2012 — Hydrogen Production and Delivery
Summary of Annual Merit Review of the Hydrogen Production and Delivery Sub-Program

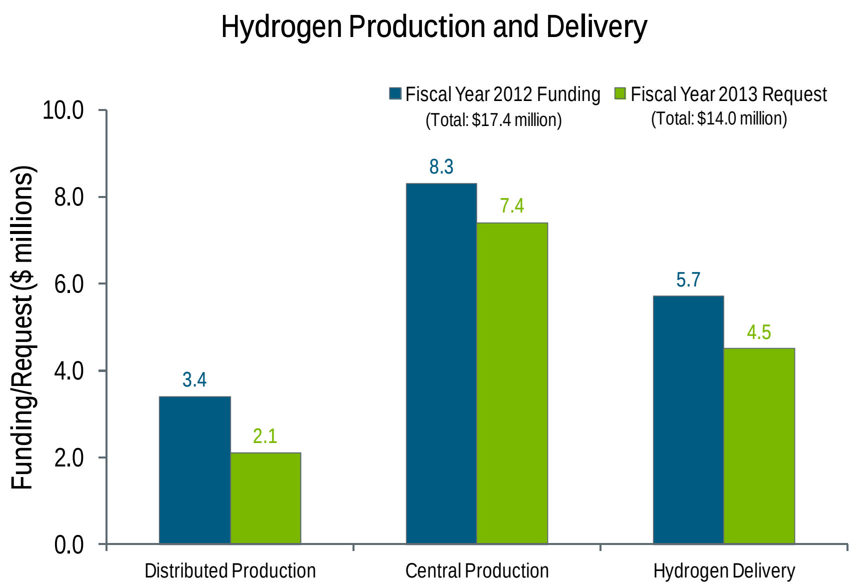
Summary of Reviewer Comments on the Hydrogen Production and Delivery Sub-Program:

This review session evaluated hydrogen production and delivery research and development (R&D) activities in the U.S. Department of Energy (DOE) Fuel Cell Technologies (FCT) Program in the Office of Energy Efficiency and Renewable Energy. The hydrogen production projects reviewed represented a diverse portfolio of technologies to produce hydrogen from renewable energy sources. Production project sub-categories included bio-derived renewable liquids reforming, water electrolysis, biomass gasification, solar-driven thermochemical cycles, photoelectrochemical (PEC) direct water splitting, biological hydrogen production, and separations technologies. The hydrogen delivery projects reviewed included R&D in advanced composite tube trailer vessels, low-cost pipeline materials, pipeline and forecourt compression, electrochemical compression technology, and delivery cost analyses.

The reviewers recognized the production and delivery projects as well aligned with DOE goals and objectives, and found that these projects, in general, had made substantial progress during the past year. Specific progress was noted in optimized component and reactor designs and in materials fabrication. Reviewers stressed the importance of continued improvement of performance and durability in materials, devices, and systems for renewable hydrogen production pathways, and for pipelines and compressors for hydrogen delivery. They also emphasized the need for continued cost modeling of production and delivery technologies to identify and address cost barriers, for further development of materials characterization protocols and performance metrics for early development technologies, and for expanded data collection to inform codes and standards development.

Hydrogen Production and Delivery Funding by Technology:

The fiscal year (FY) 2012 appropriation for the Hydrogen Production and Delivery sub-program of the FCT Program was $17.4 million. Funding was distributed approximately 67% to 33% between Hydrogen Production and Hydrogen Delivery, respectively, the same distribution used in FY 2011. Production funding is increasingly focused on early development, long-term, renewable pathways such as PEC, biological, and solar-thermochemical hydrogen production. This trend is expected to continue in FY 2013 with a $14 million budget request. The Delivery portfolio emphasis in FY 2012 and FY 2013 is on reducing near-term technology costs such as those associated with tube trailers and forecourt compressors, and identifying other viable low-cost early market delivery pathways.



Majority of Reviewer Comments and Recommendations:

In general, the reviewer scores for the production and delivery projects were above-average to high, scoring in the range of 2.3–3.7, with an average score of 3.1. The scores are indicative of the technical progress that has been made over the past year.

Bio-Derived Liquids Reforming: Two projects in bio-derived liquids reforming were reviewed, with an average score of 2.8. Projects in this area addressed hydrogen production through catalytic steam reforming of pyrolysis oil, and aqueous phase reforming of pyrolysis oil at moderate temperatures. In general, the projects reviewed consisted of a technically sound approach, focusing on identification of effective catalysts for use in the reforming process. Reviewers noted that the projects appeared to be well aligned with DOE objectives and demonstrated improved catalyst performance, but lacked a clear path forward to meet cost targets based on feedstock costs and catalyst cost and durability. Reviewers stressed the importance of documenting the results of these investigations and suggested that these approaches be considered for non-transportation energy applications for which higher hydrogen costs may be acceptable.

Biological Hydrogen Production: Four projects in biological hydrogen production were reviewed, with an average score of 3.4. Projects in this area encompassed a portfolio of photobiological and fermentative production methods that use various algal, cyanobacterial, and bacterial microorganisms that produce hydrogen through splitting water or fermentation of biomass. Reviewers cited a number of achievements, including the improvement in light collection efficiency of organisms, improved feedstock utilization as an intermediate step toward scaling-up fermentation, and light-induced hydrogen production by a bacterial hydrogenase expressed in algal cells. However, they also expressed concern that there could be difficulty with scaling up the projects to industrial scale. A key recommendation was that collaborations should be sought with experts in related fields and with industry, and that more specific and quantitative targets be identified for intermediate steps in the different projects in this longer term pathway.

Biomass Gasification: One project on the development of a one-step biomass gas reforming-shift separation membrane reactor was reviewed and received a final score of 2.3. Reviewers remarked on the slow progress for this project, noted that recent work has focused on modeling, and suggested that experimentation be given more emphasis next year. Reviewers recommended that integrated gasifier tests be started to validate performance and cost estimates, and stressed the need to characterize the status of hydrogen purity, selectivity, and membrane durability.

Electrolysis: Four electrolysis projects were reviewed, with an average score of 3.0. The major emphases of these projects were on cost reduction and efficiency improvement through cell and stack optimization, higher pressure operation, and validation of integration with renewable resources. Reviewers noted that the projects reported excellent progress, even those faced with early difficulties, demonstrating a good blend of analysis, design, and experimentation. They also commended the effective collaborations and quality of designs. It was recommended that the projects continue to emphasize meeting future cost and efficiency challenges, and focus on scaling up as well as qualification and manufacturing issues.

Home Refuelers (Small Business Innovation Research [SBIR]): Two SBIR projects addressing home refuelers were reviewed, with an average score of 3.0. Projects reviewed in this subsection focused on home refueling applications. Reviewers noted the emphasis placed on codes and standards as well as economic analysis. One project also made significant prototype and testing progress. Reviewers pointed out that there may be some difficulty associated with these projects due to the concerns of the public in having new technology within households. Reviewers suggested that an emphasis be placed on safety, cost, and outside collaboration to address issues related to the implementation of these systems.

Hydrogen Delivery: Nine projects in delivery were reviewed, with an average score of 3.3. Projects reviewed in the Delivery sub-program portfolio continued to receive high marks from reviewers for the sound progress made toward the sub-program's cost goals. High-capacity tube trailer vessels, pipeline materials, and pipeline compressors all showed progress toward the targets. Reviewers highlighted the level of expertise in this broad topic area and were impressed with the degree of collaboration within many of the projects. While recommendations for improvements tended to be project-specific, there was a general consensus that future work should focus on cost analysis with industry participation and include scaled testing and validation of the components in hydrogen environments. Reviewers encouraged testing activities to collect necessary data for the creation of codes and standards relevant to the use of the components in hydrogen applications.

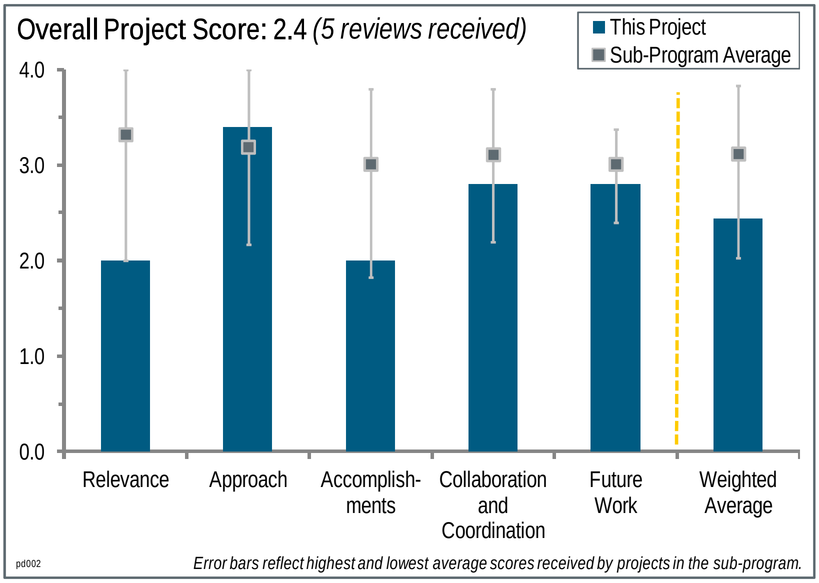
Photoelectrochemical Hydrogen Production: Four projects in PEC hydrogen production were reviewed, with an average score of 3.2. Reviewers felt that projects in this area were well aligned with DOE objectives, with a broad focus on developing viable PEC material systems and prototypes. They acknowledged notable accomplishments in improved current-voltage characteristics and enhanced durability in the promising materials under investigation. Projects were rated highly for material improvements in catalytic activity, for successful accomplishments in meeting durability milestones, and for effective collaborations with the PEC Working Group. Recommendations for future work included stronger emphasis on identifying the sources of losses in the PEC material systems, continued work on demonstrating extended durability, and continued techno-economic analysis to determine long-term viability of this pathway.

Separations: Two projects in separations were reviewed, with an average score of 3.2. The first focused on the development of hydrogen separation membranes for use in a water-gas-shift (WGS) membrane reactor. The second, an SBIR Phase III project, addressed development and demonstration of a biogas clean-up system. For the first project, reviewers commended the progress made in membrane development, but stressed the importance of integrating the WGS unit into the membrane reactor and testing the system in actual synthesis gas streams. For the second project, the reviewers commented on the importance of biogas purification for fuel cell applications. Reviewers commended the good design and promising sorbent material for this technology, but remarked that it was not clear that it represents a major advance over current commercial technology. They suggested that the project team consider additional applications for this technology.

Solar-Driven High-Temperature Thermochemical Production: Four projects in solar-driven high-temperature thermochemical hydrogen production were reviewed, with an average score of 2.8. Efforts in these projects were directed toward improving reactor designs, improving voltage and overall efficiency, and addressing membrane crossover issues. There was also an investigation into isothermal reactor operation for a two-step metal oxide cycle. The domestic and international collaborations in these projects were favorably noted. Reviewers recommended longer durability tests and degradation characterization for membranes and reactor materials. They felt that development of specific performance metrics and continued economic analysis are needed in order to frame the value proposition of these hydrogen production cycles. It was suggested that, since water-splitting reaction materials are still being screened, future work should focus on advanced materials research to improve redox capacity and cycle life of reactor materials.

Project # PD-002: Biomass-Derived Liquids Distributed (Aqueous Phase) Reforming

David King; Pacific Northwest National Laboratory



Brief Summary of Project:
The primary objective of this project is to develop aqueous-phase reforming (APR) catalysts and technology to convert bio-derived liquids to hydrogen (H_2) that meets the 2012 U.S. Department of Energy (DOE) cost target of $3.80/gge, as verified by the Hydrogen Analysis (H2A) model. Specific objectives related to feedstock issues are to: (1) identify primary compounds in bio-oil that are extractable into an aqueous phase, (2) determine the effectiveness of aqueous-phase reforming in producing H_2 from these water-soluble compounds, and (3) estimate the cost of H_2 production using the best catalytic results as a function of feedstock cost.

Question 1: Relevance to overall DOE objectives

This project was rated **2.0** for its relevance to DOE objectives.

- The project goal is very reasonable: explore aqueous-phase reforming using renewable bio-oil feedstock as a way to achieve the DOE target of $3/kg of H_2.
- This project is involved in downstream processing of the bio-oil (generated from biomass) to produce H_2. The concept was that some H_2 could be generated from what is essentially a waste product. However, the current cost of bio-oil renders this project a no-go regardless of the technical proficiency of the experimental team.
- The project has little likelihood of being able to support the goals unless the aqueous-phase components of bio-oil can become significantly cheaper and the acetic acid content is dramatically reduced from the stated 7%–10% or mitigated in some way. There are costs shown that are competitive, but they require significant and unrealistic improvements in reaction extent.
- Eight years into the project, there still does not appear to be a lot of advancement in aqueous-phase bio-derived liquid reforming, especially in meeting the DOE production cost targets. The project is relevant in that it is developing a technology for producing H_2 using renewable biomass as a feedstock, particularly a feed stream that is considered to be of little value. However, the technology does not fit well into DOE's definition of centralized or distributed production technologies. Given that it targets the aqueous-phase by-products from a biomass pyrolysis project, it is not suitable for distributed forecourt application, and the heating value of the aqueous-phase by-products is much too low for a centralized process. All results presented indicate that it will not hit DOE cost targets even if full conversion of all components could be achieved. There appears to be no pathway for reducing the cost of H_2 below that of the cost achieved if full conversion is realized, which will be nearly impossible to achieve.

Question 2: Approach to performing the work

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

- Attempting to identify catalyst candidates with a high-throughput combinatorial reactor is a very reasonable approach.

- This project is sharply focused on critical barriers. It was determined that the critical barrier of the cost of bio-oil could not be overcome.
- The approach was well tuned to identifying the compounds from a surrogate bio-oil source (pine sawdust), screening for appropriate catalysts, evaluating those catalysts, and using H2A to derive H_2 costs. Unfortunately, the data and results showed a very high cost of H_2, with no realistic options for cost reduction.
- The approach is to focus on attributes of the aqueous-phase bio-oil and screen reforming catalysts. This reviewer has concerns about the basic focus on the aqueous pyrolysis products. Apparently, 45% of the carbon in the feedstock is a solid and therefore not used in the H_2 generation process. This is a huge hit to overall efficiency. This reviewer wants to know what the energy content is of this solid material, and if there is a plan to use it. If not, this reviewer wonders if the economics can survive based only on the liquid portion. Perhaps a system that processes the whole spectrum of pyrolysis products would be better.
- The principal investigator (PI) and team members are technically very strong, and the overall technical approach is strong. The technical approach of using combinatorial approaches for accelerating the optimization of catalyst performance is a significant advantage of this project and appears to have been somewhat successful in identifying "optimal" catalyst compositions. From a catalyst consideration, the PI has identified three major technical barriers for improving catalyst performance: (1) overcoming the catalyst "poisoning" effect of acetic acid, which is present in significant concentrations in the aqueous-phase extract from pyrolysis oil; (2) controlling selectivity between carbon-carbon and carbon-oxygen bond cleavage; and (3) dealing with the inability to reform the light hydrocarbon gases that are produced. Unfortunately, there appears to be no clear path forward at this time for dealing with any of these three major issues. Stability is another major issue that needs to be addressed. Results presented indicate that higher reaction temperatures may be detrimental to catalyst durability, which may limit the ability to deal with the acid poisoning issue.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.0** for its accomplishments and progress.

- The technical work is sound. The PI and his team are very knowledgeable. The technique of the aqueous-phase reforming has merit, but the cost of the bio-oil feedstock negated this approach. The presenter rightly assessed the project's viability as poor due to results from his economic analysis using new cost estimates from DOE. The presenter also calculated what the cost of bio-oil should be in order for aqueous-phase reforming to be economically viable.
- There was progress in each task and the accomplishments achieved were well done, but, again, they provide little to no progress toward a goal for the production of H_2 from aqueous-phase bio-oil at anything close to the goal.
- Progress toward optimizing catalyst performance has been markedly improved over the past year through the use of combinatorial techniques. There appears to be little progress toward dealing with the major technical barriers affecting catalyst performance—acid poisoning and carbon-oxygen versus carbon-carbon bond cleavage selectivity makes it nearly impossible to see how this technology could meet DOE cost targets.
- The project team has achieved a solid accomplishment in identifying the major components in the aqueous fraction. Determining the approximate fraction of each component would be even more important if, as is suggested, some components are hard or impossible to reform. Combinatorial progress identified platinum (Pt)-containing catalysts as having substantial activity, which is no big surprise. However, work does suggest that Pt-Co and Pt-Zn are promising catalysts. The project demonstrates a well-thought-out approach to using chemical reaction theory to select a promising catalyst. The H2A task was well done and clearly shows that major improvements in H_2 conversion and bio-oil cost need to occur to get the H_2 cost down to even $5/kg. Bio-oil cost is a problem not because bio-oil is dramatically expensive, but because they need to consume a lot of it due to losing a large fraction of heating value in the pyrolysis step and in the low conversion of H_2.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- There appears to be good, long-term collaboration with Washington State University (WSU) and Virent.
- The partners are fairly well coordinated.

- There is collaboration with WSU and the batch APR studies at high temperatures to mitigate problems with species such as acetic acid.
- There are very strong collaboration partners, including Virent, which is the leading industrial organization developing aqueous-phase reforming of biomass, and Professor Jingguang Chen and Dion Vlachos at the University of Delaware, which has historically had very strong programs in catalysis. It is difficult to judge the contributions of Brookhaven National Laboratory and the University of Delaware, given that their role in this project focuses on x-ray spectroscopy and there was no discussion of these studies in the presentation. While the role and value of x-ray spectroscopy in these types of studies is well-known to this reviewer, the overall benefit to the project is not obvious. The role of WSU is critical, given that it is tasked with how to deal with acetic acid, which is very problematic for the catalysts evaluated in this system.

Question 5: Proposed future work

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

- This reviewer would strongly suggest that a go/no-go decision review be made soon on this project.
- The project is wrapping up. Documenting results should be the highest priority. The fiscal year 2013 plan for the examination of a single-step route to H_2 from cellulose seems like an outlier research effort begun in the waning days of the project that will receive too little effort, time, and money to achieve any useful results or conclusions.
- The project team is commended for showing, through H2A analysis, that this project is not economically viable, given the cost of bio-oil.
- The future plan is focused on addressing some of the major technical issues such as the acetic acid issue; however, the specific path forward to overcoming these issues is not defined. As such, it is not clear whether significant progress can be made. While extended catalyst testing is a desirable activity, the benefit of these tests is unclear, even if the selected catalyst proves durable over the proposed 50-hour test period, given that the major technical issues previously identified will probably not be resolved before these tests begin. The plan appears to be to terminate the project and summarize the results, which seems to be the most appropriate choice at this time.
- This reviewer thinks the future work plan is reasonable in content, but in reality, it will not bring about an H_2 source for transportation. Completing the future work beyond report writing will mostly just increase the shut-down cost of the project. However, as presented in the August 2011 Manufacturing Workshop in Hydrogen Storage Technologies, there are numerous energy applications where it is acceptable for H_2 to cost more than $100/kg. It may be worthwhile to consider “diverting” this effort toward “other markets,” similar to the way metal hydrides were diverted.

Project strengths:

- Documentation of what has not worked to date may help other research projects.
- The methodical investigation of aqueous-phase reforming is an area of strength.
- The team has good analytical and experimental capabilities in component analysis and catalytic studies.
- This project features a strong scientific team. Significant progress has been made toward improving catalyst performance through the implementation of combinatorial techniques. The team is focused on attempting to find value by processing a by-product waste or low-value biomass feedstock.

Project weaknesses:

- The cost of bio-oil is an area of weakness.
- The tough feedstock choice is an area of weakness.
- There is no clear catalyst that will work effectively to produce a high yield of H_2.
- Linkage of experimentally demonstrated bio-oil conversions does not lead to a projected cost of H_2 consistent with DOE targets.
- The technology does not seem to fit well into DOE vision of centralized and distributed production facilities. There is no clear pathway to achieving DOE cost targets for H_2. There are a number of technical issues related to catalyst performance—acid poisoning, reaction pathway selectivity, dealing with small molecules—that seem extremely difficult to overcome through improved catalyst performance. There might be some pretreatment technologies that could deal with the acid components in the feedstock, but this would introduce

additional costs to a project that already cannot meet cost targets. Given that full conversion seems nearly impossible, this reviewer would raise the issue of the cost of treating the process wastewater stream, and whether this has this been considered in the cost analysis.

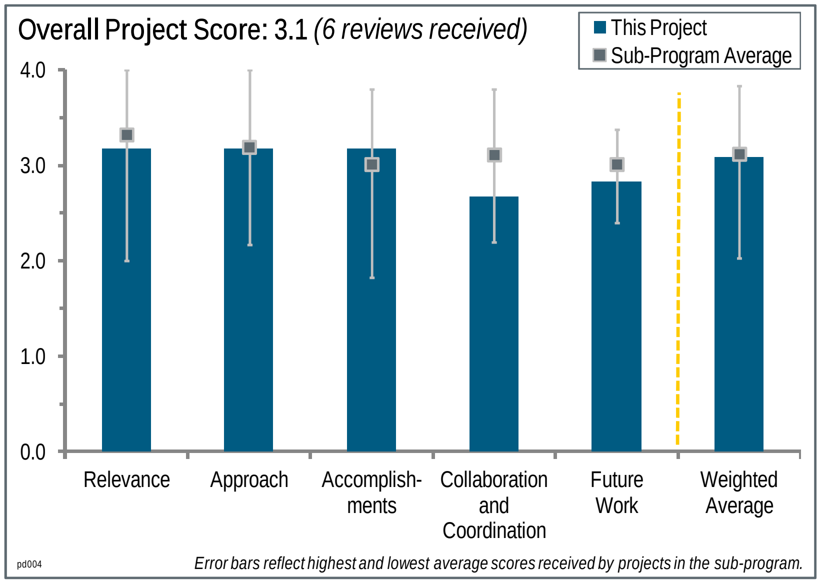
Recommendations for additions/deletions to project scope:

- This reviewer would recommend a no-go for this project. The funds can be better utilized elsewhere.
- The project team should allow the project to wrap up according to the current plans.
- The project team should look for higher-value H_2 applications or close the project.

Project # PD-004: Distributed Bio-Oil Reforming

Stefan Czernik; National Renewable Energy Laboratory

Brief Summary of Project:
The overall objectives of this project are to develop the necessary understanding of the process chemistry, compositional effects, catalyst chemistry, deactivation, and regeneration strategy as a basis for process definition for automated distributed reforming; and to demonstrate the technical feasibility of the process. For fiscal year (FY) 2012, the project will: (1) demonstrate 100 hours of commercial catalyst performance in an integrated bench-scale system, (2) achieve 100 L/h of hydrogen (H_2) production at a yield of up to10 g of H_2 per 100 g of bio-oil, and (3) assess the process energy efficiency and the cost of H_2.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- The project has very good relevance to DOE goals.
- The project is very relevant to DOE’s long-term goals.
- Generating H_2 from renewable sources helps make H_2 fuel cells even more sustainable.
- The project has the potential for developing an effective domestic source of H_2 at cost and quality that meets DOE’s targets, including greenhouse gas emissions. Uncertainty in feedstock cost is the major concern.
- The project is clearly relevant to the Hydrogen Production sub-program in terms of using renewable domestic feedstocks to produce $2/gge H_2. Depending on the assumed price of the feedstock, the cost target can be met with this technology.
- Autothermal reforming (ATR) of natural gas has been demonstrated for distributed H_2 production and has been shown to be able to meet the DOE cost target for H_2 production, suggesting that this technology could potentially be a viable approach for utilizing bio-oil—a renewable feedstock—for producing H_2. It is not clear how this technology fits into the DOE vision of distributed and centralized H_2 production. It is also not clear whether this technology could be utilized in distributed (forecourt) production, or what advantage it would have over direct gasification of solid biomass for centralized production.

Question 2: Approach to performing the work

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

- The approach resulted in continued progress over the last four years.
- The idea is basically adapting conventional steam reforming systems to the use of biofuels. The project wisely focuses on the two unique aspects of the project: feed preparation and the actual reformations. All other aspects (water-gas-shift [WGS], compression, separation, boiler) are standard chemical engineering equipment.
- First of all, the collaboration team, including Chevron, supports the initial worthiness of using fast pyrolysis followed by autothermal reforming to produce H_2. Also, in addition to H_2 production, the bio-oil can be used to produce other biofuels, which helps to spread the project risks to other potential markets. Furthermore, the switch

from attempting to develop a catalyst to the use of a commercially available catalyst appears to have significantly helped move the project forward. The overall approach appears to be very methodical and well planned.
- The technical approach is straightforward, with nearly all of the effort apparently being focused on the reformer. The choice of a platinum (Pt)-based commercial catalyst with a relatively low Pt loading is questionable based on the considerable amount of research and development effort to develop gasoline reformers. Rhodium (Rh)-based catalysts, although more expensive, are typically more active than the Pt-catalyst. A life-cycle cost analysis to compare the impact of the choice of catalyst (Pt or Rh) on the overall lifetime operating costs should be performed. It is not clear how much effort is being focused on developing and optimizing the other process unit operations. It is not clear that the technology can produce H_2 at DOE target cost, and there is no clear path forward toward meeting the cost target. The project seems more focused on demonstrating a complete system than actually designing, developing, and optimizing the complete system. It was unclear what advantage the electrochemical separator has over more proven, commercial H_2 separation/recovery technologies.
- The project team should consider simplifying the biomass-to- H_2 process by eliminating the centralized production approach for now and focus on reforming the bio-oil on-site. It is a big leap going from 100 L/hour bench scale to 1,500 kg/day (approximately 7,500 times). Perhaps an intermediate pilot is needed. It seems that the majority of the cost would be from front-end transportation and processing. Even if the bio-oil reforming is technically feasible, the costs associated with going from raw feedstock to reformer feed may turn out to be cost prohibitive. The project team should look more into alternatives to remove impurities prior to reforming. The team needs to get away from methanol additive and look into more practical alternative(s).

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.2** for its accomplishments and progress.

- More work is needed to reach the DOE cost targets.
- Advancing from the building of the bench-scale reactor in 2009 to the demonstration of 100 hours of commercial catalyst performance with 10% yield of H_2 demonstrates good progress.
- The team has made significant progress toward improving H_2 conversion, but the exact approach/methods used to improve performance are not clearly identified. The team appears to have improved performance from 7.5 g of H_2 per 100 g of fuel to 9.1 g of H_2 per 100 g of fuel. The team appears to have solved the problem of incomplete gasification by adding a high-temperature filter prior to the reactor. While this is not an ideal solution, it is practical and effective.
- Completion of the integrated bench-scale system is a major achievement to demonstrate proof-of-concept for this technology to produce fuel-cell-quality H_2. An estimate of overall process efficiency would have been appreciated, given that it appears a considerable amount of effort was focused on process analysis during the past year.
- The integrated system is performing very well, with 9.1 g of H_2 per 100 g of oil, and at 8.2 g/h H_2 production on a commercial catalyst. The main issue on progress toward DOE targets will be from the uncertainty in the cost of the bio-oil feedstock. If not solved, it has the potential of tying transportation fuel to an uncertain supply market, which is a problem even if it is domestic and clean.
- Typical H_2 content of feed pyrolysis-oil is 6%–8%. Consequently, 10.1 g of H_2 per 100 g of pyrolysis oil, reported previously (no WGS reactor) seems high. It would be helpful for the project to quantify the breakdown of H_2 contributions—feed, ATR, WGS, methanol, etc. That way, the researchers can target suitable cost reduction opportunities.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.7** for its collaboration and coordination.

- This project needs industrial partners.
- Collaboration either was not done or not reported.
- This project features very good coordination and collaboration with the Colorado School of Mines and the University of Minnesota.

- The collaboration team includes a national laboratory, academia, and industry. Each participant adds a strong element to the project, including understanding the process chemistry, developing the catalyst, and identifying the feedstock.
- Historically, this project had two strong collaborations, one with the University of Minnesota, which has been involved in autothermal reforming for nearly 20 years, and Chevron, which has been actively involved in the biofuels arena. However, both of these collaborations ended prior to the current review period, and it is difficult to determine the contribution from these two collaborations to the overall development of the technology. There is an ongoing collaboration with the Colorado School of Mines to develop a kinetic model of the reforming reactions and to define the operating conditions for the autothermal reformer, which would be beneficial to the reactor design. However, there was no discussion of any of the results from this collaboration in this presentation, so it is very difficult to judge the value of the collaboration.

Question 5: Proposed future work

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

- Longer-term testing and higher yield are very important.
- The future plans build positively on the progress to date. For FY 13, this reviewer thinks that a modeling effort should precede the plans to construct the 200-psi system.
- The project appears to be ready for the logical next step, which involves assembling the fully integrated bio-oil system and complete optimization and performance testing.
- There are very few specifics as to how the team anticipates further improving H_2 yield. The team merely states its goals. The plan to test for 100 hours is fine, but the work plan should contain specific metrics for performance at the 100 hour mark (e.g., >9.1 g of H_2 per 100 g of fuel after 100 hours of testing at 850°C, 1 atm, etc.)
- Demonstration of 100 hours of performance testing of the integrated system is critical for demonstrating the proof-of-concept of this technology and for providing a more realistic determination of process efficiency and the cost of H_2.
- The team needs to put many more hours on the bench-scale unit. Some catalyst accelerated stress testing may be worthwhile. The team should focus mostly on overcoming the technical barriers and should not worry too much about economic numbers at this point. Any cost estimates at this stage are very suspect anyhow. If this works reasonably well on a larger (pilot) scale, the reforming piece will not be a big portion of the overall cost (going from raw feedstock to high-purity H_2).

Project strengths:

- The economic analysis reveals that if the feedstock is reasonably priced, the cost and efficiency targets for H_2` production can be met using fast pyrolysis and autothermal reforming.
- The overall integrated process concept is good, and the work to date has shown significant progress toward validating the process design.
- Strengths of the project include the team's ability to manage progressive accomplishments and design and build a H_2 production system.
- The project is a clearly structured attempt to achieve high-efficiency H_2 production from bio-oil. The project correctly concentrates on the fuel vaporization and reactor. Significant improvement in H_2 conversion has been achieved. The proposed solution is straightforward and practical. The statement of reactor flow conditions (space velocity, O/C ratio, temperature) is concise and informative. The use of the Hydrogen Analysis (H2A) model cost estimator on this project is a prime example of how the tool was meant to be used. Key assumptions are appropriately listed.

Project weaknesses:

- This reviewer has concerns about the high-temperature filter required to remove soot/solid-particles after vaporization of the bio-oil. This reviewer also has concerns about the sulfur tolerance of the catalysts.
- The three critical weakness are: (1) the potential fluctuation of feedstock prices to the upside (however, this may be mitigated with natural gas use for power), (2) the atomization of the bio-oil (there are likely industrial-scale

solutions for this), and (3) bio-oil stability (a solution can be found by working with companies that stabilize other oil-based products).

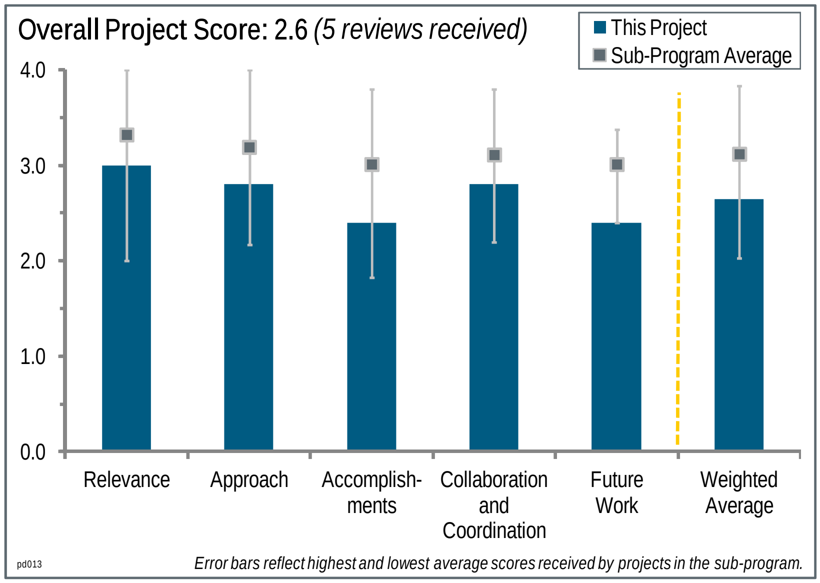
- Stationary reforming systems such as this technology are anticipated to have operating lifetimes well in excess of 40,000 hours. It is not clear that 100 hours of testing is sufficient to project the long-term durability of the catalyst. Degradation of the catalyst will result in increasing quantities of longer-chained hydrocarbon compounds exiting the reformer, which will not only impact the H_2 yield but could also have a detrimental impact on the performance of the downstream process units to purify the H_2 and may be difficult to reverse. The cost of H_2 estimates for the past few years of this project have remained relatively constant and above the DOE cost target. There is no clear path forward identified for reducing the cost of H_2 to meet the DOE target.

Recommendations for additions/deletions to project scope:

- This reviewer would add to the focus the development of a best additive package to stabilize the bio-oil.
- The long-term impact of sulfur in the bio-oil needs to be addressed at some point.
- The team should consider running a model before the 200-psi system final design and build.
- The project team need to focus on what it controls, not feedstock market prices.
- The sulfur tolerance of the catalyst and/or the ability to easily remove sulfur from the fuel should be investigated. The team should explore the cost impact of Pt at the proposed loading levels. It was unclear how much the catalyst cost contributes to the system cost. More explanation is needed regarding the basis of the capital cost estimation, as rising capital cost is part of the reason for the year-to-year H_2 cost increase. More work needs to be done to understand the key differences in bio-oil composition and how that affects H_2 production. Ideally, the system is robust and can handle a wide range of input bio-oils. The team did not discuss this aspect, but it should do so in the future.
- Given that the H_2 yield and cost is dependent on the source of the bio-oil as noted in the presentation, effort should be devoted to evaluating and estimating the H_2 production costs and yields from a wide range of bio-oil sources derived from different feedstocks.

Project # PD-013: Electrolyzer Development for the Cu-Cl Thermochemical Cycle

Michele Lewis; Argonne National Laboratory



Brief Summary of Project:

The objective of the project is to develop a commercially viable process for producing hydrogen (H_2) that meets U.S. Department of Energy (DOE) cost and efficiency targets using the copper-chlorine (Cu-Cl) thermochemical cycle. To increase efficiency in the Cu-Cl electrolyzer process, the project will investigate improvements to membrane and other hardware properties for higher cycle efficiency, reduced costs, and long-term durability of the electrolyzer.

Question 1: Relevance to overall DOE objectives

This project was rated **3.0** for its relevance to DOE objectives.

- This project is part of the DOE's long-term development portfolio.
- This project is part of DOE's portfolio of projects for long-term central production of H_2 from solar energy and is based on thermochemical cycles.
- The project represents a potential long-term option for the production of H_2 in a closed chemical cycle. It is not clear how the overall system cost compares to other electrolysis technologies—lower voltage is good, but balance of plant will be considerably more complex.
- The work is reasonably aligned, but it is hard to tell if it is relevant because there is no indication of how these specific technical goals and progress elements impact the commercial viability. In other words, there is a whole cycle here, and this reviewer does not see any explanation of the specific barriers to economic implementation of that cycle, and why these specific research items advance that goal. This reviewer wants to know what the critical targets are that must be reached to achieve the target cost of H_2. It is possible that the researchers understand this, but in a review of this magnitude, with outside reviewers, this background is essential, and missing.
- H_2 production technology remains challenging. Today, most H_2 is made using fossil fuels as starting materials. The proposed research utilizes thermal energy (i.e., heat) as the necessary reactant to synthesize H_2. The transformational concept is to generate fuel from waste heat. Low-cost H_2 could be the result. Because there are large amounts of waste heat available, this technology, if successfully developed, could generate huge quantities of H_2 with no concurrent CO_2 emissions. Moreover, significant amounts of heat would disappear, being converted into fuel.

Question 2: Approach to performing the work

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

- The approach is only fair because it is unclear how this project impacts the feasibility or attractiveness of the H_2 production goal.
- The project has an interesting membrane approach with two completely different options. It is unclear how the different approaches impact the system. The team needs to look at cell electrochemical efficiency (it is unclear where polarization is). It would also be interesting to do an overall efficiency analysis, including heating requirements.

- The project is focused on the electrolysis reaction because of the Cu crossover. The project's approach is well in line to address critical issues related to identifying membranes and optimizing the performance of the electrolyzer. However, the technical targets of the project were missing in the presentation.
- The approach is focused primarily on the development of the electrolyzer, with minimal amount of effort related to the rest of the cycle. Because the membrane needed the most improvement, that seems reasonable. The challenge with a hybrid system such as this is that electricity is very expensive, and unless the electricity is produced with renewable power, this process will have greenhouse gas emissions associated with it.
- This project is just one task in the engineering of the Cu+/Cu++ thermochemical cycle. It was decided that the electrolysis step, where the H_2 is produced, was the limiting technology, and that funds should be focused on that one process step. Therefore, the approach is to develop a useful unit operation device, which would be needed in a larger, complete system. The project targets center on technical specifications for that device. The technical approach was well thought out, and the approach for evaluation is appropriate.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.4** for its accomplishments and progress.

- The target performance necessary for demonstration of promise was successfully achieved. This advance was the result of building a new electrolysis device that allows H_2 production using only a small energy input.
- The researchers have made good progress in membrane Cu crossover issues, with multiple pathways now feasible. The team is meeting defined technical milestones, but the technology is still showing significant durability and stability issues. The team needs to increase focus on mechanisms of degradation and place a high priority on understanding and solving this issue. It is not really worth scaling until the device can last more than a couple days.
- The project made progress finding membranes that show no visible Cu deposition and reducing platinum loading. The results are promising, but tests at longer periods of time are needed because current density decreased with time due to degradation processes. The detection of Cu with appropriate methods of characterization should be better investigated to prove that the problem is solved.
- Progress is clearly being made, but again, it is unclear how this research and development quantitatively relates to the overall goal of economic and feasible H_2 production. Also, the results seemed scattered—they addressed certain items, but there was no strong theme explaining why these were the right items to address.
- The results for fiscal year (FY) 2012 seem very similar to FY 2011, so there does not appear to be a significant amount of progress. While the presenter showed improved degradation rates, the rates are still too high for a full-size electrolyzer. The Cu crossover issue has not been resolved. A visual examination after 24-hour operation is not sufficient for a system that is expected to operate for thousands of hours.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- The collaborators seem well integrated into the project.
- Interactions were not very well defined—several partners were listed, but it is not clear what their roles are or how they are impacting Pennsylvania State University's direction.
- The project has many collaborations, but this reviewer would like to better see the expertise of each partner and its contribution to the project.
- The project has a lot of partners. It is clear what Argonne National Laboratory, the Gas Technology Institute (GTI), and Pennsylvania State University have done. The Canadian group seems to be doing its own work, independent of this project; it is not clear how they are interacting with other members of the project.
- Although this project is a joint U.S.-Canadian activity, technical success depended upon collaboration between GTI and Pennsylvania State University. Those organizations were recruited because of unique technical skills, which were necessary for the demonstrated success.

Question 5: Proposed future work

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

- The project will remain focused on the electrolysis, with longer-term durability tests.
- Most of the focus should go toward degradation understanding, and the importance of scale-up should be lessened.
- The proposed future work seems like more of the same, which might be fine, but this reviewer cannot tell because it is not described how these specific activities relate to the goal of feasible and economic H_2 production.
- There are still many problems with the electrolyzer that need to be overcome prior to the scale-up. Scale-up seems premature.
- The future work was to optimize the electrolysis hardware. In any useful piece of hardware, such optimization needs to be done in concert with the rest of the system. Indeed, it probably makes little sense to move the electrolysis hardware forward until a detailed system design of the complete system is complete. Certainly, heat and mass transfer modeling is required. The concept presented called for additional membrane work. It seems that the demonstrated membrane (the one that showed adequate performance) is good enough for now. Work on the overall system is probably the next step.

Project strengths:

- The progress versus last year is good; the project is meeting its milestones.
- In the field of solar thermochemical cycles, the major advantage of the project is that the maximum temperature of the process is less than 550°C.
- This is a three-step thermochemical process that is less complex than some of the other processes. There is a large number of partners for the project.
- The concept is compelling. Making useful chemicals from normally wasted heat fluxes with no concurrent CO_2 seems too good to be true. The concept is the strongest part of this activity.

Project weaknesses:

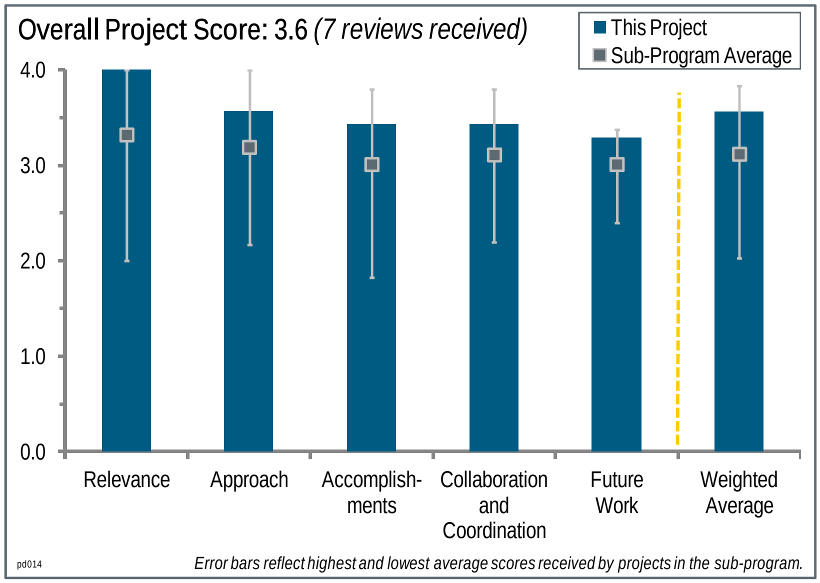
- This project addresses a very big topic and has a very small budget.
- Cell efficiency (power) should be explicitly stated in the presentation, along with any progress from the last year. For a long-term technology, the focus should be on resolving the critical materials issues such as durability; this should be the focus for next year.
- The detection of Cu by relevant and convincing methods of characterization is missing. It cannot be concluded that the critical problem of Cu crossover is solved only because there is no visible detection of Cu. Then, the results are obtained on short, 36-hour tests.
- Clearly, the weakness was framing. The value proposition, what the critical needs are to achieve that value proposition, and how each of the technical elements contributes to solving those problems were not clear.
- All hybrid cycles for thermochemical H_2 production require an electric power source. This is a significant weakness for these processes.

Recommendations for additions/deletions to project scope:

- The project team should focus on understanding degradation issues.
- This reviewer would not support any project that cannot clearly state its value proposition.
- First of all, researchers should confirm by adequate characterization that there is no Cu deposition, because this milestone is key to going further. Then, before investigating too much on membrane degradation mechanisms, the team should brainstorm with the membrane community to take advantage of the knowledge accumulated these past years on this topic in various fields (fuel cell, polymer electrolyte membrane electrolysis, etc.).
- The project team should return the emphasis to a model of the entire process, using the performance numbers demonstrated with the current, improved electrolysis. If possible, the team should do some work on (short-term) durability experiments to show that the membrane has at least the ability to run for a few days. The team should make plans (cost, time) for a project that designs, fabricates, and evaluates a prototype at a significant production rate.

Project # PD-014: Hydrogen Delivery Infrastructure Analysis

Marianne Mintz; Argonne National Laboratory



Brief Summary of Project:

This project will provide a platform to: (1) evaluate hydrogen (H_2) delivery cost, energy usage, and greenhouse gas emissions; (2) estimate the impact of alternative conditioning, distribution, storage, and refueling options; (3) incorporate advanced options as data become available; and (4) assist the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program in target setting. It will compare alternative component and delivery system options; assist in technology program planning; and support existing DOE-sponsored modeling efforts including Hydrogen Analysis (H2A) model components; H2A production; the Macro-System Model; Jobs and Output Benefits of Stationary Fuel Cells; and the Greenhouse Gases, Regulated Emissions, and Energy Use in Transportation Model (GREET).

Question 1: Relevance to overall DOE objectives

This project was rated **4.0** for its relevance to DOE objectives.

- This work has identified and is addressing the key near-term challenges to make this market work.
- This is a very helpful tool to track technology paths and adjust program area priorities.
- This work has been vital to guiding the Hydrogen Production and Delivery sub-program. It has enabled the sub-program to understand where costs are in the delivery pathways and to focus research where it will make a difference.
- Given the range of delivery options and challenges involved with each of these options, analysis is necessary to provide insight into the cost and cost sensitivities of different delivery scenarios on an “apples-to-apples” basis. The project contributes significantly to that effort.
- The H2A Delivery Scenario Analysis Model (HDSAM) is critical to understanding the cost limits of H_2 delivery. Argonne National Laboratory (ANL), particularly the principal investigator, has been a key analyst for this development and implementation.
- The project is extremely relevant. It can be used to provide a roadmap for the rest of the Hydrogen Production and Delivery sub-program projects. In particular, it demonstrates that compressor cost is a cost factor that needs serious attention. Furthermore, the project found that station capital cost varies not only as a function of throughput and power, but of vendor as well.

Question 2: Approach to performing the work

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

- The project features very convincing work. This reviewer likes the modeling work and the clear identification of operational barriers.
- It is a great approach to integrate the technology options for shipping, compression, liquefaction, storage, and dispensing for H_2 delivery in a usable model.

- Overall, this project has a good approach. It was not obvious who is setting the priorities in the sub-program's projects and who decides which topics to analyze. For example, this reviewer wants to know how industry is involved, besides providing data.
- ANL has been proactive in developing models and seeking input from stakeholders to ensure that the models are accurate. Excel-based models have been developed that are simple to use and widely available to anyone interested in H_2.
- Compression, tube trailer, trucking, and forklift cases were analyzed during the current period, representing high-priority scenarios that address important delivery barriers. The presentation did not make apparent the basis of the forklift refueling cost estimate or, thus, the confidence in and error bars around the estimated costs.
- The project evaluated current compressor technologies, high-pressure tube trailers, and H_2 tracking options, and assessed differences between fuel cell electric vehicles (FCEVs) and forklifts. The reported approach on slide 4 is quite comprehensive, as it addresses the spectrum of the solution pathways by carefully relying on data from research project results, industry, and DOE.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.4** for its accomplishments and progress.

- Overall, the project features good results. The analysis of "hydrogen trucking options" seems quite superficial and is not very surprising.
- The HDSAM update and completing three of the five evaluation milestones for several different technologies appears to be good progress.
- This reviewer appreciates the fact that the study remains focused on how to make H_2 an economical solution within the next five to eight years. Acknowledging the current technological challenges and looking for ways to resolve those challenges is terrific.
- This year's exploration of early market conditions has been important for understanding transition issues. The work has been important in understanding the trade-offs between liquid delivery with pumping versus gas delivery with compression.
- Delivery costs are a substantial barrier to H_2 FCEV and fuel development. This model has given the U.S. DRIVE Partnership team a means to evaluate the key cost barriers and address technology research and development related to its solutions.
- The project essentially assessed the delivery issues and the capital utilization for early market penetration, suggesting that delivery station cost should be pushed upstream and compressor technology cost should be addressed in earnest, as it presents several challenges (see slide 13). The results reported on slide 10 are very important because they stress a hard-to-explain variability of compressor cost among various vendors.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- The project has a broad range of collaborators.
- There is good collaboration among the national laboratories that are working together to make HDSAM better.
- The project team performed good work with stakeholders to get model input.
- This reviewer did not see explicit industry collaboration and wants to know how industry is involved.
- This reviewer did not pick up on clear collaboration with other institutions; however, this reviewer is aware of the project's effort to consult with industry. This is important because it can save time and capture the techno-economic challenges that come with certain solutions.
- The project's results demonstrate an extensive network of collaborations with national laboratories and industry. This reviewer assumes progress would not be as reported if it were not for these collaborations. The participation of M. Paster in the project is important.

Question 5: Proposed future work

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

- The directions look good.
- The goals are on-point to help H_2 fuel become a fully integrated solution in the U.S. economy.
- The future work focuses on high-priority delivery issues.
- The future work was rushed due to the expiration of the presentation time.
- It was not obvious who is setting the priorities in the projects and who decides which topics to analyze. For example, one reviewer wants to know how industry is involved, besides providing data. This reviewer is very interested in the ionic compressor analysis and the comparison to other compression technologies.
- The proposed work reported on slide 26 is the appropriate next step for the project. With regard to bullet item 3 on slide 21 (evaluate storage technology options and new concepts), attention should be drawn to how the various storage concepts will be assessed. For instance, storage concepts such as the steel/concrete vessel presented in this DOE Hydrogen and Fuel Cells Program Annual Merit Review have many deficiencies in their design and, as such, incorporation in the project's analysis may lead to erroneous results.

Project strengths:

- Of all of the projects, this may deliver the most value to the development of H_2 energy commercialization.
- The model allows flexibility regarding different options for H_2 delivery and predicts costs.
- The systematic inclusion of parameters affecting cost is a strength of this project.
- The model is a very valuable and flexible tool to help DOE sharpen its program. A strength of this project is the active national laboratory partnership.
- This project features excellent, unbiased analysis by the principal investigator and his colleagues. It offers a high return on DOE investment.

Project weaknesses:

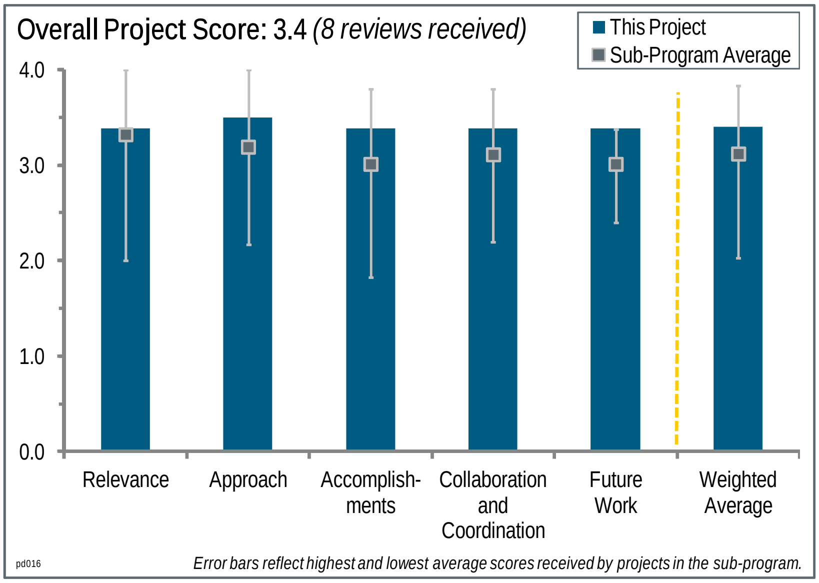
- This project really deserves more resources to fully vet the identified goals.
- The model needs validation of predicted costs.
- It was not obvious who is setting the priorities in the projects and who decides which topics to analyze. For example, this reviewer wants to know how industry is involved, besides providing data. In some cases, the analysis appears too superficial.
- It is not clear why the compressor cost is vendor dependent. Reasons need to be researched and documented. The project seems to focus on early FCEV markets; more explanation is needed on long-term markets. Also, there are no details on the technical approach taken to cost various solution pathways. For instance, this reviewer would like to know how the soundness of specific technologies is weighed in. By way of example, the steel/concrete vessel proposed to be examined in the future is a technology that is based on design estimates, and, as such, the related costing may lead to an unrealistic and unreliable assessment of the relevant solution pathway.

Recommendations for additions/deletions to project scope:

- The project team should provide transparency on how topics are selected and how industry is involved.
- This reviewer felt that there were no additional recommendations to make.
- The comparison between volume utilization results for steel and composite tubes reported on slide 16 perhaps is unfair to the steel option. It is not clear what steel design criteria have been used to estimate the tube thickness. To the knowledge of this reviewer, the criterion used by the industry—namely that the hoop stress must be 30% of the elastic limit—is over-conservative. If this criterion—which is based on a maximum allowable stress—was used, the analysis needs to be revisited. A fracture mechanics analysis of the type leak before break is the proper one.

Project # PD-016: Oil-Free Centrifugal Hydrogen Compression Technology Demonstration

Hooshang Heshmat; Mohawk Innovative Technology, Inc.



Brief Summary of Project:

The objective of this project is to design a reliable and cost-effective centrifugal compressor for hydrogen (H_2) pipeline transport. The goal specifications for the compressor are that it should be able to handle a flow of 240,000 to 500,000 kg/day, handle a pressure rise from 300–500 psig up to 1,200–1,500 psig, and be able to operate with contaminant-free/oil-free H_2.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- Relevance to the Hydrogen Production and Delivery sub-program is high because compression is a key cost item barrier for H_2 delivery.
- This project addresses the cost of large-scale gas compression as a significant economic barrier.
- Although this reviewer admires the work being performed, this reviewer does not think this project has any relevance for the next 10 years regarding aiding the commercialization of the fuel cell markets. When it does begin to have relevance, advancements in materials will probably relegate this technology to the shelf.
- Based on Mohawk's estimates, the technology can meet the DOE compression performance, capital investment, and maintenance cost targets.
- Work is 80% complete. The barriers are well defined. Project targets have been discussed and met in work completed as of today. The design concept of bearings, thrust bearings, foil seals, and compressors are very advanced and computational fluid dynamics (CFD) analyzed. The design meets DOE targets in cost and weight. Size was not addressed. Structural materials issues are resolved.
- The cost-effective and safe compression of H_2 is critical to the DOE Hydrogen and Fuel Cells Program. This project promises to deliver a compression technology that utilizes state-of-the-art technologies and new technologies emerging from current research.
- The project is extremely relevant. Compressor technology is an essential component of H_2 transport. Although the design meets the DOE targets (see slide 9), issues related to system aerodynamics, materials selection, and performance in the presence of H_2 need to be addressed.
- Cost-effective and reliable H_2 compression to 1,000–1,500 psi for pipeline transport and for use at terminals and other modest pressure bulk storage facilities will ultimately be vital for viable use of H_2 as a major energy carrier. Currently, only reciprocating compressors work for these applications; they can be unreliable, forcing the installation of backup units, which increases capital costs. Centrifugal compression technology that can be advanced to be effective with H_2, such as that being developed in this project, could result in more cost-effective and reliable H_2 compression. The widespread use of pipelines for H_2 transport is not likely to occur until H_2 has made considerable penetration into the transportation market. Recent analyses of H_2 delivery indicate that pipeline compression is only a relatively small part of the cost of delivering H_2. There are other more pressing H_2 delivery issues that need to be resolved, such as refueling station compression costs and H_2 storage costs. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. Although the range of costs approaches the 2012 and 2017 cost targets for this application in the currently posted Hydrogen Delivery Multi-Year Research and Development Plan, the costs are significantly

higher than the current (2010) costs in the recently updated and posted Hydrogen Analysis (H2A) Delivery Scenario Analysis Model (HDSAM) Delivery Model.

Question 2: Approach to performing the work

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

- It was a good approach to partner with Mitsubishi to allow developments to be used in the natural gas compression industry.
- The project approach is a modular single- or double-entry centrifugal compressor. System CFD analysis and component finite element analysis (FEA) have been performed.
- This reviewer hopes that the work performed can be applied, but this reviewer suspects that a very large testing program will be required to validate the design work and stage the technology for commercial consideration. This reviewer would not advocate DOE funding for that test activity.
- The hermetically sealed, oil-free centrifugal compressor design, assembly, and testing for single-stage compression is a reasonable approach for bench-scale analysis of pressure and flow characteristics. Running the H_2 through the single stage six times should give a good indication of the six-stage compressor performance. Also, doing validation studies at both Mohawk Innovative Technology, Inc. (MiTi) and Mitsubishi Heavy Industry (MHI) provides additional confidence in the performance results from the oil-free coupling technology.
- The project appears to have a pragmatic and systematic approach. The presentation could be improved. The approach was not very obvious.
- The components are in assembly stage and appear to be on track for full-scale testing in the near future. Tests were successfully completed on the motors.
- The approach features an excellent mix of research, emerging technologies, state-of-the-art technologies, and proven technologies. That is, if a significant improvement in compression technologies is to be made that reduces energy consumption and costs, it will not be made using only existing technologies (or it would already exist today). This project appears to be consulting and utilizing resources from other DOE projects, DOE laboratories, universities, and other entities better than any other project seen by this reviewer. In particular, this project presented its design and a list of issues, plans, and tests all designed to address the identified issues. This made the approach appear more carefully thought out and planned than other projects.
- The project is taking an excellent approach in most ways to achieve its objectives. It is using state-of-the-art centrifugal design methods, including mean line CFD and finite element analysis. It is incorporating the state-of-the-art foil bearings and seals needed to achieve breakthrough rpms while totally eliminating all oil lubricants, thus eliminating any possibility of oil contamination of the H_2. Building a full-scale single-stage unit for rigorous testing and evaluation will yield extremely valuable results without the excessive cost of building a more complete test compressor. There are two potential designs being evaluated within the project. This increases the probability of success. The project includes both MiTi, which had the original concept, and MHI, which brings tremendous experience and expertise in current large centrifugal compressor technology. The project is now getting world-class advice from Sandia National Laboratories (SNL) and Dr. Sofronis at the University of Illinois on H_2 embrittlement relative to material selection, and the plan includes testing of materials at SNL and the National Institute of Standards and Technology (NIST). More attention is still needed on material issues in this H_2 environment. It is not clear exactly what the testing at SNL and NIST will entail. It is imperative that the single-stage test unit be run with $H_{2,}$ but there are no plans to do this within this project. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. Although the range of estimated costs approaches the 2012 and 2017 cost targets for this application in the currently posted Hydrogen Delivery Multi-Year Research and Development Plan, the costs are significantly higher than the current (2010) costs in the recently updated and posted HDSAM Delivery Model.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.4** for its accomplishments and progress.

- Mohawk has built the first oil-free H_2 compressor using foil bearings.
- The accomplishments are good, but in terms of a commercial design, probably 5–8 years will be required to demonstrate a truly functional, oil-free compressor.

- The assembly of the single-stage compressor is a major accomplishment that clears the path for reliability performance testing and improved cost estimates.
- The results are promising compared to DOE targets, but they are only based on "MiTi estimates" (slide 9). Material selection is based on an extensive literature search and consultation with H_2 embrittlement experts. This is not sufficient.
- With the current status of 80% complete, the compressor system appears to meet all DOE goals and requirements.
- Very good progress has been made and the results to date are very encouraging. The compressor, subcomponents, and single-stage test unit have been designed. The single-stage test unit has been fabricated and assembled, and some testing and results have been completed. Material issues and selection for the H_2 environment have been discussed with experts in the field, and initial material selection has been completed. However, testing these materials appropriately in H_2 and under operating conditions has not yet been done.
- This project appears to excel in anticipating all of the issues and making plans accordingly. The investigators appear to be on schedule to complete initial verification testing in fiscal year 2012. The future work appears to involve some decision making between two different systems being developed by completely different parts of this team, and a redesign incorporating the best features of both designs is planned in the future. A pro-con table for these two systems was presented. However, this decision will almost certainly be made on the basis of cost. As a result, this decision will require accurate and equivalent cost estimating for both systems, which the team appears unprepared to do. Leaving cost estimating primarily to one part of the team or the other will make the decision, instead of the attributes of the different designs. That is, progress toward this decision point did not seem to be as outstanding as it was in the other areas.
- Material issues and needs have been thoughtfully identified as they pertain to the individual components. Components have been manufactured and assembled. Initial motor spin testing has been performed (slide 16), bearing temperature has been estimated as a function of time (slide 17), and compressor dynamic verification analysis was done up to 30,000 rpm. All of these accomplishments are significant.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- There are not many collaborators, but good progress was made with those used.
- The collaborations with SNL and NIST are important and beneficial to the project.
- Industry is involved. Commercial potential is identified (slide 4).
- It is clear that there is collaboration with MHI on this project, but this reviewer did not have the sense that the level of collaboration was high. Beyond MHI, this reviewer did not pick up on meaningful collaboration work.
- The collaboration with MHI brings to bear some important resources, not to mention an important industry player that could be a potential end user of the centrifugal compressor technology. Also, the compatibility studies of the foil bearing and foil seal materials with H_2 from NIST add to the design safety.
- Industry partners are already building and using 2-stage H_2 compressors. This work is seen as a natural step. National laboratories are also participating as consultants.
- The project includes both MiTi, which had the original concept, and MHI, which brings tremendous experience and expertise in current large centrifugal compressor technology. The project is now getting world-class advice from SNL and Dr. Sofronis at the University of Illinois on H_2 embrittlement relative to material selection, and the plan includes some testing of materials at SNL and NIST.
- This project appeared to be the best in this category of any this reviewer saw at this meeting. That is, the researchers appeared to not only be talking and consulting with other laboratories, but actually listening. Actual design changes could be tracked to consultations with other laboratories and projects.

Question 5: Proposed future work

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

- All testing in line with ASME requirements will be completed. The H_2 comparison will be completed. The project team needs to refine its estimates of capital cost to scale-up production.
- There is little value for the DOE Hydrogen and Fuel Cells Program in pursuing future work on this design.
- No tests with H_2 are planned or included in the existing budget.

- There are good plans to demonstrate compressor use at 60,000 rpm. It is good to see the concern for safety at 60,000 rpm.
- The future work is well thought through and designed. It includes completing full testing and evaluation of both single-stage test units in air and helium (He), selecting the best approach, and completing the full compressor design based on the results of the single-stage testing. H_2 material compatibility of the foil seals and foil bearings will be done at SNL and NIST. Finally, the cost estimate will be updated. More attention is still needed on material issues in this high-pressure H_2 environment. It is not clear exactly what the testing at SNL and NIST will entail. It is imperative that the single-stage test unit is run with H_2; this is not currently in the project plan.
- There is an excellent plan for validation testing and the evaluation of performance and costs within the scope of the budget. The final design needs to be subjected to H_2 testing and durability testing. However, the decision between these two competing systems needs to be made before this can be included as part of the project.
- The goal is to select between double- and single-entry designs and assess capital cost in relation to the DOE 2017 targets. A detailed testing plan is outlined on slide 20 with a proposal for material testing for foil bearings and foil seals to be carried out at SNL and NIST. However, testing of the entire system in the presence of H_2 is missing from the proposed work.

Project strengths:

- A strength of this project is how it considers the challenges and opportunities to develop an oil-free compressor for H_2 service.
- The two industrial partners, MiTi and Mitsubishi, are quite capable of proving or disproving the feasibility of the technology.
- Strengths of this project include industry collaboration, promising estimates, and commercial potential.
- The project looks to be on target to meet or exceed all DOE requirements.
- This project features a novel use of foil bearings for the first time in a compressor.
- The system design is in place, meeting DOE targets (slide 9); components have been fabricated; material issues have been carefully outlined on slide 11; and relevant laboratory testing at SNL and NIST has been proposed.
- This project features excellent coordination and consultation with other DOE projects, laboratories, universities, and other entities, including consultations early enough to actually affect the design process and the final design. There is a nice combination of big and small company collaborations, giving the impression of a team with combined excellence in innovation and manufacturing.
- Cost-effective and reliable H_2 compression to 1,000–1,500 psi for pipeline transport and for use at terminals and other modest-pressure bulk storage facilities will ultimately be vital for the viable use of H_2 as a major energy carrier. Currently, only reciprocating compressors work for these applications, and they can be unreliable, forcing the installation of backup units, which increases capital costs. Centrifugal compression technology that can be advanced to be effective with H_2, such as that being developed in this project, could result in more cost-effective and reliable H_2 compression. The project is taking an excellent approach in most ways to achieve its objectives. It is using state-of-the-art centrifugal design methods, including mean line CFD and FEA. It is incorporating the state-of-the-art bearings and seals needed to achieve the breakthrough rpms into the design. Building a full-scale, single-stage unit for rigorous testing and evaluation will yield extremely valuable results without the excessive cost of building a more complete test compressor. There are two potential designs being evaluated within the project. This increases the probability of success. The project includes both MiTi, which had the original concept, and MHI, which brings tremendous experience and expertise in current large centrifugal compressor technology.

Project weaknesses:

- The market need is decades away.
- This reviewer does not think that this will eliminate the need for compressor redundancy. It would be useful to converse with natural gas experts to get their thoughts.
- The material selection based on literature research is a weakness. No H_2 tests are included in the existing budget. Another weakness is the cost of approximately $7 million, compared to approximately $4 million for a compressor from PD-017.
- This reviewer felt that there were no weaknesses.
- There has not yet been a test of the compression system with H_2.

- The use of Beta Titanium as rotor material needs to be revisited. This high-solubility system is well known to be H_2-embrittlement susceptible. Testing of the entire system in the presence of H_2 is missing from the proposed work.
- The widespread use of pipelines for H_2 transport is not likely to occur until H_2 has made considerable penetration into the transportation market. Recent analyses of H_2 delivery indicate that pipeline compression is only a relatively small part of the cost of delivering H_2. There are other, more pressing H_2 delivery issues that need to be resolved, such as refueling station compression costs and H_2 storage costs. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. Although the range of costs approaches the 2012 and 2017 cost targets for this application in the currently posted Program Multi-Year Research and Development Plan, the costs are significantly higher than the current (2010) costs in the recently updated and posted HDSAM Delivery Model. More attention is still needed on material issues in this H_2 environment. It is not clear exactly what the testing at SNL and NIST will entail. It is imperative that the single-stage test unit is run with H_2. This is not part of the current project plan.
- Big partners can push small partners around without even knowing that they are doing it. This project is a year or two away from a critical decision between two competing designs being developed by big and small partners. It is unclear who will make this decision and on what criteria. It also appeared that the cost estimating input to the decision for both designs would be done by the large partner. One would like to think that performance and cost will be the only criteria in the decision, but corporate decisions are rarely this simple. What happens after this decision was also unclear. The presentation said that a redesign incorporating the best of both would be developed. Validation, qualification, and durability testing will eventually be required, and it appeared that all of this would be done by the large partner, as the current budget ends with He testing.

Recommendations for additions/deletions to project scope:

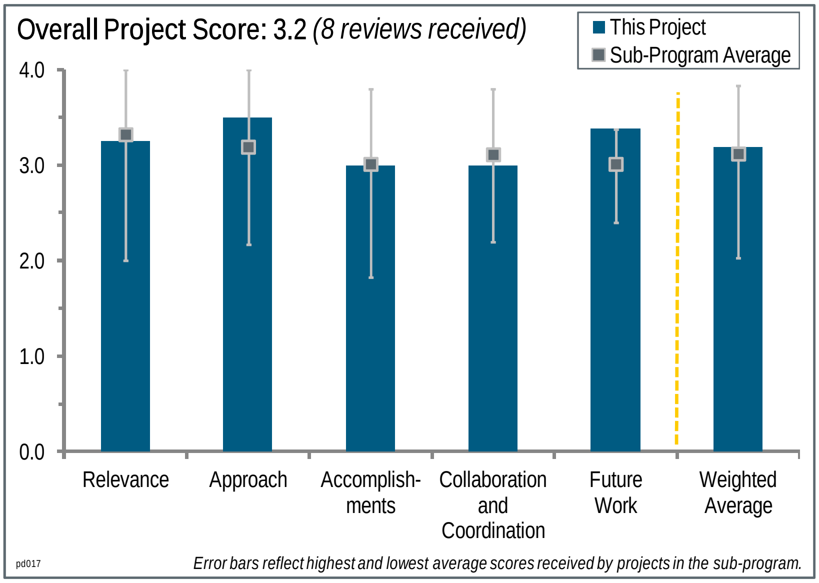
- This reviewer recommends performing cost analysis compared to other technologies, as well as material compatibility tests.
- Additional testing at national laboratories (possibly at NIST in Boulder, Colorado) need to be completed to study mechanical loading.
- More attention is still needed on material issues in this H_2 environment. It is not clear exactly what the testing at SNL and NIST will entail. It is imperative that the single-stage test unit is run with H_2.
- Testing in H_2 should be added.
- The investigators should test with H_2.
- The process for material selection and testing in the presence of H_2 should be accelerated and finalized. In particular, the selection of titanium for the rotor should be justified and validated for the given environmental operating conditions.

Project # PD-017: Development of a Centrifugal Hydrogen Pipeline Gas Compressor

Frank Di Bella; Concepts NREC

Brief Summary of Project:

The objective of the project was to design and model an advanced centrifugal compressor system for high-pressure hydrogen (H_2) pipeline transport to support: (1) delivery of up to 1 million kg/day of pure H_2 to forecourt stations at less than $1/gge with less than 0.5% leakage and pipeline pressures of >1,200 psig, (2) a reduction of initial system equipment costs to less than $6.3 million (U.S. Department of Energy [DOE] model), (3) a reduction in operating and maintenance costs through improved reliability, and (4) a reduction in system footprint.



Question 1: Relevance to overall DOE objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The work performed to date is valuable, however, as the requirements for the H_2 and fuel cell markets develop, the relevance of this development work has receded. If and when high-pressure H_2 pipelines are needed, the work presented will no longer be relevant to the commercial markets.
- Pipeline delivery of 99.99% H_2 at <$1/gge with 98% efficiency is aligned with the goals of the Hydrogen Production and Delivery sub-program.
- This project addresses the cost of large-scale gas compression as a significant economic barrier.
- The third phase is construction and validation while staying focused on state-of-the-art structural analysis. The problem is an aerodynamic issue, given the speed of the compressor.
- It is critically important that the energy spent compressing H_2 is minimized through the development of new, scientifically sound compression technologies and robust designs. The safe and reliable operation of these is a critical component of meeting this cost objective and ensuring public safety.
- This project is relevant to the success of the U.S. DRIVE Partnership program.
- The project is extremely relevant. Compressor technology is an essential component of H_2 transport. Issues related to system aerodynamics, materials selection, and performance in the presence of H_2 need to be addressed.
- Cost-effective and reliable H_2 compression to 1,000–1,500 psi for pipeline transport and for use at terminals and other modest-pressure bulk storage facilities will ultimately be vital for the viable use of H_2 as a major energy carrier. Currently, only reciprocating compressors work for these applications, and they can be unreliable, forcing the installation of backup units, which increases capital costs. Centrifugal compression technology that can be advanced to be effective with H_2, such as that being developed in this project, could result in more cost-effective and reliable H_2 compression. The widespread use of pipelines for H_2 transport is not likely to occur until H_2 has made considerable penetration into the transportation market. Recent analyses of H_2 delivery indicate that pipeline compression is only a relatively small part of the cost of delivering H_2. There are other, more pressing H_2 delivery issues that need to be resolved, such as refueling station compression costs and H_2 storage costs. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. The projected cost of the centrifugal compressor under development in this project is $4 million (for 240,000 kg/day). This is about equal to the current (2010) costs in the recently updated and posted Hydrogen Analysis Delivery Scenario Analysis Model (HDSAM) Delivery Model. The Hydrogen and Fuel Cells Program (the Program) is targeting to reduce this cost.

Question 2: Approach to performing the work

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

- This project features a very systematic and professional technical approach.
- The goals are well defined. The work will satisfy the original feasibility design.
- The conceptual development of this system is admirable; however, the real challenge in developing a technology of this nature rests with testing and manufacturing programs, which this organization is not really prepared to do in a rigorous manner.
- The compressor, if successful, may have applications to improving natural gas compression reliability so the risk can be shared by multiple industries. Also, the next phase of advancement includes building the centrifugal compressor with commercially available proven parts, such as bearings and seal technology. This, hopefully, will reduce manufacturing performance risks and costs. Once assembled, performance testing will commence and better manufacturing and installed cost estimates should be forthcoming.
- The presenter clearly defined his approach and the goal of meeting the requirements using a maximum amount of commercially available supplies and equipment in order to keep the per-unit cost down. The cost of any compromises this approach induces was unclear from the presentation.
- Concepts NREC planned on using existing equipment to design the needed compressor.
- The approach involves aerodynamic analysis of the entire system and its individual components. In this regard the project looks promising, if cost could be further brought down.
- The project is taking an excellent approach to achieving its objectives. It is using state-of-the-art centrifugal design methods, including finite element analysis (FEA). It is incorporating the currently available state-of-the-art tilt-pad bearings, gas face seals, and other components to improve the probability of success while focusing on the design and materials for the rotors needed to incorporate the breakthrough revolutions per minute (rpm) into the design. The project includes close attention to the material selection relative to required strengths and H_2 embrittlement. Testing of components in H_2 is an integral part of the project. Building a single-stage prototype for rigorous testing and evaluation, including operating it with H_2, will yield critically valuable results. The project includes collaboration with Air Products, which has extensive H_2 compressor experience, as well as centrifugal compressor experience all under commercial use conditions. It is also doing work at Texas A&M University and Savannah River National Laboratory (SRNL) on testing materials in H_2 environments. Finally, the researchers are getting advice from Sandia National Laboratories (SNL) and Dr. Sofronis at the University of Illinois, who are arguably the best in the world relative to material issues in H_2 environments. The testing and choice of aluminum for the rotors appears to be resulting in a much lower costs compared to the use of stainless steel to withstand the H_2 environment. Parts of the total compressor are lubricated with oil. Good attention has been paid in the design to try to eliminate the possibility of oil contamination, but only testing will confirm if there is no oil contamination of the H_2. It is not clear whether the work being done on coatings is of significant value, because suitable and reasonably priced materials of construction have been identified.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.0** for its accomplishments and progress.

- The organization has performed good work in the design phase of this effort.
- Until the compressor is built and fully functional, it is hard to say whether the design exercise to date has been worth it.
- There have been very promising results for Phases I and II. DOE proposal requirements were satisfied with the feasibility design. This reviewer gives the project only three points due to the lack of material analysis and tests.
- The presentation made it clear that progress is being made at an acceptable rate and that system validation testing should be completed early in the next fiscal year. Continued testing in H_2 will be important if the unit performs acceptably in other testing.
- The team has made fairly good progress. It needed to down-size from a two-stage prototype to a one-stage prototype due to cost.
- The project team has made excellent progress with developing a 6-stage compressor to meet the delivery requirements. This reviewer is not sure about the materials selection to avoid H_2 embrittlement concerns. Mass

production with aluminum casting will increase the potential of casting voids. Low-strength steels may work as well; however, they may be more comparable for high-volume production.
- Very good progress has been made and the results to date are very encouraging. Critical components have been developed or specified for near-term availability. Detailed design and cost analysis of the full-size, six-stage compressor has been completed. A one-stage laboratory unit has been designed—parts are being procured and it is being assembled. Critical materials of construction questions relative to the strength and a hydrogen environment are being addressed through both testing and expert advice. It appears the project has fallen a bit behind schedule, with testing of the prototype compressor to be completed in March 2013 versus November 2012. The compressor, subcomponents, and single-stage test unit have been designed. The single-stage test unit has been fabricated and assembled with some testing and results completed. Material issues and selection for the H_2 environment have been discussed with experts in the field, and initial material selection has been completed. However, testing these materials appropriately in H_2 and under operating conditions has not yet been done.
- The overall system analysis is appropriate and the proposed design meets the DOE target requirements. Several milestones have been reached: a full load laboratory prototype has been designed, an algorithm for emergency shutdown is in place, and FEA stress and aerodynamic analyses of all of the components have been performed. On slide 14, it is stated that the compressor has been successfully spun to 10% over speed for 15 minutes. Item 3 of slide 14 reports low blade frequency. It is not clear why the blade frequency is low.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- Collaborations cover the required expertise. Industry is involved.
- There is excellent coordination with industry subject matter experts to oversee the progress. This reviewer suggests adding vibration measurements in the single-stage system being built for future testing.
- This project features an excellent group of partners and planned collaborations.
- There is great cooperation with other vendors for compressor module as well as national laboratories and Air Products.
- This reviewer noted collaborations with Texas A&M and Air Products. Given the respected interests (coatings and design), these are good connection points and should be developed further. Given the early design and development status, it seems vital to have collaborations that target the eventual licensing and development testing of the compressor design.
- Praxair has been replaced by Air Products. This makes one wonder if this was due to a significant issue. Otherwise, if this is a reliable approach, it is unclear why a large producer of H_2 would lack interest. This reviewer is also curious about the reliability of Air Products as a collaborator and a potential tester of the field compressor. Other than these questions, the project appears to be well balanced in terms of collaborators.
- The project includes collaboration with Air Products, which has extensive H_2 compressor experience as well as centrifugal compressor experience all under commercial use conditions. It is also doing work at Texas A&M University and SRNL on testing materials in H_2 environments. Finally, it is getting advice from SNL and Dr. Sofronis at the University of Illinois, who are arguably the best in the world relative to material issues in H_2 environments. Collaboration with a commercial producer/supplier of centrifugal compressors and possibly a commercial producer/supplier of very large H_2 reciprocating compressors would add strength to this project team.
- The reported partnership with Air Products is good and collaborations with SRNL and SNL are important if they are substantial—for example, as stated on slide 20 for work with Dr. San Marchi. The collaboration with Texas A&M is not contributing to the project's health and robustness. In fact, this collaboration is a serious project weakness. The technical approach of Texas A&M on assessing H_2 embrittlement through the punch test is an approach the ASME codes and standards committee does not even consider for discussion. Force-displacement data (e.g., uniaxial tension test) are used for material screening purposes only; in fact, there is no underlying physics that justifies transferability of these data to fracture toughness estimation.

Question 5: Proposed future work

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

- The project is nearing completion in meeting the target goals.
- The project is a bit rushed on the future plans for the Phase III building compressor.
- The project team should make sure the materials are compatible.
- The control system has yet to be built. It was unclear if it has been modeled under operating conditions to validate it. Future plans are to have the unit tested at a future site still under consideration.
- The proposed future work appears to be on a slow path through system development. At this point, for the technology to be relevant for H_2 service or maybe other applications, it needs to be put into the hands of a competent test and commercial design organization that knows how to commercialize technology of this nature. One reviewer is doubtful that Concepts NREC can achieve the ultimate objectives.
- Another reviewer wonders if there will be an impact on future maintenance costs if the unit is hermetically sealed. Also, in consulting with Air Products and Praxair, this reviewer wants to know if the researchers really think that this compressor will be so reliable that they will not need a redundant compressor.
- The future work is well thought through and designed. It includes completing and fully testing and evaluating the laboratory prototype test unit with H_2. Further testing of materials is also planned. It would be better if the design and cost estimate of the complete commercial size compressor were to be reviewed and updated based on the results of the single-stage prototype testing. It is not clear that the work planned on coatings is of significant value because suitable and reasonably priced materials of construction have been identified.
- The proposed future work is reported on slide 21. The proposed assembly as a completely functioning compressor system and the installment of a laboratory prototype system are appropriate tasks to be pursued. However, testing of the entire system in the presence of H_2 is missing in the proposed work. It is proposed that coatings will be investigated in the presence of H_2. It is not clear why coatings are needed and what components of the compressor are required to operate with coatings. In fact, nowhere in the presentation was there any mention of a deficiency of the structural materials, as they would operate in the presence of H_2 and the absence of coatings mentioned.

Project strengths:

- The concept design is an area of strength.
- The design and engineering expertise appears to be sound.
- One strength is the industry collaboration. The project has produced very promising results compared to DOE targets.
- The project features strong industry partnership with excellent oversight of work.
- The team has produced a real, working one-stage demonstration for system validation testing in fiscal year 2013.
- Good progress with building a new compressor using existing components.
- A strength of this project is the complete aerodynamic and system analysis and the choice of Aluminum 7075-T6 as a rotor material. However, given the low strength of the aluminum alloy and that the rotor will operate at 66,000 rpm, the project should report the safety factor used in the stress analysis (see slide 12). It was unclear if the safety factor is appropriate according to standard practices in compressor technology.
- Cost-effective and reliable H_2 compression to 1,000–1,500 psi for pipeline transport and for use at terminals and other modest-pressure bulk storage facilities will ultimately be vital for the viable use of H_2 as a major energy carrier. Currently, only reciprocating compressors work for these applications, and they can be unreliable, forcing the installation of backup units, which increases capital costs. Centrifugal compression technology that can be advanced to be effective with H_2, such as that being developed in this project, could result in more cost-effective and reliable H_2 compression. The project is taking an excellent approach to achieving its objectives. It is using state-of-the-art centrifugal design methods, including FEA. It is incorporating the currently available state-of-the-oil art bearings, seals, and other components to improve the probability of success while focusing on the design and materials for the rotors needed to incorporate the breakthrough rpm into the design. The project includes close attention to the material selection relative to required strengths and H_2 embattlement. Testing of components in H_2 is an integral part of the project. Building a single-stage prototype for rigorous testing and evaluation, including operating it with H_2, will yield critically valuable results. The project includes collaboration with Air Products, which has extensive H_2 compressor experience as well as centrifugal compressor experience

all under commercial use conditions. It is also doing work at Texas A&M University and SRNL on testing materials in H_2 environments. Finally, it is getting advice from SNL and Dr. Sofronis at the University of Illinois, who are arguably the best in the world relative to material issues in H_2 environments. The testing and choice of aluminum for the rotors appears to be resulting in a much lower costs compared to the use of stainless steel to withstand the H_2 environment. Very good progress has been made and the results to date are very encouraging.

Project weaknesses:

- There is no clear pathway to commercialization.
- It is not certain that the project will meet the reliability assumptions and hence eliminate the need for compressor redundancy.
- This reviewer is not sure that exploring titanium components with Texas A&M is useful work for the project because aluminum rotors seem to work acceptably.
- Durability testing should be included if the design passes the functional validation testing. While the project has an excellent group of collaborators and researchers, they almost appear to be included as an afterthought. That is, this work appears to be lagging behind the design and construction of the one-stage demonstration, indicating that they started work after the critical decisions were made and had little input to this part of the project even though that is the role of this work. Of course, in this case, late may be better than never, depending on how the system validation and durability test turn out.
- No testing of the compressor in the presence of H_2 has been reported. Assessment of H_2 embrittlement of materials is done through an inadequate test. In fact, no assessment of the behavior of chromoly shaft steel and the Nitronic 50 material to be used for the enclosure has been reported. Given the significance, magnitude, and variability of the compressor cost, the two compressor projects involved in the sub-program need to be given priority and carefully administered and reviewed.
- The widespread use of pipelines for H_2 transport is not likely to occur until H_2 has made considerable penetration into the transportation market. Recent analyses of H_2 delivery indicate that pipeline compression is only a relatively small part of the cost of delivering H_2. There are other more pressing H_2 delivery issues that need to be resolved, such as refueling station compression costs and H_2 storage costs. Based on the cost estimates provided in the presentation, it is not clear if this technology will be sufficiently cost effective. The projected cost of the centrifugal compressor under development in this project is $4 million (for 240,000 kg/day). This is about equal to the current (2010) costs in the recently updated and posted HDSAM Delivery Model. The Program is targeting to reduce this cost. It would be better if the design and cost estimate of the complete commercial size compressor were to be reviewed and updated based on the results of the single-stage prototype testing. Collaboration with a commercial producer/supplier of centrifugal compressors and possibly a commercial producer/supplier of very large H_2 reciprocating compressors would add strength to this project team and improve on the final design of the compressor.
- There are no major weaknesses.
- This reviewer felt that there are no weaknesses.

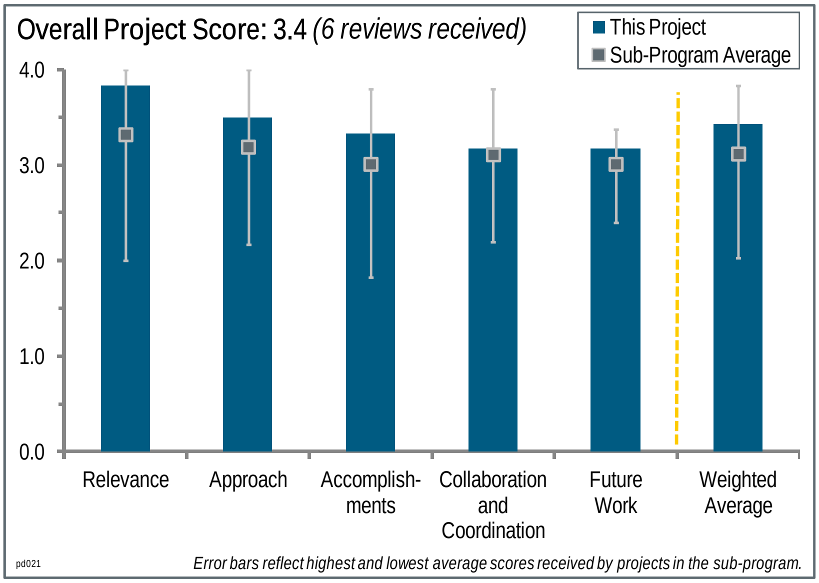
Recommendations for additions/deletions to project scope:

- This project should be brought to a conclusion and Concepts NREC should be encouraged to license the concept to organizations that are interested in funding its commercial development.
- The project team should extend industry collaboration, if possible, to ensure market relevance.
- Funding should be included for validation and durability testing if the system passes the design validation tests.
- This reviewer has no recommendations at this time, other than to review the titanium work with Texas A&M.
- The project should assess the overall system and component behavior through operation in the presence of H_2. The team should seek out expertise at SRNL, SNL, or the National Institute of Standards and Technology.
- It would be better if the design and cost estimate of the complete commercial-size compressor were reviewed and updated based on the results of the single-stage prototype testing. Collaboration with a commercial producer/supplier of centrifugal compressors and possibly a commercial producer/supplier of very large H_2 reciprocating compressors would add strength to this project team and improve on the final design of the compressor. It is not clear that the work being done on coatings is of significant value, because suitable and reasonably priced materials of construction have been identified.

Project # PD-021: Development of High Pressure Hydrogen Storage Tank for Storage and Gaseous Truck Delivery

Don Baldwin; Lincoln Composites

Brief Summary of Project:
To reduce the cost of near-term transportation of gaseous hydrogen (H_2) from the production or city gate site to the fueling station, this project aims to design and develop the most effective bulk hauling and storage solution for H_2 in terms of cost, safety, weight, and volumetric efficiency. A tank and corresponding International Organization for Standardization (ISO) frame will be developed that meet or exceed U.S. Department of Energy (DOE) low-cost, high-volume goals and can be used for the storage of H_2 in a stationary or hauling application.



Question 1: Relevance to overall DOE objectives

This project was rated **3.8** for its relevance to DOE objectives.

- High-pressure trucks are a critical component of reducing H_2 delivery costs.
- This project has good relevance for the early stage of the transition to H_2. It would be necessary to make this point by contrasting this option with liquid, pipelines, and cold-compressed delivery.
- Lincoln Composites has tackled most of the barriers and has received special permits for the new, larger systems. The targets focused on near-term cost and weight look to be completed. Technical targets were met, but they are dependent on the cost of the carbon fiber, which is outside the project's scope.
- Unless this reviewer is mistaken, tube trailer delivery is still expected to be the most cost-effective means of delivering H_2 until significant market penetration occurs. Therefore, lowering the cost of this means of delivery is critical to enabling the initial stage of market penetration beyond limited fleet vehicles such as forklifts. Therefore, this is a very relevant project with a clear focus and goal.
- The relevance of this project has increased in recent years because reducing the high cost of compression at the station could be achieved by utilizing pressurized truck tube delivery. It is essential for the DOE Hydrogen and Fuel Cells Program to have a viable infrastructure.
- Carbon fiber composite storage manufacturing technology is essential technology for 21st century mobility for onboard vehicle storage, and hopefully it will soon be more widely seen as proven technology for a wide range of H_2 storage applications: bulk H_2 transport, zero-emission consumer products, and cascade fueling of high pressure vessels (a necessary component of the local H_2 fueling station). Low-cost carbon fiber composite pressure vessel technology for mass production is employed today for worldwide use of 5,000 psig paintball cartridges, and translucent liquefied petroleum gas LPG containers developed mass-market end-user small appliances in Europe. Carbon fiber technology began with military and aerospace applications, but the manufacturing technology will serve high-volume consumer applications, H_2 fueling stations, and transport applications. The use of composite H_2 vehicle storage tank technology in stationary or transportable pressure vessel applications makes a lot of sense from an economic and physical property of material perspective. For many applications, compared with incumbent steel pressure vessel technology, composite storage systems can contain H_2 at higher pressure with a lower container mass, lower material cost, and lower manufactured cost. Steel pressure vessel technology has a long commercial history; the introduction of composite pressure vessel technology is an upset to the steel market that is being stressed by the material properties of supercritical H_2 and steels. Over the past decade, carbon fiber composting pressure vessels have been used for numerous fleets as

overhead compressed natural gas (CNG) bus fuel tanks with an excellent service record, and smaller, more limited scope composite carbon fiber storage tank transport projects have received U.S. Department of Transportation (DOT) special permits. After extensive testing and work by the principle investigator (PI) on this project, DOT approvals will result in new “American Built” commercial tube trailers and ISO shipping package design. These new products can be used in the United States to carry renewable H_2 to fueling stations and throughout the world to support U.S. Department of Defense requirements, such as the transport of compressed helium and H_2 to forward bases.

Question 2: Approach to performing the work

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

- The approach includes good work related to developing and testing large vessels.
- The approach is good, and it is clearly explained on slide 6.
- The approach is well defined, and has some adjustments in the construction and environmental testing of loads. Trade studies on 350 bar is the next logical step. The team could stay within the limits of the existing frames while dropping the cost per kilogram of H_2 delivered.
- Lincoln Composites has carried out a systematic project to increase capacity and control costs to produce an optimal design for H_2 transport. The ISO-compatible design should allow the wide adoption of tanks.
- With the goal and focus clearly defined, it would be difficult for a team with this experience to not come forward with an excellent approach, and the presentation supported this view. Every issue, future or past, seemed to be in the plan, and the team seemed to be making excellent adjustments as it progressed.
- This project is a large step forward and will allow for the use of these carbon composite commercial products in H_2 supply and distribution projects that will support the deployment of fueling stations to support the early pre-commercial fleets of fuel cell electric vehicles (FCEVs) that will be deployed beginning in 2015. The PI and DOE project management have done an excellent job of identifying barriers, including commercial barriers and fear-, uncertainty-, and doubt-related code barriers that limit the current pressure rating at 3,600 psig. There are a number of physical property aspects of H_2 that are not well understood and are still subject to some mystery and commercial intelligence. From a commercial perspective, fear, uncertainty, and doubt will manifest as a perceived threat to future business and efforts will be made to lobby against the progress of an H_2 economy. From a code perspective, fear, uncertainty, and doubt sometimes manifest as a need to keep safety levels at the highest threat levels.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.3** for its accomplishments and progress.

- The project meets many of the targets.
- While the lack of commercial pull to take this technology to the next pressure level of 350 bar (5,000 psig) is limiting the potential for higher payloads today, the out-of-the box thinking has resulting in a >5 tube trailer design that contains 26% more H_2 than the four-cylinder ISO frame model that was initially the focus of this project.
- Excellent progress has been shown toward meeting cost targets; however, the stated impossibility of measuring market size is a function of lack of market demand. The challenge is determining how to meet the market demand while justifying the business models.
- Production of tanks and DOT approval are significant milestones for the Hydrogen Production and Delivery sub-program.
- It is unfortunate that this project will not include the development of a 350-bar trailer.
- The presentation made a strong case for clear, consistent progress in the evaluation of tanks and qualification testing. A good case was also made for the decision to shelve the 350-bar system until a sounder business case could be made.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- It seems like collaborations are sparse. One reviewer wonders if the researchers could benefit from working with other institutions for improved vessel design, construction, etc.
- The PI and project team have worked well with DOT and ASME to help establish precedence for commercial tube trailers using carbon composite pressure vessels.
- There has been close collaboration with the American Bureau of Shipping to meet the regulations and standards. The team is also working with DOT on special permits.
- Lincoln Composites has worked well with regulatory agencies to obtain a permit for the Titan trailer.
- The collaboration with DOT was good. Further collaboration is recommended with the potential customer base.
- The presentation made it clear that collaborations are under way in terms of vessel qualification testing (American Bureau of Shipping, DOT), and that collaborations will be required for system components such as fire systems, valves, and manifolds. Planned collaborations and coordination in the future work were unclear. However, this group has considerable experience, and it is unclear how much further collaboration is necessary for the team to meet the program objectives and goals. Everything seems to be well on track and on a good schedule.

Question 5: Proposed future work

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

- The >5 tube trailer concept should be submitted to DOT for approval. Continuing development of the improved fire protection systems is a good focus for future work.
- The team should pursue future work with Canada and work to increase compatibility to make the project more cost effective. It should also add laboratory space to study pressurized effects with polymers and supporting equipment.
- Titan 5 and Titan 5+ designs are a good extension of current technology. Achieving 350 bar would be nice, but Lincoln Composite’s market-based decision not to seek approval was a good one.
- Everything necessary for qualification and licensing seems to be taken care of or in the plan. The issuing of a special permit by DOT supports this conclusion. If the investigators are considering adding smaller tanks on either side of the larger bottom tank, another question might be how large of a tank could be put up the middle of the four-tank upper array.
- The future work is fine, but it would be useful to also include specific actions and benefits to the project. Also, the cost studies are in the approach section, but they were not highlighted in the future work. The cost study would be an important task for this project.
- It is not clear if cancelling the 5,000 psi effort negates the potential advantages of this concept. The whole point, as this reviewer understood it, was to minimize truck and driver cost by increasing the amount of H_2 delivery per trip. This reviewer wonders if the PI can claim good progress toward achieving DOE goals at 250 bar. Abandoning the 350-bar effort due to uncertainty in future market demand seems unreasonable. If everything Lincoln Composites does is ruled by future market viability, it is unclear why the company accepts DOE money. This reviewer wonders if it should pursue the work on its own.

Project strengths:

- This project features an experienced team and good infrastructure.
- The project is building on an already available commercial product for CNG.
- These new “American Built” commercial compressed gas transportation products can create jobs and be used to bring small, centralized H_2 production to local distribution points. Another strength is the distribution (delivery) technology that enables the deployment of fueling stations to support the early pre-commercial fleets of FCEVs.
- The project benefits from market pull from natural gas markets, resulting in a very high degree of leverage of DOE dollars. Lincoln Composite’s expertise and experience have contributed to the success of this project.

- This project features an experienced company; a clear focus on the most cost-effective design; evidence of good, clear decision making; an excellent approach and plan; good accomplishments; experience with DOT licensing requirements; and a special license.

Project weaknesses:

- As this reviewer understands it, the project will now focus on qualifying components for 250-bar compressed H_2 storage. It would seem that this would be well-known technology.
- Commercial deployment and investment in new H_2 distribution systems, including high-pressure tube trailers, is quite limited due to the lack of short-term vehicle volume at the dispenser to cover station development costs. This lack of current commercial market for a 350-bar (5,000 psig) tube trailer has limited the ability of the project sponsor and the PI to justify the investment in certification of 350-bar tubes for this transport application.
- Cost-savings opportunities may be limited. This reviewer wonders if more materials testing and fatigue testing should be included. While monitored service is a proven technique for testing on the fly, it is unclear whether H_2 could afford a slip; that is, one catastrophic failure could prevent this technology from gaining acceptance and penetrating the market. There appears to be very little concern with the supporting materials of the manifolds, gauges, valves, etc.
- The project needs to align the future work with the approach outline and provide further details regarding the approach used to estimate the results that are compared to the DOE targets. In particular, the cost estimate needs further explanation.
- This project has no weaknesses.

Recommendations for additions/deletions to project scope:

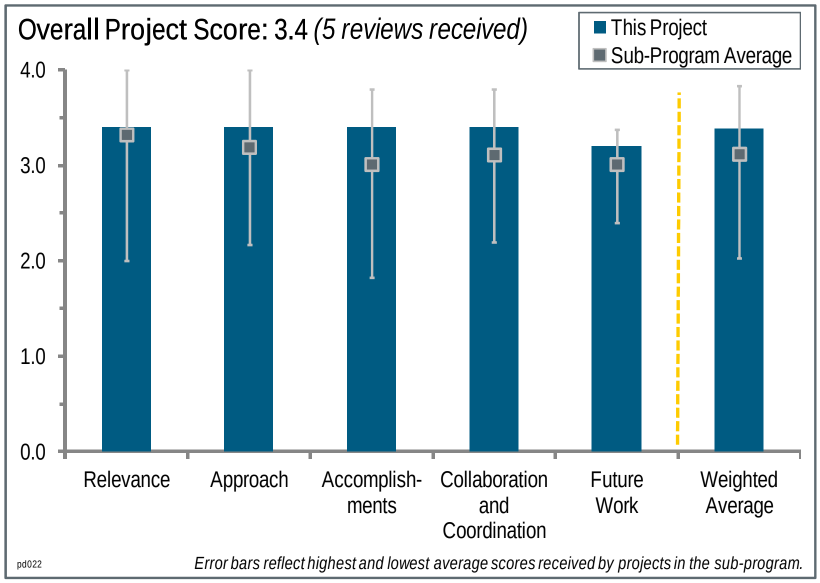
- Leaving the 350-bar option in the project shows the potential for future developments, even though there is no current market pull to higher density for distribution.
- The team should expand qualification testing of components and assembled systems, including safety systems.
- Investigators should add further interface with potential customers to define the market. They should also add the details of the cost study, and add the lessons learned from the existing CNG commercial trailer experience.

Project # PD-022: Fiber Reinforced Composite Pipelines

Thad Adams; Savannah River National Laboratory

Brief Summary of Project:

The objectives of this project are to provide test data to support a technical basis for fiber-reinforced polymer (FRP) pipelines in hydrogen (H_2) service and to integrate FRP pipelines into ASME B31.12 Hydrogen Piping and Pipeline Code by 2015. A proposed demonstration will facilitate codification and public acceptance and provide a test case for permitting. The fiberglass pipeline will serve as a test and surveillance facility and as a final proof-of-concept for FRP pipeline in H_2 service. The facility will have an integrated educational component for the public.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- FRP has the potential to significantly reduce pipeline costs.
- The project is extremely relevant. It assesses the structural integrity, safety, and reliability of FRP pipelines.
- This project is bringing together ASME and industry pipeline companies to demonstrate the viable use of FRP pipelines to transfer H_2. The goal is to get FRP into ASME codes by 2015.
- Steel pipelines for H_2 transport have high costs due to embrittlement concerns and the cost of installation due to welding joints. This potentially offers a lower-cost solution.
- Because FRP pipelines could dramatically lower the cost of installing pipelines and delivering H_2, developing test methods and data for the qualification of FRP pipes for H_2 service is critical to the longer-term goals of the DOE Hydrogen and Fuel Cells Program. This project has identified the critical issues, experiments, and priorities, and is conducting the required tests.

Question 2: Approach to performing the work

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

- The project includes excellent fatigue testing results for FRP.
- The project has a well-defined goal of incorporation into codes by 2014. Tests appear to have been well planned and executed.
- This is a decent effort at getting ASME codes to be revised for FRP pipe.
- The project relies on burst testing, as well as fatigue testing of flaws and unflawed pipelines (see slides 6–9). Also, the project involves a large coordination/collaboration effort (see slides 10–13).
- The team has identified potential failure modes and materials issues and has conducted a series of tests to verify the ability of these materials to meet the conditions of service. The team is collaborating with ASME and others to better identify the test methods and data that would be required to qualify a material and design for service. This includes a review of existing FRP pressure pipe codes and standards and workshops on H_2 service.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.4** for its accomplishments and progress.

- It appears that the project barriers and objectives are being met.
- The workshop with ASME was a good step toward FRP approval.
- The team has made good progress concerning getting codes in line, fatigue testing, flaw tolerance, etc.
- The project team has identified issues, experiments, and priorities. The team is conducting tests in accordance with its established priorities and is clearly making significant progress. Experiments current, past, and future all line up in a nice, logical sequence.
- The comparison in the behavior of the flawed and unflawed pipelines is a significant result. At this point, the third bullet item on slide 8 is hard to follow: it is unclear whether it implies that the unflawed sample is less durable than the sample with fatigue damage. In addition, the identification of how the failure progresses through fiber delamination from an inserted flaw is a significant milestone of this project.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- The collaboration with ASME on B31.12 helps speed the process for codes and standards. Looking at other standards is an excellent approach to making sure that FRP will be accepted.
- This project features good work with ASME.
- Good cooperation with partners on the South Carolina demonstration project.
- The collaboration with ASME is important and should continue.
- This is one of the best projects for interactions and collaborations. In the case of this project, it is essential that the investigators interact with designers, code setting organizations (e.g., ASME), and regulators to determine the data they would require to design, qualify, and permit this type of material for H_2 service, and they have done an outstanding job of working with collaborators to identify issues and prioritizing tests.

Question 5: Proposed future work

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

- A new demonstration project will provide some real-time data on H_2 pipelines.
- Full-scale service emulation-type long-term testing is an excellent approach for a material that has not yet seen real H_2 service, and there is little if any data on the kinetics of any potential failure mode.
- The team needs real test data on FRP performance. One reviewer suggests that the team try both and compare data for trench less and trenched installations. The team may want to incorporate active monitoring to prevent third-party damage. B31.12 ASME code needs to include FRP.
- The sub-program needs to carefully define what it can learn from a large-scale pipeline demonstration. This would be a high-cost project. A cost-benefit analysis needs to be carried out to ensure a good return for DOE's investment. Work that contributes to ASME approval should be the top priority. This reviewer is not sure that installing a pipeline is good for public opinion of H_2.
- The proposed future work was described on slides 14–19, but the focus was mainly on interactions for codes and standards and demonstration. A description of what questions need to be answered was missing. Slide 21 mentions "perform long term stress rupture tests for flawed FRP samples," but this statement is rather vague. No specifics were mentioned as to what new information is sought relative to what has already been reported.

Project strengths:

- The departure from conventional steel pipe offers a chance for a lower-cost solution.
- The spooling of pipeline reduces insulation cost. Stress cycle testing looks to be a strength of this project; however, the data may need to be run beyond the 8,000 cycles. Partners for demonstration are identified.
- Strengths of this project include collaborations with other laboratories, codes setting organizations, and regulators to identify test needs and priorities; consistent progress in testing in accordance with priorities and

goals; and coordination with local government for the full-scale H_2 pipeline testing facility. It is nice to see this material being subjected to the same kind of testing scrutiny that other materials have to go through to be accepted and qualified for use in this or similar service conditions.
- The project is moving in the right direction. The focus on how a flawed pipeline responds to fatigue loading is clear, and the related results so far on the failure mechanism are significant. The leadership of G. Rawls to steer the project in this direction is notable because this is the first time that a serious structural integrity assessment of FRP pipelines has been reported in a DOE Hydrogen and Fuel Cells Program Annual Merit Review meeting since 2006.

Project weaknesses:

- The team should provide more definition of additional fatigue testing.
- The team needs to perform a long-term stress rupture test on FRP that has been installed. The team should also monitor soil moisture and pH.
- The main weakness in the past was the poorly defined requirements for getting materials of this type accepted for H_2 service and used by designers. This has largely been solved through the development of collaborations. Now that the criteria are being set and the critical data is being obtained, this reviewer would like to see more tests, including the combined effects of absorbed H_2 and water because the pipes will be buried. Essentially, this is included in full-scale testing, but this reviewer would find quantified answers more scientifically appealing.
- Fatigue tests have shown that flawed pipeline life is affected relative to unflawed pipeline life. However, there were no conclusions drawn as to how these results can be used to predict, for example, the remnant life of a flawed pipeline. If FRP pipelines are used for H_2 transport, rigorous requirements for life assessment must be used as plans are made to use da/dN versus DK curves to design steel pipelines. The project's approach needs to be expanded by identifying the parameter space of the potential failure modes that need to be systematically investigated. Certainly, steps toward this direction have already been taken (e.g., how failure progresses from the presence of a flaw), but potential tests to understand and evaluate the structural integrity, for example, of the liner/reinforcement interface, have not been identified.

Recommendations for additions/deletions to project scope:

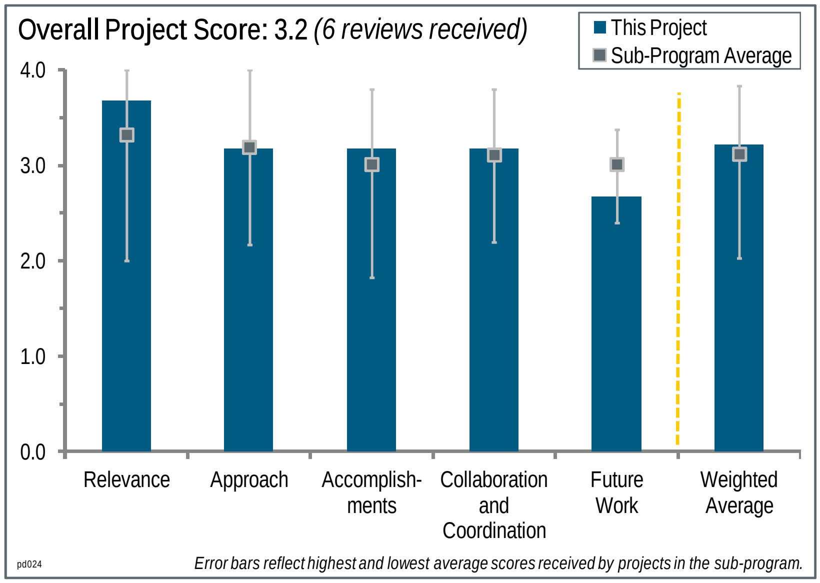
- The team needs to study the trench-less process that most gas industry companies are now using.
- The team should continue with the demonstration project in South Carolina.
- More funds should be made available so that the testing matrix can be expanded. Because the pipelines will be buried, this reviewer would like to see more on the combined effects of absorbed water and H_2, including on the following topics: (1) whether water absorption will change the permeation rate of H_2, (2) whether the permeation of water through the pipe will contaminate the H_2, (3) whether the two combined will influence fatigue performance or burst strength. Essentially all of this is included in full-scale testing, but this reviewer would prefer more scientifically quantified answers.
- The project should proceed beyond comparing and quantifying individual test cases (flawed versus unflawed FRPs). The project should start developing conclusive knowledge that can assist with predicting pipeline remnant life and safety. This can be accomplished by thoroughly identifying and quantifying potential failure mechanisms.

Project # PD-024: Composite Pipeline Technology for Hydrogen Delivery

Barton Smith; Oak Ridge National Laboratory

Brief Summary of Project:

The objective of this project is to address the barriers of: (1) pipeline capital cost, reliability, and leakage; (2) the hydrogen (H_2) compatibility of pipeline materials; and (3) technology acceptance. The objectives specifically for the 2012 fiscal year are to: (1) complete high-pressure cyclic fatigue and stress-rupture tests of fiber-reinforced polymer (FRP) pipelines, (2) reassess FRP pipeline capital cost, and (3) collaborate on ASME codes and standards acceptance.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.7** for its relevance to DOE objectives.

- The project is extremely relevant. It addresses composite pipeline technology for H_2 transport.
- FRP has the potential to significantly reduce pipeline costs compared to steel. ASME certification is a necessary step toward the incorporation of FRP in H_2 use.
- Because FRP pipes could dramatically lower the cost of installing pipelines and delivering H_2, developing test methods and data for the qualification of FRP pipes for H_2 service is critical to the longer-term goals of the Program. This project has identified the critical issues and is conducting the required tests.
- Pipelines are an important part of the chicken-and egg-problem with infrastructure and autos. Without the pipelines, cars might not be part of the picture. Showing a long-term promise for reduced cost as the infrastructure grows is an important motivator. However, localized production may alleviate some of the long-distance transport issues.
- The barriers and technical targets are well designed. Capital cost and technical targets are defined by regulatory agencies. The codes and standards are on pace to be met. New cyclic fatigue testing is proceeding. The ASTM D2143 test is under consideration. It appears that it will meet the DOE 2020 goals.
- Pipeline transport and distribution of H_2 is a potentially viable and cost-effective delivery pathway. Current analyses done for the DOE Hydrogen and Fuel Cells Program (the Program) show that pipelines are the low-cost pathway to transport large amounts of H_2 long distances (greater than about 50 miles). Such transport is likely from a central H_2 production plant to terminals at or near city gates. This project is focused on utilizing fiber composite pipelines to reduce these transport costs compared to steel pipelines, which is necessary to achieve the Program's delivery cost targets. These composite pipelines might have additional advantages over steel if they can be engineered to include sensors for pipeline integrity. However, it is important to remember that the cost to transport H_2 from central production plants to terminals at city gates if the distance is less than 50 miles is only a small fraction of the total cost of H_2 delivery. The widespread use of pipelines for H_2 transport and distribution is not likely to occur until H_2 has made at least considerable penetration into the transportation market. Recent analyses show that distribution and service pipelines are more costly for H_2 distribution than recently developed higher-pressure tube trailers. In addition, urban areas may be reluctant to embrace a H_2 pipeline infrastructure due to safety concerns and/or the disruption its construction might cause. However, pipelines for H_2 transport from central production facilities to terminals might be far easier to employ and potentially the lowest-cost option for this part of a H_2 delivery infrastructure. As a result, pipeline transport might be very useful at even relatively low market penetrations. Although recent analyses show that distribution and service pipelines are more costly for H_2 distribution than recently developed higher-pressure tube trailers, if the cost of these small

diameter pipelines could be dramatically reduced, they might become the preferred option for H_2 distribution in urban areas once H_2 has become a major energy carrier.

Question 2: Approach to performing the work

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

- The project features good experiments, and it is well designed and focused on critical issues.
- The project has an excellent work scope and a very in-depth review of the progress.
- Cyclic testing under H_2 pressure is vital to ensuring pipe operability in an H_2 environment.
- This is a very small project, so reviewers need to be realistic about what can be accomplished. The project helps keep an eye on the future (i.e., polymer pipelines), without using a lot of resources needed for breakthroughs in limiting technologies, such as clean distributed sources of H_2 and improved fuel cells for consumer vehicles.
- The project aims to understand the durability, integrity, and safety of FRP pipelines through high-pressure fatigue testing. The project also investigates the H_2 permeability of the FRP pipelines. Certainly both durability and permeability are important and need to be understood and quantified. The issue is whether investigation of fatigue alone is enough to ascertain the reliability of the FRP pipelines. One reviewer wonders if there are any other failure modes.
- This year's work is focused on testing available composite pipe for pressure and temperature cycling, plus the impact of blow down. It also includes direct measurements of H_2 leakage after the cycling and blow down experiments. These are likely the most important and practical tests that can help start to qualify composite pipeline for H_2 service. The project now appears truly engaged with ASME to start to develop codes and standards for composite pipelines for H_2 transport use, which is very important. It is vital to work with ASME and stakeholders to tabulate a full list of testing that needs to be done to fully qualify composite pipeline for H_2 service and to run those tests. This is part of the future work. The team is performing a continued evaluation of composite pipelines and materials using the recently improved test equipment. This is important to verify acceptable leakage/permeation rates for H_2 pipeline service.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.2** for its accomplishments and progress.

- Testing has shown no real problems.
- The project is making acceptable progress, considering the low (but appropriate low) funding. The results are not awe inspiring, but they seem to be solid, and if there was a huge problem with the fiber-reinforced pipe, the authors would have found it.
- The team has achieved good accomplishments over the past year. The team may need to converse with industry vendors that are doing trench-less installation to determine current cost.
- Considering the very small amount of funding provided for this project in 2012, very good progress has been made. Testing on available composite pipe for pressure and temperature cycling plus the impact of blow down has been done with very promising results. Direct measurements of H_2 leakage after the cycling and blow down experiments were also done. These tests showed no leakage from the pipe itself, very low permeability through the pipe, and some measurable but low leakage through the pipe joining fittings. These measurements are very encouraging and point to the joining fittings as the area for more careful and accurate measurements of leakage issues in the future. The team is also continuing to evaluate the H_2 permeability of composite pipelines and materials using the recently improved test equipment. This is important and showing promising results. The level of dependence on temperature and H_2 pressure is also being determined.
- Slide 17 states that no blistering or delamination of liner was evident during visual inspection of the liner following pressure blow down. Slide 18 reports that quality assurance testing of the pipeline was examined by Fiberspar, and no loss of performance capabilities of the tested pipeline was identified. On slide 16, a large leak is reported at a pressure transducer port due to a bad seal. On slide 20, an updated capital cost for the FRP pipeline is reported, and slide 21 states the FRP pipelines provide a 15% overall cost reduction in comparison to steel. A summary of the estimated costs is provided on slides 23 and 24. Lastly, slides 26 through 29 provide information from solubility and permeability tests.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- The project features good coordination with ASME and others. The team should be sure to include polymer producers.
- This project has widespread collaboration that includes the principle composite pipeline manufacturers, polymer manufacturers, Savannah River National Laboratory (SRNL), the DOE Pipeline Working Group, and ASME.
- Based strictly on the presentation, this reviewer has rated this as fair. A talk by one of the collaborators later in the day did a better job of showing symbiosis and saying how those two projects fold together. Slide 4 lists many collaborators, and that is fine. However, one reviewer was left wondering what the key and critical collaborations are, and whether the interaction is superficial or represents real engagement with industry.
- Coordination with industry standards development organizations is part of the oversight in the project. Industry input on the performance of existing pipelines provides good information with which to compare. Another reviewer suggests reviewing the use of off-shore connector technology for new construction systems.
- The collaboration with SRNL is extremely important in view of the work reported in the project number PD-022. Collaboration with ASME is also required to make sure that the underlying fundamentals of codes and standards specifically for FRP-composite pipelines are properly addressed.

Question 5: Proposed future work

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

- The future work should be tailored to provide the data needed by ASME.
- The future work plan appears to be good, but the focus did not appear to be as sharp on clearly defined barriers as an “excellent” rating would imply.
- The proposed work has nice general words concerning communication and sharing with stakeholders. However, it would have been more impressive to say the collaborators voted on or committed to the future direction to address a particular topic, rather than to mention an example of what might be looked at during the next year.
- Cycle testing of pressure cycling needs to be considered to reflect the actual pipe cycling. A possible test plan needs to be developed to study both pressure and temperature cycling. The cycling test of a limited number of cycles needs to be expanded to truly represent existing H_2 pipeline cycles and temperatures. The investigators need to understand why the properties of the polymers improve as a system.
- The future plan is well thought through. It includes working with ASME and stakeholders to review all testing that has been done, establishing the full list of testing that needs to be done to qualify composite pipelines for H_2 service, and defining the research needed to close the gaps between the work done and what is left to do.
- Slide 30, which is the only slide that reports future work, is very general and did not reference any specifics. Stated collaborations for acceptance of FRP pipelines for H_2 delivery do not count for future technical work. Slide 30 proposes to “identify research that needs to be completed to close knowledge gaps and establish plans to conduct research.” This is extremely important. In general, slide 30 does not outline a concrete research plan appropriate for a project that has been in progress for a number of years.

Project strengths:

- The revised cost estimate of the design appears to be realistic.
- The speaker had a clear and sincere manner in describing the work, especially during the question and answer session.
- The project features well-defined materials and issues with targets coming into focus due to DOE lead collaborations with industry, designers, code-setting bodies, and regulators. There is a good plan for the transition to market and cost estimates.
- A strength of the project is its focus on FRP pipelines, which can be used as an alternative to steel pipelines for H_2 transport. The project could serve to springboard the investigation of H_2 interactions with polymers and composites in general, but there is no such direction besides the permeability studies.
- Pipeline transport and distribution of H_2 is a potentially viable and cost-effective delivery pathway. Current analyses done for the Program show that pipelines are the low-cost pathway to transport large amounts of H_2 long distances

(greater than about 50 miles). This project is focused on utilizing fiber composite pipelines to reduce costs compared to steel pipelines, which is necessary to achieve the Program's delivery cost targets for this application. Pipelines for H_2 transport from central production facilities to terminals might be reasonable to employ and could potentially be the lowest-cost option for this part of an H_2 delivery infrastructure. As a result, pipeline transport might be very useful at even relatively low market penetrations. This year's work on this project is focused on testing available composite pipe for pressure and temperature cycling, plus the impact of blow down. It also includes direct measurements of H_2 leakage after the cycling and blow down experiments. These are likely the most important and practical tests that can help start to qualify composite pipeline for H_2 service. The results to date are very promising. The project now appears to be truly engaged with ASME to start to develop codes and standards for composite pipelines for H_2 transport use, which is very important. This project has widespread collaboration that includes the principle composite pipeline manufacturers, polymer manufacturers, SRNL, the DOE Pipeline Working Group, and ASME. The future plan includes working with ASME and stakeholders to review all of the testing that has been done, establishing the full list of testing that needs to be done to qualify composite pipelines for H_2 service, and defining the research needed to close the gaps between the work done and what is left to do.

Project weaknesses:

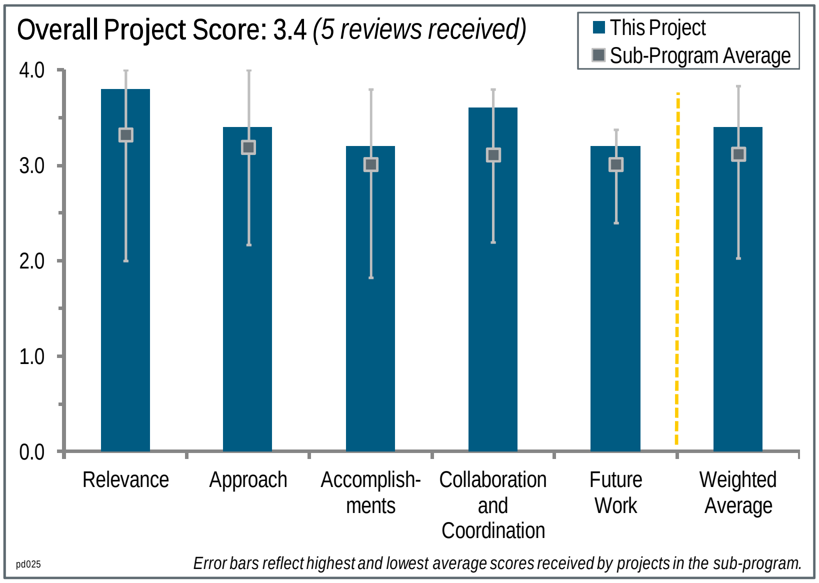
- The amount of leakage due to permeation seems high (per mile per year). This reviewer understands that it is small as a percent of the delivered amount. Perhaps a direct comparison to steel pipeline losses (industry norm) would make this reviewer feel more comfortable.
- It is important to remember that the cost to transport H_2 from central production plants to terminals at city gates if the distance is less than 50 miles is only a small fraction of the total cost of H_2 delivery. Thus, the priority of this project needs to be weighed against the priority of other projects in a limited funding environment. The project now appears to be truly engaged with ASME in starting to develop codes and standards for composite pipelines for H_2 transport use. It is vital that the investigators work with ASME and stakeholders to tabulate a full list of testing that needs to be done to fully qualify composite pipeline for H_2 service, and to run those tests. This is part of the future work.
- There are no clear weaknesses, but this reviewer would like to see the testing matrix expanded to address the combined effects of absorbed water and H_2, because the pipes will be buried. Issues this reviewer would like to see answered include the following: (1) whether water absorption will change the permeation rate of H_2, (2) whether the permeation of water through the pipe will contaminate the H_2, and (3) whether the two combined will influence fatigue performance or burst strength. Essentially, this would be covered by a thoroughly instrumented demonstration project, but this reviewer would like to see the effects quantified.
- A central weakness of this project is that it has not identified the parameter space of the potential failure modes that need to be systematically investigated. Cycling pressurization and depressurization may be recommended by ASTM, but they may not be sufficient to capture the operation of potential failure mechanisms of FRPs in the presence of H_2. By way of example, if the current project's approach and strategy to assessing the integrity of the FRP composite pipelines is applied to the case of a steel pipeline with no internal flaws through pressurization and depressurization, the steel pipeline shall never fail and hence it shall be deemed safe. On the other hand, it is known that the issue of hydrogen-induced fatigue acceleration—which is investigated using an appropriate laboratory test-piece—arises in the presence of a flaw. In other words, safe pressurization and depressurization of the pipeline is not sufficient to assess pipeline reliability. Durability, reliability, and safety must be based on accident and failure scenarios. Such scenarios and their parameter space have not been identified by the project. In addition, regarding the integrity of the liner/reinforcement interface, visual inspection of pressurized and depressurized FRP samples is not enough, nor is it appropriate. If there is a flaw on the liner/reinforcement interface, the team needs to determine how it will behave during the pressurization and depressurization cycle.

Recommendations for additions/deletions to project scope:

- The scope seems about right. It helps to keep an eye on pipelines and infrastructure.
- The team should include the permeation of water from the outer diameterouter diameter surface in permeation work and examine the effects of water absorption on the permeation of H_2.
- The project should justify the continuation of permeability studies. What new information is sought is not clearly stated. At this stage, the technical approach on how the project will achieve its goals and objectives (that is, the safety and reliability of FRP pipelines) is vague, and there are no clear targets and milestones regarding structural integrity. Assessing third-party damage as proposed on slide 30 is an important goal, but no technical approach is listed.

Project # PD-025: Hydrogen Embrittlement of Structural Steels

Brian Somerday; Sandia National Laboratories



Brief Summary of Project:

The objectives of this project are to demonstrate the reliability and integrity of steel hydrogen (H_2) pipelines under cyclic pressure and to enable a pipeline reliability and integrity framework that accommodates H_2 embrittlement. In fiscal year 2012, the project worked to quantify the effects of oxygen (O_2) impurities in H_2 gas on fatigue crack growth under high pressure and to determine the threshold level of O_2 impurities in H_2 gas.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The project examines the weak link in steel pipelines and H_2 embrittlement.
- Fully understanding the cause and potential ways to inhibit the potential for H_2 embrittlement in steel pipe has immediate usefulness to industries that transport H_2 by pipe.
- The demonstration of the reliability of steel H_2 pipelines for cyclic pressure is paramount to H_2 delivery meeting the DOE cost targets of $1–$2 by 2020 and establishing data-driven codes and standards. This project remains critical to the DOE Hydrogen and Fuel Cells Program (the Program).
- This project is clearly aligned with Program goals. Understanding and demonstrating the reliability and integrity of steel pipelines is critical for the initial rollout of H_2 infrastructure.
- The overall discussions and objectives of the project are good. The investigators need to expand the study to include the effects of H_2 toward the deceleration of embrittlement of welds, and not just the base material. This is especially true because seamless pipes are rare in installed pipelines.

Question 2: Approach to performing the work

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

- The fundamental approach involves using fatigue crack growth based on pressure cycles of H_2.
- The project is well focused; the investigators have conducted thorough research on the subject and actively collaborated with others to determine cause and effect.
- The initial focus on examining stress fractures under varying H_2 pressures for X52 is very sound. This reviewer would suggest, however, that the ability to modify temperature is a much-needed added dimension. Also, plans to evaluate other types of steels based on industry recommendations and working with the National Institute of Standards and Technology (NIST) to speed up the evaluation process clearly offer additional benefits to the project.
- The approach is good. The team needs to share with projects currently underway at NIST-Boulder and the University of Tennessee funded by the U.S. Department of Transportation to study the effects of H_2 in a range of pipeline steels.
- The researchers have taken a good approach on this project. It included a balanced combination of experimental and modeling work mainly around the objectives of understanding and determining the threshold level of O_2 to inhibit accelerated fatigue crack growth.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.2** for its accomplishments and progress.

- The team is making good progress in determining how much O_2 is needed to slow the crack growth.
- The team is taking a good approach toward understanding the effects of O_2 on H_2 embrittlement. The researchers must explore other models.
- The achievement to date is very good, but this reviewer would have like to have heard if the theory was vetted by organizations that operate high-pressure H_2 pipelines to determine if they support or challenge the concepts presented.
- Identifying the positive effects of introducing small amounts of O_2 at specific load-cycle frequencies into the pipeline is showing significant progress. This information opens the door to investigate whether the effect carries over to other types of steels, and if other gaseous impurities, such as methane, might also have a positive effect by providing a barrier for the exposed area.
- Significant accomplishments have been achieved in this project. Researchers have successfully demonstrated the effects and impacts of O_2 impurities on the mitigation of H_2-accelerated crack growth on X52 steel. It would be very interesting to extend these studies to other types of higher-strength steels.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- This project features good partnerships with the pipeline working groups.
- There is great collaboration among the research team.
- The research is excellent, but it is important to begin collaborating with organizations that install and operate high-pressure pipelines to thoroughly test the concept and validate it with empirical data. This reviewer likes the collaboration with I2CNER. This should be developed further.
- This is a nice assortment of relevant stakeholders. In particular, Exxon Mobil and Secat hopefully will provide for future analysis a variety of steels beyond X52 and X65.
- This project features very good collaboration, with a good mix of participants that include representatives from academia, industry, and national laboratories.

Question 5: Proposed future work

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

- The future work plans to develop a better understanding of the O_2 effects seem well thought out.
- The proposal for future work is light. More rigor should have been given to test the theory with organizations that have a clear interest in this area and can help fund a more rigorous validation project.
- As stated earlier, the plans to access NIST's specialized equipment and to expand to other types of steel are logical next steps.
- The proposed future work on welds is a good approach. This reviewer wonders if the O_2 and H_2 are combining at the crack to form water or another composition, or if possibly the water is being absorbed by the steel.
- The researcher plans to complete the work around X-65 steel. The expansion of testing higher-strength steels beyond the X-52 studies is a strong plan moving forward in the understanding of H_2 embrittlement of steel pipes.

Project strengths:

- This work could have a meaningful impact on existing and future pipeline designs.
- The project provides an understanding of both theory and practical application.
- The project features a good approach and future plans for inhibitors.
- The project takes a good, fundamental approach toward understanding H_2 embrittlement; the experimental work seems to complement the theoretical work.

- The project features a very sound approach and significant accomplishments. There is a good work plan for future work, with the fact that the testing will be expanded to other higher-strength steels. There is also good collaboration with a good mix of partners.

Project weaknesses:

- The project requires collaboration with organizations that have an interest in proving the theories and employing the concept with near-term, funded projects.
- The lack of specialized equipment to test the effects of temperature, pressure, O_2 concentration, and frequency all at the same time is an area of weakness.
- This reviewer is not sure if the X52 ERW is low- or high-yield-strength material. This needs to be quantified. The work is stated to be at high-pressure H_2. This reviewer wants to know what the actual operating pressure is.
- The team should perform some experiments to understand the role of O_2 in H_2 embrittlement.

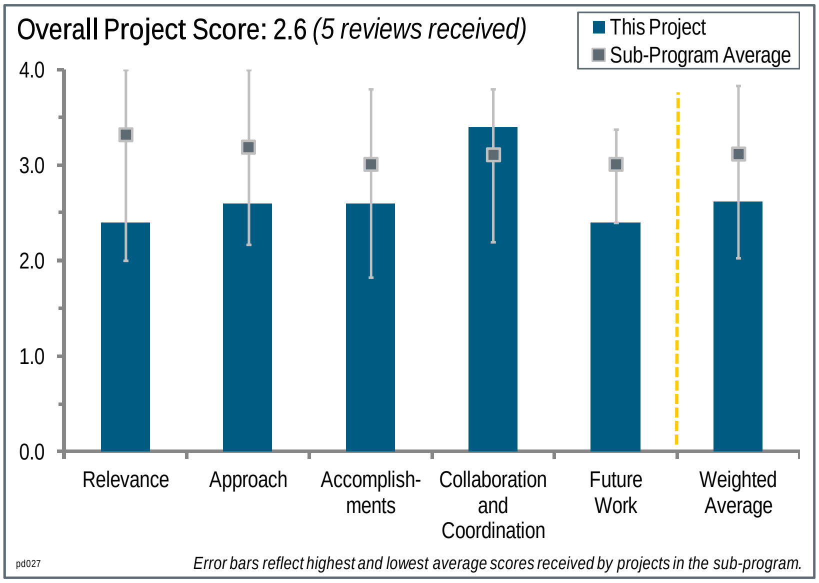
Recommendations for additions/deletions to project scope:

- The team should push harder, reach higher, and leverage DOE connections.
- The researchers should add the ability to cycle temperature.
- The future work on expanding the studies to other higher-strength steels beyond X-52 is a great addition to this project.

Project # PD-027: Solar High-Temperature Water Splitting Cycle with Quantum Boost

Robin Taylor; Science Applications International Corporation

Brief Summary of Project:
The overall project objective is to conduct research, development, and demonstration on the viability of a new sulfur family thermochemical water-splitting cycle for large-scale hydrogen (H_2) production using solar energy. More specifically, the overall project goals are to evaluate water-splitting cycles that employ photocatalytic or electrolytic H_2 evolution steps, and to perform laboratory testing to demonstrate the feasibility of the chemistry. This past year's objectives were to: (1) complete the optimization of the electrolytic oxidation process, (2) complete the evaluation of the high-temperature K_2SO_4 sub-cycle, and (3) perform economic and solar systems analyses.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.4** for its relevance to DOE objectives.

- The cycle seems to be very complex and have significant thermal management issues. This reviewer wants to know what the key advantages of this system are as compared to other chemical cycles.
- The project is aligned with DOE objectives because this work's objectives are to improve the approach to making renewable H_2. However, it is unclear how relevant the project is because the team does not identify the performance required of each step in order to meet an ultimate technological/economic goal of roughly $2/kg of H_2 production.
- This project addresses the central production of H_2 via concentrated solar. It is in an early stage of development and has yet to perform Hydrogen Analysis (H2A) cost modeling to highlight its potential economics.
- Thermochemcial cycles are of great relevance to the DOE Fuel Cell Technologies Program. This is a hybrid process that produces H_2 and electricity. It is unclear whether the H_2 cost could be significantly reduced if only H_2 were produced. This is a very complicated process. Because of it's complexity, this reviewer feels it is highly unlikely that the process will be viable.
- The project is aligned with DOE's goals of $3/kg H_2 and >35% efficiency in 2017. However, more specific milestones must be laid out along the way to 2017. It is hard to connect the progress made this year to the assessment of the likelihood of achieving the 2017 goals.

Question 2: Approach to performing the work

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

- The team has done an impressive job in attacking the different steps in a very complicated process. The researchers did a good job of using modeling to help the understanding of the process, efficiency losses, and sensitivity analysis. Some of the practical aspects should be addressed next.
- The approach is reasonable and well constructed. ASPEN modeling is a necessary and good step.

- It is not clear if this is the best approach because the critical performance requirement needed to achieve the DOE H_2 cost goal is unclear.
- This is a very complicated thermochemical cycle. It is unlikely that such a complicated system can be successfully developed for commercial application. The fiscal year 2012 scope focuses on the area that needs the most development. The molten salt work was a good start. It is important to get the viscosity; however, other quantities are required in order to understand the rheology. For example, this reviewer wants to know what type of fluid the molten salt is (Newtonian or Bingham plastic).
- The approach is still using a lot of electricity (high voltage for the electrochemical portion of the system). It would be prudent to perform early H2A analysis to determine the impact of voltage on the economics of the process. In central electrolysis, electricity use is a driving impact, and it would be helpful to know how low targets should be set for electricity use to drive research. The researcher commented that the theoretical voltage for the system is 0.113 V, while the operating voltage is 0.5 to 0.9 V. Thus, overpotential (parasitic losses) account for the majority of the electricity expenditure. The researchers need to determine what the root causes are for the overpotential (e.g., ionic conductivity, electric conductivity, contact resistance, anode activation, cathode activation, anode transport, or cathode transport). Without identifying the source and magnitudes of the overpotentials, the researchers will not be able to efficiently pursue improvements in performance.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- Progress has clearly been made, but this progress does not appear to be tied to the goals.
- It would be good to show the progress of each step in the process broken out to show where the most impact is being made and where the most future potential is. This reviewer wants to know what the storage capacity of the salts for H_2 are, and what pressure is needed for getting the H_2 into the media.
- The improvement in the voltage is a good step. The current density is very low. This will result in a very large electrolyzer that will be extremely expensive, particularly because the researchers are using Nafion and platinum (both very expensive materials). The researchers must improve the electrochemical performance and they may want to consider a high-temperature fuel cell, such as a solid oxide fuel cell. The use of ASPEN to do some flow sheet analysis is a nice addition from the previous work. The project is using a Nafion membrane but operating the system at neutral or basic conditions. The researchers may be able to significantly decrease costs by using a membrane from an alkaline type of membrane.
- The team fails to make the case for substantial and meaningful performance improvement. Ideally, there would be a set of metrics defined that lead to the ultimate goal of $3/kg and parameters, such as current density, voltage, lifetime, temperatures, capital cost of each component, etc. The project fails to show that the cost goals can be achieved at “current densities <100 mA/cm^2.” Without simultaneous targets for cell voltage and current densities, it is impossible to track progress toward the ultimate goal. No results of the H2A analysis were presented.
- The project situation appears to be in a similar footing as last year. This reviewer was hoping to see more progress in identifying and tackling the electrochemical portion of the device performance. This reviewer wonders if the research team has adequate electrochemist staffing.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- The presentation included a good description of the work from different partners and showed how the parts fit together.
- Collaborators appear to be well integrated into the project.
- There appears to be good collaboration, and the partners’ roles seem well defined. The inclusion of a utility company is a nice addition.
- There is limited basis to judge the extent of collaboration.
- It would be desirable to have collaboration with fuel cell stack original equipment manufacturers (OEMs), such as Ballard, for example. While ElectroSynthesis Inc. may provide value to the project, stack OEMs might have a lot of relevant experience that could improve the understanding of this system’s performance challenges.

Question 5: Proposed future work

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

- Technical issues are being addressed, but for this project there should also be an analysis of how manufacturable a concept such as this really is given the complexity of the system.
- The proposed future tasks are reasonable for advancing the various pieces of the technology, but it was not clear if they are the critical steps needed to achieve the DOE cost target.
- It would be interesting to see a chart showing how the work will result in the reduction of cost. For example, the researchers indicate that they need 100 mA/cm^2 for their electrolyzer. It would be useful to see how much this would reduce costs.
- Plans for future work are not adequately specific. Specific goals for voltage/current density and performance after 500 hours should be defined as part of the future plans.
- It is unclear if catalyst development is the source of electrochemical performance challenges. This reviewer believes that there are multiple, large impactors to overpotential, and a single line of attack of the problem might be insufficient. Research should first quantify all of the effects on overpotential and should have a comprehensive strategy to tackle the largest contributors. For example, if this was a fuel cell or electrolyzer development effort, the research would have tackled four or five aspects of the reduction of overpotential. Electricity expense for H_2 production will be very significant and justifies the need for more effort related to understanding the fundamentals of the electrolysis step.

Project strengths:

- The basic approach has the potential to meet DOE goals. Overall, the project construction is sound.
- There appears to be a strong team for this project. This project is funded very well. The researchers have developed a way for the process to operate close to 24/7.

Project weaknesses:

- The thermochemical cycle chosen for development is very complicated. For the large budget and amount of time in development, it seems that progress has been a little slow.
- The overriding weakness relates to project framing. The team needs to clearly state the value proposition associated with this work, and what advances are necessary to achieve that value proposition. The researchers need to clearly state how the specific research activities address the barriers to achieving that value proposition.
- The metrics are not specific enough, and the economic analysis is not presented. Therefore, there is no clear statement of the performance parameters that must be achieved to reach economic goals. Thus, no meaningful assessment can be made of current status, as all parameters must be compared against their target values.

Recommendations for additions/deletions to project scope:

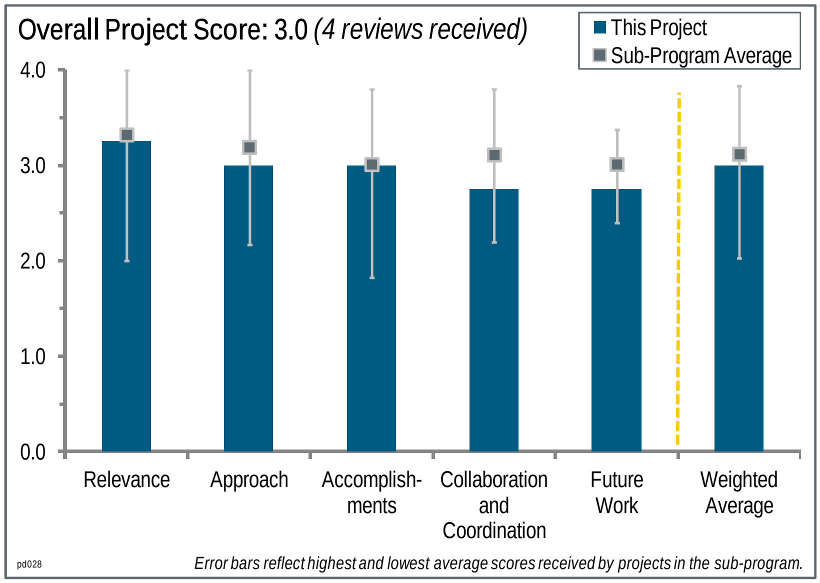
- This reviewer would not recommend supporting research that does not have a clearly defined value proposition and does not address the specific obstacles required to achieve that value proposition.
- H2A analysis needs to be conducted and included in the project. Specific targets need to be established for all parameters, which, when taken together, can be shown in H2A to lead to \$3/kg H_2. A reevaluation of efficiency needs to be done. An economic sensitivity to current density and voltage needs to be conducted. It is not clear how sensitive the system cost is to cell voltage or to system current density. Much of the economic analysis centers on the electrolyzer. However, it may be that the other system components contribute much of the cost. Consequently, a clear assessment of the balance of plant costs should be conducted.

Project # PD-028: Solar-Thermal ALD Ferrite-Based Water Splitting Cycles

Al Weimer; University of Colorado

Brief Summary of Project:

The objective of the project is to develop and demonstrate robust materials for a two-step thermochemical redox cycle that will integrate easily into a scalable solar-thermal reactor design and will achieve the U.S. Department of Energy (DOE) cost targets for solar hydrogen (H_2). Research was conducted using three approaches: (1) laser-assisted stagnation flow reactor, (2) on-sun solar reactor, and (3) thermogravimetric analyzer.



Question 1: Relevance to overall DOE objectives

This project was rated **3.3** for its relevance to DOE objectives.

- This is a simple thermochemical cycle that is part of the long-term Hydrogen Production sub-program plan.
- This project aims to develop and demonstrate robust materials for a two-step thermochemical redox cycle that will integrate easily into a scalable solar-thermal reactor design and will achieve the DOE cost targets for solar H_2. The project addresses a number of barriers identified in the Fuel Cell Technologies Program Multi-Year Program Plan, such as High-Temperature Thermochemical Technology, High-Temperature Robust Materials, Concentrated Solar Energy Capital Cost, and Coupling Concentrated Solar Energy and Thermochemical Cycles.
- Solar thermochemical cycles, such as the Hercynite cycle being researched in this project, have the promise of being cost-effective, near-zero-carbon-emitting H_2 production options. The Hercynite cycle is a very attractive solar thermochemical cycle because it is a simple two-step cycle and because it operates at temperatures below 1,500°C. These two factors result in this solar approach to H_2 production potentially meeting the DOE cost targets.

Question 2: Approach to performing the work

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

- Given the complexity of this project, the approach appears to be reasonable.
- The researchers have a very interesting approach that creates a porous matrix with high surface areas and a large number of pores. The structures do not seem to be very mechanically stable. In a large cell, this reviewer wants to know if the material would crush the pores. There are several materials in contact with each other that may impact the number of thermal cycles that the material can withstand without attrition.
- The project is focused on the development of atomic layer deposition (ALD) of ferrite materials. This is the heart of the system and is central to a successful project. The project is well thought out and appears to be integrated with the effort of Sandia National Laboratories (SNL). Resources for the project appear to flow excessively to SNL (compared to the funding for this project).
- Most aspects of the technical approach to this project are excellent. The Hercynite cycle has been well defined and characterized based on thermodynamic calculations and Raman spectroscopy. The kinetics have been demonstrated and studied using the novel laser-assisted stagnation flow reactor. The concept of generating and using porous alumina particles coated with the Hercynite to eliminate diffusion, sensible heat, and heat conduction issues is brilliant. The discovery and potential of running the redox reactions at the same temperature is very promising. The consistent attention to cost and the use of the Hydrogen Analysis (H2A) model for cost

analysis is outstanding. Not quite enough attention is being paid to the reactor design itself or the robustness of the packed bed in the reactor tubes. There also could be issues with the practical operation of the reactor design relative to two temperature regimes if needed, and with cycling the input of steam. The collaboration on this effort with the National Renewable Energy Laboratory's (NREL's) solar facilities is excellent, but beyond that, the collaboration seems limited to some interaction with SNL.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.0** for its accomplishments and progress.

- Satisfactory progress was reported.
- The project appears to have made significant progress in identifying an improved Hercynite active material and high-surface-area method of support. Researchers have successfully achieved all three milestones. Additionally, the milestones were more specifically defined than in the past.
- The researchers have done a good job of characterizing the process to make the particles. The use of CO_2 for the tests was a good innovation for understanding the material. Even though the CO_2 tests are interesting, there is still a need to do the tests with water because there are very significant differences in the chemistry. The material fabrication process seems to be very involved and may be expensive for large-scale synthesis. The researchers have not addressed the issue of the mechanical strength of the material. It may be thermodynamically stable, but the mechanical strength needs to be tested. This is a very porous material and alumina is not particularly strong. The mechanical strength needs to be measured. Even though the thermodynamics predict a stable material, the sample needs to be cycled to tested.
- Excellent progress has been made on this project, especially considering the modest funding level. The Hercynite cycle has been well defined and characterized based on thermodynamic calculations and Raman spectroscopy. The kinetics have been demonstrated and studied using the novel laser-assisted stagnation flow reactor. Researchers have demonstrated the generation of porous alumina particles coated with the Hercynite using ALD to eliminate diffusion, sensible heat, and heat conduction issues. The Hercynite-coated particles have been used in on-sun experiments at NREL, demonstrating the feasibility of the Hercynite cycle and project approach. Cost analyses have been done, demonstrating the feasibility of achieving the DOE cost targets. The discovery and potential of running the redox reactions at the same temperature is very promising. Not enough attention is being paid to the reactor design itself or the robustness of the packed bed in the reactor tubes.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- There was good collaboration between the partners.
- From other presentations, this reviewer gathers that there is adequate-to-good collaboration between the SNL team and the University of Colorado. However, collaboration was not described in the presentation.
- Collaboration is sufficient, although the emphasis of the project should be on creativity and productivity, not on collaborations, per se. (The value of this entry as a means of assessing success is questionable. The Program may wish to rethink or rephrase this evaluation question.)
- The collaboration on this effort with the NREL solar facilities is excellent, but beyond that, the collaboration seems limited to some interaction with SNL.

Question 5: Proposed future work

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

- The proposed future work is a reasonable extension of the present effort.
- The researchers must decrease the cycle time of the process in order for this process to be viable.
- The proposed work is logical but not fully detailed.
- The proposed future work is well thought through and includes investigating temperature and pressure ranges to reduce process costs, investigating Hercynite/alumina composition effects on redox performance and particle

robustness, and further investigating the potential to operate both the oxidation and reduction at one temperature. There is nothing specifically mentioned in the plan about reactor design and testing.

Project strengths:

- This project features a very strong team. The researchers are developing a simple cycle that has some chance of success.
- Solar thermochemical cycles, such as the Hercynite cycle being researched in this project, have the promise of being cost-effective, near-zero-carbon-emitting H_2 production options. The Hercynite cycle is a very attractive solar thermochemical cycle because it is a simple two-step cycle and because it operates at temperatures below 1,500°C. These two factors result in this solar approach to H_2 production potentially meeting the DOE cost targets. Most aspects of the technical approach to this project are excellent. The Hercynite cycle has been well defined and characterized based on thermodynamic calculations and Raman spectroscopy. The kinetics have been demonstrated and studied using the novel laser-assisted stagnation flow reactor. The concept of generating and using porous alumina particles coated with the Hercynite to eliminate diffusion, sensible heat, and heat conduction issues is brilliant. The discovery and potential of running the redox reactions at the same temperature is very promising. The consistent attention to cost and the use of H2A for cost analysis is outstanding. Excellent progress has been made on this project, especially considering the modest funding level.

Project weaknesses:

- It is not clear how this process can operate 24/7, which will significantly decrease its usefulness. There are cycle life concerns regarding the materials.
- Not enough attention is being paid to the reactor design itself or the robustness of the packed bed in the reactor tubes. There also could be issues with the practical operation of the reactor design relative to two temperature regimes if needed, and with cycling the input of steam. There is nothing specifically mentioned in the plan about reactor design and testing. The collaboration on this effort with the NREL solar facilities is excellent, but beyond that, collaboration seems limited to some interaction with SNL.

Recommendations for additions/deletions to project scope:

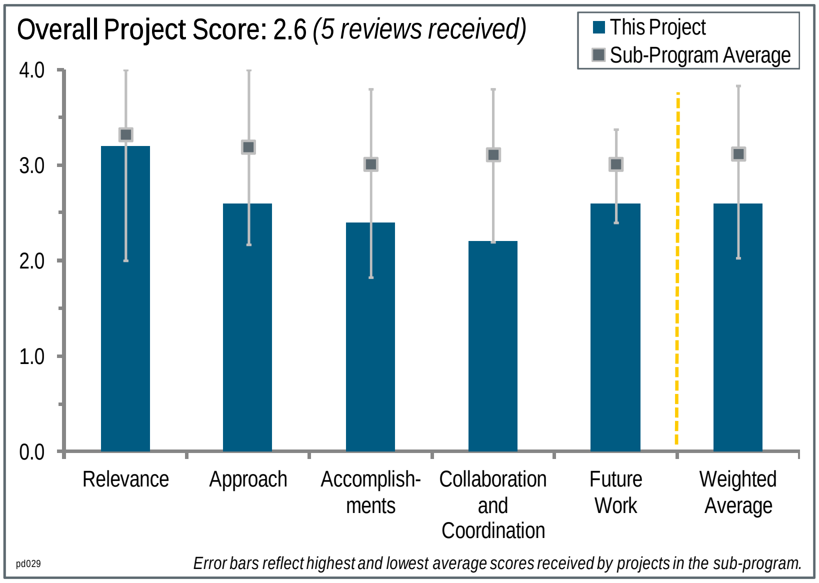
- The project team should expand the future work to include efforts on reactor design and testing.

Project # PD-029: High-Capacity, High Pressure Electrolysis System with Renewable Power Sources

Paul Dunn; Avalence LLC

Brief Summary of Project:

The current objective of this project is to produce a pilot plant (1/10th scale) design for use as a basis for an economic analysis of plant fabrication and operating costs for hydrogen (H_2) production using an electrolyzer with a nested cell. The operation and efficiency of the pilot plant will be demonstrated through laboratory testing at Avalence and field testing at the National Renewable Energy Laboratory. The project also strives to prepare a site location to accept the completed plant for commercial (300 kg/day, 750 kW) operation. The project addresses capital cost, system efficiency, and renewable power integration of H_2 production.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- This project is definitely working toward DOE project goals.
- The delivery of H_2 at 6,500 psi is relevant to the objectives.
- High-pressure electrolysis is of great importance to the DOE Hydrogen and Fuel Cells Program (the Program).
- The overall objective for a low-cost electrolyzer is a good objective.
- The project is clearly aligned and relevant to DOE goals, but the presenter could have done a better job of quantifying how the features being developed will result in economic gains.

Question 2: Approach to performing the work

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

- One reviewer was favorably impressed with the way the company is addressing the challenges that have come up in the course of the project, and its innovative approaches in overcoming them. The company's solutions are well thought out, and it has made good progress in overcoming them.
- The team learned a number of lessons from the ideas that did not work, and then moved on to the design that worked.
- Another reviewer cannot say that the approach is outstanding, because at some level it failed. However, the team seems to have learned substantial lessons from the failure, and has developed a new approach that appears to be well suited to the barriers.
- The approach is an interesting idea. However, it has been clear that the original design would not work since almost the inception of the project. It is good to see the changes made in the last year. The new approach may work, but it is not clear that substantial cost reduction will be achieved. It seems that each cell will still require its own valves, etc., so the balance of plant seems to be very large (and expensive). The controls may be more complicated than needed.
- There is very little analysis of the cell performance capability or the ability to withstand pressure differential. The approach seems entirely empirical, which is terribly risky and inefficient.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.4** for its accomplishments and progress.

- Given some of the difficulties the researchers have experienced, they have made good progress. It sounds like they have solved the major problems and are in a good position to deliver an interesting device. They have developed some very innovative solutions.
- Given the experimental difficulty with this innovative approach, the team is making good progress.
- The project is a blend of a failure of the original plan and outstanding progress on the new plan. Unfortunately, expending so much effort on finding the right structure and containment approach for the cell has reduced the team's ability to look for broader advances for the cell, which might also be needed to push this over the top.
- It takes a lot of courage to indicate that the initial design was wrong and that the project had to be significantly changed in focus. The presenters should be congratulated on this. The fact that the composite wrapping is expensive should not have been a surprise. This has been pointed out by reviewers since the first time this project was reviewed. It is not clear whether the system will be easily sealed.
- No cells or stacks with commercial promise have been developed. No analysis is available that shows a credible path has been identified. The efficiencies are inferior to state-of-the-art polymer electrolyte membrane cells.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.2** for its collaboration and coordination.

- The researchers are collaborating with companies that have the required expertise to solve the specific problem. This is a good use of their resources.
- The project features good use of collaborators, but they are mostly in the form of vendors.
- The collaboration seems to be mostly in the form of subcontracting others to do some very specific work. It is not clear how additional collaborations were done.
- There appears to be little collaboration with other parties.
- This project features no collaborations.

Question 5: Proposed future work

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

- No analysis was identified, and no safety study was prioritized.
- The researchers are on the right track. A 10-kg pilot plant is a good size for their next step, and they can either use it as a building block module or scale-up based on the results of the pilot plant operations. Reviewers will need to assess the durability of the pilot plant before going to a larger unit.
- The team has good plans for the future work after solving a number of technical problems.
- The delayed delivery pushes this toward "get it out the door" mode, but this project also needs to think more broadly about what is the next phase—assuming success—to take this technology to the next level.
- The future work seems reasonable. There are concerns about how the many tubes will be sealed to ensure that there is no leaking at the entry/exit points of the tubes. A decrease in cost by a factor of five still makes this a very expensive electrolyzer. It would be useful to see the researchers' plan for getting to the DOE target capital cost.

Project strengths:

- This is a high-risk, high-payoff project that is worthy of support.
- This is an interesting approach to achieving high-pressure H_2 without a compressor. Alkaline electrolysis has the potential to use low-cost materials.
- Strengths of this project include the researchers' ability to identify novel solutions to difficult problems, and their dedication to solving their problems. They are not quitters. They have intellectual honesty, and are making significant progress. The project has accomplished a lot on a relatively small budget. Ultimately, this is a low-cost solution that has the potential to meet Hydrogen Production and Delivery sub-program goals.

- This reviewer felt that this project has no strengths.

Project weaknesses:

- This reviewer is concerned that the researchers will run out of budget. They seem to have done a lot of work on a small budget.
- The team can benefit from some collaboration with an academic team that can apply the latest science.
- This project features no analysis, poor performance, and significant safety concerns associated with a balanced pressure cell that were brushed off.
- This presentation was poor. Too many slides are holdovers from prior years, used without much thought of what they add to this presentation. There was some excellent spoken discussion of the failings of the first approach, but there was little in the presentation to support that discussion. Frequently, the critical information (such as why purity increased) was described verbally, but was nowhere to be seen in the slides. The team needs to do a much better job of figuring out what it wants to say before creating the presentation, and then create the slides to support that message.
- The multiple tube approach will require very complicated sealing and the use of many valves and controls. This will drive up cost, so it is not clear that the cost reduction will be enough for the researchers to achieve the DOE cost targets. There is a need for long-term testing. They have shown a lower level of contaminants in the product gas during initial production and at lower pressure (2,000 psi). They need to test at the higher pressures and for longer periods of time.

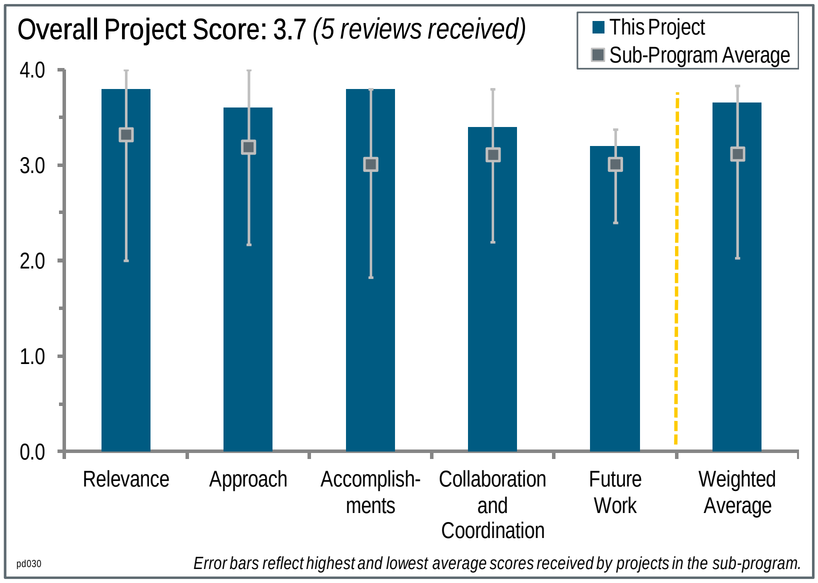
Recommendations for additions/deletions to project scope:

- The project scope is appropriate.
- This project should not receive additional funding at this time. The project represents a very high risk for the Program, since there is a non-trivial risk of a safety incident and very little likelihood of a good set of technical results.
- This reviewer had no recommendations.

Project # PD-030: PEM Electrolyzer Incorporating an Advanced Low Cost Membrane

Monjid Hamdan; Giner Electrochemical Systems, LLC

Brief Summary of Project:
The overall project objective is to develop and demonstrate an advanced, low-cost, moderate-pressure proton exchange membrane water electrolyzer system to meet U.S. Department of Energy (DOE) targets for distributed electrolysis. This task involves developing: (1) a high-efficiency, low-cost membrane, (2) a long-life cell separator, and (3) a low-cost prototype electrolyzer stack and system. The objectives for the fiscal year (FY) 2012 are to: (1) complete an electrolyzer stack and system assembly, (2) evaluate the electrolyzer's performance and efficiency, and (3) deliver and demonstrate a prototype electrolyzer system at the National Renewable Energy Laboratory (NREL).



Question 1: Relevance to overall DOE objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The project has good relevance to the DOE objectives.
- This project is clearly relevant to DOE goals, with a very clear problem statement and value proposition.
- This project is directly responsive to DOE Hydrogen and Fuel Cells Program goals and objectives.
- The project is in line with the DOE targets for distributed electrolysis to produce hydrogen (H_2). It addresses the critical issues of PEM electrolysis.
- This project meets DOE's stated objectives. Cost reductions at this small scale can be applied to larger-scale units.

Question 2: Approach to performing the work

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

- The approach is well designed, with good integration of the different steps from the membrane to the system.
- The group is looking at individual aspects of the proposed electrolyzer carefully.
- The presentation clearly identified how the features studied are designed to impact the important project and technology goals.
- This project features a very well-thought-out approach and a good blend of analysis, design, and experiments.
- This project features an excellent focus on critical components. The design for high-volume manufacturing is essential. Teaming with a large-volume commercial manufacturer is beneficial and brings reality to the project. Researchers are hitting their milestones.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.8** for its accomplishments and progress.

- The project is making good progress.
- Very encouraging progress on cost reduction has been made over the past year.
- The project made significant progress on membranes, demonstrating durability and improvements in efficiency. In addition, developments resulted in cost reductions in the stack and system. The talk gave a detailed presentation of the performances.
- Cutting the cost of the membrane by orders of magnitude is impressive, as is the separator performance lifetime estimate of >60,000 hours. The team made excellent progress on stack cost reductions. An efficiency of 47 kWh/kg for H_2 production is equal to $1.83 per kg at $0.039/kWh.
- A lot of the heavy lifting in these developments is from prior years. Progress has been outstanding over the course of the project, but this last year the team seems to be in a finishing-up mode.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- The coordination is good, covering the various competencies needed to work on the different components.
- The project has involved major industry players such as 3M and Entegris. There has been good collaboration with AREVA, which provides independent third-party performance verification.
- This project features excellent utilization of skill sets outside of the company.
- The partners are well identified and recognized, but they seem to be mostly in the mode of vendors as opposed to partners.
- This project could probably show more evidence of tight collaborations with the supply base.

Question 5: Proposed future work

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

- The proposed work for further development is well thought out.
- There is a good map of future plans based on the remaining issues to address.
- The project is nearly complete and the future plans to improve are identified. The team will continue the developments with longer testing time to deliver the prototype to NREL for evaluation, and it will also address future challenges such as the labor cost.
- This is an excellent project with good results. Manufacturing cost reductions to take out the labor component is the way to go.
- The future plans are good. At this stage of the project, it is hard to provide an outstanding plan unless Giner were to identify some bold new direction in which to work that would again dramatically improve the performance.

Project strengths:

- The team appears have a strong hold on various aspects of the technology.
- A strength of this project is its focus on design improvements with respect to key cost and product lifetime challenges.
- The team is focused on the right things. Strengths of this project include its good use of industrial partners, how its reduction of part count and design for manufacturing will further reduce costs, and its reduction of material costs by orders of magnitude.

Project weaknesses:

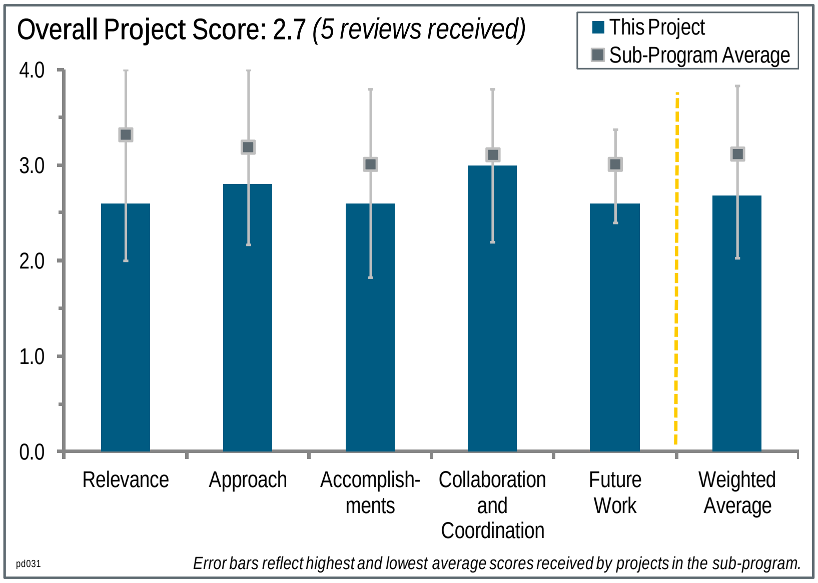
- A better focus on the reduction of the overall cost is needed, as opposed to optimizing individual components.
- This project has relatively limited partnerships.
- This reviewer did not detect any weaknesses.

Recommendations for additions/deletions to project scope:

- The quality of water seems important to achieve the performances. This issue has to be addressed in the future.
- This reviewer recommends a significant scale-up to utility scale after the current project has been completed. Hundreds and thousands of kilograms of H_2 are needed. It is not certain that PEM can meet the challenge.
- As the capital cost drops, these systems are becoming more and more limited by the cost of electricity. There is a need for some out-of-the-box thinking about how this technology could contribute to achieving lower-cost H_2.
- The project team should consider additional partnerships to enable a more commercial program scope.
- This reviewer had no recommendations.

Project # PD-031: Renewable Electrolysis Integrated System Development and Testing

Kevin Harrison; National Renewable Energy Laboratory



Brief Summary of Project:
This project strives to test, demonstrate, and analyze renewable electrolysis integrated systems. It will test the characterization and performance of electrolysis systems and electrolyzer stack and system response with typical renewable power profiles. Demonstration of renewable resources integration includes: (1) identifying system cost reductions and optimization for electric utilities; (2) characterizing, evaluating, and modeling the integrated systems; (3) characterizing electrolyzer performance with variable stack power; and (4) designing, building, and testing shared power electronics and direct-coupled renewable-to-stack configurations. The analysis of wind-to-hydrogen (H_2) involves developing cost models for renewable electrolysis systems and quantifying capital costs and efficiency for wind- and solar-based electrolysis scenarios.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.6** for its relevance to DOE objectives.

- The project does not have good relevance to DOE objectives.
- Overall, this project is relevant to the DOE Hydrogen and Fuel Cells Program (the Program) objectives. However, the project title and presentation content give the impression that the researchers were addressing system integration and testing of renewable electrolysis systems. The work presented, though still relevant to the ultimate DOE objectives, was in fact about grid integration of electrolyzer systems. None of the four stated barriers was directly addressed this year or in 2011.
- Independent third-party testing and validation of technical and economic claims by component manufacturers is critical to supporting the integrity of the Program. Wind models and wind-to-H_2 analysis are critical to understanding the economic performance of the overall system and to determining where cost-cutting efforts are needed. The overall electrolyzer efficiency reported on slide 4 is at variance with reported results by industry partners.
- There is general alignment between the project and the Program goals. However, there is no value proposition discussed for the stated objectives—there is no identification of how completing these objectives quantifiably advances the mission. Also, a disproportionate part of the progress seems to have little relationship to the stated program objectives.
- The three projects reported (membrane durability under varying current flow, mass flow measurement, and grid frequency support) support the Fuel Cell Technologies Program Multi-Year Program Plan (MYPP) very well. Spinning reserve to support grid operation is expensive; if electrolysis can quickly and economically reduce spinning reserve, it could provide a more environmentally benign approach to grid stabilization.

Question 2: Approach to performing the work

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

- The approach is good if the objectives were revised to reflect the realistic impact of the project and drop any claim to renewable energy.
- The approach is adequate for the task and seems straightforward. The team has good testing laboratory and equipment.
- The approach is weak, and the project is not likely to add significant value.
- It is not clear how this approach impacts the stated barriers of cost or system efficiency. The objectives of the project are largely geared towards renewable integration, but not much progress has been made in this area this year.
- Long-term monitoring of electrolyzer performance under “real-world” conditions will improve models and provide more accurate, reliable, and credible performance and economic forecasts that will reduce risk for those considering investment in this technology, which, when positive and competitive, will assist adoption of this technology. This monitoring addresses capital cost and system efficiency barriers by providing better quality data.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- There is not much to show for this project.
- The project team has achieved good results despite budget reductions. The mass flow measurements and the grid frequency support results are significant.
- The project team is obtaining interesting results on stack performance, including results related to decay utilizing variable power input versus constant current. This is an area that needs considerable attention. This reviewer wants to know how long an electrolyzer will last when coupled to wind. Mass flow measurement experiments are critical to the commercialization of H_2. The National Renewable Energy Laboratory (NREL) test rig is making an important contribution. The involvement of the National Institute of Standards and Technology is very helpful. The analysis of an electrolyzer for grid frequency support is an important area for evaluation.
- There is a lot of progress, particularly on mass flow measurements and grid frequency support. However, these topics are not aligned with the project objectives, and it is not clear how they impact the barriers. The work is not necessarily bad, but the team needs to lay the groundwork for the value proposition around the work, and explain why this work is pertinent to the objectives, goals, and barriers. There is a major disconnect between the objectives and the actual work done. The principal investigator can look at the barriers and believe this work addresses them, but that connection is not made for this reviewer.
- The project appeared to make significant progress during the review period, but the presenter offered little objective basis against which to compare; the presenter did not share any milestones or other schedule references against which to compare progress, nor any updated cost or efficiency numbers to compare against targets that have a time reference.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.0** for its collaboration and coordination.

- The project features excellent collaboration; especially with the electrolyzer vendors and utility company.
- There is collaboration with a wide range of entities: equipment suppliers, utilities, and researchers.
- This feels like a customer-supplier type of relationship.
- There is good information sharing with some entities. The use of a dedicated website to share information is not mentioned. This would be a good feature to add if not already in place. It would be interesting to see more international electrolyzers evaluated to see how the United States compares.

Question 5: Proposed future work

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

- The proposed future work is not commensurate with the original project scope.
- Only part of the future work clearly addresses the barriers. The project team needs to draw clearer lines between this work and the barriers.
- The proposed future scope of work is reasonable, but again it does not match the stated barriers.
- NREL provides a valuable test platform, of which future work will take good advantage, to validate a variety of renewable electric sources and electrolyzer designs.
- The future work is basically a continuation of this past year, with a few additions. This may be budget-limited and is understandable. It would be interesting to see what could be done if there was more funding available. It is unclear what the designation "medium pressure" means. The Avalence stack will be high pressure (6500 psi), not medium pressure.

Project strengths:

- The work seems to be well appreciated by industry partners.
- The collaboration with stack vendors is a strength of this project.
- This is a solid project. Strengths include the third-party validation and analysis work, the good laboratory test rigs, and the good modeling tools.

Project weaknesses:

- The team is doing work that will be of low utility. The mass flow metering is not any better than those that are commercially available.
- The project appears to have veered off from the stated renewable-energy-specific objectives.
- One reviewer did not detect any weaknesses.

Recommendations for additions/deletions to project scope:

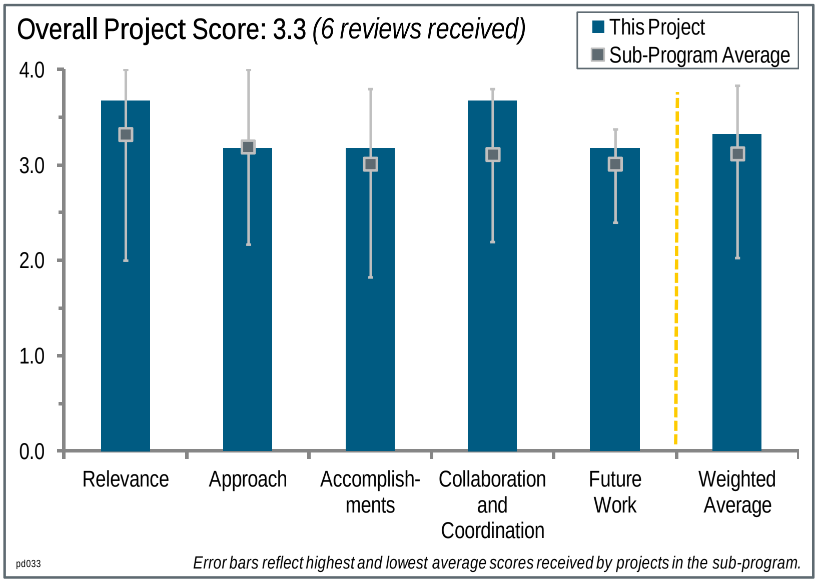
- The team should focus on renewable H_2, consistent with the original project focus.
- The project objectives should match the efforts. Although the project claims it is renewable-focused, grid integration is not specific to renewable resources.
- If not already in place, a dedicated website reporting results and giving access to the modeling would be helpful to industry and other research institutions. The project team should also develop a "bucket list" of additional work that could be done if more funding was available.
- This work needs a careful statement of the value proposition; the researchers need to clarify how the research and development components add quantitative value to overcoming the barriers. Only with that framing can reviewers identify which pieces should be emphasized.

Project # PD-033: Directed Nano-scale and Macro-scale Architectures for Semiconductor Absorbers and Transparent Conducting Substrates for Photoelectrochemical Water Splitting

Thomas Jaramillo; Stanford University/National Renewable Energy Laboratory

Brief Summary of Project:

The main objective of this project is to develop third-generation materials and structures with new properties that can potentially meet U.S. Department of Energy (DOE) targets (2013 and 2018) for usable semiconductor bandgap, chemical conversion process efficiency, and durability. Specifically, the project will develop: (1) photoelectrochemical (PEC) substrates consisting of macroporous, high-surface-area, transparent, conducting oxides upon which PEC materials can be loaded; and (2) new PEC materials based on nanostructured MoS_2 that can potentially meet DOE performance targets.



Question 1: Relevance to overall DOE objectives

This project was rated **3.7** for its relevance to DOE objectives.

- The project is well aligned with goals articulated for the PEC portfolio.
- PEC water splitting/electrolysis does meet DOE Hydrogen Production Roadmap objectives.
- PEC hydrogen (H_2) production is a long-term technology in DOE's portfolio. The objectives are in line with DOE targets to develop efficient materials performance.
- The project is clearly aimed toward improving the solar-to-H_2 conversion efficiency for PEC systems.
- This project may provide some of the key technologies related to enabling a viable PEC system. The high-surface-area scaffold can ameliorate the carrier transport issues associated with many semiconductor materials that otherwise have an appropriate bandgap for efficient PEC H_2 production.
- The reported work is outstanding, but the emphasis on engineering frameworks without efficient and durable active materials is not consistent with earlier program priorities. Neglecting the material efficiency of MoS_2 in the future work plan is a deficiency that should be addressed. That topic does not appear in future work plans. Apart from plans to address efficiency, the project goals fully support the goals of the Hydrogen and Fuel Cells Program (the Program).

Question 2: Approach to performing the work

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

- The approach is well presented and well designed to meet efficiency, durability, and cost targets. It is very relevant and different from conventional concepts.
- The approach is clear and seems well thought out. More integrated modeling and analysis could be used, as it seems a lot of the work rests on instinct.
- The project team has responded to DOE listings of critical barriers to PEC H_2.
- The technical approach involves engineering transparent, high-surface area electrodes (HSE) with both conventional PEC materials and some novel active materials (MoS_2). It is not clear if this technology is feasible.

Even though this project is still in the materials discovery and development phase, there appear to be major technical challenges with this technology.

- This effort has strong materials expertise, which will be essential to solving the many issues regarding PEC H_2 production. Of significant interest is advancing the state-of-the-art for non-precious-metal catalytic materials, as this will be a fundamental drive for the cost benefit for not only PEC systems, but also across a spectrum of other important technologies related to energy production.
- The focus on HSE prior to certifying high-performance, durable PEC materials is in violation of earlier Hydrogen Production and Delivery sub-program priorities. Materials of implementation directly affect process engineering and high-throughput manufacturing development. Such development might be a premature, absent definition of active materials. The MoS_2 focus may be flawed because efficiency remains low and the future research and development plans do not explicitly address this deficiency. Significant priority should be given to assessing the possibility that MoS_2 will never prove successful.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.2** for its accomplishments and progress.

- The project has completed two of its four technical barriers, as well as 70% of the work on the other two.
- The project made progress in developing a route to produce a fully tunable, high-surface-area, transparent electrode with a patent filed in 2011. It also demonstrated long-term stability of nanostructured MoS_2 H_2 evolution catalysts. However, the PEC efficiency is currently low. Details were given during the talk on the way to increase it in future work.
- There is a tight focus on the development of these materials, with advances taking place on two main fronts—the development of a process to produce a high-surface-area scaffold to serve as the backbone for thin films of PEC materials, and the development of a hydrogen evolution reaction (HER) catalyst (MoO_3-MoS_2 nanowires). The results shown demonstrate a continued advancement in the state-of-the-art for these technologies.
- The work needs to be calibrated with the scale of the program, which is pretty modest. While a lot of data has been generated, it remains unclear how far away the goal remains. This is tricky, as reviewers do not want to punish anyone for honestly stating that the goals are difficult to achieve. However, it would be good to better articulate how many things have to improve by significant factors to hit the goals.
- This reviewer is glad to see an 81-fold increase in surface area for indium-tin-oxide (ITO). Of course, this reviewer questions whether Graetzl demonstrated this effect with TiO_2 back in 1991. It is unclear what depositing MnO_x on an HSE support was supposed to accomplish, other than to demonstrate the extremes in achievable optical density. This reviewer hopes not too much time was spent on it. The investigator needs to better understand why increases in surface area and wider bandgap with decreased particle size have not necessarily translated into proportionately higher solar-to-H_2 conversion efficiency. Nevertheless, improved current-voltage characteristics due to the combination of high-surface-area support and nanoparticle arrays of MoS_2 were demonstrated.
- The statements that hollow-core MoS_2 nanowires are "100%" stable and that the MoO_3 core is "completely protected" are possibly misleading. It is apparent that performance after 10,000 cycles is unchanged, but it is possible that the material is modified at the electronic level, which could lead to problems at a later date or under different conditions. The apparent material stability could be significantly affected by implementation of photocatalytic activity. Electrocatalysis performance shows no discernible change after 10,000 cycles. There is no apparent basis for the assertion that MoS_2 "high efficiencies" will accompany future incorporation into an HSE framework. Such improvement depends on why MoS_2 PEC efficiency is poor. HSE incorporation would not affect efficiency if low efficiency derives from the observed MoS_2 indirect bandgap.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.7** for its collaboration and coordination.

- This project features a good set of collaborators in the science domain.
- The project is well coordinated and has good collaborations with partners that are experts in the critical areas of focus.

- Meaningful collaborations have taken place with a number of groups. Future collaborations will be important with respect to evaluating the HSE scaffold with a variety of PEC materials.
- The Stanford effort is thoroughly integrated with the PEC working group.
- More characterization of interface energy states is needed to confirm stability assertions. More attention to theoretical investigations of the effects of indirect bandgap on performance might be useful. Both of these activities can be effected through collaboration partners in the PEC Working Group.

Question 5: Proposed future work

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

- The project has identified future work to expand the HSE composition, perfect absorber contact via interfacial engineering, and integrate different nanomaterials with HSE.
- Details on the next steps were missing in the slides, but more information was given during the talk. This point could be improved to convince one reviewer that efficiency can be really improved in the future work.
- The proposed future work is a natural extension of the promising results presented in 2012. Focusing on understanding the interface between the semiconductor material and the HSE is important.
- The proposed future work is a thoughtful definition of what would be useful to do in the future. More gap analysis would be useful to ensure that the efforts are focused on the right gaps.
- The emphasis should stay on nanoparticle photovoltaics. Another reviewer is a little concerned that developing HSEs based on other transparend conductive oxides (TCOs) could become an all-consuming effort.
- The emphasis on continued HSE development represents the pursuit of a solution in search of a problem. It is this reviewer's opinion that PEC priorities must continue to address the search for an effective and durable PEC material before making significant investment in implementation frameworks. The proposed future work should explicitly address the low PEC efficiency observed for MoS_2.

Project strengths:

- This project offers some novel and outside-the-box thinking.
- This project features good collaborations and a sincere focus on improvement to the materials under study.
- The work is of high quality. The team is working hard to face the PEC issues. The choices made in processes and materials are relevant to reducing cost.
- This project has a very well-defined scope, with a well-thought-out approach that provides a meaningful way to advance the technology. Processes and methods are amenable to scaling, so there should be minimal issues for volume manufacturing.
- The project team has excellent control over nanoparticle structures. Another strength of this project is its continued improvement in non-noble metal H_2 evolution.
- HSE development will likely prove useful, regardless of its relative priority in program objectives. Nevertheless, PEC is a long-term technology option, so the emphasis on materials discovery should be retained. The strong collaboration with other institutions, organizations, and the PEC community is evident. Technical proficiency is evident throughout the project, and necessary facilities and equipment are available either through the home institution or via the PEC Working Group. Outstanding progress in defined tasks is evident.

Project weaknesses:

- There is little modeling of the phenomenology.
- The project risks being pushed by DOE to cover too much ground too fast, before accomplishments are understood.
- The Hydrogen Production and Delivery sub-program portfolio, through either management or the PEC Working Group, should revisit the earlier priorities for PEC investments and ensure that all PEC projects adhere to those investment priorities.
- According to the 2011 work, producing one metric ton of H_2 per day requires 0.03 square miles of colloidal dual-bed suspensions or 0.02 square miles of planer PEC cells. The mass production element, cost effectiveness, final system configuration, location where such a system can be installed, and balance of plant (fuel collection, fluid

system, dryers, compression, etc.) put this technology at a lower priority level compared to other technologies, such as PEM electrolysis.
- Although the project made progress with a team of high experts, the PEC efficiency is still low. The gap between the current results and the 2013 DOE targets is large. It raises the question of how these targets can really be achieved next year.
- This reviewer is not convinced that supporting a thin PEC material on an HSE will ultimately improve efficiencies. If the bulk of the losses for electron transport primarily occur in at defect-rich grain boundary/interface regions, then for a given optical density (assuming a constant particle size), the volume of defect-rich material will be conserved.

Recommendations for additions/deletions to project scope:

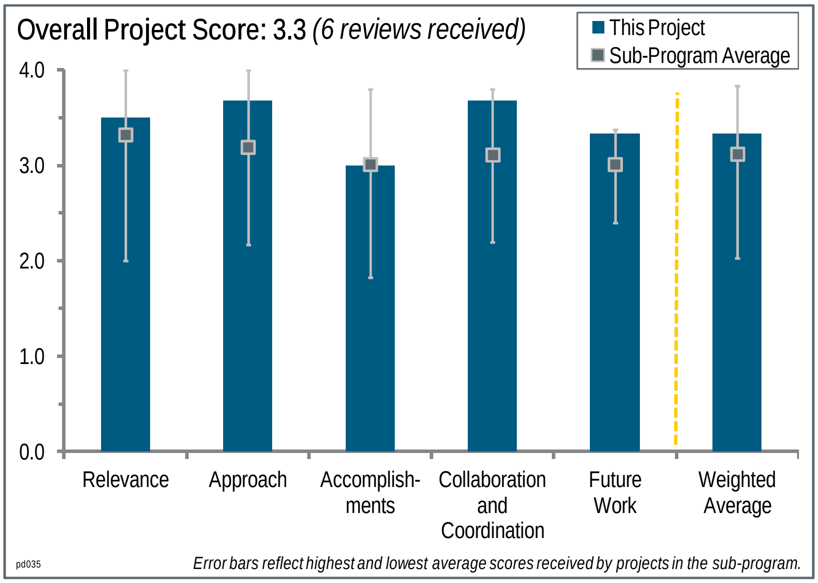
- The practicality of such technology needs to be evaluated by DOE. There are many technologies that provide significant technical advancements over the current technologies, but the majority of them end up not being practical due to mass production and fabrication challenges, being integrated in an end-to-end system, and high costs.
- The project team should develop overall models that will help estimate required improvements in materials properties.
- It is doubtful that the 2013 DOE technical targets (efficiency, durability, cost) will be achieved next year because there are still challenges. Nevertheless, the project is still at an early stage. The approach is relevant and different from others, and the results are encouraging, so the work has to continue next year to further investigate the concept.
- The HSE is an exciting development with applications across a wide spectrum of technologies. Regarding PEC, it is important to identify where the losses occur. When measuring photocurrents, to effectively compare materials, it is more telling to measure the absorbed photon to current efficiency (i.e., internal quantum efficiency). This would more clearly show how the material is performing on a fundamental level (e.g., trap states, improved photoconductivity, etc.). Using an APCE (absorbed photon conversion efficiency) metric, this reviewer wants to know how the HSE with Fe_2O_3 would compare to the planar thin film.
- The Hydrogen Production and Delivery sub-program, through either management or the PEC Working Group, should revisit the earlier priorities for PEC investments and ensure that all PEC projects adhere to those investment priorities. The project work scope should include specific attention to MoS_2 PEC efficiency issues.

Project # PD-035: Semiconductor Materials for Photoelectrolysis

John Turner; National Renewable Energy Laboratory

Brief Summary of Project:

The objective of this work is to discover and characterize a semiconductor material set or device configuration that: (1) splits water into hydrogen (H_2) and oxygen spontaneously upon illumination, (2) has a solar-to-H_2 efficiency of at least 5% with a clear pathway to a 10% water-splitting system, (3) exhibits the possibility of 1,000 hours of stability under solar conditions, and (4) can be adapted to high-volume manufacturing techniques. The main focus this past year has been to work with state-of-the-art materials that meet the U.S. Department of Energy's (DOE's) near-term efficiency targets and investigate surface treatments that promote durability.



Question 1: Relevance to overall DOE objectives

This project was rated **3.5** for its relevance to DOE objectives.

- The project is well aligned with Hydrogen and Fuel Cells Program (the Program) goals.
- This project is clearly related to the Program's goals and objectives.
- This project matches up quite well with the Program's objectives for photoelectrochemical (PEC) H_2 production. The continued development of low-bandgap materials with a focus on improving durability is relevant to achieving the 2013 technical targets.
- The project fully supports DOE research and development (R&D) objectives in PEC H_2 production by advancing materials development with appropriate and effective semiconductor bandgap, chemical process efficiency, and projected solar-to-H_2 plant efficiency with materials durability.
- Semiconductor materials for photoelectrolysis meet DOE Hydrogen Production Roadmap objectives.

Question 2: Approach to performing the work

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

- The three-pronged approach to identifying and determining systems that meet DOE targets is excellent.
- The approach includes a good mix of experiments and modeling. Some consideration should be given to the trade-off associated with the cost of high-efficiency solar cells.
- The ability of the base material to produce with high-efficiency solar-to-H_2 production has already been demonstrated. The focus on improving durability is a meaningful approach, and the methods and collaborations provide the best opportunity for success.
- It is understandable that this project is still in the materials discovery and development phase. But, it is not feasible; there are major technical challenges with this technology.
- $GaInP_2/GaAs$ has no rival for efficiency, so concentrating on aqueous stability makes sense.
- The approach incorporates long-planned combined theoretical materials analysis, leading to the understanding of behavior through materials characterization and performance observation and demonstration. Further attention to material cost and mitigation strategies might be warranted. Such strategic effort should consider the trade-off

between extending the search for equivalent or better materials and continuing the focus on the current material that performs well beyond any other known material.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.0** for its accomplishments and progress.

- The team continues to make good progress in demonstrating high efficiency.
- There are significant similarities among the 2011 and 2012 project results. It is not clear why DOE funded two projects under the same title to the same group. One started in 1991 and ended in October 2011, and one started in 2004 and is projected to end in September 2012.
- The new (preliminary) results on nitrided $GaInP_2N$ appear to be a major breakthrough with respect to improving the durability of the semiconductor material. There now needs to be a focus on further fabrication of this material to validate that these results can be repeated across larger sample sets. It appears that the samples that showed the significant progress were the same as presented in 2011. This reviewer would have expected (in light of the promising 2011 preliminary results) that a significant effort would have been made toward fabricating and characterizing a larger sample set.
- One accomplishment is the endurance of nitride surface passivation treatments, which showed >100 hours with minimal decay. All milestones have been met or are on track for completion. The team also performed efficiency benchmarking testing at the National Renewable Energy Laboratory (NREL) under real-world sunlight for comparison with laboratory light spectrum. The team is making continued progress in PEC reporting standardization.
- The major discussion was over whether etching with N_2^+ was a good idea. It appeared that the trade-off between decreased performance and increased corrosion resistance was acceptable. Unifying testing protocols is a good idea, but the researchers need to be cautious to not make the process so complicated that NREL is the only laboratory on the planet that can do the characterization.
- The demonstration of >100 hours of durable electrocatalysis operation without observable degradation in performance and with no immediately obvious material deterioration is an outstanding achievement and is presently unmatched in the field. Nevertheless, continued study, especially at the atomic level, of interface effects under conditions of full-spectrum photocatalysis should be undertaken before material durability concerns for this active substance can be said to have achieved intermediate performance goals. Additional effort is needed to better characterize the nature of nitrogen incorporation in treated samples by addressing distribution and distinction between nitride formation and nitrogen embedded freely within the treated material matrix. Such work would advance the understanding of the nature of this corrosion protection and would help focus materials stabilization schemes for this and other complex active materials. Collaboration in developing and validating theoretical studies of more complex material formulation should be emphasized. Such work should include the material for which progress is reported in this review.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.7** for its collaboration and coordination.

- The investigators make good efforts to collaborate with others.
- As a central organization to this effort, NREL has established significant collaborations with many members of the PEC community and across a wide spectrum of technical expertise, including fabrication, modeling, and characterizations.
- This project features an extremely corroborative team that very logically divides the work based on who can conduct each aspect most efficiently. It is a model of collaborative effort.
- All of the institutions affiliated with the Fuel Cell Technologies Program (FCT Program) PEC portfolio look to NREL for leadership, but only a few (the University of Nevada, Las Vegas and Lawrence Livermore National Laboratory) had much of an impact on NREL's effort on $GaInP_2/GaAs$.
- This project has taken full opportunity for collaboration and coordination provided through the PEC Working Group and makes excellent use of the skills and products afforded by other Working Group members.

Question 5: Proposed future work

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

- The project has well-laid-out plans for the future.
- All of the stated goals are fine. Based on the new results concerning nitriding of $GaInP_2$ surfaces, investigators should take a more narrowly focused effort on fabricating and characterizing a larger sample set of material in the near term, focusing on understanding the mechanisms for improved durability.
- Demonstration of a tandem cell with >1,000-hour durability (made possible by the surface nitriding treatment) would be a major achievement. One reviewer is glad to see that the investigators are pursuing this benchmark.
- The proposed future work is nothing overly exciting, but what was proposed was a logical extension of what the investigators presented.
- The stated plans in the review implicitly incorporate some of another reviewer's suggestions. Nevertheless, the progress reported should be amplified through explicit planning to fully comprehend the nature and basis of the observed performance improvement. This project, along with its collaborators in the PEC Working Group, should address issues regarding prioritizing investments between new material searches and continued improvement of current materials.

Project strengths:

- The project offers some novel and outside-the-box thinking and R&D approaches.
- This project features good cell performance and a good combination of theory and experiment.
- This is clearly the flagship facility for the FCT Program's PEC portfolio. The NREL group is the champion of the III-V semiconductor material—the material that has shown the most promise to date. The primary issue has always been the poor durability of the base material, and results over the last year indicate that a nitrided surface can alleviate this issue. Recent results for the surface nitride $GaInP_2$ and for the experimentation with pure InGaN are encouraging.
- The strength of this project lies with its logical organization and collaborative spirit. Substantial progress in PEC performance and endurance is required to meet DOE goals. However, the project is well constructed and uses an approach that is broad enough to encompass several different pathways to the goal.
- The project is focused on those high-priority objectives established earlier for the PEC portfolio. Outstanding technical proficiency is evident and extraordinary progress is reported in overcoming high-priority barriers to PEC performance. Coordination and collaboration with other institutions, organizations, and community expertise is outstanding, and this component of the research effort has directly contributed to the progress reported.

Project weaknesses:

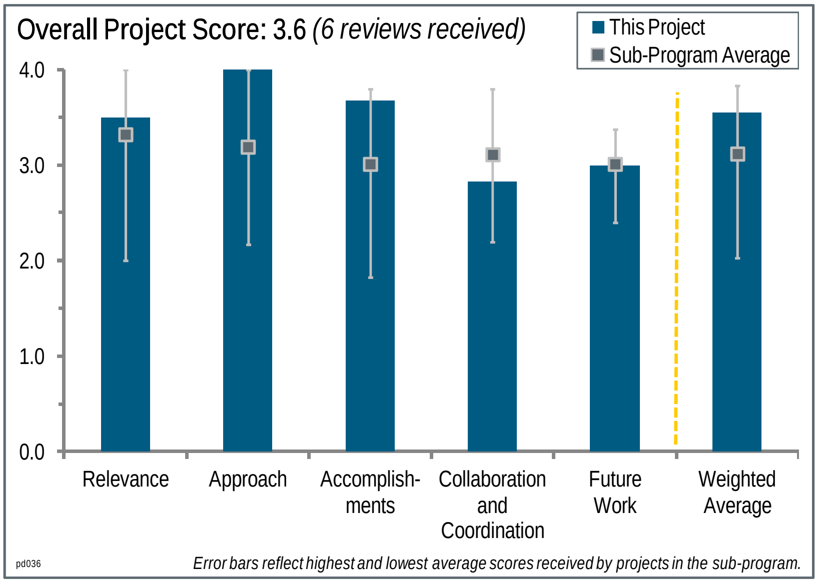
- No significant progress was made with this project since 2011. It is understandable that this project is still in the materials discovery and development phase. However, there are major technical challenges with this technology.
- The team could better address what it thinks the upside potential will be for the nitriding process. It is not clear whether it thinks the approach can yield a long-term solution or if it will only be a mid-term durability solution.
- It is unclear if high-efficiency, very expensive solar cells will be the right platform to develop.
- Surface nitriding may help, but it represents yet another process step in an already expensive fabrication technology.
- Funding levels and project workforce levels will limit the rate of progress.
- Although DOE encourages collaborations, which are wonderful opportunities to advance technologies, collaborations should not consume the effort to the point of distraction. A significant portion of the presentation dealt with the effort of other groups, either material fabrications (e.g., Williamson, Los Alamos National Laboratory) or characterizations (e.g., Heske, Ogitsu). It was therefore difficult to extract the message for significant accomplishments over the last year. Again, this reviewer would have thought, based on 2010's (2011 presentation) results, that the nitrided $GaInP_2$ would have been pushed and more fully explored.

Recommendations for additions/deletions to project scope:

- The practicality of such a technology needs to be evaluated by DOE. There are many technologies that provide significant technical advancements over the current technologies, but they end up not being very practical due to mass production and fabrication challenges, being integrated in an end-to-end system, and high costs.
- In the short term, the team should work on the GaInP-N with laser focus to determine if it is indeed a viable material when the durability issue has been solved. The PI mentioned that the cost for these materials can be offset using 10-X concentrated conditions. If this is the envisioned use for the III-V material class, then durability testing should be performed under these 10-X conditions to verify that photo-corrosion is not problematic at these elevated intensity levels.
- The project team needs to address cost issues of nitrided gallium PEC cells. Even if the durability is improved, this reviewer wonders if these cells would be cost effective. Investigators need to evaluate the performance and durability of cells under solar concentration. Concentration may be a solution to high material costs. However, investigation of treatments under solar conditions should be examined.
- A question was made about scale-up issues with regard to metal-organic chemical vapor deposition (MOCVD) It was pointed out that light-emitting diode technology seems to be doing fine with essentially the same fabrication technology. That may be true, but to make a dent in U.S. electricity demand, a household will need a photovoltaic array considerably larger than their television and computer screens. Such studies likely already exist, but it would nice for the Program to have the numbers for large-scale production at hand.
- Continued study, especially at the atomic level, of interface effects under conditions of full-spectrum photocatalysis should be undertaken before material durability concerns for this active substance can be said to have achieved intermediate performance goals. Additional effort is needed to better characterize the nature of nitrogen incorporation in treated samples by addressing distribution and distinction between nitride formation and nitrogen embedded freely within the treated material matrix.

Project # PD-036: Maximizing Light Utilization Efficiency and Hydrogen Production in Microalgal Cultures

Tasios Melis; University of California, Berkeley



Brief Summary of Project:

The objectives of the project are to: (1) identify genes and associated molecular mechanisms that confer a truncated light-harvesting antenna (Tla) property in the Tla2 and Tla3 strains of *Chlamydomonas reinhardtii*, and (2) develop protocols for the targeted truncation of the light-harvesting antenna size in cyanobacteria. These objectives are accomplished through cloning *tla2* and *tla3* phenotype genes, performing functional analysis of the transformants, and applying the TLA concept to cyanobacteria.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.5** for its relevance to DOE objectives.

- This project is clearly directly related to the objective of the DOE Hydrogen and Fuel Cells Program (the Program) to produce cost-effective renewable H_2. Increasing the utilization of captured sunlight to split water supports this objective.
- This is an outstanding piece of work, but, in the general scheme, it addresses only one of a myriad of issues affecting efficient and enduring photobiological H_2 production. Plans for integrating products from this effort were made evident during the presentation, but the lack of collaboration and coordination for program implementation were also evident, both in the project presentation and in audience participation in the review.
- The project improves the efficiency of photosynthetic organisms whose photon collection apparatus exceeds its processing capacity. In the case of algae-based H_2 production, this project strongly supports DOE objectives related to the increased efficiency of light utilization and the reduced cost of biological H_2 production.
- The project is based on increasing the efficiency of photobiological hydrogen (H_2) in the green algal *Chlamydomonas reinhardtii* to reduce the amount of heat dissipated from bright sunlight and increase the amount of transmittance of light through the growing algal culture by truncating the chlorophyll antenna size. This project aligns with the longer-term pathway goals and objectives of the Hydrogen Production and Delivery sub-program in the biological production of H_2. Determining the genetics behind antenna size may be seen as basic research; however, the principal investigator (PI) is doing a good job of focusing the project on the potential applications.
- The presenter's work focused on optimizing photosynthetic efficiency of a model organism of H_2 production, *Chlamydomonas reinhardtii*. The PI has exceeded the targets set for the project. The researcher has made efforts to protect intellectual property, giving rise to the potential commercialization of the discoveries. The discoveries of this presenter could have major ramifications in many fields that work with photosynthetic organisms.
- The project focus on increasing light utilization efficiency and H_2 production in a biological system (microalgal cultures) is clearly relevant for DOE. The study of genetic determinants influencing antenna size could potentially be considered basic research, but the PI clearly has long-term goals relevant to H_2 production in mind in the design of this project. While developing microorganisms with optimized antennae for light harvesting is an important goal, the project, as presented, seems to be directed more toward generally enhancing photosynthetic efficiency than enhancing photobiological H_2 production.

Question 2: Approach to performing the work

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

- This project is well designed and focused on accomplishing the Program's objectives.
- The success of the project has demonstrated the effectiveness of the approach in addressing the efficiency and, consequently, cost barriers. The interest in extending this to photobacterial systems demonstrates its integration with other efforts.
- The approach detailed in 2011 involved identifying genes to enable a truncated antenna size by performing a forward mutant screen and then using fluorescence imaging analysis to screen the mutants. The researchers identified three *tla* mutants and are currently in the process of phenotyping and genotyping the mutants. All of the methods are appropriate for analyzing genetic mutant strains, both genetically and biochemically.
- The presenter had a clear plan of action that had a high chance of succeeding. The PI used classic genetic techniques to identify the genes of interest and has identified structural motifs of those proteins.
- The experimental approaches are well designed, logical, relatively straightforward, and focused on identifying and characterizing genes/proteins involved in determining chlorophyll antenna size. The PI is expanding the studies in *Chlamydomonas* to cyanobacteria, which is appropriate. It is not clear whether experiments are addressing potential environmentally induced regulation or signaling, but this may be outside of the scope of the project and related funding. The focus on creating uniformly small antennae seems appropriate because these strains will most likely need to be limited to closed photobioreactor systems where light intensity, quantity, and quality can be controlled.
- The approach to achieving the objective has proven very effective. Nevertheless, it is based only on what is known, and no effort is apparent to develop greater understanding of photoactive processes that could identify alternative routes to improving light utility by the light harvesting ingredients of the same or other photosystems.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.7** for its accomplishments and progress.

- The progress is significant and consistent toward the project goals.
- This project has achieved all of its milestones ahead of schedule.
- The accomplishments and progress are clearly outstanding, but the rate of achievement seems remarkably slow. That perception could well be a figment of the admittedly deep ignorance of this reviewer. It is not possible for this reviewer to judge whether the progress is limited by attention and effort level, technical difficulty, or the inherent slowness of research steps.
- It is clear that the PI has made great strides in what is presented as a key element to the project goals. However, the same protection of intellectual property rights may have prevented the presenter from presenting as complete of a story as perhaps might have been otherwise presented. This is not a criticism of the progress, however. It is clear that the project goals are being met.
- For all three *tla* mutants, it appears the researchers have made significant headway. *tla1* appears to be tentatively identified as a variant of MOV34/MPN-containing proteins. With *tla2*, there appears to be the most significant progress with regard to determining where the genetic insertion occurred and what genes were affected and responsible for the phenotype. A patent application has been filed for the gene. The *tla3* effort is making headway, with genetic and physiological characterization completed and biochemical analysis to be finished soon. The researchers have published several papers in well-recognized journals in the past funding year. The elucidation of the genes that affected *Chlamydomonas reinhardtii* antenna size could be applied to other microalgae because, as the researcher states, *tla1* and *tla2* are highly conserved. It also appears that the PI is making great strides in achieving DOE targets for sunlight utilization efficiency and reduction of chlorophyll antenna size, specifically with the *tla3* strain.
- The team has made excellent progress overall in identifying and characterizing genes that are important for chlorophyll antenna size and reaching (and potentially exceeding) the targets. As stated at the presentation, this project was the first to appreciably improve photosynthetic efficiency. The PI also notes that interest in this research has broadened from microalgae to higher crop plants. These studies could lead to increased efficiency of solar energy capture and, in turn, an associated increase in photobiological H_2 production. The only issue is whether the project has evolved to focus more on photosynthetic efficiency rather than H_2 production, leading to

a question of relevance and direct impact for the Hydrogen Production and Delivery sub-program. Perhaps this concern is more a matter of how the project was presented, because light capture can certainly be considered a barrier in regard to increasing photobiological H_2 production.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- The project is well coordinated with NREL and others that will use the technology to produce H_2 and other products.
- This reviewer does not recall the researcher describing collaboration or coordination with other institutions. This has not seemed to hamper his progress.
- This project has not required significant collaboration to achieve its goals. However, others have sought collaboration with it to achieve their goals.
- There are no specific collaborators, although the mutant strains and techniques are being used by others, including the National Renewable Energy Laboratory (NREL), for H_2 production and for increasing the biomass and production of polyunsaturated fatty acids. The *tla1* strains are shared among numerous universities, industries, government laboratories, and high schools.
- The research is primarily conducted at the University of California, Berkeley, with some collaboration with researchers at NREL (which appears to be more providing strains). However, strains are being made available through the *Chlamydomonas reinhardtii* resource center, which allows them to be used by other university laboratories, industry, and government laboratories (and even high schools). There has also been commercial use of the Tla approach.
- There is clear awareness within the Program of work undertaken in this project. There is further indication of intention by others to incorporate advances in this project in other work. At the same time, there is no evidence of effort on the part of the PI to involve other institutions or organizations in furtherance of the Program's objectives. Additionally, there is no evidence of effort on the part of either the Program or other institutions or organizations to attempt collaboration or coordination that might serve to accelerate project progress or to accelerate integration of its progress with activities that would presumably benefit from improved light harvesting efficiency.

Question 5: Proposed future work

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

- With the exception of some final analyses and publishing the results, the project is complete. The extension of the approach to improving the efficiency of photobacteria is commendable.
- One reviewer is looking forward to the completion of the work on Tla3.
- Currently the researchers plan to finish analysis of *tla3* strain to proceed to eventual publication. The TLA concept in cyanobacteria is currently in progress. No description was given of the extended photosynthetically active radiation (ePAR) concept mentioned on the slide.
- It was not clear whether the proposed experiments for the *tla* project match the scale of the previous year's work. The researcher might be underestimating the challenges of changing model organisms. Finally, the proprietary designation of the ePAR project prevents another reviewer from accurately assessing the short- or long-term plans.
- The proposed future studies clearly build on previous results and will extend the project to cyanobacteria. These studies are relatively narrowly focused, which is appropriate considering the size of the project in general.
- Mention of the ePAR concept without any further description is of no value whatsoever to this review. The demonstration of feasibility of TLA in cyanobacteria could be valuable, but the absence of planned tasks provides no management tools for the Program to measure or judge progress.

Project strengths:

- The project seems very well focused, and it is obvious that the PI has a direct plan for completing the project with achievable goals.

- The PI has demonstrated rapid progress and has compiled expressive results.
- This ongoing project has shown significant progress over the last years and is contributing to Program goals toward producing cost-effective renewable H_2.
- Strengths of this project include its excellent progress, straightforward and logical experimental approaches, and efforts geared toward developing and sharing tools for modifying chlorophyll antenna size.
- The technical proficiency is clearly outstanding. The researchers have made clear progress toward the project objective.

Project weaknesses:

- There is some concern about the feasibility of the TLA concept in cyanobacteria, which is significantly different than *Chlamydomonas reinhardtii*.
- This reviewer hopes that more of the proprietary information will be released by the next presentation; the PI may run into stumbling blocks changing organisms.
- There is a question of relevance, specifically whether the project is directed more toward photosynthesis rather than H_2 production.
- One weakness is the collaboration and coordination with the photobiological hydrogen production portfolio. The future work planning description is inadequate.
- This reviewer stated that the project has no weaknesses.

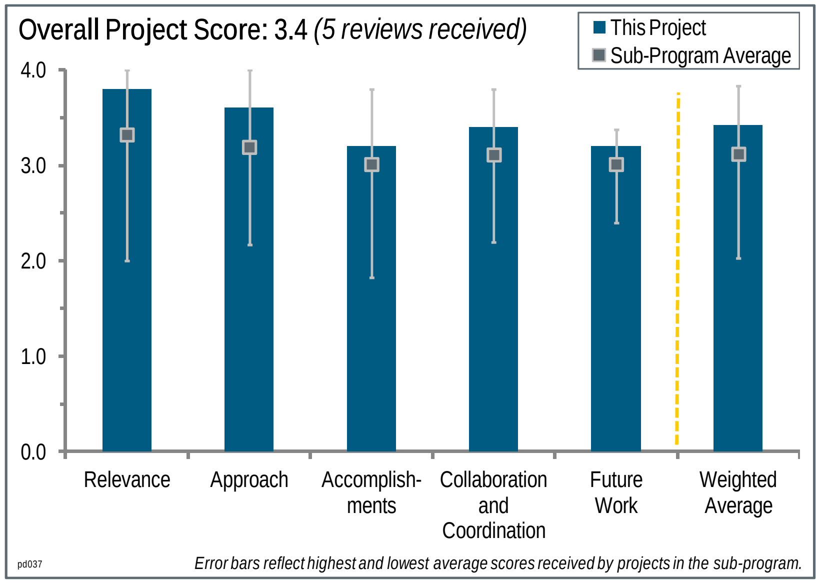
Recommendations for additions/deletions to project scope:

- The PI should develop collaborations with experts for cyanobacteria experiments and potentially use available mutant libraries for analysis. It would also be nice to see the research on Tla1, Tla2, and Tla3 translated into commercial algal strains.
- The PI’s work could be revolutionary and have implications in many fields; however, the project needs to be more linked to H_2 and present more data on how these changes to the photosynthetic structures change H_2 yields.
- The PI should seek appropriate intellectual property protection and then disseminate ePAR information that is adequate to enable recommendations regarding continued or expanded research and development support.

Project # PD-037: Biological Systems for Hydrogen Photoproduction

Maria Ghirardi; National Renewable Energy Laboratory

Brief Summary of Project:
The objective of this project is to develop photobiological systems for large-scale, low-cost, and efficient hydrogen (H_2) production from water. Specifically, the project's two tasks are to: (1) address the oxygen (O_2) sensitivity of hydrogenases that prevent continuity of H_2 photoproduction under aerobic, high solar-to-hydrogen (STH) light conversion conditions; and (2) utilize a limited STH-producing method (sulfur deprivation) as a platform to address or test other factors limiting commercial algal H_2 photoproduction, including low rates due to biochemical and engineering mechanisms.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- This researcher has clearly stated goals regarding H_2 production. The principal investigator (PI) has clearly presented goals and tasks from genetics and physiology.
- The project's objective to develop a photobiological system for large-scale, low-cost, and efficient H_2 production from water is a clearly stated objective of the DOE Fuel Cell Technologies Program (FCT Program) Multi-Year Research, Development, and Demonstration Plan.
- This project is critical to the Hydrogen and Fuel Cells Program (the Program) and fully supports the objective of cost-effectively producing renewable H_2.
- Low STH energy conversion efficiency is the primary shortfall in biological H_2 production. This project is directly relevant in that it squarely addresses this issue.
- The project is consistent with and supports DOE research, development, and demonstration objectives for biological H_2 production .

Question 2: Approach to performing the work

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

- The PI assessed that one branch of the project had no chance of success after exhausting all reasonable options and ceased work on it, demonstrating good judgment. The other tasks seem to use classical techniques to solve real challenges in the project and, to this reviewer, seem to be taking the right approach.
- This project is well designed and focused. The researchers are to be commended for recognizing and incorporating innovative new directions to overcome unexpected results.
- After the December 11 no-go decision, the work approach has centered on genetic engineering expressions to characterize in vivo O_2 sensitivity, and testing using a sulfur-deprived platform as a testbed.
- The project's suspension of targeted random mutagenesis work to acquire O_2 tolerant hydrogenases is commendable and demonstrates risk mitigation by the pursuit of lower-risk alternatives.
- In general, the approaches seem to be reasonable. For instance, attempts to minimize the O_2 sensitivity of hydrogenases by introducing the Ca1gene encodons into *Chlamydomonas reinhardtii* have yielded some valuable results. The finding that there are multiple O_2 production sites further exposes the challenge of

producing a completely O_2-tolerant hydrogenase. Further attempts to identify biochemical and engineering conditions by which STH production using suspension algal cultures can be enhanced are in progress and have shown that headspace volume of H_2 is one manageable component to improve rate and yields.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.2** for its accomplishments and progress.

- This project has made steady and significant progress toward project and Program's goals.
- The project has well-defined milestones and decision points and appears to have completed them on schedule.
- The PI understands when to cut a project where no more progress can be made. The PI has an alternate plan in place where results can again be assessed quickly. Progress on task 2, the testing of ATP synthase mutants, seems more limited. Mutants are generated and one is expressed, but any new physiology results were not presented. Results from the continuous H_2 production were limited to a single statement about one experiment, and the significance of the headspace observations could be questioned; more understanding of why the organisms change production with different amounts of headspace is needed.
- The finding that there are multiple O_2 production sites further exposes the challenge of producing a completely O_2-tolerant hydrogenase; however, given that one site has been switched off, it gives hope that through multi-gene stacking or other approaches the team may ultimately find success. Also, demonstrated progress is found in the 565 ml of H_2 gas per liter of the suspension culture, which is, according to the investigators, the highest yield ever reported for wild-type strain in a time period of less than 180 hours.
- Researchers have been able to express a double hydrogenase knockout mutant, insert the Ca1 gene, and observe substantially increased light-dependent H_2 production. Continuous flow of the medium did not produce the desired results. More should be explained as to what operational conditions might be changed to improve the results. Headspace volume dramatically improved the H_2 yield, although the reason is unclear.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- The PI seems to have good working relationships with the collaborators; there seems to be a lot of feedback between them.
- Appropriate collaboration exists and appears to be well coordinated.
- Given that the project has been ongoing since 2000, at some point in the near term it would be good to add an industry partner or at least understand why there is no interest. This will be helpful in terms of understanding commercial feasibility.
- This project appropriately and closely collaborates with scientists from Russia, North Carolina State University, Johns Hopkins University, and the Massachusetts Institute of Technology, along with related projects associated with the FCT Program portfolio.
- The collaborators' roles were not well defined in the presentation, but they were expanded upon in the reviewer notes.

Question 5: Proposed future work

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

- The plans are reasonable and focused on continued progress.
- Continued project funding is determined annually. If funded, the project continues current Ca1 and sulfur-deprivation work.
- One reviewer feels that progress in each task was not obvious, and it was unclear how progress will be made in each task. This reviewer wants to know how the researchers will explore the headspace result. The go/no-go points for many of their other experiments were unexplained.
- Another reviewer very much liked the no-go decision on the more O_2-tolerant Ca1 hydrogenase.

Project strengths:

- The people working on this project have a keen eye for innovation and seem to be good judges for the progress of projects. The collaborations are active and seem to help move the progress of the project along both in terms of physical work and intellectual discussion.
- The project features an excellent team of researchers.

Project weaknesses:

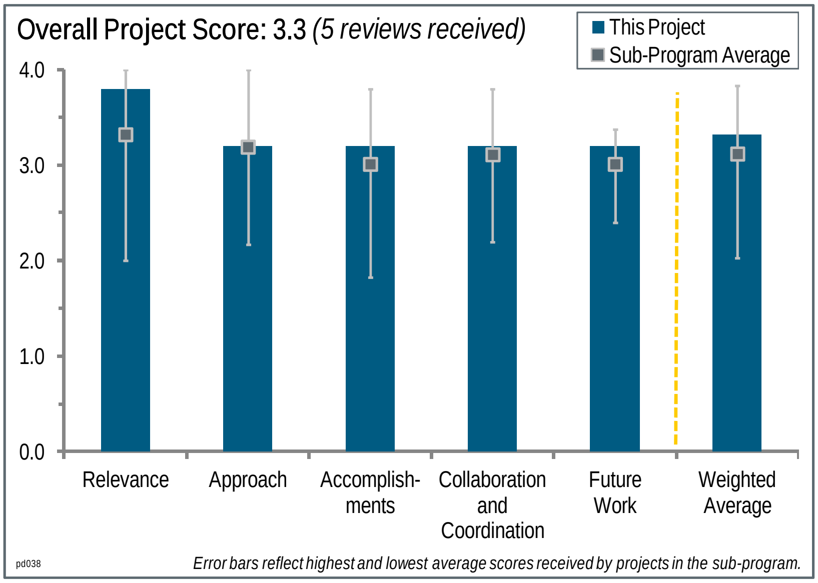
- Task 2 projects need to make more progress, and more explanation is needed of where they are going.
- Researchers have been slow in making significant progress in meeting the DOE goals. This reviewer guesses this is why this project is appropriate for government funding as opposed to industry taking on the risk.
- There is a lack of specificity in the interim goals that makes it hard to assess whether the project is on track or not. Consequently, there should be a fuller self assessment of progress compared to where the PI thought the project would be and what the PI's internal goals are.

Recommendations for additions/deletions to project scope:

- The headspace experiment seems to be where progress could be elusive. Sometimes great results can come from a simple change that no one has thought of before. More often, though, the reason why the result is new to the researchers is because other's efforts to explain phenomena have gone unpublished due to the inconclusiveness of the results, or because there is a more mundane explanation of the results.
- It is unclear what conditions other than headspace will be modified in order to increase H_2 production yields in suspension algae. Also, an industry partner might be helpful in terms of understanding what is commercially reasonable as opposed to being strictly technologically successful.
- The headspace results need to be better examined. Headspace is likely not the correct parameter; rather, gas composition, mixing, pressure, or something else seems to be having an effect. This reviewer recommends that researchers propose a theory and then experimentally confirm that theory.

Project # PD-038: Fermentation and Electrohydrogenic Approaches to Hydrogen Production

Pin-Ching Maness; National Renewable Energy Laboratory



Brief Summary of Project:

The objectives of this project are to: (1) develop direct fermentation technologies to convert renewable lignocellulosic biomass resources to hydrogen (H_2); (2) address feedstock cost and improve the performance and durability of bioreactors for H_2 production via fermentation of lignocellulose; (3) improve H_2 molar yield (mol H_2/mol hexose) via fermentation; (4) improve plasmid stability in *Clostridium thermocellum*; (5) develop an alternative forward-evolution strategy to block ethanol production; and (6) improve H_2 molar yield (mol H_2/mol hexose, N) by integrating dark fermentation with a microbial electrolysis cell (MEC) reactor to convert waste biomass to additional H_2.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The presenters had clearly stated goals involving H_2 production.
- This project is clearly aligned with the DOE Hydrogen and Fuel Cells Program (the Program).
- This project is clearly relevant to the Program's objective of cost-effectively producing renewable H_2.
- The project is consistent with and supports the objectives of The Fuel Cell Technologies Program Multi-Year Research, Development, and Demonstration Plan.

Question 2: Approach to performing the work

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

- The integrated approach with the National Renewable Energy Laboratory and the Pennsylvania State University is a highly effective way to increase the molar yield of H_2 production using more of the biomass.
- The approach is logical and well constructed. It is still unclear whether the desired approach is a co-culture or a single organism for the fermentation. The presentation presented results for a co-culture, but this reviewer comments imply that only *Clostridium thermocellum* is being pursued.
- The project is an interesting blend of bioreactor optimization, metabolic engineering, and a novel science and engineering approach to H_2 yield improvement that has very different risk profiles, which provides some confidence that at least the lower-risk objectives (if not all) will be achieved in a timely fashion.
- The principal investigator (PI) had a great grasp of the mechanics involved in the production of H_2 and applied that knowledge to build a better bio reactor. The approaches on the metabolic engineering side were, at times, confusing. It is not clear why the PI chose to make a *dcm* strain when many are commercially available. It is not clear to this reviewer how exposure to acrolein alone will result in inactive alcohol dehydrogenase (ALDH) without further rounds of mutagenesis, or how these mutants will be isolated if components of the media are detoxifying the acrolein, as was implied by the PI. Furthermore, it is unclear what effect shutting off the ethanol

pathway will have on the organism. It is not clear whether this is a guarantee of more acetate production, as the conversion of the acetyl-CoA path to ethanol may serve a physiologically relevant function.

- The trend data for *Clostridium thermocellum* and co-culture degradation of cellulose comparing cellulose concentration versus H_2 rate and yield provides very useful information. The results from modifying the sequencing of cellulose in the fed-batch reactor are also helpful. Also, the attempts at genetically modifying the cellulose to switch off ethanol production so that more H_2 is produced seem reasonable. However, it is not clear why MEC has been added to this project.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.2** for its accomplishments and progress.

- Each partner in this project has made significant progress on its own and collaboratively.
- The project has well-defined milestones and, with the exception of the MEC work, appears to have completed them on schedule. The MEC encountered contract and funding issues that created understandable delays, but it appears to have recovered very well.
- The findings that the co-culture produced 31% more H_2 than the *Clostridium thermocellum*, and that H_2 output of co-culture on untreated corn stover is 74% of that of *Clostridium thermocellum* alone advances the project toward the DOE cost and yield goals. Also, the production of the stable mutant strain *Clostridium thermocellum* defective in alcohol dehydrogenase for pathway knockout construction is a key advancement.
- Demonstration that the active microbes are immobilized on cellulose, and thereby allow easy separation of the growth medium from the acclimated culture, is a significant step toward scale-up and efficient, low-cost operation. The team achieved a milestone with the 20% increase in H_2 production and demonstration of scalability. The use of co-culture allows greater H_2 production and use of untreated cellulose. The team also developed protocols and mutants to increase H_2 yield and block ethanol production.
- Clearly the author has progressed in the batch reactor experiments, which is to be commended. It is unclear to this reviewer why the *E. coli* used had to be the conjugation strain. Many strains are capable of conjugations, and other techniques, such has tri-parental matings, solve the problem of transferring plasmids to different species. It is concerning that researchers in the presenter's laboratory are re-inventing wheels (making unnecessary strains) rather than exploring other published options for moving plasmids around. That being said, the presenter's laboratory deserves credit for developing this genetic tool. The researchers now need to exploit their tools as quickly as possible to perform their genetic experiments. This reviewer cannot tell whether the authors are on track to achieve the milestone of making an ALDH pathway knockout construction. It seems that as presented, the project is in its very early stages. The presenters do seem to be on track for the electrochemically assisted microbial fermentation, and the compositional analysis.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- This project features excellent collaboration.
- The researchers might need closer contact with Dr. Levin and Sparling to keep the genetics project on track.
- Project collaboration appears to be well coordinated and feature strong partners.
- It is unclear why there are not more collaborators on this project. It would be helpful to have industry interest.

Question 5: Proposed future work

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

- The results from the pathway knockout mutants and the bioreactor synthesis optimization make logical sense.
- Both collaborators in this project have plans that are clearly built on past progress and are reasonable and focused toward reaching the project and Program goals.
- There is a well-specified listing of future work. However, this reviewer would like to see more specific performance targets for each task.

- Continued project funding is determined annually. If funded, the project will continue bioreactor performance optimization, metabolic engineering, and electrochemically assisted fermentation efforts.
- The fermentation experiments have clearly defined goals and expected outcomes. The researchers have built their *Clostridium thermocellum* strain with the stable plasmid, now they need to execute their counter screen for their *pyrF* knockout. This reviewer would like to see more details of how that will be accomplished. This reviewer wants to know what media will be used and what phenotypes are expected. Once they isolate their mutant, the measurement of H_2 yields should be completed rapidly. It is not clear to this reviewer, given the researchers' media conditions and lack of strategy for generating mutants, how they will select for allyl alcohol killing survivors, what phenotypes they expect, or at what point they know that the project is a dead end. The goals for task 3 seem unambitious and seem to not solve a central problem of how this system will ever produce a net gain in usable energy.

Project strengths:

- There are some excellent collaborations that allow presenters to create new ideas.
- It looks like the PI is accomplishing a lot in spite of inconsistent funding.
- The demonstration of a cyclically operating batch reactor showing the scalability of H_2 production is a major accomplishment of the project.

Project weaknesses:

- Some of the historical decisions made by this group can be questioned. The researchers have accomplished some of their goals; they need to capitalize on those successes to continue making progress toward their goals.
- It is not clear why MEC is necessary. This reviewer wants to know whether this adds complications to the fermentation process.
- This reviewer is concerned about the size (and thus cost) of the MEC reactor.

Recommendations for additions/deletions to project scope:

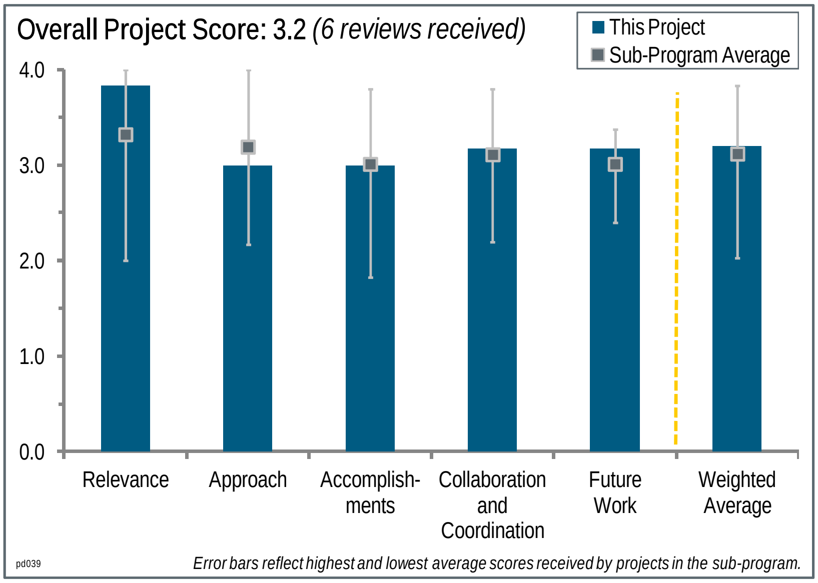
- Given that the researchers have developed a genetic tool for *Clostridium thermocellum*, they need to focus on all they can accomplish with that system. New mutant hunts may not be in the laboratory's best interest unless the researchers can identify the locus of each mutation. There needs to be clear go/no-go checkpoints for task 3.
- This reviewer recommends that more detailed metrics be established for the MEC reactor, specifically dealing with the size of the required electrode system. This reviewer is concerned that the diluted liquid feedstock coupled with a low electrode current density will require a large reactor system that is cost prohibitive. Current density should be a reported parameter from the MEC research. Instead of applying an MEC voltage bias, it was suggested that heat could serve a similar purpose. The team should expand on this possibility as a potential pathway to system cost reduction.

Project # PD-039: Hydrogen from Water in a Novel Recombinant Oxygen-Tolerant Cyanobacterial System

Phil Weyman; J. Craig Venter Institute

Brief Summary of Project:

The objective of this project is to develop an oxygen (O_2)-tolerant cyanobacterial system for continuous light-driven hydrogen (H_2) production from water. The project hopes to demonstrate a fivefold increase in hydrogenase activity from environmental hydrogenase and cyanobacteria as measured by in-vitro H_2 evolution assay, and to improve hydrogenase-ferredoxin (Fd) electron transfer to enable a 25-fold improvement in Fd docking to hydrogenase.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.8** for its relevance to DOE objectives.

- The identification and optimization of O_2-tolerant hydrogenases is important for the ultimate goal of producing H_2 during photosynthesis. This is very relevant to the long-term pathway goals and objectives of the Hydrogen Production and Delivery sub-program related to the biological production of H_2.
- The project goal is to identify O_2-tolerant hydrogenases and move them into model cyanobacterial strains, or to improve H_2 production by the native enzymes.
- Project objectives are clearly focused on the DOE Hydrogen and Fuel Cells Program's (the Program's) objective of cost effectively producing renewable H_2.
- The project's focus on developing O_2-tolerant hydrogenases should provide significant tools and insights for improving the continuity of H_2 production. Thus, it has clear relevance for the Program.
- Resolving the continuity of H_2 production is only one of many barriers to effective photobiological H_2 production, so the lack of coordination of this element with other facets of the problem could waste resources and prevent the wise allocation of resources according to prioritized needs.
- The project is consistent with and supports DOE research, development, and demonstration (RD&D) biological H_2 production. The discovery or engineering of a cyanobacteria possessing an O_2-tolerant hydrogenase would be an enabler to continuous biological H_2 production, overcoming a major DOE RD&D barrier.

Question 2: Approach to performing the work

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

- The J. Craig Venter Institute (JCVI) and National Renewable Energy Laboratory (NREL) groups have a systematic path detailed in the milestones that is appropriate for the work. The groups are well equipped to complete their objectives in a timely manner. Their genetic approaches are suitable, although this reviewer feels that looking at levels of RNA and protein produced may be beneficial to understanding what genes are "more important" in the environmental hydrogenase project instead of just putting more promoters in front of genes. From the western blot of pre-HynL to HynL, it would further suggest that something else is potentially limiting and needs to be determined. The T7 polymerase strategy seems to be quite interesting and will hopefully be useful for this project.

- Parts of the principal investigator's (PI's) approach are excellent. The identification of hydrogenases and the synthesis and cloning of the genes is efficient and effective. The second part of the project, increasing expression of the genes of interest, failed to be logical as presented. It was not clear why breaking up the single operon into four operons would have changed protein yields. The use of the T7 promoter system will likely increase protein yields, but, unless modulated, the levels of individual proteins may increase to an amount such that other cell functions would be impeded. A much more complex question not addressed in the presentation is the question of what ratio of the various protein components is required to allow functional hydrogenase complexes to be formed.
- The two collaborating research groups have individual approaches that are integrated to address the necessary barriers—the project is well designed, feasible, and integrated.
- The research strategy is logical and straightforward with clearly stated milestones. Both JCVI and NREL have access to unique resources that have been valuable to the research. The criteria for describing the enzyme as a thermostable, O_2-tolerant hydrogenase were not discussed in detail. For instance, it is unclear what temperature range resulted in the term "thermostable." While relatively well designed, it seems that in some instances, the approaches could use less "brute force" and be a bit more strategic. Alternative approaches were not considered in detail—at least in the presentation. For instance, methods to increase gene expression seem to focus more on adding more promoters, operons, etc., than on tweaking the expression of different components. Perhaps it is not a matter of adding more, but of changing protein ratios. There was also little discussion or consideration of physiology, although this can play an important role in H_2 production in these systems.
- There is no particular evidence provided that underscores the need for simultaneous pursuit by separate institutions of the same objective through development of two distinct constituents with the same purpose. Such lack of focus hinders accelerated progress. There is no a prior evidence presented to select one over the other, but it is unlikely that two parallel efforts will make a difference in the face of the millions of possibilities. There might be cogent arguments commending the approach and the selection of options, but such arguments were not apparent to this reviewer. From a management perspective, prime recipient milestones are essentially absent for nearly two years, preventing any effective oversight of project progress. In the same spirit, a complete lack of quantitative performance milestones for both participants could permit this project to continue indefinitely without significant contribution to the photobiological H_2 production portfolio. Whereas hydrogenase activity has been improved through promoter engineering, it remains unclear "how much is enough." A "100-fold increase" might be well below useful levels of activity, depending on the starting point. The prime recipient approach to seeking natural O_2-tolerant hydrogenase organisms appears to be significantly deficient. One might, for example, assume that O_2-tolerant hydrogenase organisms would prevail in O_2-rich waters of the global ocean so that a search for such should be driven by the distribution of ocean chemistry, a distribution that is well documented through years of surveys and samplings by physical and chemical oceanographers. No apparent effort to coordinate the sampling with ocean chemistry is evident. If, as suggested by the presenter, it is true that all samples were taken from "oxygen-rich surface waters" then one must wonder why the global sampling project should have been tapped to gather samples at all.
- NREL and JCVI are well integrated in pursuing their parallel efforts to acquire a cyanobacteria possessing an O_2-tolerant hydrogenase. NREL is developing two *Synechocystis* recombinants that possess different promoter arrangements. The pursuit of two approaches, as well as two recombinants in one approach, to develop an adequate organism is a valid risk-management strategy for higher-risk research.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.0** for its accomplishments and progress.

- The PIs appear to be making good progress toward reaching their milestones, and it is commendable that they have a go/no-go decision established with a definitive hydrogenase activity goal. The *Rubrivivax gelatinosus* Casa Bonita Strain (CBS) is a difficult system and it appears they have made good progress. The identification of homologous hydrogenases from the global ocean sampling project is quite interesting, although it appears that there is much work ahead to see whether it will be a viable option. Although this reviewer is aware of the difficulty of the potential project, this reviewer does wonder how many truly novel hydrogenases are being overlooked because they are not homologous to known hydrogenases.

- The presenter claims 80% completeness, but some of the final goals to be achieved will be the hardest, for example, bringing H_2 yields to the target number. There is no guarantee of a linear extension of the current data. Some of the “accomplished” goals—cloning and expression of genes—are not noteworthy.
- The project has made significant progress toward the objectives and overcoming the barriers.
- Overall, the progress toward the stated goals has been good and steady; however, there is some question whether alternative approaches are being adequately considered that could improve progress. CBS is a challenging organism in that tools are lacking compared to model systems, but the availability of the sequence should enhance progress. Indeed, analysis of the genome revealed a new set of hyp genes. While appreciating the value of using homology to potentially identify novel environmental hydrogenases, this reviewer feels that the researchers may be missing some truly novel hydrogenases that do not necessarily look like a conventional hydrogenase at the sequence level, but nevertheless function as an O_2-tolerant hydrogenase. Identification of such an enzyme could be extremely challenging, but if it could be found, it could provide some very important insights into O_2 tolerance and enzyme structure and function.
- Progress is made evident in the development and displayed activity of both of the O_2-tolerant hydrogenase choices, but there is insufficient evidence for this reviewer to judge the likelihood of useful success because of the lack of quantified barriers.
- The project has well-defined milestones and decision points against which to compare progress. The project appeared to complete this period’s milestones on schedule. Task 2 had only one milestone during this period—and a 20-month interval between that and the next milestone—so progress against milestones was difficult to judge.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.2** for its collaboration and coordination.

- The overall milestones and presentations indicate that the two institutions are working in parallel, but that information is being shared between the two groups. The two primary institutions are also collaborating with other universities and companies for needs outside of their expertise.
- The presenter detailed collaborations that are spread out over many institutions.
- There is significant and fruitful collaboration between the project researchers and also with other institutions.
- The research appears to be well coordinated between JCVI and NREL, with good communication and sharing of results. The integration between the two institutions could be better illustrated, as they seem to be working more in parallel and synergism between the two groups is not obvious. The additional collaborations appear to be appropriate.
- It appears to this reviewer that the two institutions are preceding pretty much independently along two separate paths toward a common objective. Whereas there is some level of supportive participation by others, there is no strong evidence of collaboration between the two primary institutions. There is evidence of coordination and communication, but the accelerated progress that could be afforded by true collaboration and by focused work on a single pathway is not evident.
- In addition to the project partners, NREL and JCVI, collaborations exist with Vanderbilt University and Michigan State University, as well as with free genome sequencing from Pacific Biosciences.

Question 5: Proposed future work

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

- It appears that there is a definitive, logical plan to build on previous progress. There is still much work to be done on engineering cyanobacteria and increasing the H_2 amounts to levels that would be suggestive of viable production strains.
- The project has many significant challenges ahead, and therefore it has clear goals. It is no small feat to increase the protein expression 100-fold, achieve the level of H_2 production targeted, and maintain the viability of the culture. Generating gain-of-function mutants that have improved electron transfer is a difficult genetic task and has a strong possibility of failure. If the foreign hydrogenases are not linked to the natural systems of the model organisms, linking the two may require another round of mutagenesis or more engineering of foreign genes.
- The future work is well laid out and reasonable.

- The proposed research is logical and clearly builds on previous studies. The PIs have set some quite challenging research goals and appear to be cognizant of the challenges of the systems under study.
- The proposed future work is much along the same lines as the current work and is subject to concerns.
- Continued project funding is determined annually. If funded, Task 2 will pursue existing milestones and Task 1 will extend the existing work.

Project strengths:

- The researchers have developed a well-planned path with milestones. It is nice that they have an applied goal of increasing hydrogenase activity from the environmental hydrogenase in cyanobacteria by fivefold for the continuation of the project.
- The researcher has a large library of genes to work with to find other genes that may be required.
- Development of a stable, O_2-tolerant hydrogenase is an important goal. Overall, the project has a logical approach and complementary expertise between the research groups.
- Strengths of this project include its technical proficiency, equipment, and facilities.

Project weaknesses:

- While there has been progress on the goals set forth by the investigators, the translation of this work to a scaled-up system seems to still be very far off. It appears there is much ground to be covered with both the CBS and environmental hydrogenases expression and the overall effect on the physiology of the cell due to the expression of these genes. The investigators mentioned that a reviewer hit upon this last year, and that they are addressing the issues. It would have been nice to hear more about what avenues they are taking in that regard.
- It seems that some of the biggest challenges of the project are ahead of the researchers. Obtaining the activity goals is not guaranteed. If the foreign and native systems are not linked, that will be a tremendous undertaking, and the undertaking of a "forward screen" mutagenesis project to improve electron transfer is a potential non-starter.
- Alternative approaches and strategies to fine-tune expression could have been better addressed. While the researchers clearly appreciate the complexities of the pathways, it was not evident from the presentation whether the experimental approaches were effectively addressing these possible complications. Physiological effects were not addressed in a comprehensive way.
- The collaboration within the project and with other institutions, organizations, and the community of experts was noted as a weak point. The project also failed to provide future planning and work descriptions with sufficient information to provide program management with metrics to judge progress. The project strategy to pursue a two-track path is questionable because that hinders progress without any clear basis for such an approach.

Recommendations for additions/deletions to project scope:

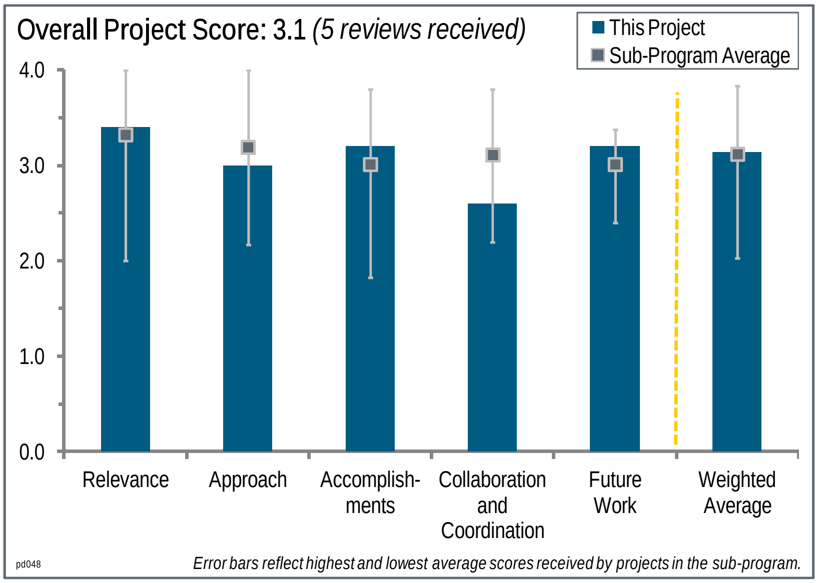
- The current position of the project is such that there is no guarantee that trimming and refocus will improve the chances of success. The authors have many irons in the fire, but many of them strike this reviewer as being high risk. Success in any of the stated goals will be significant, but focusing on one over the other may be too much of a gamble.
- Absent a compelling basis for independent development of distinct O_2-tolerant hydrogenases, the project should be encouraged to select one path and establish effective collaboration, and thereby accelerate progress to test and demonstrate a single O_2-tolerant material.

Project # PD-048: Electrochemical Hydrogen Compressor

Ludwig Lipp; FuelCell Energy, Inc.

Brief Summary of Project:

The overall objectives of this project are to: (1) design and develop an electrochemical hydrogen (H_2) compression system that is more reliable than mechanical compressors; eliminate contamination from lubricants, thereby increasing H_2 quality; (3) improve compression efficiency (goal 95%); and (4) reduce the cost of gaseous H_2 delivery to meet the U.S. Department of Energy (DOE) long-term targets.



Question 1: Relevance to overall DOE objectives

This project was rated **3.4** for its relevance to DOE objectives.

- The goal of a highly reliable and low-cost compressor is highly relevant to the DOE Hydrogen and Fuel Cells Program (the Program) objectives.
- From this reviewer's point of view, this is only a long-term solution.
- The technical concept is brilliant, but this project needs to be funded under a Basic Energy Sciences budget that recognizes a 20-year development horizon. For the Program, the project has little relevance.
- It is great that DOE is funding turbo pumping as well as the electrochemical H_2 compressor (EHC). It is too soon to tell which will win or, more likely, where the boundaries are that favor one approach over the other. The authors presented very credible work with good prospects for scaling up. The possibility of EHC incorporating the rapid technological advancements in fuel cells adds impetus to the project.
- Hydrogen compression is definitely one of the critical and relevant aspects on H_2 refueling. Electrochemical compression is a very promising technology, and this project in particular is addressing several key areas, such as reliability, efficiency, and cost, in order to make this technology a very competitive one.

Question 2: Approach to performing the work

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

- The technology is on the right path and development work will be slow, but the progress to date is meaningful and the potential is huge for many industries.
- The approach seems focused on the right issues and appears to be disciplined. The researchers are making good progress with small cells. Scale-up to somewhat larger systems could be as simple as adding units, at least for the near term.
- There are a lot of possible knobs to turn; the researchers should identify the most critical ones.
- The approach presented in this project was well defined, but the key parameters should have been highlighted and presented in more detail, along with discussion of how these parameters will impact the performance, durability, and cost of the system.
- The general approach appears to be correct, but the specifics of the research are not provided. As an example, the list on slide 5 is nice, but details are not provided regarding progress to these goals. This would be equivalent to indicating that the onboard fuel cell stack needs to lower costs without explaining the specific technology used to achieve that goal. In particular, the progress made by EHCs 1–7, as referenced in slide 11, should be further explained.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.2** for its accomplishments and progress.

- The accomplishments in terms of pressure, current density, and design are magnificent, but the overall rate of development is slow.
- The project showed good progress with regard to durability and energy consumption, and it demonstrated the feasibility of 12,000 psi. The cost target is far off.
- The future is very bright. The theoretical specific energy consumption is a big improvement over mechanical compressors; however, other losses reappear, so the two technologies seem fairly similar in performance. This reviewer worries a little about whether the three-stage mechanical compression numbers include all of the tricks, such as intercooling, etc. However, the simplicity and the lack of moving parts are terrific.
- Several technical accomplishments were presented for this project, including the demonstration of single-stage pressure capability above 12,000 psi. It would have been very beneficial to present more details on the improvements made that had the greatest impact on this accomplishment.
- Achieving 12,000 psi compression in a single stage was a very good accomplishment. The pressure needs to be greater for the current assumption for 70 MPa onboard tanks.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.6** for its collaboration and coordination.

- It may be due to intellectual property issues, but the collaboration seems limited.
- Only one collaborating partner was presented. It would be very beneficial to partner with an electrolyzer company with experience in high-pressure electrolyzers.
- Given the potential for this technology, it seems that the project team is not doing enough to develop collaborations with organizations that understand the technology's potential and want to help fund commercialization.
- This reviewer rates this as "good" because of the potential to utilize advances in fuel cells. This reviewer would really like to see collaboration or a test at Linde or another specialty laboratory, or at a refinery (they use a lot of high-pressure H_2).
- The collaboration with Sustainable Innovations appears to be effective for the cost analysis. It may be worthwhile to seek other collaboration with electrolyzer suppliers to understand the full extent of commercialization costs.

Question 5: Proposed future work

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

- This reviewer strongly recommends that the future work plan includes collaboration with well-funded, interested parties that can help accelerate the development and commercial awareness of the technology.
- The future is bright. This reviewer is very curious about other applications the researchers may be interested in besides pipelines. This reviewer would encourage funding opportunity announcements that would promote small-scale applications of high-pressure electrolysis systems (which are related to EHC).
- The team should perform cost analysis based on a "perfect world" assumption and include life-cycle cost compared to other technologies.
- The proposed future work seems reasonably good, especially in the areas of scaling-up and stack designs for higher pressures. Durability and cost analyses at higher pressures should be considered in future plans.
- The future work appears to be focused on durability and cost reduction, which are the key barriers for this technology.

Project strengths:

- This is a truly passive compressor.
- The small-scale modular approach is very versatile and can provide inexpensive analysis and development.

- This project features a simple operating principle with no moving parts.
- This is a very strong and promising technology that could have a significant impact on the H_2 refueling stations rollout and addresses the challenges associated with conventional compression technologies in terms of reliability and efficiency.
- The project has made significant progress in advancing toward higher compression ratios.

Project weaknesses:

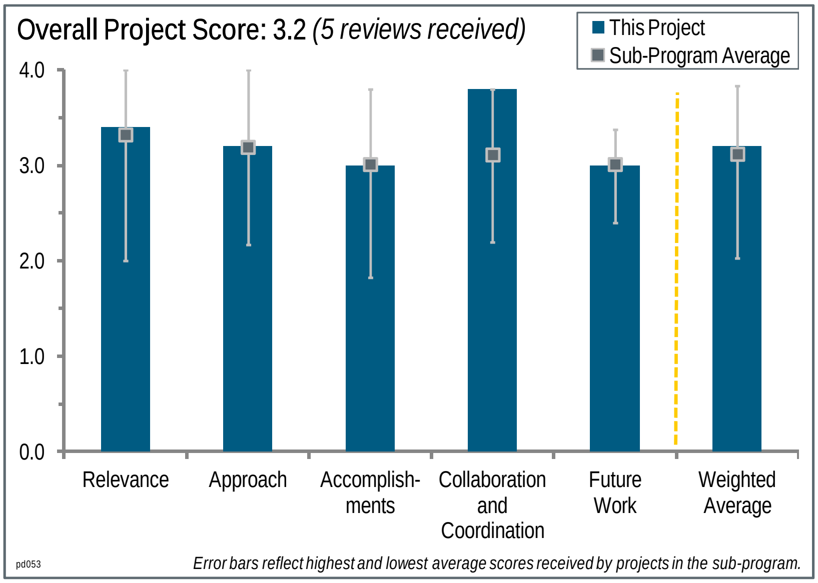
- There is a lack of development funds and capable technologists to accelerate development and commercialization.
- Sometime soon, the issue of the purity of the feedstock will have to be addressed. More realistic long-life tests are also needed, for example switching between on and off, temperature excursions, and impurities. It is a very large multidimensional space of horrors.
- The hard-to-reach cost target is an area of weakness.
- The project has limited partners.
- The project does not have a clear path to achieving the DOE cost targets. The project overview could be improved by providing specific details regarding the past and future design changes. Also, it would be useful to provide additional test data for the units being tested.

Recommendations for additions/deletions to project scope:

- One reviewer recommends keeping this project alive, but finding a more productive development path.
- Another reviewer would encourage more effort in small-scale applications. The modular approach allows for easy scale-up (even if not perfectly elegant or ideal); therefore, if a little one is working, it will be easy to advance to larger applications. Small units should be attractive to a wider market, so economies of scale (manufacturing a lot of small units versus a few big ones) could get the ball rolling faster. The team should think laptop and cell-phone-scale units, not big centralized utilities.
- The team should compare the technology to other compression technologies in PD-014. The researchers should also perform cost analysis based on a “perfect world” assumption and include life-cycle cost compared to other technologies.
- The team should add additional details regarding the cost analysis, including the possible best case projections. It should also add the evaluation of scale-up to higher per-day capacity to assess potential issues.

Project # PD-053: Photoelectrochemical Hydrogen Production

Arun Madan; MVSystems/Hawaii Natural Energy Institute



Brief Summary of Project:

The objective of this project is to develop a monolithic hybrid photoelectrochemical (PEC) device powered by MVS' low-cost amorphous silicon-based tandem solar cell. The hybrid devices will test three materials: (1) amorphous silicon carbide (a-SiC), (2) metal oxides, and (3) copper (Cu)-chalcopyrite-based materials. The goal is for the device to have a solar-to-hydrogen (H_2) efficiency of 5% and a durability of 500 hours.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- The project is clearly aligned with DOE Hydrogen and Fuel Cells Program goals.
- Photoelectrolysis H_2 production does meet DOE Hydrogen Production Roadmap goals.
- This project matches up well with the DOE objectives for PEC H_2 production. The continued development of low-bandgap materials with a focus on improving durability is relevant to achieving the 2013 technical targets.
- Researchers are addressing several of the major photoelectrode contenders in the PEC H_2 field.

Question 2: Approach to performing the work

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

- The group has a focus on developing a variety of materials for PEC H_2 production. This effort has a materials focus, which is important because there is currently no particular materials class that has been shown to have both efficiency and durability. Each material also uses a hybrid device design, where an underlying solar cell can used transmitted photons to provide the voltage assist to overcome overpotential issues for the PEC material.
- This effort focuses on three different material classes using the same a-Si tandem solar cell engine. The structure of evaluating the photo anodes and photo cathodes together, and all three material classes in the same laboratory, is excellent.
- Several different pathways are under evaluation—likely too many for this size project. It is unclear whether there is synergy between the different elements of the project.
- The researchers are well attuned to the barriers for each of the three technologies presented and have adopted different strategies to deal with each one accordingly.
- Even though it is understandable that this project is still in the materials discovery and development phase, it is not feasible; there are major technical challenges with this technology.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.0** for its accomplishments and progress.

- It has been tough going on some of the experiments, but overall the researchers have done a good job achieving the stated milestones.

- There are significant similarities among the 2011 and 2012 project objectives and results (slide 4 in the 2011 and 2012 Annual Merit Review presentations). The project milestones on slide 5 show that no significant progress has been made since the 2011 project report. The catalytic activity of a-SiC PEC hybrid devices shows improvements with Ti-CH_3.
- In the past year there has been a continuation of effort with respect to advancing the three materials classes under exploration. There was both materials exploration and studies on materials' durability. For a-SiC, modest improvements have been realized using functionally catalyzed surfaces to reduce overpotentials. Exploratory research on the $CuWO_3$ shows that it has an ideal bandgap but poor carrier transport. Preliminary results using carbon nanotubes indicates that providing a high-conductivity scaffold can ameliorate some of the transport problems.
- The team has made very good progress improving the a-Si system using surface treatment. The metal oxide durability issues are still unresolved. The team identified novel device integration structures for the Cu-chalcopyrite system and conducted durability tests for all three material systems.
- Clearly a lot of data is generated. It remains unclear how close the team is to having a viable device or technology; one does not expect the technology to be ready, but one does want to understand the gap.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.8** for its collaboration and coordination.

- The project features well-established and functioning partnerships.
- The project appears to have excellent collaboration among team members. The overall PEC Working Group interaction is a model of collaboration.
- This project does a good job of leveraging the extended expertise in the PEC Working Group. These collaborations enable a better understanding of the underlying material properties and help provide direction for future research.
- The investigators seemed to have legitimate interaction with nearly all of the PEC Working Group participants.

Question 5: Proposed future work

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

- The team has identified well-constructed and specific plans for each material system.
- The proposed future work seems to suggest a more focused approach in the future.
- More detail on how to deal with $CuWO_4$ conductivity issues would have been good.
- The proposed future work takes place on three fronts: (1) a-SiC, there is a general plan to improve the performance via further experiments on lowering overpotentials; (2) $CuWO_3$, accepting that the transport properties are poor, it is proposed to combine this material with Stanford's high surface area electrode; and (3) CIGSe, it is proposed to continue efforts for co-planar integration, which this reviewer views as a photo-catalytic non-precious metal electrolyzer. Although this is a nice exercise, it is not scientifically very interesting if the work is not moving the materials in new directions.

Project strengths:

- The project offers some novel and outside-the-box thinking and research and development approaches.
- Strengths of this project include the collaboration among team members, organization of the approach, and logical pursuit of each material system.
- Strengths of this project include its partnerships and link to the Program goal.
- The team has made positive developments in three distinct PEC technologies.
- Strengths of the project are its capability to fabricate a wide variety of materials using different fabrication techniques, including the low-cost spray pyrolysis to plasma-enhanced chemical vapor deposition (PECVD)/sputtering in a cluster tool, and the fact that it provides a sophisticated suite of characterization to understand the fundamental properties of each material as it relates to PEC H_2 production. The investigators covered a lot of technological ground along three fundamentally distinct materials classes.

Project weaknesses:

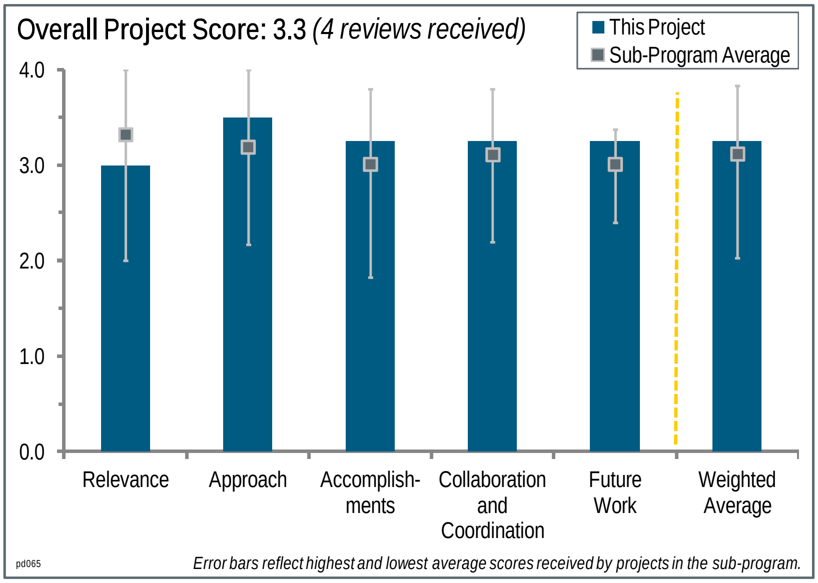
- There are too many material systems.
- No significant progress was made in this project since 2011. Even though it is understandable that this project is still in the materials discovery and development phase, there are major technical challenges with this technology.
- One reviewer would expect the proposed future activities to yield only incremental improvements, not the large jumps needed to meet the DOE benchmarks. Regarding the co-planar photovoltaic (PV)-CIGSe, the promoted benefit of the PEC system relative to a PV-electrolyzer is a reduction in the complexity for the top of the cell (e.g., transparent conducting oxide layer and silver grids). This benefit is lost with the co-planar approach.
- The project is almost exclusively focused on the pursuit of near-term DOE goals (of 5% efficiency and 500 hours) without much discussion of the pathway or retirements needed beyond these goals.
- Another reviewer still wonders about laterally deployed integrated PV systems, but the presenters had references to back them up, so until this reviewer reads the studies, this reviewer will not offer any critiques. This reviewer supposes that much of the project’s effort involves new materials development, which has inherent risk attached, but somebody has to do it.

Recommendations for additions/deletions to project scope:

- The team should down-select materials systems.
- The practicality of such technology needs to be evaluated by DOE. There are many technologies that provide significant technical advancements over the current technologies, but they end up not being very practical due to mass production and fabrication challenges, being integrated in an end-to-end system, and high cost.
- In addition to clear targeting of the near-term goals, the team should lay out a pathway to assess and pursue achievement of the longer-term goals (i.e., higher efficiency and longer endurance). There should be an assessment (both quantitative and relative to each material system) of the prospect for meeting the long-term goals.
- It needs to be clearly determined if a-SiC is indeed stable. There are confirmed changes after 500-hour durability tests that need to be fully characterized and understood. There has been an emphasis on giving PEC data as simply photocurrents or incident photon-to-current efficiency. With respect to the ultimate goal of using these materials in an eventual system, this is an extremely important metric. However, as these materials are significantly removed from viability in a system, this data has limited usefulness. As an example, a twofold improvement in photocurrent was presented for the $CuWO_3$ material from 2010 to 2011. At face value this looks impressive, but it is not clear that there was a true improvement (maybe the absorber layer was simply twice as thick). More telling would be data that shows the absorbed photon to current efficiency (i.e., internal quantum efficiency). This would more clearly show how the material is performing on a fundamental level (e.g., trap states and improved photoconductivity).

Project # PD-065: Unitized Design for Home Refueling Appliance for Hydrogen Generation to 5,000 psi

Timothy Norman; Giner Electrochemical Systems, LLC



Brief Summary of Project:

The project objectives are to: (1) detail, design, and demonstrate subsystems for a unitized electrolyzer system for residential refueling at 5,000 psi to meet DOE targets for a home refueling appliance; (2) design and fabricate a 5,000-psi electrolyzer stack; and (3) fabricate and demonstrate a 5,000-psi system. The project will develop and evaluate membranes; develop, fabricate, and evaluate electrolyzer cell and stack technologies; and improve the safety and reliability of systems for home refueling appliances.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The project addresses both the technical and economic objectives. It is very focused on the economic objectives.
- The project fully supports the DOE Hydrogen and Fuel Cells Program (the Program) and its goals align with DOE research, development, and demonstration objectives.
- Home refueling supports many of the Program's hydrogen production goals. However, due to its scale, it will have greater difficulty achieving the Program's technical targets (e.g., cost); thus, it inherently does not support the goals to the extent of larger-scale technologies, including distributed production.
- The project has merit, but it is not critical to the Program in this reviewer's opinion. While the idea that refueling can occur overnight using hydrogen (H_2) generated at home is a great concept, there are other aspects of the Program that have higher priority, such as reducing the cost of fuel cells and fuel cell vehicles. In addition, the technology for home refueling appliances does not appear overly difficult, and its development now is not timely given the shortage of research and development (R&D) dollars.

Question 2: Approach to performing the work

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

- The project features an excellent emphasis on safety and meeting codes and standards. It is very focused on meeting the economic objectives. The team has made good innovations that take a whole-systems approach. The investigators also leverage what they have learned in other projects.
- The project has adopted a higher-risk and higher-reward approach by developing a new design. The project has encountered some technical issues.
- The project offers an innovative design for addressing issues such as not requiring a compressor or storage, appropriate new robust materials for higher efficiency, and low cost.
- Giner Electrochemical Systems' approach is well designed and well executed. The team completed R&D in specific components to reach the higher pressures, and also integrated the system. Safety issues were addressed and a preliminary economic analysis of a commercial home refueling appliance system provided projected costs within DOE's targets for when there is a significant sales volume.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.3** for its accomplishments and progress.

- The team has made significant progress toward its objectives.
- The presentation included the definition of barriers and showed progress in overcoming the barriers.
- The project features a very good project plan that is being well executed. The project is cost driven, which is excellent, and it is making significant progress toward accomplishing its goals. The investigators have made some technical breakthroughs to reduce costs.
- The project is less than 60% complete with about 85% of the time expended. Critical integration and testing efforts remain.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.3** for its collaboration and coordination.

- The project has several industrial partners and a consulting partner.
- The team placed good emphasis on safety using an outside consultant. The project featured good interface with suppliers to drive costs down, and it did a good job of leveraging efforts of other projects.
- Partners include people or companies with expertise in H_2 safety codes and components (e.g., nanostructured thin film catalyst and membrane and carbon cell separators) as well as system controls design. The work is well coordinated and complements Giner Electrochemical Systems' expertise.
- Some collaboration exists and partners are fairly well coordinated. Collaboration with the National Institute of Standards and Technology (NIST) or national laboratories could offer more advantages.

Question 5: Proposed future work

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

- The project is well planned and executed. The investigators will probably need an extension. Two years was probably unrealistic to accomplish the whole project.
- This reviewer thinks that there should be a greater focus on safety because the general American public will use the system; there should be multiple redundancies in the safety system. Costs have to come down.
- The project's proposed future work is to complete the remaining tasks.
- The proposed future work should include collaboration with NIST or national laboratories to make the project more effective.

Project strengths:

- Strengths of the project include its solid approach, smooth execution, focus on cost reduction, and good technical innovation.
- The project offers an innovative design to address technical barriers and reduce the manufacturing cost.
- Comparison of presentations from Giner Electrochemical Systems and Proton Energy Systems shows that Giner Electrochemical Systems has focused its efforts and is getting the job done. This comparison is valid because both companies started work in August 2010 and have similar funding levels.

Project weaknesses:

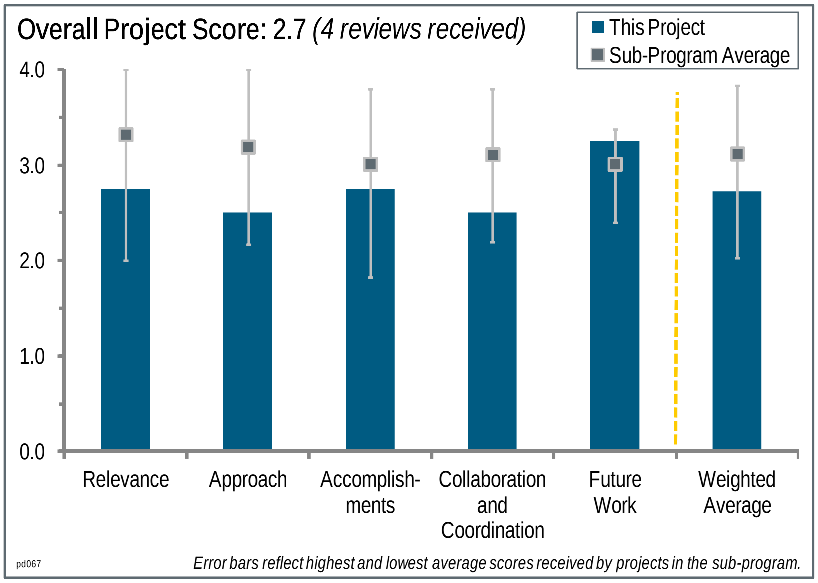
- The acceptance of pressurized H_2 systems in the garages of the American public and the associated costs has not been addressed.
- The project is lacking technically strong collaborators such as NIST or national laboratories, which could help in standardizing the process as well as providing technical input to the design.
- This project has no weaknesses.

Recommendations for additions/deletions to project scope:

- The team should keep going.
- It is a good project, and the project scope is sufficient.

Project # PD-067: Hydrogen by Wire – Home Fueling System

Luke Dalton; Proton OnSite



Brief Summary of Project:

The objective of this project is to develop key technologies that will enable home fueling. Specifically, the project focuses on the design and fabrication of a polymer electrolyte membrane (PEM) electrolysis cell stack and balance-of-plant components for 350-bar operation. The project's goals are to develop a system capable of 350-bar differential pressure electrolysis and to demonstrate prototype operation in terms of both hydrogen (H_2) generation and fueling capability.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.8** for its relevance to DOE objectives.

- The project aligns with DOE-stated objectives.
- Home refueling supports many of the DOE Hydrogen and Fuel Cells Program's (the Program's) H_2 production goals. However, due to the project's scale, it will be difficult for the project to achieve Program technical targets (e.g., cost); thus, the project does not support DOE goals to the extent of larger-scale H_2 production technologies, including distributed production.
- The project has merit, but it is not critical to the Program. While the concept that refueling overnight using H_2-generated in one's garage is great, there are other aspects of the Program that should have higher priority, such as reducing the cost of fuels cells and fuel cell electric vehicles (FCEVs). The technology for home refueling appliances does not appear to be overly difficult. At this time, using very limited research and development (R&D) dollars for development of this technology is not cost effective. Of concern are the acceptance of the American public and safety issues related to in-home deployment (e.g., kids, weather-related events, and collisions with cars).
- The work is presented by Proton OnSite and demonstrates a prototype home refueling system for H_2 wire delivery. The project presented is based on PEM stack technology at 350-bar differential pressure for home delivery to support FCEVs. There is no doubt that the project itself is well aligned to the DOE Fuel Cell Technologies Program's (FCT Program's) mission and goals by demonstrating an H_2 refueling system for the homeowner. In fact, a DOE Office of Energy Efficiency and Renewable Energy (EERE) FCT Program Funding Opportunity Announcement (FOA) was recently released on the topic of "Validation of Hydrogen Refueling Station Performance and Advanced Refueling" (FOA 626). This project is in the prototype and validation stage, but the team should consider applying to the FOA to collect and test real-world data for DOE. This week the team won a Small Business Innovation Research (SBIR) project (Phase I, release two) to support its membrane technology. The DOE Hydrogen Production and Delivery sub-program target is to reduce the "delivery" portion of this cost to $1–$2/kg by 2020. At this stage, the project does not present a cost analysis of the delivery system.

Question 2: Approach to performing the work

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

- This is a straightforward systems engineering approach to the technical solution. This reviewer does not see a strong focus on meeting cost objectives. It is not clear if the solution is going to be cost effective. The investigators have not addressed the H_2 codes and standards.
- Most of the effort in the last 1.5 years has been expended on design, and comparatively little effort has been spent on testing, identifying, and overcoming the barriers. There was little technical detail on component research (e.g., membranes, membrane electrode assemblies, and seals and separators suitable for 5,000–7,000 psi operation). This work may have been done, but, if so, it was not presented adequately on the poster. Preliminary economic analysis and safety issues were touched on but not addressed.
- The project approach controls risk by extending concepts applied and demonstrated in an existing 2,400 psi design that has more than 20,000 hours of operation.
- The approach to the work appears to be sound and reasonable; however, much information was given on the details of the PEM technology, or stacking details, to assess the relatively improved production performance. Still, the overall approach based on the information released appears to be sound and based on PEM technology as appropriate. Per product development, the system is designed, prototyped, and validated. No concrete details on R&D involved were given, and the project is at present more of a prototype unveiling and demonstration potential stage. Demonstration does not include performance details or production output, which would have been nice for this evaluator.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.8** for its accomplishments and progress.

- The accomplishments and progress appear to be relevant and productive. The team has demonstrated completion of the system design/fabrications component procurement and completed the H_2 phase separator. The stack design pressure tests have been completed and the prototype design has been finalized. The stack embodiment hardware is validated along with the cell flow fields and differential pressure testing. Integrated testing systems including power supplies and pump testing are completed. No detailed data was presented on the component output data, but the project presents tasks as complete.
- The project is making good progress toward producing a technical solution. Researchers are hitting their technical objectives. The project has not made any cost projections. The economic viability of the solution will be a challenge. It is too early to determine if the economic goals will be met. Slides 7 and 8 do not match up regarding the status of progress.
- A successful test of the 5,000 psi electrolyzer was completed after hours on May 15. However, the overall rate of progress has been slow. The barriers with respect to the performance of the various components were not addressed specifically. Considering that this project was started in August 2010 (the same time as Giner Electrochemical Systems' project), this reviewer believes that more details on these issues should have been presented. No preliminary cost analyses were completed and it is impossible to determine if progress was made toward meeting DOE's cost and efficiency targets.
- The project is more than 90% complete with less than 90% of the time elapsed. System design and fabrication, stack design, and the majority of component verification is complete, and integrated testing has begun. Testing, packaging optimization, and any economic evaluations that are performed will demonstrate the extent of progress against DOE goals.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.5** for its collaboration and coordination.

- The project features a good use of Oak Ridge National Laboratory (ORNL) capabilities for an important aspect of the design work. The team is working with parts suppliers to address costs.
- ORNL is listed as a collaborator for assessing the durability of metallic and coated separator materials. No details were provided either for ORNL or for the industrial suppliers.

- Limited collaboration exists; given the progress made, more collaboration appeared to be unnecessary.
- The team is working with ORNL to provide the characterization and analysis of the metallic separator and its durability. It is not clear to this reviewer why ORNL was selected for this task. It would be nice to understand this selection, and why the ORNL expertise was needed or why the required analysis tools could not be found elsewhere, or in house. No R&D was presented on the stacking design or PEM, but a university collaborator might in the future support a larger project effort. Proton OnSite is well integrated in the community and with its industry component suppliers and end-product users, including DOE.

Question 5: Proposed future work

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

- The future work is well planned.
- The only remaining project activities are integrated testing, package optimization, and fueling demonstrations.
- The team proposes the future work under the DOE award to include further integration of the operational testing, scale-up of cell count to increase total output, and optimization of the system packaging for siting requirements and cost effectiveness. This appears to be a logical next step for the future work, but no output data was given to assess the need for future optimization. The team also presents various future ideas for new prototypes and products outside the scope of the funding. The team presents a very nice future roadmap for its products that is very impressive and includes a 5,000 psi home fueling system and a nice loop into a DOE trade study on home fuelers. This will fit nicely with a response to the DOE FOA on validation and data collection.

Project strengths:

- This is a good technical solution. The team is making good progress on getting to a system for testing.
- The summary slide indicates that Proton OnSite has the experience with commercial products to complete the design and safety analysis.
- The strength of the project is its alignment with the FCT Program’s interests in validating H_2 refueling systems. This product will be a nice addition to the community to advance the use of FCEVs in the home, which is an urgent need for infrastructure. The product will also be well suited to be tested under the active DOE FOA 626 on validation and data collection in real-world environments of H_2 fueling stations. This project/product should participate. It is commendable that the team is continuing to leverage other DOE support avenues and recently won a prestigious SBIR Phase I, release 2 project to advance its membrane technology.

Project weaknesses:

- The team needs to provide cost projections and show how the system will meet the cost targets. The project needs more focus on safety issues.
- Presenters do not provide enough details for reviewers to evaluate the project’s strengths and weaknesses. This reviewer cannot believe that all of the technical details are proprietary.
- This is not necessarily a direct weakness, but it would have been nice to see more performance and output data. This reviewer realizes this may be proprietary at this point. It is not clear why ORNL was the selected team member and, without the more detailed explanation, it could appear a bit “thrown in,” to have a national laboratory in the presentation.

Recommendations for additions/deletions to project scope:

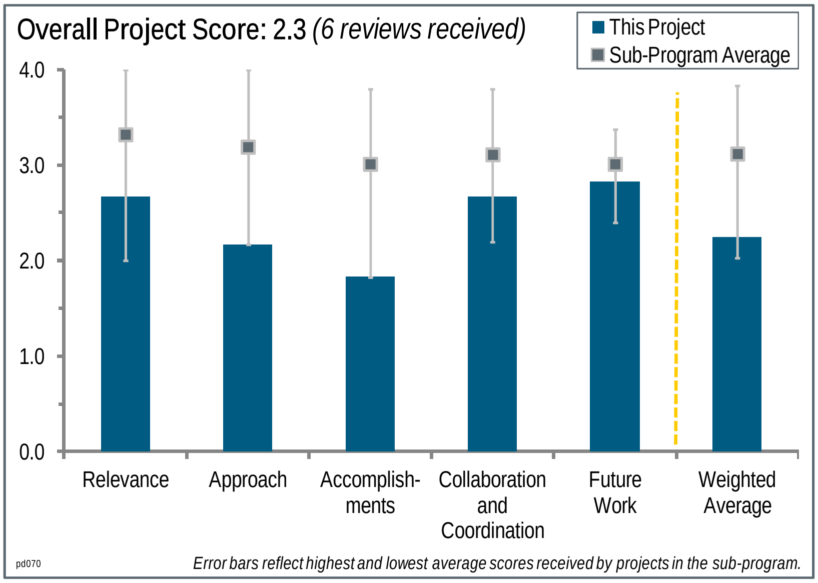
- The scope is fine. The team should work on cost reduction.
- This project should continue to be funded as much as possible. The future product plans are impressive, will contribute to the fuel cell community, and are well aligned with the DOE mission and FCT Program goals.

Project # PD-070: One Step Biomass Gas Reforming-Shift Separation Membrane Reactor

Mike Roberts; Gas Technology Institute

Brief Summary of Project:

The long-term objective of this project is to determine the technical and economic feasibility of using the gasification membrane reactor to produce hydrogen (H_2) from biomass. The short-term objective is to evaluate synthesized metallic and glass ceramic membranes to fabricate a module for testing with a bench-scale gasifier. The project scope includes membrane material development, gasification membrane reactor process development and economic analysis, bench-scale biomass gasifier modification, integrated testing of the initial membrane with the gasifier, and integrated testing of the best-candidate membrane with the gasifier.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **2.7** for its relevance to DOE objectives.

- The long-term project goal meets the H_2 production cost targets, but not the H_2 purity targets.
- H_2 production from renewable sources is important to the overall sustainability of H_2 fuel cells.
- Low-cost membranes for H_2 separation are a key component to achieving the $2/kg cost for H_2.
- The concept of using a gasifier membrane to produce H_2 is novel and potentially interesting. However, there were many questions about the actual operation, or planned operation, that were not addressed in the slides or by the presenter.
- The economic analysis indicates that the one-step biomass gas reforming-shift membrane reactor has no significant advantages in power requirements because the excess power generated is required for gas compression. Producing H_2 from biomass with power cogeneration therefore does not increase efficiency. It is not clear whether there is a cost advantage to this process. The cost analysis did not specify that the Hydrogen Analysis (H2A) model was used, as slide 19 implied that the detailed capital cost estimate is from the ASPEN Plus Model. The summary slide showed a comparable cost estimate for the conventional biomass reactor using pressure swing adsorption (PSA), a well-known technology. The H2A version three estimate was $2.00/kg for the PSA process while the H2A version three estimate for the membrane reactor was $1.82/kg. Whether this difference is significant depends on the performance of the membrane in terms of durability, flux, contaminant effects, etc., about which nothing has been reported. Therefore, it is not clear if the project will meet DOE research and development objectives.
- This reviewer wants to know why the long-term H_2 flux and purity goals are less than the 2015 DOE targets, and why durability is not a goal of this project. If the goal is to get to 270 SCFH/ft^2, it was unclear why the bar has been lowered for this milestone. It also appears lower than the previously achieved 2006 status. Unfortunately, the presenter did not provide satisfactory answers at the DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR).

Question 2: Approach to performing the work

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

- In 2011, the researchers tested the performance of the metal glass ceramic membrane and found disappointing flux and low H_2 permeability as compared to metallic membranes and advanced process simulations. The H_2 flux membranes module is in fabrication and should be complete by August 2012.
- The approach as provided was generic, and gave no indication of specific difficulties or areas where greater focus should have been applied. Moreover, the approach in terms of milestones went back as far as 2008, suggesting that this slide has been recycled. It was difficult to tell how much the approach might have been modified based on earlier years' work.
- One of the comments from the 2011 review was that greater focus was placed on the modeling effort compared to the experimental effort. The same comment applies in 2012. Work related to factors that will control overall performance such as durability and the effect of contaminants on the membrane surface has not been addressed. The project was started in February 2007, and the final candidate membrane was just selected. Total project funding was $3,396,186. This amount seems high for the economic analyses, membrane development work, design of the membrane reactor, and fabrication of the membrane module without other testing.
- The project is 75% complete to date and the progress is disappointing. Having the initial candidate selected in 2008 become the best candidate in 2012, with only marginal results demonstrated on alternates, does not reflect a reasonable approach based on the funding. The same is true with the design of the module. It was completed in 2010, but it will not be fabricated until August 2012. If there are assembly problems, it may be delayed. This appears to be a poor-performing project with unrealized expectations on how it will perform. Costs are still modeled, and the manufacturing costs are still of a rough order of magnitude and have not been validated to show they can get to the $1.82 H_2 cost per kilogram.
- The project title, "One Step Biomass Gas Reforming-Shift Separation Membrane Reactor," is unclear. It was unclear if this means the approach is to have reforming-separation-shift processes all in one step. If so, the approach may need serious revision. This reviewer is not sure how a separate stream to membrane and gas cleaning works and would like to know if the researchers are integrating the shift within the membrane. The presentation suggests that there is removal of the shift reactor with the membrane system (page 8). It was unclear if retentate is sent to gas cleaning, and what the mechanism is for the cyclone to "split," compared to the "conventional" approach, where only a single stream exits the cyclone. Again, the presenter did not provide satisfactory explanations.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **1.8** for its accomplishments and progress.

- It looks like mostly modeling work was performed over the past year, along with some membrane testing.
- The researchers have made good progress in cost analysis, but the fabrication and validation of hardware has been slow.
- It appears that the membrane modules were still in the process of being fabricated, and no apparent testing has been carried out. Actual membrane results, primarily carried out at the National Energy Technology Laboratory (NETL), appear to be the same as last year, or possibly earlier. Most of the accomplishments and progress seemed to be based on HYSYS and ASPEN modeling. The presenter was insufficiently knowledgeable to answer specific questions about the accomplishments.
- There does not appear to be an efficiency advantage in terms of power consumption. The summary slide shows a cost comparison for the conventional biomass reactor and the one-step membrane reactor. Three refinements were presented. The third set of values was $2.00/kg and $1.82/kg, respectively. The PSA technology is well known, while the performance of the membrane reactor is still under development. It is possible the costs of the former will be relatively stable, while the costs for the latter will increase. The only experimental work reported is the selection of the $Pd_{80}Cu_{20}$ as the final membrane, and it appears that the work was done at NETL, in a clean system presumably. No details are given in the NETL slide. The unit for performance criteria is $STCH/ft^2$, not $mol/m/s/Pa^{0.5}$ (for permeability), as in the NETL slide. Data on the flux rate in the presentation are very confusing. For example, the flux rate for 2006 status (slide 4) was >200 $STCH/ft^2$. The milestone reported

completed on 6/15/2011 was for developing the membrane with a flux of 125 SCFH/ft^2. Finally, the summary slide reports the module is capable of a flux rate of 80+ SCFH/ft^2. Significant improvements will have to be made in the module. It is not clear why performance decreased from 2006 to 2011. Some of the inconsistencies may have been better addressed if the scheduled presenter had been able to attend.
- Laboratory results are very limited with long delays in the fabrication of the module. Based on the funding level, it would have been anticipated that at least 5–6 modules would have been built and tested to date to verify the performance of the assembly. Based on other program gasification projects, this may have significant performance issues that might not be addressed because most of the funds have been expended.
- It was not clear why the presenter showed permeability and not flux or H_2 selectivity, as these are measured goals, and what has been accomplished in this cycle. Even the permeability data (apparently, the only experimental data shown) was presented at the last AMR (page 11). Since this reviewer is not sure of the researchers' proposed process (shift or gasification integrated with membrane), this reviewer is also unclear on temperatures (page 12). Gasification temperatures may be as low as 750°C, but 900°C and higher are better. High temperature shift is about 400°C. Membranes at 800°C do not appear to be a good temperature for integration. Unfortunately the presenter was not able to clarify this issue. There is a fairly broad range of temperatures (700°C –980°C) for the gasifier and reformer. The syngas has a small amount of tars (heavy py-oil, essentially). The reformer is there to convert to more syngas. The purpose of the metal-glass ceramic membrane effort with Schott is unclear, because the palladium-copper (Pd-Cu) membrane had already been selected. An inconsistent dollar reference was given—both 2005$ and 2007$.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.7** for its collaboration and coordination.

- This project features a nice mix of industry, academia, and national laboratories.
- NETL provided work on the membrane selection.
- It is not clear why the major (metallic) membrane players are not involved.
- The collaboration with NETL, Schott, and ATI Wah Chang seems to be adequate, in principle. The collaboration with NETL does not seem to be recent, however, and ATI Wah Chang was not cited in terms of the membrane module fabrication or assembly. Therefore, the true extent of collaboration on this project is not clear.
- Based on the presentation, the collaboration was average. Plans as to how the team will address the critical parameters and the construction of the module are lacking. The modeling approach seems reasonable, but this should have been a very small component of the project.

Question 5: Proposed future work

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

- One reviewer is looking forward to the results from the fully integrated unit.
- The summary of the proposed future work is logical, but the list is long. It seems it will be quite challenging to achieve the final milestone by June 2013, given what appears to be somewhat slow progress to date.
- The future work includes completion of fabricated parts, testing, and H2A analysis. All of these are critical to correctly assess the value of this project.
- Integrated testing with the gasifier will be very helpful to validate the performance and cost estimates.
- There is a go/no-go decision in June, but this has not been incorporated into any of the future plans. It was unclear what will happen if the modules cannot be sealed or the membranes get damaged or do not perform as modeled. Also, the difference between $1.82 and $2.00 per kg for the cost of H_2 is within the statistical accuracy of the estimates, and is probably not relevant at this point of the project.
- It was unclear where the researchers stand on H_2 purity, selectivity, durability (membrane life), and flux (SCFH/ft^2), and what their plan is to reach the goals. The future plan is vague. There appears to be no milestones for 2012.

Project strengths:

- The work is lead by Gas Technology Institute (GTI), a leader in gasification technology.

- The potential novelty of the approach is a strength of this project.
- GTI has experience with gasifiers.

Project weaknesses:

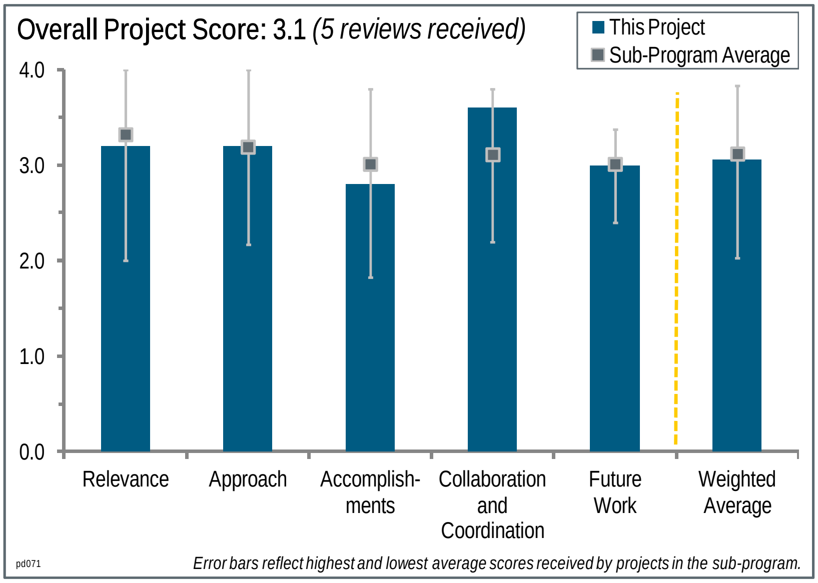
- There were long delays in testing and developing components for modules. There was also a long delay in selecting membranes for continued evaluation. The go/no-go was too far into the project to make any difference.
- The five stages are unclear. This reviewer wants to know how the syngas moves from the membrane (800°C) into the water-gas-shift (WGS) (400°C) over the five-stage process. This reviewer also wants to know if there are heat exchangers in each stage between the membrane and WGS. It is unclear as to which membrane composition was selected.
- One weakness is the lack of a clearly identified path to success. The proposed steps are fairly generic. Methods to be employed, for example to avoid fouling of the membrane with tars, were not clear.
- There is very little preliminary experimental work on the reactor itself and on testing the membrane with a simulated stream of gases from the gasifier. The figures in the slides were not as good as they could have been. For example, slide 28, which shows more details of the membrane reactor, should have been included in the body of the presentation so that the audience could understand the concept.

Recommendations for additions/deletions to project scope:

- Although the 80 Pd-Cu showed the best results, the accomplishments for 2011 highlight the performance with the Ag/Pd metal-glass ceramic membrane. This is confusing.
- Progress as reported is quite slow for the amount of time and effort expended. If the H2A analyses of the membrane reactor do not show a significant advantage to the membrane and conventional reactor, this reviewer would recommend that the project be terminated unless there are data that have not been reported due to time constraints.
- It is unclear if this project was to select membrane materials, develop a module design, or actually test a module in realistic conditions. The results shown appear to demonstrate that the researchers have spent more effort on developing analytical models and running economic analysis than on completing experiments and incorporating the results in hardware designs.
- One reviewer is not confident the project will achieve its own lower goals, let alone the DOE goals. This reviewer recommends that the project be stopped or have the milestones seriously revised with a meaningful and accountable plan in the upcoming go/no-go meeting. The presentation should have been given by someone familiar with the project.
- Another reviewer cannot identify any recommendations. All of the proposed steps seem to be important, although whether they can be accomplished within the next 12 months is questionable.

Project # PD-071: High Performance, Low Cost Hydrogen Generation from Renewable Energy

Katherine Ayers; Proton OnSite



Brief Summary of Project:

The project objectives are to improve electrolyzer cell stack manufacturability through consolidating components and incorporating alternative materials, and to reduce the cost of electrode fabrication by reducing precious metal content and employing alternative catalyst application methods. The project addresses high-impact areas of flow-field cost and labor reduction.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- This project features excellent alignment with DOE's Hydrogen and Fuel Cells Program (the Program) goals and objectives.
- The development of a better electrolyzer system that meets DOE goals is within the objectives.
- Water electrolysis is a near-term technology for the production of hydrogen (H_2). The project (polymer electrolyte membrane [PEM] electrolysis) is well aligned with the needs of the Program and fully supports the objectives to increase efficiency and reduce cost. This project is mainly focused on cost reduction; another project is mostly focused on efficiency.
- The project is clearly aligned and relevant to H_2 goals. The team clearly showed how features under development contribute to cost, and clearly identified the value proposition of the work. This reviewer did not rate the project as "outstanding" only because this particular project impacts only a part of the key features of the cell.
- Slide 2 indicates that system efficiency is being addressed, but slide 3 states that the project addresses high-impact areas of flow-field cost and labor reduction. The presenter stated that efficiency is not being addressed. Slide 4 states that this project supports Proton OnSite's overall roadmap for cost-effective renewable H_2 production. Slide 5 is basically a marketing slide, and this reviewer thinks that it is irrelevant. Slide 6's last bullet once again promotes Proton OnSite's objectives. Overall, Proton OnSite should present how it is supporting DOE objectives and not its own.

Question 2: Approach to performing the work

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

- The approaches seem very well designed to address the issues for this project.
- This project features a very well-laid-out and structured approach.
- The approach of the project is well designed. The project exploits the results of the past work to make progress on the cost reduction and the scale-up through the consolidation of components, the incorporation of alternative materials, and the use of advanced application techniques.
- Given the limited scope, this approach is satisfactory. Task 1 looked at catalyst loading, which appeared to be outside of the scope described in slide 3. This is sending a mixed message
- The approach seemed to involve lots of combinations of efforts, which may not provide a quantum enhancement in the design.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.8** for its accomplishments and progress.

- Very good progress has been made on cost reduction and efficiency improvement.
- The project has made good progress in validating stack cost reduction with a new design, has a prototype on test, and has a clear path for continued progress.
- It is unclear why noble metal loading is being reported if it is not in the scope of the project. The investigators have made good progress in reducing flow-field cost, having demonstrated the feasibility of a 40% reduction.
- This reviewer is not sure about the accomplishments because the presentation was more of a marketing effort and lacked scientific/technological descriptions.
- The team has made great progress on alternative designs, but the rate of progress on the coatings work is a little disappointing (i.e., good but not great). It is not clear how critical the nitride work is to achieving the goals, so it is hard to rate this progress higher.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.6** for its collaboration and coordination.

- The collaborations are well coordinated.
- The project features a good mix of industry, academia, and national laboratories.
- There is good collaboration with academia and industry.
- This project takes an aggressive approach to leveraging partner capabilities.
- The project features really appropriate collaboration, taking the form of a partnership with Oak Ridge National Laboratory and perhaps some of the early Entegris work, but perhaps some of the other collaborations were more of vendor-type relationships.

Question 5: Proposed future work

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

- The project features very well-laid-out future plans.
- The project has a clear plan for the end goal of the Program and is focused on scale-up, manufacturing, and test qualification.
- The emphasis on scale-up to a 50 kg/day stack is a good approach for future work.
- From the presentation, it is not clear what kind of solid plan the company has for this project.
- The future work appears to involve the important efforts of finishing up the current tasks, but little here is aimed at overcoming the next barrier or making the next cost or efficiency breakthrough.

Project strengths:

- The approach of the project is good, and it has good partners to address the critical issues.
- The team made good progress on reducing plate costs. It is scaling-up to a larger stack.
- The team appears to be strong.
- Strengths of the project include its structured approach, effective execution, and aggressive leveraging of partner capabilities.

Project weaknesses:

- As the project enters its last year, the testing conditions should be more detailed to evaluate if they will allow a full qualification of the performances of the prototype. This is critical to drawing clear conclusions on the real performances of the prototype at the end of the project.
- For the amount invested by DOE in this project ($3.4 million), it seems that more could have been accomplished. The project is narrowly focused on plate design. The presentation was confusing—efficiency was not part of the

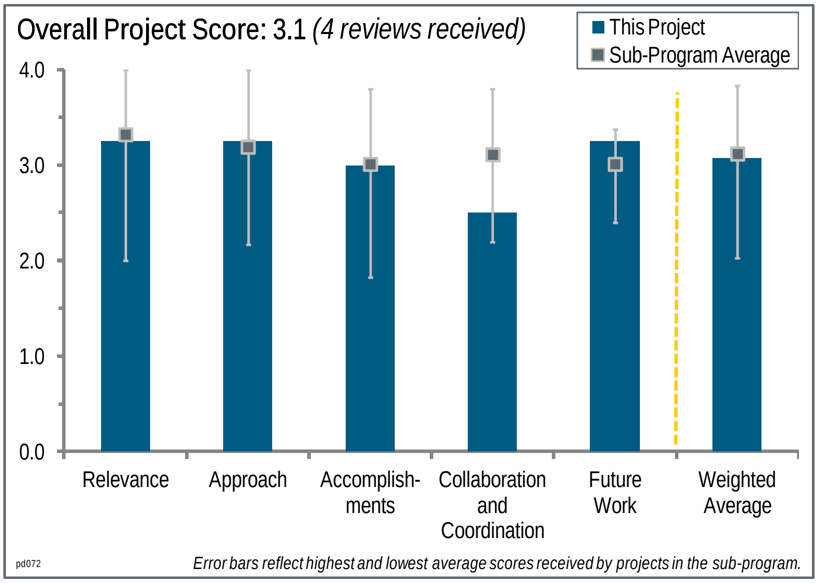
project, but it was reported on. There was too much selling of Proton OnSite and not enough technical discussion. The presenters should leave the sales pitch to the sales people.
- The presentation was highly lacking in the technological innovations. This reviewer was simply asked to believe the cost saving. In the future, the presenter should describe the relevant technological innovation that led to the cost saving.
- The project could probably use some more modeling to predict the performance or stability of the materials.

Recommendations for additions/deletions to project scope:

- Long-term testing is needed to validate the prototype.
- The team should consider where modeling might be useful.
- The project should have a rationale design for cost savings based on sound science.
- As stack capital drops, the cost of H_2 from these systems becomes more and more dominated by electricity cost. Some out-of-the-box thinking is needed regarding how to break out of those constraints with these electrochemical systems.
- This reviewer could not identify any recommendations.

Project # PD-072: Development of Hydrogen Selective Membranes/Modules as Reactors/Separators for Distributed Hydrogen Production

Paul Liu; Media and Process Technology Inc.



Brief Summary of Project:

The general objectives of this project are to: (1) develop, fabricate, and demonstrate field implementable hydrogen- (H_2) selective membranes and modules; (2) intensify and improve conventional the H_2 production process via a membrane reactor; and (3) prepare field-test modules and conduct a field test for H_2 production and purification. The specific objectives for the 2012 fiscal year are to: (1) develop improved palladium membranes with cooling stability in the presence of H_2, and (2) design and fabricate a catalytic membrane reactor for field testing.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The combination of a low-cost, H_2-selective membrane coupled with an integrated water-gas-shift (WGS) catalyst appears to be an attractive alternative to conventional upgrading of reformate for H_2 production.
- Improved H_2 purification from reformate streams is important to improving overall system efficiency.
- The project supports the cost performance targets for H_2 well, and it has the potential to improve the system performance of H_2 reforming.
- Process intensification is often employed to reduce overall production costs. This reactor/separator technology targets process intensification for syngas-based approaches (e.g., steam reforming, authothermal reforming, and partial oxidation) for producing H_2. To date, only methane-based technologies have been shown to be capable of meeting DOE targets related to the cost of H_2. Renewable bio-based technologies being pursued under the Fuel Cell Technologies Program (FCT Program) are having a difficult time meeting the DOE cost targets, and this technology could potentially benefit these projects.

Question 2: Approach to performing the work

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

- The presenter described a logical progression to the development of a membrane on a ceramic support that is thin, relatively low in cost, and able to withstand the heating and cooling cycles. The progression from identifying problems to identifying solutions seemed to be very good.
- The overall approach is very strong and is well designed to keep the project on track toward meeting its targets. This reviewer's only concern is how the cooling stability studies are being conducted. It is not clear that cooling in H_2 alone is sufficient to determine membrane stability, given that water will be present and may condense out depending on whether the lowest temperature experienced is below the dew point.
- The use of bundles and cooling subsystems is a rational approach to this performance problem. Improving thermal performance coupled with lower manufacturing costs is a good approach. The 600 hours of stable thermal performance was a significant accomplishment.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.0** for its accomplishments and progress.

- The team built and tested a prototype membrane bundle reactor.
- It seems that there is still significant work to be done in integrating the membrane with a WGS catalyst; moreover, dealing with the possible deactivation of the WGS catalyst in heating/cooling cycles does not seem to have been considered. However, progress appears to be quite good during this recent one-year period.
- Accomplishments to date are impressive in terms of improved designs, performance, and durability, although there still seem to be unresolved issues with meeting durability targets during cooling cycles. It is not clear if there is a path forward for solving the stability issues during cooling.
- The technical progress is good. The researchers have tried a number of membranes and approaches to use a ceramic substrate to control cost and porosity, and the laboratory performance demonstrated by their membranes showed improvement over published data. They have developed a sealing mechanism that should work in their bundled configuration. This reviewer still believes the cost data has not been validated and is still an estimate that has a statistical variance of 25%.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.5** for its collaboration and coordination.

- Although the list of collaborators is good, it was not obvious how much any of these collaborators contributed to the recent work on this project. There was no specific discussion regarding their contributions.
- Collaboration was discussed, but it was not clear how the partners interface to discuss and resolve critical issues and design concerns. It appears that most of the organizations operate independently.
- The project features strong industrial partnerships such as Chevron, which is identified as a potential end user of the technology, Ballard, which is identified as the partner that will integrate this technology into its fuel processing technology, and Johnson-Matthey Fuel Cells Inc., which will provide the shift catalysts. It would be beneficial to better understand the nature of the collaboration with Ballard and the extent of interaction, because this is the key interaction. In particular, this reviewer would like to know if Ballard's implementation of this technology has had an impact on the cost of their H_2 fuel processing technology.

Question 5: Proposed future work

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

- Validating the laboratory results in a field test is very important.
- The project appears to be approaching an end. The proposed future work plan appears to be good. It is not clear whether the forward work plan also includes the WGS catalyst integration with the membrane.
- While the overall work plan is sound, the effort toward integrating the WGS reaction into this membrane system seems to be lagging behind. It is not clear whether an optimal system will be ready for the field test, given the project is in its final months.
- The future work is a reasonable approach based on previous accomplishments. The fact that the investigators are using a third-generation membrane presupposes it has a much higher probability of success. The field test with laboratory manufactured synthesis gas is not as important as actual synthesis gas produced from a working gasifier. However, this information can validate the thermal design.

Project strengths:

- The team has a good understanding of membrane development and implementation on ceramic supports, and is building on previously developed capabilities. The team has made good progress and the work plan is good.
- The project has made tremendous progress toward developing the membrane system to meet its performance and durability targets.
- The project features a good engineering approach to achieving the desired flux, a good engineering approach to sealing, and a good experimental database.

Project weaknesses:

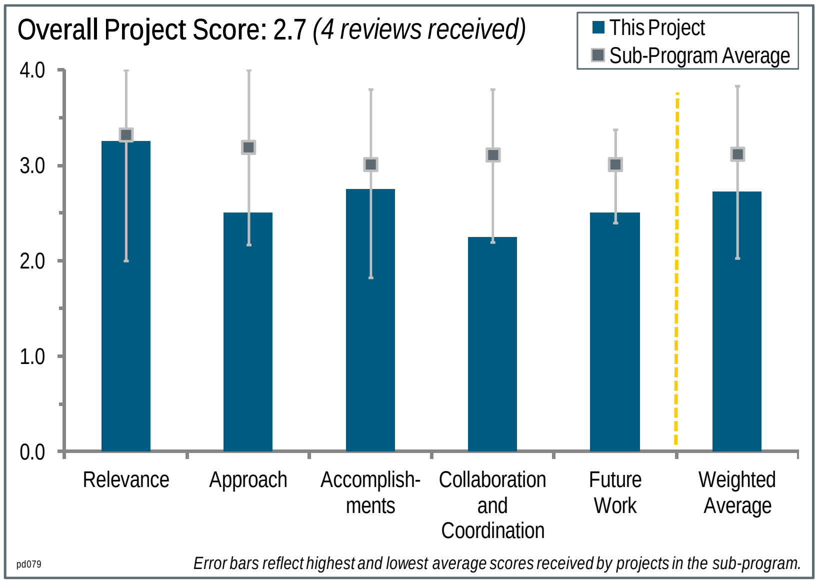
- No outstanding weaknesses were identified. The work might benefit from stronger support on the WGS catalysis side; it is not clear if anything other than catalysts will be provided by Johnson-Matthey Fuel Cells Inc..
- It is difficult to quantitatively evaluate the impact of this technology on the cost of producing H_2 using fuel processing technology, given that this is not a stand-alone technology. In particular, it is difficult to determine if this technology will have a significant bearing on improving the cost performance of biomass-based reforming processes, which to date have been unable to meet the DOE cost targets. Natural-gas-based technologies have already been demonstrated to meet the DOE cost target, so the impact on this technology may be minimal. The integration of the WGS reaction into the membrane unit has progressed extremely slowly. There is no clear path forward for addressing the stability issue during cooling under H_2. It is not clear if these tests were done under dry H_2 or in the presence of moist H_2, which will be the case for the operating unit. It is not clear what impact condensation would have on the stability of these membranes, particularly during turn-up if the turn-up rate is high. This reviewer would recommend working with Ballard to understand how its system will operate during periods of stand-down. This reviewer's impression is that it will continue to operate, but at a significantly lower capacity to maintain the catalysts near their optimal operating temperature. There are significant benefits for doing this in terms of maintaining catalyst performance and inhibiting catalyst deactivation due to physical processes associated with rapid turn-ups.
- There are insufficient publications and peer-reviewed articles on the work.

Recommendations for additions/deletions to project scope:

- One reviewer suggests that the WGS reactor is also tested with cycling.
- The team should test the final configuration in an actual synthesis gas stream from a pilot- or semi-works-scale gasifier to show that the laboratory and modeled performance will be validated.
- Another reviewer had no recommendations.

Project # PD-079: Novel Photocatalytic Metal Oxides

Robert Smith; University of Nebraska at Omaha



Brief Summary of Project:

The principal objective of this project is to develop improved solid-state photocatalysts for the decomposition of water into hydrogen (H_2) gas using solar radiation. The near-term objectives are to: (1) engineer the bandgap of cesium niobate ($Cs_2Nb_4O_{11}$) through computer modeling for optimum photocatalytic activity in the visible portion of the solar spectrum, and (2) fabricate and experimentally examine the materials identified.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.3** for its relevance to DOE objectives.

- The project seeks to reduce the cost of H_2 via solar photoelectrochemistry by taking a stable oxide material and lowering its bandgap energy to make its optical absorption more responsive to solar illumination.
- The project objective addresses only one of the four outstanding issues in photoelectrochemical (PEC) H_2 production. Bandgap modification in the absence of band edge alignment is unlikely to provide PEC performance that meets the ultimate goals. This work should be represented within the PEC Working Group to permit its participation in the full scope of PEC technical targets.
- This project certainly supports the objectives of the DOE Hydrogen and Fuel Cells Program (the Program). However, the research is still at an early stage of the technology.
- This project to designed to engineer advanced bandgap of $Cs_2Nb_4O_{11}$ catalysis through both computer modeling and experimental fabrication processes to improved solid-state photocatalysts for the decomposition of water into H_2 gas using solar radiation. The project is directly aligned with the Program's goals, including both delivery and production goals for 2020. The Fuel Cell Technologies Program (FCT Program) proposes to improve fuel cell electric vehicle competitiveness by reducing the cost of producing, delivering, and dispensing H_2 to below a threshold of \$2–\$4/kg of H_2 by 2020. FCT Program production goals include the development of distributed and central technologies to produce H_2 from clean, domestic resources at a dispensed cost threshold of \$2–\$4/kg of H_2. This project drills down on an advanced catalysis system aimed at increasing H_2 production. The novel approach to advancing PEC catalysis of water is named as a critical target area for the Program. While the presentation does not include projected cost, the presentation targets improved production and the cost of materials, such as niobium, should be minimal.

Question 2: Approach to performing the work

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

- The approach is generally effective, but it could be improved.
- The approach to the work is comprehensive and includes a detailed computational analysis complemented by an experimental approach. The work uses a physical approach to examine and tailor the bandgaps and band edges of the proposed materials to optimize the energy absorption efficiencies. Computer simulations are used to design and modify the bandgap structures. The work includes an experiment aspect that measures absorption spectra for comparison with the model outputs.

- Lowering bandgap energy is only a small part of the PEC problem. This reviewer wants to know what the extinction coefficient is for charge carrier generation. Reasonable conductivity within the newly generated or altered conduction bands must be demonstrated. Long-term stability of the doped materials must be readdressed.
- There are significant deficiencies in the technical approach to calculating and engineering bandgaps for a complex material, $Cs_2Nb_4O_{11}$. Bandgap relevance to photoactive materials is primarily confined to the interface, and the density of states (DOS) determined through density functional theory (DFT) calculations at interfaces are untrustworthy because of the cyclical boundary conditions required for the DFT numerical framework. Furthermore, the extraction of in-bulk bandgap information from DFT-DOS calculations is uncertain, especially in regions of sparsely occupied states. The uncertainties described above are compounded by proposed "doping" of the $Cs_2Nb_4O_1$ with various ancillary materials in the absence of molecular dynamics calculations that ensure stable doped configurations. The evidence of widely distributed and sparsely occupied states is suggestive of a configuration that is unlikely to be maintained under any significant local disturbance, as would likely accompany charge displacement through photon absorption or other energetic phenomena.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.8** for its accomplishments and progress.

- A significant level of effort is demonstrated in performing DFT-DOS calculations, but the outcomes are unlikely to prove helpful in identifying new photoactive materials with improved PEC performance.
- The progress could be better if there is collaboration with an outside institution, such as the National Institute of Standards and Technology (NIST). Taking advantages of resources from NIST is always beneficial. Collaboration with national laboratories is also a good way to get confirmation on new materials development efforts.
- Based on the budget, which is quite modest, a significant amount of work has been accomplished as proposed, and the presenters demonstrated progress and results. The bandgap calculations via computer simulation have been completed, and the calculations agree with photoluminescence. The sol-gel synthetic route for $Cs_2Nb_4O_1$ production has been perfected, and the bandgap structure has been determined to be 3.5 eV by luminescence spectroscopy, as proposed. An extended computer doping study was set up; initial calculations and tests expect to be completed by May 2012 and are 90% complete. Synthesis of the new doped materials was started in December 2011, and dopants and levels will be determined from the computer study. This work is 50% complete, as appropriate. Overall, the results demonstrate the impact of various dopant amounts of sulfur-doped $Cs_2Nb_4O_1$ that has been synthesized by passing gaseous carbon disulfide over $Cs_2Nb_4O_1$ at elevated temperatures for a set amount of time, and their relative bandgap structures. Chemical analysis is being performed to determine dopant amounts as a function of experimental conditions.
- A lot of calculations have been performed on the effect of potential dopants, but actual synthesis of these materials seems to be just starting. The calculations examined the effect of substituting tantalum (Ta), vanadium, and sulfur on bandgap energy of cesium niobate (CNO). It appears that some work has been done with sulfur and doping with vanadium has yet to begin, and it is not clear whether the researchers will even try Ta, as the calculations did not depict a bandgap lowering that was as effective as they had anticipated. This reviewer wonders if the doping affects the color of the material. If CNO is white to start with and still white after doping, then little was accomplished with respect to solar absorption.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.3** for its collaboration and coordination.

- It would benefit the researchers greatly to collaborate with some other institutions that would help them characterize or show them how to characterize a photovoltaic/PEC material.
- This effort should be incorporated in the PEC Working Group to ensure timely peer review of the proposed work and the provision of advice and expert assistance in pursuit of the stated objective.
- There is a lack of collaboration with other institutions outside of the University of Nebraska.
- The project includes collaborations with the University of Nebraska at Omaha, the University of Nebraska-Lincoln, and National Taiwan Normal University. It is not clear why National Taiwan Normal University was chosen for the collaboration. It would be nice to see the project team collaborate with a DOE national laboratory,

such as Oak Ridge National Laboratory (ORNL), on the computational side. This might be a nice fit to use ORNL's extreme computational resources to extend the research; for example, using Jaguar.

Question 5: Proposed future work

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

- It is hard to think of future work when the researchers have so much of their current work ahead of them.
- Future work on this project should be delayed until subject matter experts have conferred with the principal investigator regarding some concerns.
- The proposed future work should include methods on how to scale-up the technology for H_2 production as well as information regarding what the barriers could be.
- The future work will include calculation of the effects of doping $Cs_2Nb_4O_1$ with metals by evaluating the bandgaps and band edges of the various composites. The team will perfect the synthesis conditions for the sulfur-doped $Cs_2Nb_4O_1$ and characterize the properties of composites by experimental analysis. This is a very logical next step to complete the findings.

Project strengths:

- This is an innovative idea to produce H_2.
- One strength is how the team is using the computer-aided bandgap analysis to drive the design of novel PEC catalysis materials for H_2 production. The computer assistance approach is outstanding.
- The project features a strong theoretical effort predicting the effect of the dopant on bandgap energy.
- Strengths of this project include the team's proficiency in running DFT-DOS and its wet chemistry dopant incorporation processes.

Project weaknesses:

- It is not clear why National Taiwan Normal University is needed for the optical measurement support or why this may not be performed in the United States.
- Much of the experimental work is still ahead of the investigators. They may hit a home run, but so far it appears that they have not made a dent in the optical absorption of NDO.
- An area of weakness is the team's understanding of the limitations and uncertainties of applying DFT-DOS calculations to bandgap prediction at an interface.
- The project lacks strong outside collaboration. It would be helpful if the innovative process is verified or confirmed by other institutions outside of the University of Nebraska at Omaha.

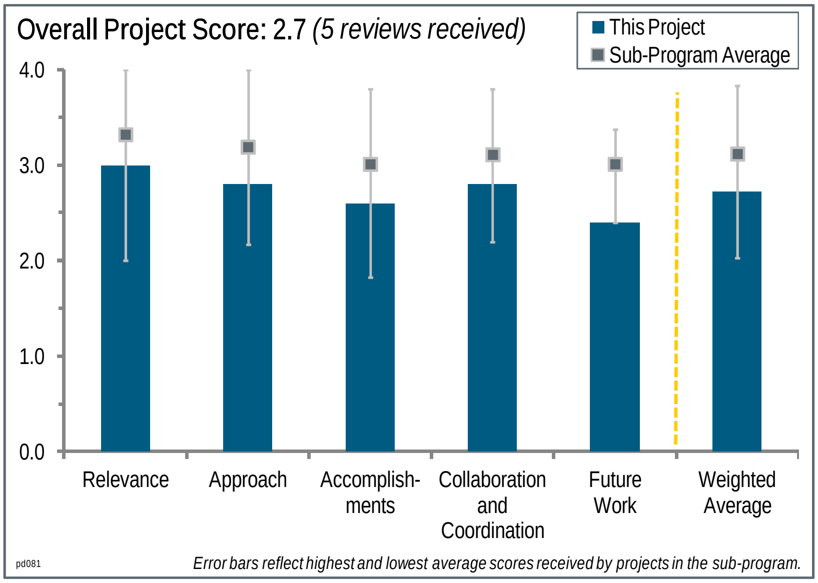
Recommendations for additions/deletions to project scope:

- The researchers should perform a thorough characterization of the new materials they produce, or group them with partners to get it done.
- The project scope should include a scale-up process for the mass production of H_2. Collaboration with outside institutions, such as NIST or national laboratories, is recommended.
- The proposal may be strengthened by including a national laboratory in the work, for example ORNL and its advanced computational resources. Also, it is recommended that the team consider the cost impact of the technology in future discussions of the work. It is unclear whether the work is economically feasible. This reviewer believes it is, but it would be nice to have this market analysis discussed and presented by the proposing team.

Project # PD-081: Solar Hydrogen Production with a Metal Oxide Based Thermochemical Cycle

Ivan Ermanoski; Sandia National Laboratories

Brief Summary of Project:
The overall project objective is to develop a high-temperature (HT) solar thermochemical (STCH) reactor for efficient hydrogen (H_2) production. The successful development of this reactor will provide a solar interface for most two-step, non-volatile metal oxide cycles. The objectives specific to 2011 and 2012 are to: (1) discover and characterize suitable materials for two-step, non-volatile metal oxide thermochemical cycles; (2) establish a screening protocol for candidate reactive materials and structures; (3) design particle receiver-reactor concepts and assess feasibility; and (4) construct and test reactor prototypes.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.0** for its relevance to DOE objectives.

- The project is relevant to DOE's goal of STCH H_2 production, and DOE's specific efforts to find the optimum HT material and solar reactor design.
- This is a simple, two-step thermochemical cycle for H_2 production that is applicable to the DOE Fuel Cell Technologies Program (FCT Program). This is such a long-term process that the near-term goals of the FCT Program will not be met. As with all solar-driven processes, there is the concern that where solar power is available, there is a limited supply of water. This is a challenge for all of the STCH projects.
- The project addresses the development of an HT solar thermochemical reactor for efficient H_2 production. The successful development of this reactor will provide a solar interface for most two-step, non-volatile metal oxide cycles. Several barriers are being addressed, including HT thermochemical technology, HT robust materials, and coupling solar and thermochemical cycles.
- Solar thermochemical cycles have the promise of being cost-effective, near-zero-carbon-emitting H_2 production options. The simple, two-step redox cycles included in this and other solar thermochemical cycle projects funded by the FCT Program are particularly attractive for this H_2 production approach. Much of this project is focused on a novel reactor design that could be utilized on the two-step redox cycles under examination in this project, or for other two-step redox cycles, such as the very promising hercynite cycle being studied in another DOE-funded project. The solar-thermochemical cycle focus of this project has been the ceria-based cycle, which has a relatively low redox capacity within each cycle and a relatively high reduction temperature, which make it less attractive than other potential solar-thermochemical redox cycles, such as the hercynite cycle. This project does intend to look at perovskites in the future plan.

Question 2: Approach to performing the work

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

- The project appears to feature a well-balanced approach to the development of both difficult materials challenges and a mechanically sophisticated process system.

- The researchers are studying the mechanisms of the reactions that are necessary to understand in order to make a successful process. There are large concerns over H_2 and oxygen (O_2) mixing. Even with long tubes and slow permeabilities, these systems will be operating continuously for years. While the researchers have identified the important activities, it is not clear if the order of work is going in a reasonable manner.
- The following approaches were outlined by the presenter: (1) discover and characterize suitable materials for two-step, non-volatile metal oxide thermochemical cycles, (2) establish a screening protocol for candidate reactive materials and structures, (3) design particle receiver-reactor concepts and assess feasibility, (4) construct and test reactor prototypes.
- The approach for screening the high temperature (HT) metal oxide materials and the reactor design is sound. However, the current solar reactor design is unlikely to be practical for a number of reasons: (1) maintenance of a reactor with mechanical moving parts and daily thermal swings on top of a tall tower structure should be of particular concern; (2) the heat recuperation assumption is too optimistic with the reactor likely located 30–60 m above ground and in long pipes; and (3) there is no material that can withstand 1,500°C operating conditions for too long, especially with the likely daily temperature swing, MO_x sintering issues will come up.
- This project is taking a good approach to meeting its overall objective of developing a cost-effective solar-thermochemical cycle method for H_2 production. It has invented a very novel screw conveyer reactor design that has a high potential to meet the needs of two-step, metal-particle-based redox thermochemical cycles, including efficient heat recuperation. The project is strongly focused on the reactor design, theoretical modeling of performance, and demonstration. This includes appropriate efforts on materials of construction that can withstand the HTs and reactivity of the metal oxides used in the thermochemical redox cycles. It is also using a novel laser-assisted stagnant flow reactor and sound kinetic analysis to determine the kinetics of ceria and a few other metal-based redox systems. The effort has included the evaluation of iron-based redox systems, which are generally known not to be good candidates for this approach to H_2 production. Part of the effort has focused on doping the ceria oxide cycle with other metals to improve its performance. This has not proven to be a useful approach. The ceria cycle has a relatively low redox capacity within each cycle and a relatively high reduction temperature, which make it less attractive than other potential solar-thermochemical redox cycles such as the hercynite cycle work in another DOE-funded project. This project does intend to look at perovskites in the future plan. The work done so far to demonstrate the feasibility of the novel reactor design is not nearly aggressive enough. So far, only ambient temperature testing for short durations (one hour) has been done. Much more work is planned though. Although the H_2- and O_2-generation steps occur at different places in this reactor design, there is still the possibility of them diffusing through the packed powder and mixing. This issue should be evaluated experimentally as soon as possible within the project. There does not appear to be cost-estimate work being done to ensure the cost effectiveness of the reactor and the overall approach of the project.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **2.6** for its accomplishments and progress.

- A system concept improving the design to one moving part is a major design improvement.
- Progress for the past fiscal year was reported.
- There has been significant progress made on this project. The novel reactor design has been completed, built, and tested under ambient conditions for short durations (one hour), and it has successfully conveyed powder particles as designed. It has been theoretically modeled and shows promise of achieving its objectives, including sufficient heat recuperation. Alumina has been tested and appears adequate as a material for construction of the reactor. Ferrite and ceria redox systems have been characterized for their kinetics in a novel laser-assisted stagnant flow reactor. Ceria doping has been tried to improve the ceria cycle kinetics, but this proved unsuccessful.
- The project has achieved reasonable results despite the enormous challenges associated with all STCH processes. The researchers tried to address the HT materials and thermochemical technology aspects of the project barriers; however, little was presented on the stated solar and thermochemical coupling challenge. Although it was mentioned that the material was stable for up to 30 cycles, no data was provided. The particle size did not affect the reduction reaction. It would be helpful to know if similar studies were carried out for the oxidation reaction.
- The finding that the materials are not kinetically limited is not surprising at 1,500°C operation. There has been quite a bit of mechanistic studies on the different materials. The analysis work seems to be well done. Moving away from ceria to non-rare-earth materials is a good step, given the recent concern over rare-earth-material

availability. Moving to doped materials may result in decreased durability compared to the pure CeO_2. It seems the heat recuperation will be limited due to the fact that it is a solid-solid particle heat exchange. The tests on material compatibility for CeO_2 are a good start. The researchers are 75% done with the project and have not yet developed the protocol for material characterization. One would think that this was the first thing that would have been done. With 75% of the project complete, it seems that they have focused on reactor and particle conveyance development, but they have not even finished the most basic milestone of developing materials characterization protocols, nor have they identified materials. The criteria for success are not well identified. The focus of the work does not seem appropriate. They need to demonstrate that the chemistry can work, develop testing/characterization protocols with metrics for success, and develop the materials. These critical areas seem to have a lower priority than the hardware, but without the material, the hardware is not needed.

Question 4: Collaboration and coordination with other institutions

This project was rated **2.8** for its collaboration and coordination.

- There seems to be good collaboration for this project.
- The coordination between the University of Colorado and Sandia National Laboratories appears to be very good.
- The collaboration is sufficient, although the emphasis of the project should be on creativity and productivity; not on collaborations. (The value of this entry as a means of assessing success is questionable. The Program may wish to rethink or rephrase this evaluation question.)
- There is good collaboration with the University of Colorado on kinetic studies, and with Bucknell University and Jenike and Johanson, Inc. on the reactor and reactor screw design.
- STCH projects require a higher degree of collaboration than other projects in the Program, such as materials, redox chemistry, mechanical and reactor engineering, process engineering, and heliostat design.

Question 5: Proposed future work

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

- Demonstration of the reactor prototype platform will be a good addition.
- Their future work seems very ambitious. Because material development is still being done, it is not clear why the process is being scaled-up. It seems that it would be better to develop the material prior to scaling the process. The investigators need to develop their protocol for material characterization, and it should include clear metrics for success.
- There is a good plan for future work. Perovskites will be studied because they can potentially yield a redox cycle that is more suitable than ceria. A reactor prototype will be built that can be tested at relevant temperatures. Design and analysis of overall system performance will be done.
- On one hand, the researchers propose to go back to the basics, as in new perovskite-related material screening, acknowledging the ultimate challenges and unrealistic expectations of CeO_2-based materials. On the other hand, they are chugging along with developing a CeO_2 manufacturing process. This is a disconnect.

Project strengths:

- This is a large, well-funded project with a strong team. The thermal chemical cycle is very simple, which improves the chances for success. The researchers have developed a way to operate 24/7, which overcomes the diurnal limitation.
- The reactor design work is an area of strength.
- The particle elevator is an improvement over the previous design.
- Solar-thermochemical cycles have the promise of being cost effective, near-zero-carbon-emitting H_2 production options. The simple, two-step redox cycles included in this and other Program-funded solar-thermochemical cycle projects are particularly attractive for this H_2 production approach. Much of this project is focused on a novel reactor design that could be utilized on the two-step redox cycles under evaluation in this project, or for other two-step redox cycles. This project is taking a good approach to meeting its overall objective of developing a cost-effective solar-thermochemical cycle method for H_2 production. It has invented a very novel screw conveyer reactor design that has a high potential to meet the needs of two-step metal-particle-based redox thermochemical cycles,

including efficient heat recuperation. The project is strongly focused on the reactor design, theoretical modeling of performance, and demonstration. This includes appropriate efforts on materials of construction that can withstand the HTs and reactivity of the metal oxides used in the thermochemical redox cycles. There has been significant progress made on this project. The novel reactor design has been completed, built, and tested under ambient conditions for short durations (one hour), and it has successfully conveyed powder particles as designed. It has been theoretically modeled and shows promise of achieving its objectives, including sufficient heat recuperation. Alumina has been tested and appears adequate as a material for construction of the reactor. Ferrite and ceria redox systems have been characterized for their kinetics in a novel laser-assisted stagnant flow reactor. Perovskites will be studied because they can potentially yield a redox cycle that is more suitable than ceria.

Project weaknesses:

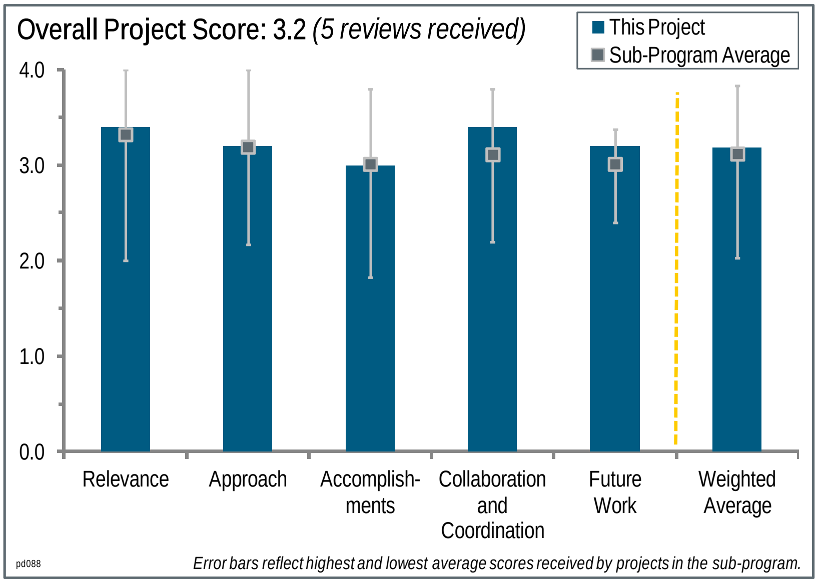
- There is a lack of sufficient collaboration.
- The project is 75% complete, and the researchers still have concerns about the materials. The progress seems to be slow. The project is 75% complete, but the protocols for material characterization are only 50% finished, while the particle conveyor concept is 100% complete. It does not seem that the work has progressed in a logical order. It seems, at a minimum, the protocol for material characterization should have been completed first. It is unclear why the researchers are spending effort on scaling the reactor up while they are still looking for the right material. They need to look at how this project is being managed.
- Any system with 1,100°-1,500°C temperatures and moving mechanical components is asking for trouble. No real support is given for a solar-to-H_2 efficiency of 25% (yearly average).
- The solar-thermochemical cycle focus of this project has been the ceria-based cycle, which has a relatively low redox capacity within each cycle and a relatively high reduction temperature, which make it less attractive than other potential solar-thermochemical redox cycles such as the hercynite cycle work in another DOE-funded project. The effort has included an evaluation of iron-based redox systems, which are generally known not to be good candidates for this approach to H_2 production. Part of the effort has focused on doping the ceria oxide cycle with other metals to improve its performance. This has not proven to be a useful approach. The work done so far to demonstrate the feasibility of the novel reactor design is not nearly aggressive enough. So far only ambient temperature testing for short durations (one hour) has been done. Much more work is planned though. Although the H_2- and O_2-generation steps occur at different places in this reactor design, there is still the possibility of them diffusing through the packed powder and mixing. This issue should be evaluated experimentally as soon as possible within the project. There does not appear to be cost-estimate work being done to ensure the cost effectiveness of the reactor and the overall approach of the project.

Recommendations for additions/deletions to project scope:

- It would be nice to see a breakdown of the efficiencies by the process. This way the reviewers can see where the heat loss is and clearly identify the motivation for where the researchers are working. The project does not seem to be focusing on the critical elements for it to be a success. The researchers are working on scaling-up the reactor when they do not even have a protocol for material characterization, and, apparently, the criteria for a successful material has not yet been shown.
- Solar-to-H_2 efficiency is reported to have a high heating value (HHV) ratio, but use of low heating value (LHV) is standard. The cycle life of the materials should be stated and tested. Cycle times have a significant impact on H_2 cost and should be overtly addressed in the project.
- The team should include evaluation and testing of the hercynite redox cycle in the novel screw reactor system. It should also do a thorough H2A cost analysis of a complete commercial-sized facility based on this novel reactor design.
- It may be beneficial for the Hydrogen Production and Delivery sub-program to revise the work scope and performance targets for all STCH projects, not just this particular one. STCH projects have been going on for years, and yet there is almost no convergence to a clear pathway to success. In practice, thermodynamically favorable, HT water splitting materials are still being screened. At this point, it does not make much sense to pilot a closed-loop solar reactor in an actual solar field with non-ideal materials. Researchers could be burdened and discouraged if they are asked to reach an almost impossible target.

Project # PD-088: Vessel Design and Fabrication Technology for Stationary High-Pressure Hydrogen Storage

Wei Zhang; Oak Ridge National Laboratory



Brief Summary of Project:

The objectives of this project are to: (1) address the significant safety and cost challenges of the current industry-standard steel pressure vessel technology, and (2) develop and demonstrate the composite vessel design and fabrication technology for stationary storage systems of high-pressure hydrogen (H_2). The approach includes the use of commodity materials (e.g., structural steels and concretes), the mitigation of H_2 embrittlement to steels, an automated manufacturing process for a layered steel tank, and embedded sensors to ensure safe and reliable operation.

Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.4** for its relevance to DOE objectives.

- Safe and affordable high-pressure storage at refueling stations is critical to achieving market penetration.
- The project team identified that storage is a barrier to H_2 infrastructure and vehicle development.
- From previous presentations, it is clear that compressors are the largest cost in a station. In order for this reviewer to properly evaluate the relevance of the project, the project team should better indicate the effect of vessel cost on H_2 delivery cost.
- This project is very relevant to the objectives of creating a low-cost, novel composite vessel technology to contain high-pressure H_2 at fueling stations.
- Pressurized H_2 bulk storage is a significant cost in gaseous H_2 delivery infrastructure. In order to meet the DOE Hydrogen and Fuel Cells Program’s (the Program’s) targets for the cost of H_2 delivered to a refueling station, it is necessary to reduce the cost of this storage. The approach taken of utilizing low-cost concrete, which enables a reduction in the use of more-costly steel, holds the promise to meaningfully reduce the cost of H_2 bulk storage.

Question 2: Approach to performing the work

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

- The project features a very novel approach to achieving lower-cost storage by using layered stainless and alloy steel enveloped by concrete.
- The cost per unit of pound weight does not tell the whole story. It should list the cost per unit of strength in whatever form is relevant.
- The investigators propose an innovative method of using some low-cost materials, including pre-stressed concrete with a composite metallic pressure vessel construction fabrication method that has been used for industrial pressure vessels in a few select applications.
- The basic approach and analysis seem very good, and in some aspects it is outstanding; however, in other aspects it appears that the investigators made a decision early in the design process that they now accept without question, when perhaps they should revisit the earlier decision. Anytime an item is changed in a system design such as this, one needs to evaluate the impact of that change on every other part and design decision. The range

of liner materials under consideration and fabrication techniques should be expanded. Rather inexpensive oxide coatings have been found to be effective barriers to H_2 permeation for ultra-high vacuum systems, and this reviewer wonders if they could also be used for H_2 storage tank walls. If the concrete is in compression, this reviewer wants to know if the steel walls should also be in compression. It was unclear if the elimination of tensile stresses in the steel would eliminate the potential for H_2-assisted fracture, regardless of the material or alloy. It would also be helpful to know if a stress analysis of the system would help answer these questions.

- This project is investigating utilizing a concrete steel composite structure to reduce the cost of pressurized H_2 storage. The low-cost concrete reinforcement permits a reduction in the use of more-costly steel. This approach holds considerable promise. The current design utilizes an inner barrier layer of expensive stainless steel, three layers of carbon steel, and then reinforcement with pre-stressed concrete. Recent available data shows that carbon steel can be sufficient as an H_2 barrier, and its H_2 embrittlement can be sufficiently mediated with proper design and thickness. The project should consider eliminating the costly stainless steel barrier layer to further reduce the cost. It would also be interesting to look at the cost of utilizing a carbon fiber composite-based inner vessel lined with high-density polyethylene as a barrier layer with reinforcing concrete to reduce the amount of costly carbon fiber needed. The approach being taken is to design the reinforced concrete vessel using engineering calculations and existing pressure vessel codes, layout the manufacturing process in detail, estimate the cost of the vessel, and then optimize the design to lower the cost. All of this will be done before construction and testing. This is a very good and cost-effective approach. The vessel design includes the use of friction stir welding. This is a concern because this welding technique has not yet been commercialized, but it would be a very cost-effective welding approach, especially if it could be automated as planned. The project has excellent collaborators, including commercial engineering companies; experts on specialty concretes, high-strength steels, and friction stir welding; and the U.S. Department of Transportation (DOT).

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.0** for its accomplishments and progress.

- The project team is making good progress and has produced a lot of new results.
- The project has produced a lot of calculations and estimations, but little demonstration of the concept.
- Good progress was made on the cost estimates for the pre-stressed concrete sleeve and the composite steel tank. It was not encouraging to see the projected cost go up with this more detailed analysis.
- There has been considerable progress made on this project. Two concrete-reinforced vessels and a standard all-steel vessel have been designed to meet the applicable pressure vessel codes. The manufacturing process has been laid out in detail and cost estimates have been completed that show promise for this approach to reduce the cost of H_2 storage.
- The team is making steady and significant progress toward the goals and objectives of the project. Researchers have identified and overcome significant barriers. A summary table of issues addressed, as well as materials and tests used, would help present this progress.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- This project features good collaborations.
- The project team includes a wide range of industry and research representatives.
- The project has excellent collaborators, including commercial engineering companies; experts on specialty concretes, high-strength steels, and friction stir welding; and DOT.
- There is significant collaboration in the design of the concrete composite, cost estimating, welding, and regulatory testing.
- There was a fair amount of collaboration with the project partners, but this reviewer did not hear about their contributions.

Question 5: Proposed future work

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

- The team needs to think about how to fabricate the final systems on-site because shipping large vessels seems problematic.
- The team needs to demonstrate the concept with real H_2 storage.
- The future plan is well thought through and complete. It includes optimizing the vessel design to minimize cost through engineering calculations and manufacturing cost estimates. Both materials and strain sensors that could be incorporated within the structure to detect any problems during use will be tested for durability in a pressurized H_2 environment. A vessel will then be constructed and fully tested for use against the appropriate codes and standards.

Project strengths:

- This project represents a novel concept for low-cost storage.
- The strength of this project is the potential for cost reduction in H_2 pressure vessel production.
- This project features a good, hard-working team with good ideas and a nice design. The team has done an excellent job of assembling relevant collaborators and using collaborations. There is a good plan and progress is being made toward completion.
- Pressurized H_2 bulk storage is a significant cost in gaseous H_2 delivery infrastructure. In order to meet the Program's targets for the cost of H_2 delivered to a refueling station, it is necessary to reduce the cost of this storage. The approach taken in this project of utilizing low-cost concrete, which enables a reduction in the use of more-costly steel, holds the promise to meaningfully reduce the cost of H_2 bulk storage. The approach being taken is to design the reinforced concrete vessels using engineering calculations and existing pressure vessel codes, layout the manufacturing process in detail, estimate the cost of the vessel, and then optimize the design to lower the cost. All of this will be done before construction and testing. This is a very good and cost-effective approach. The project has excellent collaborators, including commercial engineering companies; experts on specialty concretes, high-strength steels, and friction stir welding; and DOT.

Project weaknesses:

- The team should review the idea of using a high-cost stainless steel liner. It needs to demonstrate the concept.
- One weakness of this project is that $920/kg of H_2 seems high. This reviewer seems to remember Argonne National Laboratory publishing $20/kWh for automotive composite vessels that are higher pressure and lighter than these. This reviewer wants to know if infrastructure vessels are really expected to be more expensive than vehicle vessels, and if there is an advantage of these vessels that justifies the higher cost.
- One potential challenge appears to be that the technical requirements of the manufacturing process producing the pre-stressed concrete sleeve will be such that this manufacturing step will not be easily done in the field. Shipping the completed concrete-reinforced tanks from a central location may be challenging, particularly for the targeted 1,500-kg storage system proposed for fueling stations.
- Recent available data shows that carbon steel can be sufficient as an H_2 barrier and its H_2 embrittlement can be sufficiently mediated with proper design and thickness. The project should consider eliminating the costly stainless steel barrier layer to further reduce the cost. It would also be interesting to look at the cost of utilizing a carbon fiber composite-based inner vessel lined with high-density polyethylene as a barrier layer with reinforcing concrete to reduce the amount of costly carbon fiber needed.
- The team fails to take a truly global view toward materials selection and design for avoidance of H_2 embrittlement. For example, if the steel wall is in compression, because the concrete has to always be in compression, then the inner liner material is a redundant system for H_2 embrittlement avoidance. The inclusion of a redundant protection system is a great approach, but only if it is not expensive. Therefore, it is unclear why one would spend so much on a redundant system, especially when there may be much less expensive solutions (e.g., high density polyethylene, ceramic coatings, and inexpensive ceramic slurry coatings).

Recommendations for additions/deletions to project scope:

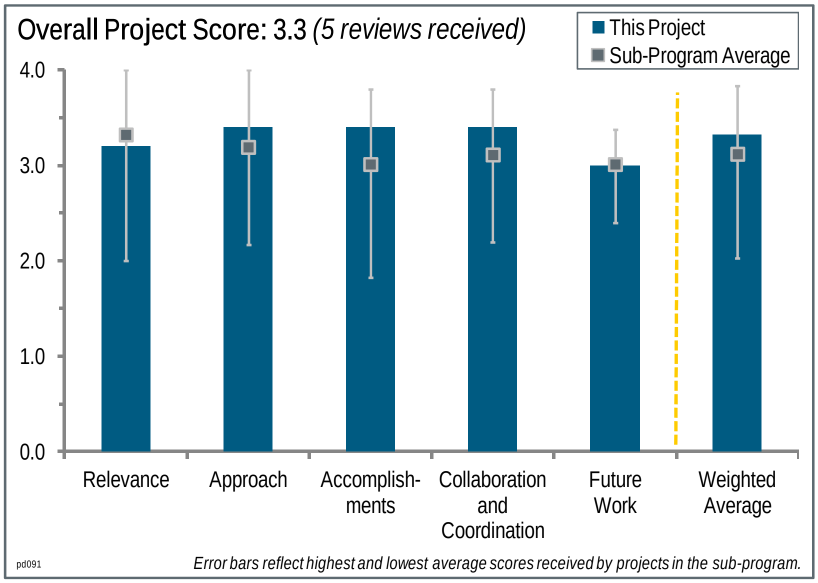
- Recent available data shows that carbon steel can be sufficient as an H_2 barrier and its H_2 embrittlement can be sufficiently mediated with proper design and thickness. The project should consider eliminating the costly stainless steel barrier layer to further reduce the cost. It would also be interesting to look at the cost of utilizing a carbon fiber composite-based inner vessel lined with high-density polyethylene as a barrier layer with reinforcing concrete to reduce the amount of costly carbon fiber needed.
- The team should increase the scope to include the evaluation of the effects of different liner materials on H_2 permeation and the evaluation of whether or not liner materials are really needed (stress analysis).
- This reviewer has no recommendations at this time.

Project # PD-091: Bio-Fueled Solid Oxide Fuel Cells

Gokhan Alptekin; TDA Research

Brief Summary of Project:

The overall objective of this project is to provide ultraclean biogas to demonstrate the operation of a high-efficiency solid oxide fuel cell (SOFC) stack in a waste-to-energy application. More specifically, the objectives of the project are to:
(1) develop and demonstrate a high-capacity sorbent to remove sulfur species from biogas, thereby providing an essentially sulfur-free biogas that meets the cleanliness requirements of SOFCs;
(2) demonstrate operation of a 2 kW_e biogas-fueled SOFC stack integrated with a biogas cleanup system in two different waste-to-energy applications; and (3) demonstrate the economic viability of TDA Research's sorbents to clean up biogas.



Question 1: Relevance to overall U.S. Department of Energy (DOE) objectives

This project was rated **3.2** for its relevance to DOE objectives.

- The TDA Research gas cleanup system unit has already met the DOE goal to purify biogas for fuel cell applications. This helps to meet the objective of using domestic, clean energy.
- The effort is relevant because it will enable the use of fuel cells with biogas.
- Cost-effective biogas purification is of great value to the attractiveness of fuel cells because it allows for the use of renewable biogas.
- Sour gas cleanup is a well-established and highly mature technology that is practiced extensively on biogas and natural gas. It is not clear how this technology is superior to existing off-the-shelf cleanup solutions. See: http://www.quadrogen.com/wp-content/uploads/2011/10/Quadrogen-IBCS-for-Fuel-Cells.pdf and http://www.biothane.com/en/medias/articles/onion.htm.

Question 2: Approach to performing the work

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

- TDA Research has a well-designed project that does an excellent job of addressing the barriers to desulfurization of organic sulfur-containing fuels. However, this reviewer is not sure how far along FuelCell Energy (FCE) is with producing its skid.
- The team features a good team of collaborators whose support upon successful completion of the project will be integral to accelerate commercialization.
- The skid-mounted system appears to be well designed and constructed. The landfillable sorbent is a good development. The sorbent has high sulfur capacity.
- The approach is consistent with the development of adsorbents. It was not clear if the down-selection process of the adsorbent material was based on the tests. The criteria used to assess the materials and how the results compare to the DOE targets were not addressed in the poster. The work only presented test results on H_2S, and not the more complex sulfides. Skid testing appears to be appropriate, yet from this poster it is difficult to assess

the viability of the design. Knowing what the design criteria were for the skid unit would be helpful. The breakdown of the work performed by TDA Research and FCE is unclear.

Question 3: Accomplishments and progress towards overall project and DOE goals

This project was rated **3.4** for its accomplishments and progress.

- The TDA Research gas cleanup system unit is skid mounted and has already been successfully field tested.
- The purification team has conducted a thorough investigation of the challenges associated with biogas purification.
- The system has been developed on time and on budget. There has been minimal impact on the cost of electricity.
- It would have been helpful to have presented the work plan with the work schedule. It is difficult to assess the accomplishments from this work as presented, except for the development and build of the skid-mounted unit. Little detail on adsorbent efficiency, breakthrough, etc., was presented, especially with respect to the more-complex species and how it impacts the process. It was also not possible to understand the gas quality that the fuel cell will see. This reviewer wants to know why only H_2S was discussed and what the adsorbents are. It is mentioned that siloxane cleanup is of interest, yet there is not any work presented on this species. It is reported that there are “expandable” and regenerative adsorbents, yet the presenter briefly explained that the skid is based on the expendable approach. It is unclear what tests were performed to decide this was the most viable approach. The piping and instrumentation design of the skid-mounted unit appears to be appropriate, yet without additional engineering criteria it is difficult to assess the viability of the unit, especially in light of the different species. This reviewer wants to know if the polishing bed can handle everything but H_2S. Slide 8 states that “we decided to use our own bulk desulfurizer…” This reviewer asks if whether this implies that from the work plan, a design and build test task (task 2) was not required.

Question 4: Collaboration and coordination with other institutions

This project was rated **3.4** for its collaboration and coordination.

- TDA Research has assembled a very strong team for the design, manufacture, and testing of the fully integrated system.
- The collaborative efforts appear appropriate for this project.
- There is a strong collaboration team.
- Collaboration seems limited to TDA Research and its customers.

Question 5: Proposed future work

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

- TDA Research is ready to move forward with the fully integrated system and test it at Cal-DeNier Dairy.
- The proposed work appears to be appropriate.
- The future work appears to be on target for a successful completion of the project.

Project strengths:

- The project features a strong collaboration team.
- The project features a good design and a promising sorbent.
- There are several obvious strengths, including TDA Research’s expertise in gas cleanup for natural gas, the fact that TDA Research has actually field tested its skid and is in the process of contracting with fuel cell manufacturers, and the successful field testing involving gas clean up for wastewater.
- TDA Research’s experiences and expertise in sorbent development and process operations are significant and should facilitate the success of this effort. FCE is fully knowledgeable of integration challenges of this sort. FCE also fully understands (fuel) gas requirements for the SOFC technology.

Project weaknesses:

- It is not clear that the project represents a major advance over the current commercial technology.
- One reviewer would have appreciated a chance to ask questions about the SOFC. Because biogas-fueled SOFC units are not that common, it would have been helpful to understand the perceived challenges associated with producing the SOFC unit as well as with full integration of both the biogas cleanup unit and the SOFC.
- It was not clear if there are weaknesses, yet it is easy to identify areas that might not have been thoroughly reported, presented, or addressed. The data reported at the dairy site was not comprehensive, and this reviewer wants to know if this possibly impacts the approach.

Recommendations for additions/deletions to project scope:

- The team should perform a joint presentation with FCE next time.
- It may be worthwhile to explore the project's applicability to other technologies and for H_2 generation.
- This reviewer could not identify any recommendations.