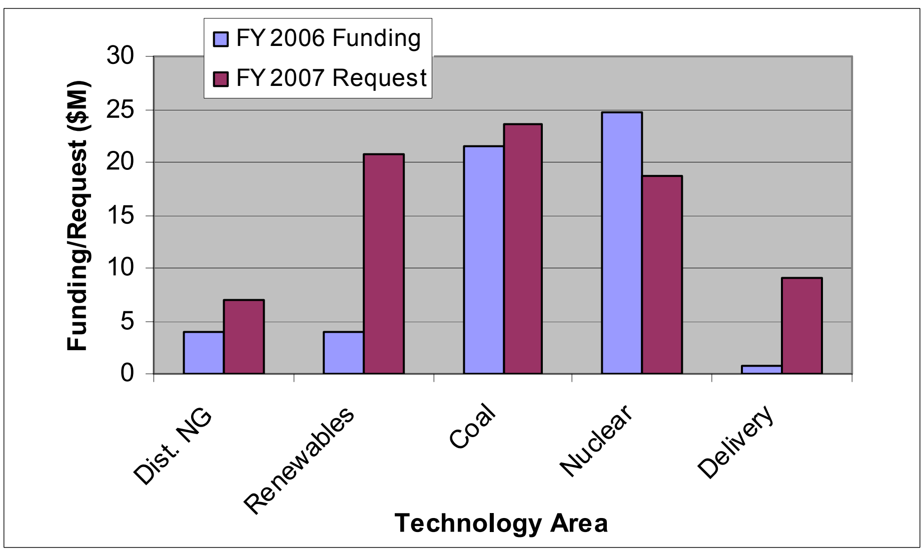
Hydrogen Production and Delivery

Summary of Annual Merit Review Hydrogen Production and Delivery Subprogram

Summary of Reviewer Comments on Hydrogen Production and Delivery Subprogram:

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

Hydrogen Production and Delivery Funding by Technology:



Majority of Reviewer Comments and Recommendations:

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

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

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

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

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

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

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

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

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

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

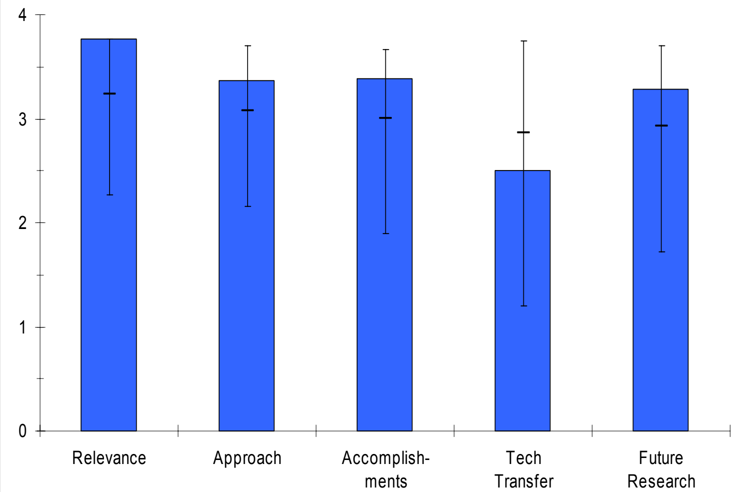
Project # PD-01: Low-Cost Hydrogen Distributed Production Systems

Frank Lomax, Jr.; H2Gen Inno. Inc.

Brief Summary of Project

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

Overall Project Score: 3.4 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

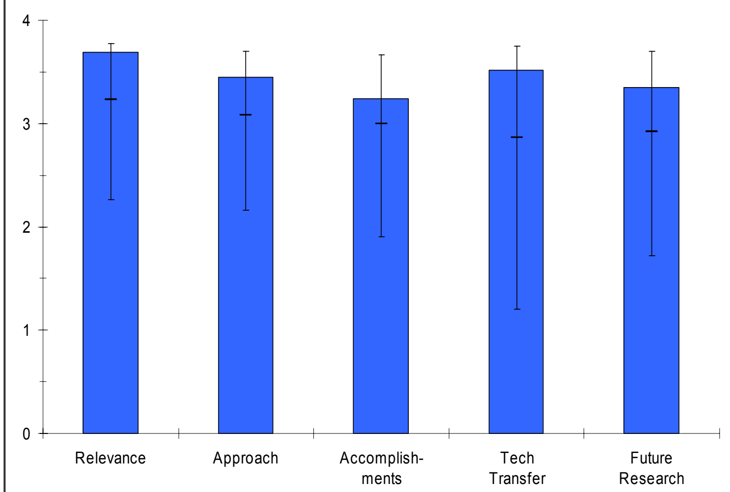
Specific recommendations and additions or deletions to the work scope

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

Project # PD-02: Integrated Hydrogen Production, Purification & Compression System
Satish Tamhankar; BOC Group, Inc.

Brief Summary of Project

Overall Project Score: 3.4 (8 Reviews Received)



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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

Strengths

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

Weaknesses

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

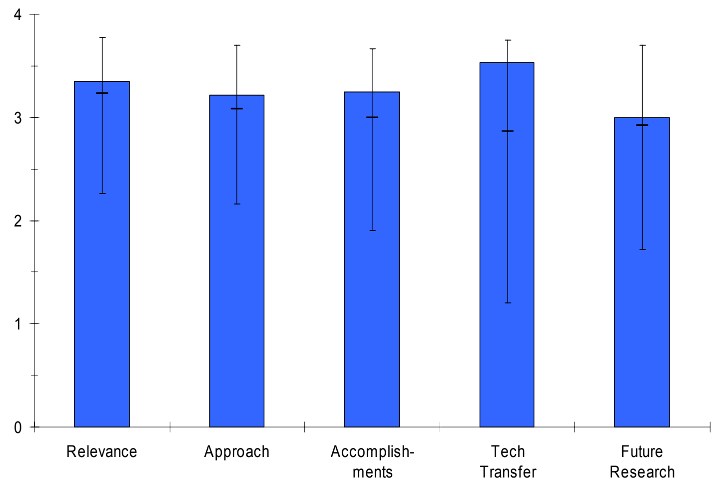
Specific recommendations and additions or deletions to the work scope

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

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

Ke Liu; GE Global Res.

Overall Project Score: 3.3 (6 Reviews Received)



Brief Summary of Project

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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

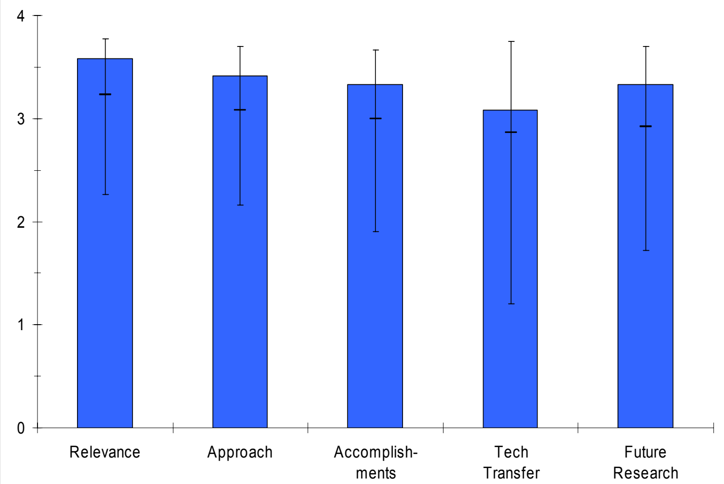
Specific recommendations and additions or deletions to the work scope

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

Project # PD-04: Hydrogen Generation from Biomass-Derived Carbohydrates via Aqueous-Phase Reforming Process
Randy Cortright; Virent Energy Sys.

Brief Summary of Project

Overall Project Score: 3.4 (6 Reviews Received)



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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

Strengths

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

Weaknesses

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

Specific recommendations and additions or deletions to the work scope

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

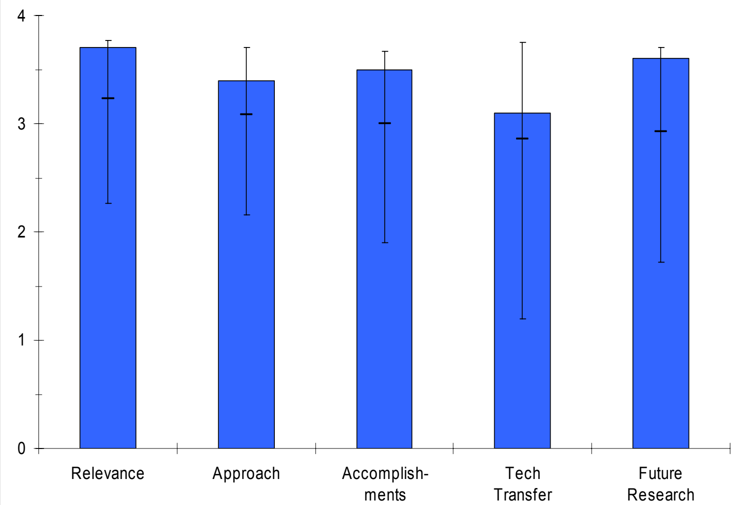
Project # PD-05: Distributed Bio-Oil Reforming

Bob Evans; NREL

Brief Summary of Project

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

Overall Project Score: 3.5 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

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

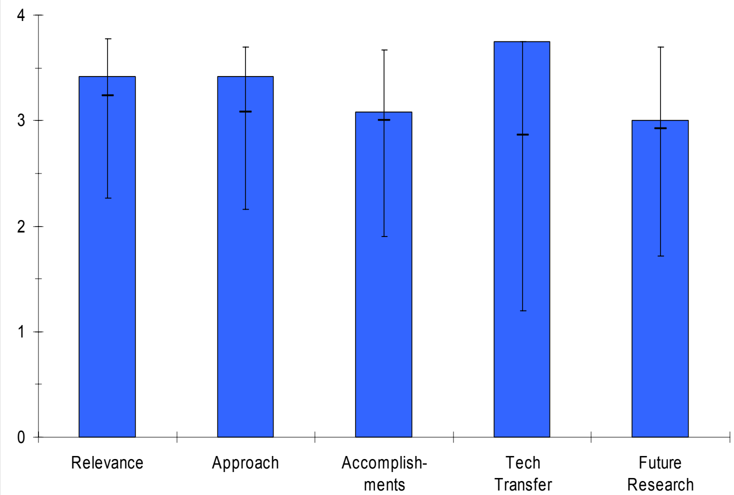
Project # PD-06: Photoelectrochemical Hydrogen Production: UNLV-SHGR Program Subtask

Eric Miller; U of Nevada

Brief Summary of Project

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

Overall Project Score: 3.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

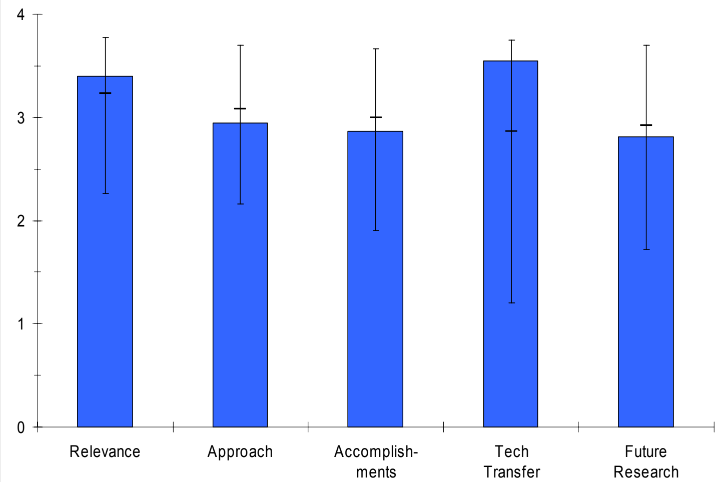
- Evaluate the feasibility of engineering a workable H_2 generation system based on this technology (including compression, safety analysis etc).
- Add to plans for future work on Materials Durability.

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

Ben Kroposki; NREL

Brief Summary of Project

Overall Project Score: 3.0 (8 Reviews Received)



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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Weaknesses

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

Specific recommendations and additions or deletions to the work scope

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

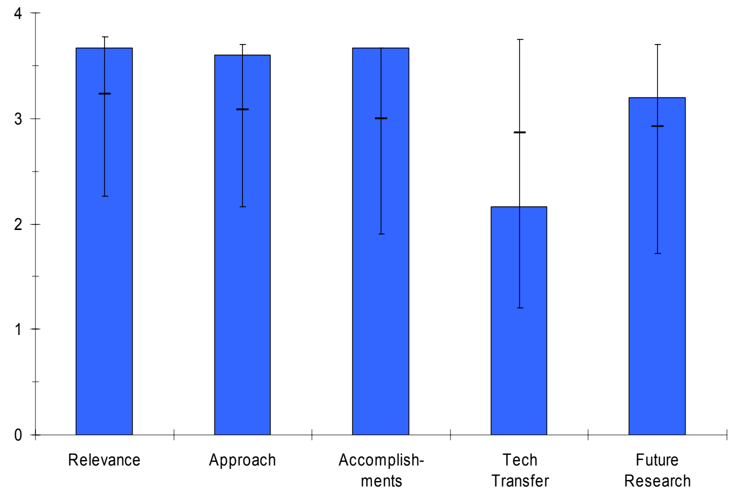
Project # PD-08: Advanced Alkaline Electrolysis

Richard Bourgeois; GE Global Res.

Brief Summary of Project

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

Overall Project Score: 3.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

Strengths

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

Weaknesses

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

Specific recommendations and additions or deletions to the work scope

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

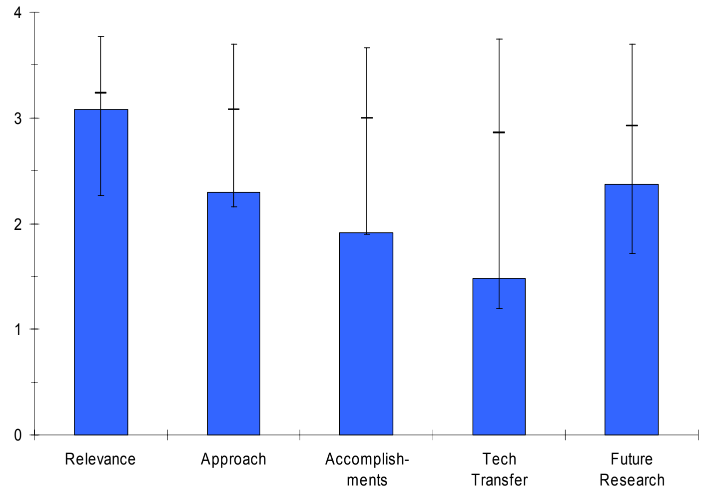
Project # PD-09: Alkaline, High Pressure Electrolysis

Samir Ibrahim; Teledyne

Brief Summary of Project

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

Overall Project Score: 2.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

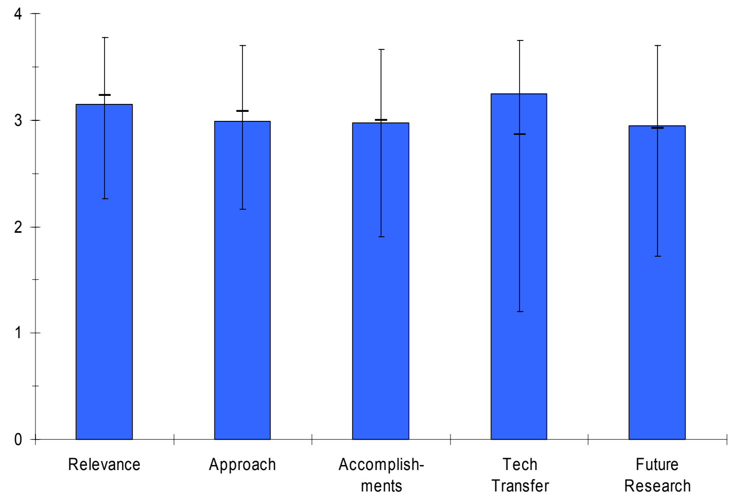
Specific recommendations and additions or deletions to the work scope

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

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

Alan Weimer; U of Colorado

Overall Project Score: 3.0 (8 Reviews Received)



Brief Summary of Project

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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

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

Specific recommendations and additions or deletions to the work scope

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

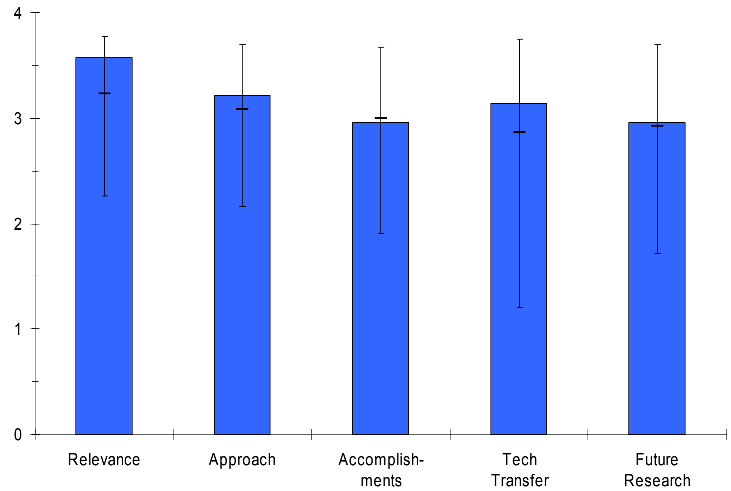
Project # PD-11: Hydrogen Delivery Infrastructure Options Analysis

Bruce Kelly; Nexant Inc.

Brief Summary of Project

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

Overall Project Score: 3.1 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

Weaknesses

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

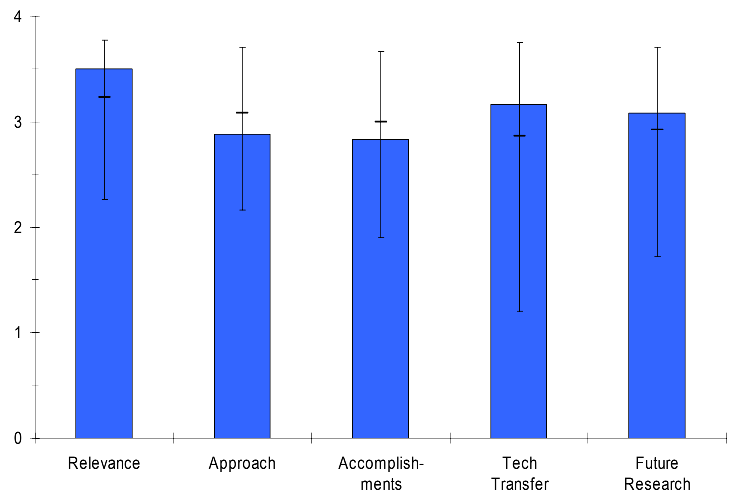
Specific recommendations and additions or deletions to the work scope

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

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

Rod Judkins; ORNL

Overall Project Score: 3.0 (6 Reviews Received)



Brief Summary of Project

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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

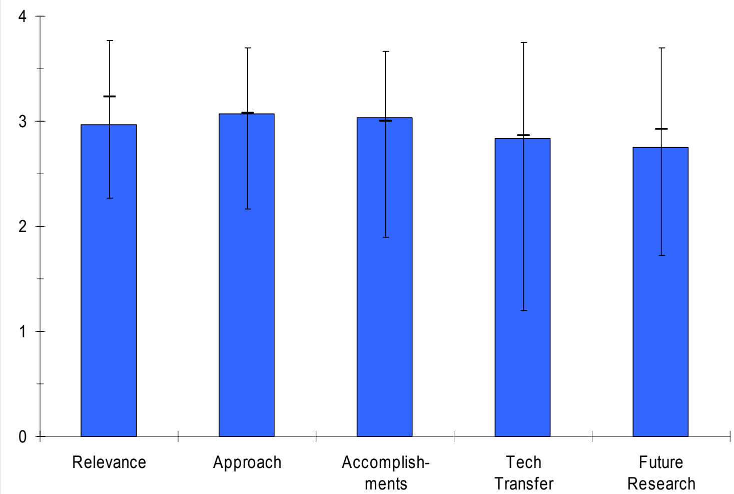
Specific recommendations and additions or deletions to the work scope

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

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

Paul Grimmer; Eltron Research Inc.

Overall Project Score: 3.0 (6 Reviews Received)



Brief Summary of Project

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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

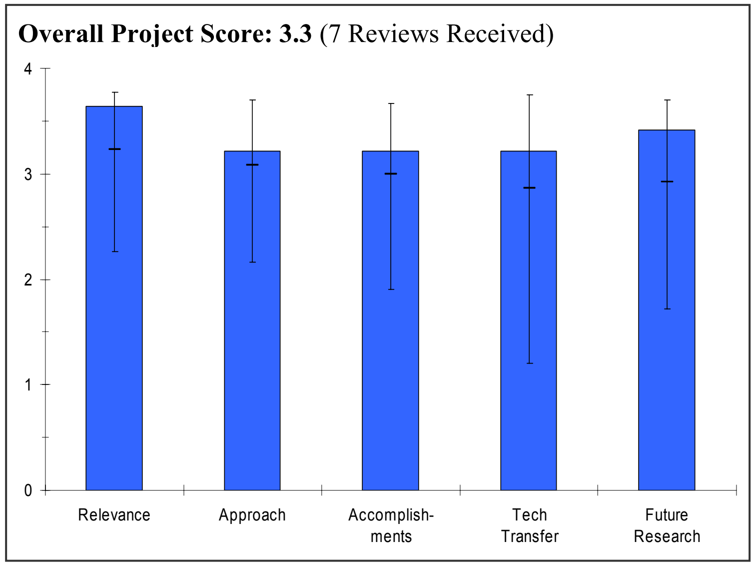
Specific recommendations and additions or deletions to the work scope

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

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

James Arps; SwRI

Brief Summary of Project



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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

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

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

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

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

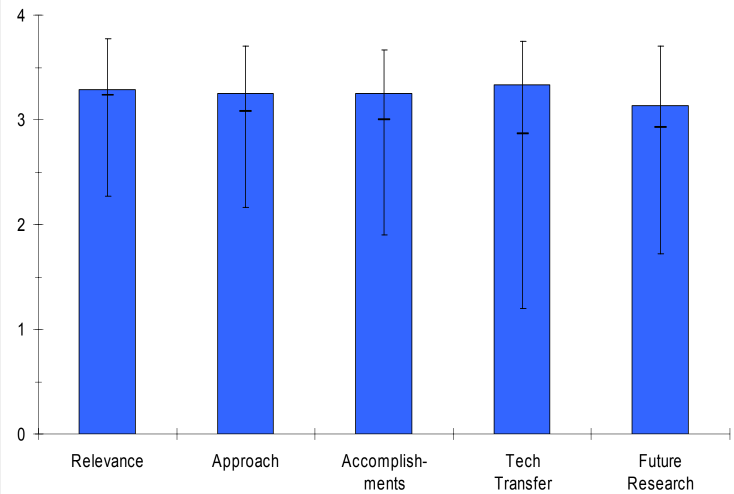
Project # PD-15: Sulfur-Iodine Thermochemical Cycle

Paul Pickard; SNL/INL/GA

Brief Summary of Project

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

Overall Project Score: 3.3 (9 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

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

Specific recommendations and additions or deletions to the work scope

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

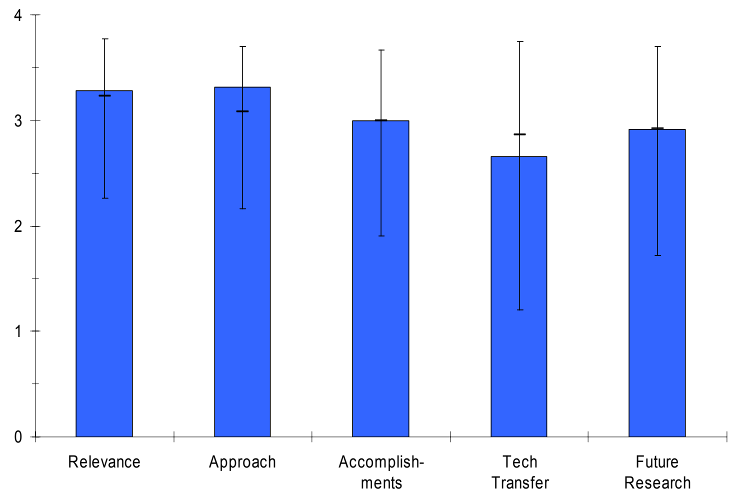
Project # PD-16: Evaluation of a Continuous Calcium-Bromine Thermochemical Cycle

Richard Doctor; ANL

Brief Summary of Project

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

Overall Project Score: 3.1 (9 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

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

Specific recommendations and additions or deletions to the work scope

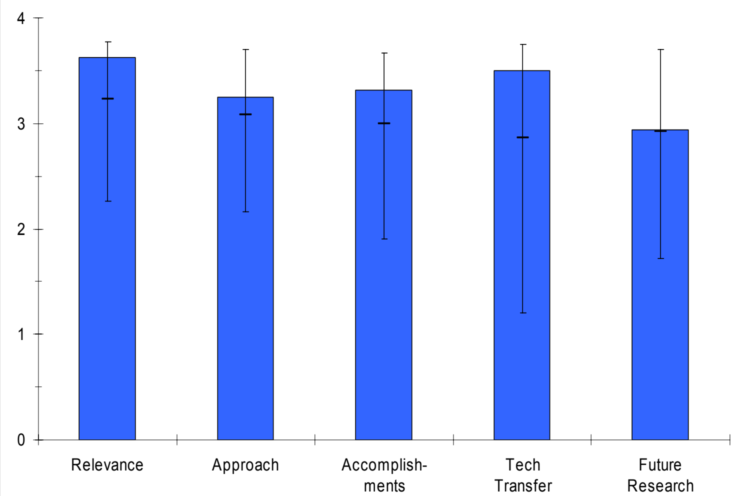
- Serious effort must be focused on using an electrolyzer for HBr dissociation.
- Future work should have been discussed more explicitly.
- Unless the PI can indicate a current commercial plasma process to make a commodity chemical, the plasma reactor option should be deleted from the project. This indicates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

Project # PD-17: Laboratory-Scale High-Temperature Electrolysis System
Steve Herring; INL/ANL/Ceramatec

Brief Summary of Project

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

Overall Project Score: 3.3 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

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

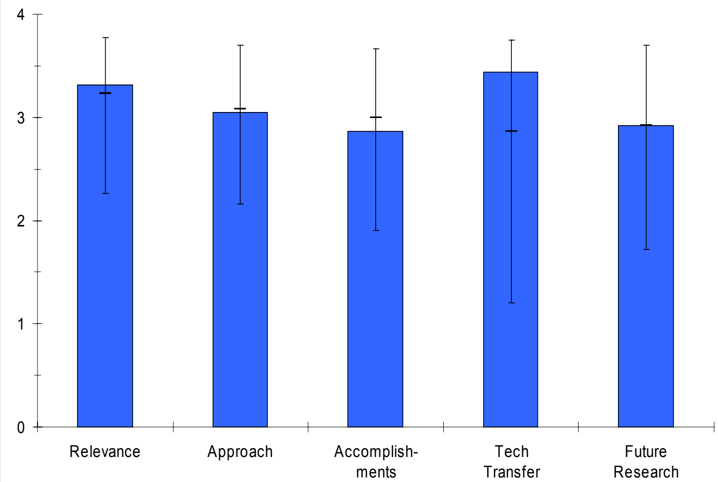
Project # PD-18: Nuclear Reactor/Hydrogen Process Interface

Steve Sherman; INL

Brief Summary of Project

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

Overall Project Score: 3.1 (9 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

Strengths

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

Weaknesses

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

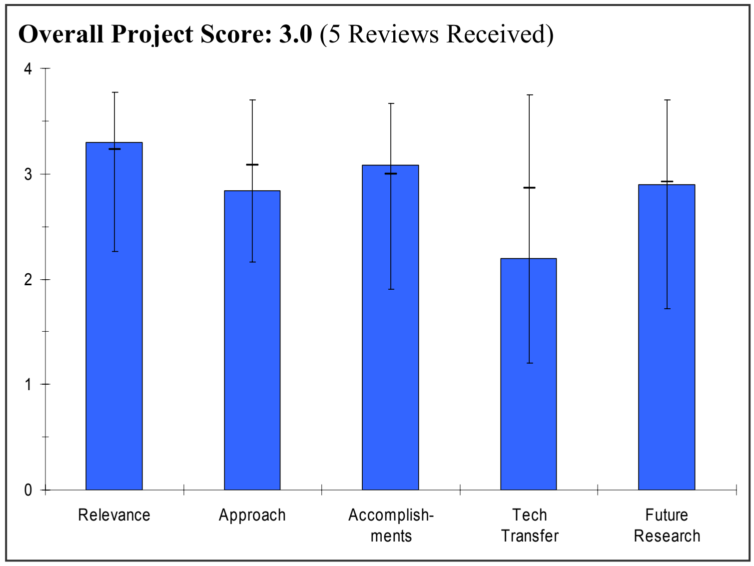
Specific recommendations and additions or deletions to the work scope

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

Project # PDP-02: Low-Cost, High-Pressure Hydrogen Generator
Cecelia Cropley; Giner Electrochemical

Brief Summary of Project

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



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

None provided.

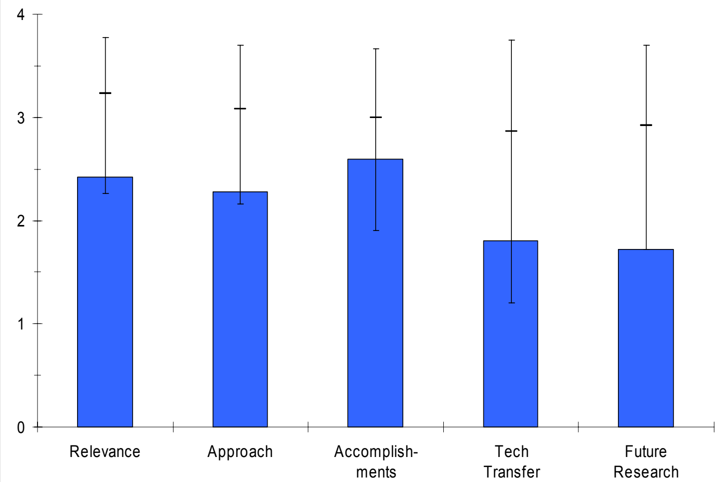
Project # PDP-06: Adapting Planar Solid Oxide Fuel Cells for Distributed Power

Andres Marquez; Ohio University

Brief Summary of Project

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

Overall Project Score: 2.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

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

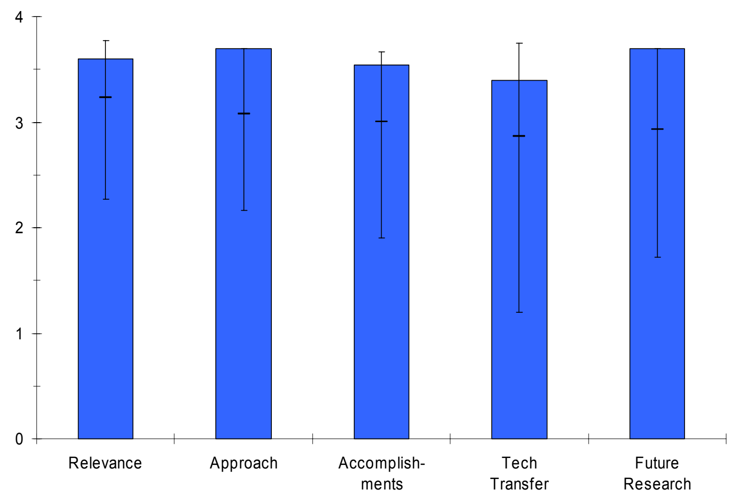
Project # PDP-08: Zeolite Membrane Reactor for Water-Gas-Shift Reaction for Hydrogen Production

Jerry Y.S. Lin; U of Cincinnati

Brief Summary of Project

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

Overall Project Score: 3.6 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

Strengths and weaknesses

Strengths

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

Weaknesses

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

Specific recommendations and additions or deletions to the work scope

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

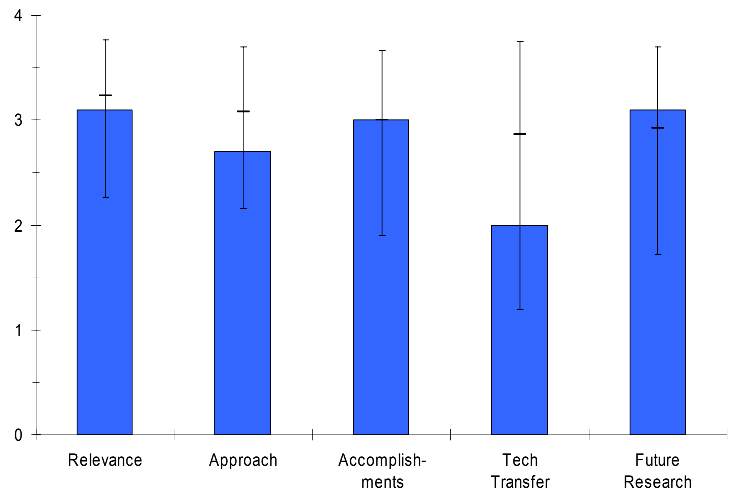
Project # PDP-10: Startech Hydrogen Production

David Lynch; Startech Environmental

Brief Summary of Project

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

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

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

Specific recommendations and additions or deletions to the work scope

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

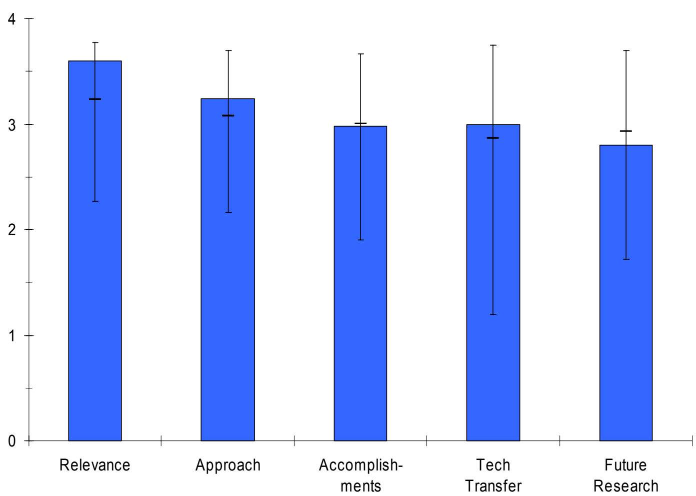
Project # PDP-12: Critical Research for Cost-Effective Photoelectrochemical Production of Hydrogen

Liwei Xu; Midwest Optoelectronics

Brief Summary of Project

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

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

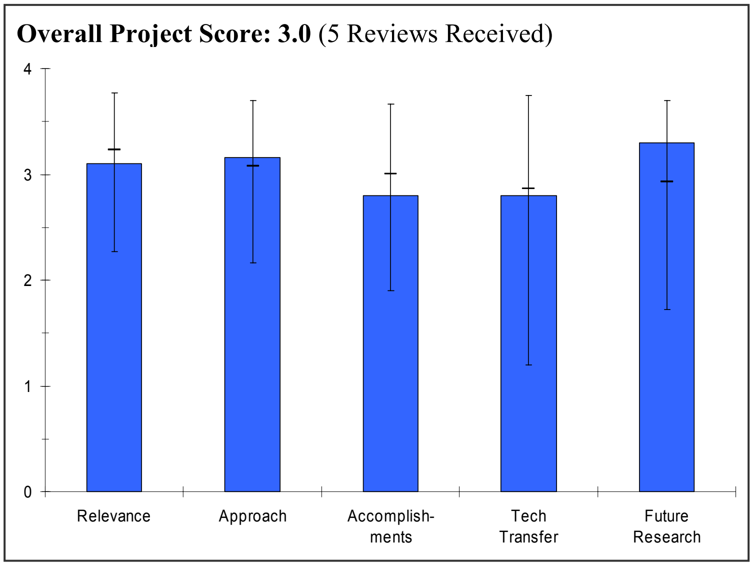
- Develop plan for increased focus on system and durability testing.
- Develop clear goals and targets for durability, performance, and costs that allow the developing technology to be evaluated in the context of DOE goals and competing technology.

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

Neal Woodbury; Arizona State U

Brief Summary of Project

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



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

Weaknesses

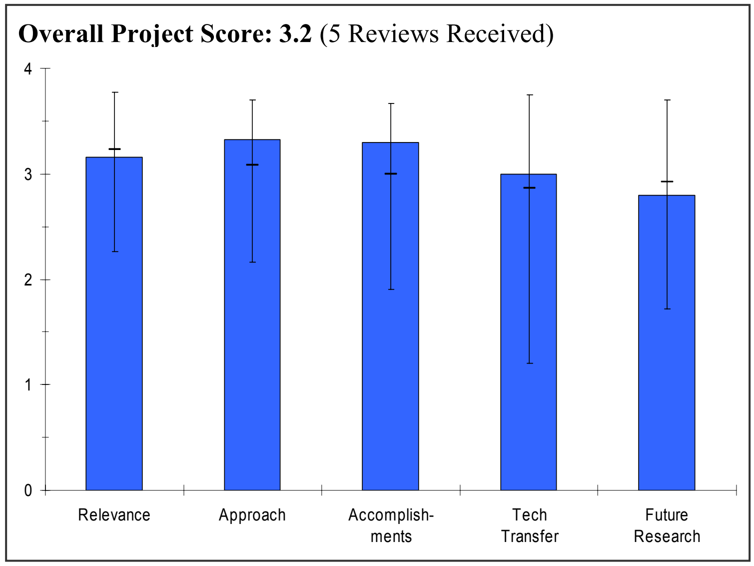
- Ultimate scale-up of the process to significant levels of hydrogen production may be difficult.
- The specific context of being able to generate water splitting with this approach is uncertain. Limited collaborations with groups outside of the university.
- The only weakness in this project is the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

Specific recommendations and additions or deletions to the work scope

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

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

Alan Weimer; U of Colorado



Brief Summary of Project

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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

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

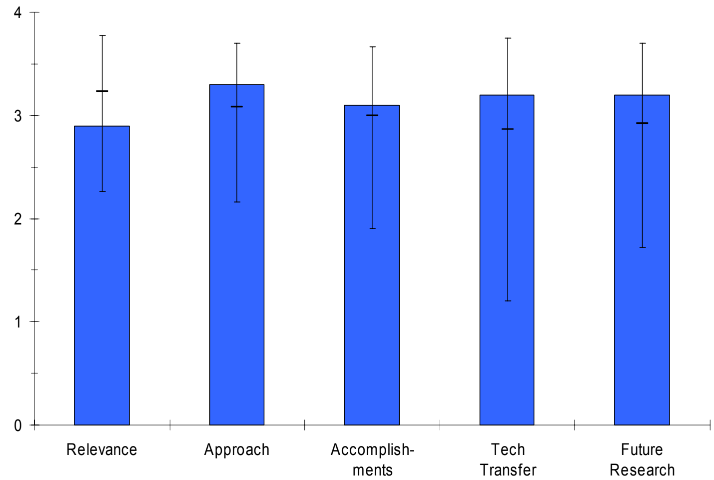
Project # PDP-16: Hydrogen Embrittlement of Pipeline Steels: Causes & Remediation

Petros Sofronis; U of Illinois

Brief Summary of Project

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

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

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

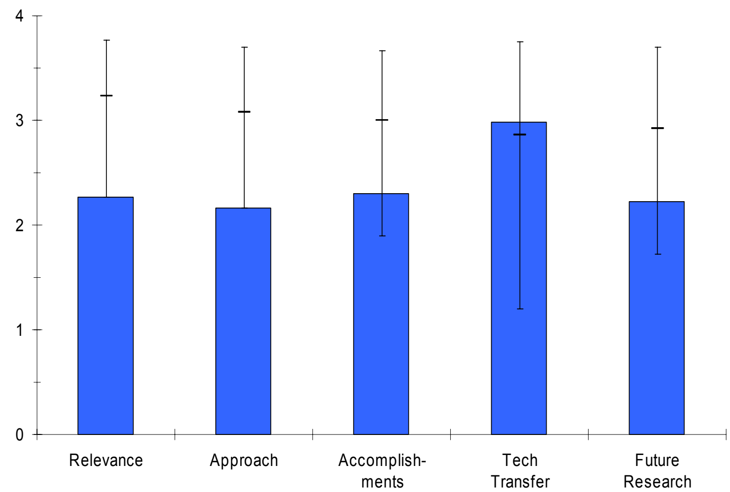
Project # PDP-17: Hydrogen Regional Infrastructure Program in Pennsylvania

Linda Eslin; Concurrent Tech. Corp

Brief Summary of Project

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

Overall Project Score: 2.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

- Industry partners are active in hydrogen business.

Question 5: Approach to and relevance of proposed future research

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

- Goals do not seem to be clearly spelled out.

Strengths and weaknesses

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

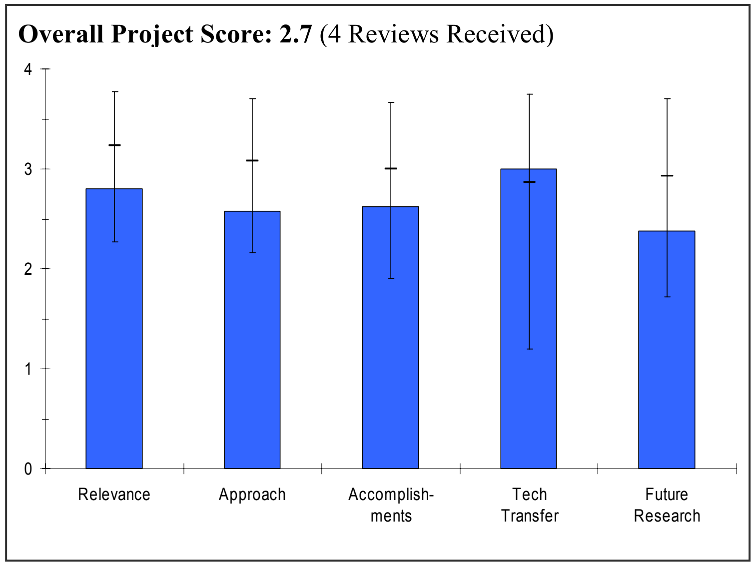
Weaknesses
- Not a good value. Funding is breath-takingly high for what has been accomplished.
- Analysis supporting objectives "Quantify tradeoffs between alternative hydrogen (H_2) production and delivery approaches" and "Determine most economic delivery scenarios. . ." was limited in scope: study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

Specific recommendations and additions or deletions to the work scope

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

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

Michael Martin; Edison Materials Tech Center



Brief Summary of Project

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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

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

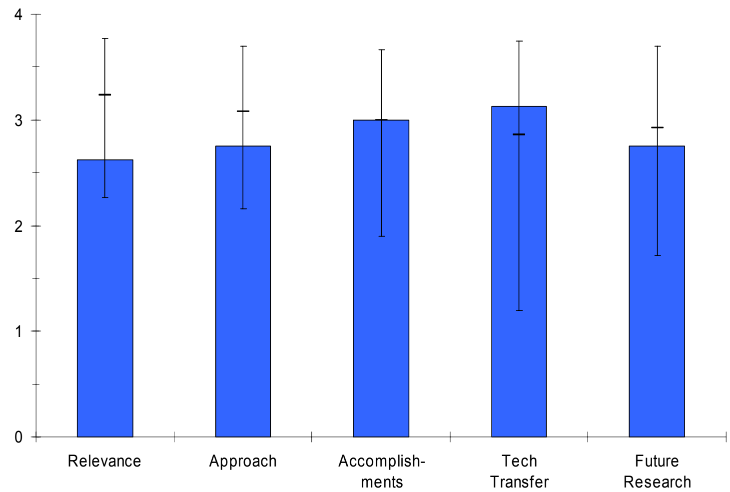
Project # PDP-31: EVermont Renewable Hydrogen Fueling System

Tom Maloney; Northern Power Sys.

Brief Summary of Project

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

Overall Project Score: 2.9 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

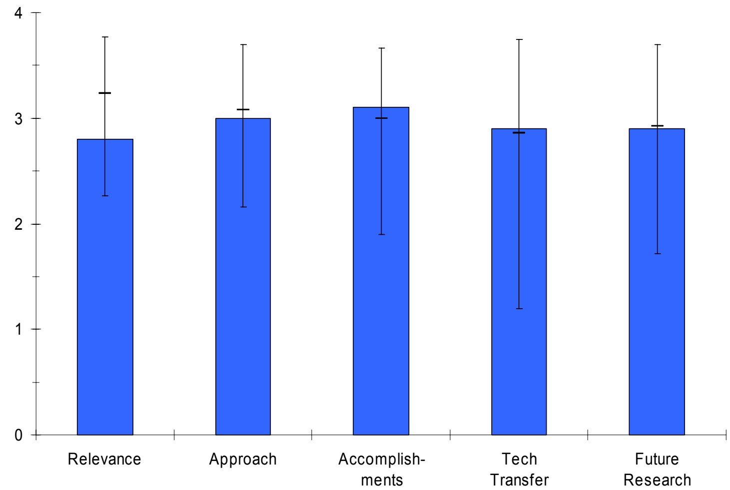
Specific recommendations and additions or deletions to the work scope

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

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

Greg Tao; Materials and Systems Research

Overall Project Score: 3.0 (5 Reviews Received)



Brief Summary of Project

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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

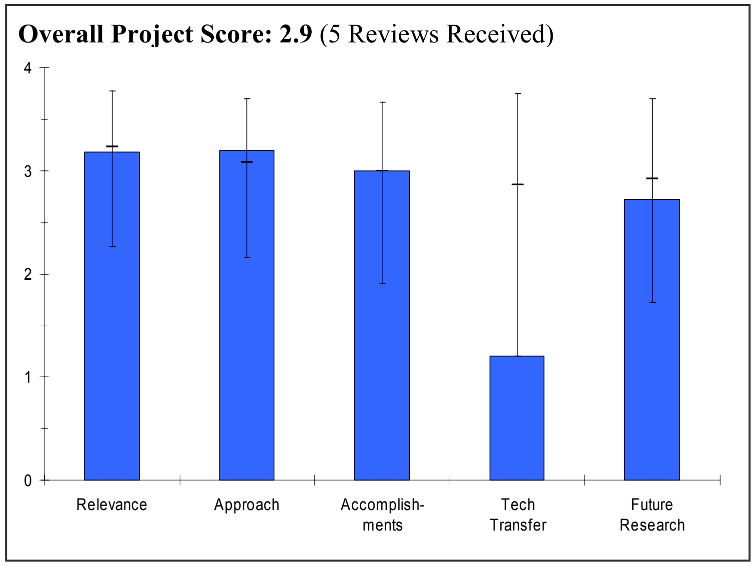
- Address sealing issues in stack development. Finish materials effort before proceeding to large stack demonstration. Enough funding should be allotted only to fund materials development and short stack (multiple cells, say three) long term demonstration. Project should be given one more year of adjusted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

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

Nguyen Minh; GE HPGS

Brief Summary of Project

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



Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

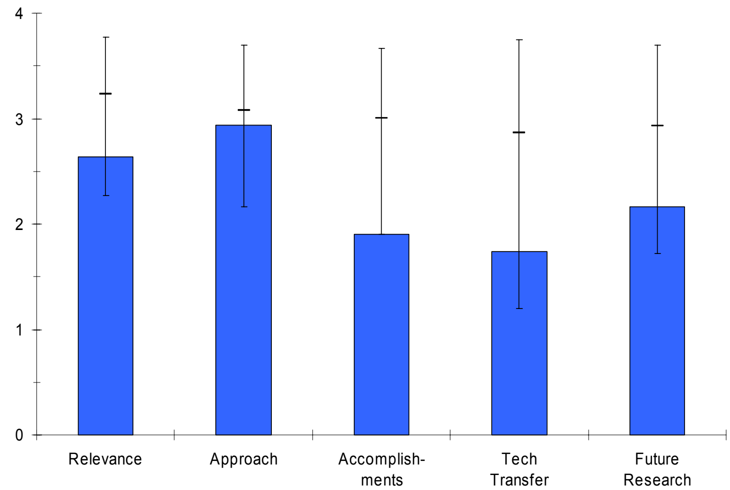
Specific recommendations and additions or deletions to the work scope

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

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

Daniel Graves; Nisource Energy Tech.

Overall Project Score: 2.3 (5 Reviews Received)



Brief Summary of Project

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

Question 1: Relevance to overall DOE objectives

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

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

Question 2: Approach to performing the research and development

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

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

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

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

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

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

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

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

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

Question 5: Approach to and relevance of proposed future research

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

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

Strengths and weaknesses

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

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

Specific recommendations and additions or deletions to the work scope

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