeOH oxidation activity - maybe MeOH oxidation should be first.

- What is the trade-off of low current density versus low precious metal usage? Should this be modeled to target ideal Pt loadings?
- Develop a correlation between structure and properties.
- Develop an understanding of role of the binary system as a catalyst/support layer.
- Start to build up a theory re: choice of metals alloys. Reach out to other collaborators.

Project #119: Fuel Cell Power System for Transportation - Gasoline Reformer
Tosca, Mike, UTCFC

Brief Summary of Project

In this project, UTC Fuel Cells is developing an atmospheric fuel cell power system for transportation. UTC will deliver a 50 kW fuel processing system which, although focused on gasoline operation, will utilize fuel flexible reforming technology that can accommodate fuels such as methanol, ethanol, and natural gas, as well as fully integrated 50 kW and 75 kW PEM fuel cell power plants.



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

This project earned a score of 3.00 for its relevance to DOE objectives.

- This project is targeting automotive applications, but seems to be projecting 5 min start-up time, 75% efficiency for FP.
- Still have the issue that on-board reforming has limited CO_2 reduction. (Reduction from efficiency improvement and fuel isn't cleaner like H_2.)
- Goals were set by DOE.
- System development and size and weight reduction are important elements of bringing fuel cell technology to commercialization.

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

This project was rated 3.25 on its approach.

- Unclear about what research and development is being done.
- Did not present approach methodology - difficult to understand what technology is being developed when a black box presentation is made.
- Cannot tell if significant component development is occurring.
- Good job switching to alternative technologies when problems were identified (e.g. eliminating steam generator and finding smaller valves).
- Would be good to compare performance against current car standards and California low emissions standards. How close are you?
- Significant progress was made toward achieving project targets. However, little detail was presented on how that was accomplished.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.25 based on accomplishments.

- Showed progress from prior system in reducing mass/volume.
- Good job presenting current progress versus targets.
- Results look encouraging.
- Few comments on cost factors. (Cost is one target on their opening slide - showing cost improvement along with other improvements is important).

- This was somewhat difficult to assess. Significant progress was made by size reduction. But, the view graph listing progress towards specific targets showed little progress until updated orally. Hard to believe that much progress was made between submitting view graph and oral presentation.

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

This project was rated 2.33 for technology transfer and collaboration.

- Hard to assess.
- Company has in-house FC capability.
- Working with Shell.
- Not clear how much car company input they are getting. (Want to be sure everything fits their needs - not just what DOE thinks they need).

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Do not show future plans/research.
- Start looking at cost targets as well.
- Track system weight as well.
- Make sure to document transient responses going down in fuel flow. (No problems are expected but only increased when presented.)
- Focus on start up and system volume, okay. But don't ignore cost and weight.
- Impossible to assess. No details given to approach future work or even if goals could realistically be met.

Strengths and weaknesses

Strengths
- Good progress due to systematic R & D.
- Good job changing system design as barrier became obvious.
- Significant progress made towards accomplishing target goals.

Weaknesses
- This project is difficult to review as little technical details were presented.
- Limited operation to date about 150-200 hrs.
- Don't know what bench-scale data exists to understand how full-scale system is being designed.
- Cost projection is missing.
- Not clear what barrier will be addressed in future work or what approach will be taken.

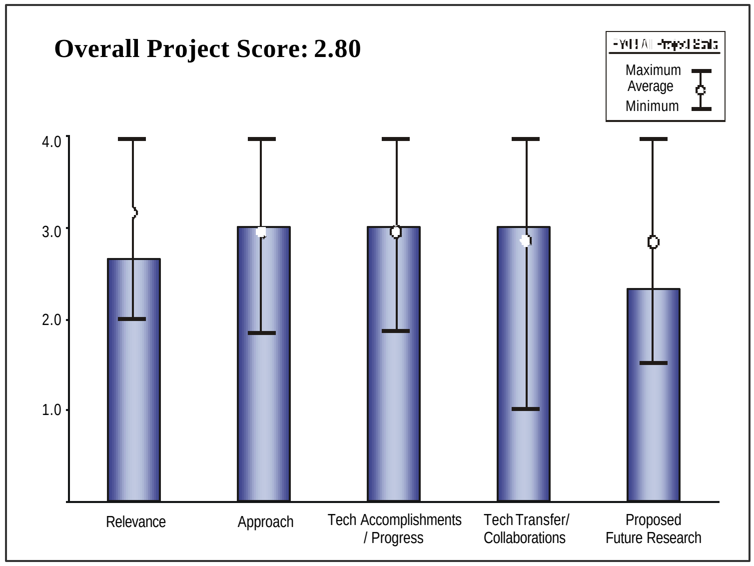
Specific recommendations and additions or deletions to the work scope

- Project would benefit by a more technical presentation.
- Addition of cost project needed.
- Cold start from below freezing should be addressed for complete power plant.
- Continue project after future work plan on schedule has been submitted.

Project #120: PEMFC Power System on Ethanol
Richards, Thomas, Caterpillar, Inc.

Brief Summary of Project

As a team Caterpillar, Nuvera Fuel Cells, and Williams Bio-Energy are developing and demonstrating a 15 kW ethanol-fueled PEM fuel cell system. This project will demonstrate performance, durability and reliability, remove technical uncertainties, and identify correlation and gaps between different applications, collect data to evaluate economic feasibility, and assess commercial viability.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.67 for its relevance to DOE objectives.

- Renewable aspect of ethanol makes this project an important part of DOE's portfolio.
- Target efficiency of 25% seems low.
- The project utilizes a renewable fuel but is best suited to regions that have a significant bio-ethanol resource.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Approach looks reasonable.
- Economic studies need to be shown - with and without ethanol subsidy.
- The project is intended to prove feasibility of the concept. However, the technology for reforming is two generations old.
- Additional verification using the newest generation of reformers and fuel cells is required for Caterpillar to have confidence in a commercialization decision.

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

This project was rated 3.00 based on accomplishments.

- Excellent progress towards objectives.
- Too little test data to accurately judge system accomplishments.
- The project is about on schedule to begin the field demo phase in June 2003.

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

This project was rated 3.00 for technology transfer and collaboration.

- Appropriate interactions with Nuvera and Fuel Cell Energy. No apparent interactions with national labs or other (non-commercial) partners.
- Increasing national lab participation might help in the analysis of the data collected.
- Interactions appear one-sided.
- There appears to be good collaboration among Caterpillar, Nuvera Fuel Cells, and Williams Bio-Energy.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.33 for proposed future work.

- Clear plan for completing the scheduled installation and testing, but plans beyond current contract are not clear.
- While not specifically called out, future plans appear to be to complete demonstration.
- No specific future research is proposed.
- At the end of the test period, Caterpillar hopes to determine the future prospects for this technology.

Strengths and weaknesses

Strengths
- Great to have ethanol PEM project in the portfolio to keep renewables at the forefront.

Weaknesses
- None specified.

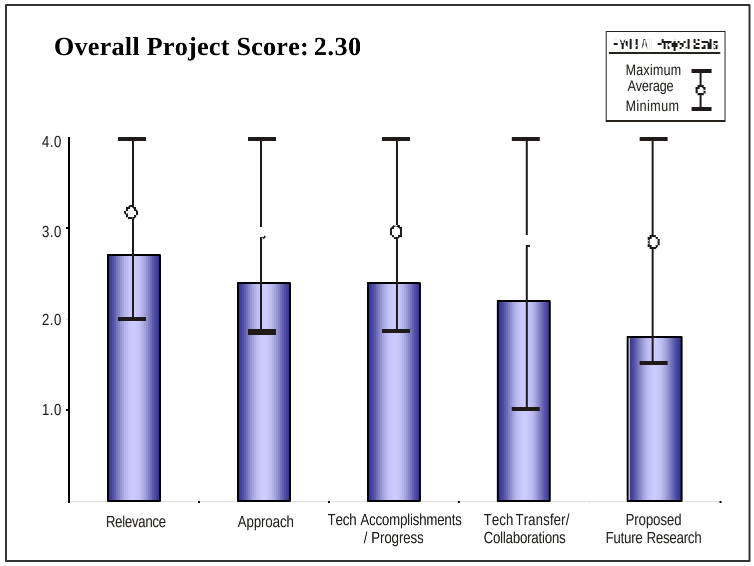
Specific recommendations and additions or deletions to the work scope

- Examine the feasibility of using a modified version of this system for the automotive market.
- Efficiencies greater than 25% need to be demonstrated; otherwise why is this project relevant?
- Cost analysis is needed to demonstrate complete economics.

Project #121: New Solid Sulfide Thio-acid Membranes for High Temperature PEMFCs
Martin, Steve, Iowa State University

Brief Summary of Project

In this project, Iowa State University is investigating a new class of anhydrous proton conducting membranes using sulfide materials. To do this, they are preparing stable, solid thio-acid compounds with proton conductivity ~10^{-2} S/cm at ~200^{o}C, investigating the ionic mobility and thermal stability of all produced thio-acids, and modifying thio-acids to increase conductivity, thermal and chemical stability. This will minimize fuel cross-over, increase sulfur tolerance, and eliminate strict requirements for membrane hydration.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.70 for its relevance to DOE objectives.

- High temperature membrane is very relevant to H_2 goal.
- The development of new membrane materials for PEMs (all types) is critical for the realization of commercial FC-based power systems.
- What is good about this project is that it focuses on one new class of materials.
- This research is very long range and specifically focused on one important element of a potential solution to one problem area of the EERE R & D plan. It is high risk research, with potentially high payoff if successful.

Question 2: Approach to performing the research and development

This project was rated 2.40 on its approach.

- The thio-acid is interesting to look at as a proof-of-concept study. However, it is too narrow-field material chemistry so there are not many options.
- While the project appears overall to be well designed, I wonder if it is focused too narrowly on being a purely ceramic membrane.
- Should the possibility of hybrid organic/inorganic membranes also be considered?
- Is it technically feasible to prepare "thin" films with large surface area and be completely crack-free? What is their definition of thin? Is there an upper thickness limit for best power density?
- Also causing a great deal of concern is the issue of attaching electrodes to fabricate MEAs, and whether or not Pt electrocatalysts will work.
- The membrane conductivity goal appears to be too low and requires a thin, less durable membrane to achieve < 0.1 ohm cm^2 membrane resistance objective.
- The conductivities of the ceramic materials are still 1-2 orders of magnitude from being useful. Instead of just focusing on one material system (ISU-a), it might be more advantageous to screen a wider field of materials in the same family (he said there were many), to see if there is any potential this class of materials can overcome this limit.
- Parallel work to understand the implications for catalyst requirements and membrane formation feasibility.
- These activities are far outside the budget allowances at present.

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

This project was rated 2.40 based on accomplishments.

- The project has done a good job at the feasibility study. However, the results seem to prove this approach has little hope to succeed.
- Too many obstacles including the need for a non-Pt catalyst.
- The progress from materials with very poor conductivity to potentially viable materials has been very good. However, without more data about the "New Anhydrous Proton Conductors" and how they plan to prepare and test 160 more variations in the series quickly, it's hard to evaluate progress.
- Would like to see more discussion of proton-conduction mechanism in these materials.
- Other properties such as oxygen and hydrogen permeability should be measured.
- The level of effort (budget) is very small for such a challenging task.
- Although good materials understanding is being obtained, it has little if any impact on overcoming the barriers.

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

This project was rated 2.20 for technology transfer and collaboration.

- Link to UTC is great.
- The key is to get MEAs into the hands of collaborators who are able to evaluate them in high temperature applications.
- The work is still in early stages, i.e. I don't expect to see an MEA in the short-term, by the end of the year would be an accomplishment.
- It appears some interactions outside their lab are now just beginning, near the end of the contract.

Question 5: Approach to and relevance of proposed future research

This project was rated 1.80 for proposed future work.

- The work so far does not support further work in this material.
- The project needs to focus on fundamental properties and try branching out to other materials.
- Making MEA should not be a priority too early.
- The key barrier that needs to be removed quickly is the compatibility of this membrane with traditional PEM electrocatalysts.
- I do not share the presenters' optimism that non-noble metal catalysts will be available soon.
- Aren't there half-cell experiments that can be done now, prior to working out the thin-film processing, that will indicate whether or not Pt electrodes can be used with this material? If not, then truly viable alternatives to Pt need to be identified now.
- The proton conductivity goal needs to be further raised.
- The current goal requires a very thin (~1 μm) membrane that is not robust mechanically.
- Endurance testing is required to assess durability at high temperatures.
- The program is 2.5 years into a 3 year contract, and so is nearly done.
- It seems unlikely that any further substantial advances in these materials will be made in the next 6 months continuing to focus on just the ISU-a materials.

Strengths and weaknesses

Strengths
- Despite ranking of "2" mostly, nice and pretty important fundamental work.
- New class of high temperature membranes with reasonable conductivity.

Weaknesses
- Crack-free thin films of the new materials at 5 cm^2 and larger?

- MEA fabrication will not be trivial - traditional electrodes have ionomer.
- What will they use and how will they achieve good adhesion?
- Mechanical stability.

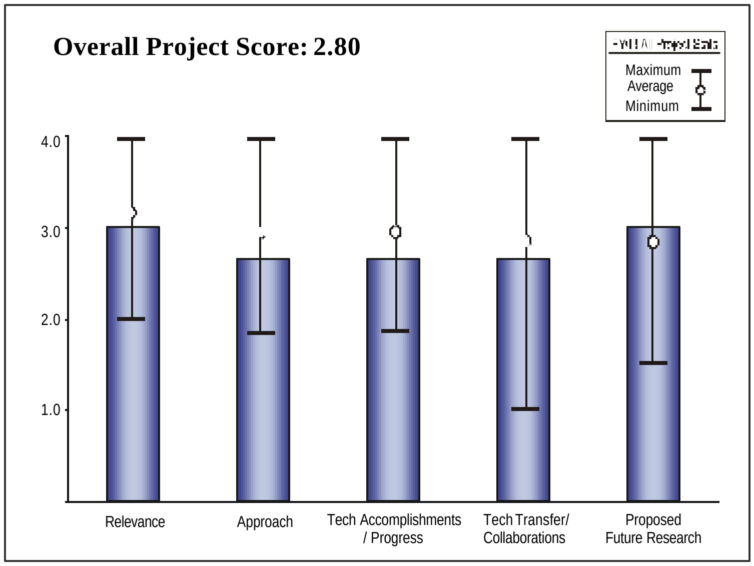
Specific recommendations and additions or deletions to the work scope

- Study the electro-catalytic impact of this thio-acid by doing simple electrochemical measurements.
- Funding should be continued.
- I recommend determining whether or not Pt can be used ASAP. If it can, then full-speed ahead.
- Perhaps some collaboration with a national lab?
- Please provide cost-benefit analysis and compare with PAFC (which operates near the same temperature range as this proposed membrane).
- This kind of fundamental materials work is important to indicate the potential for a new pathway to solving a critical barrier. However, experience indicates that the usefulness of the approach is generally going to reveal itself fairly quickly by showing the limits of the material properties under investigation.
- Also, when a new approach is taken to solve one issue, it's value is compromised if it introduces new problems or exacerbates old ones.
- In this case, I think that the potential for these materials to reach the useful proton conductivity range of at least 0.025 S/cm without water is very slim, and even if it did, there are tremendous issues with finding catalysts, and forming electrodes that will work with these membranes.
- There are equally significant issues with cost effective means for forming membranes with acceptable mechanical properties from these ceramic materials.
- It is unlikely that any company would consider the investments required to scale up these materials without a clear indication that the overall benefit is substantial. The value of this research is then not going to be that it will help eliminate the barriers of the EERE R, D, & D, in even a 20 year time frame (just witness how long Nafion has been around and it only lacks one of the above criteria, i.e. conductivity at high temperature, which is what this ISU material approach is trying to solve).
- Its value for the DOE program may have already been realized by assessing the initial potential for a new materials pathway. The decision then is whether this modest amount of money can now better be spent on some other fundamental research into a new pathway to solve some other DOE program barrier.

Project #122: Effects of Fuel Composition on Fuel Processing
Kopasz, John, ANL

Brief Summary of Project

In this project, ANL identified and evaluated the effects of major constituents, additives, and impurities in petroleum-based fuels on reformer performance. The approach adopted was to (i) investigate autothermal reforming of fuels and fuel constituents in microreactor, (ii) rate performance based on byproduct formation, catalyst deactivation, and hydrogen yield and conversion, (iii) test blends of fuel components and compare results with those for pure components and (iv) use long-term (>1000 h) tests to determine effects on catalyst stability, poisoning and long-term degradation.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Important project to understand complex fuel mixture behavior.
- Results emphasize the complexity of this problem. If we want to understand reforming, we need this type of work.
- Very good project, well aligned with the US goal and the role of national labs.

Question 2: Approach to performing the research and development

This project was rated 2.67 on its approach.

- Is this tied to ATR work at ANL? Believe so but needs to be made clearer how ANL reforming work is integrated.
- Continue work looking at mixtures.
- Need to increase modeling efforts.
- Nice experiment setup and step-by-step approach.
- Encompass a comprehensive list of additives/gasoline components.
- The work seems unfocused, which may be due to too loosely written objectives.
- The presented conclusions are all perfectly trivial---that fuel composition can have a substantial effect on reforming, that different fuel components compete for reactions on catalysts, and that the kinetic rates decrease for more strongly bound species.
- It is very unclear what this work hopes to eventually accomplish that will be of practical use to FreedomCAR objectives.

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

This project was rated 2.67 based on accomplishments.

- Direction is good.
- Need to continue work on bi/tri system - I suggest emphasis on S-content to determine deactivation mechanism…this would be helpful to the ANL ATR effort.
- The only reason for not rating this "outstanding" is the lack of progress in performance enhancement additives. Otherwise good progress!

- It seems as though progress has been made on getting labs set up to do fuels testing, but there is little or no apparent progress towards FreedomCAR goals.

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

This project was rated 2.67 for technology transfer and collaboration.

- Appears to have interactions, but what each entails is unclear.
- The collaboration with oil companies seems unidirectional---the project is more about testing various mixtures from the OCs than providing useful direction to them for developing better mixtures.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- No off-ramps but timeline is reasonable.
- What is missing is future work on timeline, e.g. - modeling.
- Unclear how future directions will provide more positive impact on reaching project objectives.

Strengths and weaknesses

Strengths
- Good systematic investigation of complex fuels.

Weaknesses
- Needs more fundamental work in modeling.
- More university involvement?

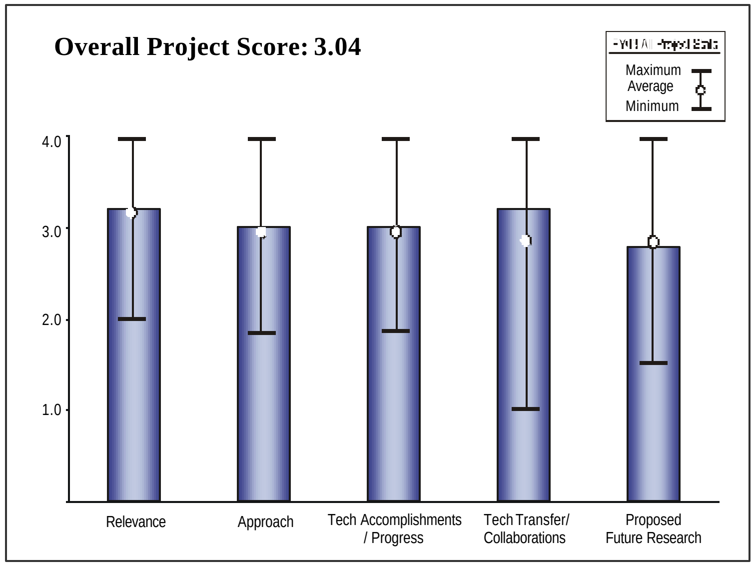
Specific recommendations and additions or deletions to the work scope

- Look at how to develop an integrated reforming effort that clearly shows collaboration of efforts at different labs, e.g. NETL modeling work on diesel components could be applied to ATR work at ANL. Experimentation and modeling appear to be a mixed bag of efforts.
- Also add more university collaboration, e.g. a MURI on reforming.
- De-emphasize conventional gas additives.
- Dramatically increase focus on new performance enhancers.

Project #123: Testing of Fuels in Fuel Cell Reformers
Borup, Rodney, LANL

Brief Summary of Project

Los Alamos National Laboratory is testing fuels in fuel cell reformers by exploring the effects of fuels, fuel components, additives and impurities on performance. More specifically they will examine fuel composition effects on energy efficiency, durability, cold startup, transients, NH_3 concentration, cost and power density, along with the parameters that affect fuel processor and fuel cell lifetime and durability.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.20 for its relevance to DOE objectives.

- Important information on reformer performance parameters beyond just efficiency. Important for reliability and durability.
- Properties of fuel for efficient, durable reforming is important.
- The technical objectives align reasonably well with programmatic goals, if the key uncertainties of reformers are start-up time and durability.
- Fuel variability is a key issue in fuel processing.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Looking at lots of variables to determine best operating parameters, particularly those important for start-up and reliability/durability.
- Data may help in fuels formulation for future fuels.
- Approach to fuels testing could be better described with more concise detail.
- I am not convinced that the approach taken really focused on the expressed uncertainties.
- I appreciate the fundamental characterization of start-up reformers but need to see how this was tied directly to increased reformer durability. When a single fuel, S/C injection method is chosen, what is the reformer durability and what is the failure mechansim?
- The approach could be revised - "Measure Carbon Formation" is mentioned twice.
- A kinetic/reactor model should be developed to quantify fuel effects and catalyst deactivation.
- Good approach to complex problem.

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

This project was rated 3.00 based on accomplishments.

- Excellent progress.
- Lots of good, valuable data. Data provides very useful info on reformer performance.
- Directly transferable to additive effects.
- Very useful information was gained about the contribution that aromatics and long chains make to light off, coking and kinetics.

- The volume of experimental work is impressive; yet, the lack of mathematical model(s) make interpretation of the results difficult.
- Progress towards overcoming barriers is unclear.
- Good progress since last presentation; addressed previously raised concerns satisfactorily.

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

This project was rated 3.20 for technology transfer and collaboration.

- Good interaction.
- Increase university collaborations.
- Good interactions with Delphi, Phillips Petroleum and catalyst makers.
- There was a strong list of collaborators indicated, however, the integration into the project was not established to me.
- Good coordination exists.
- Customers should be included early in planning/testing.
- Very good interactions with technology developers and oil industry.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.80 for proposed future work.

- Good timeline/project plan.
- Introduction of drive cycle dynamics and start-up for next year is a plus as well as work on strategies to reduce carbon formation and regeneration of catalysts.
- I especially like the proposal of operating the system in a duty cycle operating mode. Although this sounds challenging, I have not seen any durability data yet.
- Future work should be reformulated aiming to the design of fuel processors able to handle the composition variability found in current infrastructure fuels.
- On good track to meet program objectives.

Strengths and weaknesses

Strengths
- Lots of good, relevant data that provides empirical insight into overall reformer performance start-up and operation.
- Good insights gained of fuel composition characteristics to prevent reformer coking and to achieve quick light-off.
- Extensive experimental work.

Weaknesses
- Need more fundamental work.
- Need better description of experimental approach that was used to realize results.
- Work plan needs to focus on how to handle fuel variability.

Specific recommendations and additions or deletions to the work scope

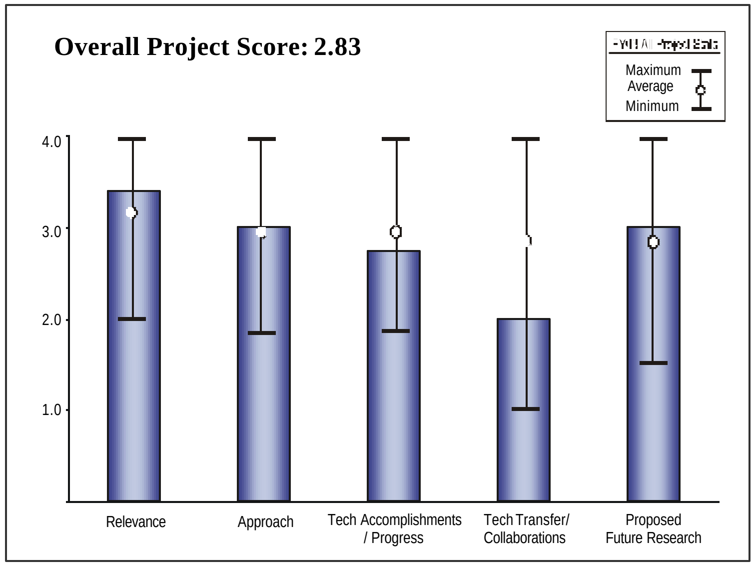
- Would like to see lab efforts integrated with modeling efforts at university… cost-effective way to get fundamental information.
- May want to discuss this w/ DoD.
- Further work in this area.
- The durability objective of this project is very important and I hope it will be actively addressed.
- During the presentation, what is preventing this team from operating durability testing was not indicated - the failure mode for reformers would be very valuable knowledge.

- When a reformer fails, how is this failure observed (or sensed) by the fuel cell? Even if reformer and therefore stack life is short - this diagnostic data is very important.
- Develop mathematical models to quantify catalyst deactivation, carbon formation, light off temperatures.
- Publish in journals.

Project #124: Carbon Monoxide Sensors for Reformate-Powered FCs
Garzon, Fernando and Mukundan, Rangachary, LANL

Brief Summary of Project

Los Alamos is developing sensors for CO level monitoring and feedback control in reformate gas-powered fuel cell systems. One set of sensors will operate in temperatures > $200^{o}C$ and will measure CO concentrations of 0.1 to 2% in reformate gas for PrOx reactor control. The system will be based on differential inhibition of the hydrogen oxidation reaction at an oxide/metal interface. The second set of sensors will be a low temperature amperometric device and will measure 10-100 ppm CO concentrations for stack poisoning control. This system will be based on CO inhibition of hydrogen oxidation kinetics.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.40 for its relevance to DOE objectives.

- Industry teams focusing on same topic.
- This is a good project with high relevance to H_2 economy.
- Such sensors are clearly needed for current reformer systems.
- Talk was well-presented and methodical.
- Good comments re: merits and demerits of the different systems (some materials reversible; others not.)

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Do not have sense that project has clear understanding of all the sensor requirements.
- Focus on fundamental aspects of technology - let industry develop product.
- Need to consider interferences and degradation processes at front end.
- Low T sensor should explore alternatives or more options.
- Novel ideas that do not require precious metals and reduce response time sensitivity would be valuable.
- Right now, it's basically one approach with electrode optimization.
- Approach clear and well organized.
- Solid and sound - PEM systems for low temperature; oxides for high temperature.
- Need more rationale for choices for experimentation (i.e. should research break "out of the box"?).
- The approach is targeted at known systems - PIs need to "stretch".
- Honest conclusion re: high temperature CO sensors.

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

This project was rated 2.75 based on accomplishments.

- Issues: High temperature stability of electrodes and poisoning. Low temperature sensitivity to sulfur compounds.
- Execution seems to be very good and the project has made good progress in establishing the concept's validity.

- Hope to see impurity tolerance and improved low T sensor performance.
- Results not unexpected; but experiments were well-conducted and conclusions are reasonable.
- Need to move from challenges and results into more innovative suggestions for new programs.
- "What next" is critical.

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

This project was rated 2.00 for technology transfer and collaboration.

- Not apparent.
- May be too early to do more.
- Didn't have potential commercial partners identified, collaborations within LANL good - would benefit from technical interaction with other universities in order to avoid group-think.
- Need to reach out to other collaborations in light of results and conclusions.
- Which other groups/companies can help out?
- No need to stay within academic and national labs group.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- Rapid dismissal of points brought up during discussion concerns me - it's important to keep an open mind and really listen to other technical points.
- Need sense of urgency in reaching out to other parties (outside of traditional co-investigators) in light of problems/challenges encountered.
- Need to understand whether "structural" modifications can improve performance, sensitivity and reversibility.

<u>Strengths and weaknesses</u>

Strengths
- Excellent presentation and methodical experimentation.
- Sound approach - difficult task.
- Keep the process simple (ex. CO poisoning).

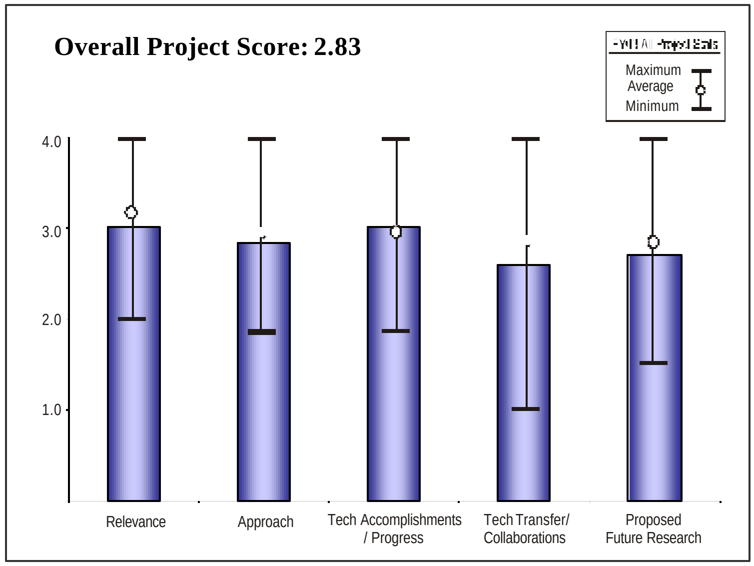
Weaknesses
- Understand overall sensor requirements?
- How does this work/approach compare with other approaches?
- Need more stretch to establish baselines, because when impurities such as sulfur compounds, etc…, are introduced, the problems get tougher!
- Need industrial partnership to assist in manufacturability.

<u>Specific recommendations and additions or deletions to the work scope</u>

- Status of commercially available CO sensors?
- Stability of metal electrodes at high temperatures?
- Define how much power these sensors require for operation.
- Involve more (and other non-traditional) parties who have a greater sense of urgency.
- Try new analytic procedures/techniques: potential pulsing, etc…, to speed up reversibility.

Project #125: Electrochemical Sensors for PEMFC Vehicles
Martin, Peter, LLNL

Brief Summary of Project



Lawrence Livermore is developing solid-state electrochemical sensors for safety and fuel monitoring applications. The safety sensor will utilize new electrode materials and well-known oxygen conducting ceramics. The fuel sensor will utilize novel proton conducting ceramics in a traditional sensor concept.

Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Industry teams focusing on same topic.
- Project is well focused on DOE needs and requirements for sensor development.
- Very relevant to DOE goals for safety and fuel sensors.
- Although the objectives are stated, the direct correlation seemed uncertain, particularly with respect to the response rate and ranges.
- Are the safety and fuel requirements really different? Without performance how can cost and size be addressed?
- H_2 safety sensor development is critical for fuel cell vehicles. Currently, the H_2 fuel sensor is for on-board fuel processing only and it is not focused for vehicle application. For H_2 safety sensor for vehicles, the electrical power consumption is critical. This target should be identified.

Question 2: Approach to performing the research and development

This project was rated 2.83 on its approach.

- Do not have sense that project has clear understanding of all the sensor requirements.
- Focus on fundamental aspects of technology - let industry develop product.
- Need to consider interferences and degradation processes at front end.
- Automotive oxygen sensors use electronic ceramics sensors so general approach is a good one. Challenge is to modify for H_2 detection.
- Approach is well thought out and methodical.
- The PIs appear to be keeping the practical application of these sensors in mind.
- The operating temperature for the safety sensor is too high.
- Technology being investigated seems to be rediscovering the problems associated with this technology that were known from prior work. Where is the innovation?
- What is being done to address the issues known to limit performance? Performance before size and cost considerations.
- As long as oxygen-conducting ceramics are being used, the sensor requires a heater element to keep the electrolyte material high temperature. To reduce the electrical power consumption, it is necessary to use an alternative approach or design to reduce thermal mass to reduce heater power.

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

This project was rated 3.00 based on accomplishments.

- High temperature sensor for ambient environment sensor.

- Reaction with air contaminants, S, organic compounds may lead to unstable performance.
- Detectabilities and response times are promising. CO_2 interferences could be significant.
- The project has shown adequate progress. I think H_2 sensors must be a pretty significant challenge given how light H_2 is.
- Excellent progress on safety sensor.
- Lowering the temperature of operation and power consumption are necessary and these don't seem to have been improved.
- Test conditions, such as flow variables, have not been addressed.
- Sensor stability needs significant improvement but is this the correct technology for the application? What about interferents?

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

This project was rated 2.60 for technology transfer and collaboration.

- More interaction or feedback from industrial electrochemical sensors manufacturers.
- Some contacts (Ford, e.g.) have been made but project could benefit greatly from greater contact with sensor makers or FC makers/ integrators.
- Collaboration with Ford looks good.
- Need to start working with a commercial partner to identify real-world issues.
- Need more external interest in testing sensor.
- Technology development needs to be assessed by a sensor business firm to show relevance and the application requirements need to be reviewed with fuel cell and vehicle developers.
- Good collaborative work with industry to develop heater element.
- Need to get a requirement of electrical power consumption for vehicle application from OEMs. FreedomCAR Fuel Cell Tech Team could be helpful.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.70 for proposed future work.

- Agree with project conclusion that initial results are promising but there's a long way to go… Project needs input from sensor makers or auto companies.
- Recommend a more practical approach for testing the H_2 safety sensors.
- PI needs a better idea of where the sensors are going to be in the final system.
- Future research for the fuel sensor is a bit vague, however it is early in this project.
- Needs to determine if the investigations planned will provide the basic improvements necessary for this technology to work.
- For the H_2 Safety Sensor, recommend pursuing the competitive analysis with industry H_2 sensors such as RIKEN.
- Electrical power consumption reduction should be focused for vehicle application of hydrogen safety sensors, if this project is continued.
- Consider alternative approach or design to reduce heat mass to reduce electrical heater power for hydrogen safety sensor.

Strengths and weaknesses

Strengths
- Promising approach to H_2 sensors with good initial results.

Weaknesses
- High power demand for heat sensor.
- Sensor must run continuously - need lower power drain.

- Need to consider overall sensor requirements. Does this technical approach meet all requirements? Should consider at start of the program.
- With this good base, projects need to establish ties with FC makers or auto companies.

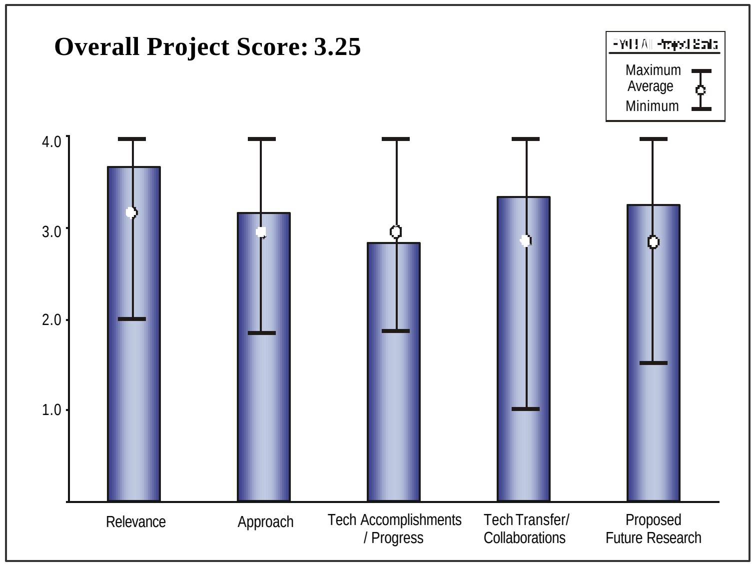
Specific recommendations and additions or deletions to the work scope

- Did not understand comment on proprietary material and that they could not discuss composition - this is a national laboratory program.
- There needs to be a roadmap developed to determine the viability of this technology - determine what needs to be done to identify the technical problems, how much effort is needed and where it is a cost effective venture.
- H_2 Safety Sensor: Recommend pursuing the competitive analysis with industry H_2 sensors such as RIKEN. Electrical power consumption reduction should be focused for vehicle application, if this project is continued. Consider alternative approach or design to reduce heat mass to reduce electrical heater power.
- For automotive applications, a fuel sensor won't be necessary until on-board reformer technology can be feasible. Recommend discontinuing.

Project #127: Development of Sensors for Automotive PEM-Based Fuel Cells
Knight, Brian, UTCFC

Brief Summary of Project

UTC Fuel Cells is currently working to develop automotive PEM-based fuel cell sensors that will be able to detect CO, H_2, O_2, H_2S, NH_3, flow, temperature, pressure, and relative humidity. UTC will investigate/utilize the following technologies to achieve DOE sensor targets: polymer capacitive and MEMS strain gauges, ultrasonic vortex shedding, turbine meters, thermal dissipation, differential pressure flow sensing, thermocouples, thermistors and RTDs.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.67 for its relevance to DOE objectives.

- The program is comprehensive in addressing the sensor goals for the H_2 program.
- Target and barriers were discussed in presentation.
- A very broad program addressing the majority of the DOE sensor goals.

Question 2: Approach to performing the research and development

This project was rated 3.17 on its approach.

- A multi-sensor approach with physical, electrochemical, and MEMs type is key to addressing all the issues and this approach is being followed.
- Focus on sensitivity to interfering substances is key to success of the sensor suite and this needs to be spelt out more clearly in the tasks and downselection process.
- Good test facilities at UTRC are a key part of the sensor evaluation.
- Milestones are well defined and laid out.
- The program is well-designed, but market barriers could be better developed.
- Difficult to assess technical approach from this broad program review - very few technical details in talk.
- Good integration of team members, however there appears to be redundancy between UTRC and IIT test facilities.

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

This project was rated 2.83 based on accomplishments.

- The program is in the initial stages and results are sparse. Therefore, it is not clear as to how far the technical barriers have been addressed. The program approach however seems to be geared to addressing these issues.
- Good progress according to time line.
- Also difficult to assess progress from broad overview of talk. The most detail was given in the description of UTRC test facility.
- Accomplishment in this area is good - excellent system design.

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

This project was rated 3.33 for technology transfer and collaboration.

- It appears that a good team is in place.
- The potential ATMI role could possibly move to tech transfer and manufacturing.
- A clear manufacturer has not been identified in the program.
- Good teaming approach.
- Excellent integration of sensor developers and testing of sensors.
- Good mix of university, small companies, and a large fuel cell manufacturer.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.25 for proposed future work.

- The program is in its initial phases and it appears that the program is well-poised to proceed with the plans.
- Timeframe is adequate, should provide input for '05 decision (Go/No-go).

Strengths and weaknesses

Strengths
- Good technical team and strong leadership from UTC would be a great strength.
- DOE lab involvement could provide some support in addressing technical issues down the line in the program.
- Strength is comprehensive approach.

Weaknesses
- Need to look at other sensor work as well.

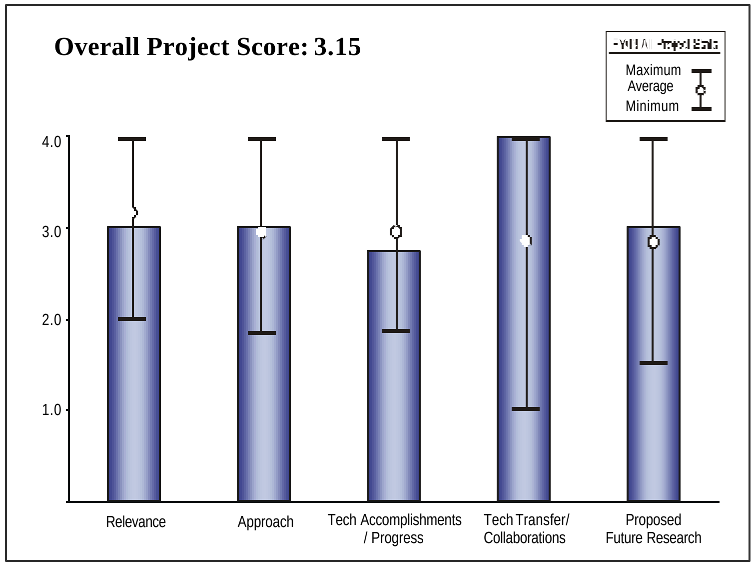
Specific recommendations and additions or deletions to the work scope

- None at this time.
- Need to integrate sensors with control subsystem and vehicle systems.

Project #129: Sensor Development for PEMFC Systems
Figi, Bruce, Honeywell Sensing & Controls

Brief Summary of Project

In this project, Honeywell is developing sensors for PEMFC systems. Honeywell will begin this process by conducting a broad market survey to obtain input on customer requirements for the design task. They will then begin sensor development, paying particular attention to relative humidity sensing - stability over application life; ability to provide feedback in two phase flow – and flow sensing – providing a stable, accurate output in non-condensing and condensing environments. A prototype will be developed and then field tested.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Hard to understand the types of sensors being considered - even the "given".
- An integrated sensor package is important.
- As sensor work in general, this project is highly relevant to the program objectives.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- Approach is customer driven and therefore attractive.
- Unclear as to where the approach is headed - no example of "sensor package."
- Data-driven approach is promising, however at this early stage it is impossible to say whether or not it is going to work in the end.

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

This project was rated 2.75 based on accomplishments.

- Very early on in project.
- Interviews completed.
- Too soon to judge strictly technical progress towards goals, but good effort overall in the attempt to define sensor requirement.

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

This project was rated 4.00 for technology transfer and collaboration.

- Good spectrum of companies.
- Very good interaction with potential customers at this very early stage.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- On track.
- Good and logical plan for future work.

Strengths and weaknesses

Strengths
- Integrated sensor package important concept.

Weaknesses
- Unclear as to what prototypical package would look like.
- This has been a strategy development effort so far rather than R & D.

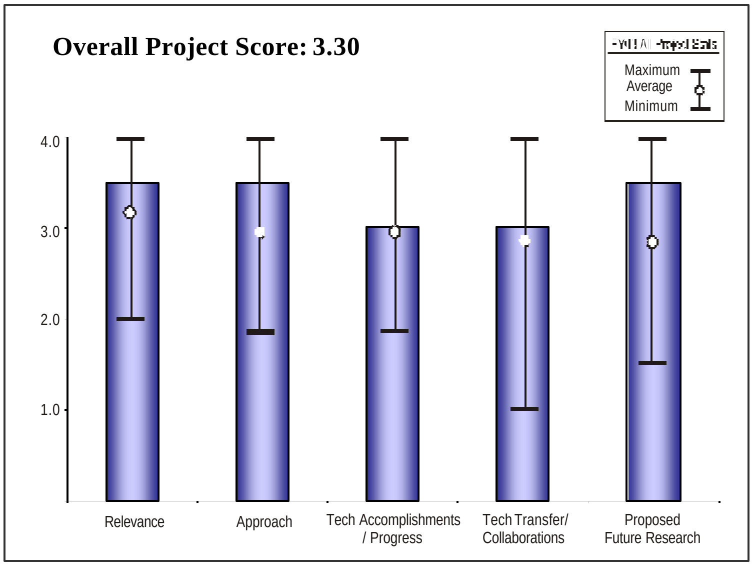
Specific recommendations and additions or deletions to the work scope

- Need to have "strawman" systems.

Project #130: Fuel Cell Turbocompressor
Gee, Mark K., Honeywell

Brief Summary of Project

Honeywell is developing an optimum turbocompressor configuration for integration into a PEMFC that reduces costs while increasing design flexibility. Honeywell is utilizing their expertise in automotive and aerospace turbomachinery 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 3.50 for its relevance to DOE objectives.

- Smallest and most advanced C/E unit with M/C developed. Can be run with or without expander on hydrogen or reformate.
- Note that your sponsor is DOE - your customer is still car companies. (Make sure you meet their needs).

Question 2: Approach to performing the research and development

This project was rated 3.50 on its approach.

- Objective is to make unit even smaller and M/C even smaller.
- Cost reduction is a major goal.
- Looked at several options and equipment configurations.
- Started with an existing product rather than from scratch.

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

This project was rated 3.00 based on accomplishments.

- Honeywell needs to support project more aggressively.
- Met most of performance objectives.
- Clearly identified areas for improvement.
- PI/company appears to be knowledgeable on technical issues.

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

This project was rated 3.00 for technology transfer and collaboration.

- GM has/is testing the device.
- Would like to see some car companies involved. Don't want to rework to meet their needs.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.50 for proposed future work.

- Good future plan to further improve machine and lots of discussion with OEMs.
- Doesn't appear to be a real aggressive program.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- None specified.

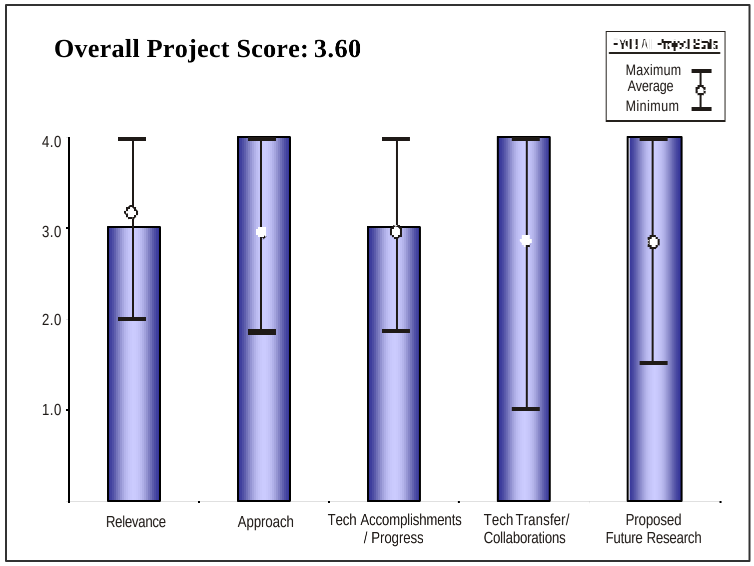
Specific recommendations and additions or deletions to the work scope

- None specified.

Project #131: Development of a Torroidal Intersecting Vane Machine Air Management System for Automotive Fuel Cell Systems
Bailey, Sterling, Mechanology, LLC

Brief Summary of Project

Mechanology is developing and testing a high-efficiency, integrated compressor/expander based on torroidal intersecting vane machine (TIVM) geometry. Mechanology will select and demonstrate design features to assure adequate sealing, minimum porting pressure loss, and low friction operation and then measure the performance of the TIVM compressor/expander across the operating range.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Unique design - beginning to show that they can achieve low vane friction and brush sealing to limit air leakage.
- Unit is small and lightweight. Can be run with or without expander on hydrogen or reformate.

Question 2: Approach to performing the research and development

This project was rated 4.00 on its approach.

- Working with many people to obtain necessary targets.
- Have excellent response to peer review objectives.

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

This project was rated 3.00 based on accomplishments.

- Made significant progress toward peer review comments.
- Researched friction, improved sealing, and low power consumption.

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

This project was rated 4.00 for technology transfer and collaboration.

- Have close coordination with OEMs and other suppliers.

Question 5: Approach to and relevance of proposed future research

This project was rated 4.00 for proposed future work.

- Very focused on peer review comments and doing good to excellent job to overcome difficulties.

Strengths and weaknesses

Strengths

- None specified.

Weaknesses

- None specified.

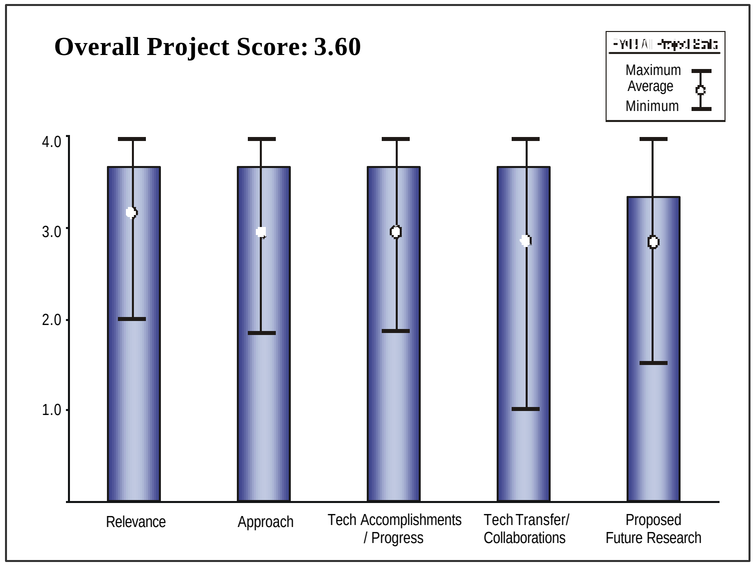
Specific recommendations and additions or deletions to the work scope

- None specified.

Project #132: PEM Fuel Cell Air Blowers
Clark, Tom, UTCFC

Brief Summary of Project

UTC Fuel Cells is developing a small, lightweight, motor blower technology to provide cathode air and fuel processor air for a near ambient pressure fuel cell operating on gasoline. As part of this project, UTC will demonstrate the performance of various types of air blowers, evaluate both regenerative and centrifugal fuel processor air blower approaches, and develop manufacturing methods that will allow the blower to be produced at low cost, in large volumes.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.67 for its relevance to DOE objectives.

- The blower work appears to address the weight and efficiency goals for the program.
- Only ambient automotive FC systems to-date: however, UTC is developing blowers for themselves. The hope is that suppliers will sell blowers to other companies that develop ambient FC systems for stationary and/or automotive systems.

Question 2: Approach to performing the research and development

This project was rated 3.67 on its approach.

- Key barriers cited are being addressed.
- UTC wanted scalable designs to mount to hardware directly. PADT CAB originally overheated and had to be redesigned. Motor was to be designed for 75 kW_e, now it is designed for 50 kW_e. Good plan for M/C (motor/controller) though and now they understand overheating and could scale-up.
- R & D dynamics selected for RPS blower; heavier than anticipated by 30 lbs which means it is more costly - 140K RPM at 75 kW_e probably not going to be utilized as new unit good for 50 kW_e.
- Have met technical specs for cathode blower - close on cost target. FPS - PADT regenerative properly ruled out.
- R&D dynamics design; looks like can make technical specs.
- Cost is high.
- Scalable design appropriate.

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

This project was rated 3.67 based on accomplishments.

- Efficiency and weight goals are being met.
- Durability issues need to be addressed for all designs.
- Close to technical goals; a little high on cost but doing well.

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

This project was rated 3.67 for technology transfer and collaboration.

- UTRC and R & D dynamics have the wherewithal to do manufacturing, and costing appears to be attractive.
- Need to get more ambient FC partners involved.
- Working with competent suppliers.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.33 for proposed future work.

- Efficiency goals must be met at the end of the program or in the down selection stages.
- Can take system up to 100 kW_e or so.

Strengths and weaknesses

Strengths
- Good competent team including suppliers.

Weaknesses
- Noise not yet addressed.

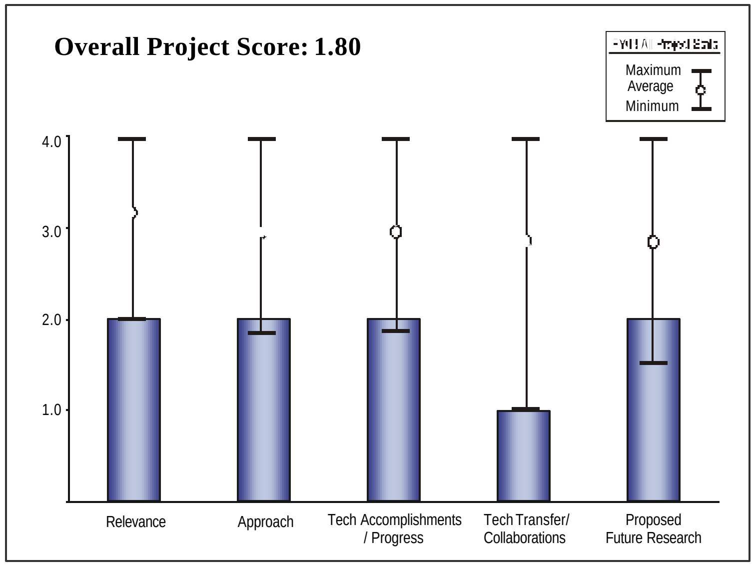
Specific recommendations and additions or deletions to the work scope

- Need to get more ambient pressure FC partners involved.

Project #133: DOE Compressor/Expander Module Development Program
Selecman, George, TIAX

Brief Summary of Project

In this project, TIAX will design and build a hybrid compressor/expander module using both turbomachinery and scroll compression technology. This will involve developing the algorithms and hardware to ensure stable and effective control of the hybrid system. TIAX will later deliver a system with equivalent thermodynamic performance, at significantly lower weight and volume compared to previous scroll compressor/expander modules.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.00 for its relevance to DOE objectives.

- Novel design but not sure it is workable.
- T/C scale down from suppliers' large unit in doubt.
- Control is questionable.
- Size and weight are questionable.
- Noise?
- Start-up power?
- Poor turn-down - more like positive displacement rather than letting pressure float with load.

Question 2: Approach to performing the research and development

This project was rated 2.00 on its approach.

- Don’t like bypass from expander to scroll inlet.
- Don’t think compressor/expander machine can easily be built and expander needs all the power it can get to drive compressor to its lower scroll power.
- Combination of scroll and T/C (turbocompressor) is a doubtful approach.

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

This project was rated 2.00 based on accomplishments.

- Hard to judge; no hardware, just layout design and some modeling.
- Many unknowns.
- Doubtful if concept works.
- Don’t like addition of intercooler. Still have to have one after scroll before cathode inlet.

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

This project was rated 1.00 for technology transfer and collaboration.

- Very little or no outside interactions of which I am aware.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.00 for proposed future work.

- Need better modeling.
- Need some hardware in quick time to see if concept even works.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- None specified.

Specific recommendations and additions or deletions to the work scope

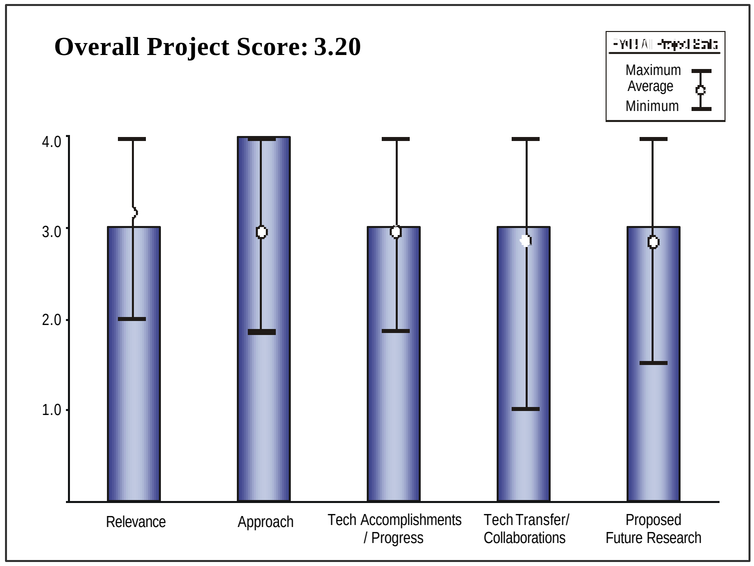
- Need more outside interaction.
- Don’t think this project is worth the money DOE is spending.

This category includes projects that provide technical validation of hydrogen and fuel cell technologies for transportation, infrastructure, and electric generation in a systems context under real-world operating conditions. The aim of these efforts is to achieve the following milestones established in the Hydrogen, Fuel Cells and Infrastructure Technologies Program Multi-Year Research, Development and Demonstration Plan:

Technology Validation

- Validate an integrated biomass/wind or geothermal electrolyzer-to-hydrogen system for \$3.30/kg at plant gate by 2010.

Project #19: Technical Analysis: Integrating a H_2 Energy Station Into a Federal Building
Unnasch, Stefan, TIAX



Brief Summary of Project

TIAX will conduct an analysis on integrating a hydrogen energy station in a federal building. This project will involve an analysis of potential 50 kW PEMFCs that are suitable for installation in federal buildings, an assessment of options for system components, and the cost and efficiency of different system configurations. It will also seek to assess the potential for heat recovery from a FC/H_2 production system, cogeneration application possibilities, and potential fleets that could use hydrogen for vehicular operations.

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

This project earned a score of 3.00 for its relevance to DOE objectives.

- Provides focus on distributed infrastructure development.

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

This project was rated 4.00 on its approach.

- Excellent knowledge of systems and their cost.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- Provided good information on many varieties of distinct infrastructure.

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

This project was rated 3.00 for technology transfer and collaboration.

- Coordination with industry for cost data noted.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- A defined endpoint not shown.

<u>Strengths and weaknesses</u>

Strengths
- Knowledge of equipment and its cost is prevalent.
- Survey of potential users shown.

Weaknesses

- Does not explore all potential scenarios. Should look at scenarios where the equipment grows and evolves over time to match demands for H_2 and electricity.

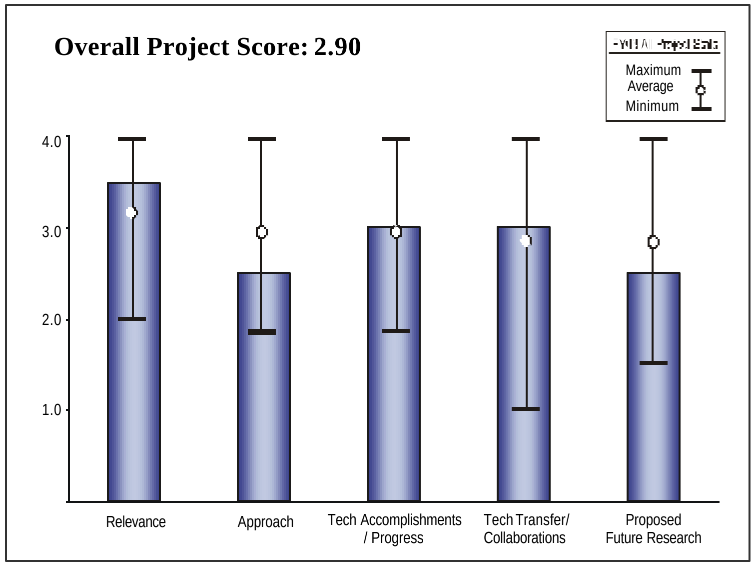
<u>Specific recommendations and additions or deletions to the work scope</u>

- Recommended addition of an identification of the highest value and near term scenarios where FCs and H_2 production for vehicles would first make sense. Add scenarios that can grow and evolve over time to match technology to applications that are closest to commercialization.

Project #21: Validation of an Integrated System for a H_2-Fueled Power Park
Carlson, Todd, Air Products and Chemicals Inc.

Brief Summary of Project

In this project, Air Products, Inc. studied the economic feasibility of producing power with a PEM Fuel cell from natural gas. Simulations were conducted to evaluate the efficiency and waste heat availability. Cost of operation models were developed to analyze the capital, operational and maintenance cost associated with it.



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

This project earned a score of 3.50 for its relevance to DOE objectives.

- Hard to follow the poster.
- Questions needed to be elaborated to justify conclusions.
- Good similar findings.
- This program uses a variety of projects and scenarios to fit existing conditions. It combines several components to allow hydrogen to be integrated as the fuel of choice into many commercial and domestic applications.

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

This project was rated 2.50 on its approach.

- Needs to define their approach better.
- Could not get an explanation from posters.
- Much of the program is based upon Air Products projects.
- The partnerships involved are to supply the necessary components or demonstration sites.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

- Project looks good. Well outlined.
- Automotive fuel cell development is not a significant part of this program.

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

This project was rated 3.00 for technology transfer and collaboration.

- Not enough detail provided.
- The largest part of this program is a direct collaboration with Penn State University and to be developed and implemented on campus.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- Future research is not proposed.
- This program did not include the development of automotive fuel cells.

Strengths and weaknesses

Strengths
- This program integrates a variety of hydrogen activities into the campus of a major university.

Weaknesses
- The utilization of this program into the educational curriculum could have been more thoroughly described.

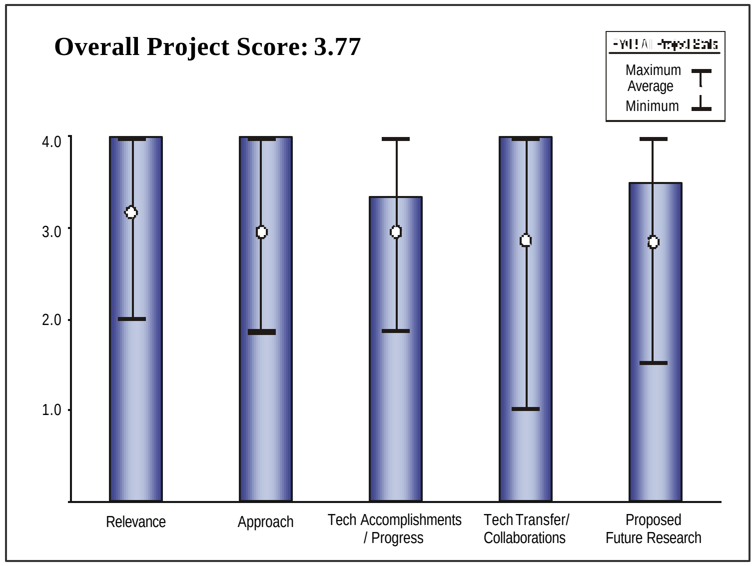
Specific recommendations and additions or deletions to the work scope

- A discussion of the integration of this program into the Penn State Engineering curricula is recommended to be included in detail.

Project #23: Hawaii Hydrogen Power Park
Rocheleau, Rick, University of Hawaii

Brief Summary of Project

In this project, the state of Hawaii along with the University of Hawaii worked on demonstrating an integrated Hydrogen Power Park comprised of an electrolyzer powered by renewable sources, hydrogen storage and distribution system, a PEM fuel cell and a hydrogen dispensing system for vehicles. Technical barriers as well as the economics for this project were analyzed along with gathering general public interest and support.



Question 1: Relevance to overall DOE objectives

This project earned a score of 4.00 for its relevance to DOE objectives.

- Shows breadth and depth of relevance through multiple aspects of energy usage. Good use of multiple technologies, renewables, and fossil-based.
- This program provides a multi-faceted demonstration site that links power, hydrogen generation and fueling.

Question 2: Approach to performing the research and development

This project was rated 4.00 on its approach.

- They addressed the permitting issues using lessons learned from other DOE projects.
- Good integration, good progress (ahead of schedule) and good partnering.
- This program brings several renewable energy sources into the hydrogen/power generation components.

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

This project was rated 3.33 based on accomplishments.

- This is their first year and they are off to a great start.
- UTC FC is at 5 kW. They need plan to increase it to 75 kW.
- This project does not focus on automotive fuel cells.

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

This project was rated 4.00 for technology transfer and collaboration.

- Multiple collaborating partners.
- Good partnering.
- The project team has viable partners from government, industry, universities, and transportation.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.50 for proposed future work.

- The project can be ended at a variety of points across technologies. Very flexible.

- Good.
- Future work plans include completion of program installation and testing.
-

Strengths and weaknesses

Strengths

- The program demonstrates a variety of renewable energy sources and several components on hydrogen and power generation.

Weaknesses

- The plan for future activities beyond the original program are not fully developed yet.

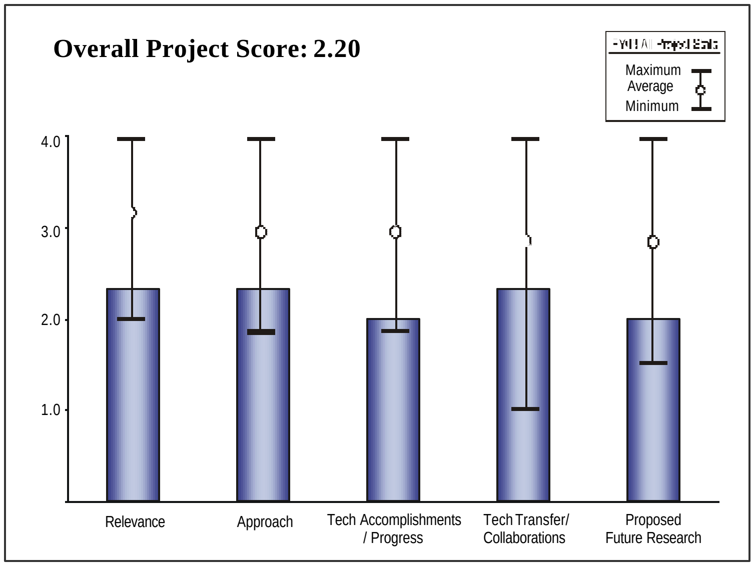
Specific recommendations and additions or deletions to the work scope

- I recommend the addition of a section that proposes future enhancements and additions to the program.

Project #24: Power Park
Hobbs, Ray, Pinnacle West

Brief Summary of Project

In this project, Pinnacle West Capital Corporation conducted studies on the following topics (i) economic hydrogen production (ii) renewable energy opportunities (iii) integration of distributed generation and transportation fuel production (iv) incorporation with existing energy assets (v) scalability (vi) integrated business opportunities (vii) identification of technical barriers (viii) identification of market opportunities. Under the economic production of hydrogen, the options looked at were (i) solar reforming of natural gas (ii) low cost electrolysis (iii) hydrogen purity requirements (iv) heat energy recovery and (iv) chemical by-product value.



Question 1: Relevance to overall DOE objectives

This project earned a score of 2.33 for its relevance to DOE objectives.

- More detail and explanation needed on the poster. Story needs to be laid out: why, what, timeline, etc.
- This program presents a plan with components to be addressed but does not present the project parameters and details to show the significance and impact of those components to the program.

Question 2: Approach to performing the research and development

This project was rated 2.33 on its approach.

- Needs more detail. What are the barriers? What are the specifics?
- The project has potential and has apparently achieved several significant milestones.
- The plan is not easily relatable to the components of the plan.

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

This project was rated 2.00 based on accomplishments.

- No accomplishments or progress indicated. Timeline missing and details are needed.
- I feel that the program has achieved much more success than is apparent from the presentation.

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

This project was rated 2.33 for technology transfer and collaboration.

- Good start.
- More effort should be placed on describing the collaborations and partner contributions.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.00 for proposed future work.

- Project has not started.
- Needs more details and not enough information is given.
- Future development/evolution of the program needs to be prominently discussed to see future directions and plans.

Strengths and weaknesses

Strengths
- None specified.

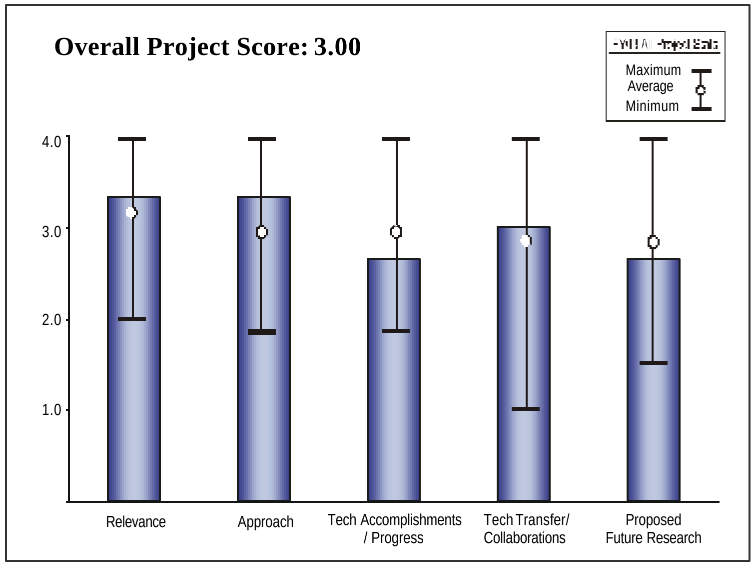
Weaknesses
- The program seems to have been implemented but the presentation lacked detail of the various projects.

Specific recommendations and additions or deletions to the work scope

- The work scope should be more clearly defined and presented in graphic or schematic form.

Project #25: DTE Hydrogen Power Park
Regan, Rob, DTE Energy

Brief Summary of Project



In this project, DTE Energy would develop and test a working prototype of a hydrogen-based energy station concept that utilizes solar & biomass power combined with electrolysis and stationary PEM fuel cell technology to take advantage of low-cost power during off-peak hours to generate hydrogen for on-peak power generation and vehicle fuelling. 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 3.33 for its relevance to DOE objectives.

- Oriented toward infrastructure with mention of vehicles.
- This project has several components that demonstrate fuel cells, hydrogen generation and renewable energy use.
- Excellent poster. Well laid out.

Question 2: Approach to performing the research and development

This project was rated 3.33 on its approach.

- Lists a number of barriers that exist with a community (city, county, state).
- The project is conceptual and as such, does not have details regarding the interface with partners and users as well as DOE.
- Good identification of areas.

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

This project was rated 2.67 based on accomplishments.

- The project has potential but has not been implemented.
- Good timeline.

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

This project was rated 3.00 for technology transfer and collaboration.

- Working mostly with utilities.
- The teams are in place with industry and government. The university ties could be established but the details need to be developed.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- The program is a future work plan with most of the development awaiting implementation.
- Good project.

Strengths and weaknesses

Strengths
- The program has a variety of established partners and a workable concept design.

Weaknesses
- The details of implementation could be more thoroughly developed.

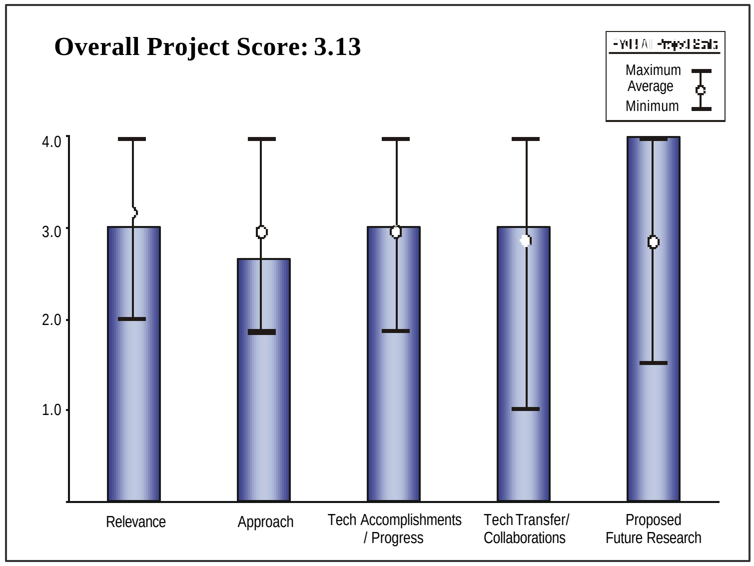
Specific recommendations and additions or deletions to the work scope

- It will be important to include project details and specific descriptions of the program as they are developed and implemented.

Project #26: Filling Up With Hydrogen 2000
Fairlie, Matthew and Scott, Paul, Stuart Energy

Brief Summary of Project

The purpose of this project is to design and build fuel appliances based on new low-cost electrolyzer technology, demonstrate hydrogen vehicle re-fueling using fuel appliance systems, obtain 'third party operating experience feedback' in refueling applications, establish precedents for development of codes and standards, and determine the cost effectiveness of the fuel supply pathway.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- This project is nearing completion. It appears to have provided key operating information for development of alkaline-based electrolysis technology.

Question 2: Approach to performing the research and development

This project was rated 2.67 on its approach.

- Good progress was made in developing electrolysis for near-term distribution applications.
- Somewhat hard to assess since work is essentially completed.
- The presenter was very knowledgeable and helped in explaining the system and progress.

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

This project was rated 3.00 based on accomplishments.

- Appears to have made good progress in meeting their objectives.
- This is hard to assess since I have not followed this area and it is my first exposure. The accomplishments were substantial and impressive.
-

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

This project was rated 3.00 for technology transfer and collaboration.

- Individual project and coordination with other industrial partners appear to have been very good.
- Should consider near-term opportunities other than automotive such as scooters in Asia.
- Not aware of the collaboration other than DOE. Stuart is a private company. There is a collaboration in the works regarding PEM-based electrolysis.

Question 5: Approach to and relevance of proposed future research

This project was rated 4.00 for proposed future work.

- Next step commercialization.

Strengths and weaknesses

Strengths

- Appears to have been a very successful program.
- Good progress and solid systems development.

Weaknesses

- It is not clear that they can meet the cost targets.

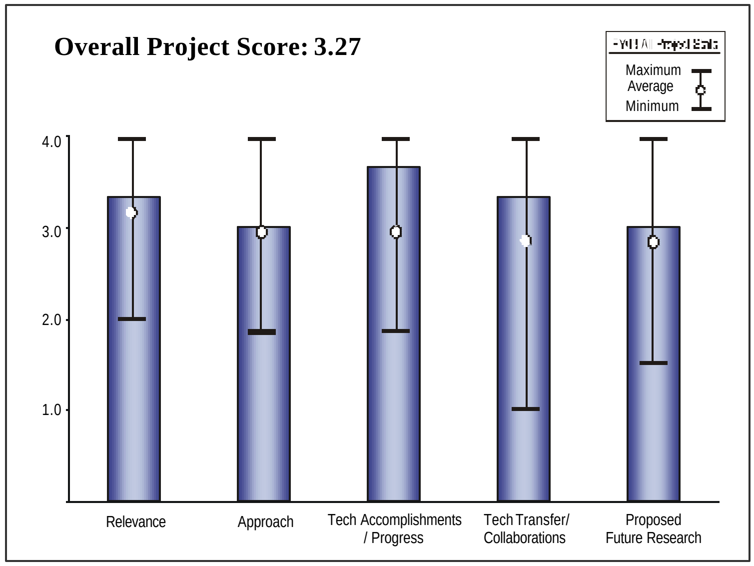
Specific recommendations and additions or deletions to the work scope

- Recommend end DOE R & D and move to commercialization.
- Continue work to develop more efficient and durable cathode and anode catalysts and substrates.

Project #27: Mixtures of H_2 & Natural Gas (HCNG) for Heavy-Duty Applications
Collier, Kirk , Collier Technologies

Brief Summary of Project

Collier Technologies is developing a low-emissions, heavy-duty vehicle engine package to seamlessly repower today's buses and trucks with existing natural gas and diesel engines that will exceed DOE's goal of reducing 1998 emission standards and maintain or enhance vehicle drivability. This will be accomplished through the incorporation of alternative engine designs and the addition of hydrogen to the natural gas fuel mixture.



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

This project earned a score of 3.33 for its relevance to DOE objectives.

- While this project does not directly address fuel cells, it offers a near-term alternative vehicle application for hydrogen.
- Provides a pathway to increase infrastructure and brings hydrogen and blended hydrogen projects into common use faster.
- Allows for faster learning for handling H_2.
- Good approach with good explanation.

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

This project was rated 3.00 on its approach.

- The project provides some test data and a variety of applications for future evaluation and data collection.
- Addressing infrastructure codes and standards brings utilities into picture.
- Good project.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.67 based on accomplishments.

- This project has paved the pathway for hydrogen-fueled internal combustion vehicles.
- Very well done displaying emission data from testing of project.
- Did not identify barriers and issues on posters, but did address them verbally.
- Linked with power park.

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

This project was rated 3.33 for technology transfer and collaboration.

- The coordination between the program and universities could be more fully developed.
- Broad range of participants: city(1), private(2), university(2), utilities(2) one is hydroelectric.
- Good collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- This project reflects the evolution of implementation of the technology.
- Project is focused on ICEs.
- Good start.

Strengths and weaknesses

Strengths
- The project shows impressive results regarding pollutant emissions with the opportunities available to build the database with each new application of this technology at new sites.
- Poster needed more detail, but explanation was good. Good use of photos.

Weaknesses
- None specified.

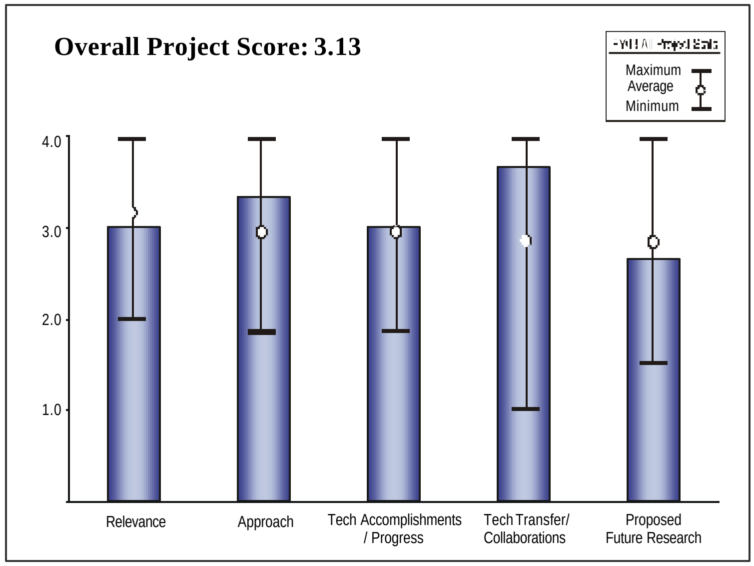
Specific recommendations and additions or deletions to the work scope

- I recommend that some additional schematic or graphic representations be added to the presentation to further clarify and describe the technology and the process involved.
- Also should show linkages to power park program.

Project #33: Hydrogen from Biomass for Urban Transportation
Yeboah, Yaw, Clark Atlanta University

Brief Summary of Project

NREL and its collaborators, interested in 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 technical feasibility of the process, discovered agricultural uses of the carbon product, and identified economical co-product options for the bio-oils.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- Process to convert biomass to a useful fuels is critical. However it is important that the process handle as a broad a range of biomass feeds as possible. This should include not only wastes e.g. peanut hulls and tailored biomasses - switchgrass and other crops.
- Renewable sources of H_2 on a long term goal of HFCIT isogram.
- Process seems limited in its application.
- Feedstock is available in limited number of sites and it is not clear if process will accept different forms of biomass.
- Process seems complicated and needs to make sure an estimate of cost of H_2 provided meets DOE targets.
- The goal of peanut-shell pyrolysis is to meet CO_2 concerns but the pilot scale process required supplemental fuel consumption. It is not clear that the net effect will be a benefit so a CO_2 balance should be performed.
- Economics also seems to depend on the value of the other products produced.

Question 2: Approach to performing the research and development

This project was rated 3.33 on its approach.

- After validating long term performance with peanut hulls, it should be demonstrated on other fuels. After use of 3-4 maximum volume waste/crops then it should be sealed up. Not sure it can afford a special gasifier for each fuel/waste.
- Non-fossil renewable sources of hydrogen are needed in the long term as a part of the HFCIT program.
- The experimental approach seems to be defined appropriately. There are good partnerships in place to pursue the benefits of other products from the process; the fertilizer and adhesives.
- Recommend more system analysis of the process.

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

This project was rated 3.00 based on accomplishments.

- Need to complete the 1000 kW demo successfully.
- This project assumes that fuel processing from oil to gas(H_2) is not a problem - this should be verified so that overall economic analysis is sound.

- Learning curve has identified processing issues that have been resolved.
- Upcoming 1000 -h test should provide a good indication of the strong and weak design points.
- The initial pyrolysis/reformer tests show some interesting data. The next set of tests should provide more data on thermal efficiency of the process over longer operation at larger scale.

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

This project was rated 3.67 for technology transfer and collaboration.

- Has state and industry on board. However unclear whether commercialization entity has financial depth to carry to full production for market.
- University connection to Georgia and the moving of equipment for future tests will strengthen the project.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- Scale up by factor of 10 is next crucial item and should be done once a more general applicability to biomass feeds is established.
- Project seems to be moving forward with modifications being made to reduce technical barriers as they occur.
- Not sure when you declare success, perhaps when development costs exceed the potential benefits.
- Future work plans need to include system analysis for energy, carbon balances and projected economics of the process.

Strengths and weaknesses

Strengths
- None specified.

Weaknesses
- DOE needs to spend more effort on feed preparation. All gasifier developers feel that there are no problems once they have a good feed. However, I am not comfortable that good feeds are easily come by. Peanut hulls, wood waste, capice willow, RDF, SMW, switchgrass all have somewhat different needs for providing a good form feed that is easily handled and fed to a gasifier.
- Needs to define the potential impact of the application of the process to the hydrogen supply base that needs to be developed to support the hydrogen vision.

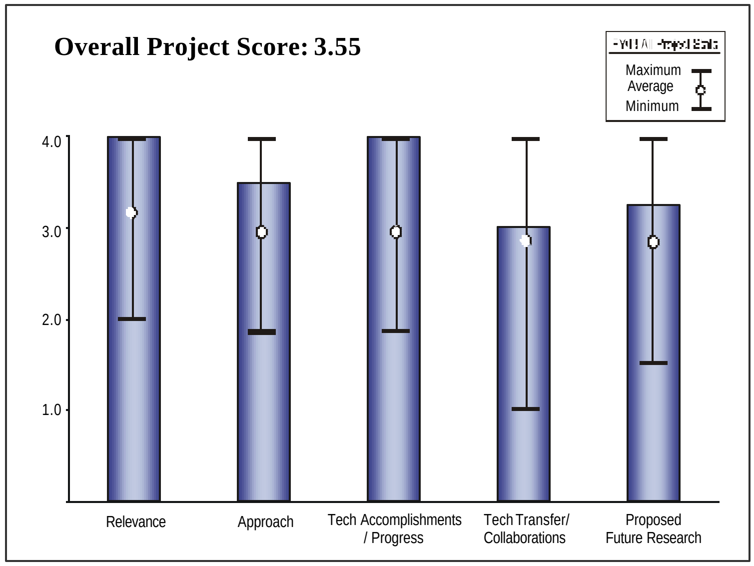
Specific recommendations and additions or deletions to the work scope

- None specified.

Project #36: Power Parks System Simulation
Keller, Jay and Lutz, Andy, SNL

Brief Summary of Project

The objectives of this project are to develop a system model to simulate distributed power generation in power parks, demonstrate the potential of hydrogen technologies for power generation, and analyze the dynamic performance of the system to examine the thermal efficiency, power availability, and cost.



Question 1: Relevance to overall DOE objectives

This project earned a score of 4.00 for its relevance to DOE objectives.

- Excellent explanation of relevance.
- Comprehensive but excludes autos infrastructure issues.

Question 2: Approach to performing the research and development

This project was rated 3.50 on its approach.

- Good approach.
- Addressed efficiency.
- Focused on matching theoretical and actual data.

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

This project was rated 4.00 based on accomplishments.

- Good project. Didn’t see barriers and could use more detail.
- Good suggestion to go back to PV company.
- Actual simulations with electrolysis and PV arrays done.

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

This project was rated 3.00 for technology transfer and collaboration.

- Good connection with 2002 comments.
- Limited to laboratory but expect to improve as data becomes available.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.25 for proposed future work.

- Good identification of proposed work.
- Linkages with other programs evident.

Strengths and weaknesses

Strengths

- None specified.

Weaknesses

- None specified.

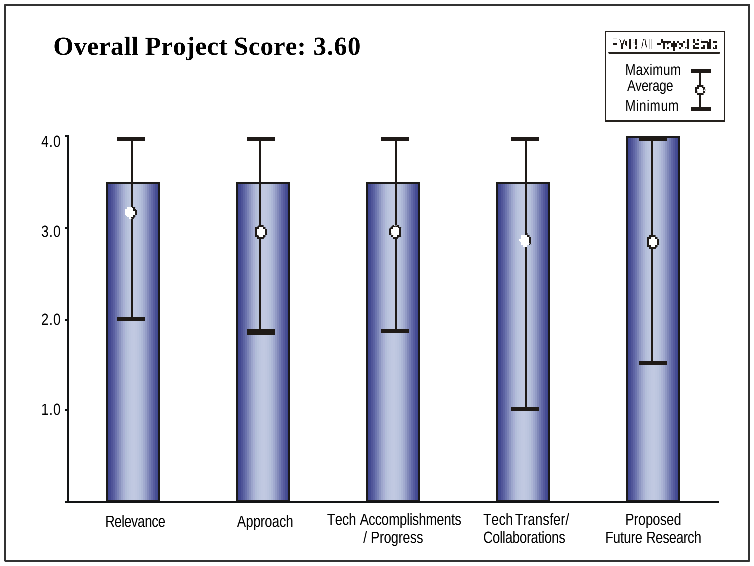
Specific recommendations and additions or deletions to the work scope

- None specified.

Project #37: On-Site Hydrogen Generation & Refueling Station
Davies, Michele, Hyradix/ SunLine

Brief Summary of Project

Hyradix/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, compress and store hydrogen at 5000 psi, and demonstrate the refueling of fuel cell & HCNG buses, street sweepers, and cars.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.50 for its relevance to DOE objectives.

- Good demo of current technology.
- Demonstrates hydrogen refueling station.

Question 2: Approach to performing the research and development

This project was rated 3.50 on its approach.

- The demo will generate good data on current technology.
- Will identify problems of on-site hydrogen filling station.

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

This project was rated 3.50 based on accomplishments.

- The first on-site hydrogen filling station will help develop an understanding of how much maintenance is needed.

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

This project was rated 3.50 for technology transfer and collaboration.

- Uses Universal Oil Products sulfur removal technique.

Question 5: Approach to and relevance of proposed future research

This project was rated 4.00 for proposed future work.

- Long-term use of the refueling station to develop sound economics in the future.

Strengths and weaknesses

Strengths
- Real-world demonstration shown.

Weaknesses

- No specific weaknesses apparent.

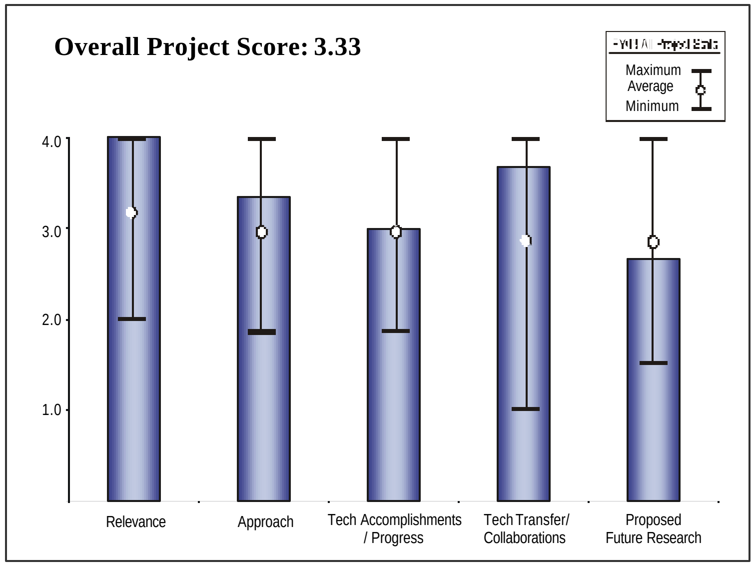
Specific recommendations and additions or deletions to the work scope

- Incorporate new components as they become available.

Project #61: Development of a Natural Gas to H_2 Fueling System
Liss, William, GTI

Brief Summary of Project

GTI is designing a competitive, fast-fill natural gas-to-hydrogen fueling system with 40-60 kg/day delivery capacity that meets DOE cost goals of $2.50/kg of H_2 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 4.00 for its relevance to DOE objectives.

- Good emphasis on reducing footprint size for fueling station.
- "Turn down" capability is important to allow fueling stations to grow.
- Addresses the "real-world" problems of H_2 refueling facilities.

Question 2: Approach to performing the research and development

This project was rated 3.33 on its approach.

- No discussion of installation and use/market.
- Good integrated approach.
- Addresses all facets of refueling facility design.

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

This project was rated 3.00 based on accomplishments.

- Only project looking at thermal management of cycles during filling.

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

This project was rated 3.67 for technology transfer and collaboration.

- Very appropriate industrial partners.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.67 for proposed future work.

- Deployment not addressed.
- Should be clearer about criteria for future experiment variants.

Strengths and weaknesses

Strengths
- Addresses all areas of refueling facility design.

Weaknesses
- Economic analysis could be more robust.

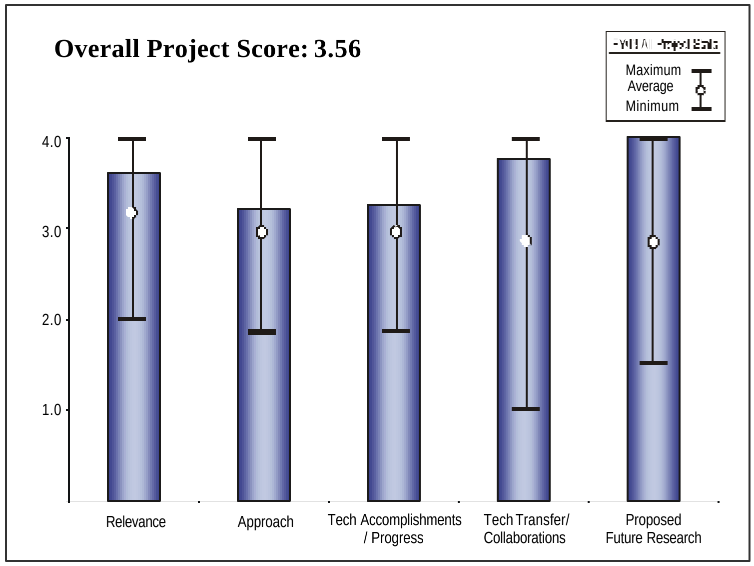
Specific recommendations and additions or deletions to the work scope

- Recommend incorporating advanced components as they become available in the future.

Project #65: Renewable Energy Transportation System
Williams, John, SunLine

Brief Summary of Project

Sunline is conducting a 3-year development and demonstration program on utilizing renewable energy to power fuel cell vehicles. This project utilized both demonstrations and modeling programs to assess the feasibility of utilizing renewable energy – particularly solar energy to power electrolyzers to make hydrogen.



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

This project earned a score of 3.60 for its relevance to DOE objectives.

- There's nothing better than real working systems.
- Great demonstration of potential to produce and use H_2 in multiple configurations.
- Excellent test bed for field trials/demos of technologies supporting the development of a hydrogen infrastructure and hydrogen-fueled transportation system.

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

This project was rated 3.20 on its approach.

- Project is not sharply focused - many detailed technical issues could be addressed, but they need to be prioritized with adequate resources for instrumentation and data acquisition.
- Provided excellent information on technical performance of hydrogen/vehicle technologies as well as economic performance data.
- Excellent educational and public outreach tool. Provides lessons learned on permitting.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.25 based on accomplishments.

- Not sharply focused - many detailed technical issues could be addressed, but they need to be prioritized with adequate resources for instrumentation and data acquisition.
- Providing considerable performance data on a broad range of H_2 generation and H_2 vehicle technologies as well as lessons learned that should result in technology improvements and reductions in the currently high cost of these technologies.

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

This project was rated 3.75 for technology transfer and collaboration.

- Great education and outreach!
- Has worked with numerous technolgy developers.

Question 5: Approach to and relevance of proposed future research

This project was rated 4.00 for proposed future work.

- Not sharply focused - many detailed technical issues could be addressed, but they need to be prioritized with adequate resources for instrumentation and data acquisition.
- Plans include testing additional power generators, H_2 production, and H_2-fueled vehicles.
- Include capital recovery in economics. "Economics" without these costs are misleading.

Strengths and weaknesses

Strengths
- Wonderful program.

Weaknesses
- None specified.

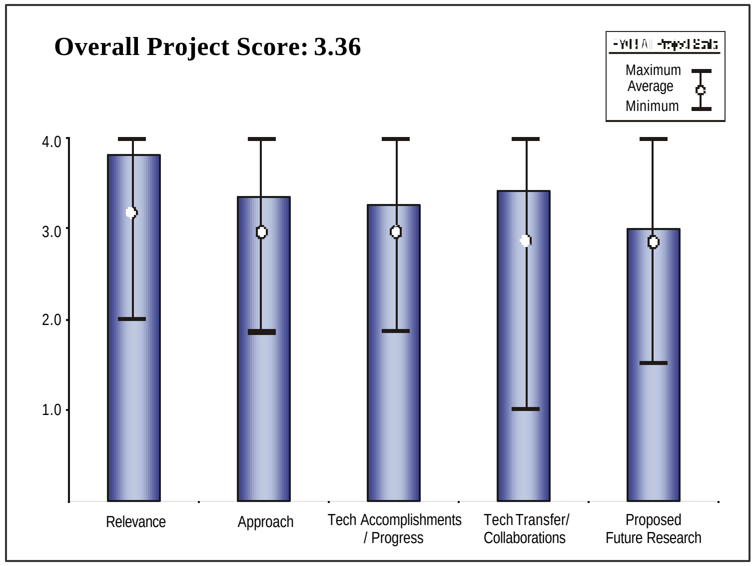
Specific recommendations and additions or deletions to the work scope

- Site should be considered as a plan for demonstration of stationary fuel cell systems.

Project #66: H_2 Storage and Compression: LAX
Bollinger, Robert, Praxair

Brief Summary of Project

In this two year project, Praxair will design, develop, install, and operate a H_2 fueling station that features integration and packaging of existing technology electrolysis based on-site production, up to five light-duty vehicles per day, five minute "fast fills," growth flexibility to meet demand, and enabled for heavy-duty fills. Praxair will also provide a demonstration of a hydrogen based fueling infrastructure capable of supporting a small fleet of hydrogen fueled vehicles in order to meet the California Fuel Cell Partnership's goal to introduce up to 60 HFCVs by 2003 and is compatible with other fueling stations.



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

This project earned a score of 3.80 for its relevance to DOE objectives.

- Small H_2 stations are needed.
- Good demonstration of electrolysis for fueling stations.
- Will offer real-world demonstrations of a hydrogen refueling facility using electrolysis-based H_2 generator.
- They will provide lessons learned on permitting.
- Will help establish current cost of delivered H_2.
- They should somehow link back to metrics.

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

This project was rated 3.33 on its approach.

- Recognition of impact of footprint is very important.
- The economics of the project needs to be addressed better.
- The use of ASME tank storage is a positive attribute missing from several other demos.
- Pricing should be decided in advance perhaps in line with a DOE goal or a relation to other fuels. Could base price based on fuel cell efficiency giving $/MI equivalence to gasoline.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.25 based on accomplishments.

- Good progress.
- Project just recently started, ambitious schedule.
- N/A because project just started.
- Should project lease H_2 vehicles?

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

This project was rated 3.40 for technology transfer and collaboration.

- BP, as an advisor, is excellent.
- Good potential - needs to be emphasized.
- Working closely with BP on design of refueling facility.
- Stuart Energy, LAX, and code officials on permitting.
- Is BP a participant?

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Future plans are very unclear.
- Decommission after two years?
- Future plans should include longer-term expansion.
- Should compare costs to SMR when projects is up and running.

Strengths and weaknesses

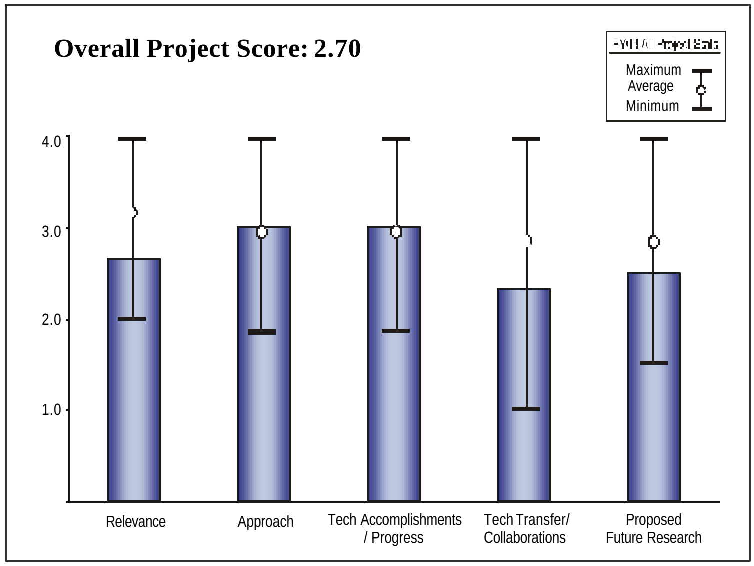
Strengths
- To be determined as project matures.

Weaknesses
- To be determined as project matures.

Specific recommendations and additions or deletions to the work scope

- Find an auto manufacturer that would lease fuel cell cars at the airport.

Project #88: Advanced Underground Vehicle Power & Control FC Mine Locomotive
Barnes, David, Vehicle Projects LLC



Brief Summary of Project

Vehicle Products LLC is developing a zero-emissions, fuel cell-powered metal-mining locomotive that operates on a 14kW fuel cell powerplant. Hydrogen will be stored in metal-hydrides. Vehicle Products will evaluate the locomotive's safety and performance, primarily in surface tests, and evaluate its productivity in an underground mine in Canada.

Question 1: Relevance to overall DOE objectives

This project earned a score of 2.67 for its relevance to DOE objectives.

- The project reported on has already been completed. Based on what was shown in the poster, it was a successful demonstration.
- Apparently, the locomotive is still undergoing endurance testing, although this was not evident from the poster itself.
- Very good demonstration of a practical system.
- Direct replacement of less efficient batteries is impressive but could probably drive commercialization for an on-site traction fleet.
- Project demonstrated technology - H_2 storage and fuel cell - in a niche application.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- The approach of using an existing locomotive and replacing the battery pack with a fuel cell system was a good engineering approach. This reduced the costs of starting from scratch, yet provided a meaningful demonstration.
- Useful in overcoming acceptance barriers for hydrogen.
- Narrow field of use and partnering not apparent.
- This is a demonstration project with little or no R & D.

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

This project was rated 3.00 based on accomplishments.

- The demonstration project apparently met its objectives.
- Successful use of fuel cell system in operational settings.
- Only some cost data has been provided.
- Only 30 hours operating time after so much cost and effort to build system seems like a low return.
- Hopefully, other lessons learned in project are more worthwhile, and are being made available to public domain.

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

This project was rated 2.33 for technology transfer and collaboration.

- There was no external collaboration listed, other than CanMet and Placer Dome, who were sponsors.
- Project allowed demonstration of some technologies under development.
- Good coordination among multiple organizations.

Question 5: Approach to and relevance of proposed future research

This project was rated 2.50 for proposed future work.

- No future work was listed other than discussions for potential interest.
- Scaling to high power/larger systems can broaden market/acceptance.
- Project is over.

Strengths and weaknesses

Strengths
- The demonstration was successful, even if only 30 hours of actual operation time were logged.
- The cost analysis summary is helpful in identifying the major cost components for such fuel cell systems.
- Successful introduction of a fuel cell transporter to the mining industry.

Weaknesses
- None specified.

Specific recommendations and additions or deletions to the work scope

- This project is already complete and terminated.
- Broaden team to reach out to other commercial applications.

This category includes projects that will develop and implement the practices and procedures that will provide safety assurance in all DOE-sponsored R&D activities and in the marketplace. These projects will also facilitate the creation and adoption of a set of model codes and standards for domestic and international production, distribution, storage and utilization of hydrogen. The aim of these efforts is to achieve the following milestones established in the Hydrogen, Fuel Cells and Infrastructure Technologies Program Multi-Year Research, Development and Demonstration Plan:

Safety

- By 2010, publish a handbook of Best Management Practices for Safety that will provide guidance for ensuring safety in future hydrogen endeavors.

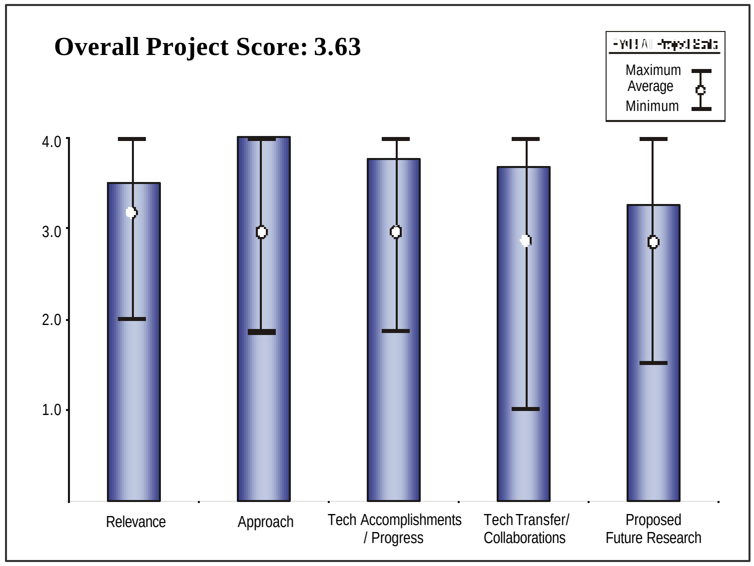
Codes and Standards

- Complete U.S. adoption of Global Technical Regulation for hydrogen fuel cell vehicles by 2010.

Project #68: Codes & Standards Analysis
Swain, Michael, University of Miami

Brief Summary of Project

The University of Miami is working on codes and standards to conduct a building safety analysis for the California Fuel Cell Partnership (CaFCP), including an assessment of safety issues related to garaged vehicles, develop a method to determine hydrogen sensor placement, and analyze safety issues for the writing of codes and standards. This will be accomplished by identifying concerns on hydrogen installations and designing and testing and verifying computer programs to accurately model hydrogen interactions.



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

This project earned a score of 3.50 for its relevance to DOE objectives.

- Fundamental study of ignition limits will provide data critical to safety analysis, leading to input into codes and standards.
- Test results will be important in developing codes and standards for storage of hydrogen in confined spaces.
- Difficult to extrapolate to all possible cases from limited experimental data.
- Need for H_2 properties to be well understood so that impact on C&S is accurate and rational. C&S must be based on fact. Inappropriate standards could be fatal to economics of some applications.
- Science-based data will help set codes and standards.
- PI very knowledgeable on specific scientific and also applied codes and standards development.

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

This project was rated 4.00 on its approach.

- Experimental approach is novel and will advance the understanding of ignition limits in the more realistic scenario of a H_2 turbulent jet - a real H_2 leak.
- PI has been studying (theoretically and experimentally) the conditions under which hydrogen can be ignited due to leaks and/or confinement in enclosed spaces - results should be helpful in defining safety systems needed and the establishing codes and standards.
- Project is well focused.
- It might be better to plan series of experimental goals for PI and fund a critical mass to accomplish the work. Feasible, difficult with internal technical barriers. Stability and continuity may allow faster progress.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.75 based on accomplishments.

- Current project just started a month ago. Already have preliminary data, showing confidence that further data will be obtained.
- Although a new start, project seems to be moving forward at a rapid pace.
- May impact needs for electrical shielding and reduce the installation costs - early results imply current standards based on an overly conservative view of ignition.

- For the amount of time since the project was awarded, significant technical progress was made.
- Modeling results already available for quiescent conditions.
- PI has outstanding dedication to field.

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

This project was rated 3.67 for technology transfer and collaboration.

- Collaboration with national lab and SRI is useful.
- Presumably, results will be published in technical journals.
- Seems to be adequate with involvement of SNL. Uncertain how it integrates with ongoing efforts at SNL.
- Outstanding interactions with industry, academic, labs and standards and codes organizations.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.25 for proposed future work.

- Most of the project is future work at this point.
- This work obviously expands on previous work on H_2 safety issues.
- Project seems focused on answering a specific issue.
- Uncertain what the future plan is in any detail.
- Within the framework of codes and standards, bring in "fire marshals and non-technical" people into the design of experiments.
- PI already tapping working groups.
- Finding a champion of "fire marshal" (like Arizona/Phoenix) would help.

Strengths and weaknesses

Strengths
- PI's experience and demonstrated expertise in area under study is the major strength of this project.
- Long career in field.
- Outstanding quality of work.

Weaknesses
- None specified.

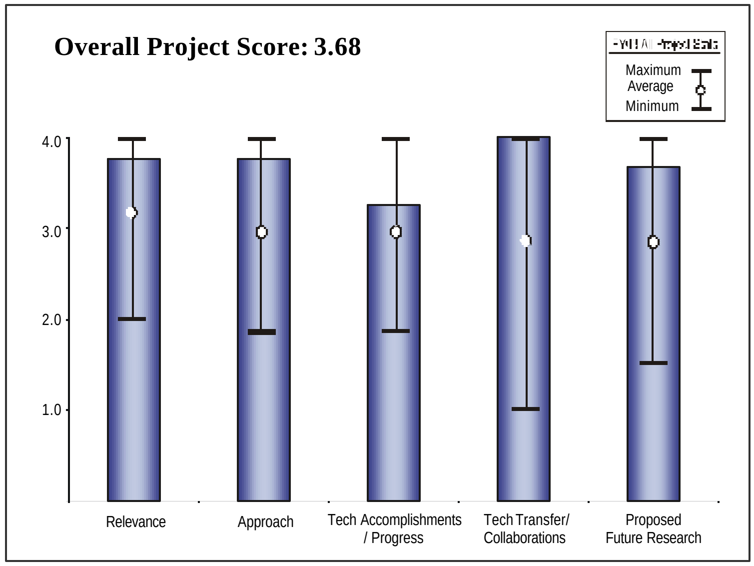
Specific recommendations and additions or deletions to the work scope

- "Need more" Mike Swains in the program.
- Tapping community for other areas - modeling/computational.
- Complementing teamwork excellent.

Project #69: Hydrogen Codes and Standards
Ohi, Jim, NREL

Brief Summary of Project

In this project, the National Renewable Energy Laboratory will work on hydrogen codes and standards to expedite hydrogen infrastructure development, coordinate such development activities for HFCIT, and incorporate hydrogen safety considerations into existing and proposed national and international codes and standards. This will be accomplished by bringing together experts to address key issues, coordinating a collaborative national effort 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 3.75 for its relevance to DOE objectives.

- Program aimed to overcome the barrier of codes and standards for H_2.
- Should help elimination of the possibility of conflicting codes and standards.
- Could prove valuable in helping to educate code officials.
- DOE needs to assure that C&S efforts are done thoroughly and correctly.
- Essential to reach the President's vision.

Question 2: Approach to performing the research and development

This project was rated 3.75 on its approach.

- Aim project to collect data and assemble codes for a Global Technical Regulation.
- Maintaining electronic newsletter and C&S matrix on website are good approaches to spreading information.
- Totally focused on issue of getting required codes and standards to "eliminate" institutional barriers to technology development, and more importantly - implementation.
- Involvement with organizations worldwide to assume consistency. Needed to allow our US products to be acceptable in foreign markets.
- It is a "continuous improvement and self definition" kind of project.
- Depends on outstanding technical people with interpersonal and social skills to address codes/standards barriers.

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

This project was rated 3.25 based on accomplishments.

- Drawing in several agencies, such as AGA, to collaborate together.
- Holding several public meetings to promote coordination of C&S.
- H_2 safety in '03 model codes of ICC; code organizations surveyed for comment.
- Developing draft templates for national standards.
- Playing a significant role in helping more codes and standards setting activities forward.
- Not appropriate feature of this project.
- Effort needs increased critical mass.

- Although outstanding work has been done and very significant progress, to accomplish more will take additional efforts, people, and skills.

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

This project was rated 4.00 for technology transfer and collaboration.

- Coordinating every organization under the sun.
- Working closely with codes and standards setting organizations and industry.
- Extensive interaction and collaboration with all stakeholders in codes and standards.
- Crucial feature is the generation of training/education modules for the code officials and the general public.
- Team DOE/NREL has outstanding coordination nationally and internationally.

Question 5: Approach to and relevance of proposed future research

This project was rated 3.67 for proposed future work.

- Identified key standards that will direct an R & D plan for validation.
- The only logical end point for this activity is when fuel cells enter the marketplace in large number and codes and standards issue is no longer a barrier, but a tool that can be used to facilitate this market penetration.
- This program won't end for quite awhile - the training/education will need to be improved/updated continuously.
- Broad subject; gaseous/liquid and potentially high pressure increases with times; i.e. 10K PSI storage.
- Should use as much as possible to use web tools and additional ones for information dissemination.

Strengths and weaknesses

Strengths

- Level of interaction with all stakeholders in establishing codes and standards is a definite strength.
- All participants/stakeholders have been identified and their involvement assured via numerous working groups. NREL is active member of appropriate WGs.
- Technical knowledge of team.
- Enthusiasm shown.

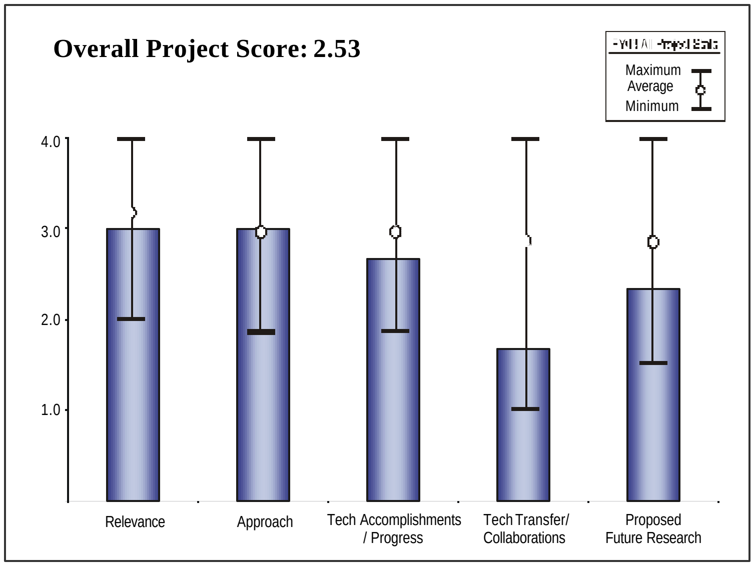
Weaknesses

- Needs additional people (critical mass).

Specific recommendations and additions or deletions to the work scope

- Engage Weaver/Hollywood for some sense of the demo - what's good and bad.
- Get some additional social/technical links into program.

Project #93: Gallium Nitride Integrated Gas/Temperature Sensors for FC Sys Monitoring for H_2 & Carbon Monoxide
Pyke, Steve, Peterson Ridge LLC



Brief Summary of Project

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

This project earned a score of 3.00 for its relevance to DOE objectives.

- There is a need for rapid response high-temperature CO sensors for on-board fuel processing for process control and for protecting the PEMFC.
- Gas sensors will be important for safety. (Good)
- CO sensors are important for safety and to prevent the poisoning of fuel cells. (Good)
- This project is in the second tier of importance in the development of fuel cell technology.
- Development of a CO sensor still depends on discussions on fuel reformation and on technologies under development to mitigate CO effects on fuel cell performance.

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

This project was rated 3.00 on its approach.

- The technical approach for developing the sensor is well-thought out but may need to be redirected. Based on the high-operating temperatures, it may be more appropriate for use in PrOx and WGS control at CO concentrations of 1-2% instead of <100 ppm.
- Goal is a commercial material so processes are known to manufacture large numbers of sensors. (Good)
- Using paired sensors allows correlations of two gases. (Good)
- Pt is expensive, but not much would be needed per sensor.
- A 200-300^{o}C operating temperature complicates the sensor.
- Proof-of-principle for use of this technology has been demonstrated in that a signal is generated in response to the presence of CO. However, accurate and reproducible measurement, quantitatively, has still to be demonstrated.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 2.67 based on accomplishments.

- The project has made good progress towards meeting its goals; however it needs to better identify where it fits into fuel processing (on-board).
- Because of its high operating temperatures (200-250^{o}C), I do not think it will be applicable for the role the developer has in mind - this is not bad; just redirect.
- Initial tests showed good results and the investigator used knowledge to make further improvements.
- It is not clear if temperature's current changes will require an integrated temperature sensor. This could increase costs. (Bad)
- Little or no data on interference from other gases which could cause false positives. (Bad)
- High temperature sensor to improve description rate will increase cost. A necessary evil.

- Progress has been less than expected. Large technical barriers still exist and will require considerable additional time and effort.

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

This project was rated 1.67 for technology transfer and collaboration.

- Has identified potential industrial collaboration although I question how close this technology is to being brought to market for this application.
- Mostly testing collaboration - no manufacturing partnerships in place yet.
- It is good that they're having other researchers confirm results.
- Two sensors plus a heater, plus a temperature sensor, plus electronics and power supply gets complex. Can better signal processing eliminate the temperature effects?

Question 5: Approach to and relevance of proposed future research

This project was rated 2.33 for proposed future work.

- Proposed future research is good but experimental plan needs to be focused on where this technology fits into the fuel processor. Need to team up with fuel processor developer.
- Still need to demonstrate reliable operation over longer ranges.
- Need to show no interference from contaminants in a vehicle or industrial setting.
- Recommend more focus on one or two specific or similar applications. (Get it to work well on one thing, then look at other markets.)

Strengths and weaknesses

Strengths
- There is a need for CO sensors not only to protect the PEM sensor from CO poisoning (<10-100 ppm), but also a need to monitor activity of CO cleanup processes, i.e. PrOx and WGS reactions. This is an important issue that needs to be addressed.
- The optimal temperature range of this sensor (approx. 200-250^{o}C) would make it more appropriate for use as process control for WGS and PrOx instead of protecting the PEM. For most fuel processors, the CO concentration at the 10-100 ppm level will only occur at temps <250^{o}C.
- Look at more advanced signal processing. Interference effects will be important.
- An interesting technical approach.

Weaknesses
- Insufficient progress to determine if this technology can compete successfully with other technologies currently under development.

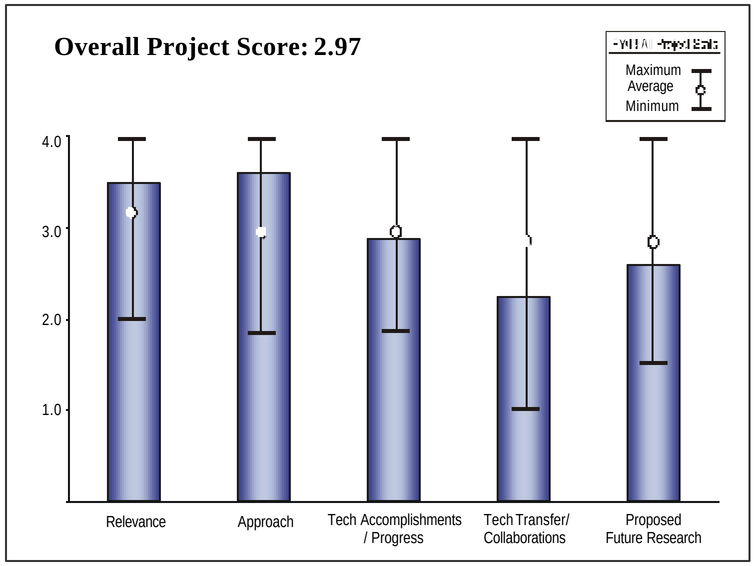
Specific recommendations and additions or deletions to the work scope

- Need to address the issue of the effect of water vapor and H_2S.
- Need to demonstrate a response time of <1 second.
- Team up with group developing fuel processors for field testing (look at PrOx and possibly WGS for control processes).
- Don't go through too many material screenings (Ir, Pt, Pd, Ag, Rh) if you've found one that works well. Get that working first, then improve it.
- Discuss the impacts of high temperature on design and if temperature will adversely affect accuracy.
- This project should be given one year of additional funding with the mandate of producing a working device. If sufficient progress toward that goal has not been demonstrated at the end of that period, the project should be terminated.

Project #126: Interfacial Stability of Thin Film H_2 Sensors
Pitts, Roland, NREL

Brief Summary of Project

The National Renewable Energy Laboratory is currently working to develop and make technology available that would produce safe, reliable, sensitive, fast, lightweight, and inexpensive hydrogen sensors. To do this, NREL will look at the factors affecting the stability and performance of thin film sensors, such as suspect contaminant gases, temperature variations, and humidity impacts, in practical environments and find solutions for extending the lifetime and functionality of thin film hydrogen sensors.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.50 for its relevance to DOE objectives.

- Address safety concerns related to storage of H_2.
- Need for sensors in this program is obvious.
- Clear understanding of performance needs and goals.
- Sensitivity issues and atmospheric contamination/ degradation issues need to be better explained.
- This addresses the potential needs and could offer both safety and control aspects of sensors, therefore reducing the cost.

Question 2: Approach to performing the research and development

This project was rated 3.60 on its approach.

- Good overall approach to product development.
- Electrochromic/ fiber optic approach to detecting H_2 is an interesting one.
- Pd membrane potentials ensures selectivity.
- I'm glad to see something other than amp/pot sensors under development as well.
- Background info was a little confusing as presented. Spent too much time on it too.
- Approach is different and innovative - calorimetric changes are idiot proof (excellent for practicality if they work).
- Objectives are outstanding recognizing ease-of-use by customer. Design criteria impressive.
- Negative: translation to practice may be challenging.
- It seems that there is a basic lack of understanding as to what the basic technical problems are and what needs to be done.
- There has been vacillation between sensor materials and how it would be implemented.
- Interferents are a major obstacle (H_2O) which is not a new discovery and needs to be focused on.

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

This project was rated 2.88 based on accomplishments.

- Good piece of technical work. Identified technical problems and organized approach for addressing issues.

- Promising results demonstrated so far but response at low temperature and to various humidities is troubling, as well as long term degradation (after 1 year).
- If you look at the project timeline it might seem that progress has been slow, however I think that it's more reflection of the difficulty of developing H_2 sensors.
- Alluded to the technical difficiencies without elaborating on what those difficulties were - this prevents anyone from offering a suggestion that might be helpful.
- Choice of materials - not clearly articulated: history of electrochromic materials compensated for above weakness.
- Chemical changes resulting in color changes, defactor, tend to be slower that potantiometric or amperometric systems.
- Sensitivity and durability issues remain significant.
- Funding issues!
- Although several test conditions have been investigated the process seems to be slow. It seems that the experience in the effort over a long period is not being used to its fullest.

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

This project was rated 2.25 for technology transfer and collaboration.

- Electrochromic mirrors are used now in auto applications. Companies like SageElectrochromics (Minn.) are developing for electric window applications and could possibly contribute. Also WalterJuda Inc. has developed Pd membranes. These two companies might help in overcoming problems.
- Some good interactions have been established.
- Appears to have good collaboration with other industry.
- Perhaps some close academic collaborations with the appropriate material/polymer scientists would help move things along faster.
- Choice of collaborators - curious! DCH went bankrupt. Need to choose more stable players.
- Need to understand commercial realities - who are the appropriate end-users.
- Not much effort to work with companies who are in the fiber optic business.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 2.60 for proposed future work.

- Gets a 3 because of efforts.
- Needs more input from other companies that have worked in this area and could contribute to a more robust sensor approach.
- Funding seems to be a significant barrier (I count 3 delays due to funding issues). What's the deal with that?
- Sensitivity, operational range, durability issues (and possible improvements) need to be debated in depth.
- May have to make a tough Go/No-go decision in light of results especially because of irreversibility of reactions.
- Why design a complete sensor package when no "fix" has been identified?
- Need to understand the basic problems to take this technology forward.

<u>Strengths and weaknesses</u>

Strengths
- Good overall approach to product development, consideration of all requirements - technical, use environment, end-user needs, maintenance…
- Interesting approach to H_2 sensor development. Results are both promising but show some of current performance limitations.
- Clearly innovative. Moving away from electrochemical approaches.

Weaknesses

- More outside contracts with expertise in this area are needed ASAP.
- Technical reality may be tough. Decision on go/ no-go needs to be made before long.

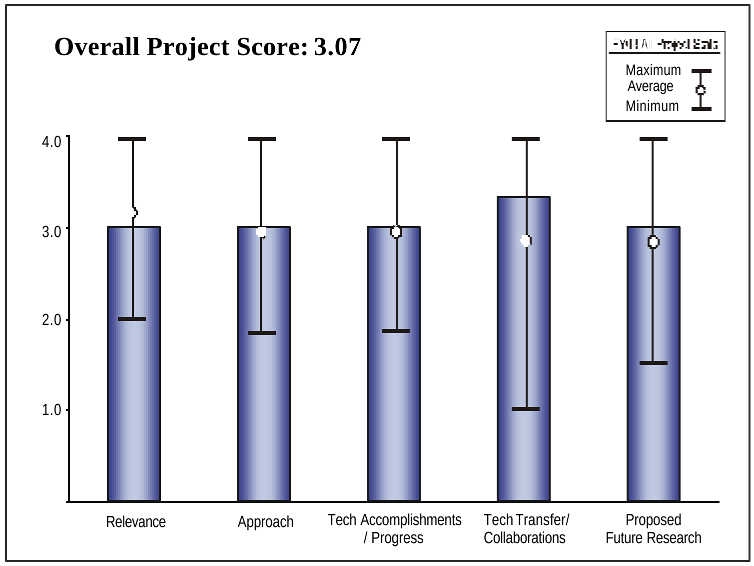
<u>Specific recommendations and additions or deletions to the work scope</u>

- Keep up the good work.
- I'd like to see how much power this system needs for operation.
- Attach and acknowledge the difficulties re: durability, sensitivity, etc.
- While approach is excellent, need to broaden investigation into other calorimetric materials or drop the program if persistent issues of durability, sensitivity cannot be overcome.
- This technology has a lot of potential but there needs to be a concerted effort by people who have fundamental knowledge in the material selection.

Project #128: Micro-Machined Thin Film H_2 Gas Sensors
DiMeo, Frank, Adv Tech Materials Inc.

Brief Summary of Project

ATM, Inc. is working on micro-machined thin film hydrogen gas sensors and is seeking to optimize micro-hotplate based hydrogen sensor design and fabrication, investigate potential sensor cross sensitivities and degradation mechanisms, and demonstrate an extrapolated sensor lifetime greater than 3 years. To do this ATMI will investigate the use of alternative micro-hotplate geometries, develop surface treatment processing to minimize the contact resistance, construct test manifolds and signal-conditioning circuitry for advanced sensor testing, and study sensor response to contaminant gases and extended life performance.



Question 1: Relevance to overall DOE objectives

This project earned a score of 3.00 for its relevance to DOE objectives.

- The project objectives are consistent with DOE targets for FC development.
- H_2 specific sensor important for fuel cell/H_2 introduction.
- Low cost sensor goal important and progress seems good.
- Does a very good job of identifying project relevance in relationship to 2002 National Hydrogen Energy Roadmap.
- Project is well directed towards addressing the need for H_2 sensors.

Question 2: Approach to performing the research and development

This project was rated 3.00 on its approach.

- There is a need to find out what is affecting the response time (so far there's a big variation).
- The interference of O_2 not clear yet. The dependence on O_2 concentration needs to be addressed.
- How the sensor respond to altitude?
- Cost barrier being addressed.
- Selectivity addressed.
- System development important.
- Approach is well-thought out for making good technical progress.
- Does a good job at addressing program objectives although the technical barriers that need to be addressed could be better defined.

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

This project was rated 3.00 based on accomplishments.

- Good progress in the fabrication process control.
- Valuable long-term test accomplished.
- Efforts in understanding sensor operation need more attention.
- Appear to have achieved major goals.

- Project seems to be close to finalizing fabrication process and systems development field testing will be critical for defining if reevaluation of fabrication system development is necessary.

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

This project was rated 3.33 for technology transfer and collaboration.

- Good collaborations for independent evaluation of the sensor, particularly with UTC.
- Teaming/ collaboration with systems (H_2/Fuel Cell) important. Need to broaden beyond UTC.
- Good progress toward establishing industrial collaboration. Interaction with UTC Fuel Cell (positive results) could help promote further industrial collaboration.
- What is the plan for deploying prototypes externally to ATMI?

Question 5: Approach to and relevance of proposed future research

This project was rated 3.00 for proposed future work.

- Good plan for the rest of the time schedule.
- More attention on sensor operation should be considered.
- Appear to be near completion of sponsored work.
- Future work plan directed towards improving fabrication.
- Field of testing is critical.
- Identification of target market for H_2 economy (i.e. safety, process control) would be helpful.

Strengths and weaknesses

Strengths
- Successful product development.
- Appears to have cost reduction in hand.
- Need to gain acceptance by users.
- Does a good job in identifying how this project addresses National Energy Roadmap.
- Well focused on manufacturing/fabrication development.

Weaknesses
- Presentation of data for H_2 selectivity in presence of CO, H_2S, and IPA would have been appreciated.

Specific recommendations and additions or deletions to the work scope

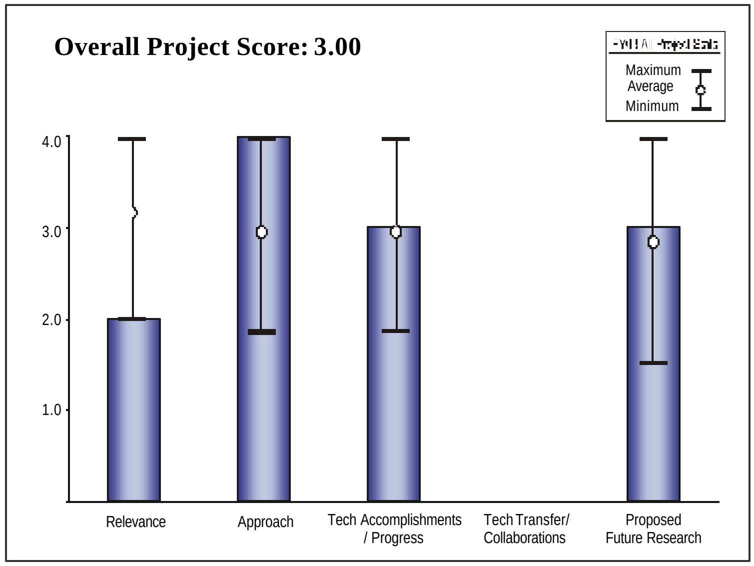
- On the slide, "Communication/Collaboration", there is a statement about "detection of sulfur-containing gases" - can this technology be extended to detecting ppb level of H_2S in reformate?
- Cost projections would be beneficial.
- Need to identify what is its target appreciation for the "hydrogen economy" - appears to be safety and monitoring and not necessarily production.

SECTION 6: OTHER

This section contains all projects funded in FY 2003 by the HFCIT Program that do not fall into the previous categories. The technical knowledge gained from the successful completion of these projects, however, would benefit other HFCIT work.

Project #28: Toward the Development of a Thermodynamic Fuel Cell
Van Blarigan, Peter, SNL

Brief Summary of Project



Sandia National Laboratories is designing and demonstrating an ideal, thermodynamic cycle based electrical generator utilizing a homogeneous charge compression ignition that charge combusts due to compression heating, achieve constant-volume combustion, is multi-fuel capable with no flammability limits, and features NO_x control by dilution. A thermodynamic fuel cell will be able to compress the fuel/air mixture rapidly to reach high compression ratio at ignition, electronically control compression ratio, be capable of surviving high peak, short duration pressure pulse, and have mechanical simplicity for high reliability and low cost potential.

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

This project earned a score of 2.00 for its relevance to DOE objectives.

- A novel IC engine (OTTO cycle) for efficient utilization of hydrogen fuel with low emission.
- Excellent fundamental R & D with high risk sizeable benefits potential.
- Unclear why this project is in the HFCIT program. If done anywhere it should be in an ENGINE program within DOE.

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

This project was rated 4.00 on its approach.

- Largely a theoretical and modeling study to evaluate the concept prior to proceeding to a hardware phase.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

This project was rated 3.00 based on accomplishments.

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

This project was not rated for technology transfer and collaboration.

<u>Question 5: Approach to and relevance of proposed future research</u>

This project was rated 3.00 for proposed future work.

- A high risk project that needs a Go/No-go decision on whether to proceed and, if yes, in what direction.

Strengths and weaknesses

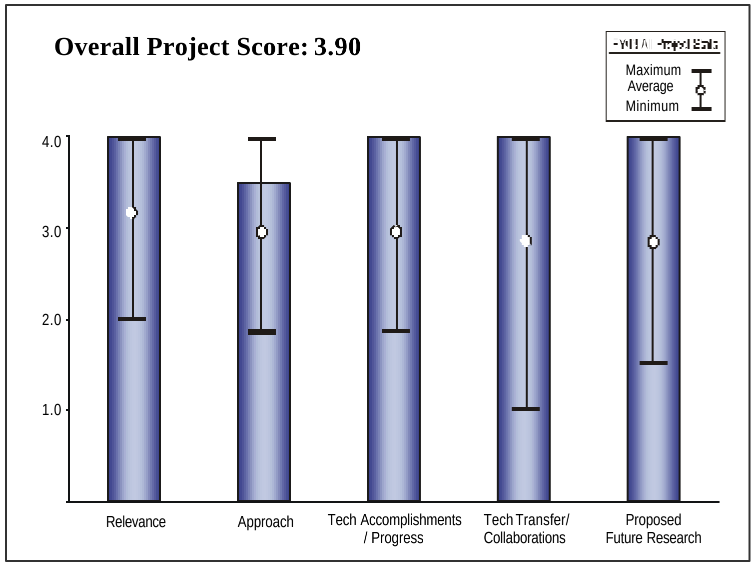
Strengths
- Excellent science.

Weaknesses
- None specified.

Specific recommendations and additions or deletions to the work scope

- This project has been in place for several years. A decision is in order to either commit the necessary resources to take this ICE concept to the hardware (proof-of-concept) phase or make a decision to discontinue the project.

Project #29: Reduced Turbine Emission Using H_2-Enriched Fuels
Keller, Jay and Liss, William, SNL



Brief Summary of Project

Sandia is working to reduce turbine emissions using hydrogen-enriched fuels by quantifying the effect of hydrogen addition in various areas of a gas turbine, establishing a scientific and technological database for lean combustion of hydrogen-enriched fuels, establishing numerical simulation capabilities that will facilitate design optimization of gas turbine combustors, and by developing criteria for use of hydrogen addition as a control knob to eliminate instabilities related to varying product gas composition.

Question 1: Relevance to overall DOE objectives

This project earned a score of 4.00 for its relevance to DOE objectives.

- Applications which utilize H_2 efficiency are crucial to the growth of an infrastructure.
- Substitution of hydrogen for some of the fuel used in turbines has the potential to reduce pollutant emissions as well as CO_2, while helping preserve fossil fuels.

Question 2: Approach to performing the research and development

This project was rated 3.50 on its approach.

- Unclear whether they have incorporated prior studies done with Westinghouse/IGT in the DOE/FE advanced turbine program in the early 90's.
- Emission reduction is not the only a benefit, some system designs have significantly increased efficiency.

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

This project was rated 4.00 based on accomplishments.

- The work is using state of the art tools to get the data needed to understand and hence improve the use of the H_2 emission reduction.
- There is a need to broaden the concept of thermochemical recuperation for efficiency increase.
- Enriching conventional fuels with H_2 looks like a very promising method for reducing turbine emissions.

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

This project was rated 4.00 for technology transfer and collaboration.

- Actual project partner involvement is unclear.
- Excellent collaboration - national and international - including industrial interest.

Question 5: Approach to and relevance of proposed future research

This project was rated 4.00 for proposed future work.

- This should be able to be brought rapidly into the markets.
- Major barriers are probably not technical but educational.
- The growing use of CH_4-H_2 blend should make this easier. The development of a future reformer for the fuel preparation has not been solved.

Strengths and weaknesses

Strengths
- Excellent progress.
- Significant interest/collaboration in project.

Weaknesses
- None specified.

Specific recommendations and additions or deletions to the work scope

- A minor task should be started to examine the current advantages of thermochemical recuperation to utilize waste heat for fuel preparation which increases system efficiency.

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APPENDIX B: HFCIT FY 2003 MERIT REVIEW AND PEER EVALUATION REVIEWER FEEDBACK

These notes summarize the comments received at the various reviewer feedback sessions. The comments received were generally focused on the basic meeting process, however, where relevant, notes specific to a particular session will be designated and called out.

General Comments:

- **General Positive Responses**: The new HFCIT program covers the whole "waterfront" of hydrogen and fuel cell issues, with good balance and funding. DOE should be congratulated on its efforts. Huge benefits could be obtained from the mix of the two technologies. Several reviewers also felt that the pieces are now in place to make real progress in these domains. This review meeting was a "benchmark" workshop which will be difficult to improve upon. However, at least one reviewer commented that following the recent program transitions, there may be projects that are included in the review process that are not fully relevant to HFCIT. The project list should be reviewed and weeded/trimmed with this in mind. Another reviewer noted that this merit review has become very large, encompassing both information exchange and merit review purposes. With this in mind, he suggested that more projects be addressed through the poster session – perhaps those with lower funding levels. Another reviewer liked that there were more oral presentations this year, and liked the duration of the meeting.
- **Poster Sessions**:
  - *Viewing Times*: There needs to be additional poster viewing times, rather than just 2 specific time allotments – one of which was scheduled at the same time as the lab tours. Furthermore, it may be helpful to have business card collection envelopes/containers at the bottom of each poster so that if the PI is not present, contact can more easily be made.
  - *Space*: Space was tight at this review – posters should be located in a more open room away from the hors d'oeuvres.
- **Meeting Logistics**:
  - *Seating*: All attendees should be able to sit at tables.
  - *Hand-outs*: An inexpensive tote bag should be provided to carry the meeting materials.
  - *Reference Materials*: Reviewers discussed whether it was better to have the presentation materials available in print or on CDs, and at the meeting or after it. There was not broad agreement on which medium was preferable though many felt it important to have the materials available at the time of the meeting. One reviewer said this year's binder was "head and shoulders" above anything put together in the past although it should have tabs to separate the different areas.
  - *Timing*: Reviewers liked the use of electronic timers, and the staggering of breaks for crowd control.
  - *Parallel Presentation Tracks*: Reviewers discussed the setup of having two parallel presentation tracks, and many appreciated the balance of flexibility in allowing participants to move back and forth, and efficiency in getting through a lot of presentations in a limited amount of time. Reviewers unanimously voted that this setup was preferable to having either more or fewer tracks.
  - *Computer Cluster*: One reviewer requested that the review form be made available electronically, and that a small "computer cluster" be set up in the conference center where reviewers could go to record their reviews electronically.
- **Reviewer Assignments**:
  - *Assignment Areas*: Reviewers should review several projects in the same area, rather than reviewing projects scattered across all topics.
  - *Advanced Information*: Reviewers would like to know what projects they are reviewing in advance and would like to receive full project reports prior to the meeting so that they can have time to read up on them.
  - *Project Report Formatting*: Format the project reports in smaller PDF files so they're accessible by project area, rather than only as a single, extremely large PDF file.

- **Presenter Guidance**:
  - *Basic Formatting:*
    - *Black & White vs. Color Graphs*: The meeting binder is in black and white for understandable cost-related reasons, but this doesn't allow presentations that use color to be adequately represented. Presenters should perhaps receive guidance that their presentations be viewable in B+W.
    - *Units*: All presentations should use SI units to foster transparency as well as consistency. Lower heating values (LHV) and higher heating values (HHV) should also be specified and made consistent.
    - *Photographs*: Presentations that use photographs should provide an indication of scale within them.
    - *Funding Levels*: Presenters should include their funding levels in the introductory slide/s.
  - *Presentation Content and Consistency*: Additional information would be beneficial for project evaluation. One reviewer said more information should be presented on funding history/cost statements, man-years spent on a project, and the maturity of a project within its timeline. Assumptions and methodologies should be made clear. He also noted that a synopsis of why a project is new or special would be beneficial. It was suggested that if DOE has a one-pager on each project, this could be helpful to the reviewers. Presentations would benefit from concise statements of:
    - *Progress*: Project progress in the past year.
    - *Stage*: What stage the project is in (just starting, extensive data generated, etc.)
    - *Person-years expended to date*
    - *Funding levels*
    - *Energy efficiency goals and achievements*. It was noted that for some projects, efficiency is dependent on scale.
    - *Energy Balance*: Information on energy balance was missing in the presentations. The presentations need to be standardized so that all the presentations contain necessary information.
    - *Potential Return Analysis:* It would be helpful to have in each project an analysis of potential returns as compared to the targets (going beyond "next steps") – especially for hydrogen storage projects, for which the targets are quite aggressive. Discussions regarding the relevance of future work should perhaps be moved to the front.
    - *Go/No-go Decision Points*: Some projects lacked clear decision points.
  - *Oral Presentation:* It should be impressed upon the PIs that they need not read every slide in the oral presentation – in some cases it's enough to have the slide available for reference or background. Publications could be excluded from the oral presentations, and could be incorporated either as an addendum, or in the annual progress report.
- **Review Form**: The review form was difficult to apply to some projects. For example, for projects focused on basic research it was difficult to pin down the indirect connections to form fields. Additionally, some basic science projects are not well benchmarked to DOE targets (or clear DOE targets do not exist), further complicating the process. One reviewer commended the existence of a project standard for collaboration, although others noted that collaboration could be difficult for private industry, especially for competitors, for reasons like proprietary materials.
- **Length of Presentation**: Some felt that presentation time should correspond to DOE funding level. It was noted that this has been tried and is done to some extent, but is difficult to carry out in a time-effective manner. Furthermore, it was mentioned that some projects had just started and were not ready for presentation.
- **Intellectual Property Issues**: Some PIs were not open about their projects for intellectual property reasons. They need to come up with ways to talk about the project that do not run into this. Other reviewers noted that performance and cost reports are not proprietary. It was noted that where IP presents real difficulties, reviewers could indicate this on the review forms for DOE to pursue more closely in a private setting.

- **Hydrogen Production:**
    - *Inter-program Coordination*: Other DOE offices should be encouraged to make presentations and/or attend the merit reviews. A lot of activities have been going on in the synthetic coal and fuel processing area. What comes out of these would help the hydrogen activities. Hence there should be coordination between the fossil, nuclear, solar, and wind programs. Likewise, lessons on cost effectiveness and yield can be learned form the solar industry, which suffered setbacks in their thin film production efforts.
    - *Lack of Environmental Discussion*: Fuel cost targets were addressed but environmental and other concerns were not addressed by the PIs.
    - *DOE Targets*: The targets set forward by the government need to be reevaluated.
    - *Benchmarks*: Project leaders need to come up with benchmarks and the science needs to be benchmarked too. A limit needs to be set.
    - *Hydrogen Initiative*: The Hydrogen Initiative should generate targets similar to those for FreedomCAR that break down to the component level.
    - *Component Synergies*: It is difficult to look at synergies between components when they are developed separately. There needs to be a way to look at system trade-offs not only in cost, but also in technical approach, that might not be apparent from the development of individual components. A hydrogen "well" to wheels model was suggested to assist component makers.
    - *Computational Techniques*: There was very little use overall of computational techniques, especially in chemistry projects, although this is a relevant and popular approach at this time.

Additional Comments:

- Demonstration projects are providing valuable data/feedback for the hydrogen/fuel cell community.
- Comments on diversity in technology were brought up during the discussion.
- A challenge for next year would be to look at the projects and see if some of those need to stay or go to bring in new projects in order to move forward to achieve the objectives.
- One reviewer noted that reviewer comments sometimes come across as "random," and that they seem to be given equal weight regardless of relevance. It was noted that this is not the case, that they are in fact read in the context of the reviewer's background, and a renewed effort should be made to select reviewers carefully and incorporate their comments coherently into the report.

2003 DOE HYDROGEN, FUEL CELLS, AND INFRASTRUCTURE TECHNOLOGIES PROGRAM REVIEW MEETING

Project Evaluation Form

SESSION: **PROJECT NO.:** **REVIEWER NAME:**
PRESENTER: **ORGANIZATION:**
TITLE:

Using the following criteria, please rate the **work** presented in the context of program objectives. Please provide **specific** comments to support your evaluation. Note: These evaluation criteria have been modified to more closely reflect the Office of Management and Budget's scoring criteria for applied R&D investments.

1. Relevance to overall DOE objectives. The degree to which the project supports the President's Hydrogen Fuel Initiative and the goals and objectives in the EERE Hydrogen, Fuel Cells, and Infrastructure Technologies Program R, D, and D plan.

4 - Outstanding. The project is critical to realization of the President's hydrogen vision and fully supports the objectives of the R, D, & D plan.		Specific Comments:
3 - Good. Most aspects of the project align with the President's Hydrogen Fuel Initiative and R, D, & D Plan objectives.		
2 - Fair. The project partly supports the President's Hydrogen Fuel Initiative and the R, D, & D Plan objectives.		
1 - Poor. The project provides little support to the President's Hydrogen Fuel Initiative and the R, D, & D Plan objectives.		

2. Approach to performing the research and development. The degree to which market barriers are addressed. The degree to which the project is well-designed, integrated with other research, and technically feasible.

4 - Outstanding. The project is sharply focused on one or more key technical barriers to development of hydrogen or fuel cell technologies. It is well integrated and it is 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 significant progress in overcoming these barriers. Some integration with other research apparent.		
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 the project objectives and unlikely to make significant contributions to overcoming the barriers.		

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3. Technical Accomplishments and Progress toward project and DOE goals. The degree to which research progress is measured against performance indicators. The degree to which the project elicits improved performance (effectiveness, efficiency, and benefits.)

4 - Outstanding. The project has made excellent progress toward overcoming one or more key technical barriers to development of automotive fuel cells as evidenced by progress measured against performance indicators; progress to date suggests that the barrier(s) will be overcome.		Specific Comments:
3 - Good. The project has shown significant progress toward overcoming barriers as demonstrated against performance indicators.		
2 - Fair. The project has shown a modest amount of progress in overcoming barriers, and the overall rate of progress has been slow.		
1 - Poor. The project has demonstrated little or no progress toward overcoming the barriers.		

4. Technology Transfer/Collaborations with Industry/Universities/Other Laboratories

4 - Outstanding. Close coordination with other institutions is in place; industrial partners are full participants.		Specific Comments:
3 - Good. Some coordination exists; full coordination could be accomplished fairly quickly.		
2 - Fair. Some coordination exists; full coordination would take significant time and effort to initiate.		
1 - Poor. Most or all of the work is done at the Lab with little outside interaction.		

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5. Approach to and Relevance of **Proposed Future Research.** The degree to which the project plan has off-ramps, i.e., decision points where the project could be ended.

4 - Outstanding. Future work plan builds on past progress and is sharply focused on one or more key technical barriers to development of automotive fuel cells in a timely manner. Upcoming decisions and project end points are clearly defined.		Specific Comments:
3 - Good. Future work plan builds on past progress and generally addresses removing or diminishing barriers in a reasonable timeframe. Decisions points defined.		
2 - Fair. Future work plan may lead to improvements, but should be better focused on removing or diminishing key barriers within a reasonable time period.		
1 - Poor. Future work plan has little relevance or benefit toward eliminating barriers.		

Specific **Strengths and Weaknesses**

Specific **Recommendations/Additions or deletions to the work scope**

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