

2007

Hydrogen Production and Delivery

Summary of Annual Merit Review Hydrogen Production and Delivery Subprogram

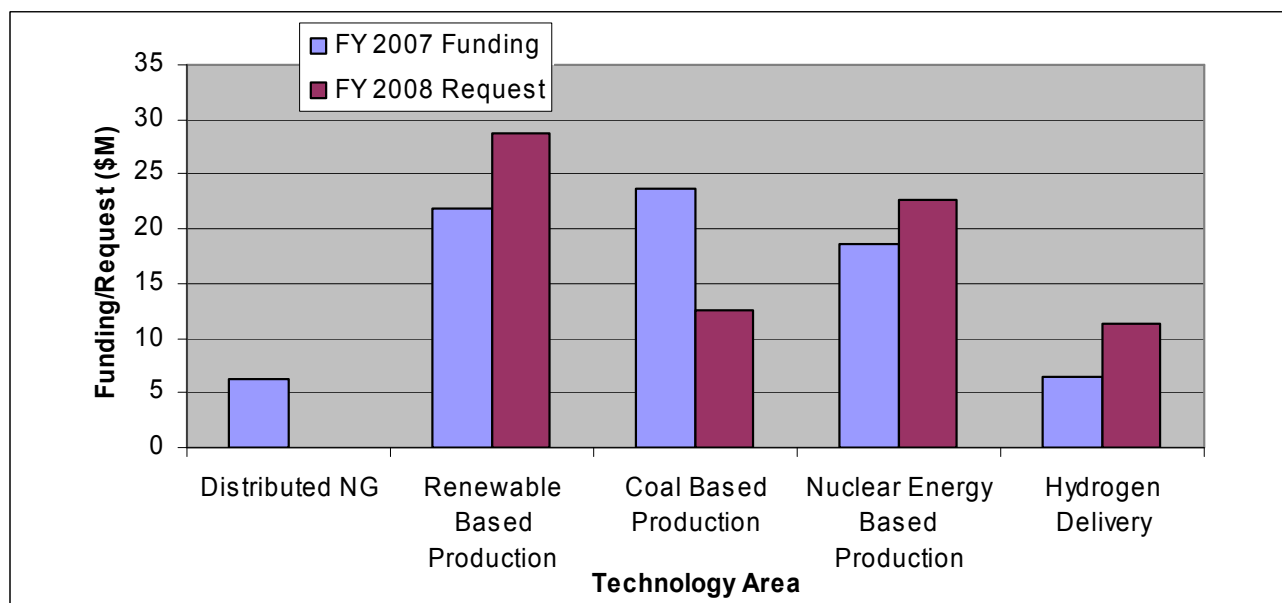
Summary of Reviewer Comments on Hydrogen Production and Delivery Subprogram:

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 Fossil Energy, Nuclear Energy, and Energy Efficiency and Renewable Energy. The production and delivery projects are generally considered to be well-aligned with the goals and objectives of the Hydrogen Program.

The production projects include diverse energy sources and technologies for hydrogen production including natural gas reforming, water electrolysis, bio-derived renewable liquids reforming, biomass gasification, solar-driven thermochemical cycles, nuclear-driven thermochemical cycles, photoelectrochemical direct water splitting, biological hydrogen production, and hydrogen production from coal. The projects were judged to have made considerable progress, despite a low level of funding in some areas. The major concerns identified in some projects 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 and other technical information is needed to assess progress; and, 4) specific go/no-go decision points are needed on some projects.

The delivery projects reviewed included the next stage of development of the H2A Delivery analysis models, and several of the key hydrogen pipeline research efforts. The reviews were very complimentary of the advances made in the H2A models and recognized significant and very relevant progress in the pipeline research despite limited funding.

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. Recommendations and major concerns are summarized below.

Distributed Natural Gas and Bio-Derived Liquids Reforming: Reviewers acknowledged that low cost forecourt (on-site) units are critical to meeting DOE Hydrogen Production objectives during the transition to use of hydrogen as a major energy carrier. In addition to reducing the footprint, it was stated that a remaining challenge for technology developers is to scale-up these highly efficient, small capacity reformers to achieve economies of scale that will enable the technology to reach the DOE target for distributed natural gas of \$2-\$3/gge by 2015. New technology being developed for distributed reforming from bio-derived liquids (e.g. ethanol, sugars) will build on distributed reforming from natural gas technology while helping solve outstanding issues with the on-site hydrogen production to reach the bio-derived liquids cost goal of \$3.00/gge by 2017. While there were mixed reviews on the ability of the U.S. to provide sufficient bio-derived feedstocks for transportation energy needs, the majority were highly in favor of pursuing bio-derived liquids to hydrogen, praising technology efforts in distributed gas phase and aqueous phase reforming.

Electrolysis: Projects in electrolysis development received generally favorable reviews. The reviewers identified electrolysis using renewable energy as “one of the two most viable options for hydrogen production in the near term.” 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. They stressed the need for decreasing the cost related to these systems, increasing system reliability and performance, validation of models, and continuing the work of integrating the electrolysis units with renewable energy sources such as wind power. Successful completion of these projects may yield multiple options for efficiently producing hydrogen from water and renewable energy at economically acceptable costs.

Biomass Gasification: Two projects in this area were reviewed; one researching the potential to integrate a hydrogen membrane in a biomass gasifier for process intensification and thus capital cost reduction; the other researching the potential of central plant, low temperature, single step, aqueous phase reforming of hydrolyzed biomass. Both projects received below average scores based on the fact that the projects are high risk compared to standard biomass gasification. These projects are just being initiated. If successful, either could substantially reduce the cost and complexity of central hydrogen production from biomass. They include early decision points for continuation of the work based on their risk level and progress.

Solar-Driven High Temperature Thermochemical: The large collaborative project reviewed in this area received very favorable comments. It was viewed as extremely relevant to the Program goals for renewable-based hydrogen production. There was significant research progress on the five down-selected thermochemical cycles. The reviewers urged the project to follow their plan to further down-select to 1-3 cycles in FY08 and encouraged additional work on materials research, and heliostat cost reduction.

Photoelectrochemical Hydrogen Production: The teaming approach in some of the projects in this area was specifically called out by the reviewers as effective and necessary to achieve the DOE targets. Several of the projects received high ratings from the reviewers. Nearly all 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, and that down-select decision points and criteria be established.

Biological Hydrogen Production: The projects in this area were rated very high and the general conclusion from the reviewers is that the researchers are moving toward the DOE goals in this long-term renewable hydrogen production area. Some reviewers suggested increased collaborations with industry to apply the exploratory results obtained from this project.

Separations: Reviewers commented, similar to prior year reviews, that there is a great need for investigators to test their hydrogen separation and purification membranes using realistic, mixed gas streams. The potential for membrane technology to reduce the on-site hydrogen production footprint (by eliminating PSA) and reduce capital costs were frequent comments. The ability of the research to be applicable to many hydrogen production technologies was viewed by many reviewers as advantageous. Some reviewers thought that technology advances in separations are not occurring quickly enough.

Hydrogen from Coal: The projects reviewed in this area received mostly favorable ratings from the reviewers. Reviewers observed that the projects were in alignment with the DOE Hydrogen Fuel Initiative and Hydrogen from Coal Program goals and objectives. The projects had achieved good technical progress and their overall research approach was determined to be solid. Reviewers observed that some projects needed to improve or highlight their technology transfer and partnership activities. Also, reviewers suggested that some projects need to focus on tests using more representative mixed gas streams to address issues regarding real-world applications

Hydrogen Production Using Nuclear Energy: In general, the projects reviewed in this area were scored favorably. 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. Economic analyses and high-level assessments of licensing issues were also recommended as areas for future efforts.

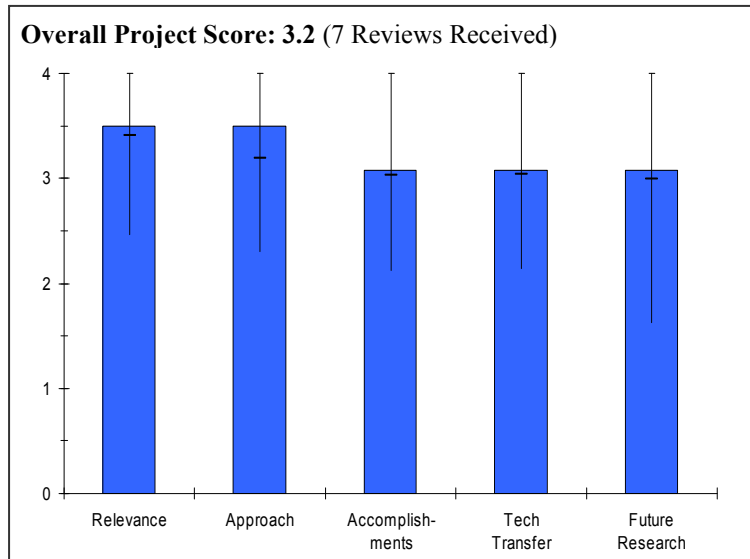
Hydrogen Delivery: All but one of the projects reviewed in this area received above average ratings. The reviews recognized significant and very relevant progress in the pipeline research despite limited funding. They complimented the broad spectrum of collaboration across industry, national labs, and universities as well as a good mix of theory, modeling, and experimental work. There was enthusiasm for the fiber reinforced pipeline project as being a step out approach that could significantly lower capital cost and avoid the embrittlement concern of steel pipelines. Stronger and broader collaboration and a more inclusive set of testing were suggested for this particular project. Reviewers requested that one project improve alignment and reduce duplication with other current program activities.

Project # PD-01: Low Cost Hydrogen Production Platform

Tim Aaron; Praxair

Brief Summary of Project

Praxair is developing an integrated system for the turnkey production of hydrogen at 4.8 kg/h for transportation and industrial applications. The design is based on existing steam methane reforming technology and existing chemical processes/technologies to meet the design objectives. The baseline design, therefore, consists of a steam methane reformer, pressure swing adsorber system for hydrogen purification, natural gas compression, steam generation and all components and heat exchangers required for the production of hydrogen. The focus of the project emphasizes packaging, system integration and an overall step change in the cost of capital required for the production of hydrogen on site. One objective of the project is to approach the DOE hydrogen cost goal of \$1.50-\$2.00/kg (production only).



Question 1: Relevance to overall DOE objectives

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

- Low cost forecourt units are critical to meeting DOE objectives.
- Strongly supports near term goal for natural gas distributed reforming; Phase 3 necessary for evaluation of technology in an overall, integrated system.
- Directly addresses production program goal of reducing cost of distributed natural gas reforming via capital cost reduction.
- Program clearly addresses the goals of the DOE H₂ Program.
- Design for manufacturing has been implemented well to reduce capital cost.
- Good design for turndown to support market demand.
- Good concept with small footprint for distributed applications.
- Addresses several technical barriers identified by DOE in the areas of hydrogen production and technical validation.
- Supports distributed production of hydrogen for transportation application.

Question 2: Approach to performing the research and development

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

- Approach is well designed.
- Drawn out schedule may cause inefficiencies.
- Use of BDI and manufacturing contractor rounds out team.
- Work on codes and standards ISO committee augments knowledge.
- Good approach; good effort reducing part count and manufacturing cost; questionable scale-up to 1500 kg/day.
- Solid approach designed for success.
- Sound engineering, catalysis, and packaging to produce a product that should perform.
- Capacity is small compared to new DOE production program target...but they are building what they were contracted to build...and demonstrated in the presentation that there might be an important niche for 120 kg/day hydrogen generators for smaller stations in the early stages of infrastructure rollout.

- Approach is well thought out and comprehensively addresses safety, construction and operation.
- Good use of off-the-shelf technology (i.e., commercially available burner instead of custom design). Integrating off the shelf technology saves time, money and increases reliability.
- In a growing market, makes sense to have small unit and add units as demand increases.
- Unclear approach for 700 bar compression needed more information.
- Toxic emissions from reformer due to lower residence time were not discussed.
- Unclear who will commercialize, provide service, and whether this is a licensing application.
- The process is quite standard and would serve well as a demonstration project.
- Focus on manufacturability is a good strategy effectively addressing the manufacturing barrier.
- Tried to address the reformer capital costs. The projected cost estimate of \$2.75/Kg of H₂ is on the higher side of DOE cost goals.
- Plan to address codes & standards but not much is done.

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

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

- Using commercial catalyst.
- Not completely proven, but if catalyst is shown to be good for full 7.5 yr design life, then it would be a significant design feature.
- More data (beyond costs) compared to goals would be helpful; Need demonstration phase for insight into performance parameters (reliability, durability, etc.).
- Work appears to be on track. Their cost models indicate they might meet DOE targets even with systems that have this low capacity.
- Skid appears to be well designed; easy access to components for ease of maintenance.
- Significant accomplishments achieved including approaching H₂ cost, durability.
- Good accomplishment on catalyst selection. Unclear about the long term reliability of the PSA unit at the 50 percent turndown rate. More accelerated testing should be done to verify performance and cost values.
- Reformer and shift catalyst testing data has been shown but it is not very revealing of progress. H₂ yield data, durability, space velocity, durability, etc., would be more meaningful.
- Cost of hydrogen has been estimated but does not include compression costs.
- Completed techno-economic study, proof-of-concept component testing and most of lab-scale testing.
- Full-scale test apparatus constructed. Off-the shelf components used wherever possible & highly integrated system (addresses the fuel processor capital costs barrier). Risk analysis completed (Control and Safety barrier addressed).

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

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

- Minimal collaborations; much focus on Praxair.
- Good coordination of work with component vendors and suppliers. Collaborated with Boothroyd Dewhurst and Diversified Manufacturing to develop component selection and manufacturing strategies to get parts count and manufacturing costs down.
- Good to see collaboration with two experienced companies on design and construction, otherwise appears to be light on collaboration and technology transfer.
- Coordination, but insufficient info on how it's used to deal with cost or performance problems.
- They are working with commercial catalyst vendors and manufacturing.
- They have a manufacturer on the team.
- Boothroyd Dewhurst is in charge of system optimization. Diversified Manufacturing is in charge of manufacturing the skid and prototype.
- Praxair has good track record in commercializing technologies. Praxair is the only U.S. H₂ supplier in all sizes (cylinders to liquid to pipelines).
- Praxair has smallest industrial SMR-based product line. They have designed and built over 300 PSA H₂ units.

Question 5: Approach to and relevance of proposed future research

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

- A demonstration phase would be useful; especially with start-up/shutdowns, transients, etc; daily usage profile important as system appears designed for steady-state performance; no discussion of role of storage in Phase 3.
- Project is well planned; future proposed work will add great value to achieving future commercialization.
- Unclear how reformer will perform with normal variance of natural gas composition, especially if higher levels of carbon dioxide.
- Presumably the team has enough experience in hydrogen production using this standard process. Unclear why the team would spend resources on natural gas compression and high temperature materials.
- This reviewer was confused regarding the plan for testing of components/proof of design. At first this reviewer was led to believe that the proof-of-concept component testing was completed during Phase I of this project. Unclear why this would be done again.
- This team plans to perform comparative analysis with supply alternative (considers contingencies).

Strengths and weaknesses

Strengths

- Project is right on target for what needs to be done for low cost forecourt NG reformer systems.
- DFMA methodology is good to employ.
- Good effort reducing parts and designing for manufacturability; good cost reduction.
- Good use of existing technology to address near term transition to H₂.
- Strong team with strong technical performers
- Used past failures on small reformer technologies to develop an impressive concept.
- Good management visibility.
- This is a demo of a pretty standard process that is most likely to be commercialized in the near-term, and so it is an important project for data generation.
- Praxair has experience in smallest industrial SMR-based product line. They have considerable experience with PSA H₂ units.

Weaknesses

- Too little detail is in presentation: Unclear about efficiency, life (cycle) testing, and the capital cost not just the bottom line cost of hydrogen.
- Questionable scale-up to 1500 kg/day design; proposed solution of multiple units could result in reliability issues (undoes effort to reduce part count).
- Commercialization plan and market study needed.
- No real data. Catalyst performance data could be more elaborate. There should be more information contributed to the public domain.
- Co-content in H₂ was not addressed properly. The purity of hydrogen may not be suitable for PEM application.
- Thermal cycling will be a problem for this small reformer.

Specific recommendations and additions or deletions to the work scope

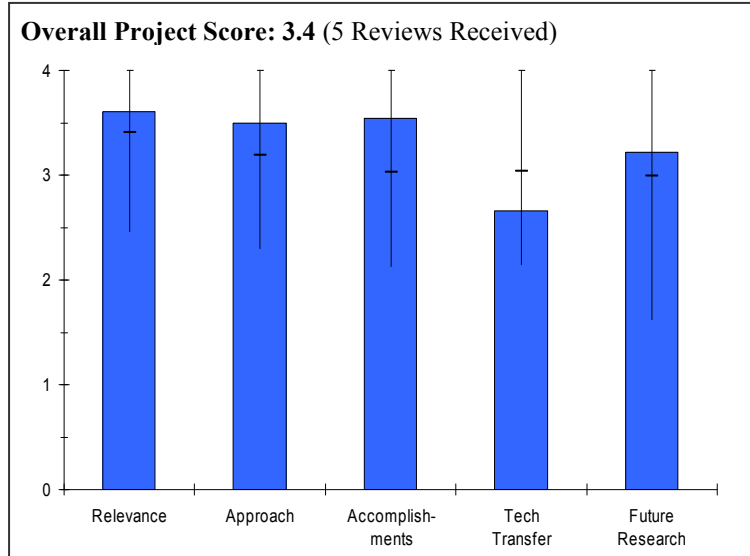
- As part of a demo, test under daily usage profile similar to H₂A assumption.
- Adding the demonstration of 700 bar hydrogen is a good addition of project scope in that it pushes the envelope of standard practice and will generate data on cost of the product and the overall process efficiency at those conditions.
- Need to firmly establish ease of maintenance and operation to be accepted by industry.
- Based on other reformer projects more accelerated performance testing to validate small scale reformer/PSA unit cost and reliability.
- Need to document the contaminants, their level, and the impact on cost of hydrogen.
- Address CO₂ issue.
- Longer-term data are needed.
- Can this team increase the purity of the H₂ from their unit (CO less than ppm level)?

Project # PD-02: Low-Cost Hydrogen Distributed Production Systems

Frank Lomax; 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, to meet the DOE 2010 interim cost target of \$2.50/kg. A catalyst suite suitable for use with fuel grade ethanol to facilitate renewable hydrogen production is also being developed.



Question 1: Relevance to overall DOE objectives

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

- Strongly supports natural gas distributed reforming goals; more feedback concerning performance versus goals necessary.
- Project clearly meets the DOE Hydrogen Program targets.
- Good concept to achieve high reliability and cost.
- Meets the need for renewable hydrogen production from ethanol.
- Management understands and appreciates the requirement to achieve the DOE cost targets and is on track to meet them.
- Can be used for distributed and stationary applications.
- A demo of the natural gas to hydrogen process is appropriate for the near-term.
- H2Gen is not a catalyst company. Not clear about their novel concept for ethanol catalysis.
- This project addresses barriers such as fuel processor capital cost, manufacturing, O&M, feedstock issues, and control and safety in the Multi-Year RD&D plan.

Question 2: Approach to performing the research and development

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

- Development of natural gas reforming is well designed; addition of ethanol research, primarily the conversion from ethanol to methane is questionable.
- Well conceived, staged approach; sufficient testing of individual components.
- Good design and operations estimates.
- Could not tell if there were independent analyses on cost.
- Ethanol R&D looks promising.
- The process approach is good and makes sense for a refueling center.
- Fuel process capital cost, manufacturing costs, O&M, feedstock issues, and control and safety barriers are addressed in this project.
- This team has perfect approach.

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

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

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- Strong accomplishment towards achieving cost targets; more performance parameters versus goals required.
- Impressive accomplishment to complete 565 kg/hr platform.
- Good progress and cost reduction.
- Materials of construction - limited data on reliability.
- Compression related work not clear to achieve cost.
- Good performance of reformer as demonstrated by data.
- New PSA designed to increase capacity.
- Not much data to evaluate technical progress.
- Unclear on Slide 8, where a plot is shown without any y-axis, what is being measured.
- Cost analysis is promising.
- Ability to convert ethanol is not very meaningful. Unclear how much H₂ and what else is produced.
- This team has made excellent progress toward meeting their overall objectives.
- Based on test experience at 113 Kg/day scale, they have redesigned reactor and flowsheet for 565 Kg/day platform. This system has low pressure drop burner, compact and low stress steam generator and linear combustion air supply system.
- To reduce risk and cost this team has tested the burner, steam generator, and air supply system at full 565 Kg/day scale.
- Designed skid to industry standards.

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

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

- Minimal collaboration; nearly all technology in-house.
- Good coordination, but commercialization and service support were unclear.
- Team roles unclear once R&D goals are achieved.
- Unclear how IP shared in the next generation technology.
- Partners have been identified for future site location.
- Sud-Chemie is a good partner, possibly in catalysis research.

Question 5: Approach to and relevance of proposed future research

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

- Scale-up of approximately 115 kg/day unit to 565 kg/day will provide useful information and potential path to 1500 kg/day.
- Need to (eventually) have clear reasons/objectives for building the 2nd generation plant; should be based on a clear need with significant performance benefits.
- Good focus on research goals, accelerated reliability studies should be considered to validate longer term cost projections.
- The 565 Kg hydrogen generator will be tested at field site as well as building and testing a second 565 Kg hydrogen generator at a second field site.
- Based on test data, they will identify areas for improvement – excellent plan.
- In addition to the primary objective of reforming natural gas, H2Gen has secondary objective of using ethanol as a feedstock. They have plans to demonstrate the durability of ethanol reforming catalyst.
- They will make go/no-go decision based on durability tests.

Strengths and weaknesses

Strengths

- Simple design, skid mount approach useful.
- Good team with strong technical expertise. Past experiences have influenced current project and management oversight is good.

- Natural gas to hydrogen plant demonstration is promising with respect to cost targets.
- Very good project planning and execution.

Weaknesses

- Requires integration into overall production, compression, and storage system to determine transients, turndown, etc.
- Scale up from 113 to 567 was not clearly defined as to risk both on a market penetration basis and reliability of performance. Unclear whether the availability of components is within their current analysis or if a new evaluation will be undertaken since there is limited performance data available.
- Catalyst development strategy does not fit with this project.
- Unclear if it is necessary to pre-reform ethanol.
- No safety and code analysis for the installation of the hydrogen generator at the field site.

Specific recommendations and additions or deletions to the work scope

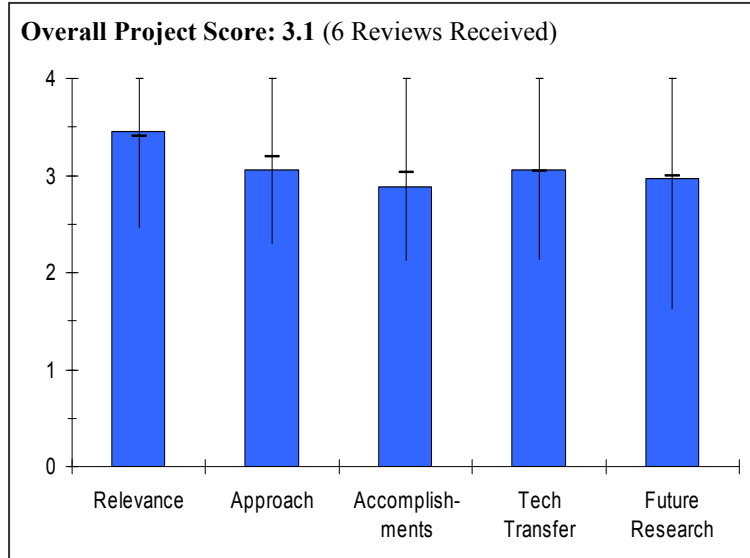
- Continue to complete this project to benchmark progress to goals.
- Add task to track hydrogen quality and its impact on cost of hydrogen.
- Maybe they should focus on their primary objective which is the development of hydrogen generator for distributed production of hydrogen using natural gas as feedstock. Would like to see more of these hydrogen generators built instead of redirecting their effort on reforming ethanol.
- It is a good idea for DOE to fund H2Gen for ethanol reforming.

Project # PD-03: Integrated Hydrogen Production, Purification & Compression System

Satish Tamhankar; Linde

Brief Summary of Project

The goal of this project is to demonstrate a low-cost option for producing fuel cell quality hydrogen that can be adopted to meet the ultimate DOE cost and efficiency targets for distributed production of hydrogen. 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 was 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 **3.5** for its relevance to DOE objectives.

- Success in the project would have significant positive effect on hydrogen cost.
- Novel approach; potential to simplify overall system; probable high risk.
- Project clearly meets the objectives of the DOE Hydrogen Program.
- Good to simplify process and reduce components.
- Capital cost reduction.
- Good solid state design compressor.
- Existing industry is important to DOE success if they will commercialize and service units.
- Addresses DOE identified technical barrier.
- Project is appropriate for near-term refueling centers.
- This project's objective is to develop a fuel processor system that directly produces high pressure, high-purity hydrogen from a single integrated unit and therefore has good relevance to overall DOE objectives.

Question 2: Approach to performing the research and development

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

- Not clear how waste heat of compressor is used to aid reformer.
- Minimal test data presented; only benchtop component testing to date.
- Novel approach to addressing the problem of H₂ production from CH₄.
- Well conceived plan to build an initial proof-of-concept prototype and then an advanced prototype based on learnings.
- A novel approach is only beneficial if there are clear advantages over conventional technology; need a head-to-head comparison with conventional technology.
- Good plan for hazmat and resulting improvement to safety.
- Higher costs due to design modification for hydride compressor.
- 10 week setback was unplanned? How did management deal with impact instead of sitting on their hands?
- Good progress on technical barriers.
- How much has been completed on market penetration studies?

- Novel approach offers opportunity for new technologies.
- H₂ removal membranes have not been demonstrated with high flux.
- H₂ permeating membranes yield low pressure hydrogen.
- Reforming at pressure with H₂ permeation can lead to carbon formation in the reformer unless operated at high steam to carbon ratio. The latter penalizes system efficiency.
- Metal hydride compressor is novel.
- Addresses fuel processor capital costs, O&M, and reliability and costs of hydrogen compression barriers by building an integrated fluidized bed reformer and metal hydride compressor system. Process intensification will bring the cost down to DOE target levels.

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

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

- Substantial achievement in completing mechanical design and fabricating system.
- Longevity/membrane attrition testing mentioned during the question period but details not available.
- Minimal accomplishments to date (based on presentation); lifetime questionable; path towards high pressure output (10,000 psi) unknown; more information of performance versus goals necessary, especially efficiency.
- Significant accomplishment to construct the proof-of-concept prototype.
- MHC performance needs to be directly compared to conventional compression costs (operating and capital cost).
- Benefits of fluid bed reformer need to be clearly compared to conventional reforming technology.
- Behind schedule at go/no go decision, and management planning for schedule recovery is unclear.
- Revised safety plan was good.
- Only one data plot, and without the unit for flux.
- The projected efficiency and other performance parameters should be listed. Q&A revealed efficiencies in the "mid-70s" at a steam to carbon ratio of 3, and it is unclear how this is defined. Unclear what is included in the efficiency calculations.
- Unclear if energy losses are associated with the hydride compression technology.
- Pros and cons of reformer skid assembly addressed.
- Auto-thermal fluidized bed membrane reformer assembly has been fabricated and installed.
- Pd-membrane has been tested at operating temperature and pressures but not using reformer product streams.
- Completed appropriate safety reviews.
- Review comments from last year's review were addressed.

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

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

- Good collaboration between partners; good use of capabilities.
- Appears to have close collaboration, however seemingly limited to industry.
- Unclear about the cause of delays and how it was communicated to team members.
- Unclear how catalyst life is (still a problem) is being addressed.
- Unclear who is responsible for market plans and commercialization.
- The presentation time is better utilized with one speaker with others participating in Q&A.

Question 5: Approach to and relevance of proposed future research

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

- Minimal testing of integrated system; integration of components could be challenging.
- Future work is properly dependent on success with POC prototype.
- Project should proceed only if clear benefits for novel approach are established and communicated.
- Accelerated reliability should be considered to determine impact on cost.

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- Unclear what technology life cycle costs are.
- The project end date is June 2008. By the end of May 2007 this team plans to complete site installation of reformer.
- MHC will be commissioned for one month of integrated testing. Unclear if one month of testing is sufficient to gather all relevant data needed to move forward.
- It would be helpful to provide future plans after the completion of this project.

Strengths and weaknesses

Strengths

- >200 compression ratio, release at 1500psi is very good.
- Able to operate at lower temperature due to hydrogen removal.
- No coke formation.
- Good technical team, teaming arrangement and strong management group.
- The combination of membrane and hydride compression is novel.
- The cartridge system for membrane is nice. Unclear if these can be hot swapped.
- Lower capital cost possible compared to conventional fuel processors by reducing component count and sub-system complexity.

Weaknesses

- Need to elaborate on the benefit and cost (energy, efficiency).
- Not clear how Pd membrane separator will ever be cost effective.
- Contaminants getting through the membrane will contaminate the hydride reactor. Unclear if there is an option for a guard bed.
- Should have shown membrane long term tests results.
- Where is capital cost estimate? What about substrate cost?
- Minimal time for integration and testing.
- Higher cost of operation.
- System efficiency appears lower than conventional.
- Not much data has been shown.
- How much sulfur can the membrane tolerate?
- No reason given for the delay in getting the MHC skid ready.
- Unclear what the "Advanced Prototype" is. And what will happen to this project after this phase of the project ends in June 2008.
- Membrane life and catalyst durability are of significant concern.

Specific recommendations and additions or deletions to the work scope

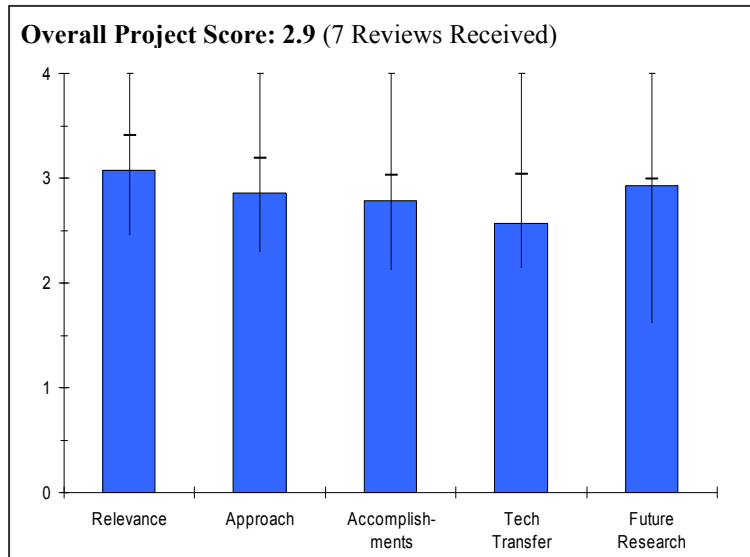
- More testing of integrated system needed.
- Independent risk assessment of concept needed.
- Flux data should not be proprietary. This is a performance parameter that has a target defined by DOE, just like parameters such as efficiency.
- Reviewers change or may not remember details from the previous year. Some information such as expected performance, if presented in past years, is worth repeating.
- Add the ability to track the effect of process conditions on contaminant levels, and ultimately on the cost of hydrogen.
- Test the membrane and catalyst under "real-world" application conditions before integrating the membrane reactor with the MHC.
- Provide commercialization plan for this system.

Project # PD-04: Bio-Derived Liquids Reforming

Yong Wang; PNNL

Brief Summary of Project

The objective of this project is to assist DOE in evaluating and developing alternatives to gasification and pyrolysis of biomass for hydrogen production that can meet the DOE 2017 cost target of <\$3.00/gge. The objectives for FY 2007 were to develop stable and selective catalysts for vapor phase reforming of ethanol to produce hydrogen and to understand the reactivity and selectivity of aqueous phase reforming intermediates to enhance hydrogen productivity. An isothermal aqueous phase reforming reactor has been developed to screen catalysts and understand reaction mechanisms for improved hydrogen productivity.



Question 1: Relevance to overall DOE objectives

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

- Early evaluation of catalyst performance for reforming of bio-derived fuels, important topic as research moves beyond natural gas fuel processing.
- Project directly addresses Program goal of hydrogen production from renewable biomass resource.
- Potential long term solution to distributed reforming system with very low well to wheel CO₂ emissions
- Meets the DOE Hydrogen Program goal of cost effective H₂ production from bio-liquids.
- Presentation went into excessive detail on liquid phase reforming; needed to be balanced with how this technology has the potential to meet DOE H₂ production targets.
- Project is critical to the realization of renewable sources for hydrogen at the DOE targeted production cost of \$3.00/gge by 2017. However, looks complimentary or redundant to Virent efforts in catalyst optimization.
- Critical to evaluate bioliquids as a feedstock for hydrogen.
- Capital cost reduction is critical for any hydrogen system.
- It is not clear whether any of the biofuels to hydrogen pathways shown make sense from a cost or efficiency perspective (i.e., there are probably more efficient ways to use biomass/biofuels).
- Ethanol is not competitive with gasoline without needing significant subsidies.

Question 2: Approach to performing the research and development

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

- Gas phase system approach not clearly presented.
- APR approach is well defined and effective.
- Need better definition of goals; minimal information provided on reactor rig and test plan; clear direction not presented.
- Very strong science focus on understanding catalytic fundamentals in this relatively unexplored system, including assessment of heat/mass transfer, identification of chemical intermediates, and exploration of potential reaction network.
- Concentrating on vapor phase ethanol because it is a current infrastructure fuel; will need to be extended to other bioliquids.
- Makes sense to explore liquid phase reforming; has obvious potential cost advantages.

PRODUCTION AND DELIVERY

- Good summary on current state of catalyst development. It appears that both PNNL and Virent are working on improving hydrogen selectivity for the Virent catalyst. So far the Virent catalyst has the best performance.
- Good that PNNL (and Virent) are working on processing low cost sugars with their catalyst.
- Fundamental mechanistic studies are well planned.
- Plan was not well explained.
- Unclear how team members were selected and how is each performing.
- Management review was unclear.
- Project doesn't seem focused on any of the barriers except identifying better catalysts.

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

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

- Hard to assess progress since specific goals not well defined.
- Very early in evaluation process; catalyst showed comparable performance.
- Tremendous amount of work done including:
 - Development of isothermal reactor (need this for mechanistic and kinetic studies).
 - Identification of stable and selective catalysts.
 - Complete product identification.
 - Mechanistic studies and influence of added KOH on overall reaction network.
- Demonstration of liquid phase reforming is a significant accomplishment; however, seems that the reactor productivity is very low (1-4 g feed/g catalyst/hr). Exploratory research notwithstanding, it is unclear how these levels of production will be commercially viable.
- Liquid phase reforming needs to be put in perspective with what catalyst advances would be required to meet DOE H₂ cost targets.
- Appears that a significant amount of work has been done to understand liquid phase reforming and many leads were generated on how this technology can be improved.
- The findings that microchannels, higher space velocity, and pH control can improve hydrogen conversion for the RePt catalyst may be important information to industry.
- Unclear if the project was reviewed for safety.
- No progress has been made on cost or efficiency – which were identified as barriers addressed.

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

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

- Minimal to date, but attempting to bring in more partners.
- Claim collaboration with Virent, but wasn't clear from the presentation what that partnership involves.
- Same goes for claimed collaboration with Ohio State research group...not clear what the nature of the collaboration is.
- Good collaboration with ANL and Ohio State.
- This seems to be limited in terms of collaborative influence. They are still not sure of likelihood that oil and ethanol producers will find value in project.
- Catalyst issues still a problem.
- Unclear whose responsible for carbon monoxide and other emissions.
- While numerous presentations appear to have been given, it is not clear if significant interactions occurred.

Question 5: Approach to and relevance of proposed future research

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

- Future work for 2008 lacks specific targets for each planned task.
- Typical catalyst testing of selectivity, stability, etc.; planning to evaluate impurities. Evaluation of potential system performance would be helpful.

- Plans include minimizing acid formation, developing kinetic models, improving catalyst activity, selectivity, stability and developing process economics.
- The first two represent valuable and sensible science development at a national lab. Skeptical about the latter two activities and if these reflect appropriate activities for a national lab, when industrial labs are now pursuing these same targets. If PNNL is successful in developing promising new catalysts, it is unclear who will develop them.
- Mentioned performing a comparison with DOE cost targets; needs to be done sooner rather than later.
- Because the benefits of liquid phase reforming are great, work should continue to advance this technology.
- Future research to focus on larger reactor is sound if catalyst life and selectivity can be improved.
- Needs more clarification on go/no go decisions for different approaches.
- There doesn't appear to be appropriate off-ramps if the project results are not promising.

Strengths and weaknesses

Strengths

- Initial effort to evaluate vapor versus aqueous reforming of bio-derived fuels.
- Building basis of strong catalyst fundamentals in this new catalysis area.
- PNNL has good experience with small scale reformer R&D.

Weaknesses

- Very academic presentation: lots of information without much analysis of its significance. Made it hard to deduce true worth of work.
- Require better definition of goals and projected performance.
- Appears to be on a path to try and optimize catalyst life, activity, and selectivity, which would put it in competition with research going on in industry.
- Not sure of the differentiating benefit of this project here unless the Virent catalyst is just used as a baseline comparator.
- Market evaluation.
- Risk assessment.
- Commercialization.
- Very difficult to follow and understand the presentation.

Specific recommendations and additions or deletions to the work scope

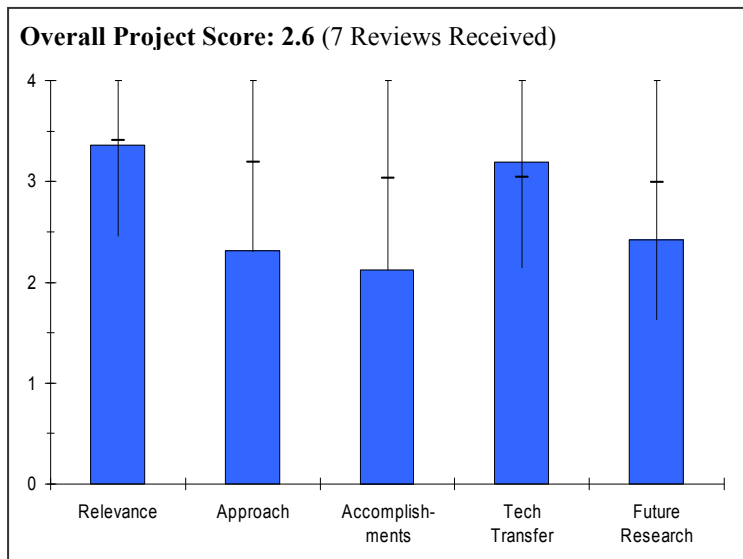
- No comment as no clear path forward presented.
- The project needs to perform preliminary cost and “well to farm to wheel” efficiency assessments to compare to other options (like using ethanol efficiently in an ICE) in order to establish performance targets for go/no-go decision points.

Project # PD-05: Biomass Gasification

Michael Roberts; GTI

Brief Summary of Project

The objectives of this project are to 1) reduce the cost of hydrogen produced from biomass gasification to \$1.60/kg H₂ (at the plant gate); 2) develop an efficient membrane reactor that combines biomass gasification, reforming, shift reaction and H₂ separation in one step; 3) develop hydrogen-selective membrane materials compatible with the biomass gasification conditions; and 4) demonstrate the feasibility of the concept in a bench scale biomass gasifier. Thermodynamic analysis indicates that there is potential for over 40% improvement in H₂ production efficiency over the current gasification technologies. The scope of the project includes membrane material development (ceramic, metallic, and composite materials), gasification membrane reactor process development and economic analysis, and bench-scale biomass gasifier design and construction.



Question 1: Relevance to overall DOE objectives

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

- Co-production of H₂ with renewable energy facilities economically viable on their own is an important initiating pathway for commercial H₂ as an energy carrier. Fluidized Bed (FB) gasification of biomass is one such opportunity, but impurities, variability of feedstock, and ash present significant challenges. This project begins to address several problems.
- A single step reformation-shift-separation method has shown advantages in other feedstreams (syngas), and is a reasonable goal for biomass as well.
- Addresses Program barriers: H₂ Production from biomass and related challenges in increased efficiency, impurity tolerance, and H₂ membrane targets of improved selectivity, flux at low temperature and impurity tolerance.
- Has not demonstrated that membrane approach has advantages over conventional biomass gasification/reforming (multiple reactors or combined).
- Biomass will be an important contributor to the hydrogen future.
- Relevant since it tries to use low cost biomass as fuel.
- Hydrogen from biomass is a renewable pathway. Solids handling and feed diversity are challenges for this feedstock.
- The project aligns well with the MYPP for a central production facility.
- The project offers the opportunity for CO₂ capture from biomass — more than a zero greenhouse gas emission approach, rather, a negative GHG emissions approach.
- The technology proposed has a high risk, high return nature appropriate for DOE support.
- Project objective is to reduce the cost of hydrogen from biomass to \$1.60/Kg H₂ (without delivery) and therefore there is relevant to overall DOE objectives. The project objective will be met by developing an efficient membrane reactor that combines biomass gasification, reforming, shift reaction and H₂ separation in one step.

Question 2: Approach to performing the research and development

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

- The initial spectrum of four types of membrane transfer types is the right place to start this project.
- The apparent initial plans to first evaluate all membrane types and then to begin in-reactor evaluation is not as expeditious as having at least some in-reactor experience earlier. Critical additional challenges to membrane media working in biogas will likely reveal additional limitations on membrane design not anticipated when designing to theory or synthetic biogas.
- Down-select criteria and process were not apparent.
- As this reviewer heard the approach: the target is a biomass gasifier with in-situ removal of hydrogen via a hydrogen permeable membrane. The goal is to enable gasification at lower temperature which in theory could improve efficiency.
- Not discussed in the talk is the fact that biomass gasifiers make much more oil and tar when operated at low temperatures. These are materials that we expect would at the very least form liquid films on the membrane modules and at least inhibit hydrogen transport (by adding a liquid film transport requirement) and possibly foul the membrane by poisoning.
- Fundamental knowledge of chemistry at these temperatures should lead to the EXPECTATION that the membranes will be fouled by reaction with one or more of the following: Cl, S, N, and liquid alkali oxides/hydroxides. The project tasks should be modified to set a much higher priority on demonstrating membrane stability to all these impurities before they are scaled up to the full scale expected for integration in the gasifier.
- Membrane won't be 100% selective for H₂. Plans for polishing? Small PSA?
- Alkali condensation on the membrane at these temperatures is a very serious concern. What is the temperature at the membrane?
- Reforming of methane is favored at high temp, while WGS is favored at low temp? Project should include thermodynamic simulations to determine equilibrium; pulling off the H₂ will solve part of this problem, but temperature is still important.
- What about dust clogging the membrane? Might be better to have some cyclone separation before the membrane.
- Many membrane scientists on team. Good combination of scientists but no party that knows how to make or commercialize membranes.
- The project uses a hydrogen membrane to remove the hydrogen. This may aggravate the prospects of coking the reformer if not accompanied with a high steam to carbon ratio. The latter penalizes system efficiency.
- The hydrogen yield is at a low pressure.
- GTI is experienced in biomass processing. That leaves the project revolving around the membrane. Membrane studies should be the highest priority and should be followed with a go/no-go after Task 2.
- Is oxygen feasible, cost wise?
- Substantial efforts by others have gone into applying H₂ membranes to cleaner environments than biomass gasification (e.g., natural gas SMR, WGS), yet none has achieved significant commercial success. Applying H₂ membrane technology to an environment as complex and dirty as a biomass gasifier presents a substantially greater challenge. Early, extended exposure of membranes to real (not simulated) gasifier conditions is important to reduce risk. The investigator does not plan to perform such tests until relatively late in the project.
- Presentation did not differentiate how this project's approach differs from previous projects' approaches (e.g., membrane SMR technologies) that were not successful.
- Chances of developing a membrane that can perform inside the biomass gasifier are low.

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

This project was rated **2.1** based on accomplishments.

- Since the project has just received its first funding, progress beyond getting funding is not expected.
- Not really applicable...project just got started.
- Need a better PFD. Cannot possibly be working off the one presented. Not detailed enough and contains an error (only one gas stream out).

PRODUCTION AND DELIVERY

- What pressure does the H₂ come out at? Will need compression. Team should do a better job of understanding the entire system.
- Why hasn't a detailed cost analysis been done?
- What is the source of the cost data presented?
- Work done on breaking down H₂ cost from biomass not clearly based on own work. No references. Is this their work?
- Claimed 40% efficiency improvement over conventional biomass reforming. What is the anticipated efficiency?
- No data yet. Funding delays.
- This project started just two months ago. Therefore no technical accomplishments and progress presented.

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

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

- Bringing the diverse membrane supply options in as project collaborators should provide DOE with a good platform to compare pathways, and may reveal synergistic applications.
- Pursuing diverse membrane types will not likely develop close collaboration among the champions of those diverse suppliers.
- Plans for coordination between GTI and several commercial materials suppliers sound good. As noted above, should work toward a plan where we don't have to go to full scale membrane module development before significant testing of membrane flux in presence of expected impurities.
- Good project team, but who is integrating the work efforts of each member?
- Should take into account membrane work being carried out by other institutions.
- Good broad-based group of collaborators if all participate.
- Using membrane reactors. Could collaborate with PD3 on technology if IP concerns do not cause problems.
- Multiple organizations are involved in this development.
- Good quality collaborators.
- Project would benefit by involvement of an industrial gasifier vendor (GE, Shell, ConocoPhillips, . . .) by providing access to extended operating experience at scale and possibility to expose materials to gasifier conditions early and for extended periods. (Admittedly coal and coke are different than biomass, but they do provide an opportunity for extended exposure.)
- Teaming with Arizona State University, NETL, Schott North America, and Wah Chang is good.

Question 5: Approach to and relevance of proposed future research

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

- The project proposers have laid out a reasonable plan, but it is too early to determine follow-on research.
- As noted above, need to focus on membrane tolerance to impurities before making ANY plans to integrate with a gasification reactor.
- Scope of work should be given as a timeline.
- Why is the economic analysis being done in Task 2? Should be done throughout project and especially at beginning to drive research.
- The "road map" slide is not a roadmap. Following this, the project team is bound to get lost.
- Good plan. Program just started.
- Presentation contained few fall-back pathways – Q&A indicated the investigators are aware of some options (such as relocating membranes downstream of reactor, near cyclones) but apparently haven't analyzed process consequences.
- Membrane materials development (metal, ceramic, and composite membranes) will be done by the sub-contractors.
- GTI will incorporate the membrane module inside a biomass gasifier and carry out the test.
- It appears there is disjoint between the membrane development work going on at ASU and what GTI presented.
- Chance for success is low.

Strengths and weaknesses

Strengths

- FB gasification of biomass is an important emerging renewable energy source for the electric utility industry. Co-production of H₂ is an obvious place for H₂ research to be.
- Project addresses reasonable spectrum of initial membrane choices.
- Single stage reforming-shift-separation is reasonable starting assumption.
- It is good that this concept will be tested.
- Good project team.
- GTI's known strength is gasification.
- Three alternative membrane technologies are being evaluated. Maintain focus in these areas.
- CO₂ capture potential.
- Membrane H₂ separation – membranes' potential advantages in SMR, WGS and gasifier reactors are significant and well understood.
- Reasonably good teaming arrangement.
- GTI's persistence to continue this concept.

Weaknesses

- Lab-scale (2" diameter) FB reactor as first in-reactor test cell is unlikely to yield same results on biomass at scale.
- Plan for addressing spectrum of feedstocks, or alternatively reasons for selecting a given biomass feedstock for this work, was not given.
- No detailed plans.
- Flowsheet not sufficient.
- Cost analysis should guide research.
- Membrane survival with dust/ash/char/tars clogging and alkali condensation a concern.
- No partner that understands membrane manufacture.
- Scale-up/scale-down of fluid bed units is difficult. Need to address this. Would anyone invest in technology based on size of unit gathering data?
- Project has been delayed by funding availability.
- Experience has taught at least one industrial gasifier vendor to test materials to be used in gasifiers *in situ* as soon and as long as practical. The opportunity exists for this project to encounter unexpected, unfavorable performance relatively late in the project.
- Challenges in developing a practical H₂ membrane solution are substantial — historically, few practical successes have been achieved in much milder environments than gasifiers.
- Position of membrane within the biomass gasifier is of great concern. Will it really shift the WGS equilibrium?
- Efficiency of the proposed concept is missing.

Specific recommendations and additions or deletions to the work scope

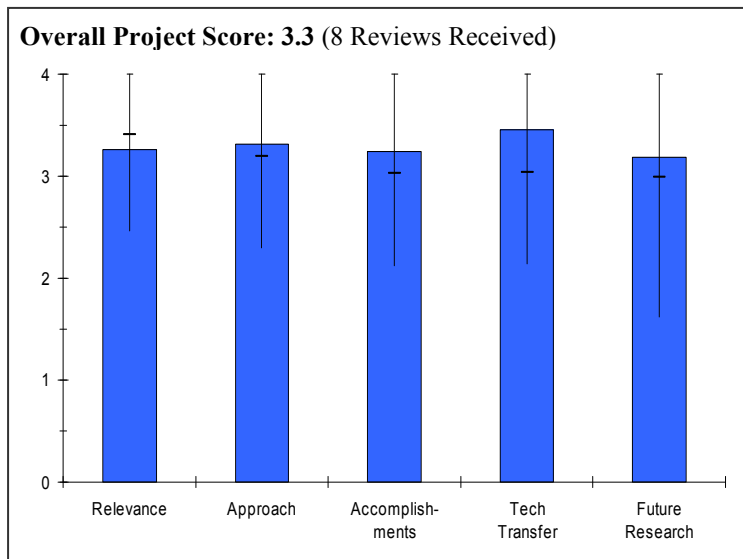
- Cost analysis should precede additional R&D.
- More focus on needs for membrane development / mfr / contaminants, etc.
- Understanding scale-up for mega-scale units.
- Characterize biomass characteristics that work well /not well in this type of process.
- Include a go/no-go after Task 2.
- Identify what (species and their level) will foul each type of membrane.
- Chances for success are low.
- Ask GTI about their earlier work on this particular concept.

Project # PD-07: Carbon Molecular Sieve Membrane as Reactor for Water Gas Shift Reaction

Paul KT Liu; Media & Process Tech.

Brief Summary of Project

Media and Process Technologies has developed a membrane with high efficiency conversion capable of converting the CO created during on-site hydrogen production to 99+% as opposed to approximately 70% through the use of high temperature shift alone. The high conversion is a result of the new membrane configuration. A pilot test of the new membrane in a reactor will be conducted at the end of FY 07. Process simulation; hydrogen production economic analysis; design, simulation and economic analysis of the polishing step to achieve 99.999% purity will follow in preparation for conceptual design of the field test unit.



Question 1: Relevance to overall DOE objectives

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

- Viable concepts for combining reforming/shift/separation unit processes are vital to a long term, low system cost.
- The contractor is focusing on the development of microporous membranes which are an area of interest to DOE for hydrogen separation.
- The presentation was extremely vague on the specific targets required by DOE. No cost estimates were provided. In addition, no clear flux targets were provided – just a vague comparison to metallic membranes – which appears low and in odd units.
- The work does address process intensification – which is a key area of DOE interest.
- The temperatures being considered are low – 200+ degrees. There should be consideration to get this a bit higher – maybe into the HTS range.
- Goals of this program are well aligned with the DOE Hydrogen Program.
- Interesting work, but not a critical piece of the puzzle for hydrogen success. Conventional WGS works pretty well and isn't overly expensive.
- Hydrogen purification/separation is a critical component of hydrogen production.
- Developing reactive separation membranes that do not use expensive materials (such as Pd-alloys) can have very significant payoff in overall hydrogen production costs.
- Need to address the effect of non-infinity separation factors for microporous membranes.
- A membrane within the water-gas shift reactor should benefit hydrogen production pathways that use reforming.
- Carbon molecular sieve membrane as reactor/separator for water-gas shift reaction will be developed in this project. Streamlining unit operations involving CO conversion/H₂ separation and purification is very relevant to overall DOE objectives.
- Developing a small, more cost effective (capital and operating) hydrogen clean up system is relevant to the DOE objectives.

Question 2: Approach to performing the research and development

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

- Well thought out and described.

- The approach is reasonably straightforward and involves general testing of existing membrane materials. There is no significant novel development, for example, no new membrane materials are being developed.
- Scale-up to a larger size unit should provide valuable information.
- Catalyst testing and membrane development appear to be occurring along two separate paths. They need to be integrated together into a single development path.
- Testing of membrane performance is comprehensive with a clear line of sight to DOE hydrogen production cost targets and reliability.
- Development of a mathematical model will be critical to ultimate process design. This is a very sound and comprehensive approach to ultimate process design.
- Nice plan for success, including scale-up, pilot testing, economic analysis, safety, and end-user tests.
- Unclear what the hydrogen pressure is exiting the membrane.
- This project is using a process development approach to determine the needed hydrogen purification and reaction kinetics to meet the performance targets.
- Using TSA-based adsorption as the final polishing step offers the potential to meet the desired hydrogen purity levels.
- The target CO value should be decreased to 1 ppm CO for distributed hydrogen production for dispersing to fuel cell vehicles.
- There will be other impurity level constraints in addition to CO, for these systems that must be met for the systems to be deployed.
- A membrane within the water-gas shift reactor will improve the CO conversion and/or reduce the size of the water-gas shift reactor.
- This process produces hydrogen at low pressure which then needs to be compressed. The ultimate measure is in the cost of hydrogen, where the hydrogen is at 300 psi.
- Unclear if the presence of H₂S and water affect the membrane.
- Team will develop a reactor/separator system capable of producing 99.999% pure H₂. To achieve this goal they will incorporate carbon molecular sieve membrane with a water-gas shift catalyst and eliminate the two separate water-gas shift reactors (low and high-temperature). By removing hydrogen using the molecular sieve membrane, they will shift reaction equilibrium.
- It is a well planned approach.
- Presenter did not mention the barriers addressed in their project. This information is missing in their slides.
- The approach to generate bench scale data, generate a model, extrapolate to pilot scale, and then validate the model is standard engineering process.

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

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

- Benchtop experiments yield useful data.
- Good comparison of simulation results and test data.
- The technical progress is difficult to judge. Although actual results are alluded to and appear to be included on some of the graphs, much of the work appears to be mathematical modeling. However, it is not clear which are results produced from this work and what is from the literature. It appears that the project has been minimally funded (30%) so it is not clear how much actual progress has been made.
- The use of microporous membranes will not likely give the most effective separations. The purity and percent recovery will remain a concern and the contractor has not done a lot to improve on the situation. The separation mechanism itself will likely limit these parameters.
- Impressive testing of membrane in real-world conditions by using a slip stream from a hydrocracker that included impurities and higher hydrocarbons.
- Very good technical accomplishments such as development of a model and demonstration of a membrane in a small scale reformer confirmed by industry end user.
- Great verification of model with experimental data.
- Excellent use of analysis during project and by end-user.
- Included polishing step which shows excellent systems understanding.
- Great pilot testing plan and results; although, membrane longevity was not discussed.

PRODUCTION AND DELIVERY

- Appropriate trade-off between lower purity and higher recovery, completed with polishing.
- They have completed modeling and some experimental verification at the component level.
- The hydrogen flux at 200 to 250 C is comparable to, or better than, that in Pd-alloy systems.
- The Pd-alloy systems typically operate at much higher temperatures, yielding 20 to 50 times hydrogen fluxes; thus, comparing the performance of the carbon molecular sieve membrane at the lower temperature is not very meaningful.
- Although the presenter indicated that the carbon molecular sieve membrane material is stable, the data show significant degradation from the 3 hour values to the 100 hour values.
- The model seemed to over-predict the observed performance indicating that the model's parameters need adjustment.
- They presented good data which indicates promising results.
- Insufficient explanations and legends lead to confusion.
- Mathematical model has been experimentally verified.
- HiCON process has been developed for the small-scale reformer by the end user of this technology.
- Process optimization study demonstrated that 97-99% H₂ purity can be accomplished. This is lower than the objective of producing 99.999% H₂ purity. The team has plans to obtain higher purity.
- This project start date was October 2003. Only modest technical accomplishments have been achieved.
- The technical accomplishments to date are somewhat confusing. The stated goal is 99.999%, but reports that 97-99% clean-up was accomplished and that a non-defined adsorbent would polish the hydrogen to 99.999% with CO apparently being the principle impurity at < 10 ppm. This must be addressed.
- The <10 ppm CO target seems out of step with fuel cell targets of < 0.2 ppm.

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

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

- Partner collaboration and contributions were not clear.
- Technology transfer and industry involvement appears minimal. There are some academic publications. The intro slides did list involvement of other partners, but their roles and contributions are not clear. It appears that the testing is primarily being conducted by MPT and the University of Southern California. The industry partners need to be more involved to ensure that the developed technology is worthwhile.
- Impressive collaboration between academia and industry; this project meets the ideal for partnership; result is the leveraging of new technology from academia (membrane) applied with an eye toward ultimate commercialization (provided by industry end user).
- Excellent inclusion of end-user!
- The project has good collaborations; academic, industry, and end-user.
- Having Chevron involved in the project is helpful to keeping a realistic check on the project's progress.
- Good combination of collaborators - include academic, manufacturer, and user.
- Good team. Team includes USC (for membranes research), Johnson Matthey (for catalyst development), and Chevron (end user).
- Seems to have good collaboration among team members.
- Working with USC, JMI and Chevron is a very good plus.
- Was consideration given to include one of the specialty gas companies (APCI, Air Liquide, BOC/Linde, Praxair)?

Question 5: Approach to and relevance of proposed future research

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

- Future direction is proceeding towards testing of a moderate scale membrane assembly. This appears to be an existing apparatus and testing should be straightforward.
- The testing needs to incorporate realistic gas feeds.
- Proposed future work is valuable; glad to see that it will include an economic analysis by industry end user; this ensures an objective, reality based analysis.

- Analysis should include a comparison with conventional, proven H₂ production technologies (e.g. steam methane reforming, water-gas-shift, pressure swing adsorption).
- Not a lot of future research proposed. Depends on funds availability?
- Researchers should present ideas for future research.
- Pilot-scale testing would help to obtain the data needed to validate the performance models.
- The proposed economic analysis is needed to substantiate the potential cost attractiveness of this process.
- In particular, the use of the polishing step needs more careful analysis, as the ultimate purification may add more to the cost than expected on the basis of a preliminary analysis.
- The pilot-scale testing and economic analysis are both needed.
- In FY2007, the team plans to complete pilot scale testing using a single, full-scale hydrogen selective membrane and synthetic feed to demonstrate the optimized HiCON process. End user will complete preliminary economic analysis.
- Field demonstration will be done in or after 2008.
- The above plans are reasonably good. The PI presented detailed path for moving forward.
- The future work proposed makes sense and fits in with the stated plan.
- Work to improve the effectiveness should be included.

Strengths and weaknesses

Strengths

- Good that a preliminary economic estimate of capital cost was made.
- Overall, a very good science and engineering effort.
- Straightforward test project that should be able to be completed in a minimum amount of time.
- The project has a good combination of modeling and experimental efforts.
- The project has a good combination of academic and industrial collaboration.
- A membrane can eliminate moving parts and switching valves of PSA based systems.
- A membrane will shrink the shift reactor.
- Good team.
- Good planning.
- The design as portrayed appears to be applicable for a number of merchant grade hydrogen applications and thus applicable.

Weaknesses

- Preliminary economic analysis capital cost estimate did not seem to properly include fabrication costs. Seems to be a materials summary only.
- The contractor needs to provide more details on the experimental conditions. Only limited data is provided to evaluate the work, and this makes it almost impossible to assess the actual state of the work.
- Although the presenter referred to 10 ppm CO as being readily achieved, during the Q&A it was stated that CO was at 20 to 30 ppm. It was not clear what caused this discrepancy.
- Compression energy is quite significant, particularly for compressing the product gas from 15 psig to 300 psig. This must be included in the process simulation; otherwise comparisons with alternative approaches will not be consistent.
- Not specifically a weakness, but it is not clear why the test feed gas had up to 31% nitrogen and only 44% hydrogen in the separation verification tests; this composition is reflective of ATR reformat rather than SMR reformat.
- If a PSA can remove the CO down to 0.1 ppm and a significant retentate is needed in the burner, then why use a membrane to convert CO, especially if the membrane will work only at low temperatures?
- Standard information about barriers addressed that was present in all other presentations is missing here.
- This project started in October 2003 and modest accomplishments were reported over the past three years — what are the reasons?
- What about long term durability of carbon molecular sieve membranes under process operating conditions?
- They missed on clean up – 99 vs. 99.999%.
- There is a lack of suggestions to determine the cause of the miss.

PRODUCTION AND DELIVERY

- There is a lack of a plan to correct the miss.
- The energy requirement for storage is not fully included. H₂ supplied at 15 psi not 300(?) psi.

Specific recommendations and additions or deletions to the work scope

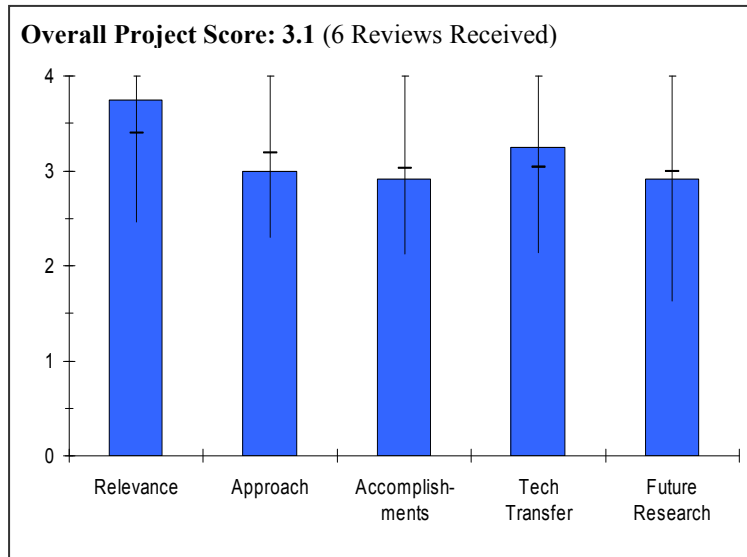
- The contractor needs to finish the work as proposed and provide some solid technical data - particularly on the reaction conditions.
- DOE needs to provide the conditions to the contractor for testing.
- Appears to be an excellent research team. Suggest DOE provide additional funds to carry this project to completion and request involvement in other R&D efforts.
- The permeate from actual reformat should be analyzed in great detail to identify all species that makes it into the hydrogen. One set of data from a pilot test shows C1 and C2 species in the hydrogen.
- Very high hydrogen recoveries are not necessary since the retentate can be sent to the burner to generate heat for the reforming reaction.
- The use of a membrane is much more effective if it can operate at higher temperatures, where the CO conversion kinetics and perhaps the permeance are even faster.
- Considerable data has been shown. These should to be evaluated closely and in detail.
- No change is needed. Continue the work.
- Investigate the escape.
- Evaluate the application of a less extensive clean-up for other merchant usage (this might free up existing facilities for the higher quality demand expected in 2015-2020 time frame).

Project # PD-08: Renewable Electrolysis Integrated System Development and Testing

Kevin Harrison; NREL

Brief Summary of Project

This 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 and develop standard testing procedures; design, build and test shared power electronics packages and controllers to reduce cost and optimize system performance; identify opportunities for system cost reduction through breakthroughs and incremental improvements in component integration focused on commercialization and manufacturability; and test, evaluate, model and optimize the renewable electrolysis system performance for both dedicated hydrogen production and electricity/hydrogen cogeneration.



Question 1: Relevance to overall DOE objectives

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

- Renewable electrolysis supports the President's Hydrogen Fuel Initiative.
- Addresses overall program objective of hydrogen production from renewable resources. Attempts to address specific barriers related to cost, efficiency, and integration of renewable energy supplies.
- Project directly addresses the DOE Hydrogen Program goal of generating H₂ from renewable electricity.
- The project assists with system and cost optimization to advance hydrogen production from distributed and central wind electrolysis closer to the DOE cost and performance targets.
- Good effort on improving the overall system efficiency by targeting the power electronics and system integration.
- The project needs a quantification of overall average system efficiency improvement goals and timelines.
- Cost of renewable (e.g., wind electricity) energy should be used for projecting utility cost contribution in the cost of hydrogen production.
- Renewable-based hydrogen production is a very important part of the Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

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

- It appears that there is limited research to reduce cost.
- There appears to be lots of modeling, but more validation is needed.
- Focusing on the overall system is good.
- Specific goals are lacking.
- Applied solid engineering approach to improving efficiency of integrated electrolyzer and performance of system seems suboptimal at low power levels, which was due to a very narrow and specific design constraint of the electrolyzer.
- Sound approach combining modeling with real-world validation; then using the model to optimize electronic control.
- Well conceived plan to ultimately maximize recovery of theoretical maximum available wind power.

PRODUCTION AND DELIVERY

- Collaborations enable access to real-world wind power generating facility.
- Design, installation and baselining component performance appears to be well executed. It is unclear if they have systematic approach for modifications made to electrolysis units.
- Using a combination of modeling and experimentation to optimize the overall system efficiency is a good approach as this is a complex system.
- Hardware-in-the-loop, as being done in this project, is an effective approach to design the power controller and electronics for this system.
- Further listing of "technical gems" in the approach is needed/should be highlighted.
- Good combination of analysis and technology development and testing.

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

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

- The progress seems modest for 3.5 years into the project.
- There is a need for more data – one hour is not impressive. With the system set up, there should be quite a bit of data generated.
- It appears that they have developed good safety procedures.
- They demonstrated that there is a lot of room to improve efficiency of wind turbine/electrolyzer combination.
- Significant progress in demonstrating systems to integrate electricity from wind to electrolyzers.
- Added benefit of demonstrating integrated H₂ production with compression and storage through collaboration with industry partner.
- Completed individual dual component baseline including stack characterization, but not a lot of rigorous testing under variable current conditions, also no simulated wind modes.
- Project appears to be in early stage of accomplishments on the experimental side, though quite some time has elapsed.
- Simulink model development and hardware-in-the-loop testing work is progressive and looks good; however, model validation data should be presented.
- Improvement in captured efficiency from Gen1 to Gen2 is impressive.
- Not clear what the impact (i.e., cost) of system performance optimization is.
- Control approach shows significant promise.
- Good progress has been made characterizing existing technology.

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

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

- It appears to be a good team and there is good team integration.
- NREL is providing valuable feedback to their partners on how to improve their products.
- HUG collaboration effort is good since it should enable rapid distribution of information.
- Close collaboration and financial support from utility industry partners. Wasn't clear from the presentation what support came from electrolyzer or wind turbine suppliers, the two industries which could best benefit from understanding how best to integrate their hardware for this purpose (hydrogen production plus grid electric supply).
- Impressive collaboration with industry and academia.
- Valuable project cost leveraging with Xcel Energy.
- Working with a number of key players in wind presents a great opportunity to share best practices from both an industry and academic perspective.
- Good collaboration between so many diverse team members.
- This project has a significant number of partners.

Question 5: Approach to and relevance of proposed future research

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

- One of the objectives is to identify areas for cost reduction; this does not appear in the plan.
- Validation of the models using the generated data should be a goal.
- The future path builds on the past direction of engineering optimization. The plans are sound, but it is unclear why public funds are being used to do this work.
- The presentation was unclear about a number of things including: (1) their approach to electrolyzer modification for greater efficiency and optimization (e.g. higher pressure, higher temp, and ancillaries), (2) integration of power electronics for wind turbine and electrolyzer. These items are probably well thought through, but they need to be documented better.
- Proposed future research is the highlight of this project. The results from the integration of wind energy and electrolysis in several different possible configurations shall be very useful for the technical community.
- Step-wise approach, as presented, is the ideal way to move forward in testing/optimization of the complex system.
- A lot more work needs to be done with the remaining budget.

Strengths and weaknesses

Strengths

- The summary report is reportedly going to detail lessons learned and other important items.
- They are using some real world wind data in their model.
- It appears to have a well balanced team including industry, universities, and national laboratory.
- They have produced several formal reports.
- Overall relevance to DOE's technical and portfolio goals.
- Good teamwork between several partners and subcontractors.

Weaknesses

- Since they have the system built, there should be more real world data.
- This is a straightforward engineering exercise in optimization of hardware and software for commercial products.
- The project strongly needs a fuel cell and electrolyzer expert on the team.
- Lack of clarification on the targeted deliverables in terms of cost, efficiency and timelines. Just citing DOE targets is not sufficient.
- Slow progress.

Specific recommendations and additions or deletions to the work scope

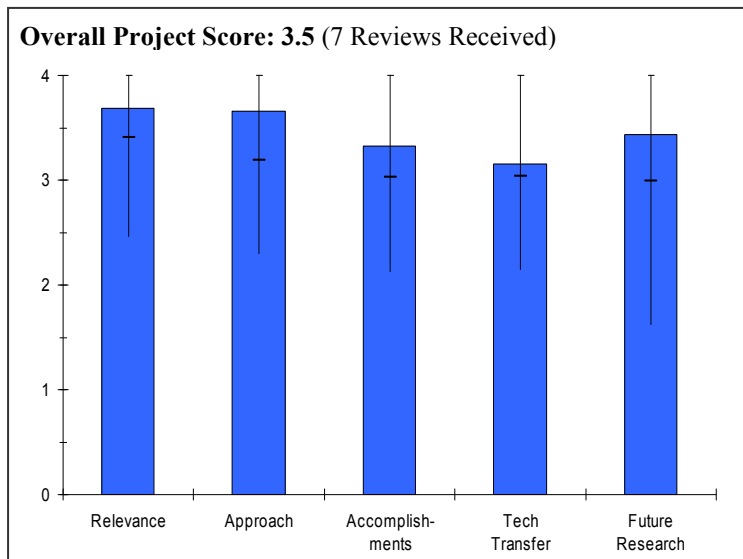
- They need to generate long term performance data.
- They should address areas of cost reduction.
- A plan to achieve the MW scale wind to electrolysis system needs to be developed. It should include the critical path (i.e. what are the highest risks to achieving the goal) and a risk mitigation plan.
- They may want to discuss this project with some refineries in Texas (Class 4 wind state) to see if the refiners have an interest in producing H₂ from a green source.
- The scope is adequate. Further additions might further delay the progress.
- The project team should determine how much the performance targets will reduce system cost.

Project # PD-09: Biological Systems for Hydrogen Photoproduction

Maria Ghirardi; NREL

Brief Summary of Project

The goals of this National Renewable Energy Laboratory project are to develop and optimize anaerobic and aerobic photobiological systems for the production of hydrogen from water and to integrate photobiological with fermentative organisms to more efficiently utilize the solar spectrum and the substrates/products from each reaction. The project is organized into three tasks: engineer a H₂-producing catalyst ([FeFe]-hydrogenase) that prevents O₂ from inactivating the enzyme's catalytic site under aerobic conditions; improve the light conversion properties of a H₂-producing anaerobic algal system by immobilizing the cells on a flat matrix; and test the ability of H₂-producing, fermentative organisms to consume algal biomass and to produce extra substrate (acetate) required for high yields of algal or photosynthetic bacterial H₂ production in a second reactor.



Question 1: Relevance to overall DOE objectives

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

- This project presents a novel approach to long-term renewable hydrogen production.
- The project is in line with DOE program objectives for hydrogen production.
- The work addresses a key technical barrier for biological production.
- This project seems to be directly relevant to the DOE Hydrogen Program for production of H₂ from biological systems.
- The proposed work is very important to the RD&D plan. These studies could be far reaching and apply broadly to a variety of biological hydrogen production strategies. For example, the goals of subtask one will contribute directly to the feasibility to utilize hydrogen producing enzymes in bio-inspired or biomimetic materials. Meeting the future energy needs of the planet will clearly require the implementation of a number of mechanisms and the NREL group and the current project is well placed to contribute emerging technologies that we probably have yet to appreciate their potential and their importance in the future global alternative energy profile. The engineering of an oxygen sensitive [FeFe]-hydrogenase is an ambitious goal but the group has provided some basis that this is possible and this is a significant contribution. These challenges for this goal are compounded by the absence of a direct selection method for oxygen tolerance, but the rational approach the group has implemented is promising and starting to produce positive results.
- The project goals are critical to the Hydrogen Initiative and fully support DOE RD&D Objectives for hydrogen production.
- Project supports MYPP long-term biological R&D.
- Project is high risk, potentially high pay off R&D appropriate for DOE investment.
- Project addresses multiple issues of proposed biological H₂ production process.
- Proposed process is a low greenhouse gas emissions process — potential exists to design process to have negative GHG emissions.
- This is assumed to be a longer term solution - the work is considered relevant.

Question 2: Approach to performing the research and development

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

- This project has a well-defined approach to achieve progress toward the DOE targets. Future work is also defined in the same sub-task structure.
- The subtasks for the year are appropriate to move the project forward toward meeting the objectives. However, the work appears to be open ended (no project end date is give), whereas all other production projects are finite, with defined scopes and timeframes and are fairly strictly held to DOE targets and schedules. This makes it difficult to assess the extent to which this approach will contribute to the overall due objectives a technology readiness decision in 2015.
- Speaker clearly understands and communicated that the state of the art is significantly far from performance needed for this technology to be commercially viable to meet DOE H₂ production cost targets.
- Approach was clearly stated and appears to be well thought-out.
- Impressed by the use of molecular dynamic simulation to aid catalyst synthesis.
- The subtasks all address the target technical barrier – continuity of H₂ production.
- The project is well-designed and the team has excellent experience to address the subtasks.
- The project is technically feasible and the investigators have provided key data that indicate that the project can be advanced in a timely manner.
- Although the subtasks are not discreetly integrated, each is important to the overall goals and is nicely complementary to the other funded efforts in the program.
- The project subtasks are well-focused on specific technical barriers.
- The project subtasks are well-balanced with respect to different technical barriers.
- The project subtasks are clearly designed to integrate with other research supported by the DOE Hydrogen Program.
- Approach is fairly straightforward.
- The approach presented is rational and systematic.

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

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

- Progress was described for each subtask and the project was able to obtain interim support from another source to complete work that would have been cut due to lack of funding from DOE.
- The progress toward 2010 targets was not adequately addressed. In the presentation, there did not appear to be a direct comparison for the duration target. There was no discussion of any annual milestones, decision points, etc.
- Program appears to have significant accomplishments given limited budget and funding discontinuities.
- Technology will need significant breakthroughs to meet DOE H₂ production cost targets.
- The investigators have made a clear comparison to the relevance of the proposed project to the goals of the DOE.
- The favorable performance on some indicators may suggest the DOE goals may need to be adjusted and could be more ambitious.
- For this project realistic cost and benefits estimates for this longer term solution technology is difficult to address and was not reported in the presentation.
- The group has made significant progress on all subtasks in the overall project.
- The selection or derivation of specific milestones and performance indicators was not evident; however, the progress towards specific performance parameters has been excellent. In evaluating this project as a research endeavor, the progress has been outstanding. There have been numerous publications resulting from this funding, with more in preparation.
- The demonstration of continuous hydrogen production under sulfur-deprivation growth conditions is very promising.
- The progress in computational modeling to guide experimental design of the oxygen-tolerant hydrogenase was very good, with the identification of a novel pathway for oxygen entry to the active site.

PRODUCTION AND DELIVERY

- The progress has been all the more remarkable given the reduction in FY06 funding.
- Good, considering funding.
- Good efficiency improvement from sub-task 2.
- The computation analysis appears to be yielding insight on defining next steps.
- The 25% improvement in yield was encouraging.

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

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

- The project is coordinating with other partners, but the contribution of each partner to the work was not included in the slides.
- Partners were listed and briefly mentioned, but the full extent of the collaborations wasn't clear from the presentation.
- A dedicated slide regarding collaborations would have helped.
- Program appears to be doing an excellent job in leveraging academia expertise in its work.
- The NREL group has numerous collaborations with other academic institutions.
- The project boasts strong collaborations at the University of Minnesota and the University of Illinois.
- The project does not list any industrial collaborators.
- The coordination of the work done in subtask one has been very productive and provided the basis for the design of site-specific amino acid substitution experiments.
- Most of the research is not developed to the point where opportunities for technology transfer are apparent.
- The investigators mention in a tangential fashion the use or development of proprietary materials, but there does not appear to be a coordinated, strategic plan to partner with industry for design and scale-up of the cell immobilization matrix.
- The investigators demonstrate excellent coordination and collaborations with university researchers.
- The investigators demonstrate good coordination and collaborations with international researchers.
- Good academic domestic and international collaborations.
- The collaboration with other research institutions is very good.

Question 5: Approach to and relevance of proposed future research

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

- A timeframe for completion of the "Future Work" described in the presentation was not specified, which would have been helpful considering the fact that a project end date was not provided.
- The proposed future work is appropriate to move forward.
- "Newly identified" targets were mentioned, but it wasn't entirely clear what those new targets are.
- The group has provided a clear and rational plan for each of the three subtasks based on the past progress.
- The implementation of high throughput screening methods for identifying oxygen tolerant hydrogenases will expedite the work.
- The explicit description of clear contingencies is not addressed and the investigators could strengthen the overall plan by addressing potential outcomes more directly.
- The investigators clearly present a plan to build upon their modeling success for designing an oxygen-tolerant hydrogenase, both in the choice of using smaller residues in the channel, as well as reducing protein flexibility.
- The plan to study hydrogen yield from fermentation under sulfur-deprived conditions is a logical next step from the current experiments.
- The plan to refine the architecture of algal immobilization is good.
- Project strategy contains several alternative approaches to achieve goals.
- Pretreatment of algal biomass prior to fermentation is a good idea.
- The proposed additional work sounds rational.

Strengths and weaknesses

Strengths

- The project includes a strong computational component to complement the experimental work.
- The groundwork is laid for each of the ongoing subtasks.
- The assembled team to conduct the proposed work.
- The clear plan for the future work in addressing each subtask.
- The fundamental importance of the subtasks to the mission of the hydrogen program in general and biological hydrogen production specifically.
- The investigators demonstrate clear experimental objectives with respect to their computer simulation and modeling, and do an excellent job in closing the loop back to experimental refinement of the model.
- The tenacity of the investigators to obtain alternative funding and collaborators is to be commended.
- The proof of concept for the linked fermentation/photobiological hydrogen production system was very well-done.
- The knowledge of these investigators for the microbial and algal systems under study is well-focused towards the project goals.
- The pure long term science may pay dividends in the future.

Weaknesses

- Milestones and decision points were not addressed. The P.I. also stated that the work is heading in three different directions, but there was no discussion of a down-select process of any kind to narrow the focus at appropriate times. The work is very exploratory, but the project should still have a finite scope, schedule, decision points, etc., to be fair with other production projects that are held to higher standards of definitive progress toward 2010 and 2015 targets.
- The lack of industrial interests and partnerships directly in the work.
- The lack of specific stated potential experimental outcomes and contingencies.
- It is unclear whether the mutagenesis work and modeling will be performed in parallel with the random mutagenesis experiments. There should be a clearer definition of checkpoints and cross-talk for these experiments.
- The investigators might consider whether they should continue to push for hydrogen production under algal growth conditions—given the goal of cell immobilization, they might want to try to balance algal biomass production with the microbial consortia fermentation rate. It is not clear which component contributes towards the "cost" of the entire integrated process.
- The selection of different cell immobilization matrices was not described in sufficient detail to ensure a systematic or rational choice of matrices vs. an ad-hoc, random approach. This is especially important if cost of the matrix becomes an issue.
- I do have a concern that mutating the various microorganisms may generate a bio-hazard. Are safe guards in place to address this?

Specific recommendations and additions or deletions to the work scope

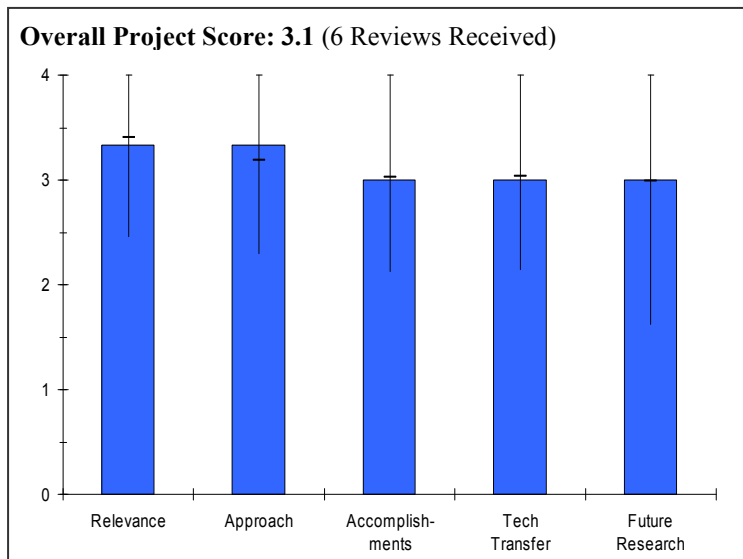
- The scope should include milestones, decision points, down-select criteria, and an end date. Such open-ended research contradicts EERE's evolving focus on tech transfer, near-term commercialization targets, etc.
- None.
- You might consider working with some of the companies dealing with sewage treatment to get an understanding of how to apply this technology.

Project # PD-10: Photoelectrochemical Water Systems for H₂ Production

John Turner; NREL

Brief Summary of Project

The goal of this research is to develop a stable, cost effective, photoelectrochemical-based system that will split water using sunlight as the only energy input. The objectives are to: 1) identify and characterize new semiconductor materials that have appropriate bandgaps and are stable in aqueous solutions; 2) study multijunction semiconductor systems for higher efficiency water splitting; 3) develop techniques for the energetic control of the semiconductor electrolyte interface and for the preparation of transparent catalytic coatings and their application to semiconductor surfaces; and 4) identify environmental factors (e.g., pH, ionic strength, solution composition, etc.) that affect the energetics of the semiconductor, the properties of the catalysts, and the stability of the semiconductor.



Question 1: Relevance to overall DOE objectives

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

- The project objectives are in line with the program objectives.
- The work addresses key barriers for PEC production of hydrogen.
- The overall NREL project is well aligned with the DOE Hydrogen and Fuel Cell Initiative goals and objectives.
- Project is clearly aligned with the DOE Hydrogen Program goals.
- Speaker clearly stated the significant challenges required to compete with commercial photovoltaic/electrolyzer systems which are near-commercial.
- Good relevance and an important pathway to realize the DOE's long term objective of renewable hydrogen.
- Competes with PV electrolysis. Relative merits and demerits of this approach against PV electrolysis should be discussed.
- If this is assumed to be a longer term solution, then the work would be considered relevant.
- This activity might also compliment NASA's space exploration concepts of generating fuel (hydrogen) and oxygen from ice collected in space and on stellar bodies.

Question 2: Approach to performing the research and development

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

- The approach is well-defined and highly integrated with other research efforts.
- The approach is appropriate to make progress toward the objectives. The challenges and drawbacks to the different materials were adequately identified. Down-selection processes and criteria were not discussed. The work is open-ended, making it difficult to assess the degree to which this approach will contribute to the overall DOE objective of a technology readiness decision by 2015.
- The efficiency and energetics issues are relatively well addressed in the project.
- Technical barriers are clearly understood as are the targets that need to be achieved to compete with photovoltaics.
- Materials fabrication is critical – is the program leveraging with collaborators who are practicing the state-of-the-art in materials fabrication (not a criticism; but required to maximize chances for success)?

- It would appear that the ability to test large numbers of different materials is critical to project success (guided, of course, by the application of solid state physics). Are there rapid material screening techniques that could be used?
- Good approach to focusing on materials starting with PV library and modifying compositions/structures.
- Good fundamentals (e.g., lattice structure) driven approach to materials development.
- Quantification of targets for the "ideal material" appropriate and an adequate way to address the gap. Right set of test matrix for the screening exercise.
- The approach appears sound.

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

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

- Progress was described for each subtask and the project was able to obtain interim support from another source to complete work that would have been cut due to lack of funding from DOE. Their partners allowed for continuation of work despite DOE budget restrictions.
- The work is moving forward, but it's not clear what exactly they're progressing towards because the project is open-ended (no end date is given). No decision points or off-ramps of any kind were mentioned.
- Granted the program has been not adequately funded over all these years, but it indicates lack of significant progress for the project to see the PI's still talking of identifying suitable materials after more than 15 years.
- Project appears to have achieved a significant number of technical accomplishments for a very modest budget.
- Low water splitting efficiency demonstrated so far. However, the problem is challenging because of competition between conversion efficiency and the corrosion resistance of the choice of materials.
- CGS system promises high splitting efficiency but still a materials challenge in terms of fabricating such a system.
- The progress is at a pace which is to be expected for a science research project.

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

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

- The project is coordinating with many other partners who are all working together to leverage the expertise and work of each team.
- Partners were listed and briefly mentioned, but the extent of the collaborations was not clear from the presentation. A dedicated slide regarding collaborations would have helped.
- Parallel stability and durability tests of promising high efficiency materials by independent entities, perhaps by industrial partners, are needed.
- Work is clearly exploratory and far from commercialization, so it's very positive to see the significant collaboration with academia as well the solar cell industry.
- Good collaboration between diverse team members. Partnering with UNLV was a good approach to negate the effects of 2006 funding pitfalls.
- The collaboration appears to be appropriate with the state of progress on the program.

Question 5: Approach to and relevance of proposed future research

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

- The future plans are not well defined for FY08; however, this is likely due to the highly collaborative effort. This project has made good progress in the past and this will more than likely be continued in their future research.
- The proposed future work is appropriate to continue making progress, but it's not pointed at any specific target or materials down-selection.
- Fundamental questions should be addressed to whether the high solar to hydrogen efficiency of these III-V or I-III-VI materials would ever overcome the cost and durability issues the PI's face.

PRODUCTION AND DELIVERY

- Is there an optimum efficiency, materials cost and durability performance that need to be met?
- The establishment of some sort of a standard photoelectrochemical test protocols should also be addressed.
- Future plans slide is a bit vague on future research for 2008. Challenges to overcome and distance from ultimate goal are great; more detailed research plan is essential.
- Proposed future research is adequate to deliver on the scope of the project. The question remains on whether with the new approach the goals of the program (presumably high water splitting efficiency to compare with PV electrolysis) can be addressed.
- The proposed future research appears to be consistent with the goals of the project.

Strengths and weaknesses

Strengths

- The project has a clear idea of efficiency goals and how this concept must measure up with competing technologies.
- Well addressed efficiency and energetic issues.
- Good alignment with DOE goals.
- Good relevance to DOE's long term technical and portfolio goals.
- Good teamwork between several partners/subcontractors.
- Good (and justified) focus on fundamentals to design the new material.

Weaknesses

- No milestones, decision points, or down-select criteria were mentioned. The work is 16 years old and still going without an end date. The work is exploratory, but the project should still have a finite scope, schedule, decision points, etc.
- There does not appear to be enough emphasis on material stability and durability issues, which is critical.
- Continued work on semiconductor materials, no matter how efficient they may be under ideal laboratory environments, might not be worth the effort if they degrade under real life conditions (need industrial partner(s) to conduct real life testing).
- Lack of clarification on the targeted deliverables in terms of water splitting efficiency and timelines.
- How do the materials being researched match with the "green" and recycle requirements coming out of Europe and California?

Specific recommendations and additions or deletions to the work scope

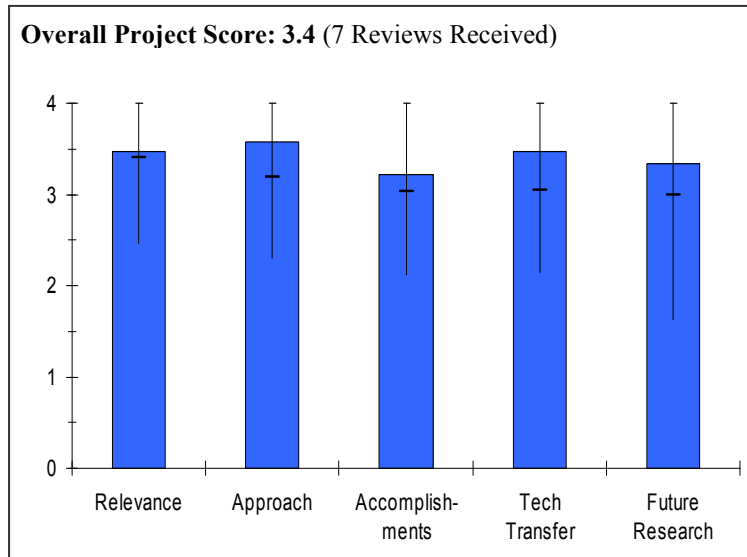
- Add to scope to look at material stability & durability issues.
- Overall, the project objectives are important in future hydrogen mix and this work should continue to be funded. However, the project needs infusion of new ideas to achieve significant gains in efficiency, lower material cost, and higher stability and durability performance.
- The scope is adequate. Further additions might further delay the progress.
- Keep track of "green" requirements. Generating a material that industry would be prohibited from using would be counterproductive.

Project # PD-11: Development of Solar-powered Thermochemical Production of Hydrogen from Water

Chris Perkins; UNLV

Brief Summary of Project

The purpose of this project is to develop solar thermochemical water-splitting routes to hydrogen production. The objectives are to: 1) identify one or more competitive solar-powered water splitting process for hydrogen production; 2) conduct experimental studies to complete quantitative selection; 3) perform numerical and experimental evaluation of solar receiver concepts for integration with thermochemical processes; 4) implement consistent methodology for comparing economic viability of cycles; and 5) demonstrate at least one potentially cost competitive solar thermochemical water splitting cycle on-sun at a small scale.



Question 1: Relevance to overall DOE objectives

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

- Most of the project’s aspects align with the DOE RD&D objectives.
- The funding level seems high for the effort.
- Producing H₂ from non-finite resources that are plentiful in America is an essential part of a sustainable hydrogen economy. Producing H₂ directly and efficiently from solar energy is an important part of this.
- Has relevance – long term potential – solar concepts do not have a record of being economical.
- In that the Program's goals include a significant amount of hydrogen production from renewables, solar-derived hydrogen cannot be ignored. This project offers an excellent opportunity to utilize this country's vast solar resources.
- The project is not taking into account the cost of delivering hydrogen from central concentrated-solar production sites, which may be remote and far away from hydrogen demand. While the H2A model can be used to calculate the central production cost, it is highly recommended that the analysis include H2A delivery analysis to determine if the viable locations for this technology result in unusual and unacceptable hydrogen delivery costs.
- Solar powered water splitting has the potential to meet the need of hydrogen production from renewables if the capital cost can be reduced. This high risk research that requires some significant R&D breakthroughs will likely only be undertaken by the federal government, so good fit.
- Good relevance and an important and interesting pathway to realize the DOE's long term objective of renewable hydrogen.
- Competes with PV and direct photoelectrolysis. Relative merits and demerits of this approach against those approaches should be discussed.
- Supports MYPP for high temperature solar thermochemical technologies.
- High risk, high payoff project appropriate for DOE.
- Low greenhouse gas emissions approach to H₂ production.

Question 2: Approach to performing the research and development

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

- The project approach focuses on the technical barriers effectively.

PRODUCTION AND DELIVERY

- It is good that cost is being used as a determinant in the selection of the cycle. Materials are a big issue, but this is not being fully addressed.
- Good mixture of modeling and experimental work.
- Concentrating on the very high temperature capabilities of solar concentrators is a good selection. This increases the range of possible chemical reactions, and thereby casts a wider net of opportunity.
- Consideration of potential environmental impacts in the process down-select criteria is a plus.
- Avoiding duplication of effort on the Sulfur/Iodine cycle is good management.
- Has screening and down select mechanisms – good approach.
- Much parallel work – need to down-select soon to focus funding on the best approach(s).
- See analysis (costs, manufacturability) to help in down-selecting, but balance of analysis vs. experiments.
- While this technology may be cheaper than PV/electrolysis, does the inability to co-produce electricity reduce its potential application in a future hydrogen/electricity energy sector? Also, co-production of electricity can reduce necessary storage costs. Project economics need to evaluate the impact of storage costs.
- Great work on heliostat design.
- On-sun testing is very appropriate. Glad this isn't just a lab simulation or a CFD analysis.
- Good focus and progress on electrochemical cell design.
- Pleased to hear that 30 or more experts assembled to assist in generating ideas for designs that could lead to a lower cost heliostat.
- Good collection of various solar thermochemical cycles. Overall good approach to address the gap by expanding the funnel of ideas followed by down-selection.
- Excellent efforts to build closed experimental cycles and then use observed performance as the selection criteria.
- Good combination of experiment and CFD modeling.
- Good inclusion of H₂A in evaluation of cycles.
- Good risk reduction – carrying along multiple different (5) cycles.

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

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

- It appears that good progress in the experimental work has been made.
- Economic evaluation is extremely important and it is great to see that progress was made on the heliostat cost reduction. It would be good to see the cost reduction numbers validated.
- Grinding of the CdO to improve hydrogen production rates seems to require too much work for the hydrogen produced, especially considering the progress on the other candidates.
- Halfway thru the project they have made good progress.
- Screening and scoring hundreds of potential reaction cycles, they reduced the focus to a dozen cycles potentially compatible with solar concentrating capabilities.
- Five process flow sheets were downselected that offer conceivable pathways to success.
- Processes and reactor configuration options have been identified for these five. Lab-scale hardware investigations with on-sun proofs of concept have been thought through and offer reasonable chance of success for the latter half of this project.
- Good review of many thermochemical cycles.
- Considerable experimental work – lots of reactors.
- Excellent progress on down-selecting options.
- Need more information on how the 351 cycles were scored and how so many were eliminated to get down to 12.
- CFD modeling great for designing reactor.
- Good progress on doing on-sun experiments. Suggest these be integrated with analysis on system design, including storage and delivery requirements.
- Great that project is doing cost analysis with H₂A. Need more info on capital cost estimation. Who is reviewing cost analysis?
- Down select and experimental testing of 5 cycles and transference of eight from a starting point of 351 is impressive.

- Slow technical progress. At this stage some of the cycles should have been ruled out by identifying the showstoppers.
- Quantification of technical accomplishment (in terms of improved efficiency) should be provided.
- Good progress and solid technical work in design of the solar receiver reactor.
- Comparison between ZnO vs. HyS costs was informative. Similar should be done for all cycles.
- Good progress in improving conversion in aerosol reactors.
- Good recognition of need for and pursuit of heliostat cost reduction.
- Good progress on reactor development for several different reactor types.

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

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

- There is a large team assembled, but it is hard to tell who is contributing what to the project. It would be useful to indicate which team member is responsible for the different aspects and tasks.
- Good progress in selecting candidate processes and conceptualizing process configurations for these diverse processes is indication of good teamwork across the spectrum of program participants.
- Keeping up with the commercially available hardware capabilities rapidly becoming available in Europe, especially Spain, is an important source of collaboration.
- Broad group of collaborators.
- Excellent team. Would be good to show who is doing what in the presentation. This was a comment from last year, which although responded to, was not incorporated into the presentation.
- Workshops on heliostat design were a great way to get expert advice and direction. Is one of the project partners going to carry out new design analyses?
- A number of government agencies as well as private industry will benefit from the ongoing fundamental material analysis.
- Good and seamless collaboration between diverse team members.
- Technology transfer appears easy once the downselected cycle and its merits have been demonstrated, because the choice of scale is adequate.
- Broad participation of academic, national lab and industrial partners.
- Workshops brought in additional expertise.

Question 5: Approach to and relevance of proposed future research

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

- There is a great deal of work that needs to be done in a relatively short amount of time.
- There should be enough data to begin down selection and then materials development for the cycles selected.
- The team is focused well on the materials and process challenges presented by their down-selects, and is positioned to be successful in the second half of this project.
- Have a good down-select plan but need to do it and not stay in the science mode.
- Suggest systems integration field work.
- Need to provide more details about lifecycle impact of successful cycle.
- Concern remains over the costs associated with the heliostat.
- Proposed future research is adequate to deliver on the scope of the project.
- Heliostat cost reduction a big barrier to success of the project and the promised cost reduction. Strong efforts needed to achieve that (outside of the scope of the current project).
- Good continued use of H2A.

Strengths and weaknesses

Strengths

- There is a broad team with plenty of funding.
- There appears to be a good mix of modeling and experimental work.

PRODUCTION AND DELIVERY

- They are focusing on cost as a discriminator for the cycles.
- This is an important renewable energy pathway for American society's energy needs in this century. The researchers know that and have dedicated their efforts accordingly.
- The solar research facility at Sandia is well suited to perform the on-sun testing, and the team's familiarity with high temperature solar capability and limitations provides a reliable project configuration.
- Deployment of new solar trough electrical generation stations and the emergence of heliostat collector fields for central electrical station use is reducing the cost of solar components and building Solar-Thermal capability in several parts of the world. Hydrogen production based on this technology base will be a winner.
- Good understanding of chemical cycles.
- Solid approach and technical depth.
- Good teamwork between several partners/subcontractors.

Weaknesses

- They should have developed the solid particle receiver earlier to get more data.
- There are significant materials challenges that have not been addressed particularly for the higher temperature operation.
- At this stage, it seems the team is focused on too many cycles.
- The team seems reluctant to down select to a top 1-3 cycles and focus their resources on them.
- While concentrating on the very high temperature capabilities of solar-thermal technology makes good strategic sense, progress in developing practical process hardware for the Sulfur Iodine process that is compatible with CSP is not being pursued.
- High temperature materials issues are the apparent major barrier to development of practical process hardware for the selected cycles. Despite its focus on very high temperature cycles, this project is constrained to select from existing materials rather than develop new ones compatible with the temperatures and reactant environment.
- Strong reliance on Heliostat cost reduction.

Specific recommendations and additions or deletions to the work scope

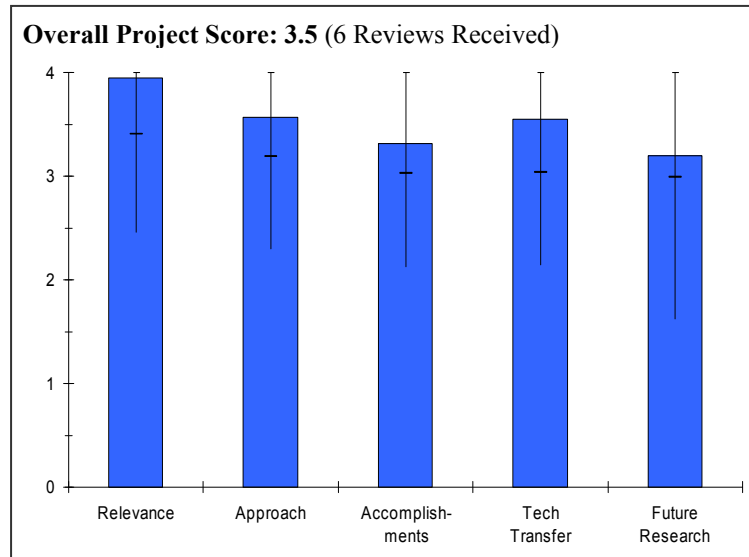
- It is recommended that they down select to 1 or 2 cycles.
- It is recommended that they need to focus more on materials development.
- While the use of H₂A will "likely" conservatively cover costs of parasitic process losses, it's reasonable to have the researchers consider specifically the possible energy losses due to materials handling to support the process concept. This may be particularly important in evaluating acceptability of reactor efficiency and material recycle ratios up and down a tower.
- Heliostat cost reduction is possible and would help the economics of these processes, but pursuing those reductions lies outside the scope of this project, I believe. If heliostat cost reduction is necessary to meet project economic targets, identify the reductions needed.
- Practical hardware configurations for solar powered H₂ production with the Sulfur-Iodine process should be designed by this team as soon as the nuclear energy based project team shows successful closed loop operation and can make process design information available.
- Much better analysis efforts.
- The scope is adequate. Further additions might further delay the progress.

Project # PD-12: Hydrogen Delivery Infrastructure Options Analysis

Bruce Kelly; Nexant Inc.

Brief Summary of Project

The objectives of this analysis are to: 1) refine technical and cost data in H2A Component and Scenario Models to incorporate additional industrial input and evolving technology improvements, including significant data additions and delivery system storage analysis and optimization; 2) explore new options to reduce hydrogen delivery cost, including novel carriers; 3) expand H2A Component and Scenario Models to include new options leading to Version 2 models; and 4) provide bases to recommend hydrogen delivery strategies for initial and long term use of hydrogen as a major energy carrier.



Question 1: Relevance to overall DOE objectives

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

- Understanding delivery options and costs will be critical to understanding hydrogen production and delivery infrastructure.
- Breadth of delivery options is necessary.
- The project objectives are right in line with DOE objectives.
- For delivery analysis, critical barriers are addressed.
- Focused on key elements of delivery cost.
- The data collected in the project moves DOE closer to providing a robust H2A model that can (and likely will) have global reach. This is a critical tool for analyzing the costs claims for other DOE funded programs.
- Project well aligned MYPP delivery analyses.
- This project appears to be dead on with the activities to support the Hydrogen Fuel Initiative.

Question 2: Approach to performing the research and development

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

- Hydrogen production system considerations should be included in minimum storage requirement optimization. For example, on-site production systems will not necessarily have 100% availability. What considerations have been given to maintenance schedules when determining that only 0.3 days storage will be required?
- Appropriate not to place too much emphasis on currently unknown alternative delivery options.
- Although geologic storage is certainly interesting, optimizing an infrastructure that appropriately considers location and feasibility is an entire research project in and of itself.
- The approach is very thorough, very detailed, and appropriate for meeting project objectives. The analysis includes examination of many key variables that must be considered for different delivery options.
- Looking at many pathways.
- Modeling approach which gives DOE understanding of costs of all options.
- Giving inputs to H2A which is key cost modeling system.
- The collection of information from industry sources will be helpful to industry; however, it would have been useful to see some of the actual station designs that resulted from the new cost and dimension inputs for storage and compression.

PRODUCTION AND DELIVERY

- Objective approach.
- Demand data obtained from industry is crucial to provide realistic representation of system performance requirements.
- This approach is very rational and appropriate.

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

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

- Project continues to make significant progress, although there appears to have been some scope growth over the last year. It is important to focus first on the most mature and understood technologies for the initial optimization. Too much is unknown at this point about some of the higher risk options, such as carriers and geologic storage.
- The project appears to be on track. A significant amount of work has been done over the past year.
- Project milestones were not adequately addressed in the presentation.
- Many small accomplishments which add up to excellent H2A delivery model costs.
- Much data — very useful in understanding options.
- For \$2 million dollars I would expect to see the actual improvements to the model demonstrated. Perhaps this will be part of next year's update. Also further explanation is needed around the level of maximum storage for worst case demand period (July 4th on a Friday). Seems counter intuitive that it would be only 1/3 of a day's need. Probably worthwhile having a special session with Energy company employees who actually work on optimizing fuel supply chain to stations. These individuals probably do not reside on the tech teams.
- Very good progress developing models and developing requirements.
- The accomplishments listed to date appear impressive and are timely.

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

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

- Good diverse team.
- Project participants were listed, but the extent of the various collaborations were not adequately addressed in the presentation. A dedicated slide to this would have helped.
- Good, broad team.
- H2A work some of best collaborative work in DOE portfolio.
- As a robust H2A model will have global reach, the potential contribution is high. Actual reports based on analysis results are needed for a full appreciation of the contribution.
- Project has good national lab and industry (gas and petroleum) representation.
- The collaboration is appropriate and diverse.

Question 5: Approach to and relevance of proposed future research

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

- Would be useful to consider hybrid systems for managing peak demand cycles for non-pipeline-fed stations. Instead of optimizing station design around July 4, what happens if it is optimized around a more average summer day? Can peak be met with delivery from a central plant?
- In addition to delivery strategies, it would be useful to have recommendation on areas where there are gaps in current technology. For example, in the refueling site design, steps up in compressor size had an obvious impact for peak/average flow considerations.
- Although I recognize that assumptions on permitting and siting costs have to be made and that those costs for similar fuels are the appropriate starting point, it would be useful to include more variability (e.g., significantly higher costs) in this parameter for the sensitivity analysis and design optimization.
- The proposed future work is appropriate to advance the work toward the project objectives.
- Modest work for future.

- Work should be declared a success and funding stopped.
- I think greater emphasis should be placed on the current delivery models to make sure that they are as sound as possible. For instance what would a forecourt design look like with the new data? How would a centralized plant and distribution pipeline be designed for optimization?
- The proposed future work appears to be consistent with the reported activity to date.

Strengths and weaknesses

Strengths

- Team brings in a lot of industrial expertise, as well as strong modeling and analysis capabilities.
- Many collaborators.
- Excellent team play.
- The work to date reflects the current state of technology well.
- The work will be helpful with estimating the economics of the process.

Weaknesses

- Perhaps considering too many options and venturing into areas where the error bars are significant. Focus first on the known technologies.
- The effort is 70% complete, but it does not appear that even a preliminary guess as to the leading options has been formulated. This is a minor weakness because rankings and recommendations are included in future plans. Still, a preliminary estimate of leading options as soon as possible would be valuable because it could help identify R&D priorities for the next few years.
- No system to get current feedback on total costs. By this, I mean estimate – get true final construction and owners costs and then change model.
- H2A is incorporated in several models already (Hypro, Hytran, etc.) a written management of change plan that highlights how and when these other models will adopt the changes would be helpful.
- The work does not seem to address issues related to contamination of the fuel during transport and storage.

Specific recommendations and additions or deletions to the work scope

- Minimize emphasis on more advanced delivery options.
- Not knowing how service station demand data was aggregated, I'm not certain of reliability of 0.3 day storage oversize. It differs significantly from a rigorous, but much more limited (3 station) simulation study and from station oversizing currently practiced by at least one oil company. If further service station study is performed, it would be useful to learn rules Chevron applies to size stations – which should consider not only demand, but other system factors (e.g., distribution disruption factors) some of which might apply to H₂ distribution.
- Evaluate potential sources of contamination from the well to wheels and factor in the cost of maintaining the fuel quality.

Project # PD-13: Fundamental and Modeling of Pipeline Hydrogen Embrittlement

Petros Sofronis; U. of Illinois

Brief Summary of Project

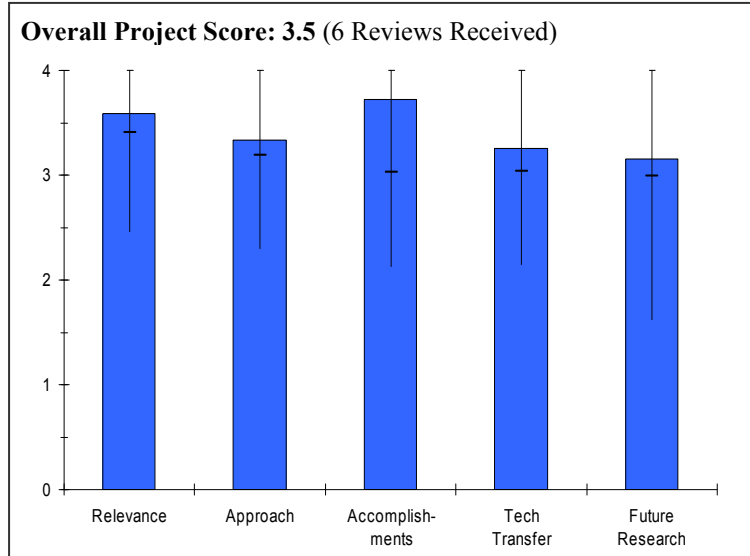
The objective of this project is to come up with a mechanistic understanding of hydrogen embrittlement in pipeline steels in order to devise fracture criteria for safe and reliable pipeline operation under hydrogen pressures of at least 7MPa and loading conditions both static and cyclic (due to in-line compressors) for the existing pipeline steels and to propose new steel microstructures.

It is emphasized that such fracture criteria are lacking and there are no codes and standards for reliable and safe operation in the presence of hydrogen:

- There are no criteria (codes and standards) with predictive capabilities;
- Pipeline steels may be dangerously susceptible to fatigue failure in the presence of hydrogen.

The Illinois mechanism-based approach will:

- Develop design criteria to be used for codes and standards for safe and reliable operation;
- Avoid unnecessary repairs and shut-downs by minimizing unnecessary levels of conservatism in the operation of pipelines;
- Reduce capital cost by avoiding conservatism.



Question 1: Relevance to overall DOE objectives

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

- For a viable H₂ economy, hydrogen embrittlement must be understood and strategies to mitigate it must be developed. This project is developing a mechanistic understanding of the embrittlement problem. It is not clear that it should be funded by the production and delivery group. It may be more applicable to the Codes and Standards group.
- Hydrogen pipelines remain the most cost-effective means for delivering hydrogen over wide areas. Understanding materials issues is key to understanding materials, operational, and permitting requirements.
- Although this project does not address the economics of pipelines, it does help to address the DOE safety and reliability objectives for transition pipelines.
- Pipelines and hydrogen embrittlement is a key barrier to hydrogen infrastructure and transportation issues.
- The results from this work shall feed effectively into codes and standards development and also to some extent in identifying appropriate material/design for hydrogen storage.
- This project's objective is to come up with a mechanistic understanding of hydrogen embrittlement in pipeline steels in order to devise fracture criteria for safe and reliable pipeline operation under hydrogen pressures of at least 7 MPa. Fracture criteria for hydrogen pipeline is missing and this project tries to look into it and therefore there is very good relevance to overall DOE objectives.

Question 2: Approach to performing the research and development

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

- The approach is good considering the funding level.

- Model validation is required; however, the low funding level may prevent this. Maybe the industry partners would be willing to validate the model as part of their cost share?
- Modeling effort is very good, but is unclear how adequately potential intensified interactions are being addressed. For example, what is the effect of multiple cracks and how does proximity influence this?
- Fully-reflecting the impact of defects and impurities will be challenging.
- The morphology studies of used pipeline material are quite helpful in understanding the proposed degradation mechanisms.
- Combined modeling and analysis of hydrogen transport and materials failure is the right approach to address the problem of hydrogen embrittlement.
- Strong focus on fundamentals and measurements to feed into the model that will allow for development of predictive tool to predict failure criteria.
- Impressive breadth and depth in establishing the mechanism and solution approach for predicting failure of hydrogen induced crack propagation.
- Development of simple design criteria for failure is a good target.
- Tension experiments to identify macroscopic plastic flow in pipeline steels – very useful.
- Permeation experiments to identify diffusion characteristics – needed to understand crack growth mechanism.
- Experiments to determine stability of crack propagation to assess catastrophic failure scenarios.
- Development of a mechanistic model to establish failure criteria with predictive capabilities – very important to establish codes for hydrogen pipelines.

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

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

- Considerable work has been accomplished.
- There has been a lot of modeling efforts; however, experimental data to validate the models is required.
- Very strong preliminary effort within the available funding.
- Given that funding has been inconsistent and could have been a key barrier to progress, their completion of the permeability measurements and piping characterization studies really demonstrate a very high level of commitment to the work and program.
- Solid progress in model development, validation, measurements and predictive capability.
- Effort of the project team is exemplary and accomplishments easily outdistance the amount of funding provided to date.
- Significant progress for small and intermittent funding.
- Slide #20 "Accomplishments vs. Project Milestones" is an excellent one. All the milestones were met. Hats-off to the PI for doing a lot with very little funding. Glad to see this type of work is funded by OHFCIT.

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

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

- There are some ties with industry.
- Large number of presentations and publications
- Good breadth of partners identified.
- Looks to be a nice mixture of industry and the national labs. Still I am unclear about the full role of Air Liquide and APCI. Are they just a used pipe supplier or are they integrally involved in information and project design exchange?
- Happy to hear willingness to work with ASME B31 pipeline working group.
- Good collaboration with ASME in terms of establishing the appropriate factors of safety for hydrogen pipelines. This is highly needed.
- Need to lay-out a plan for future technology transfer and codes and standards development.
- Strong collaboration within delivery working group and with international delivery organizations.
- Used the Oregon Steel Mills steel for their initial experiments.

PRODUCTION AND DELIVERY

- Already looked at the inclusions in the steel structure used by Air Liquide.
- Good partners from industry and national labs (ORNL and Sandia).

Question 5: Approach to and relevance of proposed future research

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

- Model validation is needed. Perhaps they can compare the model to industry provided data since the funding levels are low?
- The research plan does not include studies of mixed use pipelines, which is the more likely scenario. The impact of the cycling should be investigated.
- Would benefit from more in-depth discussion of the design for fracture testing experiments.
- Would be nice to better understand the international connection and benefits to the project.
- Proposed future research is adequate to meet the stated objectives of the project.
- Quantification of targets will be useful in assessing the merits of proposed future work.
- Future work is good, but could be broadened to consider fatigue studies. Need to work with delivery working group to determine proper test to be carried out and threshold properties to be achieved.
- The future plans given in slides #21 and #22 are very good. PI will measure diffusion characteristics of pipeline steel samples provided by both Air Liquide and Air Products. He will determine the uniaxial tension flow characteristics in the presence of hydrogen. In collaboration with Sandia, the PI will carry out fracture testing. PI has a long term plan to continue this work.

Strengths and weaknesses

Strengths

- The PI appears to be using a sound approach and modeling.
- It appears that there are good partnerships with relevant companies.
- Good focus on non-uniform materials.
- Outstanding technical leadership provided by the PI.
- Solid partnership and team execution.
- Developing strong fundamental materials basis.
- Strong background in fracture mechanics.
- Availability and training of graduate students and post-docs.
- Complete understanding of the pipeline materials problems.

Weaknesses

- Model validation is lacking, however, the PI definitely would like to validate the model. Perhaps, industry partners can provide data or validate the model?
- The project is not examining the effects of impurities or swings in pressure.
- This project is of great interest to industry for many applications in addition to the President's Hydrogen Initiative. Yet, the contractor cost share is relatively low.
- Multi-fracture and impurity interactions will be challenging. This is not so much a weakness of the project, rather a challenge that may be difficult to fully address.
- It will be worthwhile to set up a qualification test based on the results of this project at a pilot scale where such a pipeline is tested at an appropriate scale in the expected cyclic conditions, and failures are verified.
- Need stronger collaboration with industry (e.g., AIR LIQUIDE) to utilize lessons learned with existing hydrogen pipeline infrastructure.
- Does not contribute to screening/evaluating current pipeline materials. Modeling and experimentation under ideal conditions need to expand to real work gas conditions pipes may see and include mixed or trace contaminant affects on fracture.
- Seems the mechanical properties are measured on flat samples. How will the properties correlate with tubular samples?

Specific recommendations and additions or deletions to the work scope

- Fully funding this project would significantly improve its impact.
- This project should be of great interest to industry. The industry cost share should be higher.
- The project should address the effects of impurities (water, CO, CO₂, sulfur ...) that may be included in the gas.
- The project should address the effects of how cycling of either the gas composition or the gas line pressure affects the pipes. For example, changing the gas in the pipes from natural gas to hydrogen and back or using the pipeline for not only delivery, but for storage with its resultant pressure swings.
- It would be good to know how the use of the hydrogen transport model with Kinder Morgan progresses as a test to the models reliability.
- Recommend to continue the project as is to establish the failure criteria but further emphasize on creating an interface to assist in establishing factor of safety and codes and standards development.
- Future scope might include doing similar analysis for off-board storage at different pressure and establish failure criteria under those conditions.
- Impact of impurity and their concentrations is also an additional future scope that shall be considered.
- Study the effect of inclusions.
- Model for other gases such as Hythane and NG.
- Study the effect of moisture and trace impurities (such as sulfur and chlorine) in the hydrogen gas going through the pipeline.

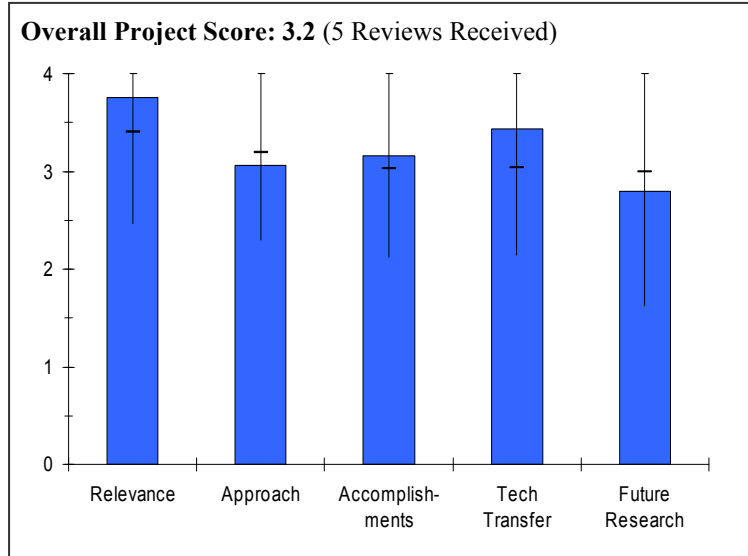
PRODUCTION AND DELIVERY

Project # PD-14: FRP Hydrogen Pipeline

Barton Smith; ORNL

Brief Summary of Project

The overall objectives of this project are to: 1) reduce the cost of hydrogen transport from central and semi-central production facilities to the gate of refueling stations and other end users to <\$0.90/gge by 2012; and 2) investigate the use of fiber-reinforced polymer (FRP) pipeline technology to transmit and distribute hydrogen and achieve reduced installation costs, improved reliability, and safer operation of hydrogen pipelines. Objectives for fiscal year 2007 are to: 1) demonstrate integrity of FRP pipeline during hydrogen exposure; 2) assess hydrogen leakage in existing liner materials; 3) assess joining methods for FRP pipelines; and 4) determine integrated sensing and data transmission needs.



Question 1: Relevance to overall DOE objectives

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

- Addresses the key element of pipeline cost (welding, materials, embrittlement) by coming up with a new groundbreaking technology.
- The project is clearly relevant to DOE's objectives of identifying low cost, safe and reliable approaches for hydrogen transmission via pipeline.
- High relevance due to cost and performance of existing pipeline systems.
- Applications for transmission and distribution.
- Since instrumentation can be integrated into manufacture and installation, very cost effective.
- The work to date is extremely relevant to the hydrogen initiative.

Question 2: Approach to performing the research and development

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

- It is unclear how adequately supply and demand interactions for fiber-reinforced polymer materials have been addressed. Cost assumptions are likely overly optimistic.
- Need to ensure that effects of weathering in the presence of hydrogen are adequately addressed.
- Need to ensure the effects of impurities on pipeline integrity are adequately addressed.
- Good technical approach, but more effort needs to be funded to assume success and determine if FRP systems could be economically better than current systems. Some work but more needs to be done.
- May need more "contractor" input as they evaluate sensor and joining technologies.
- Although some ASTM test methods were listed, the accelerated ageing test appears to be steady state. What happens with pipe performance over time when pressure and temperature are cycled? Is a year long enough for accelerated ageing testing? The principle should provide an exhaustive list of tests to be performed on the FRP pipe along with expected outcomes.
- Current material is commercial; this program is more characterization than R&D.
- Material systems are critically needed and address one of the largest barriers.
- Due to funding issues, this activity needs to be accelerated to provide DOT characterization of pipeline safety.

- The research to date has been focused on one composite pipeline material, which makes sense because it allows for the generation of an evaluation methodology.

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

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

- Reasonable progress within available funding.
- Progress limited by funding – good progress considering funding.
- Given that very little funding has been provided the level of effort to understand the economics and potential of FRP pipe is impressive.
- Progress was limited by funding.
- Good initial data on hydrogen permeation.
- Progress is about where it should be based on schedule and budget.

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

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

- Good partnership with pipeline supplier, but might benefit from partnering with pipeline users and/or standards development bodies (e.g. ASME).
- Good collaborators with other researchers and industry.
- Pipeline working group is a good team.
- The project includes industry and academic participants. However I do think that they should make the effort to discuss their experimental design in detail with the scientists at U of Illinois to see if Dr. Sofronis et al. may be able to assist in developing a predictive model of failure modes under varying environments. U of Illinois may also learn from this project. Perhaps this is happening via the pipeline working group.
- Good collaboration.
- Focused on using existing manufacturing technology.
- Provides manufacturer a means to improve existing technology for oil and gas industry.
- The collaboration is not as broad as I would hope. Collaboration with API, ASME, ASTM, CSA, CGA and SAE would have been expected.

Question 5: Approach to and relevance of proposed future research

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

- Would benefit from more detailed discussion of performance testing parameters and how this will be incorporated into economic analysis.
- Weak future plans – maybe this is due to shortage of work so far.
- Needs to propose work that gets FRP pipelines to the cost target.
- Good evaluation of codes and standards needs.
- I believe the plan could be made more robust through greater engagement with hydrogen gas suppliers. Would be good to have their input on the reasonableness of the experimental design and the economics.
- Proposed work is important to determine if instrumentation and joining will hold up to prolonged exposure.
- Accelerated aging studies must be performed on liners and formed joints.
- The proposed future activities are consistent with the goals.

Strengths and weaknesses

Strengths

- Good identification of alternatives to conventional pipeline materials.
- Big step-out possibilities.
- Good team.

PRODUCTION AND DELIVERY

- Good approach to reducing cost of installation.
- This project addresses an escape in the present activities.

Weaknesses

- Unclear how adequately pipeline integrity testing has been addressed.
- Inconsistent funding.
- Need risk assessment on damage tolerance.
- Field manufacture needs to be verified for pipe sizes over 4 inches.
- The project is presently limited to a single material. Evaluation of other similar composite and plastic materials is needed.

Specific recommendations and additions or deletions to the work scope

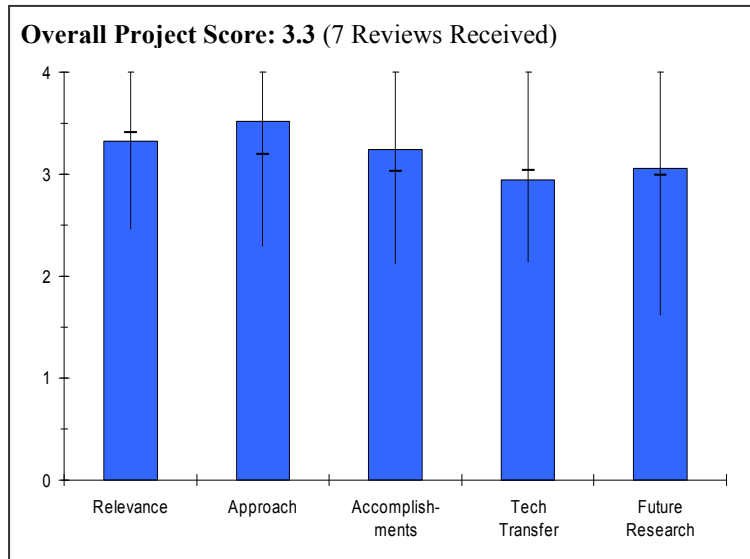
- Could they do more, faster if they had more money? As pipelines are becoming more important to the transition, this project should probably be elevated in terms of priority.
- Provide funding to complete the study in a timely manner.
- Collaborate with SAE and ASME on other non metal, common plumbing materials that should be evaluated.
- Collaborate with ASTM and API on any material specifications for these materials which need to be revised or generated.

Project # PD-15: Scale-up of Hydrogen Transport Membranes for IGCC and FutureGen Plants

Doug Jack; Eltron Research Inc.

Brief Summary of Project

The objectives of this project are to: 1) develop high-throughput, low-cost H₂ separation system suitable for application with coal-based synthesis gas, including improved tolerance to contaminants (S, Hg, etc.) and enabling cost effective capture of CO₂ for sequestration; 2) select candidate mechanical configuration (tube vs. plate; metallic alloy vs. cermet) considering cost, performance, and manufacturability of membrane and system; 3) scale up membrane and system from 0.45 lb/day of H₂ using lab gases to 220 lb/day in coal-derived syngas; 4) integrate membrane design into a 4 ton/day H₂ production unit; and 5) determine optimum process design and cost and compare vs. other systems.



Question 1: Relevance to overall DOE objectives

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

- Good presentation, relevant to FutureGen program including sequestration.
- The project aligns well with the President's Hydrogen Fuel Initiative by developing a critical element needed for commercial hydrogen purification, i.e., the scale-up of hydrogen transport membranes for FutureGen and IGCC applications.
- CO₂ capture in line with DOE goals.
- DOE targets for high flux and selectivity met.
- Exceeds DOE goal for CO₂ capture considerably.
- The development of a technology for hydrogen separation from fossil fuel combustion exhaust gases is at the heart of a sustainable transfer to a hydrogen economy. It could not be more in line with America's demand for future energy.
- Low cost, higher performing membranes are needed as an alternative to PSA.
- Carbon sequestration is a major challenge due to the amounts of gases needed to be processed.

Question 2: Approach to performing the research and development

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

- Company has solid background in materials.
- Scale up steps are appropriate – progress made to skip a step.
- The technical approach follows five major steps to meet the objectives of the project. These steps include the scale-up of the membrane system through two stages (220 lb/day and 4 ton/day) and culminate in an optimum process design and cost comparison.
- The technical approach also includes an identification of the business development challenges associated with the project and strategies to address these challenges.
- Built on past work to identify a preferred material of choice for the membrane that will be used in the scale-up tests.
- Proposed acceleration of scale-up is viable.
- Excellent summary of objectives and approach.

PRODUCTION AND DELIVERY

- Excellent methodology and approach, economics will drive the adoption of new technology.
- Approach is good.
- Development schedule could be more aggressive.

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

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

- Elton has already met or exceeded 2010 targets.
- Material development efforts can compete with existing materials.
- Improved CO₂ capture with their approach.
- Good scale-up of catalyst deposition.
- Achieved the first scale-up step under full WGS conditions.
- Improved the knowledge base of the impact of membrane materials and preparation techniques on degradation and embrittlement.
- Developed necessary process engineering and economic tools for system optimization.
- Developed a membrane system that met/exceeded the 2010 DOE targets for flux and selectivity.
- Constructed a 1.3 lb/day unit that will now be used for collecting data for life cycle analyses.
- Promising membrane pretreatment methods developed that can improve membrane performance.
- Membrane system meets 2010 DOE targets for flux.
- PI could provide more details on decay but did indicate their system performs better than Pd.
- 5% improvement compared to Selexol.
- Warm gas cleaning shows promise.
- Avoidance of hydrogen embrittlement and hydride formation is very important. The investigators noted this but gave no details on how project is addressing these two issues.
- Already meets 2010 targets for hydrogen separation. Would consider adjusting targets for the project.
- Lifetime/durability testing needs further work.
- Developed their own new materials.
- Level of intellectual properties.
- 97/98% capture from exhaust stream – Excellent work.
- There is some concern that as the project advances to larger development scale, it may encounter some of the same problems as other, similar past projects. Companies have been involved in a number of similar technology programs, but approach seems mired in the same problems as ITM, SECA, etc.
- Manufacturing and sealing for joints have not been addressed in material selection 3.

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

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

- Technology transfer/collaborations not adequately addressed due to time, N/A?
- Would have liked to spend some time on this area, given how close they are to "commercialization."
- Would like to hear more about patents that have come out of this research.
- The project has three partners, all of whom appear to be well qualified to participate in a project of this nature.
- It is unclear if any of the participants will invest in the effort to commercialize the concept or if their level of participation will remain on a fee-for-service basis. Eltron appears to be aware of this and is seeking a commercialization partner.
- Good collaboration between partners.
- Coordination among collaborators.
- Intellectual property issues appear unresolved.
- Market penetration and commercialization issues appear unresolved.

Question 5: Approach to and relevance of proposed future research

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

- Schedule looks reasonable, based on logical steps and accomplishments to date.
- The proposed future research effort hinges largely on the identification of a suitable commercial partner. No contingencies or optional pathways have been discussed to address project risks, including that mentioned above.
- Focused R&D plan culminating in the design, construction, and testing of a 4 TPD unit.
- Consider adding partner for catalyst coating.
- Determine mechanism of failure. Collaboration with a materials characterization group would be valuable.
- Key areas of future work are defined as a result of the previous work.
- Working towards addressing lifetime/durability issues, for example hydrogen embrittlement.
- Scale up is planned into future work.
- Economics look favorable.
- Appears "business as usual".
- Project could be more aggressive or market driven.
- If current industry demand required technology it might be developed faster.

Strengths and weaknesses

Strengths

- Met or exceeded DOE goals.
- Company seems to understand technical issues such as embrittlement.
- Good that Eltron looks at manufacturing and not just technical components.
- Promising approach for membrane scale up.
- Developed model for membrane performance that can minimize the number of tests needed.
- Results for cermet membrane vs. Pd membrane appear very promising.
- Gantt chart for future work provided.
- Large amount of intellectual property being generated; 3 patents already plus another 4 in processing.
- Clear leadership working with academic and industrial institutions.
- Good technical team.
- High performance hydrogen transport membrane family identified.

Weaknesses

- Put dimensions on reactor slide to show relative size.
- Put dimensions on tube design slide to show relative size.
- Address key technical HTM issues identified so that they do not become show-stoppers.
- Conclusive tests will be needed to address the potential of hydrogen embrittlement.
- Design life of catalyst (for hydrogen disassociation) is 5 years. If catalyst fails, the entire membrane assembly will have to be recoated and reassembled.
- No management push to drive technology development faster or propose more aggressive solutions.
- Marginal improvement.
- No independent assessment of risk or cost.
- Not clear how they will fund 220 lb / day unit and test by end of 2009.

Specific recommendations and additions or deletions to the work scope

- The project produces a by-product stream of CO₂ with about 5 percent hydrogen. Economics of capturing this 5 percent of hydrogen may be dictated by the CO₂ sequestration techniques. There might be an opportunity for a CO₂ sequestration project to team up with this project to address this matter perhaps through joint funding.
- A thorough study of the mechanisms of hydrogen-related failure is needed. What is the mechanism? Hydrides and embrittlement?
- Very good project, which is meeting 2010 goal. However, the level of spending is very high for one element of the hydrogen community, when compared to other DOE funded projects. Eltron is currently generating lots of patents, will this hinder future development of carbon capture and storage?

PRODUCTION AND DELIVERY

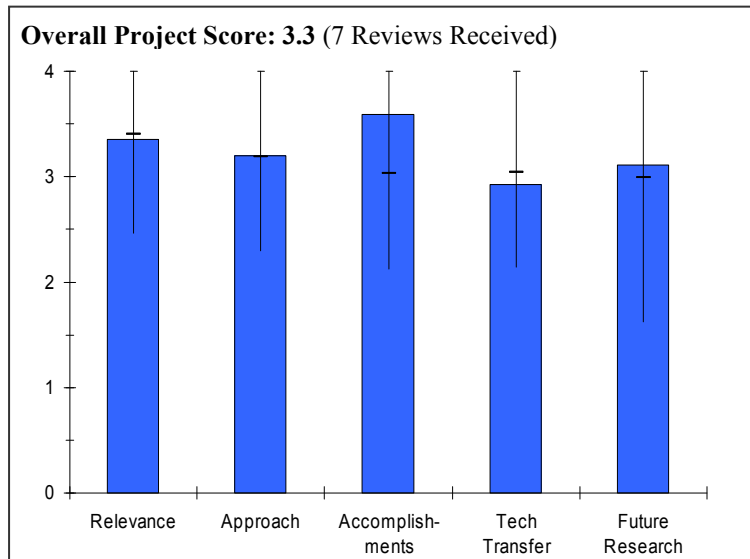
- I have scored the project highly for its technical merits, but it is my professional opinion that funding should be split between different parties so that no one organization holds the intellectual property rights.
- Cost share should have been higher.

Project # PD-16: Cost-Effective Method for Producing Self-Supporting Pd Alloy Membrane for Use in the Efficient Production of Coal-Derived Hydrogen

Kent Coulter; Southwest Research Institute

Brief Summary of Project

The objectives of this project are to: 1) develop a process methodology for the cost-effective manufacturing of thin, dense, self-supporting palladium (Pd) alloy membranes for hydrogen separation from the mixed gas streams of coal gasification processes; 2) reduce Pd membrane thickness by >50% over current state-of-art, and show potential to meet DOE 2010 technical targets; 3) demonstrate viability of using large-area vacuum processing to “engineer” a membrane microstructure that optimizes hydrogen permeability, separation efficiency, and lifetime; 4) demonstrate efficacy of large-batch and/or continuous roll-to-roll manufacturing of membrane material with performance and yields within pre-defined tolerance limits; and 5) demonstrate separation efficiency of thin palladium membrane in commercial-type fuel processor using mixed gas streams.



Question 1: Relevance to overall DOE objectives

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

- Able to down select to appropriate membrane.
- Concept of Pd-alloy separating membrane is definitely relevant.
- However, use and production of free-standing film may not be readily translatable as a practical, low-cost option.
- Good work for basic manufacturing explorations (hence, high relevance); but not too useful for large-scale, high-volume, low-cost, production.
- The project aligns well with the President's Hydrogen Fuel Initiative by developing a critical element needed for commercial hydrogen production, i.e., a self-supporting Pd-alloy membrane for hydrogen separation.
- Strives to address the following areas: Defects (high yield, large area), Selectivity (>99.9%), Flux (>100 scfh/ft²), Cost goal (<\$1500/ft²), all of which are barriers identified by DOE.
- Supports the Hydrogen Fuel Initiative.
- Targets barriers related to defects, selectivity, flux and cost.
- Separation of H₂ from mixed gas streams is necessary for several pathways.
- Highly relevant to DOE objectives.
- Very appropriate to DOE goal.

Question 2: Approach to performing the research and development

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

- Looks like a good approach.
- Good solid work to understand basic process manufacturing; but the weaknesses are: free standing thin films must be integrated into functional modules which requires a multi-step process!
- Not clear if project addressed cost of module manufacturing.
- The technical approach follows a staged 3-year plan, 2 years of which have been successfully completed.

PRODUCTION AND DELIVERY

- A no-cost extension has been granted to complete the remaining tasks.
- There are no concerns with the proposed approach to reach the stated objectives.
- Hydrogen gas used for reported flux results.
- Should use synthetic mixed gas or actual coal driven synthesis gas.
- Clear goals.
- Cost realistic and cognizant.
- Excellent steady year by year progression in size and quality of membranes.
- Magnetron sputtering is a very appropriate strategy to maintain Pd/Cu alloy composition during deposition.

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

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

- SRI understands membrane "manufacture".
- Achieved flux goals.
- Collaborated with Idatech back to DOE on scaling techniques.
- Unique way to estimate cost of Pd.
- Given that it's basic work on films, work shows solid progress in reduction of thickness and good permeation results; but sensitivity numbers cannot be defended if there are no real mixed gas experiments.
- Somewhat disappointed that real module data is not available, which means good current results could be lowered when the films are subsequently incorporated into modules.
- Achieved the first scale-up step under full WGS temperature and pressure conditions.
- Improved the knowledge base of the impact of membrane materials and preparation techniques on degradation and embrittlement.
- Developed necessary process engineering and economic tools for system optimization.
- Completed most of the planned accomplishments for Year 3.
- Membrane composition for Pd concentration identified.
- Down selection to batch process is a logical determination.
- Increased size of membranes produced.
- Exceeds DOE's target performance.
- Achieved 4-micrometers thick Pd film, pea-hole free, on oxidized Si wafer. Range of ~3 to 12 micrometers.
- Met H₂ 99.95% quality goal.
- Impressive Pd based film that is pea-hole free.

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

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

- Good work based on Colorado School of Mines and SWRI activities.
- Good acknowledgement of partners.
- Clearly, good cooperation between SWRI and Colorado School of Mines.
- Collaboration with IdaTech is not truly evident at this stage due to delays in module testing.
- Paper highlights SWRI's strengths and capabilities and Colorado School of Mines solid manufacturing approaches.
- No clear definition of issues with IdaTech Is it more a serial transfer of materials rather than active partnership?
- The project has only one for-profit partner, who is likely to be the sole entity to spearhead the commercialization effort. Delays with partner caused the project to require a no-cost extension.
- There is no distinct assurance that the project will move to commercialization once DOE funding ends.
- Sealing of foil on full scale design reviewed by DOE. Now it is time for implementation.
- Palladium cost \$35 out of \$45 using industry established model indicates program is going in the right direction.
- Collaboration between industry and university for ternary work.
- A good balance of team members.

Question 5: Approach to and relevance of proposed future research

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

- Milestones clearly developed and communicated.
- Unlike other papers, speaker did not give rationale for choice of ternary alloy components. No references are made to other work on ternary alloys, or why or how they will approach the ternary work.
- Appears to want to continue same vacuum deposition approach without defending why this is best for ternary systems.
- The proposed future research effort involves two additional partners – Carnegie Mellon University and TDA Research. The plan is to utilize an iterative modeling, rapid fabrication, and testing approach to develop and demonstrate <5-micron thick ternary Pd-alloy membranes.
- Reducing cost as mentioned for thin films to \$10 range would be a major cost reduction.
- Plan to go to ternary alloys 74 Pd-24 Cu4M.
- Approach and relevance is appropriate.

Strengths and weaknesses

Strengths

- Good presentation, hit all the appropriate points
- Produced thinnest membranes that exceed DOE targets.
- Good presenter – answered questions thoroughly.
- Solid pedigree based on Dong Wong's work.
- Reasonable approach for binary systems and vacuum deposition is time-honored and proven for other high-volume film processes.
- Uses a novel, scalable vacuum deposition method to fabricate free-standing Pd-alloy hydrogen separation membranes. The vacuum deposition method allows for tight control over the layer composition and thickness.
- Produced <3 micron, and 110 in² membranes and verified their performance, which was acceptable.
- Competent team of a commercial partner (IdaTech), Colorado School of Mines, and a new partner of CMU.
- Final tests under more aggressive conditions to develop new ternary alloy formulations with increased durability.
- Preliminary cost calculations show the cost of membrane is \$10/ft² more than the current price of Pd.
- Gave credit to foundation work at Colorado School of Mines.
- Has aggressive commercial partners.
- Development of (3-5 micron) self-supporting PdCu films.

Weaknesses

- Pd-Cu alloy and stoichiometric choices not unique or innovative.
- No extension to practical modules (work delayed).
- No mixed gas results to verify separation efficiency.
- No clear analysis of solubility effects versus diffusional parameters.
- All tests completed so far were performed using pure hydrogen. No synthesis gas mixtures (simulated or real) were used in the evaluation. It is unclear if the membrane's efficacy will be retained when gas mixtures are used.
- Full-scale prototype tests not completed with project 95% complete.
- Sputter deposition techniques are sophisticated.
- Magnetron deposition times.

Specific recommendations and additions or deletions to the work scope

- Need better defense of future work on ternary alloys – why certain choices? And would similar process work on ternary systems? And would that be meta-stable status?
- Definitely need mixed gas data and reduction to practical modules to get more realistic cost/ft² estimates.
- None. Project is near completed and is under a no-cost extension.

PRODUCTION AND DELIVERY

- Complete full-scale prototype testing.
- Should cross reference H₂ sensor work to understand the performance/lifecycle issues of annealing per the characterizations of catalytic coatings.

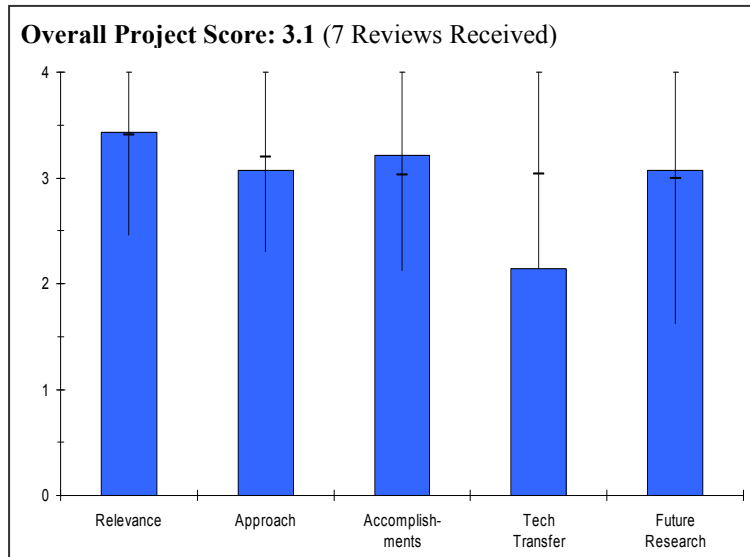
Project # PD-17: Advanced Water Gas Shift Membrane Reactor

Suzanne Opalka; United Technologies

Brief Summary of Project

The objectives of this project are to: 1) identify through atomistic and thermodynamic modeling a suitable Pd-Cu tri-metallic alloy membrane with high stability and commercially relevant hydrogen permeation in the presence of carbon monoxide and trace amounts of sulfur; and 2) identify and synthesize a water-gas-shift (WGS) catalyst with a high operating life that is sulfur and chlorine tolerant at low concentrations (0.004 atm partial pressure at 42 atm total pressure) of these impurities.

Question 1: Relevance to overall DOE objectives



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

- Work is highly relevant and innovative to combine WGS catalyst with H₂ separations system.
- Atomistic and thermodynamic modeling efforts are very thorough and well-explained.
- Objectives well-specified.
- The project aligns well with the President's Hydrogen Fuel Initiative by its effort to address two critical elements needed for commercial hydrogen separation, i.e., the development of a robust Pd-Cu tri-metallic alloy membrane with high stability and hydrogen permeation rate and to identify and synthesize a water-gas-shift (WGS) catalyst with a high operating life that is sulfur and chlorine tolerant.
- Lower cost conversion of syngas to H₂ and CO₂ key to producing H₂ from coke oven gas.
- Handling of sulfur is an important issue.
- Mathematical modeling appears to be outstanding.
- Clear goals, objectives, and analysis provide great direction.
- Results show excellent progress.
- A well thought out project that addresses DOE goals.

Question 2: Approach to performing the research and development

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

- Crosses over with fuel cells.
- From design to modeling to analysis – the approach is well done and systematic.
- Solid presentation of modeling results and correlation with measurements.
- Good understanding of both solubility and diffusional effects on permeation through designed structures.
- The technical approach involves computational modeling of membranes coupled with experimental verification of predicted results. This approach has significantly shortened the time to develop, test, and optimize the preferred catalysts for the said purposes.
- Several new alloy combinations have been identified via modeling. Their hydrogen solubility, permeability, and diffusivity appear promising, but have yet to be verified by experiment.
- Sound approach using modeling thermodynamics followed by testing. Avoiding mixed phase regions identified.
- The approach was very clear but not enough mention of cost relative to performance.
- At this stage, not enough emphasis on exponentially integrating WGS catalyst with PdCu membrane.

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

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

- Appears appropriate work.
- Able to downselect ternary alloy.
- Impressive progress integrating computations and correlations with basic experiments.
- Good cross-correlations with data from other sources.
- Solid understanding of meta-stability of systems.
- The technical accomplishments and project progress are up to date and on track.
- Two Pd-Cu-transition metal alloys and WGS catalysts have been selected and are ready for durability and optimization studies.
- Good agreement obtained between experimental results and modeling predictions.
- Several new alloy combinations have been identified via modeling. Their hydrogen solubility, permeability, and diffusivity appear promising, but have yet to be verified by experiment.
- Theoretical work on solubility and phase separation complete and PdCu membranes - PdCu alloys selected.
- Work on catalysts complete.
- Much higher H₂ diffusivity achieved by focusing on Pd-Cu B2 body centered cubic alloys.
- Phase transitions for two transition metals - denoted as J6 and G5 – tested and addition of transition metal G5 broadens B2 phase field while maintaining H₂ solubility.
- Very good progress and accomplishments.

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

This project was rated **2.1** for technology transfer and collaboration.

- Did not really discuss this area in detail.
- Collaborations were not well explained during the presentation.
- Unclear what the responsibilities were of each partner.
- Once the 1500-hr test is completed and the final report is forwarded to DOE, it is unclear whether the concept will be commercialized.
- Most of the work is focused on modeling, so whether it will be attractive to investors at the termination of the project is uncertain at this point.
- Good capable team..
- Did not appear to be any special collaboration outside UTRC.

Question 5: Approach to and relevance of proposed future research

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

- Program almost completed.
- Appropriate with respect to durability testing, but not very clear what steps follow after durability testing.
- A 1,500 hr test remains to be completed. No time period was provided as to when this would be initiated.
- Future testing worthwhile in particular stability testing of alloys and shawny throughput of integrated system.
- WGS catalyst family has been identified and will be tested.
- Direction for future work was very clear and compelling.

Strengths and weaknesses

Strengths

- Able to downselect catalysts.
- Systematic, well-presented and explained project
- Clear articulation of why downselected materials were selected.
- Good fundamental grasp of the processes and architecture.

- A good blend of modeling and laboratory verification. This will prove useful to minimize the number of experimental tests that have to be run.
- The development of the WGS reaction catalyst and the hydrogen separation membrane provide some synergy toward the production of hydrogen from coal. By removing the hydrogen in situ, the equilibrium limitations of the WGS reaction are overcome.
- Sound technical approach.
- Detailed mathematical modeling.
- The perspective of a large company.

Weaknesses

- Spent more time on first slide and did not cover all main points on slides which possessed good information.
- Presenter did not thoroughly cover material on slides.
- Need to explain and account for CO₂ interactions via experimentation as well as modeling.
- Need durability test data.
- There is some concern that the hydrogen separation catalyst and the WGS catalyst developed by this process may only be tolerant to sulfur and chlorine concentrations that are too low for actual operations. The targets are set so low that precleaning of the syngas may be warranted.
- Although the combination of the WGS reaction and the hydrogen separation step can be viewed as process intensification, the project does not address some of the other issues associated with this combination, such as catalyst poisoning by the excess CO₂ remaining in the reactor.
- The WGS catalyst is to be developed by both modeling and experimentation, but the tri-metallic alloy for hydrogen separation is limited to only modeling. It is unclear how useful the end product will be.
- Cost unproven compared to state of the art over a full cycle.
- Not much indication of relevance to the other people's work and attention to cost goals.
- Have not yet zeroed in on key integration experiments.

Specific recommendations and additions or deletions to the work scope

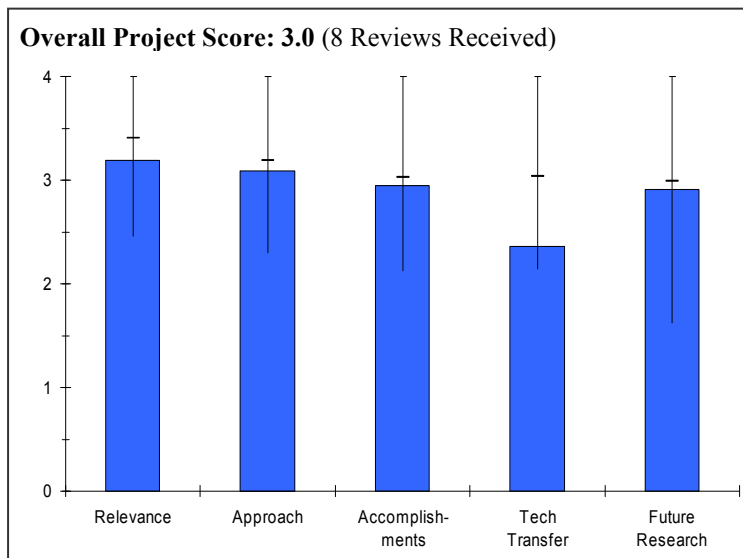
- Need more clearly defined scope of future activities beyond durability testing.
- Need scale-up to live-testing where CO₂ effects could be determined.
- Include both modeling and experimental verification for both the WGS catalyst and the tri-metallic alloy for hydrogen separation.
- In addition to Pt-based catalysts, it is suggested to test cheaper Fe-based WGS catalysts.

Project # PD-18: The Integration of a Structural Water Gas Shift Catalyst with a Vanadium Alloy Hydrogen Transport Device

Thomas Barton; Western Res. Ins. & U of Wyoming Res. Corp.

Brief Summary of Project

The key to a commercially scaled device that integrates metallic hydrogen transport membranes and water gas shift catalyst will be a catalyst with high compressive strength and no friability and a practical low cost method to attach the membranes to structural alloys. The objectives of this project are to: 1) develop a structural water gas shift catalyst capable of withstanding compressive forces; 2) develop vanadium alloy hydrogen separation membranes for fabrication of devices by brazing; and 3) integrate the WGS catalyst and metallic membranes into a device and test under gasifier conditions.



Question 1: Relevance to overall DOE objectives

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

- Relevant to DOE program.
- The project aligns well with the President's Hydrogen Fuel Initiative by its effort to address two critical elements needed for commercial hydrogen separation, i.e., the development of a low-cost vanadium alloy hydrogen separation membrane and a structural WGS catalyst, which unlike typical WGS catalysts are not friable powders, but can withstand high compressive forces.
- The use of vanadium, which is a lower cost metal compared to palladium, might help these membranes to meet the DOE's cost goals.
- Reduced WGS cost supports the H₂ Fuel Initiative.
- High compressive strength requirement for WGS catalyst with no friability dependent on reactor design.
- Integrated WGS and H₂ separation on a structural support many have a variety of applications and may be scaled down to small systems.
- It appeared to have a very limited scope and difficult to see how it was connected to other projects.
- Relevant for the production of hydrogen through improved catalysts using the water-gas shift reaction.
- The project assumes vanadium-based hydrogen transport membranes are the solution. This may be premature.

Question 2: Approach to performing the research and development

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

- Good exchange of information to share results with general public.
- The technical approach involves four tasks starting with the preparation of a monolithic ceramic or impregnable substrate and a brazable vanadium membrane that will be integrated and tested under coal gasification conditions.
- First three tasks are near complete. The final testing with a gasifier is yet to be done.
- There do not appear to be any inherent flaws with the approach.
- Structural WGS catalyst to integrate with membrane technology.
- Focus on fabrication for making a device.
- Brazing of membrane to tube for manufacture.

- Cost analysis of monolith versus impregnated substrate.
- Highlights practical issue of developing a device.
- Can what is learned be applied to other work?
- Use of a structural WGS catalyst and a vanadium-alloy H₂ membrane is a practical, economical approach.
- Not much connection to barriers.
- Very limited data.
- Goals are not clearly outlined or defined.
- May lead to more durable and longer-lived catalysts.

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

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

- Prepared a new water-gas shift catalyst and incorporated it into a structural water-gas shift ceramic material.
- Demonstrated vanadium alloys with excellent brazing characteristics for fabrication into membrane devices.
- Fabricated equipment for testing the integration of membrane and catalysts combinations under bottled gas and coal gasification environments.
- Cerium additions of 2% look promising.
- 15% alumina optimum.
- Analysis of vanadium alloy properties to optimize H₂ transport and brazing completed.
- A system has been assembled that can clean a slip stream of particulates, condensate, sulfur, and mercury.
- Identified materials and fabrication process to construct test reactor.
- Optimized the composition of the Fe-Al-Cr-Cu-Ce WGS catalyst.
- Identified alloying additives that improve H₂ transfer and brazing properties of vanadium.
- Some progress but not much data or clear description of progress.
- Data indicates that good progress is being made.
- Question whether 80 hours is a long enough test under gasifier conditions to show viability of concept.

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

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

- Good collaboration with university.
- Once the tests with the working gasifier at WRI are completed, the project is silent on regarding any discussion on continuing efforts and eventual commercialization.
- Industry / University collaboration.
- Does not feature as a strength but they do have a small coal gasifier for testing.
- Not much indication there was any collaboration.
- Little explicit mention of outside collaborators.
- Needs to push collaboration with one of the other contributors

Question 5: Approach to and relevance of proposed future research

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

- Project almost complete; some tasks fully completed.
- The 80 hour testing with WRI's gasifier on Powder Basin coal has already been initiated in April 2007.
- Investigate use of Ce.
- Next steps will depend on test results.
- Tests with gasifier using PRB coal are just beginning. They should be completed.
- Did not clearly outline future goals and expected directions.
- Only the briefest mention of further research and that is mainly of commercialization paths.
- Approach is appropriate.

Strengths and weaknesses

Strengths

- PI was good presenter.
- The project if successful, will lower capital costs by using a lower cost metal and help meet 2010 or 2015 program goals.
- Identified elements that are potentially positive to both hydrogen transport performance and brazing performance. While other elements with favorable transport characteristics may exist, their brazing performance is less desirable.
- Practical issues of materials fabrication addressed.
- Small device constructed that can be tested and replicated.
- The use of low-cost membranes and catalysts is refreshing.
- Appears to yield a better catalyst.
- Showed that brazing is a good way to attach catalyst, but unclear if this is new information.
- Good insights into vanadium alloy brazing being developed.

Weaknesses

- Should include scale on photos on slides.
- Some alloying elements beneficial to hydrogen transport will interfere with brazing of the membranes to structural alloys. This will require some trade-off analyses and optimization studies.
- Highest activity and stability has been shown for a catalyst with 75Fe-15Al-8Cr-2Cu alloy blend with small amounts of CeO₂, but producing high surface area monoliths of this catalyst series may be problematic due to sintering at higher operational temperatures.
- Project investigators could expand partners to address catalyst issues.
- No test results presented of integrated device yet approach suitable for small systems. How would it scale up to coal gasifier which is expected to be very large?
- Believe that someone else will optimize an alloy compatible with this technique.

Specific recommendations and additions or deletions to the work scope

- The project is 80% complete with a planned end date of Dec. 2007. The proof of the project is based on the outcome of the tests conducted with a working gasifier at WRI, but brazing problems and hydrogen embrittlement could become potential issues.
- Include remaining needs for research in the project's final report.

Project # PD-19: High Flux Metallic Membranes for Hydrogen Recovery and Membrane Reactors

Robert Buxbaum; REB Research & Consulting

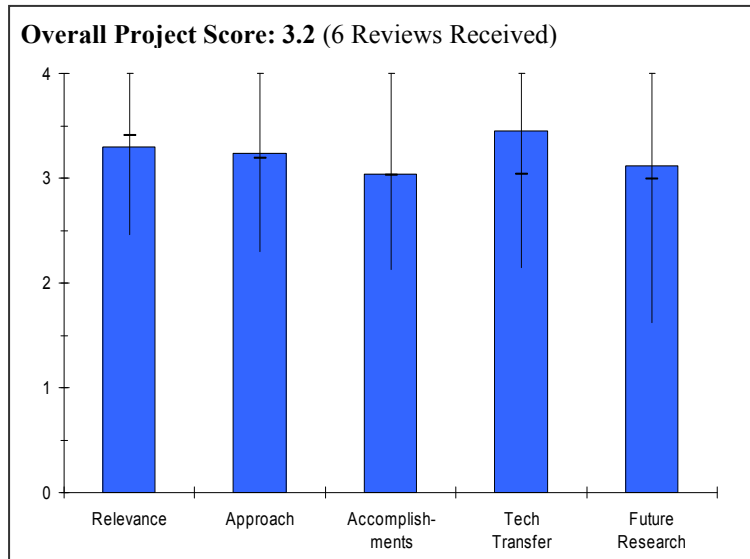
Brief Summary of Project

The project objective is to find a base metal replacement for palladium (\$470/oz) and for the principal investigator’s own sandwich membranes for use in hydrogen purifiers and membrane reactors with the following properties:

- Stable at 350-400 C;
- 100% selectivity like Pd;
- \$100/ft² vs. \$3000/ft²;
- 50 scfh/ft² UHP H₂ at ΔP=200psi;
- 15+ life, no embrittlement.

Question 1: Relevance to overall DOE objectives

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



- Relevant to DOE work to develop cost-effective membrane.
- The project is aligned with the President's Hydrogen Fuel Initiative by its effort to help meet cost targets for membrane materials used for hydrogen separation.
- There is some concern that the project is more focused on producing hydrogen from methanol rather than hydrogen from coal.
- Lower cost membranes with increased flux supports Hydrogen Fuel Initiative.
- 15-year life, while not a direct HFI goal is highly commendable.
- Testing with methanol reformer does not seem to be well aligned with current reforming activities.
- Innovative and creative approach.

Question 2: Approach to performing the research and development

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

- Seems appropriate for work at level of involvement.
- The project effort is to find a base metal to replace palladium (currently at \$470/oz) that will be stable at 350-400 C, demonstrate a 100% selectivity like Pd, cost range of \$100/ft² vs. \$3000/ft², 50 scfh/ft² UHP H₂ at ΔP=200psi, and demonstrate a 15+ year life without embrittlement.
- There do not appear to be any inherent flaws with the overall approach, but whether the end goal is achievable.
- Base metal membrane with Pd on surface to dissociate H₂ on surface.
- B2-intermetallic material allows for thinner or no coating of Pd which would lead to lower costs.
- Tubular membrane approach with 15 year lifetime/durability is impressive.
- The focus is on B2-ordered alloys in which vanadium is one of the elements.
- The B2 alloys still face embrittlement issues under H₂ environment.

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

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

- Great slide on year-by-year accomplishments shows progress made.
- Identified failures as well as successes.

PRODUCTION AND DELIVERY

- Identified and evaluated about 60 alloys, some having over 100 times the permeability of Pd and costing only 1/100 times as much, but they embrittle in an H₂ environment.
- Some intermetallic alloys, like NiTi show some promise, but require further development.
- Exceeded the performance goals set for membranes in 2005 and 2006, i.e., selected alloys and achieved test flux of 51scfh/ft² at 44psi.
- Interesting point that 12% storage of hydrogen is possible in methanol/water liquid carrier.
- Identified elements with higher H₂ permeability than Pd.
 - Aim for B2 alloys to be ductile and stable with less cost than Pd–Cu alloys.
- Identified two alloys with comparable permeability to Pd.
- Identified the need for long life of material for commercial applications.
- Brazing-scatter approach to testing is crude, but effective.
- The use of B2 alloys is an interesting and different approach. No real hydrogen transfer data at this point.
- Good progress to date.

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

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

- Good selection of partners, good leveraging of funds.
- The presentation does not describe any plans for continuing efforts and eventual commercialization.
- The PI did not mention if his company will commercialize the outcome of this project.
- Excellent collaboration between industries, university and government laboratories.
- Very strong collaborations with partners.
- Good team put together.

Question 5: Approach to and relevance of proposed future research

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

- Project is 50% complete.
- Good thorough approach.
- Plans are to improve alloys for high flux with no embrittlement, fabricate and test membrane and membrane reactor, and confirm that behavior matches flux, cost, and durability goals.
- Identified alloys not useful for hydrogen separation that could have other potential market applications.
- Reduced embrittlement of alloys.
- Embrittlement testing of brazes.
- Some work should be focused on how the membranes/reactors can be scaled up for larger applications.
- Test membranes/reactors on fuels other than methanol.
- Good approach.

Strengths and weaknesses

Strengths

- Good follow on activity from SBIR phase 1 funds (leveraging).
- Good presenter who offered thorough explanations.
- Great “show and tell” and good use of visuals.
- The project if successful, will lower capital costs by using a lower cost metal and help meet 2010 or 2015 program goals.
- There is a possibility that some of the developed alloy combinations that failed to meet the requirements for hydrogen membranes might find applications elsewhere.
- Targets commercial application.
- Use of methanol feed eliminates sulfur contamination issues.
- Good collaboration between project partners.
- Interesting idea.

- The B2 alloys show great promise in the areas of cost reduction at comparable flux and selectivity to those of Pd membrane.
- Good collaborations with universities and national labs.
- Highly creative project team which will lead to new novel membranes of interest for DOE.

Weaknesses

- There is some concern that this project overlaps other efforts to identify suitable alloy combinations.
- There are some major issues associated with the oxidation and the embrittlement of the intermetallic material (B2) that have not been addressed.
- Reported membrane life is based on theoretical estimation and not actual experimentation.
- Hydrogen embrittlement is not only present in the membrane, but also the seals and other components, which have yet to be addressed.
- Need to address large scale application of methanol feed for hydrogen infrastructure
- Very little hydrogen transfer data.
- The project team has not addressed the durability (embrittlement) issue.

Specific recommendations and additions or deletions to the work scope

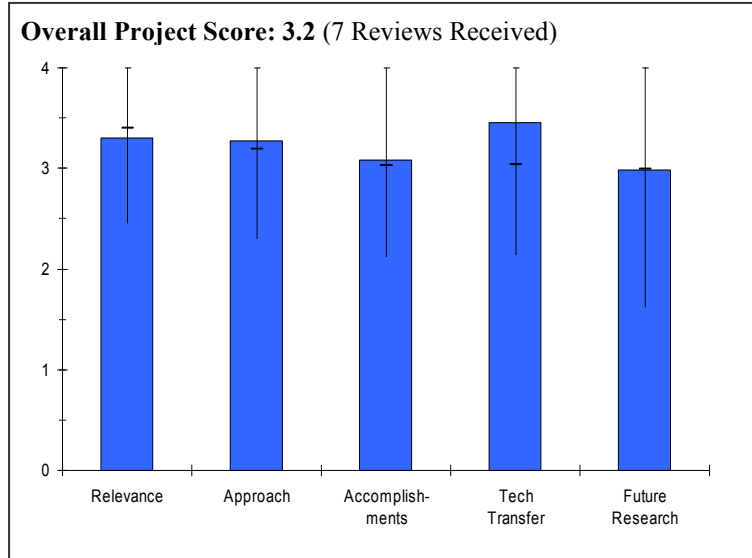
- The project team did not report thermal cycle results. Some thermal cycle studies are needed.
- Look into how the membrane materials can be applied for larger scale applications (500 - 3000 kg/day).
- Some focus on testing with H₂S is needed.

Project # PD-20: Sulfur-Iodine Thermochemical Cycle Laboratory-Scale Experiment

Paul Pickard; SNL/GA/CEA

Brief Summary of Project

The overall objective of this project is to determine the potential of the Sulfur-Iodine (S-I) cycle for Hydrogen production using nuclear energy and to: 1) explore the potential for high efficiency and technical maturity in sulfur cycles; 2) Evaluate and test process options, construct integrated lab scale experiment to demonstrate S-I cycle; 3) provide basis for cost projections and comparisons; and 4) support Nuclear Hydrogen technology selection decision (FY2011). The phase one objectives are to evaluate process options, establish baseline flowsheets, and conduct experiments on process options and materials. Phase two (Integrated Lab Scale Experiment – ILS) objectives are to: 1) develop and test the 3 major reaction sections for S-I; 2) assemble the 3 major reaction sections into an integrated, closed loop demonstration experiment; and 3) conduct S-I integrated lab scale experiments program.



Question 1: Relevance to overall DOE objectives

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

- It is recommended that long term durability studies on the materials be completed.
- The Sulfur-Iodine thermochemical cycle has been extensively evaluated and has promising hydrogen production efficiency. Demonstration of integrated loop process hardware compatible with next generation high temperature nuclear reactors will enable co-development.
- Efficient hydrogen production capability can be ready when the nuclear reactors are, and hopefully the rigid specification barriers in nuclear reactor system design will be chosen in a way not inadvertently exclusive of this hydrogen production pathway.
- Project supports Nuclear Hydrogen Initiative.
- Potential high efficiencies.
- The sulfur iodine process is well matched to the most likely design for the Next Generation Nuclear Plant project.
- Clearly makes H₂ which is DOE goal.
- Not clear technology could be available by 2017 to support DOE goals.
- Success in this area will support the development of a hydrogen economy.
- Is this the best opportunity for using nuclear energy for hydrogen production? Why was this cycle chosen? What about other cycles?
- This is the most advanced and most important project related to thermochemical water splitting using nuclear heat. One might argue that if this cycle isn't developed to high efficiency, then thermo chemical water splitting will be no longer considered for integration with the next generation nuclear reactor.

Question 2: Approach to performing the research and development

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

- Major issues have been identified.
- Would be good to see a critical path to achieve the 2011 goals.

- It seems that integrating 3300 bayonets is not a trivial problem. An advanced design for higher production rates may be required to minimize the quantity of bayonets.
- The trifurcation of process and hardware is a reasonable parsing of project capabilities, and promotes, hopefully, good collaboration among project partners.
- Scale of the process hardware is reasonably sized for proof of integration of the three main sections, while remaining small enough so that substantial changes in individual components could be affected at reasonable cost.
- Partners operate 3 independent reactor sections then combine for shake down in 07 closed loop 200 lph H₂.
- Good use of individual partners strengths at start of development.
- Project ties in with nuclear hydrogen initiative.
- Costs should be considered in evaluations of different approaches to addressing the major issues
- Three labs working together.
- Combining all lab work at GA seems to be a good approach.
- Need to assess risks with this process.
- Need to look at costs of complex metallurgy.
- Just because SI cycles are the most researched (as stated during presentation) doesn't mean that they are the most optimal.
- Good reactor design concept.
- PI should know how much hydrogen would be produced for the various scales being studied. Reveals a lack of understanding of the hydrogen markets that they're hoping to produce hydrogen for. Need a better systems understanding to appropriately design the production system they're working on.
- Extractive distillation is a good idea for this project. Shows that the project team is not sticking to only conventional systems.
- What about the concern about ceramics embrittlement?
- Excellent focus on overcoming technical barriers. Ties to INL to find alternative methods for concentrating HI solution are also good.

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

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

- Progress has been achieved, but seems that for the funding more progress should have been made.
- One of the objectives is to provide a basis for cost estimates. Nothing was reported on costs.
- The three principal process pieces have been designed, built, and apparently have met their specific targets for processing rates.
- The skid mounted sub-process assemblies are ready for integrated assembly. On target for integrated mode testing.
- H₂SO₄ decomposition corrosion issue addressed with SiC bayonet design allows high temp ceramic section with low temp process connections.
- No seals in reactor at temperature.
- Economic impact of additional step for phosphoric acid for HI should be reviewed.
- X and Y axes on the SO₂ production charts should be in the same units (ideally, moles SO₂/hr v. moles H₂SO₄/hr). Also, it is not clear whether "moles acid/hr" refers to moles of H₂SO₄ or moles/hr of solution at, for example 38 mole % H₂SO₄. Comparing L of SO₂ to moles of H₂SO₄ is not very user friendly.
- Experiments under way at 3 sites.
- Appears to be high-quality experimented work.
- Very good progress on planned research.
- Not sure that the costs presented in response to questions pass the laugh test.
- Excellent work on new reactor design, including easily-scaled reactor.
- Good progress has been made but the use of phosphoric acid in the HI concentration step adds a lot of complexity and risk to achieving acceptable efficiency. More emphasis should be placed in developing alternatives that have less complexity.

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

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

- There appears to be good coordination and well defined rolls for the team members.
- The project partners and scope were chosen in a way that should assure good excellent collaboration; we will see shortly if this worked!
- Integrated process testing at GA is a good way to assure that Next Generation HTGR's will be Hydrogen Production ready.
- Good mix of partners from national labs universities, industry and international.
- How will this project benefit from the lessons learned in the other high temperature integrated laboratory scale experiments?
- Collaboration / design of work among 3 sites.
- Not clear how much collaboration occurs outside of core group.
- Doing significant amount of process development without process collaborator to do significant cost analysis.
- Good project team.
- Excellent recognition that works elsewhere can help this project.
- The project shows excellent cooperation across institutions and has a definite pathway for commercialization.

Question 5: Approach to and relevance of proposed future research

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

- Team integration and testing work appear to be well planned.
- One of the stated goals is to provide basis for cost estimates. This is not clearly discussed.
- The investigators should define some Go/No go decisions for off ramps.
- PI reported \$2.79 H₂ per kg at 5 cents per kW power during Q&A but should include details of cost estimate in next years review.
- Experiments should evaluate the costs versus benefits of various approaches to resolving the primary issues. For example, to evaluate maximizing heat transfer to the catalyst versus longer residence time and improved catalyst surface area/support.
- Next step is to operate integrated skids.
- Good total-project plan.
- Should have a working prototype by the end of the process.
- Need a greater focus on analysis; how is research being defined?
- Future plans make sense and are well focused on problem solving and demonstration of the technology.

Strengths and weaknesses

Strengths

- There is a strong team that has clearly defined roles and appears to be working well together.
- Project partners' basic capabilities coupled with their shared leadership in developing next generation HTGR's.
- Selection of a solid thermochemical cycle with production efficiencies compatible with production quantities needed for the hydrogen economy.
- Process design, reaction kinetics, and material selections (for the HI and Bunsen sections at least) once proved can be utilized to make H₂ by other primary heat resources of sufficient temperature; principally solar thermal.
- SiC Bayonet reactor design eliminates seals.
- ILS appears to be designed for flexibility and incorporates the ability to run sections separately and exchange process equipment as needed.
- Well organized, good project team, good plan.
- Project strengths are the team and the significant investment in the technology that has been made over the last thirty years.

Weaknesses

- There are substantial materials problems, such as the catalyst degradation, that will need to be overcome.
- Integration and control of 3300 bayonet heat exchangers/reactors may be very difficult.
- The exotic materials most likely required may be very expensive.
- Reaction kinetics, proper sizing of inter-process transport and reactant carryover are interface issues of concern as the three skids are integrated.
- Assuming good closed loop(s) process results are demonstrated, doubts about scale-up could still hinder further development opportunities.
- H₂SO₄ decomposition catalyst deactivation potential for short reactor life.
- Many of the process steps do not seem to be well enough developed as independent unit operations to support integration.
- Need more economic analysis. This is a very complicated process utilizing expensive metallurgy and it must be shown to be better than other alternatives.
- Need better analysis.
- The most obvious weakness is the complexity of the phosphoric acid extraction step.

Specific recommendations and additions or deletions to the work scope

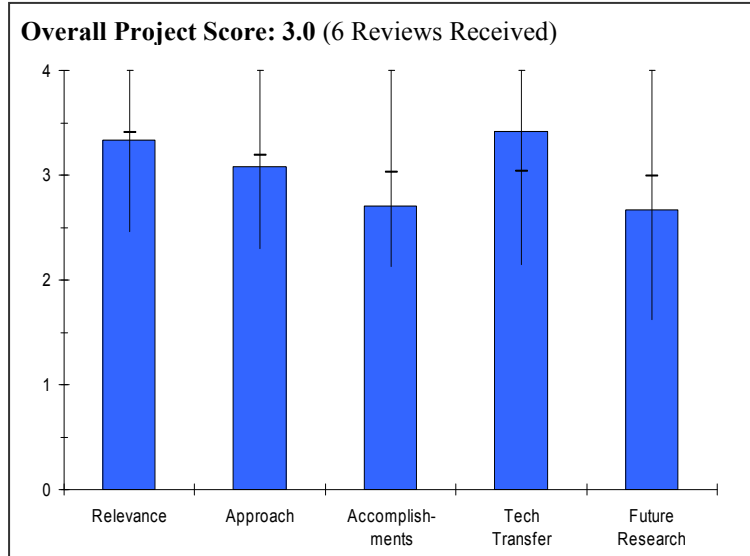
- It is recommended to include in a later phase the design of an advanced reactor design to enlarge the reactor so that not as many bayonets will be required for control and integration.
- It is recommended that durability studies on the system be included in the next phase. It is unclear that these studies were planned.
- Continue to share information on achievements and challenges with the Solar Thermal Thermochemical project participants.
- Complete corrosion testing of mixed acids in HI section.
- Project is well focused and needs no additions or deletions.

Project # PD-21: Hybrid Sulfur Thermochemical Process Development

Bill Summers; SRS

Brief Summary of Project

The overall objective of this project is to develop the Hybrid Sulfur thermochemical cycle and demonstrate in an integrated laboratory scale experiment producing >100 lph of hydrogen. The objectives for fiscal year 2006 were to develop and test an SO₂ depolarized electrolyzer (SDE) using polymer electrolyte membrane (PEM)-type cell design, including to: 1) characterize, analyze and select cell components; and 2) test single cell SDE electrolyzers at elevated temperature and pressure. The objectives for fiscal year 2007 are to: 1) develop improved electrolyzers; 2) demonstrate extended operation capability; 3) scale-up to larger size; 4) continue to identify and develop improved cell components; 5) conduct 100 hour longevity test on single cell SDE; and 6) design and build multi-cell SDE with 100 lph hydrogen capacity.



Question 1: Relevance to overall DOE objectives

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

- Focused on H₂ which is DOE goals.
- Technology is high capital / long term solution. Not clear can assist to meet 2015 DOE goals.
- Focus can only be on a large regional plant.
- This project would be more relevant to the future H₂ economy if it would focus on success of hybrid sulfur process. Inclusion of electrolyzer development and testing seems to be a diversion.
- Presentation should supply information on why the hybrid sulfur cycle was chosen over other options.
- DOE has appropriately chosen a project on the hybrid sulfur cycle.
- On the basis of the KISS (Keep It Simple, Stupid) philosophy, this project is critical to NHI for H₂ production that is cost-effective, provided that SRNL can solve the SO₂ cross-over.
- The combination of thermo chemical and electrochemical processes for producing hydrogen detracts somewhat from the overall goal of producing both hydrogen and electricity using high temperature nuclear heat. Electricity at 7 cents per kW is more valuable than hydrogen at \$3/kg.
- The project goals are critical to the Hydrogen Initiative and fully support DOE RD&D objectives for thermochemical process development for hydrogen production.

Question 2: Approach to performing the research and development

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

- Focus on electrolyzers is appropriate for this project.
- Approach is reasonable.
- For cost this appears to be a simpler / higher probability of success than other nuclear programs.
- Input of electricity is a drawback.
- Oxygen byproduct should not be shown as a bonus. Unless the nuclear power plant is next door to a gasifier, there's probably not an economical market for it.
- Need to address corrosion.
- Emphasis is on developing a PEM-type cell design and improving membrane performance, which are key.

- Determination of critical thermo physical properties is critical to obtain a good model.
- There are significant challenges with respect to the choice of anode materials, the development of new membranes that do not pass sulfur dioxide and the operation of the electrochemical cell at high sulfuric acid concentrations. Although all four of these points were raised and discussed by the investigator, no clear alternatives were offered for overcoming these problems. For example, the investigators have limited themselves to investigating only those membrane electrolytes that are commercially available. Fuel cell developers, on the other hand, are putting significant effort into developing better membranes for their applications which are technically less demanding than those of the proposed electrolysis cell.
- The project subtasks are well-focused on specific technical barriers.
- The project subtasks are well-balanced with respect to different technical barriers.
- The approach builds upon well-established technology, ensuring compact design and attractive unit costs.
- The contributions and responsibilities of collaborators and partners were clearly described.

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

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

- Cells need to be tested at higher acid concentrations and pressure and for longer durations.
- Good thought process of why technology might work and why this would be economically attractive.
- 100 hour test is not adequate progress given that this project started in 2004.
- Using specialized materials of construction do not help this project address corrosion concerns.
- The patent for this process was issued in 1975; need to show what progress in thought, if not R&D, has been made since then.
- What about cost analysis? Costs given verbally during Q&A are completely unbelievable.
- Good progress, but it appears that a breakthrough is necessary for the membrane work. Very difficult problem.
- A more quantitative discussion of the SO₂ carryover would have been useful for an assessment of the difficulty of this challenge.
- This project has succeeded in identifying several show stoppers. Among them are high cell voltage and sulfur dioxide crossover from anode to cathode. The investigators are only beginning to understand the magnitude of the problems with this system and have not yet charted a pathway to overcoming these barriers.
- The selection or derivation of specific milestones and performance indicators was well-described, with excellent progress towards specific performance parameters.
- The progress has been especially strong for the single cell testing, which reached a key milestone ahead of schedule.
- The progress is very well-documented with respect to specific components.

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

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

- Leveraging of existing PEM research is beneficial.
- The project partners appear to be working closely with each other to develop and improve the electrolyzers
- Good set of collaborators.
- Why Giner? Suggest broadening to other electrolyzer manufacturers.
- How does this project connect with SNL/GA/CEA project?
- How is Westinghouse involved in the project?
- The mix of participants is sufficient to bring this technology from the laboratory to commercialization.
- There is strong partnership and integration with other institutions and industrial enterprises.
- The characterization of experimental membranes is derived from commercially-available sources, ensuring a focus on the partners' respective expertise.

Question 5: Approach to and relevance of proposed future research

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

PRODUCTION AND DELIVERY

- Costs and potential value of byproducts should be evaluated (excess heat and oxygen).
- It is not clear that a complete integrated laboratory scale experiment, including the acid thermal decomposition step is necessary given that this step is being extensively studied for the sulfur iodine process.
- More extensive research seems to be needed on the long term performance of the cells under operating conditions and for catalysts.
- A specification for sulfur deposition to work toward should be developed.
- Does not appear to include economic analysis.
- Stating that improved membrane will be researched, "...with industry partners" is inadequate. Is there a company lined up or is the presenting team hoping to find someone to work with?
- Longer duration tests should be planned; 100 hours is not adequate.
- Not enough detail presented on FY08 plans. What about plans beyond that? Project is supposed to end in 2010; how do you know that without a plan?
- All of the future work described is necessary. The emphasis on improved cell membrane is appropriate.
- Future research should include membrane development and work with non noble metal catalysts.
- The future research continues to build towards meeting established milestones, and overall schedule performance is strong.

Strengths and weaknesses

Strengths

- Close collaboration of partners and use of prior PEM research
- Interesting cycle being studied.
- Good membrane work, although there is a question about whether electrolyzer development should be included in this project. It may dilute the effort.
- The project's primary strength is the past work that defined many of the operating conditions and a preliminary flowsheet to build on.
- Another strength is the simplicity of the cycle and involves only S, H₂, and O₂.
- Project strengths are the experience of the teams and the long history of work on this technology.
- The investigators demonstrate clear experimental design of each subtask, and have made significant progress in the project.

Weaknesses

- The project PI did not seem to be aware of newer research in electrolyzer catalysts.
- Needs more economic analysis – is nuclear Rx free – is energy (electricity and heat) free?
- Needs to look at materials needed and costs.
- Electrolyzer research dilutes effort on hybrid sulfur cycle.
- Insufficient depth in project team.
- No total project plan.
- No mention of past work.
- Westinghouse reported an integrated lab demo in 1976, where, I think, 100 L of H₂ / h was produced. Has the old technology been fully examined? SO₂ carryover and S deposition were problems in the 1970s and there was mention that progress was made in mitigating the SO₂ carryover in the 1970s.
- There seems to be a desire to focus on building demonstration cells rather than focusing on understanding to chemistry and electrochemistry. Perhaps this is the result of the program focus on demonstrating liters per hour rather than efficiency and durability.
- Contingencies are not described, and it is not clear how the investigators will actually use "innovative design approaches" to improve upon commercially-available membranes.

Specific recommendations and additions or deletions to the work scope

- Delete full HyS process integrated laboratory scale test; instead develop a joint integrated test with the sulfur iodine process.
- Needs to determine what are research goals to meet DOE targets.
- Project seems to be focused on building and testing equipment – needs to focus on economic viability.

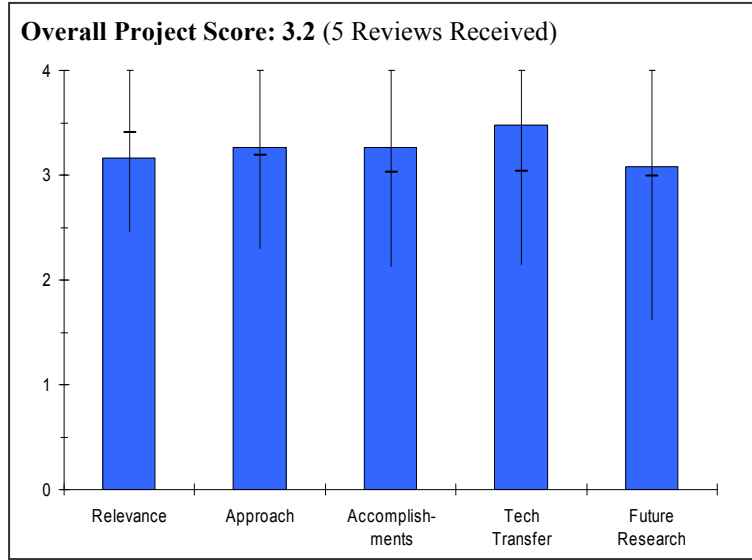
- Expand partners.
- Transfer electrolyzer research to another project.
- Add work on developing a fundamental understanding of the electrochemistry and the membrane. Also, producing high concentrations of sulfuric acid may be incompatible with a membrane cell that depends on high levels of hydration for good ionic conductivity. The specifications for the electrochemical cell are too severe. Concentrate the sulfuric acid elsewhere in the system.

Project # PD-22: Laboratory-Scale High-Temperature Electrolysis System

Steve Herring; INL/ANL/Ceramatec

Brief Summary of Project

The technical objectives of this project are to: 1) Develop and demonstrate energy-efficient, high temperature solid oxide electrolysis cells (SOECs) and stacks for hydrogen production from steam; 2) demonstrate technology at progressively larger scales; 3) perform flowsheet analyses of systems-level high-temperature electrolysis (HTE) processes to support planned scale-up to Integrated Laboratory-scale, Pilot-scale and Engineering Demonstration-scale experiments; 4) develop detailed computational fluid dynamics (CFD) models of operating SOECs; validate with experiment data; and 5) investigate alternate cell materials (e.g. alternate electrode and/or interconnect materials) alternate cell configurations (e.g. porous-metal substrates, tubular cells, porous electrodes) and applications of inorganic membranes.



Question 1: Relevance to overall DOE objectives

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

- High temperature electrolysis is definitely within the Hydrogen Initiative.
- The project supports the NHI and is in-line with the goals at the NHI.
- The development of a high temperature water electrolyzer that operates at high efficiency is certainly relevant to the DOE goals. However, the tie to high temperature nuclear reactors is not clear, since the only thing the nuke supplies is electricity. This could just as easily be tied to renewable energy sources.
- This project is consistent with the DOE program on the Nuclear Hydrogen Initiative.
- There is a concern, however, about co-locating a major hydrogen facility in close proximity to a major nuclear facility.
- There was also a comment during the Q&A in this session that such a hydrogen facility would only be for a captive large-scale user, such as a tar sands upgrading facility. This seems to be inconsistent with the EERE hydrogen production program in general, and the needed quality of the hydrogen product may be quite different for automotive use versus refinery use.

Question 2: Approach to performing the research and development

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

- Good plans for FY07 and 08, but it would be nice to see a high level plan to achieve the engineering demo in 2015.
- Leveraging off of DOE's SECA program is a good approach to improving the SOEC.
- Did the presenter consider other sources for SOFC's?
- The multi-center complex project is well-organized.
- The approach of progression from button – we test through laboratory and pilot scale to an engineering demonstration facility is well-designed.
- The project builds on research from the SECA and other programs.
- Testing to date seems to have identified the major issues and work has been done to address them.

- The approach being taken appears to concentrate on building demonstration cells before a complete understanding of the technology has been developed. The approach is to take the technology developed for solid oxide fuel cells and run it backwards. It is obvious from the presentation made by the investigator that this approach has problems.
- The approach uses a good combination of progressive experimental development (from a button cell to a 25-cell stack, to 60-cell stacks, to 4-stack modules in the integrated laboratory-scale experiment) and process and component performance modeling and analysis (flow sheet analyses, CFD analyses of cells, stacks, and modules).
- It was not clear if the larger systems, such as 200-kW pilot plant and the 1-MW engineering demonstration, would use the same size cells/stacks/submodules as in the current work or if these larger systems would require significant scale-up in cell size and/or number of cells per stack. Such scale up is not likely to be a simple extrapolation of current fabrication, assembly, and control processes and systems.

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

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

- The analysis work and design work are accomplished.
- How was progress from SECA work leveraged into the SOEC?
- The durability tests appeared to provide useful information.
- A chart that compares their progress with the DOE goals and other technologies would be useful.
- Delamination at the electrodes too been significantly reduced.
- Design of the integrated lab scale experiment has been completed and fabrication --> well underway.
- Decreasing electrolyzer cost is being addressed through development of longer format cells and evolution of other geometrics.
- Stack life-time is a concern.
- Lessons learned from the work done to address the delamination and cracking problems should be presented.
- Despite the problems of material degradation and delamination, significant progress has been made in developing and testing 22 and 25 cell stacks.
- A major accomplishment has been the 2000-h test of the 2 x 60-cell stacks, yielding from 0.6 to 1.2 normal cubic meters of hydrogen per hour (test run stopped due to shorting of the current leads to the manifold).
- Another major accomplishment has been the post-test evaluation of cells from the earlier 25-cell stack. Based on some of the answers during Q&A, the results of those analyses are being used to help improve the materials and fabrication processes for new cell and stack builds.
- The ILS development appears to be proceeding well with development of the piping and instrumentation diagram, design of steam generator/superheater, and fabrication and delivery of ILS modules (electrolyzer stacks).

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

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

- Good participation by national laboratories, more industry participation would be useful.
- There were an impressive number of publications.
- There appeared to be relatively open discussion of the fuel cell performance and how to improve it.
- This project is a well coordinated effort of multiple centers and an industrial partner.
- Better integration with the solid oxide fuel cell research program would benefit this project.
- The team working on this project is well qualified to bring this technology to market.
- The project team has members from other national laboratories and industry.
- Although "clear path to commercialization" was identified as an FY-07 THE issue/concern, there was no discussion of how that would be attempted, particularly since no nuclear industry participant has been identified.

Question 5: Approach to and relevance of proposed future research

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

PRODUCTION AND DELIVERY

- Plan with built in Go/No go decision points and off ramps is necessary.
- There has been sufficient development to be able to perform a cost analysis, which should be included.
- The project plan for future years is well laid-out.
- The project is evaluating alternate SOEC geometries at the bench scale while moving forward with testing of the 240-cell SOEC in the Integrated Laboratory System.
- Further work is needed to address performance degradation.
- The plan should include more fundamental work studying new materials and electrochemistry. Questions were raised at the presentation that indicated that even the cell geometry may be in question with the suggestion that the team is considering switching to tubular cell geometry. This needs to be sorted out quickly.
- The activities listed for future work are heavy on testing but light on the development of improved materials and processes. Perhaps the presentation did not touch on that, but the program plan includes it?
- Based on the discussion during Q&A, this project depends heavily on the developments and successes in the SECA (and other similar) program. Because of the reversed polarization and different water contents between SOEC and SOFC operating conditions, more emphasis on electrode and electrolyte materials for the SOEC application, particularly at lower temperatures, is likely to be more fruitful.

Strengths and weaknesses

Strengths

- The project has access to much of the technology being developed for SOFC fuel cells.
- The team has an impressive amount of publications.
- There was good progress made, especially performing a 2000+ hr test.
- The project is well-organized.
- The project builds on previous work.
- Good technical progress has been made.
- The project makes good use of previously developed information.
- This is an excellent team. They have at their disposal all the tools needed to succeed.
- The project includes a good mix of experimental, diagnostic, and analytical components (among the best such mix over the range of projects discussed in this session).
- The progression to successively larger test cells, stacks, and modules is commendable.

Weaknesses

- The long term goal was to show commercial viability, but there was not cost analysis.
- The stack lifetime was relatively short and there was no discussion on how to improve its life.
- The current status and development hurdle of the ORNL membrane was not adequately discussed.
- The impact of short stock life-time in system economics was not presented.
- Loss of performance has not been fully addressed.
- The project is attempting to adapt existing solid oxide fuel cell technology to operate outside the envelope for which it was designed.
- Considerable degradation has been observed in 25-cell and 60-cell stacks. It is not clear that the results of post-test analyses (completed at the end of November 2006) could be used effectively in the build of the ILS module (delivered in the second half of March 2007).
- Plots of the 25-cell and 60-cell stacks' performance over time would have been helpful. A technical discussion of the trend seen as well as a discussion of any diagnostics conducted during the long-term tests would have been very informative.

Specific recommendations and additions or deletions to the work scope

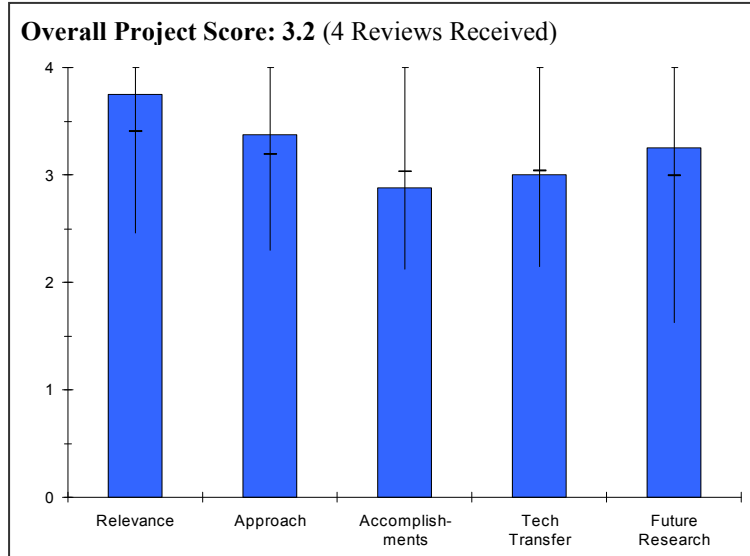
- Durability testing should be included
- Additional industrial partners would be useful; the project may want to consider other SOE vendors.
- A critical path, with risk mitigation and Go/No-Go milestones, to achieving the ambitious goals of 1MW demo in 2015 was not clear. This should be developed.
- It is recommended that a research university with significant experience in materials and solid oxide fuel cells be brought onto the team to investigate the fundamental materials science.

Project # PD-23: Nuclear Reactor/Hydrogen Process Interface

Steve Sherman; INL

Brief Summary of Project

The overall objectives of this project are to: 1) guide the development of technologies to enable the connection of a Very High Temperature [nuclear] Reactor (VHTR) to a high-temperature hydrogen plant; 2) resolve technical issues and challenges offered by the DOE Nuclear Hydrogen Initiative (NHI) and Next Generation Nuclear Plant (NGNP) Project in regard to nuclear connection design, construction, operation, safety, economics, and nuclear plant licensing; and 3) work closely with NHI Thermochemical and High-Temperature Electrolysis areas to define and test components and systems.



Question 1: Relevance to overall DOE objectives

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

- The purpose of the components database is not clear.
- It is not clear how this project will address technical issues having to do with economics as no costs or cost comparisons between alternatives have been presented.
- Required technology to provide for advanced high temperature reactor co-production of hydrogen and electricity.
- Research may also be applicable for solar thermal hydrogen production technologies.
- Linking the H₂ production plant with the reactor is critical.
- Emphasis on materials and heat exchange designs is well though out.
- This project is very relevant to the development of hybrid nuclear hydrogen systems. It addresses the bridge between the nuclear plant and the hydrogen generator. It is essential that this project identify and validate cost-effective materials compatible with both systems.

Question 2: Approach to performing the research and development

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

- This project should provide cost/benefit analyses for the various approaches to overcoming the barriers.
- Appears to take a comprehensive approach to the R&D effort.
- Good configuration management, ensuring that all changes made in materials, components, safety features, etc. are re-checked for their impacts on the other critical design parameters.
- Logical narrowing of R&D foci to the critical parameters.
- Approach is focused on technical barriers and, if implemented, will lend to an integrated nuclear plant and a hydrogen plant.
- The approach is well defined and focused on solving the problems and overcoming the barriers to interfacing the high temperature nuclear reactor with the thermo chemical water splitting system.

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

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

PRODUCTION AND DELIVERY

- The presentation was not specific enough regarding accomplishments.
- No cost or performance information was presented for the heat transfer loop as requested by previous reviewers.
- Appear to be narrowing in on many of the viable materials and design options.
- Integrated system modeling that interfaces with the reactor side modeling.
- Specific materials and components development plan.
- Costs are high for the level of accomplishments: GA work on materials for HIX – Section 3 work is listed as an accomplishment both here in PDP 31.
- INL accomplishments are not explicitly defined and matching funds spent with INL accomplishments was impossible for this reviews.
- Good progress is being made although the project could be moving faster.

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

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

- The presentation did not clearly address how this project will facilitate transfer of information to and between the various high temperature demonstration projects.
- Strong collaborations with academia, private industry, national labs and foreign entities with common interests.
- Collaborators are many – 16 partners are listed. Coordination of this many projects is difficult to achieve with one person as lead with this many collaborators.
- This project has a team well positioned to commercialize the technology.

Question 5: Approach to and relevance of proposed future research

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

- "Continued research" is proposed, but there are no clear criteria for success or prioritization of research needs.
- Well developed schedule for future work that was developed in collaboration with the NGNP project to ensure that the time when one project is waiting on the other for design input is minimized.
- Cannot tell if there are any optional paths built in, but the optional paths appear to be identified at least.
- Coordination with other laboratories appears weak. For example, INL proposes to initiate lab-scale testing of prototype heat exchanges, but this work is ongoing elsewhere. Future research for INL should be listed separately. Optional paths are being considered.
- The future plans are well focused and have a good probability of success.

Strengths and weaknesses

Strengths

- Well organized and coordinated with nuclear plant project, taking full advantage of a wide spectrum of resources and expertise.
- Work is well organized and critical needs are being addressed.
- The greatest project strength is the experience of the team.

Weaknesses

- None observed.
- Project will continue to become more and more complex as time line to full implementation comes closer. Need better coordination effort.
- The greatest weakness is not with this project but with the barriers that still must be overcome in developing the thermo chemical water splitting cycle that is to be interfaced with the nuclear reactor.

Specific recommendations and additions or deletions to the work scope

- None

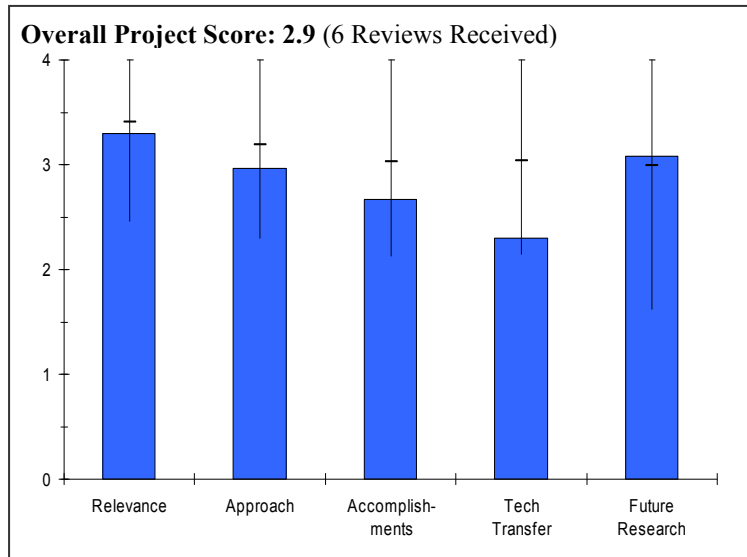
This project is well focused. No additions or deletions are recommended.

Project # PDP-01: A Novel Slurry-Based Biomass Aqueous Phase Reforming Process

Ying She; UTRC

Brief Summary of Project

The objectives of the project are to: 1) illustrate, through initial feasibility analysis on a 2,000 ton/day (dry) biomass plant design, that there is a viable techno-economical path towards DOE's 2012 efficiency target (43% lower heating value (LHV)); 2) assess the requirements for meeting DOE's 2012 cost target (\$1.60/kg H₂); and 3) demonstrate, through preliminary results, that an acid-tolerant, model sugar or sugar alcohol solution reforming catalyst has been synthesized. Future work will include hydrolysis work, catalysis discovery and testing, micro-scale continuous operation of membrane reformer with batch hydrolysis, and a final economic and energy analysis.



Question 1: Relevance to overall DOE objectives

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

- Although the hydrolysis approach may be well suited to certain feedstocks, it is unclear that it will be cost-competitive with more conventional gasification approaches over a wider range of feedstocks. Overall hydrogen production will be lower since lignin component is used for process energy only.
- Difficult to see how this process, with the added step of pretreatment, can compete with a gasification process.
- If process is incorporated with a fermentation process, the pretreatment might make more sense, but taking a dry (or relatively dry) feedstock and adding a dilute acid for the purpose of partially decomposing the biomass prior to a catalytic process that may or may not tolerate the acid and/or the water, seems unlikely to be cost-competitive.
- Analysis shows some promise, but a number of favorable assumptions have to be made about the catalyst and the process.
- Addresses DOE Program goal of low cost hydrogen production from renewable resources.
- Addresses barriers of reducing capital cost and improving efficiency.
- Biomass key element of DOE program.
- Highly rewarding if successful in converting biomass to hydrogen. The liquid phase reformability is key.
- Project supports MYPP.
- Project represents low greenhouse gas emissions process.

Question 2: Approach to performing the research and development

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

- Process flow and economics assumes high energy recovery from lignin combustion. The team needs to better address the issues that will be associated with the grade of lignin that will be coming out of this acid-treatment approach. High amounts of inorganics (alkali, etc) are likely and will greatly lower fuel value of the lignin.
- Good use of experimental design.
- It is unclear how much benchmarking of the current state of biomass hydrolysis has been performed.

PRODUCTION AND DELIVERY

- The analysis was thorough, although it is not completely clear why they performed sets of analyses where only a single parameter was varied, in addition to analyses where multiple parameters were varied (second set more useful in mapping out the experimental space).
- Overall approach involving biomass hydrolysis and aqueous phase reforming is based on known chemistry and appears sound.
- Suggest adding an early step in the program to test tolerance of aqueous phase reforming catalysts to heteroatoms expected in the hydrolysate e.g. HCl, SO_x or H₂S, NH₃ plus alkali metals.
- Should consider a "plan b" for hydrogen purification as no cost effective, robust membrane systems for removing hydrogen from this specific gas mix have been identified. Not at all clear that the proposed Pd membrane is affordable or stable to the various heteroatoms expected in the reformat (HCl, SO_x, NH₃, etc).
- Staged approach with upfront economic analysis is sound.
- Should do tornado diagram analysis on key conclusions looking at impacts of economic basis.
- Liquid phase reforming is attractive because of the lower temperatures.
- Kinetics are slow, byproducts are likely to be numerous.
- A Pd-based membrane does not appear attractive because of cost, contamination, and flux limitations.
- Limited early H₂ membrane testing.
- Limited catalyst life testing planned.

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

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

- Recommend performing sensitivity analysis around sulfur emissions and lignin combustion value.
- Progress is reasonable given low funding levels.
- Interesting analysis results.
- Good approach to catalyst development/discovery.
- Project appears to have just gotten underway, starting with a process modeling exercise. No experimental work done to date, and that is understandable
- The process and economic modeling predicts hydrogen costs that appear to be unbelievably low. Capital cost appears very low for a process of this complexity and requiring a Pd membrane module for H₂ cleanup. Feedstock cost also seems low. No accounting for disposal of by-product ash. Suggest researchers carry out a real cold eyes review with experienced industry veterans and DOE staff to validate these expectations of low cost.
- Little money so far so no expectations.
- Funding delays have limited work.
- System simulation and analysis has been conducted.
- Project at 2 year point (about 60% of project life), but only 10% complete – DOE funding to date about 20%.

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

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

- Funding limitations have limited tech transfer/collaborations.
- Industry-led project.
- North Dakota Energy Center listed as a partner...not clear how they are contributing or plan to contribute.
- Partnership of UTC and UND. Should set up "association" with similar biomass projects.
- Don't have the answer but UTC and the University of North Dakota seems light on partnerships.
- Role of UND is not clear.

Question 5: Approach to and relevance of proposed future research

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

- Need to include lignin combustion characterization and sulfur recovery in future plans to validate efficiencies and economic assumptions.
- Effective and sulfur-tolerant catalyst discovery is critical to continued effort.
- Experimental plan is sound, but should address the comments made above regarding impact of heteroatoms in feedstock on performance of hydrolysis reaction, reforming, and separations.
- Approach is fine.
- Demonstration of the performance of an acid tolerant sugar reforming catalyst is very important.

Strengths and weaknesses

Strengths

- Strong capabilities in process modeling, economic analysis and catalyst development.
- Good process engineers, with systems analysis support.
- Interesting approach for catalyst discovery.
- Clear "gated" approach.
- Promises hydrogen from renewable biomass.
- Liquid phase reforming will be carried out at lower temperatures.

Weaknesses

- As mentioned, process economics and system efficiencies rely heavily on assumptions regarding lignin combustion. Technical challenges around this have not been adequately addressed.
- Pretreatment adds a lot of complication to the process that may not add much value unless byproducts are produced.
- Only 2 collaborators. Neither has industrial experience in field of study.
- Reformability of the biomass slurry.
- Tolerance of the Pd-membrane to the byproducts.
- The reaction is slow, hydrogen release is slow. Hydrogen concentration in the gas phase will be low, only part of which will be transported across the membrane. What is the expected final hydrogen yield (kg H₂ per kg of biomass slurry)?
- Will this process require subsequent methane reforming?

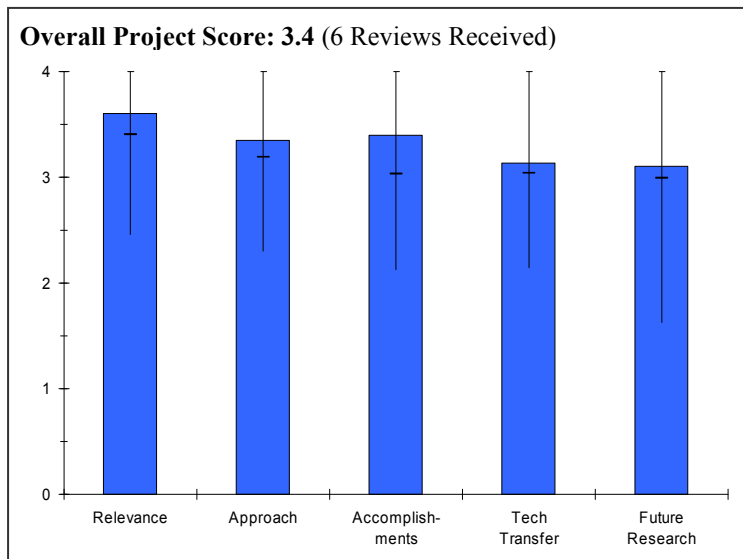
Specific recommendations and additions or deletions to the work scope

- Investigate potential interactions with cellulosic ethanol processes.
- Establish the liquid phase reformability before moving on to hydrogen separation and recovery.

Project # PDP-02: Hydrogen from Water in a Novel Recombinant Oxygen-Tolerant Cyanobacteria System
Qing Xu; Venter Institute

Brief Summary of Project

The overall goal of this project is to produce a cyanobacterial recombinant to produce H₂ continuously. The objective is to develop an O₂-tolerant cyanobacterial system for continuous light-driven H₂ production from water. Cyanobacteria have the ability to split water photolytically into O₂ and H₂, but their hydrogenases are highly O₂-sensitive. In contrast, certain bacteria have O₂-tolerant H₂-evolving hydrogenases, but they can not use water as the electron donor. The approach of the project is to transfer O₂-tolerant hydrogenases into cyanobacteria by 1) identifying novel O₂-tolerant hydrogenases from the Venter Institute's sampling in international waters and transferring them into cyanobacteria, and 2) transferring known O₂-tolerant hydrogenases into cyanobacteria.



Question 1: Relevance to overall DOE objectives

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

- Effort is focused on fundamental issues related to long-term renewable hydrogen production.
- Multiple methods for producing hydrogen will be needed, and this project supports the objective of having a diverse portfolio of technologies.
- The project has a high potential for supporting the hydrogen fuel initiative.
- The project provides a unique and desirable niche with the HFI.
- The project could make significant contributions to the stated barrier of the continuity of hydrogen photo production.
- The direct advancement to overcome or broaching the relevant barrier was not addressed in the report.
- The combination of bioprospecting and metabolic engineering is very interesting and has significant potential.
- The project goals are critical to the Hydrogen Initiative and fully support DOE RD&D objectives for hydrogen production.
- Ties directly to the program goal to "Develop advanced renewable photoelectrochemical and biological hydrogen generation technologies".
- Project is high-risk, long-term R&D which provides balance for the program's shorter term hydrogen production efforts.
- Project supports MYPP long-term biological RD&D.

Question 2: Approach to performing the research and development

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

- Looking to nature to identify good candidates, and then transferring genes to an appropriate host.
- High-risk, long-term research.
- The approach has a high risk of technical failure.
- Process/culture options for removal of oxygen should be explored.
- Information regarding the robustness of this organism, normal growth rate, normal culture needs (e.g., required rate of sparging with air), acceptable temperature range, etc. in absolute terms and as compared to other bacteria

was not presented. This information will be critical to determining whether the "view is worth the climb" for this project. A fast growing, non-fussy, "low-maintenance", CO₂ eating bacteria that can be dried out and used for biomass, and also produces a little hydrogen while it's growing would probably be much more valuable than a slow-growing, "high-maintenance" organism that produces a little more hydrogen.

- The approach is appropriate and the group is making interesting and significant progress.
- The limited number of hydrogenases identified is somewhat of a surprise and the investigators could consider mechanisms to probe deeper into the diversity of the environments further exploiting the available vast metagenomic data.
- The potential for controlled expression of hydrogenases in cyanobacteria is a very exciting element of the project.
- The project subtasks are well-focused on specific technical barriers.
- The project subtasks are well-balanced with respect to different technical barriers.
- The project subtasks are clearly designed to integrate with other research supported by the DOE Hydrogen Program.
- I would like to see a vision presented of how such a system would work in practice if a suitable organism could be engineered, and what the water and land (or other) resource requirements would be, at least in a general way. There is no discussion of what the barriers might be beyond the development of the organism itself.
- Straightforward strategy to transferring hydrogenase enzymes with desired oxygen tolerance to organisms of interest.

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

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

- Good level of accomplishment, especially considering the lack of funding.
- A great deal of work has been accomplished. A great deal more is needed.
- The group is making significant progress in the two major thrusts of the project: a) the identification of new stable hydrogenases and b) the heterologous expression of hydrogenases identified in nature.
- Probing the vastly and rapidly accumulating metagenomics data for potential contributions to the hydrogen fuel initiative is very important and merits continued support.
- The selection or derivation of specific milestones and performance indicators was not evident; however, the progress towards specific performance parameters has been excellent. What specific metrics will be devised to determine desired thresholds of oxygen tolerance in combination with volumetric productivity of hydrogen evolution?
- In evaluating this project as a research endeavor, the progress has been outstanding. There have been a few significant publications resulting from this funding, demonstrating excellent progress for such a recently-initiated effort.
- The identification of candidate oxygen-tolerant hydrogenases from a bioinformatics survey of the Global Ocean Sampling project was a significant technical accomplishment. The identification of novel hydrogenases is a good beginning to guide evolution of the Thiocapsa hydrogenase to even higher oxygen tolerance and activity.
- The progress in expression of these novel hydrogenases in Thiocapsa was good, although it was unclear whether activity had been demonstrated. If so, how did that activity compare to the endogenous hydrogenase in this organism?
- The heterologous expression of the Thiocapsa oxygen-tolerant hydrogenase and corresponding maturation proteins in Synechococcus was a good technical accomplishment.
- The heterologous expression of the Rubrivivax oxygen-tolerant hydrogenase and corresponding maturation proteins in Synechocystis was a good technical accomplishment, with demonstration of hydrogenase activity. It was unclear how the level of activity was benchmarked, and what was the eventual target.
- The progress has been all the more remarkable given the minimal level of FY06 funding—with most funding contributed by the Venter Institute.
- Need to keep in mind the DOE's ultimate goal here, which is "By 2018, verify the feasibility of these technologies to be competitive in the long term". So, as project progresses, some attention to system engineering and costs will be needed to make go/no-go decisions on this pathway.
- Progress seems to be good, particularly given lack of DOE funding in 2006.
- Good process despite lack of funding.

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 NREL.
- DOE is getting a lot for their money on this project.
- The group has an ongoing cooperation with NREL and has the potential for broader collaborations as the work progresses, especially at academic institutions.
- Most of the research is not developed to the point where opportunities for technology transfer are apparent.
- There will obviously be opportunities for intellectual property development and tech transfer; however, the mechanism may be complex due to the individual MTAs developed for the countries with jurisdiction over the sites employed in the GOS.
- The investigators demonstrate good coordination and collaborations with university researchers.
- The investigators demonstrate good coordination and collaborations with international researchers.
- It is unclear what, if any, coordination or collaboration has been done to explore other, similar, work in this field.
- Have any papers been published and presented at technical conferences?

Question 5: Approach to and relevance of proposed future research

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

- Plan builds on progress.
- May need to have more definitive strategy for discontinuation of work with specific organisms.
- The project is progressing at a steady pace, and future plans continue to support the long process of developing a viable hydrogen producing organism.
- The proposed plan is appropriate for the level of support dedicated to the project.
- A more clear description of the mechanism by which active expression in the cyanobacterial host might be achieved and potential contingencies in the experimental design would strengthen this component of the work.
- A potential avenue for increasing the scope of the work is to implement a mechanism to probe whether additional hydrogenases exist within the metagenomic sequence data collected from these environments.
- The investigators clearly present a plan to build upon their discovery-driven success for designing an oxygen-tolerant hydrogenase, through additional characterization of the novel hydrogenases identified from the GOS.
- The plan to improve Rubrivivax hydrogen production through refinement of corresponding accessory genes and heterologous expression in Synechocystis is a logical next step after the proof of principle in *E. coli*.
- Again, project should address potential systems-level barriers – the ultimate goal is cost-effective production of large amounts of hydrogen, not the design of a new microorganism.

Strengths and weaknesses

Strengths

- Dedicated participants.
- Good understanding of the concept.
- Venter Institute and NREL collaboration appears to be very strong and beneficial to DOE.
- The project represents a unique and attractive niche in the program.
- Although perhaps high risk, the project has the potential to offer something completely new.
- The combination of bioprospecting and expression directed toward metabolic engineering has exciting potential outcomes and is an outstanding strength.
- The expertise of the group approaching the project is excellent for the proposed work.
- The investigators demonstrate clear experimental design of each subtask, and have made significant progress in all initial phases of the project.
- The tenacity of the investigators to obtain alternative funding and collaborators is to be commended.
- The team is expert in the study of hydrogenases, and the combination of a strong bioinformatics/genetics effort with a focused biochemical approach is excellent.

- The knowledge of these investigators for the microbial and algal systems under study is well-focused towards the project goals.

Weaknesses

- Difficult problem (high-risk research).
- Alternative methods for overcoming oxygen intolerance (such as the removal of oxygen) should be explored.
- There is limited attention to potential outcomes and contingencies in the cyanobacterial expression component.
- It is unclear how the results from the heterologous expression studies will be synergized--how will information obtained from the characterization of novel hydrogenases be used to improve the Rubrivivax studies? Are these all to be done in parallel, at separate labs – essentially two unrelated projects under a single umbrella? There should be a clearer definition of checkpoints and cross-talk for these experiments.
- The investigators have not clearly articulated what the targets or milestones are—are they seeking for a specific level of hydrogen production for a specific oxygen-tolerance? Have they thought about theoretical limits for these enzymes (when will they know they have sampled enough ocean samples to know there are no additional hydrogenases resembling the Thiocapsa proteins)?
- The proposed experiments are all focused upon comparisons and refinements of known hydrogenases. How will the investigators recognize novel forms of hydrogenases, which may possess the desired production and oxygen-tolerance targets but not resemble canonical hydrogenases? What if these hydrogenase activities require multiple subunits, or accessory factors that do not resemble those known to date?

Specific recommendations and additions or deletions to the work scope

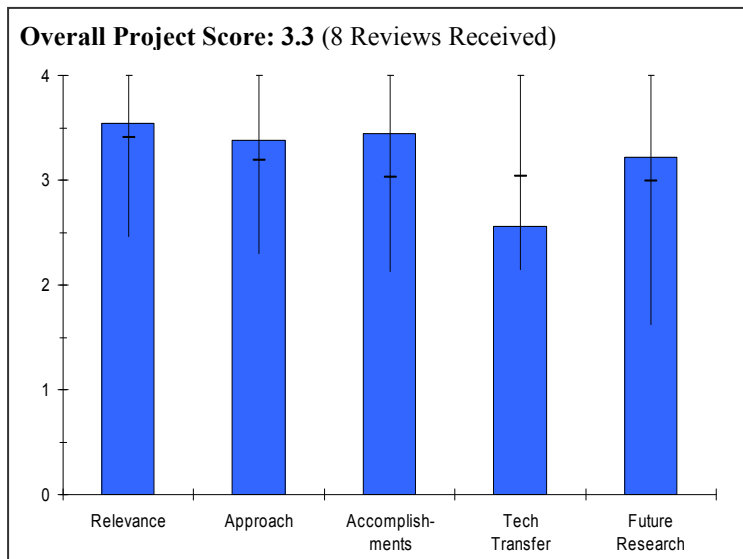
- Probing for additional hydrogenase (increased diversity) within the existing metagenomic data.

Project # PDP-06: Investigation of Bio-ethanol Steam Reforming over Cobalt Based Catalysts

Umit Ozkan; Ohio State U

Brief Summary of Project

The overall objective of this project is to acquire a fundamental understanding of the reaction networks and active sites in bio-ethanol steam reforming over Co-based catalysts that would lead to 1) development of a precious metal-free catalytic system which would enable low operation temperature (350-550°C), high ethanol conversion, high selectivity and yield of hydrogen, and minimal byproducts such as acetaldehyde, methane, ethylene, and acetone; 2) understanding of the catalyst deactivation and regeneration mechanisms; and 3) low cost for commercialization.



Question 1: Relevance to overall DOE objectives

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

- Development of a precious-metal free, low temp. Ethanol catalyst would significantly reduce H₂ cost.
- Directly related to goals; good initial analysis of catalysts; supports effort to reform bio-derived liquid fuels.
- Project supports DOE program goal of hydrogen production from renewable resources.
- Seeks to reduce cost for ethanol steam reforming by identifying base metal catalysts and thereby eliminating need for platinum-group metal catalysts.
- Project has excellent relevance to DOE H₂ Production goals.
- Identification of inexpensive (non-precious metal) catalyst is critical.
- Ethanol is only a transition fuel to the hydrogen future which cannot meet all (domestic) transportation needs and lignocellulosic ethanol production has (many technology barriers).
- Suggest looking at other biomass-derived liquids (e.g., dimethyl ether).
- Unclear as to the cost reductions achieved with CoZrO₂ catalyst and low temp processing but assume costs are lower than precious metal so likely helpful in reducing capital costs.
- Useful empirical validation.
- Important effort to find a metal free catalytic system.

Question 2: Approach to performing the research and development

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

- Too much experimental data is presented: specific approach to optimizing performance is obscured. Needs an overview to explain differences between tests conducted.
- Good initial start; good focus on reaction mechanisms.
- Sound approach based on fundamentals of heterogeneous catalysis: identify active catalytic phase(s) and synthetic techniques to optimize population of those sites; identify reactive intermediates, reaction networks and desired reaction pathways. Identify deactivation pathways and regeneration methods.
- Outstanding systematic approach to catalyst characterization and structure/property relationship.
- Firm fundamental scientific approach; strong application of catalyst characterization techniques.
- Excellent catalyst R&D. Excellent presentation of results (albeit with too many slides).
- Need systems analysis to determine if this is a viable project and to define research objectives.
- Good use of thermogravimetric and infrared analysis to follow reaction mechanism.

- Logical and well thought out approach.
- Fuel processor capital costs, O&M, and feedstock issues are addressed by this project.
- This is a university project. The approach is to select few materials, prepare catalyst, carry out reforming reaction, and study deactivation/regeneration characteristics. This seems to be quite appropriate for university-led research.

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

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

- Hard to quantify significance of test results since no longevity testing.
- Achievement of high H₂ yield at <500C is significant.
- Lifetime testing is missing. Although unknown, the performance may decline precipitously. Good initial progress, but less than half complete; quality data presented.
- Identified very active, selective cobalt-based catalyst as well as preferred support and methods of preparation.
- Comprehensive studies on characterization of bulk catalyst as well as surface intermediates.
- Good body of work on catalyst evaluation for activity, selectivity.
- Impressive property-activity correlation completed to maximize catalyst performance with minimal trial and error.
- Modified catalyst formulation has outstanding H₂ selectivity with minimal CO production.
- Outstanding application of basic science to achieve a promising commercial solution to EtOH reforming.
- Excellent understanding of catalyst performance.
- Excellent set of experiments.
- Catalyst reaction mechanism and selectivity well understood under varying temperatures.
- Need more stability data; they only have 70 hrs.
- Need a purification scheme.
- Good data; useful information to other reformer studies.
- (Good) understanding (of) the competing reaction networks in steam reforming of ethanol.
- (Good) identification of active (reaction) sites.

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

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

- No evidence of any collaboration.
- Not clear on partner roles/activities to date.
- Not at all clear what the supporting organizations (NexTech, PNNL) have contributed to this work.
- Good collaboration with catalyst manufacturer and PNNL for economic analysis.
- As catalyst technology is considered close to commercially ready, need to add an industrial collaboration.
- Could use a reactor design and reforming commercial partner.
- Should work to identify 'industrial partnerships' for catalyst scale-up.
- Industry interest is missing. Lacking active collaborations with other groups.
- Good coordination.
- Excellent body of publications and presentations. Glad to have this knowledge in the public domain.
- Teaming with NexTech Materials for catalyst manufacturing scale-up.
- Teaming with PNNL for economic analysis & catalyst deactivation studies.

Question 5: Approach to and relevance of proposed future research

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

- Need to add long term testing.
- Strong plan, especially deactivation/regeneration studies.

PRODUCTION AND DELIVERY

- Good plans for the future builds on the group's strengths: kinetics and mechanistic studies, in-situ characterization of working catalyst surfaces, and identification of deactivation pathways. This is a new catalytic system and needs to build the fundamentals in order to advance to a commercial process.
- Must assume project partners (NexTech and PNNL) will be involved in the catalyst scale up and economic evaluation.
- Future work properly focused on deactivation/regeneration, catalyst production considerations, and overall economic analysis.
- Should develop overall process design in which to use this catalyst.
- Could this catalyst technology be used for reforming of other bio-liquids?
- Would be useful to see economics of the processor and hydrogen production (costs) as calculated using DOE's H2A model.
- Although not included on poster, assume there are plans to process other feedstocks beyond ethanol.
- Well thought out and appropriate
- Research plan for future work is very reasonable.
- Kinetic and mechanistic investigations (well) coupled with in-situ characterization.
- Economic analysis based on updated catalyst system knowledge database.

Strengths and weaknesses

Strengths

- Research may have an ethanol catalyst breakthrough but it's too early to tell.
- Good focus on reaction mechanisms.
- Excellent R&D effort.
- World-class catalyst research.
- Comprehensive reporting.
- Strong basic fundamental understanding of reaction sites. Availability of graduate students and post-docs at OSU. Lower overhead rate compared to industry and national labs.
- At this point (only 40% complete), no glaring weaknesses.

Weaknesses

- Needs description of catalyst optimization approach. What is strategy beyond experimentation?
- Needs catalyst life testing.
- No systems understanding demonstrated.
- Poster presentation did not adequately provide the means to address all the issues.
- Stronger coordination with partner(s) needed.
- Too much data presented. This much technical detail is not needed for this type of review.

Specific recommendations and additions or deletions to the work scope

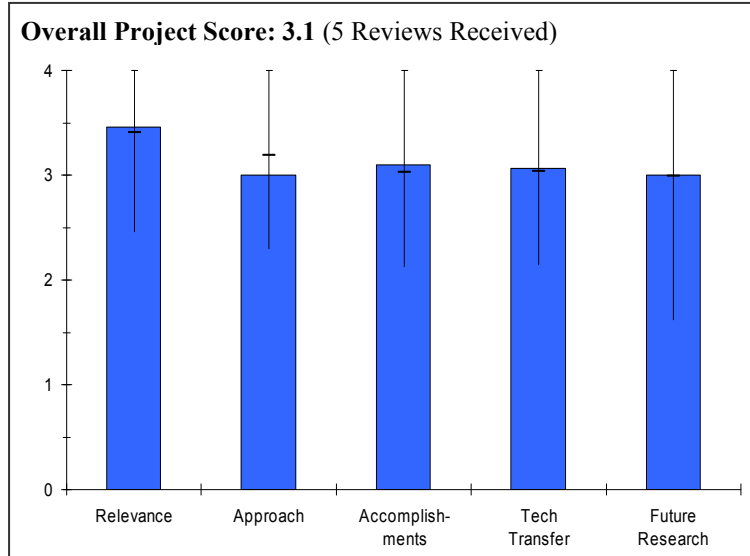
- Add catalyst lifetime testing.
- Add testing of contaminants.
- Suggest construction of integrated process development unit.
- Economic analysis should be an integral part of research in order to quantify effect of R&D achievements and steer R&D objectives.
- Continue to support the work.

Project # PDP-07: Distributed Bio-Oil Reforming

Bob Evans; NREL

Brief Summary of 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 objective in 2012 is to produce hydrogen for less than \$3.80/gge. The objective for 2007 is to demonstrate integration of bio-oil atomization, partial oxidation, and catalytic conversion to obtain equilibrium syngas composition at 650°C.



Question 1: Relevance to overall DOE objectives

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

- Important to further the understanding of biomass pyrolysis interactions.
- Developing a viable pathway to achieve a renewable forecourt production system is a major step forward.
- Good focus on conversion of biomass to liquid fuel to hydrogen.
- Key source of renewable hydrogen.
- Provides cost effective syngas for bioproducts.
- Supports MYPP gasification / pyrolysis technology development.
- The program focuses on developing autothermal reformer for bio-oil processing to meet DOE 2012 hydrogen cost targets for biofuel production of hydrogen. However, focus is on methanol currently not other more relevant alcohols like ethanol, sorbitol etc.

Question 2: Approach to performing the research and development

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

- Use of modeling in parallel with experimentation is good.
- Good focus on bio-oil conversion; unclear on whether path from biomass to bio-fuel to hydrogen makes sense from an energy cycle viewpoint.
- Addresses key thermochemical barriers to low cost syngas.

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

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

- Scale up of ultrasonic nozzle atomization not well understood/explained.
- Need to better quantify extent of sooting and degree to which it can be burned-off/cleaned.
- Reasonable progress, yet unclear if cycle is viable.
- Good accomplishments for limited budget.
- Focused on major barriers.
- Successful in developing needed analytical instrumentation / methods.

PRODUCTION AND DELIVERY

- Developed atomizer, cracking process and autothermal bench scale reactor. Validated the need for oxidation to increase CO production.

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

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

- Focused primarily on NREL activities.
- (Should) link work to thermochemical research in the Office of Biomass Programs at DOE to produce (bio)-products.
- Good representation from national lab, academic and industrial partner, yet more collaboration with industry is needed.
- Papers will be given at ACS and other public forums to share results.
- Partnership with Chevron further demonstrates project merit.
- University of Minnesota performing systematic catalyst study.

Question 5: Approach to and relevance of proposed future research

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

- Reasonable project but limited benefit as hydrogen pathway.
- More fundamental research should be proposed on mechanism of reactions, efficiency and catalytic selection.
- Investigators have considered contingency paths.
- Addition of WGS and parametric studies just need to broaden focus beyond methanol.

Strengths and weaknesses

Strengths

- Tests with Rh show promise of equilibrium reaction with reversible/recoverable sooting.
- Good data on particle oxidation.

Weaknesses

- Currently requires biomass mixing with methanol. Would prefer that MeOH mixing was not needed.
- Needs more innovation and support.
- Consider an integrated bio-refinery approach to reforming bio-oils.

Specific recommendations and additions or deletions to the work scope

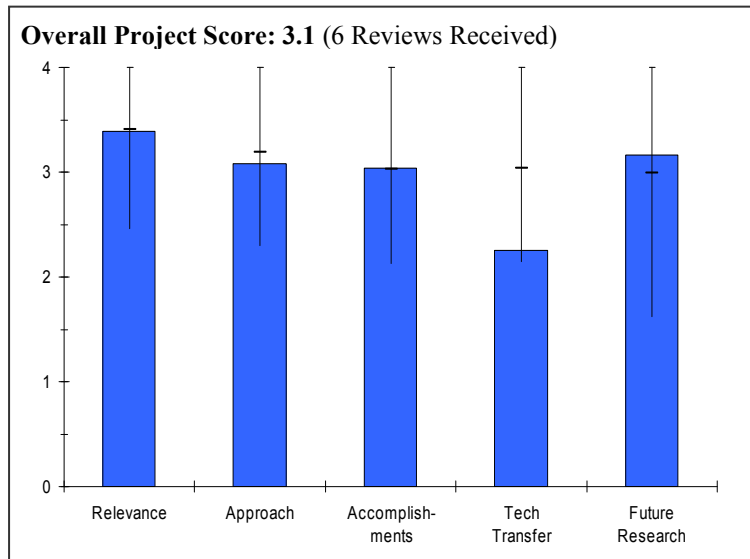
- Need to evaluate energy cycle.
- Independent review by company such as ADM which is heavily involved in products from oils.

Project # PDP-08: Hydrogen Generation from Biomass-Derived Carbohydrates via Aqueous-Phase Reforming Process

Randy Cortright; Virent Energy Sys.

Brief Summary of Project

The overall objectives of this project are to design a generating system that uses low-cost sugars or sugar alcohols that can meet the DOE H₂ cost target for distributed reforming of bio-derived liquids of less than \$3.00 / gge by 2017, and to fabricate and operate an integrated 10 kg H₂/day generating system. The objective for 2006 was to develop aqueous-phase reforming (APR) catalyst, reaction conditions, and a reactor suitable for converting glucose to hydrogen. Objectives for 2007 are to 1) continue to investigate catalyst, reaction conditions, and reactor suitable for converting low-cost sugars to hydrogen; 2) calculate the thermal efficiency and economics of the APR system utilizing different feedstocks (low-cost sugars, glucose, sugar alcohols); 3) compare the results of techno-economic analysis with DOE Hydrogen Program goals; 4) reach a go/no-go decision on whether to proceed with the design and construction of a 10 kg H₂/day demonstration system with the preferred feedstock; and 5) design a 10 kg H₂/day demonstration system.



Question 1: Relevance to overall DOE objectives

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

- Simple, single-step conversion of H₂ is a major step toward achieving DOE H₂ cost goals.
- Able to use renewable feedstocks.
- Strong focus on biomass conversion to hydrogen production; important pathway.
- Addresses DOE program goal of hydrogen production from renewable resources.
- Targets cost reductions in feedstocks, capital, operations, and GHG emissions.
- Small scale could provide significant advancement for renewables H₂ production.
- The objective of this project is to design and build a 10 Kg/day H₂ generating system by aqueous phase reforming (APR) bio-mass derived carbohydrates (sugar or sugar alcohols). This project is relevant to DOE's overall objectives.

Question 2: Approach to performing the research and development

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

- Specific approaches are proprietary. Little on which to base a score.
- Very early in the study, much of what was presented not directly linked to project.
- Continues to build on the novel concept of aqueous phase reforming as the front end of a number of biomass to fuels processes, including hydrogen.
- Work is expanding to include ever more complex sugar and sugar alcohol feeds.
- Already demonstrated that the APR can produce H₂ from glycerol and sorbitol now needs to optimize for glucose.
- Use of sorbitol or other alcohols only make sense if you are trying to maximize conversion efficiency.

PRODUCTION AND DELIVERY

- Feedstock cost, reformer capital cost, O&M, and GHG emissions are barriers addressed by this project. Due to budget cutbacks this project is focusing efforts on development of catalysts for APR.
- If feedstock costs decrease from \$2.10/gge in 2012 to \$1.55/gge in 2017, and hydrogen production cost are \$3.80/gge by 2012 and less than \$3/gge by 2017, then the approach will reach DOE's projected cost targets for hydrogen from bio-derived liquids.

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

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

- Claimed efficiency of ~78% is impressive.
- Not clear how long term testing can show steady H₂ production while gas conversion % declines significantly.
- Limited as project is early in development.
- Much work on catalyst and process development. Impressive progress for a small startup company.
- Gained a lot of knowledge from processing glycerol and sorbitol that is transferable to glucose and other polysaccharides. Still need to optimize hydrogen production efficiency and reduce Alkane by-products.
- Not much has been accomplished.
- Performance is still behind conventional reforming technology.
- Continuing to investigate catalysts for APR of glucose provided by ADM.
- Has shown catalyst lifetime of greater than a year and tested a first generation reactor system (Green Energy Machine).
- Studied effect of feed concentration. Generated hydrogen was burned for internal process use.

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

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

- Not clear on partner roles in development or milestones.
- Not clear what role partners ADM and U Wisconsin have played in the work to date.
- As this is a commercial venture they are very careful about IP protection so there appears to be limited sharing around catalyst technology and plans for generator design modifications.
- Poor communication.
- ADM is the only company interested, potentially to use a thermochemical route for corn sugar.
- Teamed with ADM and University of Wisconsin. APR process was developed at University of Wisconsin and therefore Virent is closely tied with University of Wisconsin. ADM is providing biomass derived liquids.

Question 5: Approach to and relevance of proposed future research

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

- Good plan to include go/no-go decision point; need to include overall energy analysis and quantify benefits.
- Program on a clear path to a go/no-go decision for construction of 10 kW pilot demonstration system in 2007.
- Focus on low cost sugars and PSA development seems to be on target.
- Not clear what the R&D objectives are.
- Economics are marginal.
- Needs an independent assessment or risk assessment review.
- Future work in 2007 is focused on developing catalysts that converts glucose and sugar alcohols to hydrogen.
- Virent will also investigate hydrogenation technologies to convert both monosaccharides and polysaccharides to sugar alcohols.
- Go/no-go decision will be made for the APR technology.
- The PI has responded well to previous year reviewers' comments.

Strengths and weaknesses**Strengths**

- Converter is simple and low temperature: no WGS, steam boiler, hydrodesulfurizer.
- Has clear commercialization potential if successful.
- Mentioned a commercial manufacturer, but didn't indicate who it was or if they had a market plan.
- Novel technology (APR).
- With limited funding, Virent has already built and operated a 6 NM³/hr alpha unit utilizing glycerol as feed stock.

Weaknesses

- Catalyst performance degrades significantly in <1 year.
- Strategy of raising temperature to higher catalytic activity helps only slightly.
- Substantial methane production: limits H₂ yield.
- Process economics is not addressed. Should be favorable since simple, low temperature reactor, but should be quantified.
- Project is built on assumption that sugar alcohols will be available as low cost feedstocks at fuels scale. The emphasis of the Virent approach should shift to a focus on low cost biomass feedstocks that might be available in significant volumes at costs under \$60 per ton rather than on sugar alcohols as the US is not a major sugar producer.
- No information on commercial availability of catalyst.
- Not having a clear focus on any one particular feedstock.

Specific recommendations and additions or deletions to the work scope

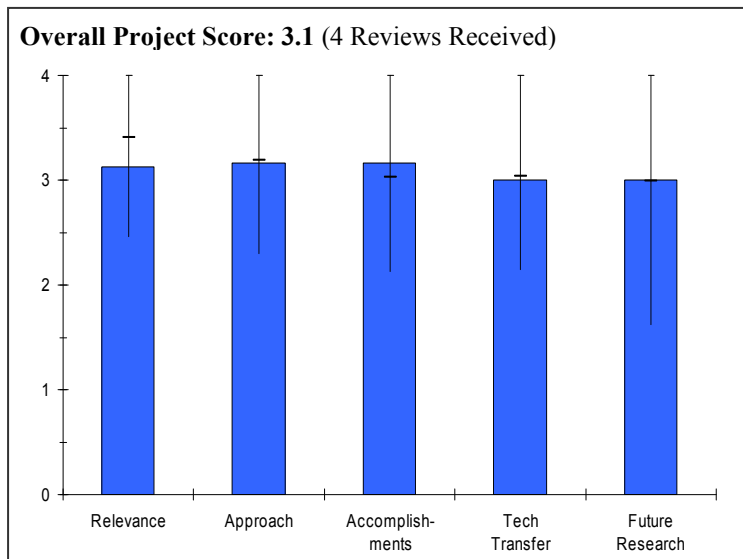
- Include overall energy analysis.

Project # PDP-09: Integrated Short Contact Time Hydrogen Generator (SCPO)

Ke Liu; GE Global Research.

Brief Summary of Project

Both short contact time and steam methane reforming catalysts are being developed and a compact reforming system was designed. An interim (2006) hydrogen production cost target of \$3.00/gge for distributed reforming from natural gas was achieved based on GEGR SCPO technology economic projections and those of other distributed natural gas research efforts. This project received an independent assessment verifying that the interim target was met. Specifically, GEGR is working with the University of Minnesota to: 1) discover sulfur-tolerant catalytic partial oxidation (CPO) catalysts; 2) develop sulfur-tolerant CPO catalysts; and 3) characterize CPO catalysts using X-ray diffraction and X-ray photoelectron spectroscopy. Argonne National Laboratory objectives include catalyst discovery, screening, durability testing, and characterization.



Question 1: Relevance to overall DOE objectives

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

- Development of simplified reformers, particularly multi-fuel reformers is a significant step towards meeting the DOE H₂ cost goals.
- Directly relevant to the economic production of H₂ in line with DOE Hydrogen Production targets.
- Reformer technology is critical to the initiative, but this technology doesn't appear to solve any problems or improve market penetration.

Question 2: Approach to performing the research and development

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

- Concept seems similar to other CPO configurations.
- Partial oxidation offers a path to compact, on site reformers, but has historically suffered from inability to operate reliably and safely at high pressure. (Good) focus on this problem as well as the identification of S-tolerant catalysts.
- Excellent leveraging of academia / national lab expertise in catalyst technology and characterization.
- Strong experimental program with impressive in-situ characterization of the reactor.
- Nothing new or innovative.

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

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

- Team has good experimental results with high space velocities.
- Great progress in operating at high pressure...a longstanding problem in this technology.
- Lots of work in evaluation of S-tolerant reforming catalyst...though not much progress towards a breakthrough.
- Successfully demonstrated applicability of short contact time hydrogen generation.

- Would be helpful to show SCPO overall process integration to produce pure hydrogen.
- Not much data presented; no comparison to conventional ATR performance.

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

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

- Impressed with strong record of collaboration with partners at U. Minnesota and Argonne National Labs. Poster clearly laid out contributions from both institutions and work processes they are using to collaborate.
- Effective collaboration between industry, national lab, and academia.
- Unclear what is being done.
- Limited publications.

Question 5: Approach to and relevance of proposed future research

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

- Testing at 1M space velocity is good goal.
- Not clear how practical 400psi operating pressure is: good from H₂ purification viewpoint but bad if one has to compress air/oxygen to that pressure.
- Clear path forward for natural gas reforming, and they are thinking about attacking more ambitious liquid feeds derived from renewable resources.
- Proposed future research needs to be more clearly stated.
- Would like to see a plan to commercialize this technology (start by building a prototype hydrogen generator).
- Question whether this technology can compete with conventional SMR; should capitalize on the potential to reform complex hydrocarbons which can't be done with steam reforming.
- Limited in scope and value.
- Uncertain how large cost reductions were derived.

Strengths and weaknesses

Strengths

- This is the best project I had the opportunity to review. It is using novel science and engineering to attack a difficult problem, and there is clearly a great deal of interaction and collaboration amongst the industry, national laboratory, and university participants. This is the poster child of how industry, universities, and national labs should work together in developing novel science and engineering to attack a national challenge.
- GE corporate involvement.
- Potential to handle multiple feedstocks.

Weaknesses

- Innovation.
- Independent review.
- Market study.

Specific recommendations and additions or deletions to the work scope

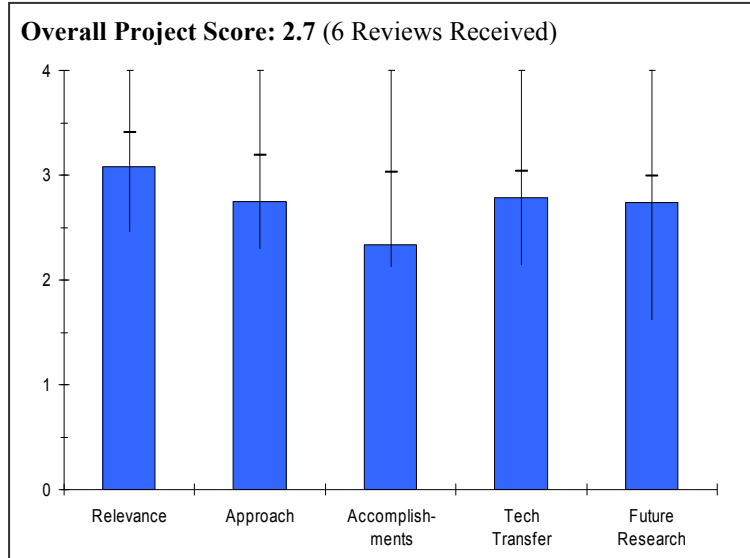
None

Project # PDP-10: Integrated Ceramic Membrane System for Hydrogen Production

Joseph Schwartz; Praxair

Brief Summary of Project

The overall objective of this project is to develop a low-cost reactive membrane based hydrogen production system that will 1) use existing natural gas infrastructure; 2) have high thermal efficiency; and 3) serve both the transportation and industrial markets – the industrial market provides immediate opportunities and will allow the project to gain valuable operating experience before fuel cell vehicles arrive. The Phase II objective is to integrate a hydrogen transport membrane (HTM) with water-gas shift (WGS) to 1) demonstrate low-cost hydrogen production, separation, and purification; 2) demonstrate HTM performance in reactive environments; and 3) develop a versatile system that can be combined with any syngas generation method for improving hydrogen production, especially at distributed scale.



Question 1: Relevance to overall DOE objectives

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

- Demonstration of process intensification is necessary to achieve DOE H₂ cost goals.
- The project is highly relevant to the production of high purity hydrogen. The work is considering membrane separation combined with WGS (process intensification). Success in either area would be of benefit to the DOE hydrogen program.
- Addresses DOE program goal of reducing capital costs in distributed hydrogen manufacture. Addresses specific barriers such as increasing membrane durability, tolerance to impurities, zero defects, high flux and selectivity.
- Integrated membrane system shows some potential for reducing hydrogen production costs.
- This is more an application engineering project.
- Scope seems to have changed from that proposed – a low cost, high performance reformer concept to meet DOE targets.
- Project objective is to develop a low-cost reactive membrane based hydrogen production system and therefore there is relevance to overall DOE objectives.

Question 2: Approach to performing the research and development

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

- Several concepts presented: OTM followed by HTM, and conventional reformer with HTM.
- Praxair has taken a reasonable approach. The work has focused on the development of the Zr support which appears to have been successful. In addition, they have worked on the development of alternative metal coatings (Pd and Ag). In both areas, Praxair appears to have had good success.
- Praxair has based the work on an earlier effort and has successfully leveraged the results of the prior research.
- Research at this point is primarily fundamental testing and Praxair has conducted a thorough testing program.
- The project is considering the presence of impurities. Some of the preliminary results do show some negative effect, but the Pd-Ag work does tend to indicate the material can be regenerated.
- Membrane reactors to remove hydrogen continuously during water gas shift is a sound approach to reducing size and increasing productivity of the water gas shift reactor.

- The project is relying on known materials for hydrogen separation (Pd-Cu, Pd-Ag).
- Workers recognize that thin membranes are needed to meet cost targets...wasn't clear from poster whether the membrane fabrication methods they propose to use can make these films defect free at this temperature.
- Praxair brings expertise in manufacturing the membrane substrate, with a target to do this at low cost.
- Would like to see work on thermal cycling of membranes to ensure integrity after multiple cycles.
- This project appears to have (departed from initial proposal), as it is not a high efficiency, low cost platform
- Just barely better than conventional.
- Unclear performance on wide ranges of natural gas composition.
- It appears that this particular project will not focus on reforming of natural gas (OTM) but only on HTM. Results obtained in previous DOE-funded project on OTM for reforming of NG will be used here.
- WGS catalyst might impede the performance of HTM and therefore there won't be much advantage in combining shift reaction and separation in a HTM membrane reactor.

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

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

- Total progress to date (accounting for funding delays) is good.
- Results thus far appear to be very good. Praxair is reporting that the results are meeting the DOE targets. However, reluctant to clearly present flux rate and conditions making it difficult to compare results to other work. In addition, cost information on the membranes is presented in a different format and it is not clear that they are effectively meeting the DOE targets.
- The work has resulted in the development of approximately 5 micron metal layers which is a good metal depth.
- The project has considered capital costs and the information provided suggests that this technology will improve over PSA separation.
- Prototype was developed, but not evaluated for scale-up, cost and risk.
- Manufacturing issues.
- Disappointed to see the lack of progress.
- Results are not promising for pilot plant demonstration.

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

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

- Praxair is the primary developer of the technology and is clearly interested in future commercialization of the technology. In addition, RTI has been involved in the development. This is an area that Praxair has been interested in for some time and they have a solid background in the technology development.
- Distribution of the results publicly is limited. Praxair needs to provide some additional public information. All of the data/information generated is not proprietary, and this work is of limited use to other developers and researchers.
- As membrane fabricator, RTI will be key contributor towards fabrication of modules.
- Reasonable amount of collaboration.
- Papers published, patents filed.
- Research Triangle Institute is developing Pd-based membrane for shift/separation. Praxair fabricates tubular membranes. It is not clear who is doing the catalyst work.

Question 5: Approach to and relevance of proposed future research

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

- Future plans are somewhat vague. It appears that Praxair will continue basic testing and conduct some basic economic analysis, but it is not clear if there will be any scale-up work.
- Next key step is to fabricate modules and demonstrate WGS integrated with H₂ separations.
- Assuming success, they can move on to next key tasks: defining production methods and commercial systems.

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- Proposed future work not clear or detailed enough.
- Doesn't address the issues with the technology such as durability and performance as a function of natural gas composition.
- Manufacturing defect tolerance.
- Independent assessment of risk.
- Praxair proposes to demonstrate performance in integrated WGS/HTM reactor. This will be a challenge. Effect of trace impurities in syngas on HTM is not addressed.
- Praxair plans to pursue other technology if HTM is not economical – this seems to be a good idea.

Strengths and weaknesses

Strengths

- Compelling economics if flux goals can be achieved.
- Hydrogen separation membrane appears to have good potential to effectively separate and produce high purity hydrogen streams.
- Good performance on previous projects (OTM for NG reforming) and process intensification to reduce costs.

Weaknesses

- H₂ flux accomplishments not specified. Unclear why technology performance is withheld.
- Cost goals should clarify if they are per tube or for entire separation system.
- Statement in conclusion of cost goal being hard to achieve is not substantiated.
- Unclear about sulfur/other substances contaminating the membrane. Need test data.
- No significant weaknesses.
- Poster did not clearly state assumptions for the stated cost/performance benefits.
- Not cost effective.
- No clear manufacturing cost strategy to achieve DOE goals.
- Combining shift and separation steps in a HTM membrane reactor without knowing the effect of WGS catalyst on the performance of HTM in separating hydrogen.
- No studies on effects of trace impurities found in NG on HTM performance.

Specific recommendations and additions or deletions to the work scope

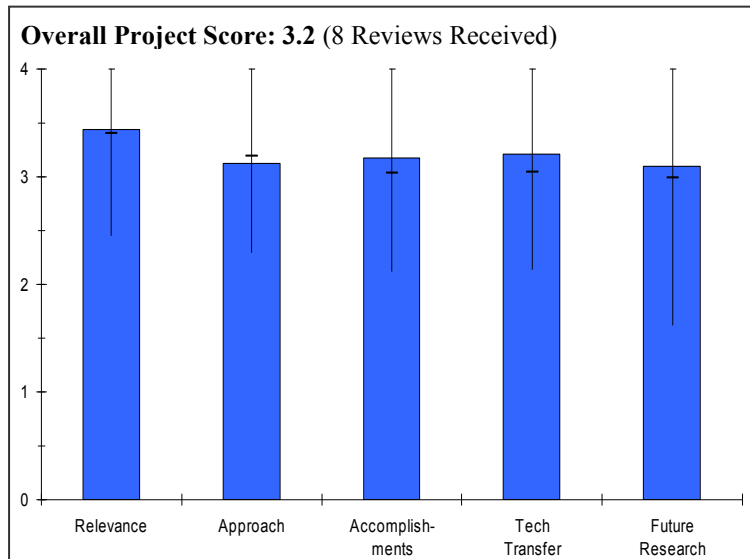
- Testing should be conducted with more realistic gas feed compositions.
- Independent review of cost benefits.
- Complete a risk assessment on whole development.

Project # PDP-11: High-Performance, Durable, Palladium-Alloy Membrane for Hydrogen Separation & Purification

Scott Hopkins; Pall Corp.

Brief Summary of Project

The overall objective of this project is to establish the technical and economic viability for use of a palladium alloy composite membrane in a distributed hydrogen production system. Objectives are to 1) develop a process that leverages the technical capabilities of a membrane for maximum economic benefit (reduced gallon of gas equivalent cost); 2) optimize membrane performance in terms of hydrogen throughput, purity and durability; and 3) minimize capital cost for the gas separation module, including pressure vessel, internal hardware, membrane, and substrate.



Question 1: Relevance to overall DOE objectives

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

- Demonstration of process intensification is necessary to achieve DOE H₂ cost goals.
- Pall is developing supported membranes for hydrogen separation. This is a primary DOE need for the production of pure hydrogen. The researchers are well aware of the DOE targets and are making a good effort to meet or exceed these targets.
- Pall is well aware that the membranes must move beyond a research stage and have a plan for further commercial development.
- Hydrogen separation and purification is a key element of distributed hydrogen production from natural gas and renewable fuels.
- At least for small scales of production, this approach can be cost competitive with the established PSA technology.
- One aspect of this approach is the need to establish reliability and durability, even after repeated thermal cycling.
- Technology could achieve cost and performance goals for a number of technologies.
- Permeation membranes are good. Pd-based membranes are challenged by pin-holes / thickness, hydrogen flux, poisons, and cost.
- Ties directly to the Program goal of reducing the costs of distributed hydrogen production.
- This project has the potential to reduce the capital and operating costs of distributed hydrogen production (and can also contribute to process intensification goals in the DOE Fossil Energy Hydrogen-from-Coal program).
- This project's objective is to develop durable Pd-alloy membrane for hydrogen separation.
- Development of a small, more cost effective (capital and operating) hydrogen purification system (relative to PSA) is relevant to the DOE objectives.

Question 2: Approach to performing the research and development

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

- Relatively simple approach that appears to have very good potential. The researchers are utilizing a porous metal tube as a support that will provide mechanical integrity. They have been able to produce membranes with

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a varied pore structure capable of accepting a Zr coating that will then accept the metal layer. Metal layer thickness has been minimized (5 micron) to maximize permeation.

- The experimental approach is reasonably straightforward and they are obtaining solid data that supports the fact that these membranes are capable of separating and producing high purity hydrogen.
- Pall realizes that membrane sealing is typically a major problem and have developed an approach to overcome this problem. Their data tends to indicate that seal leakage is not an issue with the current design.
- The project approach is based on a palladium-gold composite membrane that has demonstrated high hydrogen permeance with very high separation factors at 400°C.
- The high hydrogen flux implies that a greatly reduced active surface area can be used, which, in turn, lowers the cost of the noble metals required.
- There was no discussion of a duty cycle under which the durability testing will be conducted (as listed under Future Work).
- Addresses barriers, but unclear how costs and performance will be improved.
- Pd membranes are the only known materials that conduct hydrogen exclusively thus having the potential to produce very pure hydrogen.
- Attempting to reduce cost by developing manufacturing methods with porous supports.
- The porous stainless steel tubes appear to provide a good approach for membrane supports.
- Pall will deposit defect-free Pd-alloy membrane on porous stainless steel tubes. The deposition process for the diffusion barrier layer and the active membrane material is not disclosed (proprietary?) and therefore it is difficult to judge their approach.
- Developing a small more cost effective (capital and operating) hydrogen clean up system is relevant to the DOE objectives.
- The approach to develop a compact device is especially appealing.

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

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

- Achievement of flux goal is excellent.
- Achievement of weld joint Pd coverage (to avoid gas leakage) is excellent.
- Pall has produced small scale membranes that meet or exceed the DOE targets for flux and cost.
- They have been able to accomplish this with a minimal budget.
- Pall has been able to produce numerous membrane samples that all appear to be leak free.
- The larger scale membrane reactor is of a simple design and will be simple to assemble and test. In addition, it appears that membranes could be replaced with a minimal effort.
- High hydrogen permeance with high separation factors has been achieved.
- Thin membranes exceeding the hydrogen flux targets for 2005, 2010, and 2015 have been tested.
- Membrane / module fabrication techniques have been developed.
- Reproducibility of test data has been confirmed.
- Prepared tubes membrane tubes of 3/8-in diameter and 2-in long.
- Membranes as thin as 1 micron have been tested, though not at sizes above.
- Good progress on reducing the thickness of the membrane.
- High flux rates were achieved using measurements made under ideal conditions, however measurements were not made using gas streams typical of reformer gas. Effects of trace impurities on performance of membranes were not studied.
- The yield is very interesting.
- It is hoped the additional work is conducted to reduce the operating pressure down into the 100-300 psi range to relax the reformer operating cross pressures at temperature.
- It is hoped that the capital cost will be reduced or a projection thereof. The CAPEX looks to be currently ~\$2000/kg H₂.

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

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

- Good partnership including academics, industry and a national lab. Pall is a commercial developer of other membrane systems and appears to be committed to further developing this technology. Pall is very open with the data and information generated and even if this project is not successful, the data will be of significant value to future researchers.
- The project team includes Pall Corporation, Chevron, Colorado School of Mines, and Oak Ridge National Laboratory as active participants and testing is conducted at different project sites.
- Communication appears to be lacking.
- Lacks consultation with a supplier for the stainless steel support tubes regarding cost or manufacturability issues.

Question 5: Approach to and relevance of proposed future research

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

- Future plans including the development of larger scale membrane modules are based on larger scale modules that have been used commercially in the past, and there is a high probability that this approach will also be successful here.
- More realistic and variable gas feeds (reformate) are necessary and will be tested. For example, the membrane needs to be tested with a gas stream containing contaminants found in commercial natural gas. Currently, the data is being obtained from pure (or nearly pure) feeds which will provide some basic flux data. Mixed feed transport may be significantly different.
- Reformate should including trace species some of which may poison the membrane, before conducting optimization.
- Future work includes testing with synthetic reformate to establish operating conditions needed to achieve target performance.
- The economic analysis strategy outlined is comprehensive and should enable sound comparisons with alternative technologies, such as PSA.
- Material issues should be expanded to include more substrates.
- Longer term stability and performance.
- Planned long-term durability testing at temperature will be important.
- It is good that an economic analysis is planned as part of "future work" to estimate the cost of hydrogen production from this system.
- Other than durability testing and optimization of substrate and alloy properties, detailed future plans are lacking.
- Stated future research is rational and appropriate.

Strengths and weaknesses

Strengths

- A reasonable test project with well defined tasks. The work plan is logical and the work is progressing nicely. The project is constructed so it should be completed in a reasonable time frame.
- Good teamwork with active participants.
- Good progress towards developing gas separation modules.
- High purity.
- Good progress on flux and membrane dimensions.
- Maintaining focus on manufacturability.
- Fabrication capability.
- Pall's experience in membranes area.
- Pall's long-term relationship with ORNL.
- The yield values are very good.
- The foot print size appears to be suitable for distributed generation applications.

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Weaknesses

- Unclear how the system is viable with only a 20-40 psi trans-membrane pressure differential.
- Hydrogen recovery data is not obvious. Unclear how high recovery is possible if only a <40 psi delta P.
- The work has only looked at ideal gas transport at this point.
- The work has not considered the effect of impurities in the feed.
- An economic analysis, even if preliminary, would have been helpful to provide a first-cut at the potential costs of the proposed technology.
- It would have been useful to demonstrate that the complete membrane-tube subassembly can undergo repeated thermal cycling without degradation or failure.
- Project proposes to use conventional manufacturing, has limited capabilities.
- No innovation on architecture.
- No tests yet on sulfur tolerance.
- Based on the process concept, it is unclear what the estimate is for the cost of hydrogen at 300 psi.
- No data on performance of Pd-alloy membrane using reformer gas stream.
- Unclear what will be the effect of moisture on the membranes.
- Operating pressures are a little high for SMR materials.
- Clean-up values based on Nitrogen. The effects on the likely composition of a Reformed Natural gas reformat are not discussed.

Specific recommendations and additions or deletions to the work scope

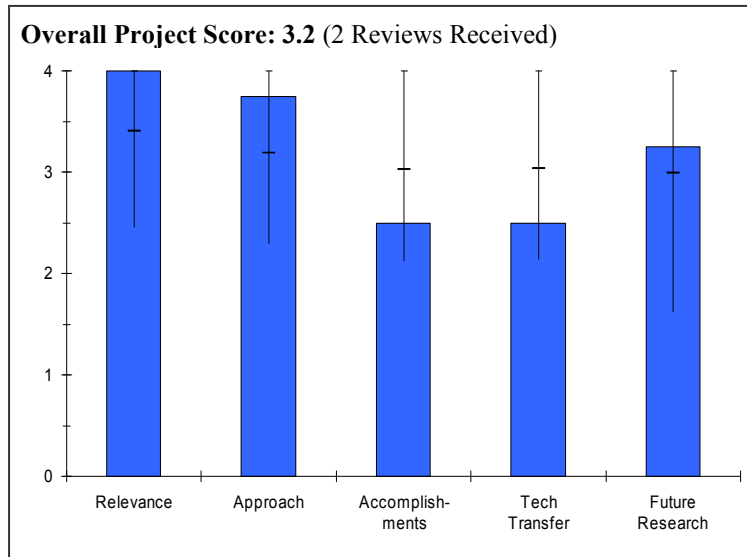
- The project is progressing nicely and needs no modifications.
- Risk assessment (is recommended) to achieve market goals.
- Economic analysis needed.
- Contaminant tolerance should be tested.
- Study the performance of the membranes in reformer gas streams (with trace impurities of sulfur, moisture, etc.).
- Evaluate the hardware for N₂, Ar, He and other trace materials that would be expected from reformed natural gas. Guidance on trace impurities is contained in the GRI Report 94/02432.2.
- Compare the results to the current thoughts on the quality needs for the vehicle OEMs. The current thinking is in a Technical Information Report Number J2719 published in 2005 by SAE. This report will be revised as additional data from fuel providers and OEMs becomes available.

Project # PDP-16: Advanced Alkaline Electrolysis

Richard Bourgeois; GE Global Res.

Brief Summary of Project

The overall objective of this project is to study the feasibility of using alkaline electrolysis technology with current-generation nuclear power for large-scale hydrogen production. Objectives are to 1) conduct a market study of existing industrial H₂ users to determine economic feasibility; 2) develop a pressurized low-cost electrolyzer to determine technical feasibility; 3) demonstrate the electrolyzer on a small scale, 4) create a design for a large scale system and 5) conduct an environmental and regulatory impact assessment to address codes and safety concerns.



Question 1: Relevance to overall DOE objectives

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

- The low cost electrolyzer technology being developed will be applicable to any electric energy source.
- Low cost electrolyzer technology is crucial to the viability of hydrogen from renewables.
- Electrolysis is one of the two most viable options for distributed hydrogen production in the near term. The problem is capital cost and cost of electricity. This project is most heavily focused on lowering the capital cost through use of GE plastics and advanced manufacturing.

Question 2: Approach to performing the research and development

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

- Emphasis now is on low cost manufacturing of cell stacks. The balance of plant system costs have not yet been critically examined, but will be addressed in a later phase.
- Good approach to examining existing hydrogen markets to identify potential electrolytic hydrogen customers. This could enable the development of distributed electrolysis system technologies while hydrogen demand from the transportation sector evolves.
- GE plastics are used to make individual cells with weld prep by injection molding. The individual cells are then manually combined to make the electrolyzer stack.
- No one has ever made an electrolyzer out of plastics. This necessitates a study of materials degradation which is being pursued in this project
- Accelerated testing is being done using higher pressures with a 10 year stack life as the goal.
- Part count reduction is big part of this project and a key to reducing stack cost to under \$100/kW goal.
- Match to nuclear will possibly utilize low cost electricity.

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

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

- Stack costs are estimated to be \$100/kW based on price quotes for all materials. A complete operating stack has not yet been built and tested.

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- Balance of plant system costs have not yet been critically examined, but will be addressed in a later phase. They are currently estimated to be about \$300/kW. Thus, the total system cost is expected to be right at the DOE target of \$400/kW.
- Significant progress has been made on defining market and requirements.

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

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

- Most of the work appears to be GE's alone, not surprising because of the intellectual property involved.
- Involvement with Entergy is focused on NRC siting qualification and access to hydrogen customers.
- Good set of partners on this work that effectively leverages expertise.
- Proprietary information will hamper dissemination of information.

Question 5: Approach to and relevance of proposed future research

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

- GE is taking steps to scale their technology up to 1MW+ sized systems. Reducing the relative costs of power electronics and balance of plant systems is an important step they are taking.
- It is likely that because of the plastic architecture that only modest electrochemical compression can be accomplished with this technology. This may mandate that the cost of external compression be reduced in order to make the technology economically viable.
- Future work includes building and testing a 10-cell stack in 2007, followed by conceptual design of reference plants in 2008.

Strengths and weaknesses

Strengths

- Development of technology that has the possibility for larger scale electrolytic hydrogen production today that is not dependent on the evolution of the hydrogen transportation market, but at the same time allows the development of the necessary infrastructure should the transportation market demand accelerate.
- Technology potentially amenable to mass manufacturing and low part count, reducing stack cost.

Weaknesses

- They have not built and tested devices to prove their thinking, but intend to do so in the next year.
- It will be difficult to pressurize hydrogen electrochemically to a useful pressure thus eliminating external compression requirements.
- Plastic degradation in alkaline electrolyte is potentially a problem that must be resolved.

Specific recommendations and additions or deletions to the work scope

- Evaluate potential safety issues resulting from degradation of plastic materials used in construction.

Project # PDP-17: EVermont Renewable Hydrogen Production and Transportation Fueling System

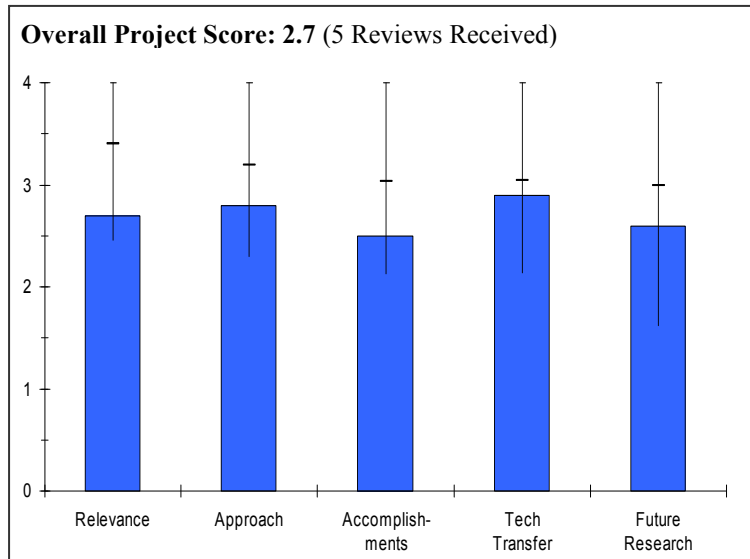
Harold Garabedian; EVermont, Inc.

Brief Summary of Project

The overall objective of this project is to develop and test advanced proton exchange membrane (PEM) electrolysis fueling station technology. The objectives for this project were to 1) complete integrated system tests in-house, 2) build a public hydrogen refueling station, 3) procure a hydrogen-fueled vehicle, and 4) monitor the performance of the refueling site and hydrogen fueled vehicle.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.7** for its relevance to DOE objectives.



- Any experience gained with electrolytic hydrogen production and vehicle fueling will provide useful information for future distributed hydrogen transportation applications.
- Linkage to renewable energy is weak since they simply take power off the grid whenever they need it, independent of whether or not the wind is blowing, and they purchase Renewable Energy Credits to offset any emissions that might be created from the grid sources.
- The key objective appears to be testing new electrolysis stack technology under cold weather conditions with a secondary emphasis on production of electricity to the grid for green energy credits.
- Demonstration of refueling station in cold climate is important for implementation of the hydrogen economy.
- This project installed a PEM electrolysis system as a demonstration unit, but did not produce publicly available data that will help in achieving the DOE goal of reducing the cost of distributed production of hydrogen from distributed electrolysis at the pump.
- The vehicle tested was a hydrogen ICE, so is not applicable to the program's goal of developing fuel cell vehicle technology.

Question 2: Approach to performing the research and development

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

- Seem to have a reasonable grasp of the technical barriers as they apply to the very small scale.
- Since hydrogen is only produced when they need to fill the tank of their one vehicle, not much is learned from stressing the system. They essentially turn the system off until just prior to a refueling event.
- Location provides good opportunity for cold weather durability testing.
- Only one vehicle fuels at the station a couple of times per week so difficult to test full reliability of components.
- The inexperience with wind turbine technology leads to suboptimal location of the turbine and wind capacity.
- Project uses a Proton Hogan 240 electrolyzer for hydrogen production to demonstrate distributed hydrogen production and refueling of a Prius ICE in a cold climate.
- This is an applied project: there is no stated objective of new materials or design development.
- Operation of the unit in a cold weather environment could provide useful information, but no data is presented on how the problems were addressed by Proton Energy.

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

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

PRODUCTION AND DELIVERY

- They built a refueling station, which has been done before.
- There was no discussion of the barriers they were supposed to address, no discussion of costs, efficiencies or lessons learned.
- Seem to be meeting project milestones and addressing cold weather issues as applicable to their system.
- Improved electrolyzer for cold weather stability.
- Learnings applied to future electrolyzer and dispenser design.
- Refueling station opened in July 2006. Hydrogen is generated at 150 psi, with compression to 5000 psi for refueling.
- The PI claims that "many enhancements" were incorporated into the electrolyzer as a result of lessons learned resulting in improved cold temperature operation, more efficient Advanced Cell Stack, improved power conservation, and easier field installation. No specific data were provided to support these claims. It is difficult to assess the true accomplishments of this project and what unique information is offered to the field.

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

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

- They did provide feed back to their suppliers to improve the balance of plant components.
- Good partnerships have permitted systems to be integrated fairly well.
- Feedback to Proton on performance of the Hogan electrolyzer is valuable from a system design standpoint.
- No collaboration or information sharing on technical advances beyond the electrolyzer manufacturer that participated in the project.
- Good collaboration with local authorities and electric utility for system siting, permitting, construction and operation.

Question 5: Approach to and relevance of proposed future research

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

- The future plans are minimal. They have just built the refueling station, now what?
- Little remains in their project work scope.
- Performance monitoring and testing will continue.
- The project is scheduled for completion in September 2007 and future work only includes "testing, monitoring and analysis". To the degree possible, the PI and subcontractors should include in their final report data on problems encountered and how they were addressed, including more specifics on electrolyzer performance improvements.

Strengths and weaknesses

Strengths

- They have worked through the permitting and built an operating hydrogen refueling station.
- They interacted well with their partners.
- Integrated distributed electrolysis fueling system and hydrogen vehicle facilitates holistic learning.
- Project has a desirable public education feature-lots of citizens have visited and learned more about hydrogen.
- Feedback to Proton on electrolyzer design.
- Lessons learned on siting and permitting also valuable.

Weaknesses

- There was no discussion of cost, efficiencies, or other barriers that the DOE needs to have addressed.
- Scale and limited system performance requirements do not really test expected real world system demands.
- Linkage to renewable energy is weak since they simply take power off the grid whenever they need it, independent of whether or not the wind is blowing, and they purchase Renewable Energy Credits to offset any emissions that might be created from the grid sources.
- Not much novelty intrinsic to project.

Specific recommendations and additions or deletions to the work scope

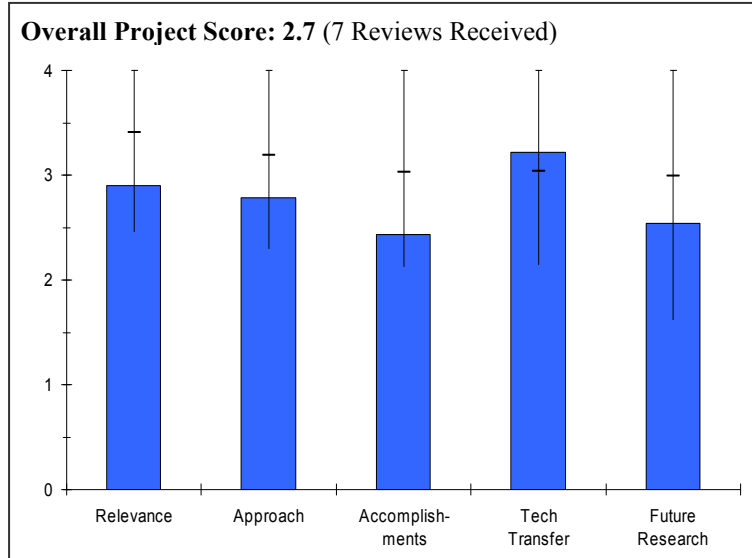
- They should include a "lessons learned" report to the DOE which identifies areas in getting permits, balance of plant, and other lessons.
- They should be able to discuss the costs and validate the cost targets.

Project # PDP-19: Hydrogen Regional Infrastructure Program in Pennsylvania

David Moyer; Concurrent Tech. Corp

Brief Summary of Project

This is a large multi-year project that includes efforts on hydrogen pipelines, off-board hydrogen storage tanks, hydrogen separations and purification, hydrogen sensors, and hydrogen production and delivery scenario analyses. The current production and delivery analyses objectives of this project are to 1) analyze tradeoffs between alternative H₂ production and delivery approaches using commercial and near-commercial options; 2) evaluate economic delivery scenarios for the I-95 Corridor and assess the feasibility of hydrogen infrastructure along the I-95 Corridor; and 3) determine Pennsylvania’s economic delivery scenarios using regional cost of indigenous energy resources (i.e., coal, landfill methane, biofuels, wind, water, municipal waste, anaerobic digestion and nuclear) using the DOE H2A Production and Delivery Models.



Question 1: Relevance to overall DOE objectives

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

- Hydrogen sensor, separation, and storage are applicable to the DOE hydrogen plan.
- Addresses DOE program goals of overcoming infrastructure barriers for hydrogen as a transportation fuel. Encompasses range of topics including local infrastructure model for I-95 corridor, hydrogen embrittlement for pipeline steels, hydrogen storage tanks for retail stations, hydrogen separations, and hydrogen sensors.
- Focused mostly on getting info on PA.
- Science already in other program projects.
- The project is divided into several sub elements, most of which have good relevance to the overall DOE objectives.
- The work on sensors is more of vendor evaluation then technology development – does not help much in progressing the hydrogen program.
- Further quantification is needed for the goals and deliverables of each subtask within this project.
- The project has three components: i) the Pennsylvania hydrogen delivery study. This component has reached a level of development that is noteworthy and should definitely be pursued further; ii) materials testing in the presence of hydrogen. This component is at an initial stage; and iii) hydrogen sensors. Definitely all three components are relevant to the hydrogen economy and support the President's initiative and the DOE R&D objectives. In particular, I would rate component (i) with a solid 4.
- This is a broad project that addresses several features of the hydrogen delivery infrastructure, and will produce some generally useful data in this regard.
- The analysis of hydrogen production and delivery options in Pennsylvania provides a useful analytical methodology for incorporating more regional analysis capabilities into the DOE's production and delivery modeling efforts.
- Overall cost of the project to DOE is high; continuing efforts are needed to make this work broadly applicable to the delivery program.
- Project consists of four parts:
 - 1) Pennsylvania infrastructure: results of little generalization ability to remainder of the country
 - 2) Pipeline and vessel materials: similar to other DOE projects

- 3) Separation of H₂ transported by CH₄ / H₂ mixture: PSA development
- 4) H₂ sensor development: DOE H₂ Production Tech Team (HPTT) recommended focus on cost of sensors being highest sensor priority, but project focuses on reliability.
- Parts lack strong focus on highest priority, core goals in MYPP.

Question 2: Approach to performing the research and development

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

- The work seems redundant to other work being done.
- The hydrogen separation and storage work does not seem original.
- Validation of the H2A model is useful.
- This project is really a potpourri of five smaller projects.
- The hydrogen infrastructure study for PA is interesting, but I am not sure what it tells me. It proposes to analyze the costs and energy sources for hydrogen production and delivery to serve the I-95 corridor, but that presupposes that a retail net limited to this corridor would generate a strong customer base for hydrogen fuel cell vehicles. That assumption needs to be tested—a market research assessment of driver comfort with such a limited infrastructure should precede the work reported here.
- The embrittlement study is fine as far as it goes...but would like to see more work on materials research to address the strain/embrittlement issue for the Heat Affected Zone (HAZ).
- H₂ separations: it's not clear how this fits in with the other projects. Also, private industry is putting a lot of effort into hydrogen PSA technology, and it is not clear to this reviewer why the DOE has to support research in this area.
- H₂ Sensors: I understand importance of this work, but it seems to me that the studies reported here should be the responsibility of developers of hydrogen sensors as part of the qualification of their products.
- Reasonable approach – segmenting into 3 parts
- Looked only at politically correct feedstocks – mainly coal.
- Approach taken by the pipeline and storage subtask is good – involves providing good direction to the collaborators and selecting the right set of tests. Similar implementation is lacking for the infrastructure study and separations & sensors subtask.
- The infrastructure study subtask will do good to seek the industry input on expected timelines for expected FCV and station rollout scenarios.
- Application of the H2A Production and Delivery Models in Pennsylvania is a unique effort addressing the barriers that a multiple urban and county region setting poses on hydrogen delivery. The approach of limited resources and transportation of the energy carrier from the western part of the State to the metropolitan centers at the eastern part is very pragmatic and addresses real world scenarios. Also the implementation of the current coal cost to the study is extremely relevant since it corrects optimistic scenarios for natural gas feedstocks involved in the H2A approach. Lastly, the study of the refueling locations along the I-95 corridor will provide a great tool for attacking the problem of implementing the hydrogen economy in a way friendly to the public. The results of this project will provide a valuable tool to the DOE on the implementation of the hydrogen economy in a way that addresses specific regional and state demands.
- Testing of materials and pipeline components against hydrogen failure and identifying the sources of failure is a critical step toward achieving hydrogen material compatibility. The work carried out at SRNL is of good quality and should continue. An interesting part of the work is the testing of the Composite Over wrapped Pressure Vessel (COPV). Burst and fatigue testing is the right approach to validate the viability of these composite structures (aluminum liner wrapped with carbon fibers) intended to be used for hydrogen storage.
- Certainly hydrogen sensors are a vital part of the hydrogen project. It seems that the work has identified hydrogen contamination as a serious source of errors in hydrogen sensing and suggested sensor A and C designs (after they were modified by the manufacturer) as possible ways to improve resistance against degradation. The issue of sensors is an important one, but it is not clear what the overall approach is. For instance, why were sensor types A, B, and C the ones tested and not another sensor, say, D?
- Good to see that the task on developing Type III COPVs for off-board storage will be focused on meeting DOE cost goals.
- PI of the Sensor task should ensure that the project is consistent with the hydrogen quality guidelines being developed by SAE.

PRODUCTION AND DELIVERY

- Research focuses on PSA adsorbents and sensor reliability, when refinery PSA operators' priorities are valve reliability and fueling station developers are requesting cheaper sensors.

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

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

- Did not show significant accomplishments to overcome the barriers that were to be addressed.
- For money/budget results could be better. Why are sensors, adsorbents, and burst pressure of storage vessels key challenges for H₂ in PA?
- The progress is slow and very little was accomplished in the area of infrastructure study and RPSA development.
- The accomplishment in the sensors field was to develop a more S tolerant sensor, which can be useful.
- Have revised the goals for the storage subtasks to off-board storage with focus on cost reduction. The goal is quite relevant to current needs, however late realization in terms of revising the goal.
- Good progress on the pipeline subtasks, with the right choice of test matrix.
- Progress in component (i) (The Pennsylvania hydrogen delivery study) is outstanding. The project clearly identified key issues with the H2A approach and expanded on their use and applicability. The analysis of the delivered hydrogen cost as it relates to the increase in the coal feedstock price is a successful one. Similarly the hydrogen delivery cost for 1% demand along the I-95 corridor is a significant one. Again, I rate the accomplishments of this component with a solid 4.
- Progress on materials testing in the presence of hydrogen is summarized by the stress-strain curves of HAZ and weld metal materials, as well as by the fabrication and testing of the COPV. In particular, the fact that the HAZ exhibits a smaller ductility in the presence of hydrogen relative to the base metal and welds is a good technical result.
- Progress on sensor testing can be summarized by the slide titled "Modified Sensor Test Results." A good description of the sensing capabilities of designs A and C.
- Appreciate that an initial cost estimate was provided for the COPV tank.
- Given the high level of funding authorized for this effort, especially relative to the size of the total Delivery program budget, the overall accomplishments are not especially significant.

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

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

- There was good interaction with sensor companies.
- The coordination between various efforts on the codes and standards was good.
- Close collaboration of Concurrent Technologies with wide range of collaborators is apparent.
- Many collaborators but not clear how much they are learning from other programs — for example, separations program looks identical to other projects funded by the Program or work going on at PSA vendors.
- Good collaboration between diverse team members and different sub elements.
- Technology Transfer is the highlight of this project as it involves directly engaging the vendors who will later commercialize the technology.
- The Pennsylvania study component is interfacing with the developers of the H2A tool and contributes significantly to its (H2A) applicability both at a state and intra-state level. The planned interactions with the stakeholders is a good approach as it will allow for real-world input and at the same time the public will be given an opportunity to be educated on the structure of the hydrogen economy and its impact on cities, counties, and states.
- The hydrogen-materials testing is interfacing with ASME. The project can become a valuable source of information to the effort for the development of codes and standards.
- It seems that the sensor-component of the project is interacting with sensor manufacturers. However, the extent of this interaction is not clear. Also it is not clear what the importance of this interaction to the viability of the project is. Is the project providing the manufacturers with key new ideas or is it just testing and modifying existing technologies?

- Good collaboration with Pipeline Working Group (including ASME and SRNL) on pipeline embrittlement tasks.
- Broad group of academic, institutional and industrial partners.

Question 5: Approach to and relevance of proposed future research

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

- They need more definition on how they are going to overcome the final barriers that they identified.
- As noted above, future infrastructure work should begin with a market survey study to define consumer expectations for availability of fueling stations. Recent studies I have seen suggest auto buyers want to see fuels available at 40% of the retail stations before they will purchase a vehicle that requires a new fuel. UC Davis studies say the figure is 10%. The discrepancy needs to be resolved before we assume the 10% figure is valid and forms the basis for an infrastructure modeling study.
- Materials for pipelines: put more focus on metallurgy and new materials concepts to address embrittlement in HAZ
- Composite H₂ storage vessels: Lots of work on this being done by private sector companies. Not clear why the DOE should be supporting work that competes with these efforts. Are there really novel approaches in the DOE funded programs that promise significant advantages over the current commercial approaches? I didn't see them in the poster.
- Rapid cycle PSA. Air Products and Questair are working on this for other applications. Not clear to me why this needs DOE funds to ensure new concepts and materials are being developed.
- Proposed future research is adequate to meet the said objectives of the project.
- Articulation of specific targets for individual elements of the project will help in assessing the merits of proposed future research.
- Again, the Pennsylvania hydrogen delivery study component has a well thought out plan for future research. In particular the consideration to meet with stakeholders for possible input seems to be an approach in the right direction. This input is extremely important to both the Pennsylvania energy options and the establishment of the hydrogen economy along the I-95 corridor.
- Proposed research on off-board hydrogen storage is in line with DOE goals. Mechanical testing of material components of COPV is needed to ascertain possible ways of improvement of the COPV capabilities. However, it is not clear what the proposed serviceability modeling of the COPV entails. There is no information to judge the objectives and potentials of this modeling.
- The proposed identification of emerging sensor technologies from universities and national laboratories is a proper task but seems too broad. Perhaps some metrics that would assist this identification ought to have been stated. Also the design and construction of the intrinsically safe package to contain safety hydrogen leak sensor system is a rather vague one. A few details on how one can achieve this objective would help elucidate whether the proposers are moving in the right directions toward such a design.
- Significant cost reductions are needed for the COPV tank in order to meet DOE cost goals, and potential pathways for achieving the cost reductions should be described.
- Continue to seek ways to add value to the delivery program and provide results that have broad relevance and impact.
- Future sensor work addresses packaging and contamination issues, not cost.
- PSA work does not address reliability issues with PSA's, especially valves that are a concern for pipeline situations where reliability is critical.

Strengths and weaknesses

Strengths

- Good team interactions.
- The progress has improved over previous years.
- Good relevance to DOE's technical and portfolio goals.
- Directly working with vendors who will potentially commercialize the technology.

PRODUCTION AND DELIVERY

- The Pennsylvania hydrogen delivery studies are indeed a significant effort that needs to be supported and encouraged to continue. The poster presenter, Eileen Schuma, did an excellent job in pointing out the strengths of the project and its potential impact on the I-95 region and its environment.
- The collaboration with the SRNL on materials testing is a good approach since SRNL has good capabilities of carrying out high quality work on hydrogen-induced degradation. The COPV project is a promising technology that should be supported and further explored.
- The people behind the project: Eileen Schuma has a continued and successful participation in the Pennsylvania delivery component; I do not know for how long David Moyer is involved in the project, but it seems that he understands well the overall project directions.

Weaknesses

- The model validation is for a small area of the US. They should have included more than just the I-95 corridor.
- The separation work is not innovative.
- As an earmarked program, not clear how much collaborating they are doing with other programs.
- There are several subtasks/projects with little or no relevance to each other. They can probably be separated into different projects for better assessment.
- Materials testing should become more focused and coordinated. Perhaps a close collaboration with ASME will help identify the types of tests required to increase our understanding on material failure, and hence help in the direction of establishing a design methodology.
- Regarding the sensor component, I may say that the poster did not provide enough information. For instance, it is not clear what the underlying science for sensor reliability is and how the palladium degradation can be avoided. It seems that much more work is needed in this direction or else this component of the project runs the risk of being typecast as one in which sensors are tested randomly and the ones performing better are selected.

Specific recommendations and additions or deletions to the work scope

- They need to show how their infrastructure analysis applies to other parts of the country.
- They need to compare their PSA results with DOE targets and with other work in the area.
- The work on sensors should be eliminated. It has little or no relevance to other project elements and is something that a vendor might be able to sort out himself.
- For the materials testing, sharing of the results with the rest of the members of the pipeline working group is an efficient approach toward establishing a scientific exchange and perhaps a better coordination on identifying new critical tests that need to be conducted.

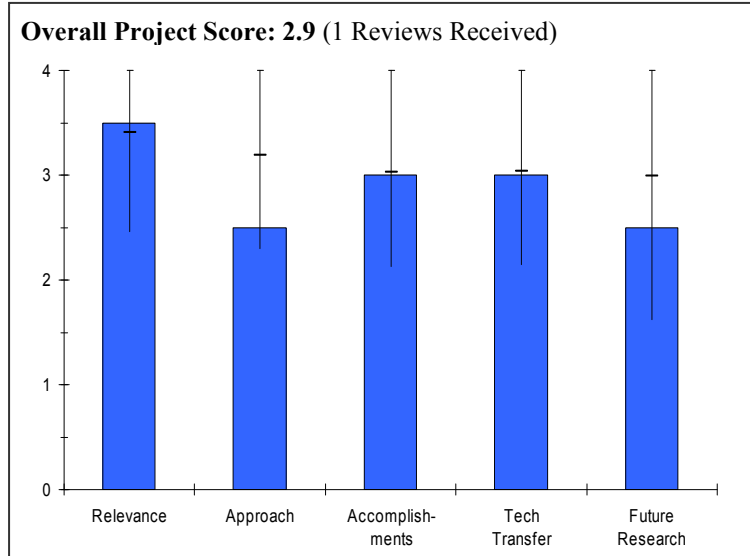
Project # PDP-23: Evaluation of Alternative Thermochemical Cycles

Michele Lewis; ANL

Brief Summary of Project

The objective of this project is to balance the temperature portfolio of nuclear heating sources with thermochemical cycles for H₂ generation, using the Gen IV Energy Conversion Program for electrical generation and the Nuclear Hydrogen Initiative (NHI) for hydrogen production. The approach will 1) identify promising cycles from the literature with various maximum temperatures to match heat output from different nuclear reactors; 2) invite university participation to evaluate cycles using consistent methodology – universities include Clemson, Howard, Massachusetts Institute of Technology, Pennsylvania State University, Rensselaer Polytechnic Institute, Tulane, University of South Carolina, University of Illinois-Chicago; 3) determine critical research and development (R&D) needs or recommend no further work; and 4) down select 1 or 2 of the most promising cycles for further R&D. The NHI methodology consists of 3 levels of evaluation:

- Level 1 based on stoichiometric reactions;
- Level 2 based on equilibrium considerations;
- Level 3 based on ‘real’ chemistry to the extent it is known;
- Pinch analysis used for heat management in all levels.



The NHI methodology consists of 3 levels of evaluation:

Question 1: Relevance to overall DOE objectives

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

- Project builds on prior research and is focused on identifying alternative thermochemical processes.

Question 2: Approach to performing the research and development

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

- Uniform evaluation of processes is a good approach.
- Laboratories involved in the project should review each other's work and validate results to verify as appropriate and feasible the most promising and/or most difficult processes.
- A basic cost/benefit analysis (e.g., basic H₂A cost analysis or scoring method) for evaluating the benefit of processes that create less challenging environments (e.g., are less corrosive), require fewer unit operations, or less complex process equipment should be included in the evaluation of the processes.

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

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

- Not all processes have been evaluated at the same level of technology maturity, which may impact final evaluation

PRODUCTION AND DELIVERY

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

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

- Very good use of university collaborations.
- Industry expert input into the potential materials, manufacturing, or other implementation problems would be very helpful.

Question 5: Approach to and relevance of proposed future research

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

- All processes should be brought, as nearly as possible, to the same level of experimental (laboratory scale) development prior to selecting most promising candidates.
- Considerations other than efficiency should be included in the down-select of promising processes.

Strengths and weaknesses

Strengths

- Excellent use of university talent.

Weaknesses

- Need to consider additional attributes of processes to fully address DOE needs.

Specific recommendations and additions or deletions to the work scope

- Add cost/benefit analysis.

Project # PDP-24: UNLV High Temperature Heat Exchanger Development

Tony Hechanova; UNLV

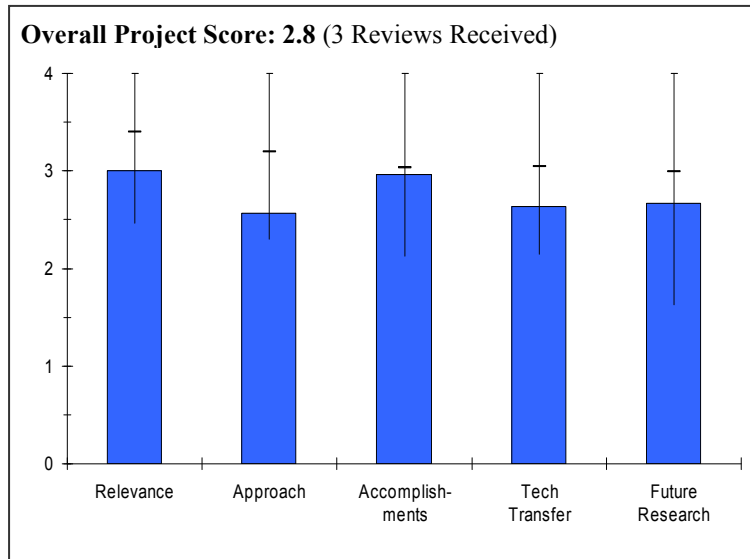
Brief Summary of Project

The objective of this project is to assist DOE-NE in the development of hydrogen production from nuclear energy through:

- Identification and testing of candidate materials and coolants for heat exchanger components.
- Design of critical components in the interface and sulfur iodine thermochemical process.
- Fabrication and testing of prototypical components.
- Innovative materials development.

Question 1: Relevance to overall DOE objectives

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



- This project is funded through the Nuclear Hydrogen Initiative. The objective of this project is to assist DOE-NE in the development of hydrogen production from nuclear energy through identification and testing of candidate materials and coolants for heat exchanger components and for thermochemical reactions.
- Project addresses key issues in the Nuclear Hydrogen Initiative, but closer coordination with specific needs of process and equipment developers should be pursued.
- This poster covered nine individual tasks of relevance to the Nuclear Hydrogen Initiative. As such it covered a range of topics, each quite briefly.
- It is apparent that the researchers have worked with the rest of the NHI to make their individual tasks as relevant as possible to the on-going work.

Question 2: Approach to performing the research and development

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

- The objective of this project is to evaluate the performance of large number of materials for production of hydrogen from nuclear sources. Several team members are involved in this project and it appears each one is doing research on their own.
- Several different experiments are used in this project.
- This project consists of several mini-projects and there is no connection between them.
- The work is focused on the barriers to the implementation of the thermochemical hydrogen production cycles, including corrosion, heat transfer in the heat exchanger and the development of catalytic materials.
- The sensitivity shown for Task 5 was quite impressive, but it is unclear how that work will be applied to the issues of electrolytic cells. The listed accomplishments state the measurements that were done, but don't draw any inferences or conclusions.
- A tighter program management system with better-defined objectives and milestones for each task is required. If this exists, it was not apparent from the information presented.

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

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

- Corrosion and crack growth studies were carried out.

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- Properties of materials were studied after exposure to acidic conditions.
- Candidate materials were examined for HI decomposition.
- Some mechanical property measurements were carried out.
- There is a wide range of progress among the nine tasks. Some appear to be just getting underway, while others seem to be well advanced.
- Same good technical progress was made, but it was hard to evaluate how much progress was made toward specific goals and overall project needs.

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

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

- Partners in this project are: UNLV, UC Berkeley, MIT, General Atomics, Ceramatec, and Argonne. It is not clear if any coordination exists among the team members.
- There is evidently a good deal of collaboration between the Nuclear Hydrogen Initiative and the tasks. It is not clear whether the technology transfer is from UNLV to the other NHI participants, or from those participants to UNLV.
- Better collaboration between university researchers and process developers (industry and national labs) should be incorporated. This will help to ensure that the research addresses specific needs, such as operating conditions and other requirements.

Question 5: Approach to and relevance of proposed future research

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

- Proposed work lists lots of tasks.
- Tasks are relevant to Nuclear Hydrogen Initiative.
- Looks like this team is shooting in the dark—hoping they will hit a star!
- The scope of future work appears to be much more aggressive than the scope of work accomplished to date, even though the work has been underway since 2003. Perhaps this is more a mark of optimism about future successes than a realistic evaluation of what can be accomplished in the next year or two.
- There is not future work listed for Task 5. Will this work continue?

Strengths and weaknesses

Strengths

- Fairly big research team.
- Has significant amount of funding.
- Facilities to carry out various tasks are available.
- The tasks span a wide range of the issues confronting the development of thermochemical cycles for hydrogen production.
- The university seems to have a wide range of new equipment at its disposal for the investigation of material properties.

Weaknesses

- Lack of real collaboration among team members.
- Lack of focus.
- It is not clear if the results of the research are being given to the rest of the NHI participants in a timely manner to influence choices of materials and other issues.
- Project seems to lack a sharp focus and a good definition of specific goals and objectives.

Specific recommendations and additions or deletions to the work scope

- Define two or three problems and develop a plan of attack.
- Recommend that the tasks be structured to include work to understand and fix identified problems and not merely to take pictures of the problems.

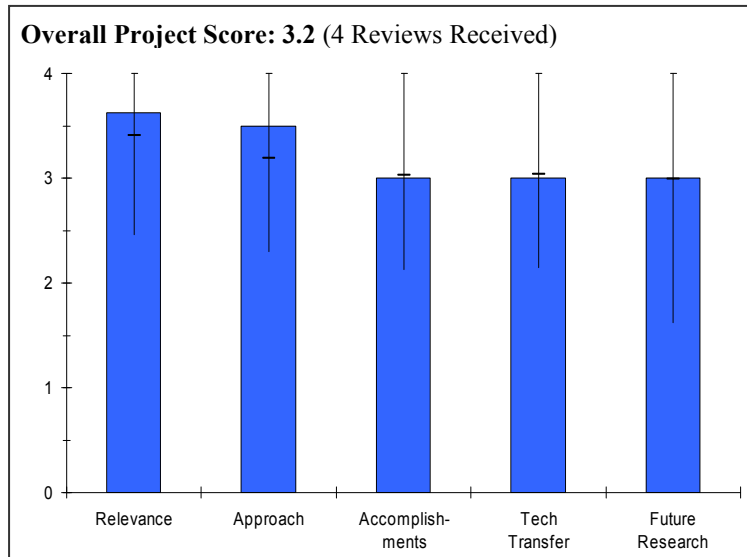
Project # PDP-26: Test of High Temperature Electrolysis Integrated Laboratory Scale Half Module

Joe Hartvigsen; Ceramatec

Brief Summary of Project

The objective of this project is to test an integrated laboratory scale (ILS) hydrogen production module. The “Half-ILS Module” test at Ceramatec will achieve the following:

- Development and testing of two 60 cell stacks in similar configuration to a full module;
- Show that performance scales with stack height;
- Assess system issues with stacks;
- Develop component production capacity (100 cells/month);
- Deliver first full ILS module to INL.



Question 1: Relevance to overall DOE objectives

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

- High temperature electrolysis is important to improving electrolytic efficiencies in large central hydrogen production facilities.
- High temperature electrolysis can be used in advanced nuclear and solar thermal applications, which are both carbon free.
- The hydrogen produced can also be used to produce carbon based fuels by hydrogenating CO₂ emissions from power plants or other CO₂ sources, and the high temperature oxygen byproduct can be used in biomass applications.
- This project is concerned with the development of high temperature steam electrolyzer (HTSE or SOEC) for use in nuclear hydrogen production.
- High temperature solid oxide electrolyzers have greater efficiencies than lower temperature electrolyzers; however, the materials issues are much greater. They match up well with nuclear.
- Good work.
- Not clear how SOEC cost compares to targeted electrolyzer cost.
- Development of high temperature electrolysis is a major option for hydrogen production by water splitting using advanced nuclear reactors. This project is an essential part of that development program.

Question 2: Approach to performing the research and development

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

- They seem to have a reasonable grasp of the technical barriers. Seals that perform well in high temperature environments seem to be one of the most significant since they limit the pressure drop across the plates. Also posing a unique barrier is the corrosive steam oxygen environment.
- SOEC is versatile-can electrolyze either water (steam) or CO.
- They have selected reasonable electrode and electrolyte materials and are steadily increasing the scale of demonstration (stack size and number of stacks).
- Greater consideration to balance of plant issues should be given.
- Very sophisticated testing.
- Too bad heat balance was not investigated earlier to avoid need for CO₂ / H₂O mix.

PRODUCTION AND DELIVERY

- Project is too focused on building stacks for testing to meet milestones. These stacks often represent older technology versus the latest design. More effort is needed on cell research and development to solve problems at the component and sub-scale level, and not just full stacks.

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

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

- Seem to be meeting project milestones and learning as they progress.
- Delivered a four-stack module in March.
- Four stacks (60 cells each) have been delivered to INEL for evaluation. Progressing to this point is a major accomplishment.
- However, in a smaller demonstration with a 2-stack system it was noted that there was 50% performance degradation in first 600 hours. They ascribed this to failure of the manifold due to corrosion, which was constructed from 440 stainless steel. Perhaps more thought should have been given to selection of balance-of-plant materials/components prior to construction.
- Good progress, but tough to differentiate from SOFC progress.
- Building and testing at larger stacks and systems is progressing very well. Duration testing (for up to 2,000 hours) is also very encouraging. However, more effort must be placed on design and operation at conditions used in the process flow sheet, particularly steam-sweep for the oxygen electrode and pressurized operation.

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

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

- Good leveraging of SOFC technology in development of the SOEC.
- Good collaborations with national laboratories.
- Not obvious from poster/discussion.
- Collaboration between Ceramtec and INL is excellent. However, involvement with other solid oxide cell developers, such as the SECA program, should be pursued.

Question 5: Approach to and relevance of proposed future research

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

- Exercise production capacity, evaluate materials and process improvements, statistical performance distributions, extended lifetime testing, cell area scale up, stack size scale up and system BOP.
- Ceramtec with their partners will conduct an evaluation of the full ILS module.
- SOEC's could be a solution. Research plan seems solid.
- Project seems to focus on building more stacks at essentially the same design. More effort needs to be directed at cell scale-up, pressurized operation, operation at flow sheet conditions, and other advanced design options.

Strengths and weaknesses

Strengths

- Leverage of SOFC technology and potential for considerable scalability.
- High temperature steam electrolyzers have high efficiency and couple well to nuclear applications.
- Similarities to SOFC's.
- Good testing capabilities.
- Project has made great progress in building stacks and testing for extended periods.

Weaknesses

- System operates at atmospheric pressure, so any compression will be a more significant cost than with other technologies. Presumably, the efficiency gains will more than offset these costs.

- Materials issues are the major problem with SOECs. Thermal cycling is an issue with regard to materials, especially interface integrity.
- Project needs more focus on fundamental and component development.

Specific recommendations and additions or deletions to the work scope

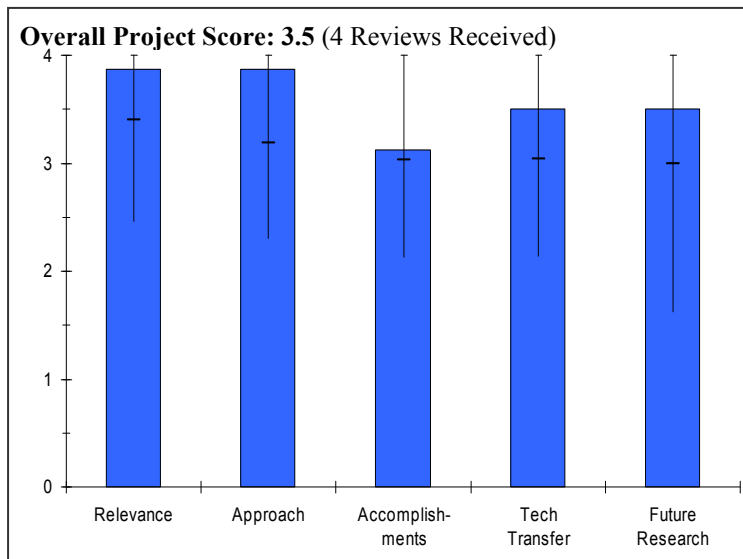
- Greater consultation on selection of balance of plant components is recommended.

Project # PDP-28: NHI Catalyst and Membrane Studies for Thermochemical Cycles at INL

Dan Ginosar; INL

Brief Summary of Project

The objectives of this project are to 1) develop enabling technologies for the sulfur-iodine (S-I) thermochemical cycle as a part of the Nuclear Hydrogen Initiative (NHI); 2) apply these technologies to the hybrid sulfur (HyS) cycle and to other non-sulfur based thermochemical cycles; and 3) have these technologies include effective catalysts for chemical conversion and membranes for chemical separations. These enabling technologies are needed to reduce the cost and increase the efficiency of the process.



Question 1: Relevance to overall DOE objectives

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

- The project supports overall DOE objectives by focusing on enabling technologies for DOE's prime candidate for thermochemical production of hydrogen.
- Research is focused on providing basic laboratory data for engineering modeling and design for integrated laboratory scale demonstration, which is necessary to meet DOE objectives.
- The sulfur/iodine cycle is still the front runner for thermo chemical water splitting using nuclear heat. Two barriers to commercialization of this cycle are the dewatering and decomposition of hydroiodic acid and the dewatering and decomposition of sulfuric acid. This project addresses both with good results.
- The project goals are critical to the Hydrogen Initiative and fully support DOE RD&D Objectives for thermochemical catalyst development for nuclear hydrogen production.
- This project is a critical part of the DOE-NE program to develop thermochemical cycles. The catalyst studies are particularly valuable. The membrane work may prove valuable as well, but it is less essential than the catalyst research.

Question 2: Approach to performing the research and development

This project was rated **3.9** on its approach.

- Membranes and catalysts were tested under expected process conditions and over a long enough period of time to detect degradation of performance.
- It is encouraging to see there are still new and innovative approaches to be tried. This is a good piece of work.
- The project subtasks are well-focused on specific technical barriers and have been selected with respect to highest potential for impact.
- The project subtasks are well-balanced with respect to different technical barriers.
- The strategies for characterization of materials are well-described and robust.
- The strategy and rationale for resolution of key barriers for catalyst or membrane uniformity in composition, durability, and performance is logical and well-described.
- The contributions and responsibilities of collaborators and partners were clearly described.
- Both the catalyst and membrane research are sharply focused. The membrane work has less well-defined objectives which should be delineated..

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

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

- Development of the project laboratory apparatus for testing in very harsh conditions required significant effort.
- Time constraints limited testing of additional membranes and more stable catalysts.
- Cause for deactivation of Pt catalyst has not been fully identified.
- The catalyst research does not appear to be complete enough to fully support the down select decision.
- The investigators have made significant progress. However, much work is still needed to address the durability of the sulfuric acid decomposition catalyst.
- The selection or derivation of specific milestones and performance indicators was difficult to discern; although, the investigators have clearly performed a great deal of work on testing of available catalysts.

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

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

- Closer collaboration with engineering team designing the ILS would provide feedback for further experimentation.
- This project at INL is well positioned to transfer technology developments to industry for scale-up and commercialization.
- There is a strong partnership and integration with other institutions and industrial enterprises.
- The complete characterization of various commercially-available catalyst materials will likely lead to opportunities for technology transfer and additional collaborations.
- Good collaboration between catalyst research and process developers, and national laboratories. Membrane research could use better interaction with potential system users.

Question 5: Approach to and relevance of proposed future research

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

- The principle investigators have identified key research needs for these projects.
- Close collaboration with the ILS design team and performance feedback from that demonstration will facilitate optimization of the process.
- The investigators have outlines a good plan forward.
- The future research is well-described with respect to the desired target properties of the respective catalyst or membrane materials and their testing in an integrated lab scale stack.
- More specific performance milestones would help to guide both the catalyst and membrane research.

Strengths and weaknesses

Strengths

- The projects are very focused on providing appropriate experimental data to support design for the ILS project.
- The experiments were conducted at or near the expected conditions for the ILS.
- Innovative approaches to problem solving are strengths.
- The investigators demonstrate clear experimental design of each subtask, and have made significant progress in the project.
- Excellent technical approach and results.

Weaknesses

- The catalyst work timing does not fully support the ILS demonstration schedule.
- The lack of a system model or other means of predicting the impact of these novel modifications on the overall system efficiency is a weakness.

PRODUCTION AND DELIVERY

- Contingencies are not described and it is not clear how the investigators will actually use information from testing on actual decomposition in the down-select process among all available materials.
- The investigators are clearly expert in empirical testing; however, a more hypothesis-driven design of novel catalysts would be a useful addition to this project, either by inclusion of a new collaborator or utilization of in-house expertise.
- The selection of materials for testing seems somewhat ad-hoc and random, perhaps leading to decreased efficiency. Since no schedule or milestones were presented, it is difficult to discern how a more focused, strategic, and mechanism-driven approach might accelerate progress.

Specific recommendations and additions or deletions to the work scope

- The project scope should be expanded to include responding to feedback and providing support after the ILS demonstration starts. For example, experimental work may be needed to optimize the materials for sealing and supporting the separation membranes.
- Collaboration with General Atomics to do process modeling would accelerate process.
- More robust computational modeling or simulation might assist the investigators in choosing future commercially-available materials for testing.

Project # PDP-30: Materials Issues and Experiments for High Temperature Electrolysis and SO₃ Electrolysis

David Carter; ANL

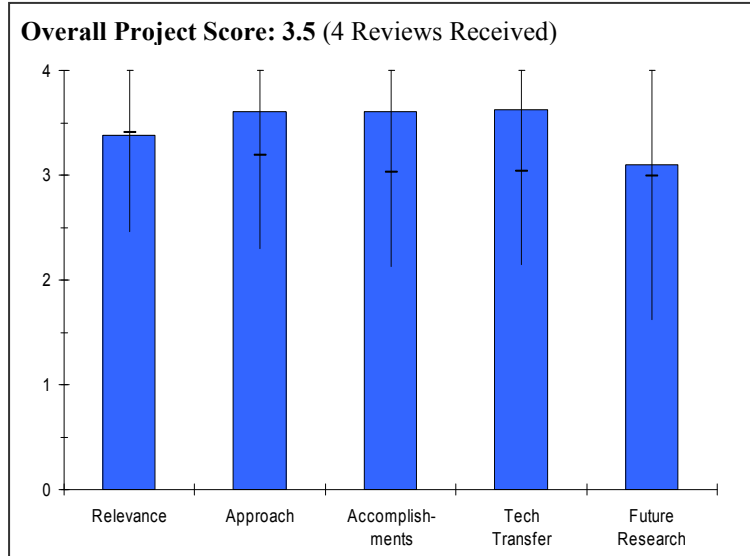
Brief Summary of Project

The objectives of this project are to 1) determine causes of degradation in stack components from 25-cell stack tested for 1,000 hrs and 22-cell stack tested for 200 hrs and 2) develop oxygen and steam-hydrogen electrodes that show significantly improved area specific resistance and durability over state-of-the-art electrodes.

Question 1: Relevance to overall DOE objectives

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

- This project is well aligned with the DOE objectives. It is addressing important issues related to high temperature water electrolysis using solid ceramic electrochemical cells. As with all electrochemical devices of this nature, performance and durability are issues that must be addressed. This project is doing well to address these issues.
- The project goals are critical to the Hydrogen Initiative and fully support DOE RD&D objectives for thermochemical electrode development for nuclear hydrogen production
- Poster presents the work of two or three groups of people working fairly independently.
- The work appears to address corrosion issues relevant to the handling of steam/hydrogen and high pressure oxygen or air, as might be encountered in the operation of a nuclear-hydrogen production plant.
- Post-test evaluations for high temperature electrolysis (NIE) are an important part of that program. Work on SO₃ electrolysis needs a better defined focus and objectives.



Question 2: Approach to performing the research and development

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

- This project has brought to bear a sophisticated array of analytical equipment on the task of understanding the performance degradation mechanisms.
- The project subtasks are well-focused on specific technical barriers and have been selected with respect to highest potential for impact.
- The project subtasks are well-balanced with respect to different technical barriers.
- The strategies for synthesis and characterization of materials are well-described and robust.
- The strategy and rationale for resolution of key barriers for electrode uniformity in composition, durability, and performance is logical and well-described.
- The contributions and responsibilities of collaborators and partners were clearly described.
- The approach seems valid in exposing samples to prototypic conditions expected in a hydrogen production plant.
- Post-test examinations on the high temperature electrolysis stack were conducted thoroughly and with valuable results. However, the results need to be timelier and more closely linked with the construction of the next generation cells and stacks.

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

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

PRODUCTION AND DELIVERY

- The project made much progress on understanding the problems causing performance decay. Now that the problems have been identified, it should be easier to find solutions.
- The selection or derivation of specific milestones and performance indicators was difficult to discern; although, progress seems to be good for this recently-funded project.
- The two ANL tasks are apparently well advanced. The INL task is just getting underway.

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

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

- This project brings together two well respected national labs that are coordinating their work to address problems associated with high temperature water and sulfur trioxide electrolysis. Material components are being supplied by a manufacturer well positioned to take advantage of the results of the study.
- There is strong partnership and integration with other institutions and industrial enterprises.
- The synthesis of various experimental electrode materials and their complete characterization will likely lead to opportunities for tech transfer and additional collaborations.
- In the first two tasks there appears to be a good deal of collaboration between the national laboratories and the industrial partner.

Question 5: Approach to and relevance of proposed future research

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

- Future plans are too generic. More specifics are needed regarding the approach and methodology to be used to select the materials to be studied.
- The future research is well-described with respect to the desired target properties of the respective electrode materials and their testing in an integrated lab scale stack.
- Recommend that the partners coordinate more closely.
- Future plans and objectives need better definition.

Strengths and weaknesses

Strengths

- Greatest strengths are the collaboration among partners and the capabilities of the analytical labs.
- The investigators demonstrate clear experimental design of each subtask, and have made significant progress in the project.
- This research may be able to identify the causes for the growth in cell resistance during operation.
- The second task may offer some understanding of the materials to be used in the balance of a hydrogen-production plant.
- The third task is an improvement of the standard sulfur-iodine cycle, but is somewhat unrelated to the other two tasks.
- Excellent technical work.

Weaknesses

- Having identified the problems, more focus is needed on the solutions.
- Contingencies are not described, and it is not clear how the investigators will actually use information from testing on actual decomposition to redirect synthetic efforts on other related electrode components.
- Need for closer coordination between the partners.
- Results need to be incorporated into the stack on a timelier basis.

Specific recommendations and additions or deletions to the work scope

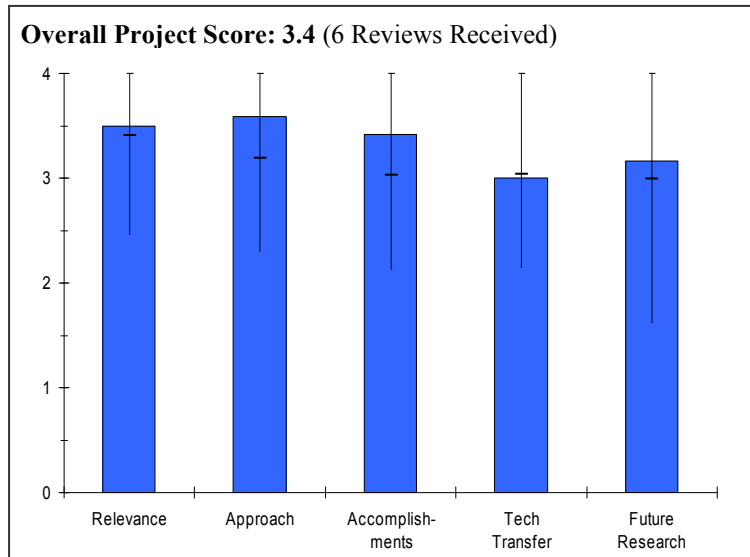
- Project scientists indicate that they would like to use XAFS as an analytical tool to sort out the chemistry. This seems reasonable to add to the project.
- Recommend that these three tasks not be grouped into one poster and thus into one review, since they deal quite different chemical environments.

Project # PDP-31: Corrosion Studies of Metallic Materials for Thermochemical Cycles

Bunsen Wong; General Atomics

Brief Summary of Project

The overall objective of this project is to develop heat exchanger construction materials for the hydrogen iodide (HI) decomposition process. The objective for 2004 to 2006 was the screening of materials candidates in HIx, HIx + H₃PO₄, concentrated H₃PO₄, and HI + I₂ + H₂ (gaseous). The objectives for 2006 to 2007 include: 1) stress corrosion and long-term testing of qualified candidates; 2) determining the effect of chemical contaminations on corrosion; and 3) testing of components with Ta cladding.



Question 1: Relevance to overall DOE objectives

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

- The project is addressing an important problem in the Sulfur-Iodine Hydrogen cycle.
- Identification of appropriate materials for construction of heat exchangers and other process equipment is critical to the DOE objectives.
- Much needed research if nuclear Sulfur-Iodine thermochemical cycle is to be a component of the H₂ economy.
- Identification of corrosion resistant materials is critical to further development.
- Project supports DOE mission for nuclear hydrogen production by performing materials evaluation for selection of heat exchanger construction materials for use in the HI process. These are critical components.
- This work is essential to the development at the Sulfur-Iodine thermochemical cycle.

Question 2: Approach to performing the research and development

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

- The project approach is focused and well laid-out.
- The experiments and experimental apparatus are well designed.
- Tests of more complete systems (e.g., loops including valves, pumps, connectors at expected process pressures) are needed.
- Tests should include HI mixture contaminated with reaction products from the previous step (sulfuric acid and products).
- Excellent approach.
- Excellent simulation of actual operating conditions.
- Weld seam testing is excellent.
- Teflon components for processes at less than 200 degrees Celsius might be a viable, lower cost alternative.
- They are completing evaluation of materials through a balanced corrosion and mechanical properties testing. Long term testing is being accelerated through use of high pressure. Following this, scaled-up prototypes will be evaluated.

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

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

PRODUCTION AND DELIVERY

- Further testing of coated components, especially with respect to fabricated equipment (e.g., at welds) is needed.
- Good progress; excellent understanding and presentation of results.
- Will be very useful to the development of the Sulfur-Iodine cycles.
- Significant progress demonstrated. However, most of the accomplishments were specified for FY06 and FY05. No accomplishments specifically identified for FY07.
- They are 85% done, but will need additional funding next fiscal year. Most of the work has been tasked to UNLV. Appropriate materials have been selected for further test.
- Materials choices have been downselected to Ta alloy coatings.
- Project has been funded for three years; however, there is significant more work in materials evaluation to be conducted. Progression to scale up might have been expected at this point.
- More life-cycle testing, including cycling and potential upset conditions would be helpful.

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

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

- The project is collaborating with UNLV for mechanical testing and industrial support.
- They are collaborating with other researchers in industry for development of the complete system.
- It was not clear from the presentation how much the project has drawn on industry experience.
- General Atomics is the natural lead in this area; collaboration with UNLV should ensure that results become useful to others in the field.
- Degree of coordination between partners appeared to be fair.
- Only mechanical testing and analytical services were identified as UNLV contribution. Only one stress corrosion test result was reported.
- Only collaborations have been with UNLV.
- Project is closely linked with process developer.

Question 5: Approach to and relevance of proposed future research

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

- The materials identification and testing program is winding down.
- The proposed longer-term testing of Ta-clad components is important.
- Project ends in September, so future work was not presented.
- Ideas for extending this work into demo units should have been proposed.
- Testing for FY06 - FY09 includes prototype testing, cost reduction studies and cross-contamination studies. All of these are important.
- In future research they will further test parts at elevated pressure.
- Ta coatings on copper and Teflon coated parts, to reduce cost, will be tested.

Strengths and weaknesses

Strengths

- The project is focused and well laid-out.
- The project has identified and tested suitable materials of construction.
- Systematic approach, proceeding from simple corrosion tests to component testing in process simulations is an effective way to screen materials.
- This was an area that needed research. Excellent approach.
- Large database of metal-corrosion test results from the 1970s at GA, Westinghouse and general literature, which was accessed.
- Balanced approach to materials testing and down-selection.

Weaknesses

- High pressure testing is needed.

- Future work is unclear. It is hoped that this effort, its results, and its follow-on don't get lost after project ends.
- The H₂O section of the SI cycle has been flow sheeted in two ways: 1) reactive distillation and 2) extraction distillation. These two methods involve different process conditions and hence different materials may be needed.
- Not a lot of interactions outside UNLV.

Specific recommendations and additions or deletions to the work scope

- N/A

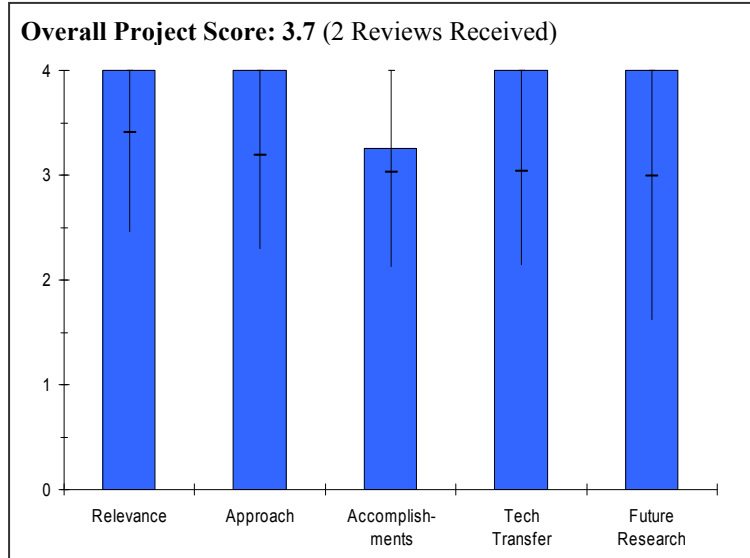
Project # PDP-32: Membrane Development for Hybrid Sulfur Electrolysis and Oxygen Separation

Mike Hickner; SNL

Brief Summary of Project

The Nuclear Hydrogen Initiative (NHI) is investigating thermochemical cycles as one of the promising methods for hydrogen production using Generation IV reactors. The sulfur-based cycles – Sulfur-Iodine and Hybrid Sulfur – are the focus of the current NHI research program. These cycles are the most technically developed of the more than 200 cycles reviewed and have the potential for high efficiencies. The ongoing work of this project includes:

- Ongoing high temperature permeation studies (Sandia National Laboratories);
- Continued structural elucidation;
- Determining the extent of corrosion during H₂SO₄ decomposition and possible mitigation steps;
- Continued membrane development (density, processing, scale-up);
- Testing on actual decomposition reactor.



Question 1: Relevance to overall DOE objectives

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

- Needs addressed are critical to NHI program. Oxygen membrane will increase yields of SO₂ and O₂ for both sulfur cycles.
- Protein exchange membrane R&D, if successful, will eliminate or mitigate SO₂ carryover and allow successful deployment of hybrid sulfur cycle.
- The project goals are critical to the Hydrogen Initiative and fully support DOE RD&D Objectives for thermochemical process development for nuclear hydrogen production.

Question 2: Approach to performing the research and development

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

- Project started with the evaluation of known ceramic membranes for this application. This gives a good baseline.
- The project subtasks are well-focused on specific technical barriers and have been selected with respect to highest potential for impact.
- The project subtasks are well-balanced with respect to different technical barriers.
- The strategies for synthesis and characterization of materials are well-described and robust.
- The strategy and rationale for the mixed ionic-electronic conductor is good.
- The contributions and responsibilities of collaborators and partners were clearly described.

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

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

- Thermogravimetric analysis data indicate promise, but also show more improvement is needed.

- Not clear what the O₂ permeation rate target is?
- Sulfonated Diels-Alder Poly(phenylene) (SDAPP) membrane performance is encouraging.
- The selection or derivation of specific milestones and performance indicators was difficult to discern, and relied substantially upon information provided by the presentation of the collaborator.

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

This project was rated **4.0** for technology transfer and collaboration.

- There is no specific mention of collaborations.
- Found acknowledgement slide that lists many collaborations.
- There is strong partnership and integration with other institutions and industrial enterprises.
- The synthesis of various experimental membranes and their complete characterization will likely lead to opportunities for tech transfer and additional collaborations.

Question 5: Approach to and relevance of proposed future research

This project was rated **4.0** for proposed future work.

- The future research is well-described with respect to the desired target properties of the respective membranes.

Strengths and weaknesses

Strengths

- Extensive experience in ceramic and proton exchange membranes. Many contributors to project with 9 authors listed.
- The investigators demonstrate clear experimental design of each subtask, and have made significant progress in the project.

Weaknesses

- No overview slide and no approach slide were presented. Presenter told me that the project represented a \$100K effort and had just started a short while ago.
- Contingencies are not described, and it is not clear how the investigators will actually use information from testing on actual decomposition to redirect synthetic efforts on other related membrane components.

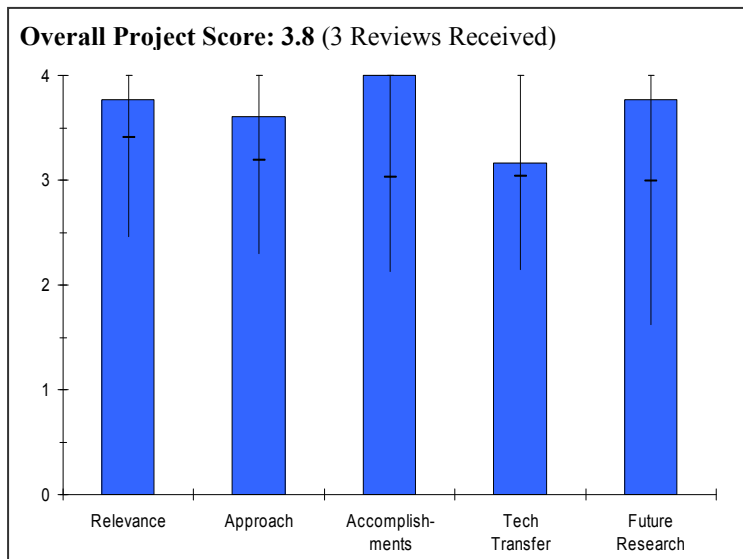
Specific recommendations and additions or deletions to the work scope

- N/A

Project # PDP-33: Maximizing Light Utilization Efficiency and Hydrogen Production in Microalgal Cultures
Tasios Melis; UC Berkeley

Brief Summary of Project

The objective of this project is to minimize the chlorophyll antenna size of photosynthesis to maximize solar conversion efficiency in green algae. First, genes that regulate the Chl antenna size in the model green alga *Chlamydomonas reinhardtii* will be identified and characterized; then, these genes will be applied to other green algae, as needed. The approach is to interfere with the molecular mechanism for the regulation of the chlorophyll antenna size by employing DNA insertional mutagenesis and high-throughput screening to isolate tagged green algae with a smaller Chl antenna size.



Question 1: Relevance to overall DOE objectives

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

- Very relevant.
- The project goals are critical to the Hydrogen Initiative and fully support DOE RD&D Objectives for photobiological hydrogen production.
- Project supports MYPP long-range biological technology.
- Project supports a low greenhouse gas emission technology.
- Project represents a high risk, long-term technology, appropriate for DOE investment.

Question 2: Approach to performing the research and development

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

- They are attacking the main barriers to biological hydrogen production.
- The project subtasks are well-focused on specific technical barriers.
- The project subtasks are well-balanced with respect to different technical barriers.
- The approach has effectively addressed the barrier of photosystem inefficiency due to antenna size.

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

This project was rated **4.0** based on accomplishments.

- Impressive accomplishments with minimal funding, well done.
- The selection or derivation of specific milestones and performance indicators was well-described, with excellent progress towards specific performance parameters.
- The investigator has made good progress on the continued characterization of the Tla1 gene and its impact on regulation of Chl antenna size.
- The technical achievements are well-documented, including the identification of the specific defect in the Tla1 gene.
- Good progress – 2010 target already achieved with 2015 target coming within reach – despite significantly reduced DOE funding.

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

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

- There is not an industry partner, but then again there is not funding to support a partner.
- The potential for commercialization or tech transfer of the Tla1 gene is strong, with the investigators having filed a patent application on the use of this gene.
- The investigators demonstrate limited coordination and collaborations with external researchers.
- Since project focuses on answering limited scientific question with little engineering or immediate tech transition requirements – sole investigator is appropriate.

Question 5: Approach to and relevance of proposed future research

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

- Good plans for the future research.
- The goal to clone the TlaX gene is logical, and should be doable given the investigator's past performance.
- The future research is narrowly-scoped, but has set clear goals with respect to measurement of antenna size.
- Proposed research has high probability of attaining target ahead of schedule.

Strengths and weaknessesStrengths

- They have achieved a lot of results with minimal funding.
- Lots of publications and presenting at the Gordon Conference is impressive.
- The investigators demonstrate clear experimental design of each subtask, and have made significant progress in the project.
- The investigator is an expert in the biochemical and genetic study of *Chlamydomonas*.

Weaknesses

- Lack of funding.
- They should include calculations of the efficiency of incident light energy to hydrogen production.
- The project is extremely limited in scope, and therefore may not have significant impact on other research projects within the program.
- The potential for scale-up beyond small bench-top reactors is unclear.
- The investigator has not adequately addressed the question from the prior review—what is the proposed function of the Tla1 gene? The homology plot doesn't provide much information, and the fact that this gene is conserved across a diverse spectrum of species should allow the investigator to posit some hypotheses. This might lead to conducting a bioinformatics search for additional Tla1 homologs—or to devising some clues for function that might lead to a rational design or re-engineering of this protein.
- The investigator mentions the potential for transfer of this gene to other algal species. It is unclear whether this will work, since presumably any algal homologs would be revealed in the initial bioinformatics screen.

Specific recommendations and additions or deletions to the work scope

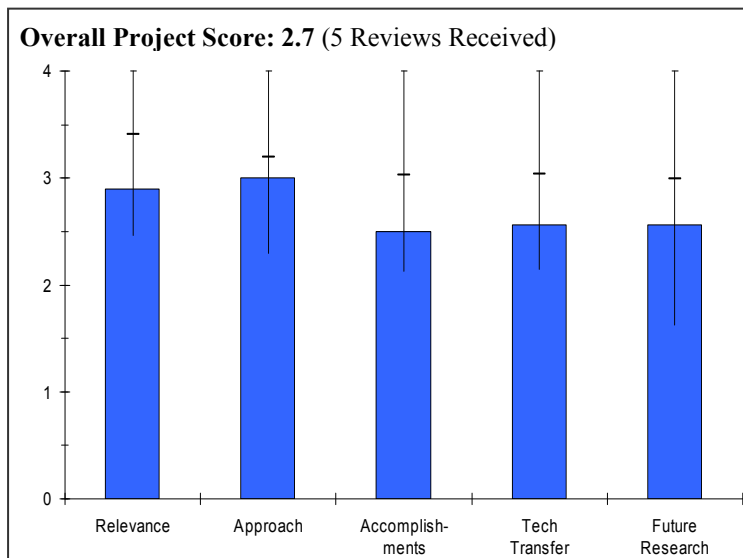
- Scope (and funding) should be enlarged to include duration of production and oxygen tolerance.

Project # PDP-36: Photoelectrochemical Generation of Hydrogen Using Sonicated Hybrid Titania Nanotube Arrays

Mano Misra; U of Nev. Reno

Brief Summary of Project

The overall objective of this project is to develop a high-efficiency photoelectrochemical cell using titanium dioxide nanotubular photo-anode and cathode for hydrogen generation by water splitting. The objectives for FY 2006-2007 are to 1) develop a new anodization technique to synthesize high-quality and robust TiO₂ nanotubes with a wide range of nanotube architectures; 2) develop single-step low band gap TiO₂ nanotubes by modifying synthesis parameters; and 3) develop a kinetics and formation mechanism of the titanium dioxide nanotubes under different synthesis conditions. For FY 2007-2008, the objectives are to improve efficiency by mixed oxide and organic-inorganic semiconductor photo-anodes; develop density functional theory to identify and modify the electronic properties of nanotubes; develop a combinatorial approach to synthesize hybrid photo-anodes having multiple hetero-atoms incorporation in a single photo-anode; and develop new TiO₂-based cathodes to increase the efficiency of the photoelectrochemical cell.



Question 1: Relevance to overall DOE objectives

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

- Most project aspects align with the Hydrogen Initiative.
- TiO₂ has been studied extensively for PEC water splitting applications, and its severe limitations in terms of bandgap, absorption and electron transport are well established; this is another case of starting over with a 'comfortable' material. While at least making some case for improvements to transport (through nanotubes) and bandgap (through "carbon" modification), the case was not compelling in this presentation.
- Considering the external bias requirements for effective photocurrent levels in this TiO₂ work, and the lack of a clear pathway for achieving the longer-term DOE photocurrent and STH efficiency goals with this material system, this work did not adequately address the DOE RD&D objectives for unassisted PEC solar water splitting.
- There may be conceivable pathways toward DOE RD&D objectives with substantial breakthroughs in TiO₂ modification in conjunction with multi-junction device configurations; however these pathways were not apparent in this presentation.
- Project supports MYPP for photoelectrochemical production of hydrogen.
- Difficult to differentiate from conventional TiO₂, which is known not to work.
- Not clear how/why nanotubes are an improvement.

Question 2: Approach to performing the research and development

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

- Step-wise logical approach.
- Does not appear overly ambitious considering the group and funding level.
- Durability testing and cost estimation is lacking in approach.

- The approach of TiO₂ nanotubes for enhancing charge transport and surface area is interesting, but does not address fundamental absorption limitations of the material.
- The approach of carbon modified TiO₂ for bandgap reduction has received much press in recent years, but the beneficial effects in visible light absorption have been generally overstated in literature, and also in this presentation. The bandgap limitations in this material class generally represent a show-stopper, and until there is clear evidence that radical band modifications are possible without complete destruction of optoelectronic properties, no amount of "nano-structuring" will matter.
- The approach for synthesizing TiO₂ nanotubes presented appears to be very effective.
- The approach of employing "off the shelf" materials modeling software for atomistic modeling and DFT is good, producing pretty pictures and graphs. The model results presented however appear somewhat 'first order', and will need some careful refinement for achieving realistic representation of the TiO₂ material system. For example, the direct bandgap indicated in the DFT calculations were particularly surprising – although this may be possible in novel atomistic structures of TiO₂ – it isn't commonly seen, and it wasn't supported in the presented data (which clearly indicated indirect bandgap). More application of the theory to validate the bandgap effects (for example of "carbon-modification") should have been a more significant part of the approach here – this would have required a greater allocation of the program's substantial resources in this challenging theoretical activity as well as a much greater reliance on external expertise, but it would be essential in addressing the main issue limiting TiO₂ in PEC applications.
- The approach of Pt Nanoparticles on TiO₂ nanotubes as an effective counterelectrode may be interesting in other higher-current density applications, but may be overkill in the solar conversion applications in which current densities remain low. Non-noble catalysts can be perfectly adequate in such applications.
- It was good to see some work on 'scaling up' the electrodes, however this is somewhat premature since the current material system is not functional for direct solar water splitting.
- Generally good (in discovery phase).
- Have identified potential roadblocks, so next year is crucial.

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

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

- Modest progress considering the amount of funding.
- Unclear why doing the carbon doping.
- Accomplishments in terms of TiO₂ nanotube synthesis were impressive.
- Accomplishments in atomic modeling and DFT calculations were interesting, although in need of further refinement.
- Reported accomplishments in terms of carbon-doping of the TiO₂ nanorods were not clearly supported either in XPS or photocurrent data.
- The implied bandgap reduction in "carbon doped TiO₂" was not sufficiently supported experimentally or theoretically; In fact, the IPCE measurements shown clearly reflect a material with some UV sensitivity, but still little visible sensitivity.
- Most of the photocurrent data presented was based on very questionable application light sources – a common error in PEC measurements. It is apparent that the light source utilized in the photocurrent measurements substantially over-compensates in the UV, which can make wide bandgap materials (which are unacceptable for PEC water splitting) look reasonably photoactive. The use of carefully calibrated light sources validated by outdoor sun measurements is critical to PEC research, especially in the evaluation of wide bandgap materials; considering the resources of this program, there is no reason for not implementing such fundamental experimental procedures.
- The conclusion that this work represents a "highly efficient photoelectrochemical cell for solar hydrogen generation by water splitting" is misleading; the need to operate under external bias and the application of illumination not representative of the solar spectrum contradicts this conclusion. That being said, it was at least encouraging to see any photocurrents in the mA/cm² range in TiO₂ material; the important issues in bandgap modification for non-UV absorption need to be more rigorously addressed in this work, with better theoretical and characterization efforts, and in conjunction with more experienced research partners.
- Concerned that investigator is 30% complete at this point in project.
- Reasonable progress in one year.
- Still skeptical regarding ultimate success.

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

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

- Unclear how group is working together.
- There was no clear indication of collaboration with any of the purported "partners"; help from other stated collaborators would prevent the types of photoelectrochemical measurement errors demonstrated.
- In light of the significant financial resources involved, collaboration with more experienced partners would have contributed enormously to the quality of the work.
- The concept for bandgap reduction in carbon-modified TiO₂ was clearly based on the work published by Dr. Khan at Duquesne University; this should have been cited, and collaboration with Dr. Khan should have been considered.
- TiO₂ for solar water splitting has been studied extensively over the years, and TiO₂ nanotubes have been explored. The fundamental limitations of absorption and carrier collection are still critical ones and still unsolved; if this work wants to work toward the significant breakthroughs that would be needed, a seriously expanded collaboration involving more experienced partners would be needed.
- Broad partnership: academic, national lab and industrial partners, including an international partner.
- Not clear how collaboration is carried out.

Question 5: Approach to and relevance of proposed future research

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

- Plans may lead to improvements, but unambitious for amount of funding.
- The proposed future work was a smorgasbord of popular ideas and buzzwords taken from TiO₂ literature.
- Some of the ideas were of interested, but not relevant until there is solid justification that efficient unbiased solar water splitting is achievable in a TiO₂ material system.
- Significant future remains in reducing TiO₂ bandgap, increasing the portion of the spectrum that the material can harvest, while reducing cost: uncertain that remaining funding is sufficient to overcome challenges.
- Need to increase their absorption efficiency.
- Need to show electron mobility for long tubes.

Strengths and weaknesses

Strengths

- Lots of publications.
- Good modeling.
- Synthesis of TiO₂ nanotubes.
- Use of atomistic modeling and DFT.
- Demonstration of photocurrents in the mA/cm² range.
- Novel idea.
- Well carried out.

Weaknesses

- Unambitious goals.
- Many people have made TiO₂ nano tubes.
- Misleading photoelectrochemical results based on improper utilization of light sources.
- Inadequate collaboration with more experienced technical partners.
- Does not identify a clear pathway toward efficient unbiased solar water splitting based on TiO₂ material.
- Inadequate utilization of program resources in appropriate theoretical and characterization activities for solving the main technical barriers.
- Perhaps relying on "nano-enabled miracle"; i.e., nano-solution that doesn't exist.

Specific recommendations and additions or deletions to the work scope

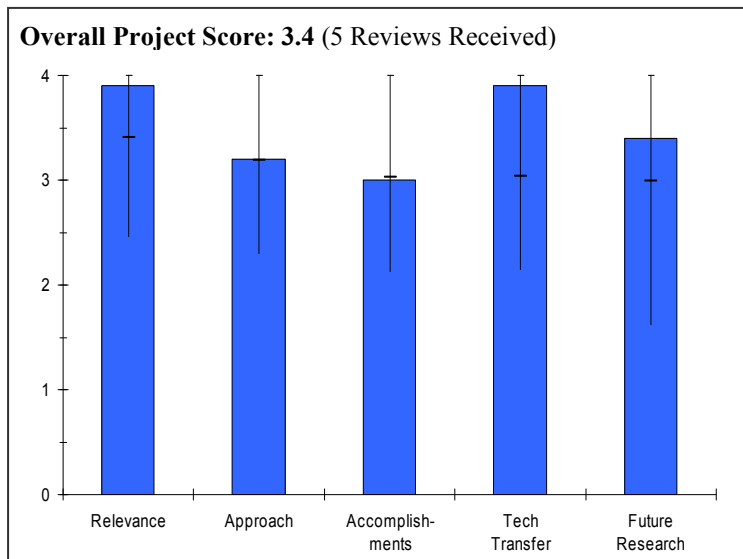
- Should add durability study.
- Should add cost analysis.
- The resources allocated to this work, if applied correctly, should be sufficient to identify once and for all whether TiO₂ has any chance of success in an efficient unbiased solar water splitting system. Expanded collaboration with other experienced research partners in materials science and photoelectrochemistry would be a necessary addition for this.
- There needs to be more emphasis on the fundamental theoretical and experimental approaches to defining the limits of bandgap modification and the implication on the ultimate efficiency of solar water splitting based on a TiO₂ system
- Less emphasis on the novel counterelectrode structure.

Project # PDP-37: Photoelectrochemical Hydrogen Production: UNLV-SHGR Program Subtask

Eric Miller; UNLV

Brief Summary of Project

The primary objective of this project is to assist the DOE in the development of hydrogen production technology utilizing solar energy to photoelectrochemically split water. The primary focus is on low-cost thin film materials (such as metal oxides) and novel multi-junction thin film devices (such as the UH-Hybrid Photoelectrode-HPE). The specific UNLV-SHGR photoelectrochemical (PEC) project goals are to 1) identify and develop new PEC film materials compatible with high-efficiency, low-cost H₂ production devices; 2) demonstrate a functional multi-junction device incorporating best-available PEC film materials; 3) develop avenues, integrating new theoretical, synthesis and analytical techniques, for optimizing future PEC materials and devices; and 4) explore avenues toward manufacture-scaled devices and systems.



Question 1: Relevance to overall DOE objectives

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

- This is relevant to the Hydrogen Program.
- Project is working to provide a feasible path to the DOE targets.
- Large collaborative effort appears to be an efficient approach to working towards DOE PEC cost and efficiency targets.
- Economical photoelectrochemical production of hydrogen is a viable and important long-range DOE objective.
- Despite decades of research, significant materials issues remain to be resolved; hopefully, a teaming arrangement will be of great aid in focusing individual researcher efforts to accomplish goals of efficiency, cost and materials stability.
- Great cross-university/industry/national lab collaboration covering multiple topics.

Question 2: Approach to performing the research and development

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

- Good combination of modeling and experimental work.
- Good team and collaboration.
- Focusing on the correct problem (materials).
- It took too long to arrive at the team approach.
- The feedback loop is beneficial in developing the theory and characterization of materials.
- Because each material presents different challenges and is being evaluated on progress toward overcoming those challenges, the project team needs to ensure that they are being evaluated against the same overall criteria.
- Initial screening via combinatorial modeling allows for quicker focus on most promising film compositions.
- Broad based participants allow faster synthesis, characterization and screening.
- Feedback loop good for quick incorporation of new learnings into subsequent experimentation.
- Work with NREL provides necessary integration into larger PEC system design.
- Though a teaming arrangement, this project is simultaneously pursuing several (5) different classes of materials.

- The benefit of this approach will be the development of theoretical tools that will help all participants and periodic (quarterly) group feedback can accelerate attainment of R&D goals.
- The team on this project should further develop effective measures to down select materials to focus on the most promising material(s).
- Multijunction, tandem approach for catching large amount of the solar spectrum appears to be a reasonable approach to improve device efficiency.
- Theory-synthesis-characterization-feedback is a logical approach.
- Good team, but perhaps too much "shotgun" approach.
- Could use more focus.

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

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

- Minimal progress with the team they have and the time spent.
- The project has made good progress in the past year.
- It is unclear how much DOE funding will be needed to continue or complete the planned work.
- Photocurrent milestones met for several PEC films.
- Highest efficiency achieved is 3.1%. This is much better than decades ago (1%?) but still very far from DOE's goal of 10-15%.
- Combinatorial synthesis approach is useful.
- Understanding of the effect of additions (e.g., nitrogen in tungsten oxide) is useful new knowledge.
- Materials-specific information gained for all 5 classes- a good body of data has been generated and is being applied to improve materials and structures.
- Good work thus far.
- Too much to present in one poster at any depth.

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

This project was rated **3.9** for technology transfer and collaboration.

- With the new teaming plan they are now starting to share their results.
- Excellent collaboration - this project really leverages teams from all types of organizations.
- Large ongoing collaboration that meets quarterly for information sharing.
- One of the strongest aspects of this project is the number of partners involved.
- Absolutely outstanding that so many people can work together effectively.

Question 5: Approach to and relevance of proposed future research

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

- There is no clear path to achieve the ultimate goals.
- They have set up a good plan on how to accomplish interaction and coordination.
- Need hard go/no go decision point to down-select materials.
- Covering a variety of technology options each with clear targets.
- Down-select of materials has been started. P.I.s should make sure that they accomplish this so more resources can be directed towards the most promising materials, structures, and fabrication procedures.

Strengths and weaknesses

Strengths

- Combinatorial approach.
- Strong group that is starting to act as a team.
- All of the available expertise.

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- Team feedback approach and sharing of theory tools and cluster tools, and other insights. Took 2 years to get everyone together.
- Wide talent base.

Weaknesses

- Slow progress for 3 years of development.
- They have not used the combinatorial approach to its potential.
- No funding for 2007- did not request an explanation.
- Unclear how momentum continues without government funding.
- People have spent decades researching photoelectrochemical devices. Time will tell but there is the possibility that these (or any) investigators may only be able to make marginal improvements. Should have a number of go/no go decision points in the coming years.
- Too much independent work.
- Lacks focus.

Specific recommendations and additions or deletions to the work scope

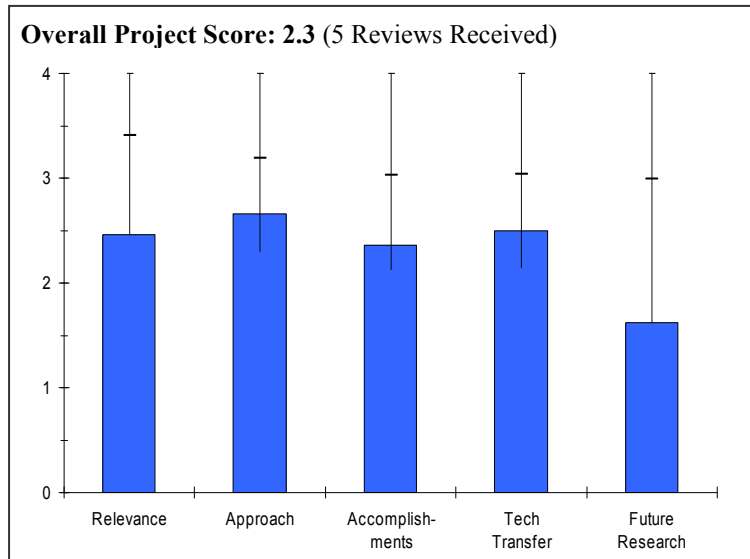
- Develop a path to achieve their ultimate goals. There is no path beyond discovery.
- Develop go/no go decision points for each class of materials.

Project # PDP-40: Adapting Planar Solid Oxide Fuel Cells for Distributed Power Generation

Andres Marquez; Ohio University

Brief Summary of Project

The objective of this project is to quantify impacts of synthesis gas composition on performance of a commercial planar solid oxide fuel cell system (cell and stack) that includes: 1) H₂S content; 2) CO/H₂ ration and energy content of gas; 3) particulate; and 4) metal content. The objective also includes the demonstration of long-term operation of planar solid oxide fuel cells (pSOFCs) using actual solid fuel-derived synthesis gas. pSOFC area specific resistance (ASR) was measured by completing V-I scans; the ASR histories were plotted and studied. Additionally, voltage (power) performance over time was monitored and studied.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- This is focused on electrical production not hydrogen production. It is not directly relevant to the President’s Hydrogen Fuel Initiative.
- This project seems to be aligned with SECA and should be done in that program.
- The use of syngas as a fuel for solid oxide fuel cells (SOFC) is an objective of the DOE Distributed Generation program.
- The project addresses improved tolerance to CO, H₂S, and other contaminants in syngas.
- Integration of SOFC and coal gasification could be a very likely scenario for stationary power generation.
- To further assess the relevance, it will be useful to look at the economic analysis of such an integration approach.
- Project focusing on effect of contaminants on solid oxide fuel cell performance. Similar work being conducted through the SECA program. Presumably, PI will communicate results to the solid oxide community at large.
- Solid oxide fuel cells have the highest efficiencies of all fuel cells; however, there are major materials issues to be resolved including poisoning, sealing, thermal cycling, etc. The proof will be the development of stacks (which this project is not doing) and long-term evaluation.
- The work appears to be competent, but the task does not appear directed toward the production or delivery of hydrogen. Appears that the managers of this review meeting have either placed this task in the wrong group, or that the task does not fit comfortably into the existing groups.
- Why has the funding been zero for the last two years?
- Appears to be answering a question of relevance to the FE or SECA programs?
- Work may have much higher relevance if SOFCs are used for the production of synthesis gas by stack gases containing H₂S.

Question 2: Approach to performing the research and development

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

- The integrated energy vision is not realistic since they are performing a water gas shift reaction prior to the Fischer Tropsch (FT) reactor (this changes the hydrogen to carbon monoxide ratio to a less desirable ratio). They are removing the carbon dioxide using a room temperature approach and then having to heat the gases

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back up, a method to remove carbon monoxide prior to the PEMFC is not included, and desulfurization was not included. The process produces a lower value commodity (electricity) at the expense of a higher value commodity (FT fuel).

- They did not properly identify the barriers to be addressed, or how their work is pertinent.
- The approach is not very original.
- The approach to quantifying and identifying the effect of contaminants in SOFC performance is good.
- The project does not sufficiently address mitigation of the contaminants or modifications to SOFC design to minimize the impact of the contaminants.
- The project scope is limited to the SOFC degradation mechanisms.
- One should not only look at the impurity impact, but also other aspects of coal gasification and SOFC integration. What are the critical barriers? The most critical ones should be addressed first.
- Generally the approach is narrowly focused. The right framework is to start with a broad set of challenges and down-select the showstoppers.
- PI is evaluating the effect of several contaminants (H_2S , HCl , AsH_3) on button cell performance.
- Button cells were provided by commercial vendor. No new materials were investigated in this work.
- Within the limitations on relevance to hydrogen production and delivery noted above, the approach is competent and effective.

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

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

- It appears that no improvements to the system were made.
- The project has identified the contaminants in syngas that are deleterious to the SOFC. The effect of trace levels of contaminants on SOFC degradation has been quantified.
- The project has not identified mitigation strategies.
- The progress is extremely slow and very little was accomplished for the time that has elapsed.
- Due to switching of SOFC partners a lot of work needs to be redone and very little time left for the project completion.
- Technically the only nugget that stands out is a sulfur tolerant anode catalyst.
- Low level of progress for funding level of project.
- Very low power density (less than 200 mW/cm^2) for button cells makes the design/materials not suitable for commercialization.
- PI has not made apparent significant progress toward most important DOE goals (40,000 hrs durability and \$1000/kWe).
- Severe cell degradation noted in this work.
- Progress seems to be good for a three-year task. However, the task has been going for five years and has not received any funding during the last two years. Shouldn't it be completed?

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

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

- How the team interacts is not clear.
- No papers or presentations were reported.
- The project is testing a commercial SOFC and is interacting with the manufacturer.
- The project is also working with other universities.
- Poor collaboration/selection of partners – switching the cell provider at such a late stage has caused significant perturbation to the project deliverables.
- Unclear how/who this technology will be transferred to. Button cell is a very early stage platform. Scale-up challenges are significant going from button cell to a representative stack scale.
- Several partners, but roles are uncertain.
- There appears to be a good deal of collaboration within the state of Ohio and some funding from state agencies.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.6** for proposed future work.

- No future plans were reported in the poster.
- The project is winding down.
- Proposed future work is longer-term testing.
- The presentation did not adequately address future work beyond testing.
- Proposed future research is inadequate to meet the said objectives of the project. This is mostly due to rework resulting from the shift in the cell partner.
- PI has collected data on effect of contaminants on cell performance, but it is uncertain of how this will be used to improve materials performance.
- Future work not presented.
- It appears that the project is ending or has ended some time ago. There are no suggestions or recommendations for future work by others taking on a similar project.

Strengths and weaknesses

Strengths

- Use of actual gas from coal gasification as the feed is useful.
- The project has made progress in identifying contaminant concerns in SOFCs.
- Good relevance to DOE's technical and portfolio goals.
- This work has identified what level of contaminants can be allowed beyond which unacceptable cell performance occurs.
- The causes of the performance loss have been identified.

Weaknesses

- The project focuses on SOFC work which should be part of the SECA program.
- There is nothing reported for future work.
- The scope of the work is limited.
- The project does not address improvements to SOFC technology. Some of this is being addressed in a companion project.
- Poor teamwork/partner strategy.
- Extremely slow progress - possibly due to lack of appropriate personnel on the project team.
- No new materials development.
- Little explanation of how the information gained in the task should be used in the fabrication of future cells.

Specific recommendations and additions or deletions to the work scope

- The project should be transferred to SECA or the fuel cell group.
- Their progress needs to be compared to DOE goals.
- Future work needs to be planned.
- They need to do some simple flow sheet modeling using ChemCad or Aspen to look at the "integrated concept".
- Given the progress, the said scope is adequate. Further addition will further delay the project. Any deletion of scope might significantly hamper the usefulness of the project.
- Include recommendation for future research in any final report from this project.

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Project # PDP-42: Ohio Distributed Hydrogen Project

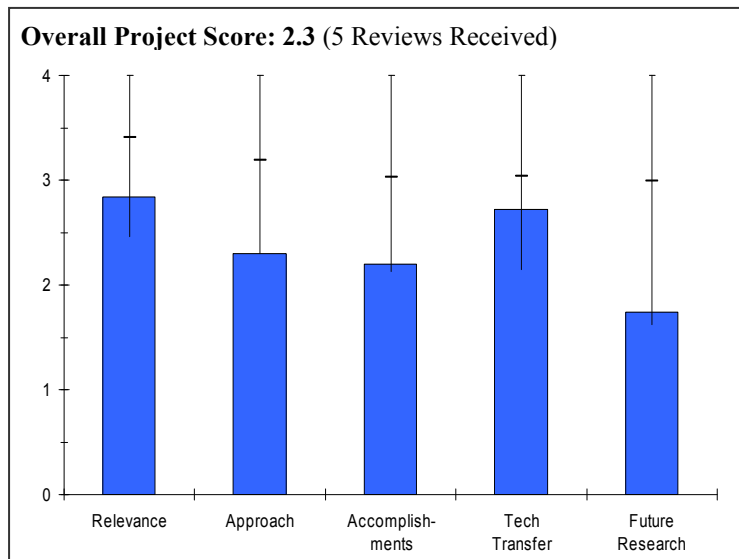
David Bayless; Ohio University

Brief Summary of Project

The objective of this project is to develop technology to aid in creation of a viable "distributed energy" system that 1) provides electricity from stationary solid oxide fuel cells; 2) provides useful waste heat from the fuel cells for other unit operations; and 3) provides usable hydrogen from the synthesis gas. The objective also includes the integration of combined heat and power into distributed H₂ production.

Question 1: Relevance to overall DOE objectives

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



- The main focus of the project is not hydrogen production, but hydrogen production is part of it.
- The project is developing technology to improve integration of a distributed energy system.
- The program aligns with the DOE distributed energy goals.
- Integration of SOFC and gasification could be a very likely scenario for stationary power generation.
- The overall "integrated energy vision" as highlighted in the project is good. But still unclear as to how the project will address the critical barriers in realizing the vision. Too broad a scope for the project.
- This project is focused on the development of solid oxide fuel cell technology for distributed energy production, and CHP applications. Integration of CHP into hydrogen production is a key element and important first step in development of the hydrogen economy. CHP applications have high efficiency.
- Project is also evaluating ceramic membranes for hydrogen separation for pure hydrogen fuel stream.
- This project is part of a novel combination of coal gasification and the growth of algae for the production of hydrogen.
- It appears to reflect some 'out of the box' thinking and thus cannot be comfortably placed in a particular category.

Question 2: Approach to performing the research and development

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

- The integrated energy vision is not realistic since they are performing a water gas shift reaction prior to the Fischer Tropsch (FT) reactor (this changes the hydrogen to carbon monoxide ratio to a less desirable ratio), they are removing the carbon dioxide using a room temperature approach and then having to heat the gases back up, a method to remove carbon monoxide prior to the PEMFC is not included, and desulfurization was not included. The process produces a lower value commodity (electricity) at the expense of a higher value commodity (FT fuel).
- When reporting data for the sulfur tolerance, the operating temperature, anode gas composition and fuel utilization should be reported.
- The electrolysis and separation approach is interesting.
- The fuel cell work is exactly the same as project PDP-40.
- The project is addressing three distinct areas: improving sulfur tolerance of SOFC anodes improving ceramic membranes for H₂ separation and optimizing H₂ production in the gasifier.
- The project effort is too divided among the three aspects. The project is not integrating the three aspects.

- The approach is narrowly focused, given the scope of the project. Most of the work is on SOFC development, with little emphasis on Fisher Tropsch synthesis and other separation steps (e.g., CO/H₂ and H₂).
- First step shall be to determine the economics of the grand scheme and then down-select the right element to focus on. Does it make sense for this whole cycle? What is the cost of (PEM grade) hydrogen from this scheme? What is the overall efficiency? All these questions need to be answered first.
- Project is developing sulfur tolerant anodes, a critical issue for solid oxide fuel cells.
- CHP configuration is being evaluated for highest efficiency.
- Pressure effects, H₂S effects, membrane flux, fluidized bed gas yields all preliminarily evaluated.
- It is felt that too many topics are being pursued in this project and that this will be problematic for making significant progress in any one particular area.

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

This project was rated **2.2** based on accomplishments.

- There has not been a significant progress toward the stated objectives.
- The project has identified an improved SOFC anode and conducted short-term testing.
- The progress in ceramic membranes is hard to judge, as state-of-the-art membrane data were not provided.
- The technical merits (and parameters) of the MEIC and H₂ separation membrane work were unclear and should be described in further detail.
- Technically, the only nugget that stands out is a sulfur tolerant anode catalyst, which is shared with PDP-40.
- Quantification of targets for each stage of the project will help in assessing the accomplishments.
- No data shown for different electrode materials.
- Button cells provided by external vendor so the researchers have not performed valuable materials research.
- Pressure effects, H₂S effects, membrane flux, fluidized bed gas yields all preliminarily evaluated. A mix of a "bunch of stuff." It is unclear how this all adds up to meet project goals.
- I don't see any energy balance to the various processes, including the electrolytic step.

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

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

- The coordination is unclear.
- There were no papers or presentations reported.
- The project is working with two other universities.
- Good teamwork/collaboration between the partners – the work on H₂ separation needs to be discussed in further detail.
- Need to lay-out a plan for future technology transfer.
- Several partners, but roles are uncertain.
- There must be close collaboration with their industrial and state government partners. Doesn't appear that DOE has been providing funding recently.

Question 5: Approach to and relevance of proposed future research

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

- No future work was plans reported.
- The presentation did not address proposed future work.
- The project does not appear to have any go / no go or down selection criteria.

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- Proposed future research is inadequate to meet the said objectives of the project. This is mostly due to the broad scope of the project.
- Quantification of individual subtask targets will be useful in assessing the merits of proposed future work.
- No future research discussed.
- No real suggestion of future research. Appears that the project is barely existing on outside support.

Strengths and weaknesses

Strengths

- They are examining sulfur tolerance.
- They are looking at some high temperature ceramic electrolysis and separation.
- Good relevance to DOE's technical and portfolio goals.
- Novel approach.

Weaknesses

- The integrated energy vision is unrealistic. The water gas shift reactor is in the wrong place.
- The models need validation.
- The electrolysis work is not original.
- The project is not integrated.
- The project effort is divided and lacks focus.
- Broad scope of the project. Would help to descope some of the activities.
- Very low power density for button cells makes the materials unattractive to scale up to a stack and commercialization.
- Project is pursuing several lines of research-solid oxide fuel cells, CHP application, and ceramic membranes for hydrogen separation. Should focus on only one line for greater benefit to DOE goals.
- Very inconsistent funding and little vision of future directions.

Specific recommendations and additions or deletions to the work scope

- The fuel cell work should be removed.
- The project should focus on only one or two areas.
- The project should be descope to improve its efficacy. Focus should be entirely directed first towards the economic assessment and then down-selection of critical barriers to be addressed. This will help in narrowing and downselecting the right focus.
- Progress in this project has been slow and it is recommended that the project should be discontinued.
- The project should either be properly funded then reviewed, or, if it is not funded, it shouldn't be reviewed.