

2015 — 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 Office (FCTO) 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 thermal and thermo-electrochemical conversion of bio-derived feedstocks, advanced water splitting, direct solar thermochemical (STCH) and photoelectrochemical (PEC) water splitting, biological hydrogen production, and hydrogen production pathway analysis. The hydrogen delivery projects reviewed included R&D for low-cost, reliable delivery technologies (pipelines and tube trailers) and forecourt components (compression, storage, and dispensing), as well as techno-economic analysis of stations.

The reviewers recognized the Hydrogen Production and Delivery sub-program as focused, effective, well managed, and having a clear strategy to achieve DOE goals and objectives. Reviewers commented positively on the relevance of delivery projects to near-term priorities and needs, and they commended the achievements of production projects in enhancing materials efficiency and durability and in innovative systems design. Reviewers encouraged more detailed studies of both near-term and future costs of production and delivery technologies, especially those at lower Technology Readiness Levels (TRLs), and they recommended broader leveraging of relevant technical expertise and greater collaboration with other domestic and international government agencies. Continued and strengthened emphasis on industrial collaboration and stakeholder engagement was strongly recommended. Reviewers also emphasized that fundamental science R&D needs to continue in parallel with the system-level applied R&D.

Hydrogen Production and Delivery Funding:

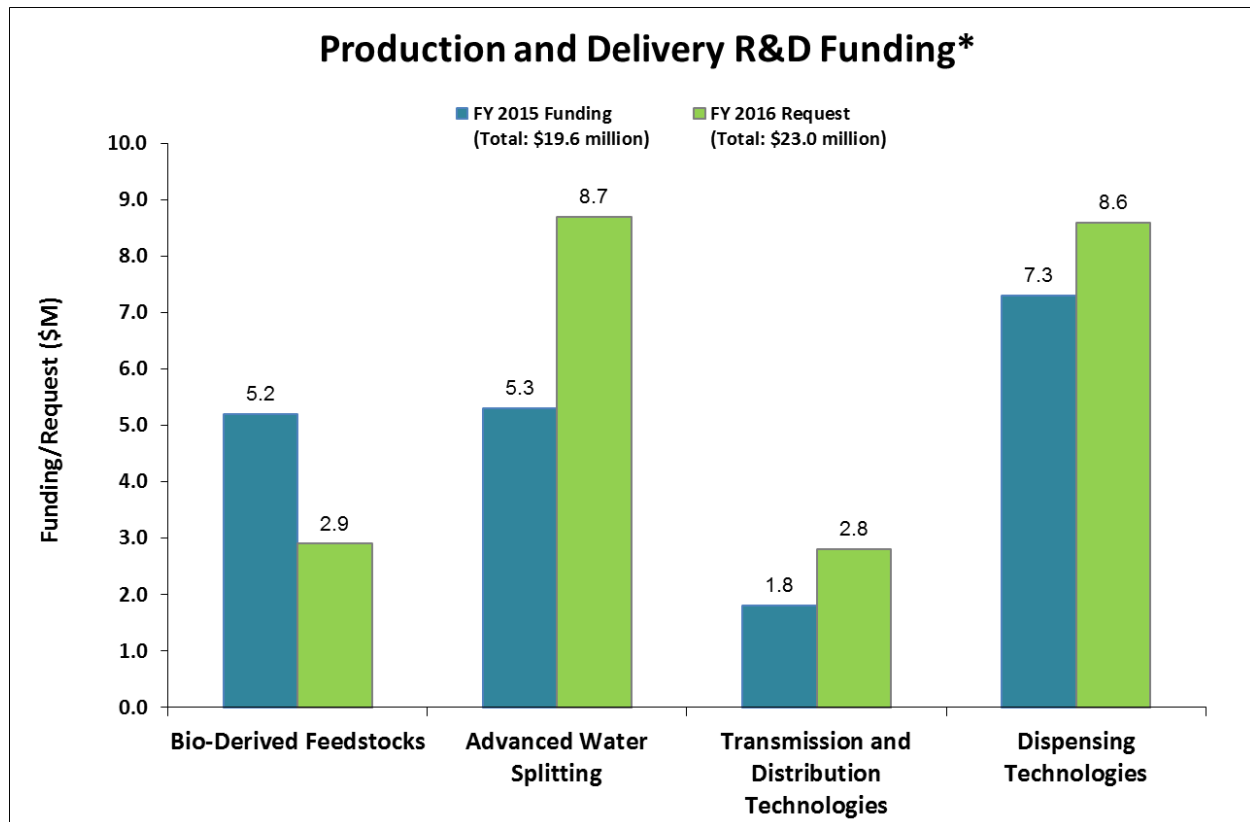
The fiscal year (FY) 2015 appropriation for the Hydrogen Production and Delivery sub-program was \$19.6 million, as shown in the chart on the following page. Funding was distributed approximately evenly between hydrogen production and hydrogen delivery, consistent with previous years. The production portfolio funding focus in FY 2015 was on mid- to long-term renewable pathways such as advanced water splitting; bio-derived feedstock conversion; and STCH, PEC, and biological hydrogen production. This emphasis will change in FY 2016 with a shift in focus to advanced water splitting pathways such as STCH, PEC, and other electrolysis technologies. FY 2016 will also include the addition of new fermentative hydrogen production projects competitively selected in FY 2015. FY 2016 planning is based on a \$23 million budget request (~\$11.5 million apportioned to production R&D). A consortium approach to accelerated development of renewable hydrogen production pathways is also planned. The delivery portfolio emphasis in FY 2015 was on reducing near-term technology costs, such as those associated with storage vessels and dispensing hoses, and on identifying additional low-cost, early market delivery pathways that are viable. This emphasis will continue in FY 2016 with ~\$11.5 million apportioned from the budget request, with an additional focus on the reliability of critical components, such as forecourt compressors and meters, as well as the performance of technologies relevant to mature markets, such as hydrogen liquefaction.

Majority of Reviewer Comments and Recommendations:

Twenty-two projects were reviewed, receiving above-average to high scores (2.7–3.8), with an average score of 3.3. The scores are indicative of the technical progress that has been made over the past year in the hydrogen production and delivery R&D portfolio.

Production Projects

Hydrogen Production Pathway Analysis: One project was reviewed in the area of hydrogen production pathway analysis. The project received a score of 3.4. Reviewers commended the project team's approach to developing



* Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area.

analytical case studies for solid oxide electrolysis cells (SOEC) and fermentation, which involved gathering information from research organizations and industry through quantitative questionnaires. The results of the analysis were seen as a necessary part of determining the feasibility of these technologies. The reviewers commented that it would be extremely valuable to evaluate near-term current costs in conjunction with the projected cost values based on high production volume. The reviewers recommended that the current technical barriers to both SOEC and fermentation be clearly articulated and documented. Reviewers also noted that the project's proposed case studies for next year focus largely on low-TRL pathways. They suggested that additional case studies of higher-TRL technologies, such as biomass gasification and pyrolysis, should be considered.

Advanced Electrochemical Water Splitting: Three projects in the area of hydrogen production from advanced electrochemical water splitting were reviewed, receiving an average score of 3.3. Projects included efforts to decrease the platinum group metal (PGM) loading of the electrolysis cell electrodes, efforts to understand electrolysis under variable electrical load, and efforts to develop membranes with advanced durability under high-temperature and high-pressure conditions. Reviewers praised the progress made toward developing low-PGM electrodes while maintaining performance and durability compared to commercial baselines with higher-PGM electrodes. They also commended progress made toward membrane testing for high-temperature/high-pressure electrolysis. All projects in the category have completed or are on track to completing major milestones. Reviewers recommended performing additional studies to better understand the long-term hydrogen cost ramifications of their improvements and technologies, as well as studies to better understand the molecular nature of the catalyst surface during operational conditions.

Bio-Derived Feedstock Conversion: Two projects were reviewed in the area of bio-feedstock conversion, and these received an average score of 3.2. Reviewers commended the bio-derived liquid reforming project for achieving significant progress towards CO₂ sorbent screening and for its initial techno-economic analysis. They expressed some concern that the project does not sufficiently address the likelihood of poisoning and coking in the reforming of bio-oil. The reviewers also suggested that more effort be spent on the catalyst R&D in order to reach

goals within the projected timeline. The reviewers complimented the reformer-electrolyzer-purifier project as being a low-risk, high-impact technology that could have the greatest chance of achieving $< \$2/\text{kg}$ of hydrogen in the near term, giving credit to the experienced project team for its focused and realistic goals. The reviewers pointed out that the carbon emission of the technology was not sufficiently discussed, and that the “free” heat source as an input in the Hydrogen Analysis (H2A) technoeconomic analysis was inadequately justified.

PEC Hydrogen Production: Two PEC projects were reviewed, receiving an average score of 3.4. Reviewers felt that projects in this area are well aligned with DOE objectives, with a focus on developing the most-promising PEC material systems and prototypes, such as those based on highly efficient III–V semiconductor and chalcopyrite thin-film materials. Projects were rated highly for advancing the efficiency and durability of PEC materials and interfaces. Reviewers highlighted the excellent collaborative successes of the projects involving the DOE PEC Working Group. Recommendations for future work included more detailed technoeconomic analysis of scaled-up technologies and more detailed schematics of proposed commercial PEC cell designs.

STCH Hydrogen Production: Three projects were reviewed in the area of STCH hydrogen production, with an average score of 3.0. Two of the projects focus on two-step, metal-oxide-based reaction cycles, and the third addresses a hybrid sulfur (HyS) reaction cycle, which includes an electrolysis step. Reviewers praised the innovative approaches and achievements in all three projects, including the (1) design of perovskite and hercynite reaction materials and the new reactor concepts for the metal-oxide STCH cycles, and (2) screening and characterization of advanced membranes and electrocatalysts for the HyS cycle. Reviewers expressed concern about the complexity of the integrated reactions and reactors for all three systems, and they recommended that project emphasis be placed on materials R&D to obtain the kinetics, durability, and other properties needed to achieve the hydrogen cost goal. Reviewers also recommended continued updating of technoeconomic analysis for the technologies, specifically including realistic capital costs.

Delivery Projects

Hydrogen Delivery Technoeconomic Analyses: Three projects were reviewed in this area, with an average score of 3.6. These projects studied the energy consumption of hydrogen pre-cooling; developed a model to characterize the forecourt costs of hydrogen delivery; and developed a report that describes in detail the costs, design, and layouts of hydrogen stations expected to be relevant in the near term. Projects were praised by reviewers for their technical robustness and relevance to DOE objectives. Recommendations were made for projects to integrate results with one another, and to collaborate more closely with potential end users (such as authorities having jurisdiction and station developers). Reviewers also recommended that analyses be expanded to include stations that may be viable in the mid term, such as those that support pipelines and tube trailers, have large capacities, or are located in different regions of the United States.

Hydrogen Delivery Technologies: Three projects were reviewed in the areas of hydrogen pipelines and tube trailers, receiving an average score of 3.4. The pipeline projects were praised for technical robustness (testing in high-pressure environments, studying welds, accounting for residual stresses, and testing multiple types of fiber reinforced pipeline). The project on tube trailers was praised for its potential for near-term cost reduction. Reviewers suggested that (1) the steel pipeline project collaborate more closely with industry to ensure real service conditions are represented, (2) the tube trailer project focus on codification where appropriate, and (3) the project on fiber-reinforced pipelines educate consumers on technology adoption.

Forecourt Technologies: Five projects were reviewed on hydrogen dispensers, compression, and storage, and these received an average score of 3.3. The project on dispensing hoses was praised for its technical approach, which included developing a thorough understanding of the material’s ability to withstand the chemical and mechanical stresses it will face in service. Reviewers suggested that the team collaborate with standards development organizations to ensure that all service conditions are accounted for, and with manufacturers to ensure that challenges with joining the hose to fittings are understood. The project on linear motor reciprocating compression was praised for its potential to lower station costs and improve reliability if successful. Reviewers expressed concern over the project’s thermodynamic efficiency compared to incumbent technologies, and they suggested that the team obtain guidance from experts in electric motors and compression. The projects on steel concrete composite vessels (SCCVs) for hydrogen storage were praised for the development of a prototype that passed a burst test, and for their progress to date. Reviewers suggested that the team compare the cost of the SCCV to existing competitors and

assess the strength of the vessel without concrete. The hydrogen storage project on wire wrapping Type I vessels was commended for its approach and promising burst test results. Reviewers suggested that the cost of this technology be assessed in greater detail, and that the team focus on acceptance of the technology by relevant codes.

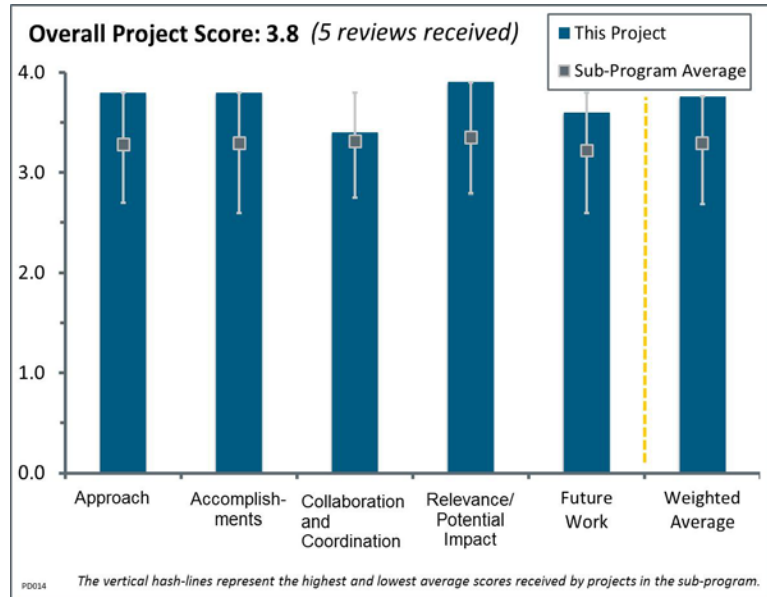
Project # PD-014: Hydrogen Delivery Infrastructure Analysis

Amgad Elgowainy; Argonne National Laboratory

Brief Summary of Project:

The objectives of this project are to (1) assess impacts of delivery and refueling options on the cost of dispensed hydrogen by (a) modeling refueling costs in early fuel cell electric vehicle markets, (b) evaluating the impact of design and economic parameters, (c) identifying cost drivers of current technologies, and (d) developing estimates of delivery and refueling cost reduction with market penetration; (2) assist with Fuel Cell Technologies Office planning; and (3) support existing U.S. Department of Energy-sponsored tools.

Question 1: Approach to performing the work



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

- The principal investigator continues to do great work. In California, focus is needed on near-term costs, and this effort does that. It is a welcome change from earlier models, which looked at the potential for long-term success. This is a great platform to facilitate ongoing learning. In California, it can be used to help determine what near-term costs should be, and it provides tools to prioritize funding.
- Early market uncertainties were well addressed. Infrastructure cost is better understood, given the learning curves, which were developed using solid data from vendors and vetted using California Energy Commission solicitations. It is very useful to finally have a cost analysis that reflects the conditions of the early market instead of assuming high volumes of component production and high market penetration.
- The work is appropriately aimed at modeling the cost associated with various hydrogen refueling station options in order to select the most cost-effective pathways and to identify areas to focus cost-reduction efforts. The use of a wide variety of external checks and reviews establishes the validity of the estimates.
- The project takes a proper approach for the analysis work by complementing the modeling work with current cost and designs of key refueling components to look into several options to identify areas for refueling cost reduction.
- The team members' efforts to understand potential options, quantify the strengths and weaknesses of each option, and assess the impact of various option configurations are impressive. It is also clear they understand how technology and market demand play into technical solutions.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The development of tools and models to estimate the cost of delivery and refueling has allowed the focus of production and delivery funding to be on critical barriers. The identification of ramp-up and economies of scale as critical factors should lead to further work in mitigating risks associated with the realization of profitable refueling stations.
- The most valuable accomplishment is the project's ability to clearly summarize the outcome of various liquid and gas solutions and show the impact as variables change. This work can immediately serve both policy initiatives and installation projects that will soon be built. The optionality for gas hydrogen delivery

by trailer is starting to change. Future reviews accounting for supplies delivered at 250 and 180 bar are happily anticipated.

- The development of the Hydrogen Refueling Station Analysis is an outstanding accomplishment. It is a great tool that will enable the development of the hydrogen and hydrogen fuel cell vehicle market by being released in the public domain. Another significant accomplishment is the development of a very comprehensive analysis of refueling component costs and of the impacts of utilization and economies of scale on hydrogen costs.
- The project is on target to assess impacts of liquid and gaseous delivery. There are already results in the Hydrogen Analysis (H2A) Hydrogen Delivery Scenario Analysis Model (HDSAM) that can be used by industry to better understand the cost of infrastructure today and potential future cost reductions.
- No progress can be made without a complete understanding of the near-term challenges and opportunities—this model does just that.

Question 3: Collaboration and coordination with other institutions

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

- The H2A Refueling Station Analysis Model has been scrutinized through the years, and it continues to stand up to this scrutiny. Clearly, inputs came from industry, which is incredibly helpful—the model appears to reflect reality.
- It is appropriate and particularly important that suppliers and vendors were interviewed to provide information on station components. It is good that the results of the model will be integrated into the Macro System Model and H2A.
- There probably should be more collaboration effort established with the car companies, station investors, and municipalities. In doing so, the project may gain insight into various challenges that exist and should be factored into the model (e.g., delivery of product by large or small trailers). Such collaboration would also accelerate the understanding that forecourt parties need to comprehend the interplay of variables that affect the success of a station and begin to forecast future evolution.
- Collaboration appears to be strong but not uniform across sectors. It is hard to tell how much input from end users has been included. The approach section mentions that reviews took place but not how well the work matched the reviews or how much “end-user” review was accessed.
- Collaboration looks appropriate for this project. The team may benefit if some collaboration with hydrogen and hydrogen station providers is also included.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.9** for its relevance/potential impact.

- The hydrogen infrastructure analysis is an extremely low-cost approach to understanding how to optimize fuel supply to fuel cell vehicles and prepare for the industry’s growth. This is vital work, and the team has proven its ability to keep pace with the market.
- This work helps DOE set near-term targets, which are incredibly important outreach and communication tools. It also benefits California planning efforts by helping stakeholders understand the state of the industry and what can be achieved with a variety of funding approaches.
- This type of analysis work is key to enabling the initial roll-out of hydrogen refueling stations and identifying options and opportunities for further cost reductions on hydrogen infrastructure.
- This project has a very practical and clear application for the implementation of additional hydrogen stations.
- The work will continue to strongly influence progress toward the goals and objectives of the Hydrogen and Fuel Cells Program (the Program).

Question 5: Proposed future work

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

- The outline presented at the Annual Merit Review will be challenging. The only recommendation is to increase the focus on collaboration to “spread the news” and receive relevant feedback from live developments.
- The model will improve with time. Of particular interest is the impact of individual components on station performance and cost through time. Anything that can be done to increase reliability and help the industry plan to keep stations online is helpful. It would be very helpful to understand the economic impact of planning for expansion vs. funding new stations.
- Including station footprint as part of this analysis will be extremely valuable to this work, as one of the main challenges right now is the space constraints within existing refueling sites.
- Future work is appropriate. It would be interesting to know how project economics may vary regionally, particularly in places where hydrogen refueling infrastructure is more likely to be deployed in the early years.
- It is great to see plans to work closely with the Codes and Standards Technical Team and other working groups. It would be helpful to continue to make models as accessible to stakeholders as possible. Providing tools with the ability to adjust or explore variables as needed would enable further exploration of the options available.

Project strengths:

- This project is aligned to needs and goals and has a variety of inputs and collaborations.
- The project’s relevance and ability to evolve with technology are strengths.
- The focus on near-term costs and targets is a strength.
- The project has appropriate stakeholder engagement.

Project weaknesses:

- Continued work to further engage and maintain stakeholder engagement should be a priority.
- The work should be used to bring forecourt personnel to a high level of understanding.
- Any weaknesses can be addressed by additions.

Recommendations for additions/deletions to project scope:

- The project should consider regional deep dives, i.e., how cost will be different in regions where hydrogen stations may be deployed in the next few years. Also, although tube trailer delivery for pre-compression has great project economics, it is not always practical in all situations, particularly where real estate is at a premium. Other options should be considered.
- It would be helpful to better understand the cost and impact of expansion of stations. If possible, it would be helpful to understand the impact of component reliability on operations and maintenance costs—and identify ways to invest where needed and cut costs where possible.
- The project should consider hybrid options to help handoff between early- and late-stage costs (liquid pumping vs. gas compression).

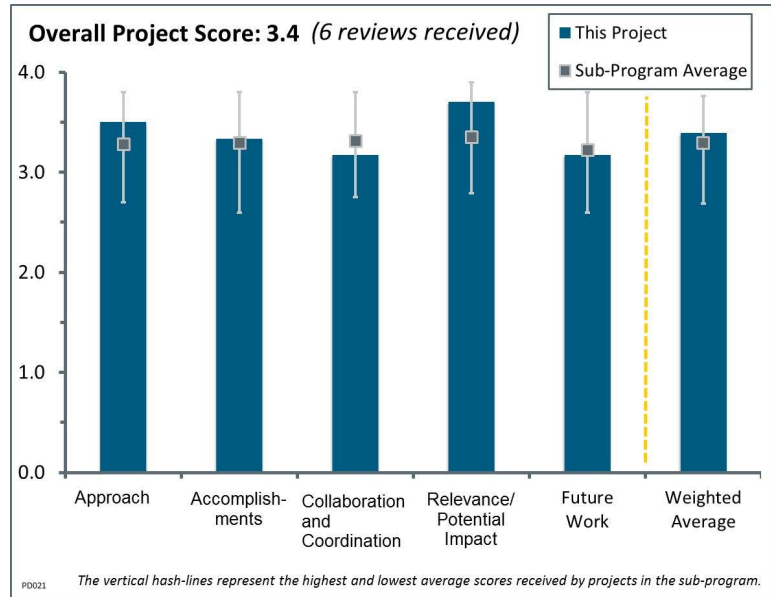
Project # PD-021: Development of High-Pressure Hydrogen Storage Tank for Storage and Gaseous Truck Delivery

Don Baldwin; Hexagon Lincoln

Brief Summary of Project:

The overall objective of this project is to reduce the cost of a near-term means of transporting gaseous hydrogen from the production or city gate site to the stations. Hexagon Lincoln will design and develop the most effective bulk hauling and storage solution for hydrogen in terms of cost, safety, weight, and volumetric efficiency. This will be done by developing and manufacturing a tank and corresponding International Organization for Standardization (ISO) frame that can be used for the storage of hydrogen in a stationary or hauling application.

Question 1: Approach to performing the work



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

- This was a very practical and productive project. The project team stayed focused on meeting practical objectives established by U.S. Department of Transportation (DOT) regulations, market requirements, and the physical manufacturing of the vessels. It is necessary for the U.S. Department of Energy (DOE) to fund a portfolio of projects but exciting when a few in the portfolio are market-ready.
- Hexagon Lincoln has taken a well-planned and logical approach to optimizing trailer capacity, using good engineering practices to increase capacity while maintaining compliance with DOT regulations.
- The project team has taken a very effective and consistent approach throughout the entire timeline of this work. It has been clearly defined and successfully executed.
- The approach is sound.
- This seems to be more of a Market Transformation or Technology Validation project in that it is testing trucks on the road and using compressed natural gas (CNG). The containers have been deployed. Some of the project goals (pressure) do not meet the DOE targets, and it is not clear that the project is offering a pathway to the DOE goals. The project is doing both hydrogen and CNG. The cycle testing is very important, and it will be interesting to see the results. The research aspect seemed to be more about engineering scale-up rather than the usual research and development (R&D) done in production and delivery work. It is not clear what the project is doing to address Barrier I: “Other fueling site/terminal operations.”
- The approach seems to be out of the realm of DOE R&D and into the commercial space. It seems unlikely that the project will produce a 540 bar system, and the 350 bar improvements are very incremental. A technical breakthrough may not be needed for the 540 bar to occur—perhaps only a business case and DOT approval. The discussion should be focused on the technical requirements to achieve project objectives.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The evolutionary work performed by Hexagon to achieve an optimal design for the market is very impressive—especially the work on the ISO frame to drive down weight and vessel configurations to maximize volume.
- Design and assembly of trailers has been carried out effectively. Current testing of Titan tanks with hydrogen is a good final step to this project.
- The technical accomplishments and the progress are impressive.
- The progress has mainly been incremental; however, with such large volumes, the incremental progress can have large impacts. The project should normalize to a materials cost so that market adjustments are not affecting the cost. The project needs to focus on the technical challenges to reach the 540 bar mark.
- It was difficult to tell what was accomplished in previous years compared to the current evaluation. The project has been going since 2008. Project team members have scope for cost reduction studies, but it was not clear what the results were. The team presented some generalities, but more specifics on the cost studies would have been helpful in the evaluation. The market the team describes cannot use the 540 bar system since, according to Hexagon, their clients rarely use the full capabilities of the 350 bar system. While this may be true for CNG, the hydrogen filling stations operate above 300 bar, and the vehicles are going to 700 bar; so it seems that the hydrogen market will be able to handle, and would probably prefer, the higher gas pressure. The on-road testing is impressive, but it was CNG. It was not clear that the project would be doing on-road testing with hydrogen. The team sort of showed a pathway to some of the DOE goals, but it was not clear.
- The new presentation did not show significant differences in the accomplishments of this work when compared to accomplishments presented in 2014. The only major difference in this year's accomplishments was the development of the hydrogen deep cycle test plan to be executed at Powertech. It was shown that some of the initial tests were conducted in April/May 2015, but initial findings were not presented.

Question 3: Collaboration and coordination with other institutions

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

- Collaboration is a little tough when the project is just ahead of market need. That said, Hexagon's work in natural gas gives the team very practical feedback from real-time users of the system. The collaborations performed with DOT, American Bureau of Shipping (ABS), Argonne National Laboratory (ANL), and Powertech were vital and clearly contributed to the success of the project.
- The project used outside testing very well at Powertech Labs and Stress Engineering Services. The presenter stated that the project had discussions and input to ANL for Hydrogen Refueling Station Cost Reduction studies; some details on these would be interesting and strengthen the collaboration score.
- The project has benefitted tremendously from "internal collaboration" with Hexagon Lincoln's natural gas transportation business.
- The ongoing collaboration with DOT on this project is very valuable to the success of this work. The addition of Powertech Labs for the testing activities will be very beneficial to the project.
- The collaboration with Powertech to perform deep cycle testing should show some technical improvements, and the coordination with DOT and ABS are appropriate to meet the project goals.
- The collaboration and coordination with other institutions are appropriate for this type of project.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.7** for its relevance/potential impact.

- The project is focused on one of the main barriers to fuel cell vehicle deployment by helping to decrease the cost of the hydrogen infrastructure and improve the hydrogen transportation opportunities. This has broad impacts beyond the Fuel Cell Technologies Office into areas such as merchant hydrogen transportation for petroleum and aerospace applications, as well as CNG and other gas delivery.
- As the early hydrogen market develops, high-pressure tube trailers will be the most cost-effective pathway for delivery of hydrogen to retail sites. Delivering at high pressures will provide more reliability and flexibility to the hydrogen retail stations, so reducing the costs for this transportation pathway is essential to the initial rollout of hydrogen fuel cell vehicles and refueling stations.
- Tube trailer delivery appears to be a low-cost delivery strategy, and Hexagon Lincoln has done an excellent job of maximizing trailer capacity to enable this delivery pathway.
- The relevance and potential impact are enormous for an alternate gaseous fuels infrastructure.
- This is a great project to demonstrate leading-edge relevance when delivery of gaseous hydrogen is required to serve a functional market. Unfortunately, the market for fuel cell vehicles has not kept pace with Hexagon's effort. As for impact, Titan will become a vital component in the early supply of hydrogen to the fuel cell vehicle market. What is unclear is the sustainability of this delivery solution over the next two decades.
- The project has been impactful, and *if* the team can get approval for the 540 bar system, then that could be very impactful for the rollout of fuel cell electric vehicles. However, perhaps the approval should have been separate from this project so that the project could focus on technical challenges, if there are any. The presenter mentioned that valves and controllers could be an issue at the 540 bar level, so emphasis should go to the design and manufacturing of these components.

Question 5: Proposed future work

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

- The cycle testing is very important. The project should reach out to more hydrogen fueling stations and fuel cell vehicle original equipment manufacturers to better understand the timing and needs of hydrogen fueling stations.
- The deep cycle testing at Powertech should add some benefit to the technical community, and the results should be shared.
- Future work proposed seems appropriate towards the development of this project, but it is the same as what was previously presented.
- The proposed future work is appropriate for this type of project.
- It appears this project is complete at this point.
- The project is nearing completion at this point.

Project strengths:

- Hexagon is a leader in composite tanks for gas delivery in the United States. An excellent company was included to do the real-world testing of tanks on the road.
- The project is doing good fundamental engineering and continuous improvement to optimize hydrogen transport.
- The project strength is the design and fabrication team.

Project weaknesses:

- There are no notable project weaknesses.

- The project seems more focused on CNG than hydrogen. There was not enough discussion of the market analysis or the cost savings. No details were given on construction, etc.

Recommendations for additions/deletions to project scope:

- In giving the presentation, the PI noted the unit capital cost for a Titan has escalated from \$500/kg to \$800/kg because of the cost of carbon fiber. Funding technologies to reduce the cost of fiber manufacturing will be necessary. There may be push-back from existing manufacturers, but if carbon fiber becomes a critical commodity to the development of fuel cell vehicles, funding in this area can make an impact by helping to drive down cost.
- It is suggested that these design advances be included in ASME Boiler and Pressure Vessel Code, Section XII (transport vessels), if viable. Additionally, it would be good to know what happens with the local authorities having jurisdiction if this pallet is removed from trailers and used as station storage without an ASME stamp.
- This project seems more like a Technology Validation or Market Transformation project than a Hydrogen Delivery project. The research aspect was not clear; it seemed to be more engineering design work.

Project # PD-022: Fiber-Reinforced Composite Pipelines

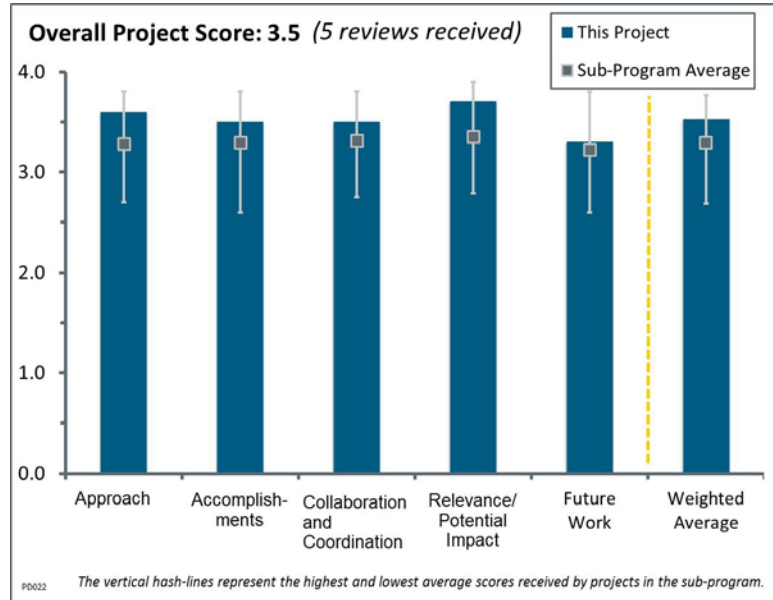
George Rawls; Savannah River National Laboratory

Brief Summary of Project:

The objectives of this project are to (1) provide data to support a technical basis for fiber-reinforced piping in hydrogen service, and (2) have fiber-reinforced piping integrated into the American Society of Mechanical Engineers (ASME) B31.12 Hydrogen Piping and Pipeline Code by 2015. Composite pipeline technology has the potential to reduce installation costs and improve reliability for hydrogen pipelines.

Question 1: Approach to performing the work

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



- The purpose of this work is to obtain data on fiber-reinforced piping for the ASME code development activities. The ASME approach—to produce an appropriate safety factor over and above non-hydrogen service using a holistic approach, start to finish, considering all processes— is the correct approach. The goal is to codify this technology. Good-quality solid data are extremely valuable to the codification process. The approach taken here is appropriate. The goal for this work is clear, the execution is on target, and progress made to completing the goal is spot on.
- This project has immense relevance for the distribution of high-pressure hydrogen to a wide variety of end users. The project team is very focused on finding and validating a commercially available product that is capable of being deployed in the near term. In doing so, the team is applying rigorous test methods and effective inspection techniques.
- The investigators are proceeding well with their work to qualify fiber-reinforced piping for use in hydrogen transportation. The incorporation of dry-wrap piping suitable for use in on-site manufacturing is a good addition to the project.
- The project clearly addresses barriers around the use of steel pipelines for hydrogen transport, including changes to the B31.12 code regulating the use of hydrogen pipelines.
- The approach is sound. It follows the standard industry practices—this is an industry with a high level of safety and a high level of intolerance for failure.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.5** for its accomplishments and progress.

- The project is building on significant accomplishments. Smart Pipe supplied a new system to the project—a dry-wrap-system-reinforced thermoplastic pipe. The burst pressure was higher than required; the burst was not at the joint but along the pipe, indicating robust joints. Permeation leaks were lower than required. The project has made progress in codification efforts, having passed the B31.12 committee—the standards committee is next—so the team is moving this along at a nice pace. Most of the comments from this committee are editorial, not particularly technical. This experience demonstrates the necessity of requiring solid data/analysis for successful movement through the codification process. This project is simply doing an excellent job to codify technologies. The team is doing its job.

- The project’s evolution is very impressive in terms of the selection and evaluation of distinct generations of thermoplastic pipe. The team is now working with a long-length manufacturer, which is very important in reducing the number of couplings (hence cost) for a commercial investment. The team also recognized the C-wrap product’s importance in terms of its ability to be sleeved by hard-shell pipe. This is innovative and has the potential to be extremely cost effective.
- Acceptance for use as part of a domestic pipeline requires inclusion in an ASME B31 Code section. The project has successfully helped the code case for inclusion of fiber-reinforced plastic piping. This is a four-step approval process. The product has passed the first step, the B31.12 Section Committee, and it has moved up to the B31 Code Committee.
- The work addresses the critical questions associated with the safe implementation of this cost-effective steel pipe alternative. The investigation of the dry-wrap pipe provides additional support for the use of fiber-reinforced piping for hydrogen transport via pipelines.
- Work with the code committee has been on time and well organized. Work to understand the dry-wrap pipe was carried out well, although it was not part of the original work plan.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- The work given to securing codification under ASME B31.12 is outstanding and satisfies the value of collaboration for a project of this nature.
- The work has been guided through collaboration with the ASME code committee, resulting in the initial stages of successful balloting. It is not clear whether the list of partners or collaborators fully represents the stakeholders.
- This activity includes several manufacturers and ASME. With acceptance by ASME, this should meet U.S. Department of Transportation Pipeline and Hazardous Materials Safety Administration regulations.
- This project works with the appropriate collaborators. This year, a new partner defined the technology for the current codification exercise.
- The work with pipe manufacturers and ASME is good.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.7** for its relevance/potential impact.

- This solution may be required for all “end delivery” of hydrogen to various use points across the country. Therefore, the relevance is immense. It also represents an outstanding economic solution that may potentially deliver value for 50 years.
- Fiber-reinforced pipe—especially on-site manufactured pipe—has real potential to lower pipeline costs. This project began as a high-risk, high-reward effort to eliminate the high costs associated with welding steel pipelines. It is nice to see it reaching these milestones.
- The hydrogen community needs to get the technologies codified in the appropriate standards/codes bodies before significant deployment can occur. This project is perfectly aligned with that need.
- The work provides the technical basis to implement a single, cost-effective steel pipe alternative.
- This has the potential to reduce pipeline installation costs.

Question 5: Proposed future work

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

- Once the codification work is successfully achieved, the team should consider playing a role in educating commercial users to rapidly adopt the technology and look for ways to improve the technology in the form of the product, installation, and/or use.
- This project is on target to get this technology codified, just about as fast as possible.

- The project is proceeding well toward completion.
- The future work appears to be focused on wrapping up the project.
- The proposed future work is appropriate for this task.

Project strengths:

- This work has a clear, well-defined goal, and the principal investigator is performing the work necessary to achieve that goal in a clean, systematic manner. It is clear that performing high-quality analysis, experiments, and measurements before approaching the code committees makes the codification process much more efficient. That is exactly the approach. The bottom line is that this work is accomplishing its goal and doing the work that is necessary.
- This is solid work that will almost surely result in access to fiber-reinforced piping as a hydrogen transport option in the near future.
- The project has strong interactions with pipe manufacturers and standards agencies.
- Industry involvement and oversight (i.e., ASME B31.12) are project strengths.
- The project's usefulness is its strength.

Project weaknesses:

- No weaknesses can be seen.
- The couplings are proprietary designs. There is the potential for mixing and matching fittings (e.g., hose to fitting or fitting to fitting). This is often an issue with non-commodity designs.

Recommendations for additions/deletions to project scope:

- The project should keep up the great work.
- More funding is recommended.
- The project should address the concern about mixing and matching fittings in a proprietary design.

Project # PD-025: Hydrogen Embrittlement of Structural Steels

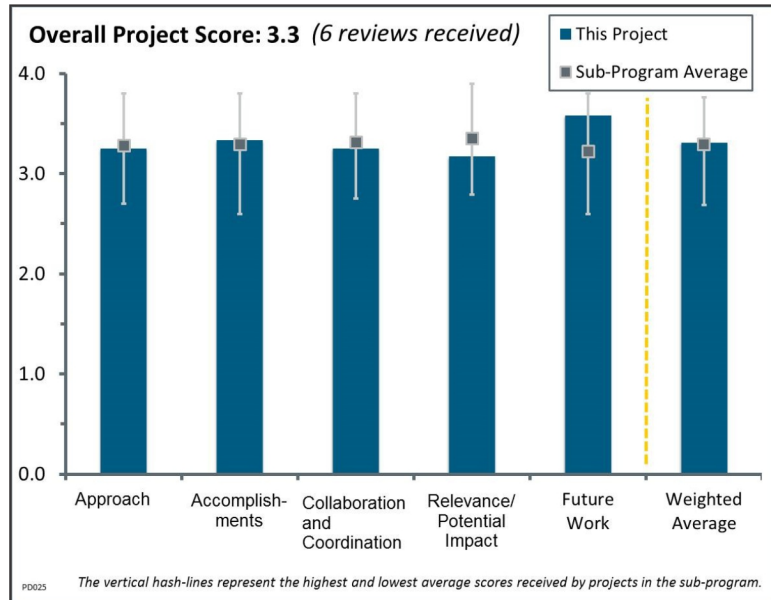
Brian Somerday; Sandia National Laboratories

Brief Summary of Project:

The objectives of this project are to (1) enable data-informed design safety factors for hydrogen pipelines, which affect both reliability/integrity and cost, and (2) answer specific questions about steel hydrogen pipelines. Sandia National Laboratories (SNL) will quantify fatigue crack growth aided by hydrogen embrittlement in pipeline steels, particularly for welds.

Question 1: Approach to performing the work

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



- The use of the established high-pressure fatigue laboratories in existence at SNL is critical for this investigation. Indeed, this capability enables the investigation of failure through fatigue crack growth rate mechanics (laws)—the appropriate analysis for this investigation. The results from this work will be published in the scientific peer-reviewed literature—nothing less is to be expected of this work. Experiments are performed in high-pressure hydrogen environments representative of service conditions. The experiments are coupled with fracture fatigue analysis to improve understanding of the experimental results. This approach provides unique, valuable data and understanding of the fatigue crack growth experience in welds. This is world-class, outstanding work.
- The approach being applied to perform this work, in the form of theoretical analysis and physical testing, appears very rational and robust. The work performed on both friction and girth welding is very impressive.
- The approach is focused on answering the critical questions relevant to the implementation of steel pipeline for hydrogen transport. The addition of the forward-looking advanced pipeline material will likely enhance long-term cost savings.
- Researchers are using state-of-the-art techniques for evaluating crack growth in steels. It would perhaps be better to do more measurements on several welded pipes to determine the variability within a single method of welding. A single weld on just one pipe may or may not be representative of that specific welding technique.
- Although the approach may be great, the presenter had difficulty communicating key concepts to the review team. Throughout the presentation, it was unclear whether the principal investigator (PI) had fed back any learnings to stakeholder groups, including the American Society of Mechanical Engineers (ASME). The approach uses perfect pre-cracks placed in the material; there did not seem to be a correlation between similar real-world crack types and what the project has been testing (or it was not stated).
- The fatigue life analysis should also be performed over a range of temperatures that correspond to possible use conditions of a steel pipeline.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- These experimental measurements were carefully performed. A residual stress analysis was performed and removed from the data, highlighting the fatigue crack growth as a function of the stress applied—this is

outstanding work. The result provided data that were then used to improve the understanding of crack growth in weld regions. They were also used to calculate an effective wall thickness based on predicted crack growth, which is a very nice addition to this work and traditional for this PI. This calculation shows the wall thickness can be reduced from traditional calculations while maintaining the same level of safety—again, this is outstanding work.

- Completing work on both validating fatigue crack growth laws and performing physical sampling facilitates the opportunity for meaningful dialogue during peer review of papers and helps to form the technical policy to guide the construction of future high-pressure pipelines. Although laborious, this groundwork will no doubt be helpful to future generations of engineers and ensure safe operation for the general public.
- A credible and effective analytical system has been assembled to assess the embrittlement of steel under exposure to hydrogen gas—a system that is backed by a credible model. The examination encompasses the pipe and the all-important seals and welds at a microscopic level.
- The completion of the friction stir weld work is critical to the implementation of the technique to minimize the cracking susceptibility associated with conventional welds. Analysis to account for residual stress is valuable to appropriately interpreting fatigue crack data and minimizing costs while maintaining safety.
- The determination of appropriate pipe wall thicknesses is a significant development that adds value to the project.
- One huge factor is out of place: all testing occurred at 295K (71°F). In real-world pipeline situations, -40°C to 40°C can be realized. In lower-temperature cases, this crack will propagate at accelerated rates, and the data presented would not be predictive. This is a very significant point to which there seemed to be no response. It is interesting that welds do not seem to be more susceptible to accelerated fatigue crack growth than the base metal. It is not clear whether there is a significant sample size to confirm this analysis.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration/coordination is spot on for this project, including the international collaboration with the International Institute for Carbon-Neutral Energy Research (I²CNER) in Japan.
- Based on comments from the 2014 DOE Hydrogen and Fuel Cells Program Annual Merit Review, the team has demonstrated expanded collaboration activities. The notably increased interaction with ASME will help ensure the work is relevant to actual pipeline operating conditions.
- There was collaboration with Oak Ridge National Laboratory, which was evident during the question-and-answer period.
- The collaborations are sufficient to enable progress. They could be used as a source for real-use conditions of pipes in pipelines to determine whether the scope of the embrittlement problem is larger than this laboratory exercise.
- It is great to see the interaction with Exxon Mobil on securing samples, but the depth of interaction with a variety of commercial leaders in the pipeline industry requires more work. The project should identify the top five leaders in the high-pressure pipeline industry (even if they do not use hydrogen) to solicit opinions of the work performed to date and recommendations for ongoing work. The project appears to be too insulated by academic thinking.
- The project had good collaboration inside the national laboratory system and with the ASME, but there has been little interaction with industry.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- It is too early to determine relevance, but the objectives of the project are extremely meaningful and fully justify the required investment. There is potential relevance in both the development of the fatigue crack

growth theory and the application of this work in the construction (materials and methods) of future pipelines.

- This work and other work on steel's compatibility with hydrogen is critical for the safe, low-cost implementation of hydrogen technologies.
- This project on steel pipes is a very necessary exercise on a conservative approach to solving the hydrogen pipeline problem.
- Steel is not a dominant cost for pipelines, so material cost reductions are unlikely to yield large reductions in pipeline costs.
- The fact that testing (for such a long project) has occurred only at 295K is concerning and not a great example of a real-world possibility.

Question 5: Proposed future work

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

- The proposed work for 2015 includes vital steps to expand awareness of the subject matter and to start a broad discussion on its relevance and improvements. The work for 2016 expands the scope to new materials and methodologies that serve to cultivate refinements and best practices. This work should be funded for decades.
- The future direction represents appropriate growth for this work. Nothing less is to be expected than getting this work into the technical peer-reviewed literature, so clearly the focus to complete refereed papers is appreciated—this is a good focus. Working with the appropriate ASME pressure committees ensures rapid dissemination into the relevant code language. This PI and group have historically had a significant impact on ASME code language; it is good to see that continuing.
- The future work contains a good combination of focus on today's implementation challenges and forward-thinking future work to maintain the long-term viability of the use of steel pipelines for cost-effective hydrogen transport.
- The fiscal year 2015 work seems appropriate. It would be great to add cyclic testing with additional temperature gradients to find out what effect temperature has on crack growth and the K factor for areas where pipelines could experience wildly swinging ambient temperature conditions.
- The proposed work is still a laboratory project. Extrapolation to the behavior of pipes in the field does not appear to have been made.

Project strengths:

- The project uses triplicate tests to get repeatable data, resulting in good consistency of the data. Calculations presented show good data for calculating wall thickness, and this will be a key factor in the overall cost of the pipeline. Such variation in thickness will make a huge impact on pipeline cost.
- Project strengths include the carefully thought-out experiments, excellent execution, clear understanding of the physics involved, and application of that knowledge in providing thoughtful analysis. The team casts the information in such a manner as to make the fundamental data immediately valuable to the community.
- Project strengths include the granular insight on the performance of high-pressure pipelines over long use and methods to ensure the community is safe.
- The project has great balance between its science and engineering aspects. Collaboration has increased.
- The PI projects competence in handling the technical aspects of the problem and is aware of the dimension of the challenges.
- The team has strong expertise in crack propagation mechanics.

Project weaknesses:

- Data are collected only at static temperature. Because there are pipelines in dynamic climates, this could be a key factor in determining the long-term costs for pipelines as a delivery method for hydrogen. Determining what the real pressure swings could be is also a key factor; it is unclear whether pipeline operators have shared this information.

- This group needs to extend the temperature operating range to be consistent with the service conditions of -40°C to +85°C.
- It is doubtful that the full range of necessary laboratory work for this project can be done at a national laboratory with this low of a budget. Such a limitation inhibits the commitment of the PI to the intellectual aspects of the project.
- The project must continue to leverage various facilities to perform the needed number of tests for implementation.
- More dialogue with commercial players is needed.
- The work is unlikely to yield large reductions in pipeline costs.

Recommendations for additions/deletions to project scope:

- The project should keep going.
- This laboratory really needs to develop a temperature capability to enable temperature ranges consistent with service conditions of ambient down to -40°C and SAE International J2601 conditions of up to +85°C.
- More pipe samples should be examined to account for the variability in welding procedures.
- A credible attack on this problem requires expansion of the laboratory work, with a concomitant increase of resources to support such an expansion.

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

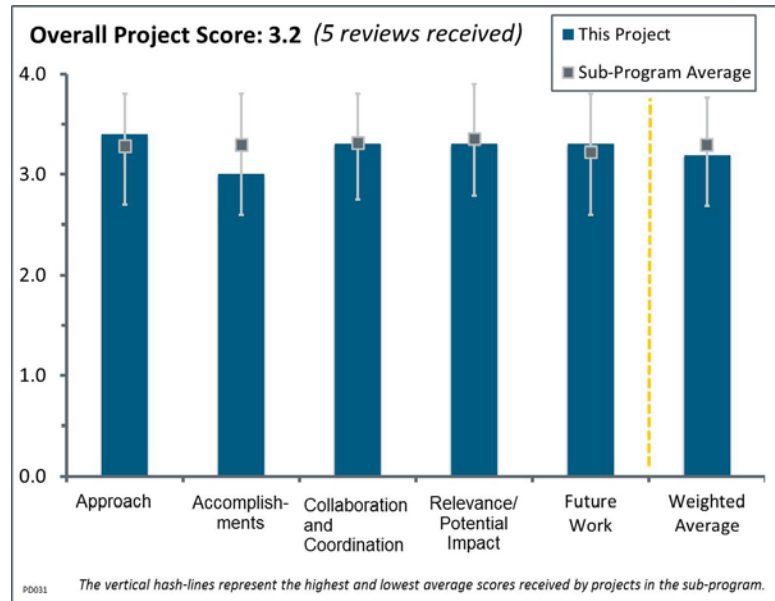
Mike Peters; National Renewable Energy Laboratory

Brief Summary of Project:

The objectives of this project are to (1) provide independent performance testing of advanced electrolyzer stacks and systems for the U.S. Department of Energy (DOE) and industry; (2) develop electrolyzer stack and subsystem components and optimize performance using grid and, especially, variable renewable energy; and (3) leverage large active area stack testing platform and balance of plant (BOP) to develop system efficiency improvements.

Question 1: Approach to performing the work

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



- The project utilizes experience and facilities developed over a number of years. The barriers addressed are the degradation of polymer electrolyte membrane (PEM) electrolyzer stacks under variable loads and the reduction of hydrogen loss due to drying. The simultaneous testing of stacks under variable and constant loads is the ideal testing environment for such an analysis; it is a pity that the stacks did not have the same run times at the start of the experiment. The tests on different approaches for drying the hydrogen are well planned but hampered by the accuracy of the dew point sensors.
- The project evaluates PEM electrolyzer performance in critical applications (wind and solar to hydrogen) with attention to the necessary details for high-fidelity results. An ancillary benefit is that it also provides hydrogen for NREL efforts on the hydrogen delivery front.
- The approach to the effort was well organized and well thought-out with DOE targets clearly in mind.
- NREL is working with several industrial partners to provide independent testing of advanced water electrolyzers and integrated systems coupled to grid and renewable power sources. The work includes evaluation of several hydrogen drying technologies based on pressure swing adsorption (PSA). This is primarily a development and demonstration project, rather than a basic research project.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The project team has made sound choices regarding the focus areas: the impact of intermittency on long-term performance of electrolyzer stacks and how to minimize loss of hydrogen in dryers.
- Electrolyzer stack performance and degradation were evaluated using three Proton OnSite stacks under constant and variable-power operating conditions over long test periods (i.e., 5,500 hours for two of the stacks and 2,500 hours for the third). The effect of variable operation on degradation is not completely clear from the results, but it does not appear to have a significant effect on the degradation rate. The project has developed the ability to simulate typical variable-demand profiles to simulate wind and photovoltaic power sources. PSA dryers were evaluated with the goal of optimizing the electrolyzer BOP under variable operation conditions. Dryer performance was evaluated using dew point sensors. The target moisture of the hydrogen product is <5 ppm.

- The project contributes to the DOE target of increased hydrogen from renewable energy sources. The stack decay rate was measured to be almost the same under constant and varying loads. Alternative drying processes still need to be verified; however, the facility is well designed.
- The effort to improve electrolyzer efficiency by optimizing electrolyzer BOP operation under variable conditions seemed to have merit; however, the approach was not adequately outlined in the presentation.

Question 3: Collaboration and coordination with other institutions

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

- This is somewhat of a stand-alone effort, but existing relationships with PEM electrolyzer providers should serve to strengthen that market.
- Partners include Xcel Energy (providing an ongoing wind-to-hydrogen demonstration project); Proton OnSite (providing 40 kW and 120 kW electrolyzers and a drying skid); and Giner, Inc. (providing 150 kW PEM electrolyzer stacks). The collaborative efforts with these companies appear to have been quite close and mutually beneficial.
- Collaboration is limited to project partners. The project also interacts with NREL's Integrated Network Testbed for Energy Grid Research and Technology Experimentation (INTEGRATE) project.
- The issues with the dew point sensors seemed to resonant with the audience. However, the lack of collaborations and coordination with experts in this field is concerning. Clearly, the data collected from these critical instruments affects resultant conclusions, etc. It is recommended that subject matter experts be brought in to address this apparently long-term problem.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The project greatly supports advances toward increased penetration of renewable energy technologies in the energy system, aiming at the storage of excess energy from renewables.
- Electrolytic hydrogen production can be coupled with renewable energy (wind and solar) for hydrogen production. This technology can also support grid stabilization with increasing penetration of renewables. Because it is based on relatively mature technologies, this work has the potential to have a significant short-term impact on the integration of renewables into the energy mix, as well as on hydrogen production for use as vehicle fuel or for other applications.
- The project supports DOE's goal of lowering the cost of hydrogen, in this case by increasing efficiency and lowering costs for the existing electrolysis approach. These efforts are not likely to have a huge impact on the final cost of hydrogen, but the value of the project compared to its funding is likely high for this type of research.
- The project seems to have a near-term potential impact with regard to hydrogen savings, with logical approaches.

Question 5: Proposed future work

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

- Additional long-duration stack testing under variable-power operation and dryer testing will be completed, as well as evaluation of additional BOP improvements, such as heat recuperation. The proposed future work is a logical extension of the work completed to date.
- The future work is well planned.
- The project identified a couple of opportunities to evaluate in terms of reducing BOP costs.
- It is important to find other opportunities to use the electrolyzer BOP to improve system efficiency. A clearer message with respect to potential avenues would have been helpful. It seems that using a cooling system and H₂O dropouts to improve dryer efficiency should have been an initial focus; however, it is nice

to see it called out for future work. Capturing heat to warm up hydrogen sweeping gas to lose less hydrogen due to drying also seems logical. Finally, the effort to continue long-duration testing, comparing stack decay rates for variable- and constant-power operation, needs better fidelity with respect to expected outcomes.

Project strengths:

- Project strengths include a high level of short-term relevance for hydrogen production from renewable energy with demonstrated load-following ability and grid stabilization potential.
- Project strengths include the use of existing facilities and established expertise over a number of years. Continuous support from DOE is vital in expanding the facilities and knowledge generated, which is valuable to the United States and the international community.
- This project helps to move electrolysis further up the S-curve by improving efficiency and reliability.
- The overall organization of the project is one of the main strengths of this work.
- This project seems to have a good test facility.

Project weaknesses:

- The lack of outside expertise seems to be the one potential weakness.
- It is unfortunate that not all three stacks had the same age (i.e., operating hours) at the start of the project.
- The project has a small stack sample size for the determination of the effects of variability.
- The wind data used appears unrealistic.
- The project is not likely to dramatically lower the carbon footprint of hydrogen.

Recommendations for additions/deletions to project scope:

- Additional evaluations should be performed to determine the effect of load variability on long-term electrolyzer performance.

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

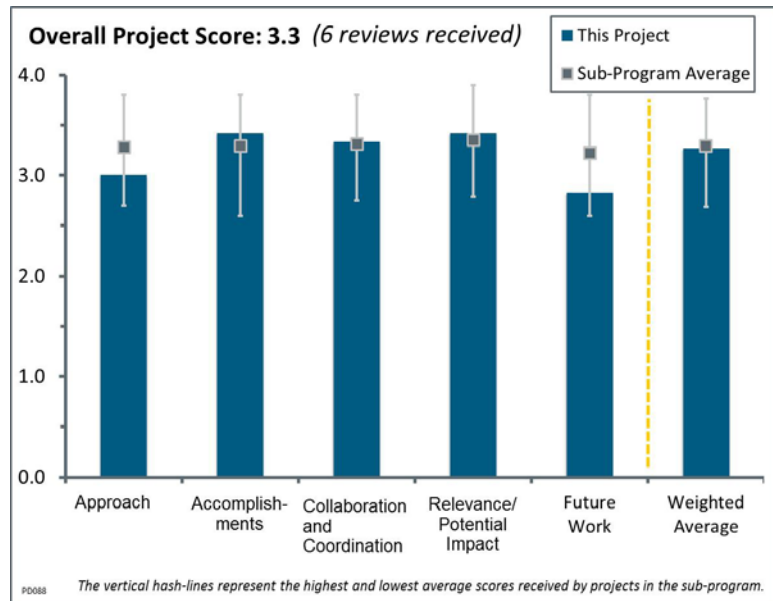
Zhili Feng; Oak Ridge National Laboratory

Brief Summary of Project:

The overall objective of this project is to develop and demonstrate the novel steel/concrete composite vessel design and fabrication technology for stationary storage systems of high-pressure hydrogen that meet the U.S. Department of Energy (DOE) technical and cost targets. Oak Ridge National Laboratory will address the significant safety and cost challenges of the current industry standard steel pressure vessel technology.

Question 1: Approach to performing the work

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



- There is intriguing integration of exiting technology with attention to cost.
 - Embrittlement-susceptible, high-tensile-strength steel is used by engineering around the embrittlement issues; the design reduces the pressure to values low enough to eliminate embrittlement concerns before the high-tensile-strength steel sees the hydrogen. This, effectively, is engineering designed leaks. The permeation leaks are below acceptable levels, and the exposed steel sees hydrogen at pressures at which embrittlement is not a concern. This is nice as long as it continues to work over the lifetime of the tank. The question was posed and addressed by the principal investigator (PI), but it would be good to see a formal response presented as part of this review.
 - The design is a bit sophisticated, making the manufacturing complex and possibly labor-intensive. Cost control is one of the goals, and the predictions are encouraging. It is clear that as this project continues, attention to manufacturing costs will remain high on the list.
 - This concept seemed, at first glance, to be a direct burial tank, in which case the tight contact with the surrounding ground would take up part of the load (if correctly designed and implemented). However, that is not the case; this is a stand-alone tank. This begs the following:
 - The concept is to use pre-stressed concrete. In this concept, the concrete is pre-stressed by steel wrappings. Concrete by itself has effectively zero tensile strength; it is great in compression but not in tension. Pre-stressed concrete, however, can handle tensile loads as long as the tensile load does not exceed the pre-stressing from the steel wrappings. In other words, the steel takes the load of pre-stressing and the pressure load of the hydrogen inside the vessel. It seems that the concrete is not necessary; indeed, this system would be a lot simpler and less costly without the concrete, and presumably the steel component can be reduced owing to the lower tensile loads.
 - This project has done a nice finite-element method (FEM) analysis, so presumably the case with and without concrete from a balance-of-plant and cost-optimization perspective has been performed. It is requested that the PI address this point in rebuttal.
- The project addresses hydrogen storage at low cost. Economic hydrogen storage in relation to tube delivery cost is an important target for the DOE Hydrogen and Fuel Cells Program, and the project is well positioned to address the relevant roadblocks. Collaborative work with industrial partners brings to the project valuable requisite expertise, such as Ben C. Gerwick, Inc., for pre-stressed concrete and Hanson Pressure Pipe for the construction of the outer concrete reinforcement.

- This is a unique approach to solving a challenging problem. There is an open question on how the small amounts of hydrogen released in the vent hole could affect the welds and materials in the micro-gaps, which causes some doubts. Likely a simple analysis could be completed to calculate the partial pressure of hydrogen experienced in the micro-gaps.
- This project has been well executed and planned. The approach of combining low-cost materials to address the cost and performance of bulk storage of hydrogen is unique. The absolute necessity of the concrete layer remains unconvincing, and the properties of the concrete used in this case should be well known, as significant degradation can occur in concrete structures that could have an impact on the vessel strength.
- As for the approach to performing the work, the project team presented a good plan and seems to be developing effective manufacturing techniques. The project is technically interesting (multi-layered vessels, stressed concrete, manifolding), but the project's value remains in question. It seems there are several proven methods to store high-pressure hydrogen below the surface at equal or lower cost. The reason for continuing to develop this concept is unclear.
- The team is making good but slow progress.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The prototype mockup of the 90 kg tank is complete. The American Society of Mechanical Engineers (ASME) hydraulic-static test is also complete—which is a major milestone! The project will be completing cycling testing in hydrogen, which is good progress. FEM analysis has been completed to understand the stress profiles and optimize the design—this is good, solid engineering. Strain gauges were imbedded into the mockup tank and were used to ensure the tank was not overstressed during construction and initial cycling. These data need to be used to compare with the FEM calculations for model validation. This will enhance confidence in the FEM and allow it to be used in further analysis and optimization. An invalidated model is not nearly as useful as a validated one. When a question about this was posed, the PI responded that this validation is planned—which is good. This is an excellent set of accomplishments.
- Significant progress toward the final project objectives has been made. The storage structure has been completed, and concerns about hydrogen leakage and safety have been addressed according to the design concepts (vent holes) underlying the project. The project has manufactured a representative prototype vessel with appropriately placed strain gauges to test, in the near future, the real-world performance of the actual vessel; this is a significant accomplishment. Individual components have been analyzed through detailed FEM analysis to optimize design, and the scheduled testing against hydrocracking is a reliable way to study the structure's integrity under severe pressure loading. In summary, the project is well beyond the construction stage, and sound future testing procedures are in place.
- The design and mock-up of the sub-scale prototype is a significant accomplishment. It will be very interesting to see the results from testing the mock-up.
- Technology demonstration is impressive. It appears that the work will provide a successful evaluation of this technology.
- The team is making good progress with this novel technology.
- The accomplishments and rate of progress are undeniably good, but there is no effort to assess competitive environment to determine if there is value in the accomplishments and progress.

Question 3: Collaboration and coordination with other institutions

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

- Industrial partners listed on slide 19 are all relevant and chosen to assist with construction of the individual components of the project. The list of partners is comprehensive and addresses all needed expertise.
- For the most part, the collaborations and coordination between partners are appropriate. While it may be a bit late for this project, adding an expert in concrete matrices is suggested, as there is significant expertise at the national laboratory level.
- The collaborators and coordination for this project are appropriate and comprehensive.

- Collaboration is appropriate for the work.
- The team is collaborating well with key technology partners.
- The majority of the collaboration is focused on technologies integrated into the project. There appears to be little effort with external organizations that are in better positions to offer insight on the project's relevance. This effort has a likeness to historical projects that were technically amazing but commercially (or militarily) of little use.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- There is definite need for high-pressure storage systems, and current costs are too high. The susceptibility of carbon steel to hydrogen attack and low-cost structural materials makes the proposed technology very appealing.
- The project advances DOE's objectives on stationary storage at refueling stations. The significant progress toward the project completion enables DOE's plans for the introduction of the fuel cell car.
- A low-cost hydrogen storage solution is needed—if successful, this technology will be valuable to the deployment of hydrogen technologies.
- This will address an immediate cost challenge associated with hydrogen storage.
- An inexpensive bulk storage technology that can be relatively easily manufactured would have a high impact on the development of hydrogen infrastructure. It is not clear that this technology can be scaled very easily for higher manufacturing rates.
- The project team seems more interested in technology integration than relevance. No effort was made to address the cost of fielding units or the logistics of delivering a unit or whether the unit can be relocated.

Question 5: Proposed future work

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

- Proposed future work is appropriate. The cycling with hydrogen will be expensive but is necessary. The project should run optimization on structure, with and without the concrete, and run optimization on cost, with and without the concrete. The model validation should be completed.
- Cyclic testing will be interesting. There is not enough time or funding remaining in the project to pursue other efforts, and cycling is the most appropriate. It would be good to cycle this vessel for a few years to develop a base case, and this could probably be achieved with a relatively small amount of funding.
- The proposed testing of the mockup vessel to assess performance under cyclic hydrogen loading is of crucial importance to the validation of the concept and overall design of the structure. In particular, the proposed testing can be used to ascertain the economics of the design, but no such plans have been mentioned. It is unknown whether there is a plan to revisit the design of the structure if the vessel performs well under hydro-testing but poorly under cyclic loading.
- The results of the proposed cycle testing will be very informative.
- The project appears to be in the wrap-up stages.
- The proposal for future work is weak. It seems focused on testing the mockup and building a forecourt installation. There has been no effort to evaluate the economics, no comparison to competing technologies, and no thought to evaluating the logistics of employing the technology.

Project strengths:

- Contributions from the industrial partners ensure appropriate expert input to the project execution. Strict adherence to the ASME codes for the construction of the inner steel vessel and the planned hydro-testing are both indicators that the project advances according to the available codes and standards for components operating in hydrogen. The mockup vessel is a positive step toward testing the safety and reliability of the design.

- The concept is novel, and the design approach and execution are excellent.
- The proposed technology offers hope of low-cost storage.

Project weaknesses:

- It seems possible that the codes and standards for implementation for use with hydrogen have not been fully explored.
- It is not clear why the combined steel/concrete structure reduces the cost of the overall design and ensures the safety of the structure.
 - There is no comparison with alternative designs to ascertain the role of the presence of the concrete layer. For instance, the cost of the pre-stressed concrete in relation to an outer vessel made of low-cost steel is not listed. In fact, the very role of the concrete in securing that embrittlement of the layered high-strength structural steel was not clear in the presentation. For instance, there is no study as to why load shedding on the concrete layer increases the layered structural steel's resistance to hydrogen embrittlement.
 - Slide 11 shows contour plots of S33 stress in MPa, whereas the applied pressure is given in psi. Slide 12 shows hoop stress magnitudes in ksi. This is an inconsistent use of units in the same project and the same presentation.
 - The reported stress values for S33 on slide 11 would have provided more value if information was given as to what constitutes acceptable stress levels in the concrete.
 - On slide 12, hoop stresses are reported in comparison to the code-allowable stress limits. The question is whether these are allowable stresses for the inner steel vessel relevant to hydro-testing or to hydrogen embrittlement. There is no mention as to how the inner steel is designed against embrittlement. More specifically, it is not clear in what ways the calculated stress magnitudes help with understanding the resistance of the inner steel against embrittlement.
 - The entire design is based on the plot shown on the right-hand side of slide 5—namely, that there are no substantial amounts of hydrogen in the layered structure because of the presence of the vent holes. Despite the fact that the amounts of hydrogen in the layered structure are insignificant, the steady-state profile in the inner stainless steel layer cannot be dismissed. There is no evaluation of the role of hydrogen in the inner layer.
- The weight of the storage systems seems to be too high for large tanks to be shipped to site, and the manufacturing techniques look too complex for site work.
- This overall approach is to be questioned. It is not at all clear that the use of concrete in this configuration has any value added. If, however, this could be a direct burial tank—in contact with the earth to take off some of the load, or the entire load—then this would be fine.

Recommendations for additions/deletions to project scope:

- The investigators are asked to address questions about the role of the stainless steel liner as opposed to another solution—for example, a coating on the layered steel shell could be used as a barrier to hydrogen egress. Such an alternative solution can help reduce the cost of the entire vessel significantly. The role of the concrete layer is also unclear. There is no convincing evidence that failure of the layered high-strength steel shell will be stress-driven in the presence of hydrogen. Even if failure is stress-driven, the layered shell can be constructed to bring the stress magnitudes below critical levels. No study was presented on the basis of the techno-economics of the layered shell's performance in hydrogen.
- The project should ensure that there is a thorough understanding of the codes and standards, with an emphasis on use in hydrogen.
- The project should be limited to an economic and logistic evaluation in comparison to competing technologies. Putting more money into testing seems to have little value.

Project # PD-096: Electrolyzer Component Development for the Hybrid Sulfur Thermochemical Cycle

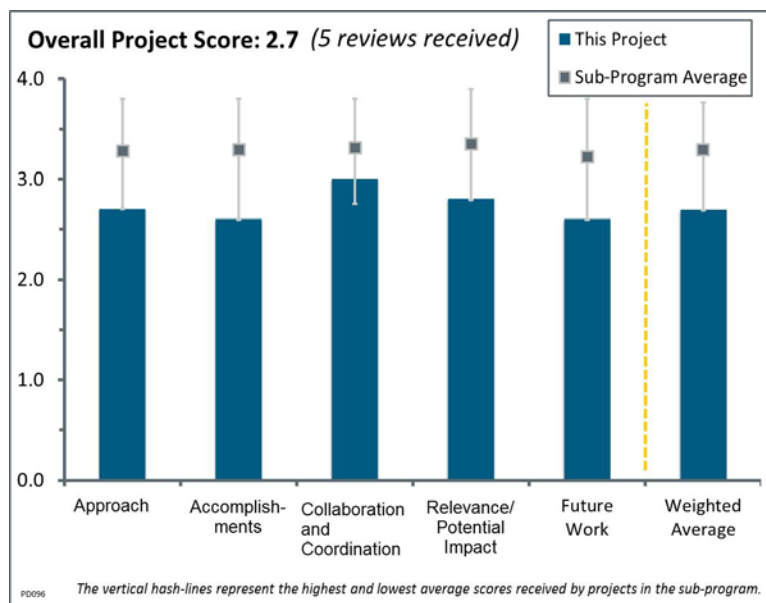
William Summers; Savannah River National Laboratory

Brief Summary of Project:

The objectives of this project are to (1) develop highly efficient process designs for coupling the hybrid sulfur (HyS) thermochemical process with a concentrated solar energy system, and (2) demonstrate sulfur dioxide (SO₂) depolarized electrolysis using improved electrocatalysts and high-temperature (HT) proton exchange membranes (PEMs) that permit high-efficiency hydrogen production.

Question 1: Approach to performing the work

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



- The objective of this project is to develop and demonstrate improved component technology for the HyS thermochemical cycle, coupled to concentrated solar energy for the HT process heat requirement. The electrical energy needed for the electrolysis step can be provided by the grid or photovoltaics. The HyS process was originally developed by Westinghouse for coupling to nuclear energy (HT reactors) for large-scale hydrogen production. The HyS cycle includes an electrolytic step operating at ~120°C and a HT (850°C) H₂SO₄ decomposition thermochemical step. The electrolyzer uses a PEM-based Nafion-membrane electrolyte. Precious metal catalyst (Pt) is required on the SO₂-sulfuric acid side of the cell. Required cell voltage (~.6 V) is much lower than for water electrolysis owing to SO₂ electrode depolarization. Activation overpotentials associated with oxidation kinetics at the SO₂ electrode represent the largest magnitude (70%) loss mechanism for these cells. Testing is being performed at small scale (button cell and single-cell). Improved membranes and catalysts are needed. Cell lifetime is also an issue. Fiscal year (FY) 2015 objectives include system design and cost analysis. The conceptual system includes a HT solar receiver and thermal energy storage. A HT (130°C) high-pressure (1 MPA) button cell test capability will also be developed.
- The project approach to developing the HyS electrolyzer system itself is very good, but the merits of continuing this project are in question because it appears to depend in on a very low probability of reducing the cost of solar thermal heliostat and tower equipment. A brief literature survey indicates that the current cost of just the heliostat and power tower equipment is on the order of \$4,352/kW of solar thermal energy. Assuming an annual capacity of 25% for the solar field and an annual capital recovery factor of 12%, then the price of hydrogen would have to be \$7.94/kg just to recover the capital cost of the heliostat array and power tower. Thus, even if the HyS system were free, the price of hydrogen would have to be over \$7.94/kg, or far above the project team's estimate of \$3.13/kg to \$4.23/kg. (It has been said that the U.S. Department of Energy [DOE] has instructed the principal investigator to assume very low-cost solar thermal collection systems developed under the "Sun Shot" project, but this is such a major ingredient in the overall hydrogen cost that the project should address the likelihood of achieving the Sun Shot goals for heliostat field costs.)
- The approach in this project is fair. Whereas it is generally agreed that the electrolyzer is critical to implementing this hybrid approach to hydrogen production, it is also true that integrating electrolysis with solar thermochemical processes is inherently complex and probably cost-prohibitive. The primary thrust of the work is development of the electrolysis unit and therefore consistent with priorities defined under earlier evaluations. At the same time, it is important to recognize that this effort has a long history and has

received significant funding, yet the enabling technology is still undemonstrated. Project concept design investment is insufficient to assure this reviewer that solar integration can be implemented in a way that supports the long-term program goal. In that regard, the project is inadequately integrated with outside efforts with far greater experience and knowledge in solar energy collection and use.

- The project team has identified the areas that need development, including solving the diurnal operation issue and improving the electrolysis step. It is not clear that the approach for improved membranes will work. It seems the team's favored membrane to replace Nafion is polybenzimidazole (PBI). However, PBI has well-known stability issues and challenges in consistent manufacturing, even from large, well-known chemical manufacturing companies.
- Stability and crossover attenuation using sulfuric acid-doped polybenzimidazole (s-PBI), and other membranes, are not known, and the reactor and project design rests on the assumption that such a membrane exists and/or can be made by collaborators.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- A flowsheet investigation was performed to evaluate the relative merits of vapor feed to the anode versus liquid feed (liquid feed is preferred). Solar receiver/acid decomposer options were evaluated as part of the process design activity. Three options were considered: direct solar heating of an acid decomposer, bayonet (indirect), and a falling particle receiver with thermal energy storage. The falling particle receiver and the bayonet decomposer were selected as baseline options. Electrolyzer development and testing included construction of a pressurized button cell test facility and quantification of the effect of pressure and temperature. Other HT membrane candidates will be tested. Progress over 2014 accomplishments has been modest.
- The technical accomplishments on the HyS system itself are good.
- The presenters reported significant improvements in sulfur cross-over reduction, membrane stability, and 1000-hour operation, but no data were shown to support these findings. The presenters need to show the data. The team never down-selected to a design; the design they presented can be either thermal storage or chemical storage. This option may have an impact on the cost analysis. It is not clear whether it did have an impact on the Hydrogen Analysis (H2A) results because there was not enough information about what was assumed for the H2A analysis to tell. It seems that the majority of solar HyS efficiency improvements are from work being done by the solar program, not work being done in this project. There was not enough detail provided on the H2A analysis to evaluate it. The capital cost assumptions (solar field size, heliostat costs, materials costs, etc.) are not included. Details on the operations and maintenance assumptions need to be included. It is unclear that the falling particle receiver design can achieve the 850°C operation required for the H₂SO₄ decomposer, especially if the project team plans on thermal storage to enable 24–7 operation.
- Accomplishments and progress of this project are poor to fair. This evaluation is based on historical funding for a far simpler embodiment of the HyS process in which the project team had to deal with only electrolysis and thermal decomposition of sulfuric acid. The acid decomposer was designed, developed, and demonstrated by another group, so the present team dealt only with the electrolysis process, and that process is still inadequate (although improved) to support plant-scale operations. Incorporation of solar power in lieu of nuclear power greatly complicates thermal interfaces so that the acid decomposition process might be different and therefore remains a barrier to efficient implementation.
- Very few HT studies have been reported to date, and the milestone that requires them is fast approaching.

Question 3: Collaboration and coordination with other institutions

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

- The University of South Carolina (USC) (self-funded) is developing s-PBI membranes for HT pressurized operation. The German Aerospace Center (DLR) is participating in the system analysis task with a focus on the solar interface for direct heating of the sulfuric acid decomposer.

- The project seems to have a good collaboration with DLR and USC. While the presenters mention Sandia National Laboratories (SNL) for the falling particle receiver design, they do not mention SNL in their collaborations. Perhaps they just use literature data from SNL, but that is not clear. More collaboration with SNL on the solar receiver and solar field design is recommended, especially for the H2A cost analysis.
- The project team is good.
- Project plans indicate baseline selection of the falling particle receiver (FPR) for solar interface design. This concept has experienced some development and testing at SNL, but at-scale performance testing has not been done. Heat transfer and material durability issues accompany thermal exchange from hot particulate thermal media to process heat applications, and these have not been tested or demonstrated. Partnering with DLR for on-sun testing by direct solar heating for sulfuric acid decomposition is value-added to the project but initiates a new and untested approach to implementation of the acid decomposition process. The FPR work will require partnering with SNL. The inventory of collaborations does not include this essential element. Collaboration and coordination is poor.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.8** for its relevance/potential impact.

- Low-cost renewable hydrogen is a needed resource. This project addresses this area. The potential impact is significant. The cycle is lower-temperature than many of the other cycles currently being investigated. It does have an electrochemical element, which means that it will need electricity. The project team currently proposes to generate electricity on-site. However, for 24–7 operation, off-site (grid) electricity would be required.
- The HT cycles have the potential to achieve higher efficiency than conventional water electrolysis. Integration with concentrated solar energy is attractive for renewable hydrogen production, but it represents a significant technical and cost challenge. Considering the technical challenges associated with the electrolytic and thermochemical steps in this process, and the coupling with concentrated solar energy, possible deployment of this technology is very long-term.
- Relevance of this work is good to excellent. Potential impact is poor to fair. Potential impact assessment is based partly on the complex (and probably costly) nature of hybrid processes. The assessment is also based partly on the project history of funded support and still-inadequate electrolysis performance. Finally, potential impact is degraded by undertaking research and development on a new and untested acid decomposition process.
- The potential impact of this project is doubtful, given the dependence on an extraordinary reduction in heliostat field costs required to make this project economically viable.

Question 5: Proposed future work

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

- The proposed future work within the realm of the HyS technology seems reasonable.
- Proposed future work includes continued button cell testing with a focus on membrane and catalyst development. Flowsheet and cost analysis will also continue.
- The presenters said in their response to reviewers that the PBI membrane had already been shown to be stable. However, the presenters have long-term performance testing of the PBI membrane in the future work section. Either this test is redundant, or they have not performed long-term testing. They need to clarify their comments. Long-term testing of the membranes is needed. They have a performance goal of 600 mV at 500 mA/cm², but there is no pathway to achieve this. The data shown in FY 2015 seem to indicate that the project is a long way off in performance and that the goal of 100 mV improvement will not achieve the performance target of 600 mV. It is important for the catalyst testing to be done on real membrane electrode assemblies (MEAs) in real systems and not just rotating disk electrode testing. The team needs to complete the H2A analysis and provide details to DOE and the reviewers. The team members have yet to demonstrate an MEA that can achieve the performance goal of 600 mW at

500mA/cm², but they are already focusing on lowering the catalyst cost. It seems they should first get something that can meet their needs and then lower their costs. They also need to determine whether the catalyst cost is indeed a significant factor in the H2A analysis.

- Future plans are poor because the vast majority of work is directed to the electrolysis process, and no specific work is directed to resolve the acid decomposition process testing, evaluation, and selection. It is likely that this work breakdown reflects the skills and interests of the project direction, further underscoring the need for better collaboration with the solar power and solar thermochemistry communities.
- There is no clear contingency plan in the event that a higher-temperature stable membrane cannot be identified.

Project strengths:

- This is an excellent team, well-versed in the HyS process dating back to earlier times when Savannah River National Laboratory was assuming nuclear power would drive the process.
- The project has a good team with a long history of working in this area.
- Strengths are in facilities and membrane electrolysis testing and evaluation.
- Progress has been made on development of the sulfur-depolarized electrolysis membranes and catalysts, but significant performance challenges remain. Overall system analysis, including coupling to concentrated solar energy, has been performed. The project is addressing the electrolyzer issues in a logical fashion, with a go/no-go decision point of September 30, 2015.

Project weaknesses:

- The technological challenges associated with the HyS concept are very significant. Oxidation kinetics must be improved. Precious metals are required for catalysts. PBI is difficult to manufacture in large quantities and has shown large degradation. Corrosion is a significant issue for all process components that come into contact with the sulfuric acid solution. The electrolysis step has been demonstrated only at small-scale (single-cell or button cell). Additional challenges will be encountered with stack-level operation.
- The PBI membrane stability needs to be demonstrated. Simply saying it has been done is not enough; the data need to be presented. The team is focusing on lowering the cost of catalysts. It is not clear that this is the main cost driver. The project needs to provide more details on the H2A analysis.
- The project is weak in areas of solar power analysis, interface design, and thermochemical acid decomposition.
- Success depends on the drastic reduction of heliostat field costs, which seems unlikely.

Recommendations for additions/deletions to project scope:

- Because the cost of the hydrogen system is so dependent on the cost of the heliostat field and solar tower, the project should add a task to determine the heliostat cost reductions needed to approach the \$3.13/kg to \$4.55/kg hydrogen costs quoted in this presentation. Also, the project needs to estimate the roadblocks to achieving the very low price for the heliostat field and the likelihood that the solar thermal developers will overcome those roadblocks.
- Stack operation of the SDE must be demonstrated (it is recognized that this will require additional funding).
- This project should be terminated.

Project # PD-101: Cryogenically Flexible, Low-Permeability Hydrogen Delivery Hose

Jennifer Lalli; Nanosonic

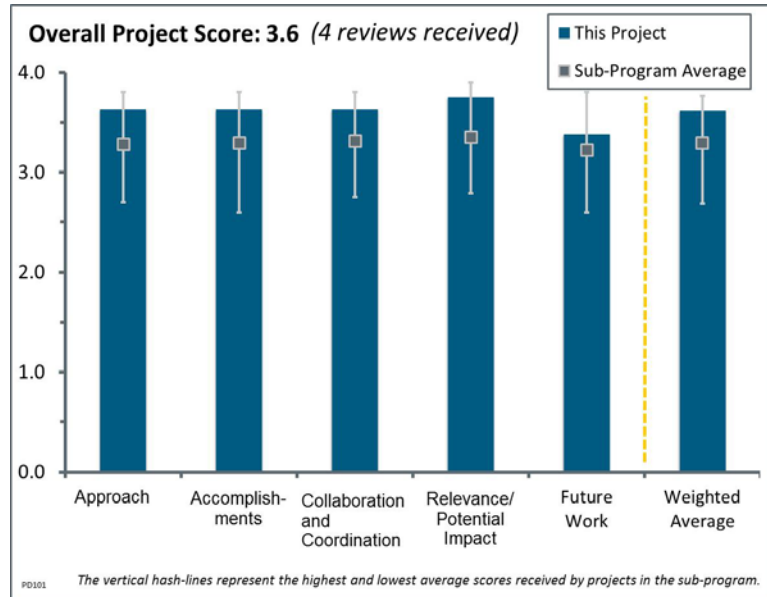
Brief Summary of Project:

The objectives of this project are to (1) develop a flexible dispensing hose to enable hydrogen delivery at <\$2 per gasoline gallon equivalent; (2) demonstrate reliability at -50°C and 875 bar for H70 service; and (3) optimize ruggedness, cost, and safety for 70 fills per day and for more than two years.

Question 1: Approach to performing the work

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

- This project is well focused on program targets with well-defined go/no-go tests. The team is very focused on achieving the technical milestones needed to make this product successful. For example, the end connectors proved to be a significant challenge that, if not solved, would have been a showstopper. The project focused its attention on solving this problem and successfully did so. This is a well-thought-out, well-executed project. The team members have chosen their partners well to complement the overall team.
- The project has an excellent approach to designing a high-pressure, cryogenic hose based on a thorough understanding of the polymer chemistry required to develop a polymer to operate effectively in this regime. Both chemical and mechanical properties are understood and effectively modified to meet the barriers.
- The approach is unique. The use of nonmetal reinforcement materials for the expected pressures is novel. The comments previously supplied through the technical teams appear to have been addressed.
- The project has done well integrating suggestions from the U.S. Department of Energy and United States Council for Automotive Research Hydrogen Delivery Technical Team.



Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.6** for its accomplishments and progress.

- The team has made excellent progress. This is an exciting technology about halfway through its development cycle. The team has all the appropriate metrics in sight, and it is solving critical problems. The current material appears to have the temperature range, predicted pressure range, tolerance to solvents and impurities, abrasion resistance, and minimal to zero outgassing. This looks pretty good.
- The accomplishments to date are interesting. Offgassing, leaching, and particulate formation may need to be addressed. The National Renewable Energy Laboratory (NREL), Los Alamos National Laboratory (LANL), University of Hawaii, and University of Connecticut are currently set up to test the effects of these compounds on polymer electrolyte membrane (PEM) fuel cells.
- This is a Small Business Innovation Research (SBIR) Phase II project that has made tremendous progress in developing a non-volatile, low-Tg polymer that will survive cryogenic and high-pressure service. The inability to effectively crimp the metallic connectors could be a showstopper for this material; however, the ceramic bonding agent is a creative idea and could hold promise.

- In regard to the progress in solvent resistance (slide 9), it is not clear whether Nanosonic investigated what companies owning and maintaining hydrogen stations (e.g., bus, passenger car, forklift fueling) typically use for cleaners and solvents during and after maintenance or after modifications in systems before hydrogen enters the hose for dispensing (this should be considered in addition to Canadian Standards Association [CSA] Group directions, which may not always be followed). Taking crimp manufacturing/application of the hose in-house was a strong decision, especially in combination with finding innovative ways to adhere/apply crimp to the hose. When doing the projected cost and cost savings comparisons, it is unclear whether Nanosonic considered that the German competitor may have included a significant margin on this product that results in the current pricing in U.S. and German markets. This can be expected to change when more competitors enter the market. Otherwise, the project has promising cost savings.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration/coordination is spot-on for this project.
- The company has done a great job collaborating and coordinating with different entities, such as CSA Group, NREL, New England Wire, Lillbacka, and others. It might also be good to start coordinating with the California Fuel Cell Partnership because California is leading the way in station build-out.
- The current collaboration and coordination are impressive. Working with a hydraulic hose supplier might help with hose end connection design selection. The hydraulic hose supplier would have the same problem and same pressures (i.e., above 300 psi) but different fluids.
- It is not clear whether it is realistic to account for complete county contribution at amounts (\$1.1 million and \$7.5 million) stated toward a DOE hose project. The New England Wire company is a good collaborator in case the project has to hand off manufacturing hoses instead of doing it in-house. It is good that the project is thinking with the future in mind.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.8** for its relevance/potential impact.

- There remains a serious gap for the safe, cost-effective deployment of hydrogen fueling station (HFS) hoses. This project is targeted to fill that gap. This makes it spot-on for relevance and, when solved, will give the United States a hose manufacturer in this market, which will have a huge impact.
- This activity is highly relevant to the hydrogen infrastructure and has the potential to make a major impact for this effort.
- With comparison to other 70 MPa hose manufacturers in mind, this project has the potential to add a competitive U.S. manufactured product to the market—a much needed (currently missing) capability.
- While a flexible hydrogen delivery hose is a necessary component, it is not a critical component because there are composite delivery hoses that are used in high-pressure hydrogen service today. However, this particular project could show the potential to lower the cost of the hose component and could extend the service life, which could be impactful.

Question 5: Proposed future work

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

- The future work is perfect to move this project along and demonstrate success on all remaining technical milestones. The predictions look very encouraging now. The systems need to be validated by measurements. The proposed future work aligns well for this need.
- The future work addresses the problems, but resolving the issue with crimping connectors is the key issue for this project at this time.

- Future work has a good focus on hose durability and quality. It is not clear how 55 gal and 200 gal resin reactors fit into production scale-up considerations. It is not clear what is “standard” in the industry for manufacturing resin for hoses or how many meters of hose can be manufactured from one batch in either one of the reactors.
- The proposed future work is a start; however, it appears to be incomplete. First, the project team appears to be the first user of the CSA Group document, a document that needs to be validated. Second, the hose end fitting issue will be a challenge. The effort should not be underestimated. Finally, the hose material compatibility with hydrogen and PEM fuel cells may also be a challenge.

Project strengths:

- The approach and materials used are promising. The initial lessons learned and the way they were applied to the development work are excellent.
- The project strengths are the approach and the need for reliable fueling hoses for both the hydrogen and compressed natural gas vehicle industry.
- This is an exciting technology posed to enter this challenging market. When successful, this technology will provide a U.S. hose technology to compete with international manufacturers. The technology looks very good and will satisfy the technical specifications.
- Project strengths include the understanding of polymer chemistry and ability to tailor chemistry.

Project weaknesses:

- Application of the crimp-on hose is a project weakness.
- The project weaknesses are the apparent lack of recognition of all of the challenges that the project will face. However, the reviewer is confident that the challenges will be successfully met; they are not insurmountable.
- Unfamiliarity with the hydrogen delivery and the safety, codes and standards (SCS) space is a project weakness. The project team should increase its partnership with NREL SCS to help address any discrepancies with the operational characteristics of the station.

Recommendations for additions/deletions to project scope:

- This project should remain sufficiently well-funded to reach the goals.
- The team should consider which one of the researched materials may be easier and cheaper to manufacture (but is distinct, say, in color) in case Nanosonic wants to supply the return hose from the nozzle to the hose breakaway as a complementary product (although it is unclear what the cost of that hose is). It seems that this secondary hose does not need to meet the same qualifications/requirements, as it is not exposed to same pressures and temperatures (and will never be because it is used for venting residual hydrogen between the nozzle and the vehicle receptacle when the fueling event is completed), but it may help with revenue generation. The project should consider what future accelerated testing may look like for these hoses, using the knowledge of materials used and lessons learned from discarded materials based on testing.
- The proposed future work is a start; however, it appears to be incomplete.
 - First, the CSA Group document the team is following was not written by the hose industry, nor has an end user tried to use it. Therefore, it might not include all of the requirements. Additionally, the test methods in the documents may not be applicable or comprehensive. Providing judgment and feedback to the standards development organization (CSA Group and International Organization for Standardization) would be prudent. Using the document as a guideline at this point in time is suggested.
 - The hose end fitting issue will be a challenge; the effort should not be underestimated. Leveraging the hydraulic hose manufacturers for guidance might be worth the effort. Swagelok sells hydraulic hose assemblies, but it may not make the hoses. Parker does both. Other manufacturers that might help are Aeroquip, Teleflex, Titeflex, and Crane Resistoflex. There certainly are others. Resistoflex is in Marion, North Carolina. Teleflex and Titeflex are in the Connecticut River Valley.

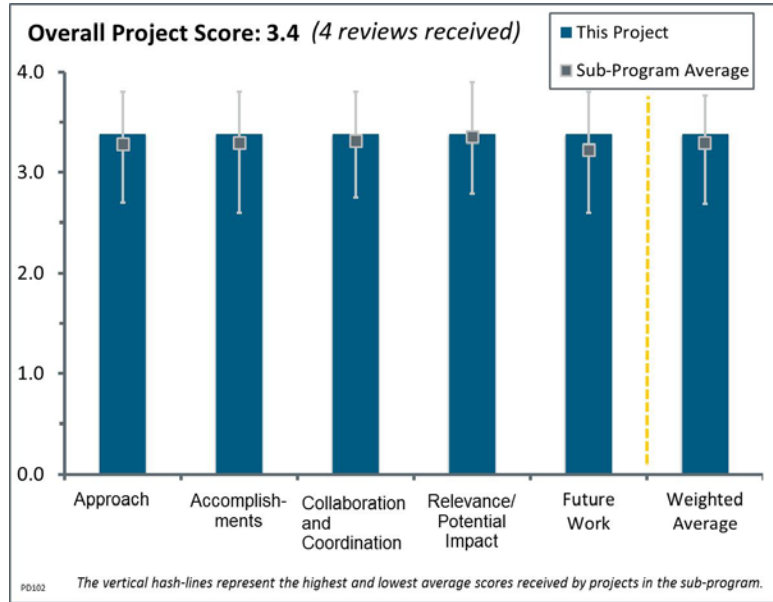
- Hose material compatibility with hydrogen and PEM fuel cells may also be a challenge. The SAE J2719 standard addresses only likely contaminants caused by the formation and processing of the hydrogen. The PEM industry has yet to evaluate every compound that might adversely affect the highly dispersed and highly reactive catalysts in use, for the obvious reason that it would be prohibitively expensive. For the perspective of this project, hose offgassing, leaching, and particulate formation may need to be addressed. NREL, LANL, the University of Hawaii, and the University of Connecticut are currently set up to test the effects of these compounds on PEM fuel cells.

Project # PD-102: Analysis of Advanced Hydrogen Production Pathways

Brian James; Strategic Analysis, Inc.

Brief Summary of Project:

The objectives of this project are to (1) analyze hydrogen production and delivery (P&D) pathways to determine economical, environmentally benign, and societally feasible paths for the P&D of hydrogen fuel for fuel cell electric vehicles; (2) identify key bottlenecks to the success of these pathways, primary cost drivers, and remaining research and development (R&D) challenges; (3) assess technical progress, benefits and limitations, levelized hydrogen costs, and potential to achieve U.S. Department of Energy (DOE) P&D cost goals of <\$4 per gasoline gallon equivalent (gge) by 2020; (4) provide analyses that assist DOE in setting research priorities; and (5) apply the Hydrogen Analysis (H2A) Production Model as the primary tool for projection of levelized hydrogen costs and cost sensitivities.



Question 1: Approach to performing the work

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

- Strategic Analysis, Inc. (SA) utilizes a well-developed, rational approach to cost analysis. SA uses a proven methodology for acquiring and processing information that forms the basis for cost estimates.
- The project has a well-established approach that includes gathering technical and economic data from appropriate industry and/or R&D representatives and using the submitted information as input in process and cost models, including H2A, to predict hydrogen cost, cost drivers, and sensitivities. This approach is well integrated with other modeling and analysis efforts in P&D and across the Fuel Cell Technologies Office (FCTO). Results are reviewed and vetted by the participating industry/R&D representatives. This approach would be difficult to improve significantly given the low technology readiness level of most of the production pathways under investigation and the modest funding for this effort.
- The project is well designed; however, a literature review could have been applied to complement the questionnaires/feedback from contributing partners. Additionally, questionnaires could have been sent to other institutions active in research in this area.
- The project addresses a couple of cases that will feed into H2A. It is well known that solid oxide electrolysis cells (SOECs) and bio-fermentation are currently very expensive technologies that are unable to compete with steam methane reforming. Given that, the project documents that these cases are cost-prohibitive. It is understandable that the H2A assumptions of high volume had to be followed, but currently there is very little information to accurately predict the cost of these pathways at high volume. It would be more useful to understand first where the industry is now at low-volume levels.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The project team has made very good progress toward its goals by completing analysis of two alternative renewable energy hydrogen production pathways. The SOEC analysis complements the polymer electrolyte

membrane (PEM) electrolysis study completed last year and will be a valuable resource for researchers and DOE. Similarly, the bio-fermentation study will be useful to DOE for portfolio planning. Although this is perhaps outside the scope of the current project, trade-off studies of various options in the electrolysis and biomass conversion/gasification groups would be of interest and of use to Hydrogen Production and Delivery sub-program planning.

- The project aims to facilitate the identification of R&D topics that DOE may fund in the future. It is a well-timed action for optimizing the allocation of R&D funds.
- These two cases appear to have been done well and provide a valuable addition to the H2A suite.
- The project is on target to achieve completion within the time specified by the principal investigators, but it is inadequate to model for the n^{th} plant as opposed to using actual numbers for current technologies. It is unclear what the definition of “demonstrated advances” is in slide 6. SOECs and bio-fermentation technologies have not been deployed at large scale (50,000 kg/day). It is not clear why the team used today’s cost at scale instead of modeling today’s cost at low production rates and then modeling a ramp up.

Question 3: Collaboration and coordination with other institutions

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

- The project team members work well together and have been successful in identifying and enlisting industry and technology experts to participate in and inform their studies. This year’s efforts addressing SOECs and bio-fermentation could be further enhanced by interactions with other DOE offices (e.g., the Office of Nuclear Energy and Bioenergy Technologies Office) to determine synergies that may favor these technologies. Examples might include an analysis of feasibility of waste heat/electricity for SOECs by co-locating them next to concentrated solar power or nuclear energy plants and an assessment of possible opportunities, geographic and otherwise, for less expensive feedstock and for R&D to enhance both concentration and hydrogen molar yield.
- SA has done a good job of working with industry and researchers to obtain the data needed for this work.
- The project has a wide network of collaborating institutions, but given the scope/structure of the project, sending contacts and questionnaires to additional institutions could have been beneficial.
- Partners and collaborators seem adequate to understand SOEC and bio-fermentation cases, although it is not clear how these partners judged the future status of the technology, given that the results do not present any variability.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- By providing analysis on cost, key barriers, and drivers for renewable hydrogen production pathways, this project provides information that allows the DOE to plan future R&D, track progress of R&D, and compare progress and the potential of various pathways. The project is critical to achieving the goal of <\$4 per gge hydrogen (dispensed and untaxed) by 2020. The analysis provided is critical for the success of the production portfolio.
- The project provides support in planning future R&D topics.
- This work shows the potential for these new pathways.
- It is well known that these pathways are expensive. They will become relevant only after a major breakthrough. Furthermore, it would be useful to have current cost numbers instead of projected numbers for high hydrogen volumes.

Question 5: Proposed future work

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

- The proposed future work presents reasonable next steps for this project; however, the project team will be directed by DOE as to which pathways to investigate next. Timely production and posting of H2A cases are important for the R&D communities to benefit from the analysis.
- In terms of future pathways to be analyzed, biomass gasification and pyrolysis could also be considered.
- The future cases have merit but will be difficult because of the early stages of development.
- It is adequate to integrate the results of these cases into H2A and add new, advanced hydrogen production cases. That work needs to be done and updated with certain frequency. However, the pathways selected are very far from being commercial, and it is unclear how this information will be used. It would be helpful to ask end users to rate the usefulness of the information and which pathways are more interesting.

Project strengths:

- SA uses a strong methodology with a proven process to render consistent and reliable projections of costs.
- The project has an experienced team providing consistent analysis across the FCTO programs.
- Project strengths include the use of both the H2A modeling tool and in-house process and cost models.

Project weaknesses:

- The production pathway cost targets are quickly becoming outdated (fiscal year 2015 is almost over). Updates to the H2A tool may also be needed but should happen if/when the office cost goals are reconsidered.
- Collection of information could have been done through literature review as well as through questionnaires.

Recommendations for additions/deletions to project scope:

- SA should make sure that the issues around future bio-fermentation are clearly stated—specifically the fact that increasing stover concentration lowers hydrogen yield.
- An analysis of the costs and barriers for distributed or semi-central production of hydrogen via biomass conversion (e.g., wastewater treatment), possibly in tandem with microbial electrolysis cells or microbial reverse electrolysis cells, would be of interest. It may not be currently feasible because of lack of available input from researchers, industry, or municipal wastewater treatment centers, but it should be considered for future attention. A comparison of relative advantages of biomass gasification and biomass conversion processes for central production would also be of interest. In addition, the production pathway targets will need to be updated soon. While this is currently outside the scope of this project, it might be helpful if the project scope were expanded to include development of an outline of needed updates and additions to the pathway targets/cases.
- The reviewer had three recommendations:
 - It would have been useful if under “Current Case,” costs were provided for both today’s limited production capacity and under the theoretical mass production capacity, in case the examined technologies were commercially available today.
 - Solid oxide electrolysis, being a high-temperature process, would suffer from frequent shutdowns, starts, and general dynamic operation of the unit. This, however, is highly likely to be the case in the long term for electrolyzers when trying to operate with low-cost electricity (during times of excess renewable energy) or when operated to provide ancillary grid services. It would be useful if this could be factored into the electricity cost to make a fairer comparison with PEM or Alkaline electrolyzers.
 - In terms of future pathways to be analyzed, biomass gasification and pyrolysis could also be considered.

Project # PD-103: High-Performance, Long-Lifetime Catalysts for Proton Exchange Membrane Electrolysis

Hui Xu; Giner, Inc.

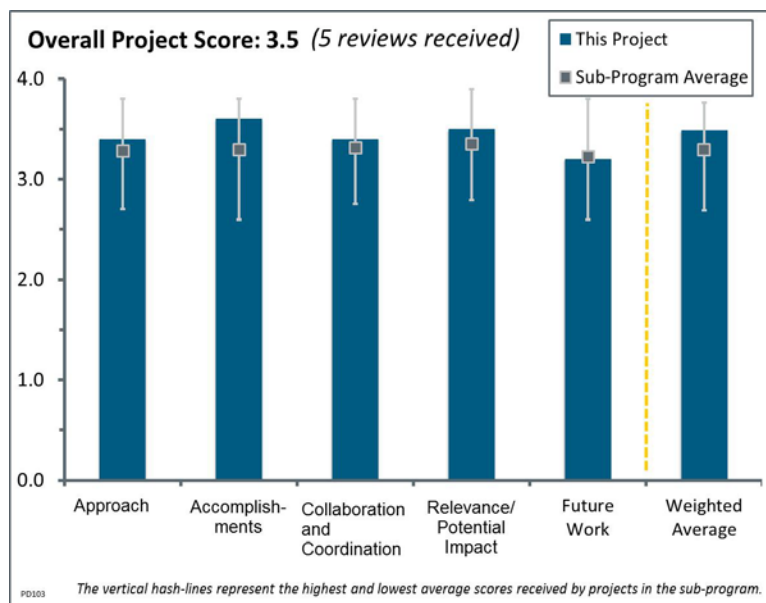
Brief Summary of Project:

The objectives of this project are to (1) develop advanced, low-platinum group metal (PGM) loading catalysts for high-performance, high-efficiency, and long-lifetime proton exchange membrane water electrolysis using three different catalysis approaches and (2) evaluate the impact of newly developed catalysts on proton exchange membrane electrolyzer efficiency and cost.

Question 1: Approach to performing the work

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

- The project has an excellent approach to reducing precious metal loading while maintaining the performance and durability of the electrolyzer.
- The project objective is the development of low-platinum-group-metal (PGM) catalysts (e.g., $\text{Ir}/\text{W}_x\text{Ti}_{1-x}\text{O}_2$) for PEM electrolysis. Various catalyst compositions, supports, and nanostructures have been evaluated for activity and durability.
- The approach is excellent; it effectively contributes to overcoming most barriers. However, a stronger focus is suggested on gaining a molecular-level understanding with respect to the catalyst surface structure reconstruction during operational conditions.
- The project is cogently constructed with a very reasonable and straightforward approach to the experimental demonstration of three avenues to Ir loading reduction. It appears that the durability testing of the Ir nanowire approach was not conducted. It is not clear why.



Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.6** for its accomplishments and progress.

- The data showing that $\text{Ir}/\text{W}_x\text{Ti}_{1-x}\text{O}_2$ achieves three times higher activity than baseline Ir black (at 2.0 V) is an exciting result. A future objective should be directed toward a molecular-level understanding of this seemingly incredible result. The simultaneous stability probably would play into the molecular-level understanding. The bimetallic (IrCo and IrNi) nanowires should be studied as model systems in controlled catalytic characterization tests to understand and correlate the surface structure to performance. Because Ir is a (weak) Mossbauer element, one could conceive of an operando-type experimental campaign. Finally, the level of accomplishment, as demonstrated here, is outstanding. There is close, appropriate collaboration with other institutions, the partners appear to be full participants, and the overall strategy is very well coordinated.
- Accomplishments included preparation and testing of $\text{Ir}/\text{W}_x\text{Ti}_{1-x}\text{O}_2$ catalysts with performance and durability evaluation using rotating disk electrode (RDE) and stack testing. Ir mass activity was demonstrated to increase with low loading. Stack performance with a 10x reduction in PGM loading was shown to be comparable to the standard loading case. Durability was demonstrated with a 1,000-hour test. Development and testing of nanostructured thin film (NSTF) catalyst substrates (conducted by 3M) were

completed. Performance comparable to baseline was demonstrated with 1/8 PGM loading. Durability of 1,000 hours was also demonstrated with the NSTF catalyst configuration. Development and testing of Ir/metal nanowires (conducted by the National Renewable Energy Laboratory [NREL]) were completed. Performance was shown to be about 10 times better than Ir nanoparticles.

- The project has demonstrated impressive reductions in precious metal loading (from 2–3 mg/cm² down to 0.25 mg/cm²) with a minimal drop in performance, eventually leading to reduced capital cost of electrolyzers and thus hydrogen.
- Excellent progress has been achieved in fabricating and testing three classes of Ir-based catalyst. The experimental results are meaningful and appropriate.

Question 3: Collaboration and coordination with other institutions

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

- There appear to be good collaboration and division of labor among the three main team members (NREL, 3M, and Giner), and there is appropriate outside assistance for particle characterization.
- The collaboration is excellent.
- Collaborators included NREL (working on Ir/metal nanowire development as a subcontract), 3M (working on Ir NSTF catalyst development as a subcontractor), Oak Ridge National Laboratory (for the catalyst and membrane electrode assembly structure), and University of Massachusetts Lowell (for the catalyst composition and structure analysis).
- There is close collaboration, but it is limited to project partners.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- The project greatly supports the goal of reducing the cost of electrolytic hydrogen by reducing the capital cost of electrolyzers and, at the same time, reducing the use of precious materials.
- Low-cost, low-PGM high-efficiency catalysts are needed for achievement of low electrolyzer system cost. Nanostructured PGM catalysts on non-precious supports can achieve high performance with low PGM loading. This work could have a significant near-term impact on PEM electrolysis performance and cost.
- The project aligns well with the DOE Hydrogen and Fuel Cells Program and DOE research, development, and demonstration (RD&D) objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
- The impact of reducing Ir loading by 10x is obviously beneficial, but it needs to be quantified. A cost analysis (at least a back-of-the-envelope calculation) should be conducted to suggest an Ir loading target at which point further reductions are of inconsequential benefit.

Question 5: Proposed future work

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

- Future work on Ir nanowires appears to have been dropped in favor of focusing efforts on the other two approaches. This seems reasonable.
- Future work is well planned out.
- Future proposed work includes further study of the Ir/W_xTi_{1-x}O₂ catalyst and selection of the best catalyst for the Giner sub-megawatt stack.
- The proposed future work does not seem to be directed at addressing a specific hypothesis. There needs to be a better, more focused effort to gain a molecular-level understanding of the surface catalysis process. This can be done by appropriately selecting and testing model catalysis systems under in situ or operando conditions.

Project strengths:

- The project appears to focus very well on the demonstration of the attributes most needing experimental assessment.
- Synthesis and testing of several advanced Ir-PGM catalysts has been completed with very promising results.
- The project has the right consortium to perform this type of work and eventually implement it in commercial products.
- Project strengths include the collaborations and accomplishments.
- The project has achieved the stated milestones.

Project weaknesses:

- The milestones might have been originally written to be lax.
- The graph on slide 19 for the Ir NSTF approach shows a polarization curve that deviates from the shape of the other catalyst curves. This should be explained. The error bars on slide 19 are considerable and obscure comparisons between the catalysts. While perhaps this amount of potential error is unavoidable, consideration should be given to how this might be improved.
- A project weakness is the lack of an approach to gain a fundamental, molecular-level understanding of the catalyst surface and its effect on performance.

Recommendations for additions/deletions to project scope:

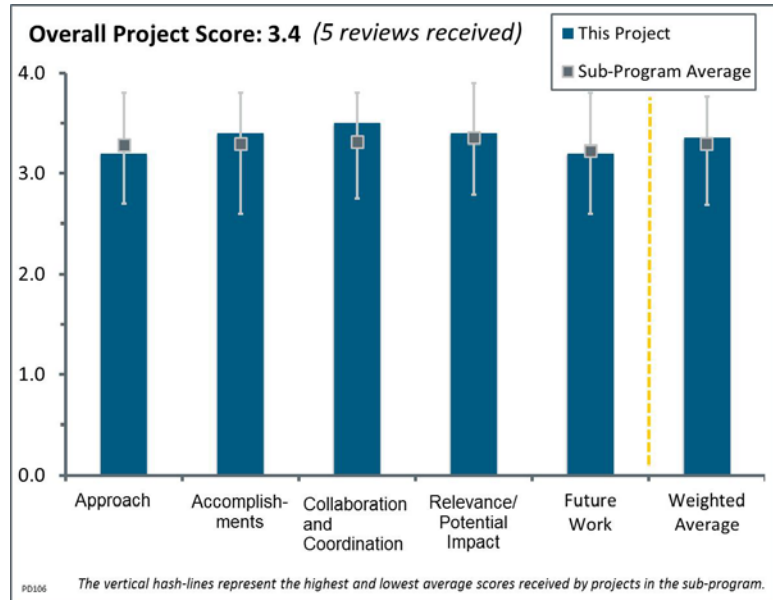
- The current project is complete. The proposed future work shown in the presentation represents a logical extension of this project.
- A basic cost analysis should be added to quantify the benefits of reduced loading.

Project # PD-106: Reference Station Design

Daniel Terlip; National Renewable Energy Laboratory

Brief Summary of Project:

The overall objective of this project is to speed the acceptance of near-term hydrogen infrastructure build-out by exploring the advantages and disadvantages of various station designs and proposing near-term optima. For fiscal year 2015, the project will (1) provide a detailed view of how these stations fit in greenfield and existing sites in relation to the National Fire Protection Agency (NFPA) 2 standard; (2) help station developers quickly evaluate the suitability of their sites for a particular station type and capacity; and (3) provide station developers and local authorities with a complete picture of the devices, components, and associated costs that make up a station.



Question 1: Approach to performing the work

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

- This approach appeared to be very thorough, systematic, and appropriate for this type of work. The results speak for themselves—they are excellent.
- The project team conducted a thorough review of current hydrogen fueling station technology.
- The presentation was informative, and the speaker was very clear and subjective in answering the questions.
- It is not clear whether relevant international efforts have been accounted for, e.g., definitions for standard capacities and performance that are used by H2 Mobility in Germany.
- It appears that most factors were considered in making the approach lead to relevant outcomes. However, it is not clear where and how municipalities and authorities having jurisdiction (AHJs) are involved in providing feedback on the work done (feedback loops were not included in the approach)—this is important, as they are identified as targeted beneficiaries of this effort.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The project is 100% complete. The team did a good job. The direct and indirect accomplishments for this work will prove to be very valuable to hydrogen fuel station (HFS) rollout—but it will also be very valuable to the quantitative risk assessment (QRA) and code development organization/standards development organization (CDO)(SDO) community. Having a vetted “standard” design provides the ability to set examples of how to determine hazards, causes, and consequences. This was not articulated during the presentation but is an example of an ancillary accomplishment and/or value added from this work.
- This review of current technology was well conceived and well executed. The timeline was very short, and the team accomplished the goals of the project very quickly.
- The task provides a valuable set of information to support early deployment of hydrogen refueling stations (HRSs).

- The presentation provided leads, which could be a deliverable.
- Based on the reasons given for the values on slide 8, even for the near term, this effort appears to lean toward the low-capacity stations. It is acceptable that some of the participants think that having many small stations providing fuel to cover a larger area is a good idea and that the project team thought it was beneficial to include 50 kg/day stations. However, based on results and given the equipment selection assumed, it is questionable if that was a good choice. The estimated costs of hydrogen at \$40–\$80/kg to reach a return on investment for the station sound unrealistic for any investor, but possibly that was the reason for including it.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.5** for its collaboration and coordination.

- This is an excellent example of how researchers from competitive national laboratories have come together to build an extremely effective team.
- There was good participation in the project. There were many interested stakeholders.
- Appropriate collaborations and coordination were part of this effort.
- Industry is involved via H2USA; it is not clear whether that includes sufficient component suppliers.
- It is not clear who the collaborators are from the H₂USA HFS Working Group.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.4** for its relevance/potential impact.

- The relevance and impact are greater than originally intended. They are excellent. The benefits go beyond those articulated during this presentation; the work will be adapted in the international community as ISO / TC 197 19880-1 gets developed and published.
- The project aligns well with the DOE Hydrogen and Fuel Cells Program and DOE research, development, and demonstration (RD&D) objectives and has been a very important step in identifying the economic challenges to station deployments. The information developed by this project is critical to advancing progress toward DOE RD&D goals and objectives.
- The project increased knowledge about HRS design and cost. Dissemination will be important for further deployment.
- The project is very relevant to our future energy and environment.
- It is not clear if the project team has received any feedback about how likely it is that station implementers will actually follow these reference station designs. Because of the focus on same-size stations (100–300 kg/day capacity), the project appears to have relatively limited relevance for near-term station implementation efforts currently underway. Inclusion of larger-sized stations (500–700 kg/day) could have put outcomes in better perspective, which would be beneficial for the considerations of those willing to make a larger long-term investment.

Question 5: Proposed future work

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

- The proposed future work is a natural extension of this current work—to extend beyond on-site delivery and to keep these numbers and this analysis up to date with the state of the art in the technology.
- The team seems to be open to further work, including modeling mixed-use stations and on-site production.
- The project has ended, but proposed topics for future work are relevant.
- In addition to the proposed future work in the presentation, it would be good to see reference station designs for material handling applications, medium duty vehicles, and heavy duty vehicles, as these are likely to support considerations of captive, private, and transit fleet owners—where economic considerations play a more significant role for revenue fleet adoption.

Project strengths:

- The project is an excellent and thorough effort.
- Project strengths include the team's good attempt at using a larger group of stakeholders to build an understanding of available technologies and components in the market.
- A broad database of station components and designs was created.
- The team leveraged the significant capabilities and experience of the project team very well. The team did good work to define the station size categories, which has not been done before for the U.S. market. However, the definition of station size and capacity is not complete.
- The project is adequate and will have potential for further improvements.

Project weaknesses:

- Not all station types have been modeled, and this gap includes the on-site generation options. While the team made a good effort to define station size (50, 100, 200, and 300 kg/day), it did not quantify the dispensing capacity (kilogram per hour) and sequential fill capacity for each station size.
- A hands-on demonstration would have been very helpful.
- It is important to focus on station design in the early deployment phase (low utilization) rather than recalculate fuel cell electric vehicle and HRS ramp-up.
- It is interesting that the identified areas under remaining barriers/challenges appear exactly like those that have been identified and are currently topics of DOE-supported projects. The outcomes appear to define not specific reference stations for small, medium, and large station categories but rather stations that are nearly identical in size to the stations currently being rolled out in California (except for the 50 kg/day station identified). Focus is needed on small capacity stations, which have been proven to be challenging to make work economically (which the added cost per kilogram of hydrogen dispensed clearly indicates).

Recommendations for additions/deletions to project scope:

- The continuation of work as proposed should be funded.
- It would be great if the station review team could provide some further clarity to the 50 kg/day station size. It is not clear how many back-to-back fills are really needed for a small station.
- The team should invite experts from national and international SDOs, such as ISO/TC197.
- Analysis should include on-site production (especially electrolysis). The effect of low utilization on the reliability of stations should be assessed, considering different design, economics, etc.
- It is recommended that the team not show the West Los Angeles station picture anymore, as that particular station design is unrealistic from an economic perspective because of the highway overpass earthquake-grade canopy structure (which is extremely expensive). There is no point in including the California Fuel Cell Partnership Roadmap numbers; the California Air Resources Board Assembly Bill 8 report numbers are now the reference point. In addition to the project scope, consider larger capacity stations because of the delay in rollout of stations. If a station were funded today, it could be operational two years from now, when larger capacity may be needed in focus cluster regions.

Project # PD-107: Hydrogen Fueling Station Pre-Cooling Analysis

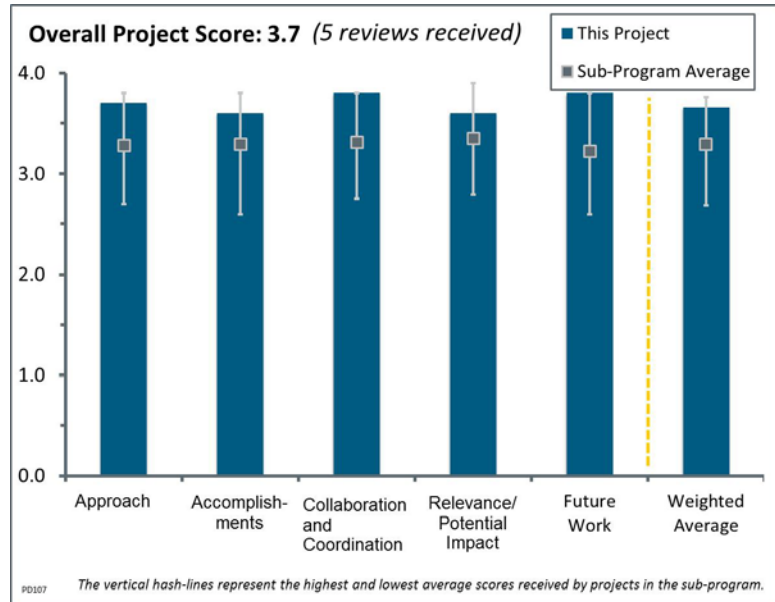
Amgad Elgowainy; Argonne National Laboratory

Brief Summary of Project:

The objectives of this project are to evaluate theoretical pre-cooling requirements at hydrogen fueling stations (HFSs), collaborate to acquire information on refueling operation and review results, examine current pre-cooling equipment design and cost, identify major drivers for pre-cooling cost and energy consumption, analyze trade-offs between different design concepts, and vet analysis results and findings.

Question 1: Approach to performing the work

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



- The project is almost done—only fiscal year (FY) 2015 work remains. The team needs to understand pre-cooling costs versus various cooling technologies and determine whether the costs can be reduced (i.e., increase the coefficient of performance [COP]/efficiency). The project should start with fundamental thermodynamics, consistent with the SAE International J2601 protocol. The thermodynamics of these processes are very well understood and readily modeled. This principal investigator is well suited to perform these analyses, and that is demonstrated in this work. This really is a very straightforward thermodynamic analysis of cooling systems. Funding was \$100,000, which is appropriate for this study. The labor spent is in line with the funds expended (which is not a criticism of the value of this analysis). With that said, the outcome from this work is enlightening regarding these systems, including the impact on cost and operation of the systems to achieve the required fill characteristics. This work is very timely because station deployment is in its early stages right now. This work really needs to be incorporated in the station design activity of the Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) project.
- Hydrogen fueling stations are expensive compared to gasoline stations, partly because some of the components in an HFS must be custom designed and built. Pre-cooling equipment and heat exchangers can contribute significantly to the station’s capital expenditures, particularly when the station is over-designed. This analysis can help address this issue.
- The approach of this analysis was excellent: developing the pre-cooling energy in both the continuous and on-demand modes of operation. It was helpful to provide these different modes in order to quantify the significant range in energy consumption that could occur.
- The analysis addresses a highly relevant topic for HFS deployment. Hydrogen cooling is relevant in terms of cost, and it has an effect on station performance, especially at low utilization.
- The approach indicates an understanding of the issue at hand and what aspects should be considered to lead to valuable results; in addition, the approach was well explained and visualized.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.6** for its accomplishments and progress.

- The project is 100% complete—the team did a good job. This really is a very straightforward application of classical thermodynamics. The labor needed to execute this is in line with the funds expended. From a first

principles perspective, the results simply describe the classical thermodynamic results. However, the comparison of different celling schemes applicable to HFS filling at this point in the HFS deployment is extremely valuable, particularly for those who need to evaluate HFS designs. Any HFS manufacturer would do this type of analysis to optimize its own system; however, this analysis does provide people who need to evaluate HFS system performance some indications into what and how these systems can differ.

- The project was effective in providing useful conclusions and sensitivity factors, such as the heat exchanger thermal mass, back-to-back fills, station usage, and operation modes.
- The objectives of the project have been met in the sense that the theoretical pre-cooling energy requirements have been identified, which has helped in developing a method for sizing the pre-cooling equipment and heat exchanger.
- The project provides very relevant analysis.

Question 3: Collaboration and coordination with other institutions

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

- It appears the project had the right mix of industry experts to coordinate the assumptions and analysis.
- International experience was taken into account.
- The right stakeholders have been involved.
- The collaborators for this project are appropriate.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.6** for its relevance/potential impact.

- This project is highly relevant to the practical implementation of stations. It is important to know about the factors that can change and influence pre-cooling energy usage because this is a critical factor in 700 bar fills.
- The project shows results that are highly relevant to optimization of HFSs, especially in the early deployment phase.
- There is a significant benefit in terms of the body of knowledge about the impact of pre-cooling hydrogen dispensed per SAE J2601.
- This analysis is very timely and needed to help the community evaluate HFS systems for performance, maintenance, and costs (capital and operating expenditures).
- It is unclear whether equipment manufacturers will make use of the resulting work, but it will be useful to incorporate the results into the Hydrogen Analysis (H2A) Refueling Station Analysis Model (HRSAM).

Question 5: Proposed future work

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

- It is necessary to continue this work to identify cost-reduction potentials and new designs or components to increase station performance (e.g., short start-up times in cases of low utilization).
- The biggest value added by this work is to make sure it is embedded into the reference station design activities to make that activity more robust.
- The work is progressing well, and the proposed future work seems appropriate to make the best use of the results.
- There are excellent suggestions based on interactions with project partners and stakeholders.
- The future work seems to involve appropriate next steps. It was interesting that some of these items (e.g., the MC Default Fill) were being covered in another project, SA-045. This connection should be noted in these slides in the future.

Project strengths:

- Detailed impacts of pre-cooling are not well understood (aside from a 10% cost impact), and this project provides industry with insight into this topic.
- Strengths include the project team's technical knowledge and modeling capabilities, as well as its stakeholder engagement.
- The project is highly useful for the practical assessment of pre-cooling for 700 bar stations.
- This is an excellent, thorough effort.
- The analysis builds on international experience.

Project weaknesses:

- The project does not have any weaknesses to report. Additional field data would be useful.

Recommendations for additions/deletions to project scope:

- Three additions are recommended. The project should:
 - Consider the impact of using the MC Formula on pre-cooling and the impact on hydrogen cost and power/energy needs: -33°C in 30 seconds is not necessarily needed when using a dynamic fueling process instead of a table-based one.
 - Consider the impact/benefit of lower-than-ambient-temperature hydrogen gas from buffer storage to chiller. Underground storage may result in more stable and lower gas temperature, resulting in additional economic benefits in terms of the cost of hydrogen dispensed.
 - Consider the impact of placing dispensers farther away (e.g., 50 feet versus 100 feet) from station equipment on pre-cooling cost and power needs.
- The project should merge the results with those of the other project, SA-045. As an addition, the analysis could evaluate the pre-cooling needed in different regions of the United States based on ambient temperature distributions to provide strategy guidance regarding the pre-cooling temperature and energy usage.
- Continuation is recommended—especially efforts to identify alternative pre-cooling options.
- The continuation of the work as proposed should be funded.

Project # PD-108: Hydrogen Compression Application of the Linear Motor Reciprocating Compressor

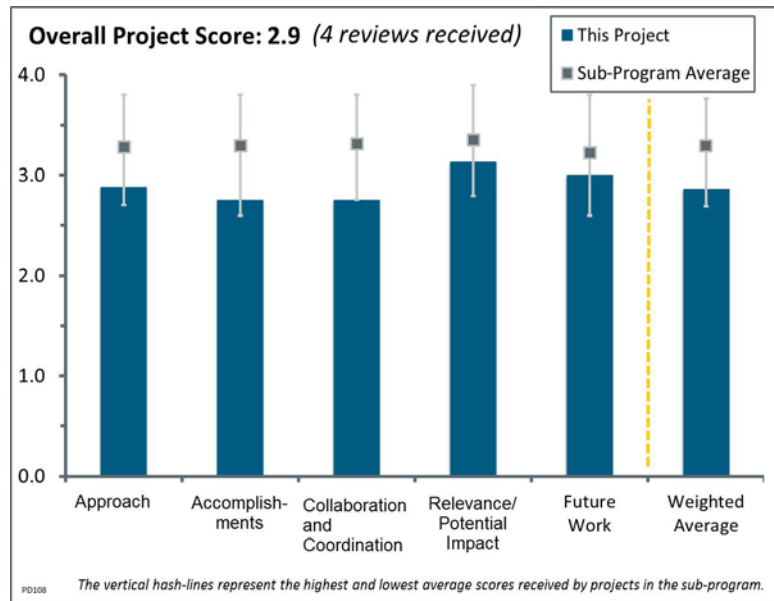
Eugene Broerman; Southwest Research Institute

Brief Summary of Project:

The objectives of this project are to (1) improve isentropic efficiency of a compressor above 95% by minimizing aerodynamic losses, (2) reduce capital costs to half those of conventional reciprocating compressors by minimizing part count, and (3) reduce required maintenance by simplifying the compressor design to eliminate common wear items.

Question 1: Approach to performing the work

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



- Initial design and modeling are proceeding well.
- The project goal is to develop a positive displacement compressor. Instead of being driven by a motor and crank shaft, the piston(s) is moved by a series of magnets generating a linear motion (linear piston). This basic concept is not new; linear hardware has been developed for other applications. This development team has extremely aggressive targets (isentropic efficiencies >95%). The path to achieving this is to optimize the fluid dynamics (aerodynamic losses). Actually, the team is attempting to optimize the thermodynamics of the process. This raises a small flag that the team has not got the thermodynamics quite right. Linear pistons have fewer moving parts compared to reciprocating machines; hence, the expectation is that reliability will go up and the cost will go down. This project is just getting started, so if the team is able to solve—or satisfy—the following issues, this could be a good project with a good outcome. These issues are the following:
 - This team needs to perform a detailed first law of thermodynamics analysis on the overall system. It may sound like a good goal to reach an isentropic efficiency >95%, but the property that drives the cost of operation is the overall first law thermal efficiency, not the isentropic efficiency. Clearly, a high first law thermal efficiency will not be achieved with a low isentropic efficiency; however, the reverse is not true. A high isentropic efficiency does not at all guarantee a high first law efficiency. This analysis should yield a figure (table) in which some measure of efficiency is identified (energy required as a function of output pressure and mass flow rate, i.e., $KW = f(\dot{M} \cdot \text{MPa})$).
 - Linear pistons are controlled by magnets whose field is modulated, resulting in the desired piston movement. The temperature of these magnets needs to be controlled. This is easily done but results in significant heat being rejected from the system. This is a large loss term not at all reflected in the isentropic analysis. A full system thermodynamic balance is critical to determine whether this system will compete with current reciprocating machinery.
 - A much better analysis would be exergy (second law). That analysis will provide information on where in the system one should focus to improve (reduce exergy production) for optimization. This needs to be done before this project proceeds further from this point. Stopping all further work until this system energy balance has been performed is strongly recommended. Only then can one determine whether this technology will indeed yield a more efficient system than exists today.

- In addition, there is a serious heat transfer issue. The drive piston (the one coupled to the magnetic field) is isolated physically from the “block” of the magnets. This piston has an energy source into it but no mechanism to dissipate energy. To reduce friction, this piston is presumably free-floating (air bearings). Air is a wonderful insulator and, hence, a horrible thermal conductor. The magnetic coupling is an energy source into the piston, but there is no coupling for energy to leave the piston. This system is targeted for continuous operation. The first law states that $E(\text{in}) - E(\text{out}) = \text{rate of storage}$; this system has $E(\text{in}) > 0$ and $E(\text{out}) = 0$, so the rate of storage is > 0 . This could be a showstopper. This is the second analysis that must be done before this project should go further.
- Based on this discussion, this category received a 2.5 score. This project could have potential, but these thermodynamic transport questions must be answered.
- On a different note, the dynamic seal required to seal the compression piston from the high pressure on the piston head (85 MPa) to ambient (0 MPa) also evokes some skepticism. Success with this will be a big challenge. At this early stage in the development, this is not a showstopper—just a concern.
- The approach to the project is sound as presented and provides an appropriate work path to the project end goal. What was not presented was an assessment of where the principal investigator (PI) believes the major costs to be within compressor designs and correlate that assessment to how this specific novel design will, in fact, reduce cost. Essentially, the PI is saying that eliminating crankcases and traditional motors could halve compression costs. Some analysis on this costing is needed to understand the basis. This part was missing. Considering the stroke, force, flow, and the fact that this is a single acting design, correlations seem off for the flow rate described. The correlations seem to be an order of magnitude off (i.e., based on the stroke and force presented, the results should be 1 kg/hour rather than 10 kg/hour).
- The concept of linear pumping is very interesting in terms of system efficiency and space management. That said, the impression given is that project development is being managed on a component-by-component basis rather than as a complete system. The team is encouraged to step back to review the whole system to construct a development plan that encompasses both system development and component development. One system review has to address thermal management of the electric motors, heat of compression, and friction.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The team has achieved much in the past eight months, but now the team members have to begin performing detailed engineering and challenging their own concepts for success. As progress continues in 2015, the team should collaborate with mentors/organizations that are knowledgeable about electric motor system design, compressor system design, and hydrogen safety. In anticipation of next year’s Hydrogen and Fuel Cells Program Annual Merit Review (AMR), it would be good to see more information about the control system required to manage electric power delivery and the dynamics of a high-speed reciprocating pump and its related energy and mass.
- The project is only six months old, but it is progressing on schedule. Refrigeration energy for cooling water (interstage and magnet) should be included in efficiency calculations.
- This project is just starting. So far it is making good progress.
- There are key features missing from this design, including:
 - A distance piece to prevent hydrogen ingress into the magnet area. When asked, the PI provided comments on seals that would prevent this ingress. However, in real-world compressor applications, seal leakage in reciprocating compression equipment is key.
 - A linear actuator current of 600 amps seems extremely high (this is perhaps the biggest challenge). Several points are unclear:
 - The equivalent horsepower associated with this
 - How this compares to current compression technologies
 - Whether it affects the installed cost of the compressor
 - The surface temperatures of the coil housing

- These temperatures in relation to requirements for hazardous area classifications of the coils

Question 3: Collaboration and coordination with other institutions

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

- The list of collaborators appears impressive in terms of individual technology competencies. Over time, the team is encouraged to develop additional collaborators familiar with integrated electric power systems and hydrogen compression. Collaboration on system safety should rise in importance as development progresses.
- At this stage, the project has the right mix of collaborators. Presumably Southwest Research Institute has the expertise to perform the thermodynamic and transport analysis requested as part of this review. If not, then the project team needs to reach out to an institution that does have this type of expertise (Argonne National Laboratory, Sandia National Laboratories, etc.).
- Southwest Research Institute has a good design partner in ACI Services, although partnership with national laboratories with experience in magnet materials good for hydrogen service could have been useful, given that it appears the hydrogen could leak into this area.
- Collaboration appears to be limited to material and component suppliers. The PIs should work with commercial compressor suppliers such as PDC Machines and Mitsubishi Heavy Industries to vet the project designs and assumptions.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- Current compressors are an Achilles heel for hydrogen fueling systems. Improving the operating efficiency, improving the reliability, and reducing costs are critical to the successful commercialization of hydrogen fueling systems and hydrogen technologies in the transportation sector.
- Conceptually, this technology will have relevance if it can be competitive in the areas of compression efficiency, capital management, and physical size. The next two AMR reviews will provide more detail on the project's success. This is the type of technology that must be funded to determine whether hydrogen compression can be achieved in a manner that is measurably better than conventional systems.
- If the project is successful, this could significantly reduce delivery costs.
- The item preventing a rating of "good" is an in-depth cost analysis showing exactly how this compressor, as presented, would be half the cost of current technologies. Further, additional cost analysis needs to be presented on simplification of design that would lead to reduced maintenance costs (as required by targets in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan [MYRDDP]). As shown, the compressor is still a compressor with common maintenance items.

Question 5: Proposed future work

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

- There is a good deal of engineering work to perform, so the project's layout for this work is appropriate. What should be added are (1) temperature data on the coils of the actuator and (2) efforts to review how to have the product approved as a listed hazardous area electrical component. This is critical path work that must be performed for viability in hydrogen refueling stations.
- The future work seems to be fragmented into component development. In addition to that, the team is encouraged to evaluate the integrated system to determine whether certain design work must cross technology boundaries. Also, the team is encouraged to consider and set objectives for the life cycle of the system. A unit designed for a fifteen-year life may have different design features from one designed for three years of life. Uptime performance is another factor that deserves attention.

- This is a new start project. The proposed future work is short two critical components: a system energy balance and an energy transport analysis focused on the temperature time history for the drive piston.
- The PIs should work to quickly determine total energy consumption for the compressor system. Stage 1 should be built and tested to verify performance before any work is begun to construct the second and third stages.

Project strengths:

- Conceptually, this project would be a good way of reducing cost, footprint, noise, and eventually maintenance at hydrogen refueling stations.

Project weaknesses:

- As the project is currently presented, polymer seals are envisioned. In current technology, polymer seals do not have a very high seal life in reciprocating compressor refueling stations, and especially not at the potential surface speeds that have been presented. Sapphire-on-sapphire sealing would present an intriguing opportunity for big sealing technology advancement. The packing design at pressure and speed is concerning. A key omission is the lack of foresight regarding the need to consider that the linear actuator must be an explosion-proof piece of electrical equipment or, at a minimum, to be rated for Class 1 Division 2 Group B hazardous locations. With 600 amps going through the coils at 10 kg/hr and when scaled up to meet the MYRDDP target of 20 kg/hr, the surface temperatures, cooling requirements of the magnets, and power requirement could be enormous and must be considered as future work.
- There are serious concerns about the overall thermal behavior of this system. This project needs to perform a detailed energy balance with a focus on understanding the system's overall energy demand. Also, this team needs to understand the temperature of the drive piston as a function of time. There appear to be energy input terms but no output terms. This system could be configured for continuous operation, but the temperature time profile of the drive piston could be a showstopper.
- The project has a component development view, which is a weakness.

Recommendations for additions/deletions to project scope:

- The project team should (1) perform a detailed energy balance/budget for this system and identify and estimate the energy demand as a function of pressure and flow rate, and (2) perform a heat transfer analysis on the drive piston to understand the drive piston temperature time profile.
- The PIs should move quickly toward establishing an energy consumption estimate expressed as kWh/kg of hydrogen compressed.
- The project should have external mentors experienced in managing electric power and hydrogen compression systems.
- The project should add the hazardous location listing of the linear actuated coil.

Project # PD-109: Steel Concrete Composite Vessel for 875 bar Stationary Hydrogen Storage

Zhili Feng; Oak Ridge National Laboratory

Brief Summary of Project:

The overall objective of this project is to develop a second-generation steel concrete composite vessel (SCCV) that will be more cost effective for forecourt hydrogen fueling station applications. Other objectives include (1) reducing the purchased capital cost of SCCV for forecourt hydrogen storage to \$800/kg at 875 bar while meeting all other requirements, including a projected service life of at least 30 years and scalability to 1,000 kg of storage, and (2) fabricating a representative prototype mock-up that captures all the major features of SCCV technology.

Question 1: Approach to performing the work

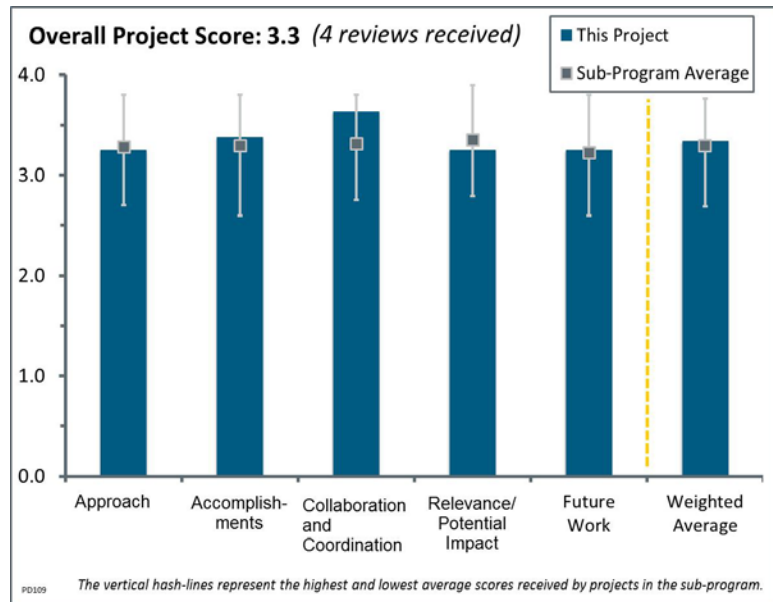
This project was rated **3.3** for its approach.

- The project team has a good focus on cost reduction. There is a bottom-up analysis on materials. Estimates show a potential cost reduction of 60% compared to the first unit, but to be conservative, only 20% is targeted. Manufacturability is being considered, but it will likely be a larger fraction of the cost than the principal investigator (PI) suggests. It would be good to see some detailed attention given to the overall manufacturability of the system. Sensors will be deployed with the expectation that the manway can be removed. This will result in additional savings. There did not appear to be any attention given to maintenance. This could be a significant component of operating expenses.
- The project is building on an earlier project developing SCCVs. This current project, in particular, has a goal of moving the working pressure to 875 bar with an ultimate cost target of \$800/kg hydrogen, which exceeds the U.S. Department of Energy (DOE) cost target of \$1,000/kg hydrogen. The focus on advancing materials and sensor technologies to achieve significant cost savings is excellent.
- The project team is taking an already proven approach that builds on the results and learnings obtained from its previous project for the Generation 1 SCCV.
- This project builds on experience gained with earlier low-pressure projects.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- There has been excellent progress: milestones were completed for the first quarter, a go/no-go was passed in the second quarter, and the work for the last two quarters of the fiscal year is in progress. There has also been excellent progress in identifying a permeation barrier. The team completed the initial, level-one vessel design, identifying significant cost reductions. This is excellent, considering the footprint and installation issues in the cost reductions. The detailed cost reduction analysis is nice and thorough.
- This project was just initiated, but it has started to down-select steel materials and is making appropriate progress.



- Considering that this is a new project, the team presented good progress on this work. Identifying available materials for the vessel as well as conducting the preliminary identification for possible opportunities for cost reduction will direct this project in the right direction.
- This project is at an early stage, and progress to date is reasonable. It seems that despite the use of concrete, most of the load is being borne by steel rebar or wound steel wire. The advantages of concrete need to be clearly understood before the team proceeds with any construction activities.

Question 3: Collaboration and coordination with other institutions

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

- The collaborations and partnerships in this project are perfect for addressing all aspects of the project: material advancement, manufacturing, demonstration, cost modeling, and commercialization. Collaboration is a very strong aspect of this project.
- It is very clear that the current level collaboration is very broad and adequate to cover all the critical aspects, which will provide significant value to this project.
- The project has a good group of collaborators from industry, universities, and laboratories.
- The team for this work is appropriate.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- Developing technologies for underground storage of hydrogen is a very important aspect for enabling the introduction of hydrogen retail stations. Currently, it seems that one of the main issues with safety distances is related to large aboveground storage vessels, so this project is very relevant in advancing the hydrogen-for-mobility infrastructure.
- This project is expanding the work of another concrete overwrap project (PD-088)—this is a cost-reduction project. With respect to relevance, inexpensive forecourt high pressure is needed to facilitate the commercial deployment of hydrogen fueling stations. The potential impact is high.
- If successful in achieving the 60% cost savings, this technology could be highly impactful. The addition of some new partners, such as LightSail, could provide novel, low-cost vessel designs that might show lower manufacturing costs; a higher rate of manufacturing; and, almost most important, the ability to be easily repaired in the field.
- Existing commercial vessels already meet DOE targets.

Question 5: Proposed future work

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

- The focus on a lower-cost barrier layer, higher-strength steels, in situ inspection technology, and advanced reinforcement wires and wrapping techniques is very appropriate to meet the goals of the project.
- The proposed work for the next two years of this project looks complete.
- The project has a nicely detailed cost-reduction analysis. The overall plan to manufacture part of this in a factory and then move it to the site for the balance of the manufacturing is concerning. It is an interesting concept, but it might become bogged down during the execution.
- More analysis is needed before any construction is considered.

Project strengths:

- This project is largely well designed and well executed.
- The partner network is a project strength.
- The project builds on knowledge and experience from an earlier project.

Project weaknesses:

- There is strong competition from existing technology.
- There are significant concerns about the overall concept. These are the same concerns that apply to PD-088. If the concept was a direct burial tank, the tight contact with the surrounding ground would take up part of the load (if correctly designed and implemented). However, that is not the case; this is a stand-alone tank. This begs the following.
 - The concept is to use prestressed concrete. In this concept, the concrete is prestressed by steel wrappings. Concrete by itself has effectively no tensile strength; it is great in compression but not in tension. Prestressed concrete, however, can handle tensile loads as long as the tensile load does not exceed the prestressing from the steel wrappings. In other words, the steel takes the load of prestressing and the pressure load of the hydrogen inside the vessel. It seems that the concrete is not necessary; indeed, this system would be a lot simpler and less costly without the concrete, and presumably, the steel component can be reduced because of the lower tensile loads. This project has done a nice finite element method analysis, so presumably the case with and without concrete from a bill-of-materials and cost optimization standpoint has been performed.

Recommendations for additions/deletions to project scope:

- The PI should justify to the reviewers and DOE that a prestressed concrete structure has enough merit over and above a tank that does not include concrete.
- It may be possible to achieve significant cost reductions by using a hydrogen permeation barrier approach with traditional Type II and Type III vessels. Cost estimates need to be examined to be sure that the increased costs associated with labor in vessel construction are included. Before any construction activities are authorized, the project should do an analysis of costs for competing vessels and be sure that costs of concrete-reinforced vessels show a significant advantage. DOE needs to fully understand the benefits of a concrete/steel-reinforced vessel versus a Type II or Type III vessel with steel reinforcement. The team should consider an independent review of these technologies.

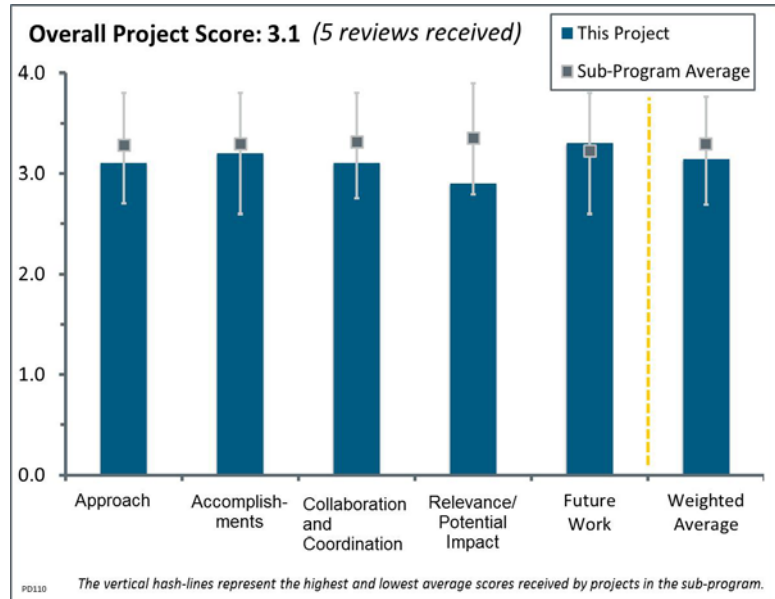
Project # PD-110: Low-Cost Hydrogen Storage at 875 bar Using Steel Liner and Steel Wire Wrap

Amit Prakash; WireTough Cylinders

Brief Summary of Project:

The overall objective of this project is to develop a pressure vessel with a capacity of 765 L to safely store hydrogen at 875 bar that also meets the U.S. Department of Energy (DOE) storage tank cost target of <\$1,000/kg hydrogen. The vessel must have a lifetime that exceeds 30 years/10,000 pressure cycles, have a safety factor of 3 on burst pressure to operating pressure, deliver hydrogen that meets SAE J2719 hydrogen purity requirements, and have design consistent with relevant American Society for Mechanical Engineers (ASME) codes.

Question 1: Approach to performing the work



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

- This is a new project for this fiscal year; it started in October. Objectives are consistent with DOE goals. The project is to take Type I tanks nominally used for compressed natural gas and hydrogen storage and overwrap them with high-strength steep wires, approximately doubling the pressure capacity (2*55 = 110 MPa). The tank will also be subjected to “autofrettage pressure” to elastically deform the internal tank, locking in compressive stresses on the liner’s interior surface. The project will first demonstrate this on small 1.9-meter tanks, then move to 9.5-meter tanks. This is an intriguing project. Hydrogen-assisted fatigue crack growth is a concern, particularly at the slow frequencies to which these tanks will be subjected. However, this team has performed a literature search on the topic, and the relevant literature would suggest that this may not be an issue. The team is referencing the Sandia National Laboratory (SNL) work, which is appropriate.
- Designing a bulk storage tank from high-strength steels, reinforced with high-strength steel wires, is a simple approach that could show reasonable benefit. There is significant experience with fatigue and embrittlement of austenitic steels, and working with SNL to understand the fatigue and embrittlement is the right approach. The project should incorporate some cost modeling to truly understand the cost trade-offs for the various materials and design considerations.
- The approach is a variant of the Type III tank design. It is a sound, derivative approach.
- The team is doing a good job of modeling tank performance to guide production and design. Burst testing of the vessel is promising. There were no economic analyses to indicate how much these vessels are likely to cost.
- The project is addressing the relevant DOE targets. With the approach taken, however, a significant improvement compared to other storage concepts cannot be expected.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- For a project that is only seven or eight months old, the accomplishments are quite impressive. The team has developed a finite element method (FEM) model for optimization, procured three tanks, completed

wire winding of one tank, initiated a burst test, initiated code approvals, and developed a fracture mechanics model. The burst test had to be terminated because of equipment failure and thus did not run to completion, so whether this technology will leak before burst is unknown. This concern was expressed during the review. Attention needs to be paid to ensure that this will indeed leak before burst. The progress is excellent for such a young project.

- The project started just recently. The team is off to a good start regarding addressing the realization and testing of the proposed concept.
- The accomplishments and progress to date are appropriate for this point in the project.
- The accomplishments thus far have been positive, although it is unclear whether the testing partner, Authorized Testing, will be able to test at the rated pressures. This will be key for future success.
- The project has made good progress in its initial phases. However, no techno-economic analysis was given.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.1** for its collaboration and coordination.

- The partners for this project are appropriate. The collaborators/collaborations are also very good.
- The project includes only a small team, but the relevant competencies are represented.
- The collaborations with Oak Ridge National Laboratory and SNL utilize the appropriate expertise. However, the project should also have a cost analysis and station design partner to provide an understanding of cost impacts at the station level.
- Reasonable collaboration appears to be occurring. WireTough is obviously working with ASME. This should be mentioned on the Collaboration slide.
- The collaboration and coordination do not seem to include any partners. The project is limited to national laboratories and a funding source. A station integrator with knowledge on end use and operating issues may be of assistance.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.9** for its relevance/potential impact.

- The technology could be relatively easily manufactured using a supply chain created by the tire industry. The technology could be subject to embrittlement, which would render it ineffective at these pressures; however, if the principal investigators can understand and mitigate the material embrittlement, then this technology could show significant impact on hydrogen delivery and meet the 2020 cost targets.
- If successful, this project will produce an inexpensive, high-pressure tank suitable for forecourt applications. Modifying existing Type I tank technologies to make them suitable for the forecourt applications is a good approach as long as hydrogen-assisted crack growth does not become an issue.
- The relevance/potential impact of this project is interesting. The potential for cost reduction and market acceptance is high. Wire wrapping may be more tolerant of local damage (e.g., suspension bridge cable).
- The project addresses an important topic of the Hydrogen and Fuel Cells Program. If successful, it may offer a lower-cost solution compared to other storage types.
- There is strong competition from carbon-fiber-wrapped steel vessels, which already meet or come close to meeting DOE targets.

Question 5: Proposed future work

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

- The proposed future work is spot on. There is concern about hydrogen-assisted fatigue crack growth, so it is excellent to see that significant attention will be paid to this problem, and consulting with SNL is appropriate. The development of non-destructive evaluation (NDE) technology for this application is spot on. The NDE will prove to be a very valuable tool as this project continues.

- Given the success at 1.9 meters, moving to 9.5-meter tubes is a reasonable extension of the work if the economics are promising. The effect of pressure cycling on cylinder lifetime needs to be examined and described. For carbon-fiber-wrapped steel cylinders, the depth of cycling has a very significant impact on vessel lifetime.
- The proposed work involves scale-up and ASME acceptance per code case. Both tasks are appropriate.
- A cost analysis task is highly recommended to gauge the cost savings and overall cost of the technology as the project progresses. There is no mention of this task currently.

Project strengths:

- This is an intriguing project. It is well planned and, so far, well executed. For such a young project, the progress is excellent. There are technical issues that need to be overcome; however, this team recognizes them and will be addressing these issues as the project continues. This is excellent.
- The strength of this activity is that it is a derivative concept based on well-known technologies (e.g., cable from suspension bridges and Type III tank design).
- The project has made a good start with analysis and construction of cylinders.

Project weaknesses:

- There are no weaknesses at this stage of the project.
- The apparent weakness is the assumption of a quick and successful campaign for an ASME code case.

Recommendations for additions/deletions to project scope:

- Technoeconomic analysis should be added. DOE should carry out an independent economic assessment of this technology compared to carbon-fiber-wrapped Type II vessels. The effects of pressure cycling on vessel lifetimes should be added.
- The project should focus on the code case acceptance. This may be a time-consuming process.

Project # PD-111: Monolithic Piston-Type Reactor for Hydrogen Production through Rapid Swing of Reforming/Combustion Reactions

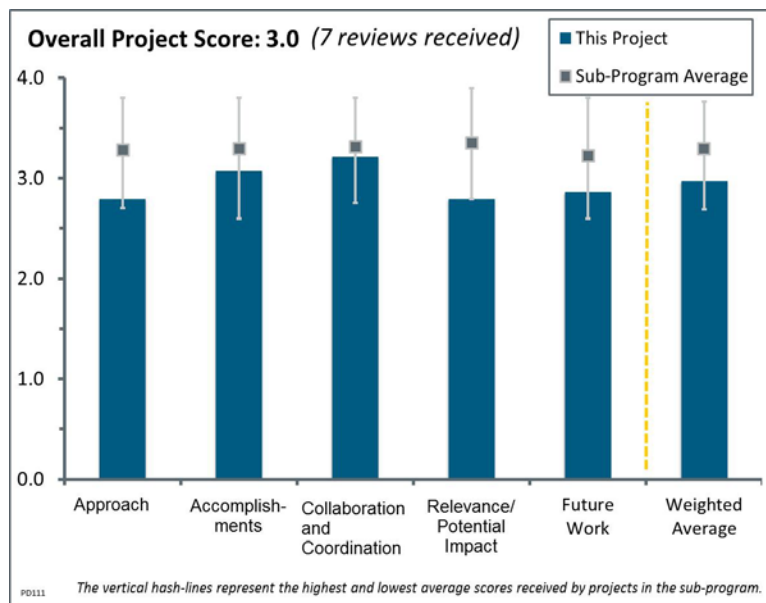
Wei Liu; Pacific Northwest National Laboratory

Brief Summary of Project:

This project is pursuing bio-oil reforming technology advancements. Pacific Northwest National Laboratory (PNNL) is working to (1) reduce the capital cost of plants by minimizing unit operations, decreasing pressure swing adsorption, and simplifying processes; (2) increase energy conversion through in situ CO₂ capture and in situ heat exchange between reaction and regeneration; and (3) increase durability by reducing operations and maintenance requirements.

Question 1: Approach to performing the work

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



- The project started in fiscal year (FY) 2015 with a well-thought-out and feasible plan of approach. The concept is innovative and challenging, but the barriers have been identified, and a research and development plan is in place to address them. The project builds on other Office of Energy Efficiency and Renewable Energy (EERE)-funded efforts, including past efforts addressing catalysts for bio-oil reforming. Additional attention to the cost and composition variations of bio-oil may be needed to fully address all barriers.
- This project has an effective approach to reduce hydrogen production cost using a simplified rapid swing reformer-combustion process. The process utilizes in situ CO₂ capture and heat transfer for improved efficiency.
- The major technical hurdles have been identified. However, it appears these issues are being addressed independently at this point, under the assumption that the developments can be readily meshed later. Introduction of the new laboratory system into the workflow should help in this regard. The team should consider devoting more resources to addressing the technical challenges associated with the catalyst—especially poison and coking tolerance when running on bio-oil—and the translation of laboratory discovery to a successful monolith. Clearly, catalyst performance has a high showstopper potential for the project. It would also be good to see high priority given to assessing the relative kinetics of hydrogen production, CO₂ diffusion into the sorbent, and mass transfer through the monolith. Results may force operation into a small parameter-space box. The assumed timeline (e.g., catalyst/sorbent system identified in FY 2015) is extremely aggressive.
- This effort was generally focused, with several challenging milestones laid out very methodically. The messaging was the right tone, especially because this effort is addressing multiple challenges. However, there is an issue that is a specific concern—the principal investigator (PI) provided and compared catalysis selectivity results with unequal conversions (i.e., 5%, 8%, and 70% conversions). It is absolutely incorrect to compare, side by side, selectivity results for a chemical reaction at different conversion levels. A catalysis expert should be consulted to add some clarity on the data collected to date. There was also some concern about operating the differential chemical (catalysis) reactor at greater than 5% conversion. When such a reactor is operated at these levels of conversion, heat and mass transfer effects creep in. These can be checked and verified as not being significant; however, these verifications were not provided.
- The project is in a very early stage, and it is the high-risk type of project the government should be doing. However, there is concern that the potential issues involving the use of bio-oils have not been well-thought-

out. For example, the potential for catalyst poisoning from the bio-oil sulfur and nitrogen complexes and the lack of a reliable bio-oil producer with controlled specifications add to the complexity and risk profile of the project. The presentation did not include a credible backup plan to address these risks, which is especially important because the catalyst may or may not be zeolite-based.

- This novel reactor design has the capability to make hydrogen from a low-cost feedstock and capture CO₂. Catalyst poisons in bio-oil have not been addressed. These poisons may make this project a non-starter. The PIs need to begin work quickly on mass and energy balances.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- Good progress has been made in a short amount of time. Literature searches have been completed, and candidate materials for catalyst and sorbent components have been identified. An integrated testing system has been designed for demonstration at 2 kg/day and for scale-up to 1,500 kg/day. If successful, the technology will offer a viable option to natural gas reforming for distributed production of hydrogen by reducing or eliminating capital and process costs associated with the high-temperature furnace, air separation, and water-gas-shift (WGS) steps.
- The level of accomplishments is high. The team has accomplished a truly significant quantity of work.
- The project started about six months ago. The team has made good progress on CO₂ sorbent identification, building the laboratory test system, and the first-draft technoeconomic analysis. It is not obvious whether good progress has been made on the catalyst.
- Given the short time frame for the project, the work over the last six months has been adequate. The initial screening of the CO₂ sorbents seems to be progressing nicely. The key concern is with the catalyst work. So far, the experiments utilize chemical-grade phenol as a model compound for pyrolysis; it is not clear whether this is a good substrate substitute for the myriad of minor complexes in bio-oil.
- The project is in its early stages. Sorbent materials have been identified for evaluation. Future progress will rely on “rapid” reaction kinetics and heat transfer during reaction and regeneration cycles.
- The progress is adequate for a project in its early stages.
- The laboratory results seem reassuring, but it is difficult to assess the chances of overall success without estimates of the capital cost of this equipment. The researchers’ goal of achieving less than \$4/kg depends on a 40% reduction in the cost of bio-oil. They need to describe the likelihood of reaching that bio-oil cost-reduction goal and what needs to happen in the bio-oil production process to achieve that cost reduction.

Question 3: Collaboration and coordination with other institutions

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

- PNNL is working with and leveraging staff and capabilities of the nearby Washington State University. Project partners include two small businesses that will lead the scale-up and commercialization of the technology if this project is successful. PNNL is continuing to reference and coordinate assumptions on bio-oil cost properties with other institutions (e.g., the National Renewable Energy Laboratory).
- The project seems to have a well-coordinated and aligned focus, with a good degree of collaboration. Further, this project aligns well with the Hydrogen and Fuel Cells Program and DOE objectives.
- There is good collaboration between the project members. Members are well versed in the fields of catalyst/sorbent material identification and reactor/process development.
- This appears to be a good team effort.
- There are four separate institutions participating in the project, each with specific relevant expertise.
- PNNL, as the central hub for the collaboration, seems to be doing a good job. It is not clear whether there are any direct interactions between other organizations; these should be encouraged, if they are not already happening.
- There is no apparent collaboration with experts on bio-oil properties and variability.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **2.8** for its relevance/potential impact.

- This project addresses and supports the goal of reducing the cost of distributed production of hydrogen from biomass-derived renewable liquids. Preliminary Hydrogen Analysis (H2A) model analysis shows that an approximate 50% reduction in the assumed cost of bio-oil feedstock would be required to achieve cost goals. It would be useful to see more discussion of how this would be accomplished and what limitations or boundaries this would impose on deployment of this technology. For example, if cost reduction is achieved by minimizing or eliminating oil transportation costs, it is not clear how that would limit use of this technology. If cost reduction is achieved through economies of scale, it is not clear what scale of bio-oil production would be needed.
- Reducing the cost of hydrogen production aligns with the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan. However, it is not clear the capital cost reductions are actually achievable because there is a great deal of reliance on the right level of thermal generation and transfer from the swing reactions to effect efficient reaction/regeneration while producing a high concentration of hydrogen without WGS. In addition, the feedstock costs remain unknown because no pyrolysis plant actually exists.
- The primary connection to hydrogen production goals is the use of bio-oil. A thorough comparison of this approach to steam methane reforming (at the same 1.5-ton hydrogen/day scale) would be helpful in judging its relevance to hydrogen production goals. It is not clear whether there are net CO₂ benefits. In addition to aggressive assumptions about reductions in capital and operating costs, given the complexity of the process being proposed, H2A shows a 50% reduction in bio-oil cost is required to meet the cost-of-hydrogen-production target. The latter should be addressed in another project.
- The DOE target for hydrogen production is <\$4/kg of hydrogen. Using this process, it is feasible to lower the cost of hydrogen production, but cost reductions may be limited by the cost of the biofuel used in the reaction.
- Most project aspects align with the Hydrogen and Fuel Cells Program and DOE research, development, and demonstration objectives. However, the impact could be greater with a better-planned approach.
- The relevance of small-scale, remote hydrogen production is questionable because of the large associated transportation costs for produced hydrogen.

Question 5: Proposed future work

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

- The project seems fairly well positioned to start integrating pieces. Execution will be key. Success of the process will require careful synchronization of multiple kinetics (e.g., reaction, diffusion, coking/decoking, and heat transfer). The team should develop a detailed process model to identify key bottlenecks and reasonable process operating points. It is not obvious when catalyst/sorbent system durability will be addressed (nor whether the replacement cycle has been included in H2A analysis).
- Reasonable project milestones were identified by project year. If it has not been submitted already, a schedule for down-selection of catalyst and sorbent materials for the integrated reactor system demonstration in Year 3 should be provided to DOE, as well as a risk mitigation strategy for this demonstration in case materials with needed performance properties are not found in Year 2.
- It would have been helpful to question Washington State University to get a better sense of the likelihood for success with its catalyst work. Coke completely deactivates the current Ni-based catalyst, so a plan is needed to develop an optimum reforming catalyst.
- The investigators should move quickly to work with real pyrolysis oils to assess factors such as coking, poisoning, coke burn-off, etc. Phenol is not a good model of pyoil.
- The project goals are to continue reducing capital cost, increasing energy conversion, and improving durability.

- The project is only 15% complete, and there were no slides explaining the future direction. The proposed plans to overcome some of the overarching challenges would have been good to see.
- There were no comments about determining the capital cost of this equipment (other than a bar chart with no details).

Project strengths:

- The project has an innovative design concept with the potential to significantly reduce the cost of distributed hydrogen from bio-derived liquids. The project partners include experienced researchers at PNNL as well as industry partners for development of components and reactor design. The latter also provide a possible pathway for further development and eventual commercialization.
- The sorbent part of the research is off to a good start. It appears the team has designed a system and purchased equipment.
- Strengths of the project include its novel approach to catalysis, separations, and process.
- The project features a well-defined cost analysis of the system economics.
- The project's novel design is a strength.
- The project features good laboratory testing experience and equipment.

Project weaknesses:

- There were no capital cost estimates to back up claims of achieving \$4/kg cost. There was no assessment of the likelihood of achieving a 40% reduction in the cost of bio-oil. It would also be good to see an assessment of the well-to-wheels (WTW) GHG emissions and water consumption for this project.
- The team has not found materials that meet the performance requirements for an integrated reactor system. Baseline materials to be used in a third-year system demonstration should be identified as a fallback option in case optimum materials are not identified in time.
- The cost of biofuel may limit this process in reaching DOE's suggested hydrogen cost target of less than \$4/kg of hydrogen.
- The project is tackling very significant technical challenges on all three fronts simultaneously. It has an overly optimistic timeline.
- No catalyst work has been done that demonstrates this part of the project will not be a showstopper.
- No clear future plans were elaborated.

Recommendations for additions/deletions to project scope:

- No major changes are recommended. The project could add value by briefly addressing the following topics during analysis and presentations:
 - The effects of changes in bio-oil composition on the performance of the catalysts
 - Reactor design modifications to allow for carbon capture, utilization, and storage (CCUS)
 - The added costs and restrictions, if any, associated with forecourt safety and handling concerns for bio-derived liquids
 - Modular reactor unit designs for "ganging" to provide semi-central production
 - Coordination with the EERE Bioenergy Technologies Office (BETO) to estimate production requirements for bio-oil to meet goals for biofuels and hydrogen production
- The researchers should carry out technoeconomic analysis for using this process not as a source of hydrogen for vehicles, but as a source of hydrogen for stabilization of larger amounts of pyoil in the field. This may be more appropriate for work with BETO, but it is likely to have better economics than producing small amounts of hydrogen remotely and then transporting it to areas with hydrogen demand.
- The team should focus on a robust catalyst/sorbent system and process modeling over the three-year project timeline. It should leave the fully integrated demonstration unit for a later phase.
- The project team should add a task to assess the WTW GHG emissions and water consumption. It should also add a task to estimate the capital cost of this system.
- The team should put more resources into catalyst development.
- The team should conduct an investigation of lower-cost fuel sources that may be applicable to this process.

Project # PD-112: Reformer-Electrolyzer-Purifier for Production of Hydrogen

Fred Jahnke; FuelCell Energy, Inc.

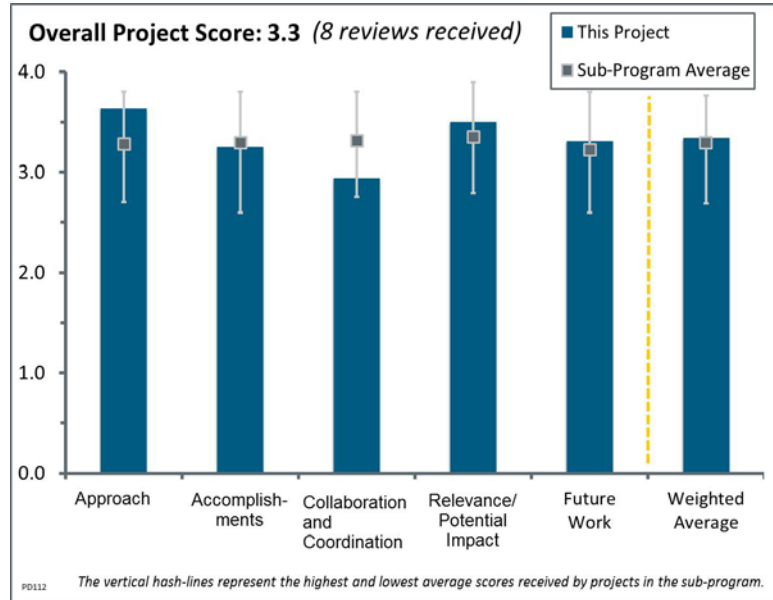
Brief Summary of Project:

The objective of this project is to demonstrate reformer–electrolyzer–purifier (REP) technology based on FuelCell Energy’s (FCE’s) proven molten carbonate fuel cell (MCFC) technology as a cost-effective and efficient method for producing hydrogen from natural gas. Potential REP technology benefits include the low cost hydrogen production, significantly reduced CO₂ emissions compared to steam methane reforming, and scalability from home refueling to central production sizes. In addition to research and development (R&D), cost analysis will also be undertaken.

Question 1: Approach to performing the work

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

- FCE has a solid track record of creating systems that meet multiple needs and are, by necessity, complex. This system has the potential to reduce the carbon footprint of hydrogen produced from natural gas. The project leverages FCE’s proven MCFC technology, which is a plus. The complexity and novelty of the system may prove to be a detriment when the system is integrated into a commercial system. It would be good to see some reliability predictions and mitigations for a final commercial system.
- This project has the right focus on demonstrating long-term performance, given that it is essentially running existing hardware (in reverse), so there is no need to focus on materials and process development. The project should consider directly addressing the impact of upstream (sulfur removal) and downstream (compression) processes, even if they are “off-the-shelf.” It is not insignificant that the feed must be sulfur-free (it would be good to know how many parts per million of sulfur can be tolerated), nor that the hydrogen produced is at atmospheric pressure (it would be good to know how compression to 700 bar would affect the economics).
- The project team’s extensive commercial experience with designing, building, installing, and operating MCFCs gives it the in-depth understanding of the processes involved and the changes and developments required to convert the fuel cell technology to operate as an electrolyzer.
- Because this project modifies existing commercial fuel cell equipment to create the electrolyzer and already has a patent filed, it presents a lower-risk profile and appears to be quite feasible.
- The project has a defined approach to modeling, optimizing, and developing a combined reformer/electrolyzer for hydrogen production.
- This project was sharply focused and addressed the critical barriers; it would be difficult to improve the project significantly.
- The work scope is well defined with no major weaknesses. The project builds on successful MCFC technology.
- The project uses a novel approach to production using existing products with modifications.



Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- There has been excellent progress to date on single-cell testing. The focus to date has been on continuous operation at load. Results suggest that long-term durability does not look to be a critical issue at this time. Hydrogen purity of ~98% has been demonstrated. It would help if the milestone deliverable dates were defined. It is hard to determine which tasks have been started and completed and which milestones have yet to be started.
- The voltage stability of the cell over time is good. This testing needs to continue out to commercially relevant lifetimes. The funding opportunity announcement (FOA) under which this project was awarded, DE-FOA-0000826, Topic 1, has a target for greenhouse gas (GHG) emissions: 5,500 g CO₂e/gge H₂. The team needs to be explicit about where it stands relative to that goal. The team members state that REP can meet 5 kg CO₂/gge, yet they present no evidence of the system's ability to meet this number. A refined GHG analysis needs to be presented every year based on the available test data. The cost analysis states the system can meet \$1.4–\$2.2/kg H₂. This is based on a questionable assumption of free heat input. This project, like some others in the Hydrogen Production portfolio, makes the unfounded assumption that high-quality free heat will be available. Because high-quality heat has value to those who produce it, it is unlikely that free heat in the required quality will be available. The team needs to make realistic assumptions about the cost of the heat input and the effect of that on the cost of hydrogen.
- The accomplishments reveal the result of good collaboration. It is clear the partners participate and are well coordinated.
- The project has shown good progress on a subscale (single-cell) level. The completion of optimization studies related to heat transfer can further improve performance and cost. However, hydrogen purity and the effect on system economics require a more detailed investigation.
- The long-term operability testing and performance modeling efforts are appreciated. It is not clear whether the project is on track to finish the work plan in the next year or so, but the track record has been good.
- The resulting capital costs are not clear. The speaker was hesitant to provide numbers. However, the system does look to be scalable with low emissions, with a projected hydrogen production cost between \$1.38 and \$2.18 at 1,826 kg per day.
- There is good progress for this early stage project.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.9** for its collaboration and coordination.

- The degree to which the project interacts with other entities and projects is outstanding. There is close, appropriate collaboration with other institutions. The partners seem to be strong participants, and the efforts are well coordinated.
- To be honest, FCE does not need that much collaboration—it is the expert in this field—but the University of California, Irvine, (UCI) provides some analytical heft to the project.
- FCE has most of the expertise needed internally, so collaboration is not so important in this project.
- This project has only one collaborator that is focusing its efforts on modeling and cost studies. Further detail in the cost analysis needs to be completed to determine overall process feasibility. That being said, the principal investigator (PI) has developed systems for industrial use and is capable of completing the work on the reformer/electrolyzer technology.
- The FCE team appears closely integrated with UCI, which is handling the bulk of the analysis work. This partnership should be leveraged to address project weaknesses.
- It is not clear what level of support UCI is receiving. The work scope of the UCI National Fuel Cell Center is extremely broad and seems to be somewhat unfocused. As such, it is difficult to evaluate the expected benefit of all activities. For example, it is not clear what the expected outcome is of evaluating all of the external heat sources; i.e., whether this information will be beneficial in determining the most appropriate targeted markets for deploying this technology, or whether this information will be beneficial in accelerating deployment (or at least demonstration tests) in the most appropriate market(s).

- It is not obvious how the single partner, UCI, is integrated into the project, other than apparently helping with the Hydrogen Analysis (H2A) modeling work. UCI could help in a direct comparison to a steam methane reforming (SMR) case. Now is the time to identify a full-scale demonstration partner.
- The only partner is UCI. It would be good to have a potential customer already signed up to assist with third-party field testing (e.g., a member of the U.S. Drive Partnership's Hydrogen Production Technical Team or the California Energy Commission).

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- This is the project with the highest chances of achieving the \$2/kg DOE goal—and probably the only approach that can even suggest the possibility of home refueling.
- The system has great potential for alternate uses such as renewable energy storage, chemical conversion, or CO₂ capture. The simulation models provided promising results.
- This project is a unique and creative way to generate low-cost, low-carbon hydrogen. It is important that this project move forward to commercialization—but only to the extent it can meet cost, GHG, and reliability targets. Progress toward these targets must be clearly analyzed and updated every year.
- This project can significantly advance progress toward DOE cost targets for hydrogen production when low-cost heat sources are available.
- This project represents a strong near-term opportunity in the Hydrogen Production portfolio. It would benefit from comprehensive comparison to SMR, given that both make hydrogen from natural gas. The project should, on the same kg H₂/day basis, compare capital and operating costs, efficiency, CO₂ footprint, heat/electrical load, and other pros/cons.
- If successful, the project could potentially provide an economical and technological solution for home refueling.
- This could be a low-cost route to hydrogen and electricity.
- The identified markets span an extremely broad level of production, and it is not clear the technology will be competitive at all production scales that are being targeted.

Question 5: Proposed future work

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

- The future work for the project seems to be well thought-out and includes confirming system economics, large-scale modeling, waste heat identification for system optimization, fuel flexibility analysis, and commercialization.
- The project has a defined path forward and includes scale-up to a large multi-cell stack platform along with an updated hydrogen production cost analysis.
- The outlined proposed future work was sharply focused.
- The PIs have outlined a reasonable plan for moving forward.
- The proposed future work for FCE is well structured to meet overall project objectives, although it would be good to see some effort focused on performance under dynamic operating conditions. The future work for UCI needs to be better focused, with projected outcomes better defined.
- It is not clear whether upstream (sulfur removal) and downstream (compression) costs have been factored into the H2A analysis—if not, they should be. It would be good to see more thorough consideration of the opportunities to use the CO₂ in the waste stream. It would be good to see a case developed in which there is integration with electrochemical compression. The project team should consider identifying what would limit large-scale deployment (e.g., availability of materials, production capacity, and/or adequate quality waste heat).
- The parametric H2A analysis needs to address the cost of heat input. The future work plan is reasonable.

- It would be good to see a clear listing of capital expenditures (capex) for the proposed stand-alone hydrogen generator. The current description is confusing because the project is utilizing the equipment from the MCFC (de-sulfurizer, etc.).

Project strengths:

- The project builds on existing MCFC technology, which has accelerated this project's R&D progress. For example, a 3,000-hour durability test has already been completed on single-cell. Development of new technology is not required, thus accelerating the time to market. The projected long-term cost of hydrogen is less than \$2/kg. The technology is projected to meet the DOE CO₂ emissions target of <5,000 g/gge, although it is not clear how this target is met.
- The system utilizes a combined reformer and electrolyzer stack that has the potential for low-cost hydrogen production and CO₂ capture.
- This is good lower-risk, near-term technology—it is effectively an advanced SMR process.
- It is a unique project with good potential to address cost and GHG goals.
- FCE has excellent commercial experience with MCFCs. This is an experienced scientific and engineering team.
- FCE has strong expertise in fuel cell manufacturing, which is transferable to development of the REP.
- The strong expertise at FCE is a real plus for this project.

Project weaknesses:

- The project targets production levels from 2 kg/day up to 16,000 kg/day. The PI should calculate the capex costs at the residential scale (2 kg/day), at the service station scale (1,500 kg/day), and at 16,000 kg/day and compare these costs against conventional SMR. Capex costs should consider all aspects of the cost, including balance of plant. For residential use, it is not clear what the payback period is or whether the payback time is reasonable such that there would be a market demand at this scale.
- Project execution needs to be strengthened to address the goals against which the project is measured.
- Economic analysis and quantified advantages over SMR are needed.
- FCE needs to close the deal with a third-party field tester in multiple climates.

Recommendations for additions/deletions to project scope:

- If increasing pressure were possible, this would be a very good addition. The standard H2A case for hydrogen production assumes hydrogen delivered from the system at about 300 psig. Costs of compression to this pressure need to be included in any technoeconomic analyses. The PIs should show technoeconomic analysis for stand-alone systems, along with similar analyses for locations with usable waste heat. The sources of waste heat need to be clearly identified and quantified. One specific analysis that would be useful would be for an early station with an ultimate capacity of 500–1,000 kg H₂/day, but the station could produce mostly electricity in early years with low demand and then swing production to more hydrogen as demand increases.
- The project should calculate capex cost at three production levels—residential, service station, and centralized production—and compare these costs against conventional SMR at these scales. Smaller-scale production rates will probably require that the system operate with frequent shutdowns and start-ups. Dynamic operation—including frequent shutdowns/start-ups—and its effect on long-term performance should be investigated. The project should define the appropriate scale for a demonstration and begin focusing R&D efforts toward preparing for demonstration. CO₂ emissions per kg H₂ produced should be quantified, including the CO₂ associated with external heat and electricity production.
- At every DOE Hydrogen and Fuel Cells Program Annual Merit Review, the team should present a single slide showing where the REP technology stands relative to GHG emissions (5,500 g CO₂e/gge target); cost using realistic assumptions, especially for heat input cost (\$1–\$2/gge target); and durability/reliability progress. This slide must be backed up by analysis, not merely a conclusory assertion that the goal is being met. It is insufficient to meet only one of these targets. As the FOA says, “Pathways to meet *both* the greenhouse gas reduction requirement and the production cost goal of \$1–\$2/gge must be clearly identified...” (emphasis added).

- A detailed economic analysis needs to be performed—specifically including the cost of external heat sources required in the reformer/electrolyzer technology. Hydrogen purity should also be considered in the analysis.
- The project should provide a detailed cost estimate of a stand-alone electrolyzer hydrogen generator system.
- Unfamiliar and undefined acronyms in the presentation (e.g., HMB, SOPO) took away from the message in some cases.

Project # PD-113: High-Efficiency Solar Thermochemical Reactor for Hydrogen Production

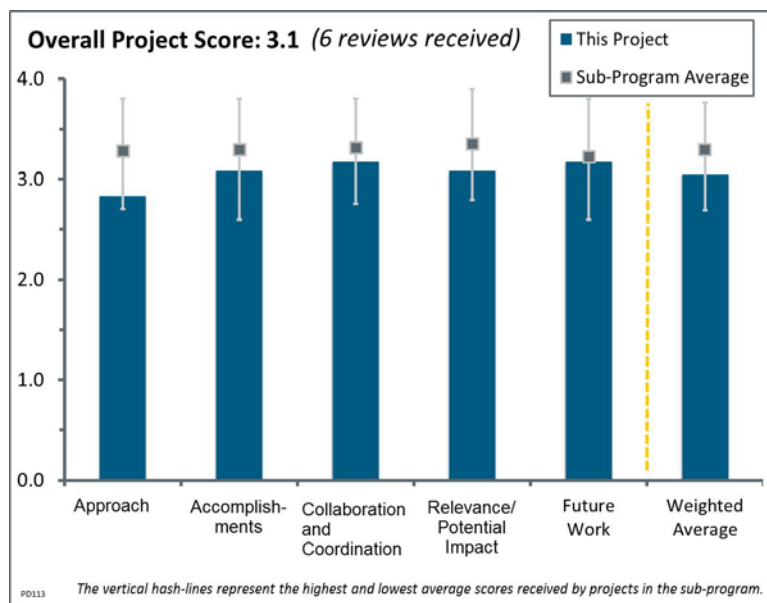
Tony McDaniel; Sandia National Laboratories

Brief Summary of Project:

The objective of this project is to develop and validate a particle bed reactor for producing hydrogen via a thermochemical water splitting cycle using a non-volatile metal oxide as the working fluid. Sandia National Laboratories (SNL) will demonstrate eight continuous hours of “on-sun” operation, producing more than 3 L of hydrogen.

Question 1: Approach to performing the work

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



- The approach taken by this project is excellent to outstanding. It is not purely outstanding only because of the lack of any attention in the plan to materials durability analysis and testing. The reviewer-only slide on “Critical Assumptions and Issues” suggests establishing collaborations with a number of institutions with the capability to develop atomistic understanding of materials behavior in the extreme environments of this process. That is an important effort that should be given higher priority than the present plan. The project excuses this deficiency on the grounds that more fundamental research is needed than is possible to conduct in this project, but if this issue is a potential showstopper (and it might be), the project should engage it sooner rather than later. Apart from this single point, the planning and approach in this project are outstanding.
- The overall project objective is to develop and validate a particle bed reactor for producing hydrogen via a thermochemical water-splitting cycle using a non-volatile metal oxide (potentially CeO_2) redox material as the working “fluid.” The project will demonstrate 8 continuous hours of on-sun operation, producing more than 3 L of hydrogen. The fiscal year (FY) 2015 effort is focused on identifying (discovering) suitable redox materials, designing a particle receiver-reactor for 3 kW operation, and conducting system modeling. An overall solar-to-thermal efficiency of $>5\%$ is targeted in the short term.
- Although the approach of coupling both new material discovery and efficient solarthermal reactor development is attractive, the probability of achieving both goals is very slim. There is a mismatch in terms of both development readiness and project timing needed to have any chance of success. The project objectives seem almost independent of each other because there is no milestone for material development, and reactor design goals are expected to be achieved with existing materials SPLM or CeO_2 . In that respect, the project’s goals, budget, duration, and skill sets do not seem well aligned.
- Stability and kinetics remain important but lesser-explored parts of this work. However, it was indicated that discovery of a new material with the desired redox and thermodynamic properties was a stretch goal, and so that material must be discovered prior to evaluation of the stability and kinetics.
- The keys to solar thermochemical (STCH) are sun-to-heat efficiency and heat-to-hydrogen efficiency. The project’s attention to both material and reactor developments is laudable. The efforts on process modeling and techno-economic analysis are also good. However, the technical challenges associated with the two key efficiency issues are so substantial that diluting the team’s focus to design and build the solar simulator unit, on the stated timeline, may risk yielding only a hit-or-miss result. The principal investigator (PI) asserts the only way to know whether the chosen STCH approach will work is to build that unit. This is concerning. Some thought should be given to how to build more confidence in the likelihood of success.

- Barriers have been identified and addressed, and the technical approach to materials design and laboratory reactor testing is feasible, if challenging. The project partners have well-defined roles in contributing to the success of the project. However, the scope of the project does not seem appropriately scaled to the project duration and available funding.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

This project was rated **3.1** for its accomplishments and progress.

- This project's accomplishments have been excellent. So far, material exceeding the expected performance of SLMA has not been found. Nevertheless, the advances in screening methodology have been significant, and the addition of entropy considerations in modifying potential candidates shows promise. The CPR2 remains an innovative and intriguing concept. The additional design work for managing material transport is interesting, although the proposed vibrational plates will likely add to the hostile environment for particle attrition. Even if the particles remain active, attrition could lead to performance degradation of the solar thermal interface windows if direct irradiation is implemented. System design options include direct or indirect particle irradiation and beam-up or beam-down configurations for solar thermal collection. Direct particle irradiation is likely more efficient, given the vacuum effects on heat transfer, but it is susceptible to particle attrition effects. Design options are partly driven by material durability in this environment. The improvements to the Hydrogen Analysis (H2A) v3 model are exemplary.
- Good progress has been made in qualifying the CPR2 for testing and in expanding the model of CPR2 and solar field performance. Materials discovery work is continuing, but it may take extended investigations beyond the scope of this project to find an optimal material of reaction with the thermochemical and performance parameter values desired to achieve solar-to-hydrogen (STH) efficiency of >20%. An innovation added this year has been to track and engineer materials system entropy as well as the onset reduction temperature and repeatable redox potential.
- Synthesis and screening of candidate redox materials is underway. Material selection will have a significant impact on capital cost and the levelized cost of hydrogen. Advanced analysis related to redox behavior (e.g., quantum theory and entropy considerations) is being used to guide discovery of high-performance redox materials. However, it is not clear from the presentation whether these analytical methods have actually led to discovery of any high-performance redox materials. Various perovskite doping strategies are also being evaluated. Other accomplishments include completion of a conceptual design of a 3 kW prototype reactor/receiver using simulated solar (lamp arrays). Technoeconomic analysis has been refined for a 100,000 kg/day plant. System analysis has been performed using Matlab. The exothermic nature of the oxidation step should be emphasized; it is not clear how the heat rejection is accomplished.
- The project has reported modest accomplishments, given that the STCH approach is extremely challenging and still at a very early stage of development, despite decades of research on this topic.
- The project's progress is difficult to judge. The presentation described mostly activities and "design criteria established"-type results, but it did not clearly translate those results to progress made on meeting technical targets. For example, it is not apparent how the current best redox material performs against the stated hydrogen/gram goal or the overall STCH efficiency goal, although it is noted that 0% progress has been made toward meeting the former. It would be good to see more concrete descriptions of accomplishments and progress.
- It is too early in the project to critically assess progress and the likelihood of success.

Question 3: Collaboration and coordination with other institutions

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

- The project team is excellent and leverages materials experience and reactor design capabilities at SNL – Livermore, and SNL – Albuquerque, respectively; academic materials discovery capabilities at several key universities; and access to DOE-Office of Science facilities. The status of work with the German Aerospace Center (DLR) needs to be resolved, and project plans and scope should be adjusted accordingly. In

addition, this project offers the team an opportunity to provide leadership in coordinating across the community to establish conventions for analysis, best practices, and key measurements.

- The collaboration and coordination with other individuals and institutions is good to excellent. The collaboration in systems analysis is particularly interesting and represents a sort of one-of-a-kind activity. It is unusual for a technology team to turn its attention outside its boundaries of control for analysis. This project deserves commendation for its open nature in this regard. A missing ingredient is collaboration with an entity with existing capability for detailed atomistic-level characterization in an effort to understand the relationship between hydrogen production capacity and active particle durability. Such capability exists and is easily engaged, as shown effectively by the photoelectrochemical community. Collaboration and coordination would be improved if rapport could be established between SNL and the university STCH project. Both teams are pursuing very similar objectives using very similar general approaches and using materials derived from the same general class. Both teams appear to have chosen competitive—instead of cooperative and collaborative—postures. The program goals would be far better supported if these two institutions could work together to solve their common and challenging problems.
- Project collaboration is broad, including DLR (solar receiver), Arizona State University (technoeconomic analysis), Bucknell University (particle heat transfer), the Colorado School of Mines (materials), Northwestern University (quantum theory), Stanford University (entropy engineering), and CoorsTek (production of large batches of redox material).
- The project features an extensive set of collaborators that seem to be integrated into the team.
- Juggling this many collaborators is a challenge that seems to be going well. However, finalizing efforts with DLR has been a struggle.
- This project demands a wide range of skill sets—material synthesis, computation, reactor design, high-temperature (HT) material expertise, solar field design, economic analysis, etc. Therefore, effective collaboration is even more critical than in other hydrogen production projects. It is surprising the project team is not collaborating with the university STCH project. At the minimum, the project could benefit from reasonable knowledge sharing on the HT redox material screening and discovery effort.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.1** for its relevance/potential impact.

- The relevance/potential impact of this project is excellent to outstanding. The magic material has yet to be found, and whether it can endure the harsh process environment remains to be determined. Nevertheless, the project has done an outstanding job in developing screening tools, general analysis of performance, and reactor concept development.
- The project supports the Office of Energy Efficiency and Renewable Energy (EERE) Fuel Cell Technologies Office (FCTO) goal of reducing the cost of hydrogen production from renewable resources to <\$2/kg by 2020. It also supports the objective to verify the competitive potential for STCH cycles for hydrogen in the long term by 2015, and by 2020 to develop this technology to produce hydrogen. Meeting the cost goal will depend on the successful completion of this project, but also on technology development beyond the scope of the project and FCTO (e.g., lowering the cost of the heliostats to the DOE Solar Program SunShot target).
- Development of any efficient, cost-effective direct solar processes for water splitting has the potential to significantly expand the role of solar energy. This STCH technology represents one possible pathway for direct solar hydrogen. However, it is fraught with several extremely challenging technical issues, including the performance of the redox material, circulation of very HT solid particulates, selection of very HT reactor materials, radiative heating of solid particles, etc. Furthermore, the potential for high-efficiency performance is limited.
- Development of a successful STCH approach is well aligned with DOE hydrogen production goals. However, the likelihood of reaching the cost of production goal is extremely low without a breakthrough in redox material, and there has been no clear progress on that front.
- The project is relevant in reaching the ultimate DOE targets for STCH, whether the project goals are achieved or not.

- This project all hinges on a longshot for materials discovery. Leveraging the Materials Genome Initiative (MGI) is a clear advantage.

Question 5: Proposed future work

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

- The proposed future work is excellent-to-outstanding. It is not purely outstanding only because of the lack of any attention in the plan to materials durability analysis and testing. The reviewer-only slide on “Critical Assumptions and Issues” suggests establishing collaborations with a number of institutions with the capability to develop atomistic understanding of materials behavior in the extreme environments of this process. That is an important effort that should be given higher priority than the present plan. The project excuses this deficiency on the grounds that more fundamental research is needed than is possible to conduct in this project, but if this issue is a potential showstopper (and it might be), the project should engage it sooner rather than later. Apart from this single point, the planning and approach in this project are outstanding.
- The project is short and aggressive, so there are a lot of high-risk milestones ahead, but the future work is appropriate for this short of a project period.
- The proposed work is reasonable within the context of the inherent thermodynamic and kinetic challenges.
- The remaining challenges were identified, and research and development (R&D) was outlined for the remainder of 2015 and fiscal year (FY) 2016 for the areas of materials discovery and optimization, CPR2 design and fabrication, and technoeconomic analysis. A reasonable mitigation strategy was presented for completing the project if an optimal redox material with the potential for >20% STH efficiency is not identified. The project is more than one-third over, and there appear to be continuing uncertainties regarding the DLR partnership. This issue should be resolved so the team can plan the remaining project work more effectively.
- The team should increase focus and resources in two areas critical to overall efficiency: (1) finding a material with much higher hydrogen production efficiency, and (2) engineering a way to significantly improve solar-to-particle heating efficiency. The team should sacrifice resources on design/build of an integrated unit to pursue those two areas. The team should develop a model to relate single-particle efficiency (i.e., grams of hydrogen/grams of metal oxide) to overall process efficiency (i.e., tons of hydrogen/day).
- For FY 2015, the team will continue the search for redox material and optimization. The reactor and solar field design will be finalized. For FY 2016, goals include production of 100 kg of redox material, on-sun testing of CPR2, and completion of a full technoeconomic analysis.

Project strengths:

- The project features an excellent project team that has the experience and available facilities to do the work. The project builds on experimental and modeling advances made in previous EERE-funded projects, including decades of reactor design experience at SNL.
- Project strengths include (1) addressing the need for integrated materials and reactor development and (2) taking a fundamental approach to discovery of a novel redox material.
- The facilities and skill sets in this project are outstanding. The project planning and execution are outstanding.
- The project supports some very interesting basic materials science. The collaborations are impressive.
- The team’s understanding of HT reactor systems is an area of strength.

Project weaknesses:

- The extreme technological challenges of this project demand consideration of alternatives. Even if consideration is restricted to purely solar-based hydrogen production technologies, it must be recognized that STH efficiencies of at least 18% can be achieved today with commercially available technology using state-of-the-art photovoltaics providing electrical power directly to conventional water electrolysis units. With HT steam electrolysis, which is at a relatively advanced stage of development (Technology Readiness

Level 5), an STH efficiency of at least 30% should be achievable using concentrated solar heat for the required HT heat addition (at 800°C instead of 1500°C) and state-of-the-art photovoltaics, even without system integration/optimization. This efficiency is three-times higher than the 2015 case associated with the metal oxide thermochemical cycle and higher than the ultimate efficiency predicted for this concept. While efficiency is not the only consideration (the bottom line is dollars/kilogram), it is hard to foresee a scenario in which this technology could be deployed as a practical, large-scale renewable hydrogen production platform. In terms of economics, as pointed out at the DOE Hydrogen and Fuel Cells Program Annual Merit Review by a reviewer, the cost of the HT solar receiver technologies alone may preclude achievement of low-cost hydrogen production using this technology.

- This would be an extremely large, amazingly complex plant. Fundamentally, it is a very challenging route to cost-effective hydrogen. Using commercial operation of cement manufacture and gas turbines to rationalize the feasibility only makes one question whether the challenges are sufficiently appreciated.
- The materials discovery work may be an ever-expanding universe of investigations, rather than one converging on a viable solution for CPR2 testing during the scope of the project. The primary focus for this project should be performing the reactor tests and demonstrating achievement of the project objective to produce 3 L of hydrogen in 8 hours.
- The level of collaboration in this project is good to excellent. The team needs to establish ties with an entity that has advanced characterization capability to explore the limits on active material durability. The project would benefit enormously from better coordination and collaboration between SNL and UC.
- Weaknesses include not fully recognizing the long-term nature of the concept and not tailoring the project scope accordingly.

Recommendations for additions/deletions to project scope:

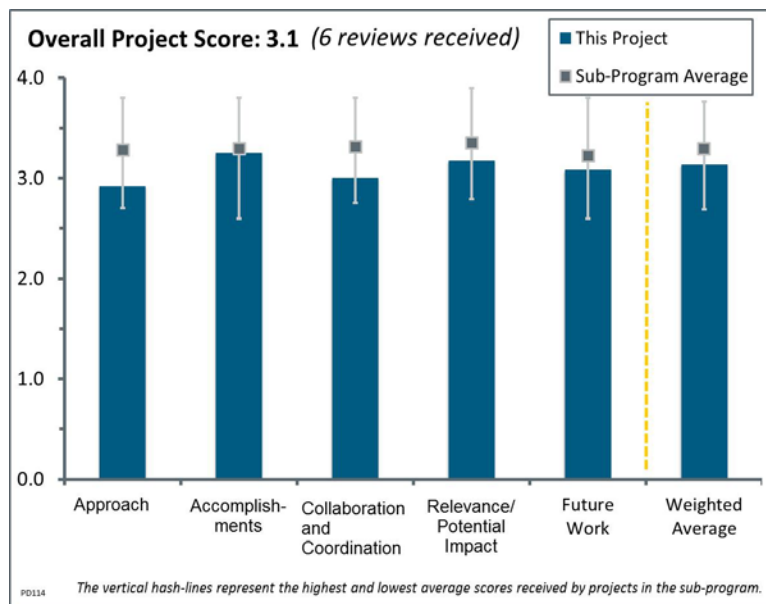
- The team should adjust the project scope as needed—in particular, the materials discovery activities—to match the time and resources available (including the absence of DLR contribution, if necessary) for this project. The team should also leverage past modeling and thermochemistry/kinetics testing to focus on a few key parameters/descriptors in order to meet project performance goals in 2016. If necessary, the researchers should rely on the mitigation plans presented to meet the project objective of 8 hours of continuous operation, under on-sun conditions, to produce more than 3 L of hydrogen. Materials discovery to identify optimal reaction material is important and exciting, but it can continue under future projects if an optimal material is not found in this project.
- Because this approach is in such an early stage of development, perhaps DOE should consider splitting the material discovery and the reactor teams into separate projects so the researchers can focus on their respective objectives without the unrealistic expectation of impacting each other's effort in the duration of the project. However, the two teams should continue to interact and share results with longer-term DOE goals in mind.
- The team should be clearer in terms of its key technoeconomic assumptions—these will make or break the eventual commercial success of the approach and need significant effort to meet. It is unclear what the current status is, how far away the target is, and what the plan is to close the gap.
- It is absolutely critical that a fully functional, integrated, on-sun, long-duration demonstration of this technology is completed as soon as possible. Successful achievement of such a demonstration would go a long way toward justifying further investigation of this concept.
- Active materials R&D should include some detailed investigation of the effects of kinetics and oxygen vacancy levels on active material bond strength for better understanding of the relationship between production capacity and material durability.

Project # PD-114: Flowing Particle Bed Solarthermal Redox Process to Split Water

Al Weimer; University of Colorado

Brief Summary of Project:

The overall objective of this project is to design and test the individual components of a novel flowing particle solarthermal water splitting system capable of producing 50,000 kg of hydrogen per day at a cost of <\$2/kg of hydrogen. Further objectives include (1) identifying and developing high-performance active material formations; (2) synthesizing flowable, attrition-resistant, long-use spherical particles from low-cost precursors; (3) demonstrating high-temperature (HT)-tolerant, refractory, non-reactive containment materials; (4) constructing a flowing particle redox test system and testing components of the system; and (5) monitoring progress toward cost targets by incorporating experimental results into frequently updated detailed process models.



Question 1: Approach to performing the work

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

- The project is a follow-on effort to previous work funded through the Office of Energy Efficiency and Renewable Energy (EERE) Fuel Cell Technologies Office (FCTO). The technology being developed in the current effort includes an innovative new reactor design for solar thermal water splitting. The project approach (including identification of materials of reaction, synthesis of reactive particles, design and demonstration of HT containment materials, iterative updating of an Aspen process model and the Hydrogen Analysis (H2A) model, and on-sun testing at the National Renewable Energy Laboratory [NREL]) is to test and validate the performance of components of the new design to move the technology concept from Technology Readiness Level (TRL) 2 to TRL 3. This approach is feasible and logical, and it includes integrating efforts from a U.S. Department of Energy (DOE)/National Science Foundation (NSF)-funded materials discovery project. Challenges and barriers were identified and will be addressed in future work. More information on the research and development (R&D) status and specific R&D plans for characterizing particle attrition resistance and high gas-gas heat recuperation would have been useful because these factors are critical to the success of the technology.
- The approach described in this project is good to excellent. The effort in materials development is outstanding, providing a high-throughput methodology for candidate active materials identification. At the same time, materials durability remains an unresolved issue. The barrier associated with chemical reactor development is addressed, but not comprehensively. Key issues such as the use of an inert gas sweep within the reactor have been addressed, but the use of a common cavity for both the oxidation and reduction reactions is problematic, unless the cavity is evacuated. Design and modeling are proposed, but absent a vacuum environment, it will be impossible to achieve anything like separately uniform temperature environments for the two reactions. Departures from uniformity mean some portion of the reactors will be at higher-than-required temperatures and some portions will be at lower-than-required temperatures. Process efficiency and the cost of active material reservoirs will suffer from a common chamber design. The approach provides for some integration with theoretical work that materially aids the identification of promising active materials, but key material characterization that could help assess the trade-off between production capacity and material durability is lacking. Material characterization capabilities are highly

specialized and require access to technology well beyond the scope of most institutions. This project would be improved by collaborating with existing characterization capability, with the aim of developing a basic understanding of the relationship between activity, reactant oxygen vacancy levels, and material durability. There is no guarantee that needed production capacity is compatible with material durability.

- The project features a good balance between fundamental understanding of materials, proof-of-principle laboratory testing, and process development. The project team might be taking the challenge of complex solar reactor design too lightly, given the importance of high efficiency for converting sunlight to very hot particles in the reduction zone. The team should maintain a heavy focus on finding redox materials with improved performance; the researchers should not let this focus slip.
- This project focuses on the development of innovative, HT solar thermochemical (STCH) water splitting processes based on a cobalt ferrite/hercynite redox cycle. The overall project objective is to design and test components of a flowing-particle solar-thermal water splitting process for 50,000 kg/day hydrogen production. The specific approach includes production and characterization of reactive materials and engineered particles, design of a particle flow reactor, development of thermal-shock-resistant containment materials, and demonstration of on-sun performance. Goals for this project include construction and operation of an HT (1500°C) particle flow redox system and demonstration for reduction/oxidation of >1 gm of active material. This project appears to be a direct continuation of PD-028, which ran from 2005 to 2014.
- Designing the reactor so that it is located in the receiver seems like an enormous technical challenge. The remainder of the proposed project seems logical, although the stability of the particle and reactor components must be addressed in more detail. Also, the precipitous drop in projected cost from the H2A model with moderate changes in parameters is shocking.
- H2A indicates the heat exchanger is key for the success of the system, but there does not seem to be a large emphasis on its development. The reactors and particle storage at the top of the tower will necessitate a very large tower that appears to be expensive. The proposed design has many obvious problems, such as heat transfer, material compatibility, challenges with HT valves, construction, etc., and should be reconsidered. The researchers are proposing to coat their large reactors with SiC for material compatibility with the redox environment. The use of a fluidized bed approach with SiC raises concerns. The fluidized bed will be similar to sandpaper wearing on the SiC, raising questions about durability and lifetime. Based on the design proposed and the H2A presented, the focus of the research should be on improved heat exchanger design and SiC material development. The improved heat exchanger design is not being done. Spray synthesis of the materials is more applicable to high-volume production than the atomic layer deposition (ALD) approach used in previous efforts.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The project's accomplishments and progress toward its objectives have been excellent. Progress in achieving the DOE Hydrogen and Fuel Cell Program (the Program) target goals is good. The project plan is establishing specific materials performance testing at the expense of investments in particle durability and concept design. The plan is good but not outstanding. There are many materials but few reactor concept designs that will enable performance leading to meeting the Program's target metrics. Absent durability, active material performance is interesting but not pertinent to Program targets. Absent reactor concepts that can meet cost and performance targets, Program goals are not within reach. A more balanced project plan that identifies specific and quantitative go/no-go metrics is needed. The update of the Aspen Plus model to include process-specific functions and to include experimental results is an outstanding contribution to assess process performance.
- Good progress has been made to date in this new project, particularly in the areas of modeling and performance prediction, constructing and upgrading test reactor systems, and materials discovery (through the NSF/DOE project partnership).
- Good progress has been made regarding materials, preliminary reactor design, and process modeling. The techno-economic analysis, with identification of key sensitivities, is also good. The very low sensitivity to redox material activity is surprising—a high-level explanation is warranted. The production of 0.00085 kg

hydrogen per kg of active material per reactor cycle (so even less on a particle basis) means there is a need to move a huge amount of solids around at a very high rate to generate nearly 2 kg hydrogen/sec (50 ton/day with production limited to 8 hours/day)—about 2,000 kg per second around the loop. Increasing that activity should be a big hammer on the overall economics, but the analysis says it is not. It would also be good to see an overall solar-to-hydrogen (STH) efficiency metric reported (current status and outline of path to target).

- The Aspen analysis is good. It is not clear that the reactor design concept is practical in a real-world, full-scale situation. The major cost savings, according to the H2A analysis, are in the heat exchanger, but it is not clear that work is being done on designing and improving the heat exchanger. The spray synthesis approach has some promising results. The sweep gas analysis was interesting, even if some of the other researchers disagreed.
- The reactor design has been updated to include an inert sweep gas, which yields improved overall performance by eliminating the low-efficiency vacuum pump. The reactor design has also been improved with a realistic receiver concept, although the details of radiative heat transfer to the particles still need to be worked out. An Aspen process model has been developed. An updated H2A model has been developed. Computational materials modeling (integrated computational, theory, and experimental approach MGI) has been performed to predict the hydrogen production capacity; the model has been validated with experimental results. Characterization of the engineered materials is underway. A particle flow reactor laboratory is being developed. SiC coating capability is being developed. Progress over the previous project has been modest to date.
- The project seems on task to date, but it is still early in the project, and many milestones are ahead.

Question 3: Collaboration and coordination with other institutions

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

- An excellent project team at University of Colorado (CU) and NREL has been assembled for this effort. In addition, leveraging CU interactions with the Australian National University (ANU) and the Saudi Basic Industries Corporation (SABIC) will provide added value to the project. The project team should take the opportunity this project offers to provide leadership in coordinating across the community to establish conventions for analysis, best practices, and key measurements.
- The project features good integration with the NSF “sister” project. It does a good job of leveraging outside funding sources (e.g., the NSF “sister project”), ANU, and SABIC. Collaboration with Sandia National Laboratories (SNL) would be good.
- Collaboration and coordination with other similar work is satisfactory to good. It would be better if rapport could be established between other similar STCH project being funded at a national laboratory and CU. Project teams are pursuing very similar objectives using very similar general approaches and using materials derived from the same general class. Both teams appear to have chosen competitive—instead of cooperative and collaborative—postures. The project goals would be far better supported if these two institutions could work together to solve their common and challenging problems. The project plan provides for some integration with theoretical work that materially aids the identification of promising active materials, but key material characterization that could help assess the trade-off between production capacity and material durability is lacking. Material characterization capabilities are highly specialized and require access to technology well beyond the scope of most institutions. This project would be improved by collaborating with an entity that has existing characterization capability to develop a basic understanding of the relationship between activity, reactant oxygen vacancy levels, and material durability. There is no guarantee that needed production capacity is compatible with material durability.
- The project features a good set of collaborators, particularly on the materials side, where the researchers are leveraging a “sister” MGI project. It is not clear whether or how the team is accessing the needed expertise on complex solar reactor design and the critical thermal recuperation step. If this collaboration is missing, the team should consider adding a partner in this area because high sun-to-hot-particles efficiency is critical.
- Collaborations have been established with NREL (high flux solar furnace user facility), ANU (solar simulator), and SABIC (materials characterization).

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.2** for its relevance/potential impact.

- The relevance of this project is excellent to outstanding. The potential impact is satisfactory to good. The potential impact assessment is based on the project's characterization of its detailed objectives. High-performance material formulation would have high hydrogen production capacity, meaning high numbers of oxygen vacancies per mole of active material, along with fast redox kinetics, meaning a significant energy difference between the initial and final states for the oxidation step. Both of these features would likely reduce active particle durability, making achievement of the cost target very difficult.
- The project supports the EERE FCTO goal of reducing the cost of hydrogen production from renewable resources to <\$2/kg by 2020. In addition, it supports the objective to verify the competitive potential for STCH cycles for hydrogen in the long term by 2015, and by 2020 to develop this technology to produce hydrogen. Meeting the cost goal will depend on the successful completion of this project, but also on technology development beyond the scope of the project and FCTO (e.g., lowering the cost of the heliostats to the DOE Solar Program SunShot target).
- Leveraging the MGI is a clear advantage.
- STCH is very well aligned with DOE's long-term goals for centralized hydrogen production. However, it is an approach with a large number of substantial technical (particularly materials and engineering) challenges with no practical base off which to build. It is not obvious how it will gain a long-term advantage over the photovoltaic electrolysis approach.
- Development of an efficient, cost-effective direct solar process for water splitting has the potential to significantly expand the role of solar energy. This project claims to have a long-term potential for low cost hydrogen production. However, it is difficult to foresee a path to low-cost, large-scale hydrogen production using this technology that is achievable in a reasonable time frame.
- There is a need for low-cost renewable hydrogen.

Question 5: Proposed future work

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

- The project team has effectively outlined future work in reactive and containment materials development, reactor design, and modeling and prediction in a logical manner, with milestones and go/no-go decision points identified. In the future, more information on activities and plans in the following areas would be helpful:
 - Lifetime (durability) of reaction particles to reach production cost goals.
 - Demonstrated feasibility of achieving the gas-gas heat exchange levels needed to reach efficiency and cost goals.
 - Scale-up feasibility of ALD and other fabrication technologies to apply materials of containment coatings to full-size components.
 - Next steps for the 19% of perovskites screened that fell within the acceptable enthalpy range.
- The proposed future work is good to excellent. This element would be improved with greater attention to active particle durability and some investment in alternative receiver/reactor design.
- The focus and plan for laboratory-scale testing is good, as is the model-guided materials development. It would be good to see a plan to address heat exchange efficiency because this was identified as the primary technoeconomic sensitivity. Also, there should be analysis of the capital costs, including sensitivities (e.g., the assumption of how much 2.9 kT of redox material would cost is unclear).
- The researchers are focusing on the material development, which is most important at this stage. According to their H₂A analysis, the single biggest need is low-cost heat exchangers, but there is nothing in the proposed work to deal with this challenge.
- Continued work will be performed on the development of reactive materials, HT reactor design and development, materials containment development, and improved system modeling (Aspen).
- More discussion of alternative pathways in case milestones fail would be helpful and beneficial.

Project strengths:

- The facilities and skill sets are excellent. This is one of the most experienced teams in thermochemical hydrogen production. The team has an outstanding history of changing directions when the focus of the work proves to be unfeasible.
- The researchers have “sister” funding through NSF. They have moved beyond the ALD synthesis to spray synthesis. Spray synthesis is inexpensive and amenable to high-volume fabrication. While this project is new, the researchers have been working in this area for a very long time.
- The project strengths include an innovative reactor design and materials approach, an excellent project team, and leveraged interactions with other researchers.
- Progress has been made on redox material synthesis. The project will support HT reduction and oxidation operations at the laboratory scale this year.
- Project strengths include the integration of modeling and experimental research approaches.

Project weaknesses:

- There are no serious weaknesses, but the future work should focus on critical points—namely, demonstration of particle attrition resistance, sufficient gas-gas heat exchange and heat transfer in the reactor during operation, and the feasibility of application of coating materials of containment to full-scale components.
- This project faces many of the same extreme technological and economic challenges as other HT STCH processes. These challenges include identification of an appropriate redox material that will enable long-duration, high-efficiency, low-cost operation; selection and demonstration of reactor materials for very HT operation; and management of solid particulate circulation and radiative heat transfer. The magnitude of these challenges should not be underestimated. Furthermore, this technology must be shown to be competitive (at least in the long term) with other hydrogen production technologies, in general, and with other solar water splitting technologies, in particular. Solar water splitting can be accomplished today with commercially available technology using state-of-the-art photovoltaics (PVs), providing electrical power directly to conventional water electrolysis units, with a demonstrated efficiency of 18% or higher. HT steam electrolysis, at TRL 5, could also be operated as a solar water splitting technology using concentrated solar heat for the required HT heat addition (at 800°C instead of 1500°C) and state-of-the-art PVs. STH efficiencies of at least 30% should be achievable with current technology. In terms of economics, the cost of the HT solar receiver technologies alone may preclude achievement of low-cost hydrogen production using the HT thermochemical processes.
- There are significant questions on the reactor/receiver design. The researchers propose to coat the reactor with SiC to improve the material compatibility. It is likely that the fluidized bed approach will wear off the SiC. This needs to be addressed.
- The level of collaboration in this project is satisfactory. The team needs to establish ties with an entity with advanced characterization capability to explore the limits on active material durability. The DOE Hydrogen Production sub-program would benefit enormously from better coordination and collaboration between SNL and CU.
- Weaknesses include the heavy academic weighting and the lack of collaborators with practical, large-scale engineering experience with complex reactors and processes.

Recommendations for additions/deletions to project scope:

- Reactive materials R&D should include some detailed investigation of the effects of kinetics and oxygen vacancy levels on active material bond strength for better understanding of the relationship between production capacity and material durability. In reactor design, the common chamber redox concept should be examined for temperature profiles assuming something close to the planned solar flux profile to qualify/quantify temperature distributions and non-uniformity effects in both reactors. It is possible the common chamber concept is not workable. Some preliminary design of separate reactor chambers might be warranted.

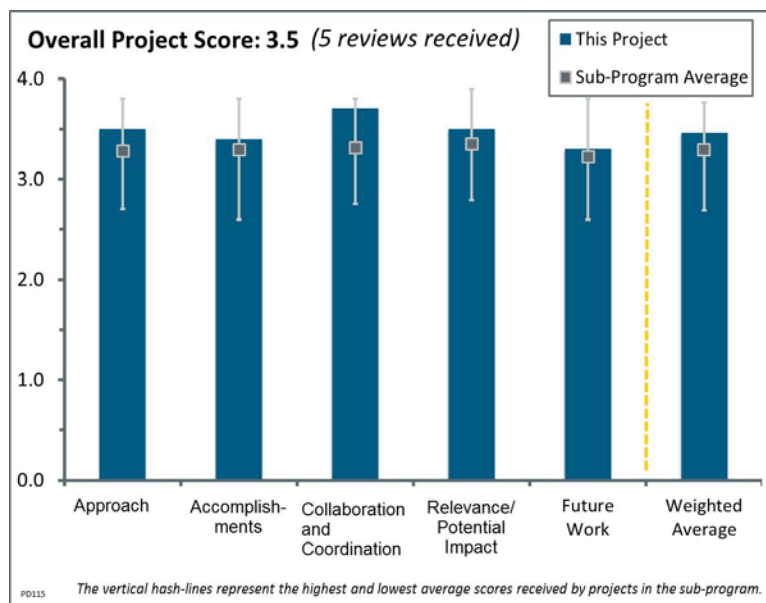
- It is absolutely critical that a fully functional, integrated, on-sun, long-duration demonstration of this technology is completed as soon as possible. Successful achievement of such a demonstration would go a long way toward justifying further investigation in this concept.
- The comparison studies between inert gas sweep and vacuum pumping appear to be a recently added task. It may be helpful to conduct a more extensive literature search on the relative merits of the two approaches before initiating R&D on this topic.
- The project team should add input from a consultant with experience in cost estimates for large circulating-bed type reactors to shore up the economics.
- The researchers need to redesign their main reactor/receiver concept.

Project # PD-115: High-Efficiency Tandem Absorbers for Economical Solar Hydrogen Production

Todd Deutsch; National Renewable Energy Laboratory

Brief Summary of Project:

The long-term objective of this project is to develop a durable, semiconductor-based, solar-driven water-splitting device with greater than 20% solar-to-hydrogen (STH) efficiency that can operate under 10–15 times (or higher) solar concentration and generate renewable hydrogen for <\$2/kg. Current year objectives include (1) pushing boundaries on achievable semiconductor photoelectrochemical (PEC) solar-to-hydrogen efficiencies through development of new materials and structures, and (2) continuing development of stabilizing surface modifications viable at high current densities.



Question 1: Approach to performing the work

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

- The objective of this project is to develop a highly efficient, durable material for PEC water splitting using concentrated solar energy. For this technology, efficiency was identified as the most significant driver for reducing hydrogen cost. Therefore, the primary focus of the work is on increased STH efficiency through the use of tandem absorbers. Durability can be improved through the use of surface modifications (e.g., ion implantation, flash sputtering, atomic layer deposition, and moly disulfide [MoS₂] coatings). New materials are sought for ultra-high efficiency. The materials discovery search incorporates theory, modeling, and experimentation. STH efficiencies of 20% are stated to be feasible. Ultimately, low-cost cell synthesis will be required. The approach includes theory, modeling, and experimentation.
- The project team has responded to U.S. Department of Energy (DOE) targets for PEC efficiency, durability, and cost barriers.
- The team's approach of focusing on III–V semiconductors is consistent with the Office of Energy Efficiency and Renewable Energy STH efficiency targets of 20%–25% because there are very few materials combinations outside of III–V materials that are capable of reaching these efficiencies. The National Renewable Energy Laboratory is well positioned to do this research, given the expertise of its III–V synthesis team and the team's history of using these materials for PEC water splitting. One potential weakness of the approach is the reliance on high optical concentration for favorable economics with III–V-based absorbers. While low concentration (e.g., approximately 10x) has been demonstrated in a large-volume laboratory test cell, there are significant constraints for a real-world reactor.
- The approach of this project addresses durability and fabrication costs of III–V materials to advance device performance levels of this record-achieving material. This is one of three proposed approaches to meeting DOE targets for PEC performance. This plan is strategic and derived from PEC community consensus that affords alternative or backup lines of investigation should any one or two of the alternative lines of research prove to be unfeasible. The general strategic plan is excellent, building on successful materials while seeking new, improved materials and device configurations in collaboration and coordination with the PEC technical community. Regarding materials durability, the presentation indicates that materials durability can be met through engineering, but theoretical studies and advanced characterization work is also warranted to understand how interface treatments are effective (or not) in improving durability while

retaining performance. It would appear that III–V material durability will not be achieved by engineering alone. In that case, the project timelines are probably unrealistic. Regarding integrated device configurations, the presenter stated that the current device design is unsuitable for solar concentration levels or the reduced electrolyte optical depth proposed for meeting efficiency targets. Design options were not presented or discussed.

- This design of the approach is sound and well thought out. It leverages years of research in the field to focus on the central issues and barriers to the success of PEC hydrogen production. The Surface Validation Team should include consideration of the kinetics of surface decomposition because the branching ratio of the rate of decomposition to the rate of all reactions determines durability. The present approach contains only examination of the static aspects of durability.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The team has demonstrated the use of an “inverted metamorphic multijunction” for increased utilization of the solar spectrum. Durability of >400 hours was achieved. Some fouling occurred; cleaning of the cell surface using methanol and nitric acid restored photocurrent to the original value. Surface modification with N_2+PtRu was achieved and characterized. Nitrogen decreases the bandgap and stabilizes the cells. A new reactor design reduces the optical path length through the electrolyte, thereby increasing cell efficiency.
- Initial results with the inverted metamorphic multi-junction (IMM) cells are very encouraging (especially considering the processing mistake limiting the current density). It seems inevitable the team will demonstrate 15%–20% STH in the near future, which will be a world record for this sort of tandem device. The design is shown for a new test cell that will minimize the distance between the electrode and the window, but no clear plans for a realistic reactor design that would operate under 10–100x concentration with minimal solution IR loss were provided. Developing a more concrete design and experiments using the fabricated reactor should be important goals. A 50–100x concentration is unrealistic. At 50x and with 20% STH, this is approximately 800 mA/cm². It may be possible to deliver reactants to the surface and prevent bubble build-up with a flow cell at these higher concentrations, but avoiding significant IR drop, even in 3 M H_2SO_4 , would require a very small gap between counter electrode and photoelectrode. A/cm² current densities are achieved in polymer electrolyte membrane (PEM) electrolysis with only a 100–300 mV IR drop, but that is because the researchers are using 50–90 μ thick Nafion solid electrolyte.
- Durability testing showed good-to-excellent progress by reaching the 400-hour durability milestone, but the testing methodology is unsatisfactory because of electrode fouling. The project should establish the cause and remediate fouling to achieve a satisfactory durability testing protocol. Additionally, the project must establish the reason for degradation of photocurrent onset potential. It is possible that the durability treatment affects catalyst activity lifetime or leads to catalyst loss. Either phenomenon would reduce durability progress. Additionally, the poor performance of the “new standard” ion gun and PtRu sputtering setup suggests continued ignorance regarding earlier success with the “magic” sample. It is possible that some unknown contaminant derived from the original setup is essential to reliable durability treatment. Investigation of IMM fabrication is an excellent initiative that promises improved junction morphology while affording potential cost reduction in both material component assembly and device production. In addition, the IMM process enables buried junction fabrication that allows effective use of the solar spectrum while maintaining adequate current density. Progress on the reactor design is good to excellent. Recognition of the electrolyte optical depth effects on STH performance is important, and implementation of design changes is underway. However, system thermal management and gas transport issues become more important and difficult to resolve. It is unclear whether the original general concept can be retained.
- Although it is still early in the project, investigators have completed several characterization studies on catalyst distribution and the effect of surface modifications. A reactor design to improve optical concentration has also been developed. A 400-hour durability milestone was completed, but some work remains to determine performance degradation mechanisms.

- The team has made commendable progress; the work was competently done and featured good technical support. It appears that “fouling” of the electrode has a greater impact on durability than any electrode/catalyst decomposition. A better grasp of this problem is needed at a chemical level.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration and coordination with other institutions and related technical work is excellent to outstanding. The PEC Working Group is inherently a broad collaborative enterprise, and improved coordination with photovoltaic (PV)-based III–V activities is an important addition to the project.
- One of the strengths of this project is its large number of collaborators in the field of PEC research and development.
- The project includes 10 university partners, plus Lawrence Livermore National Laboratory (LLNL) and Los Alamos National Laboratory.
- The coordination with other institutions is very good. If the kinetics and chemistry aspects of the problems cannot be addressed within this group, more outside help should be sought.
- There is good collaboration with the surface science group (Heske/University of Nevada, Las Vegas); the researchers have used this collaboration to understand stability. No aspects of the theory collaboration (Ogitsu/LLNL) were discussed.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- The relevance and potential impact of the project are excellent to outstanding. The PEC Working Group investment in performance metrics and testing protocols has significantly improved the reporting basis for the entire PEC community. At the same time, identification of the role of upper and lower bandgaps in optimizing STH performance of simple integrated devices has provided excellent guidance to the selection of promising materials and doping/alloying strategies for development and testing. Optimal performance of 25% STH is challenging but possible, and 20% STH appears to be achievable. The identification of component fabrication and integration processes is leading to the development of pathways to reduced device cost, which, in tandem with increased STH performance, promises the possibility of meeting DOE Hydrogen and Fuel Cells Program (the Program) goals for PEC hydrogen production.
- The project is highly relevant to renewable hydrogen production and consistent with DOE’s goals/mission. As illustrated by the Hydrogen Analysis (H2A) model’s technoeconomic analysis and discussed in the presentation, the potential impact of this technology is highly dependent on the ability to bring down III–V synthesis/fabrication costs with alternative, scalable manufacturing methods.
- This work supports the development of direct STH PEC cells. PECs operate at low temperature and are potentially more efficient and less expensive than PV/electrolysis for solar water splitting. Successful development of this technology would expand the role of solar energy to include distributed hydrogen production for the transportation sector.
- PEC water splitting is a long-term technology in DOE’s portfolio. The objectives are in line with DOE targets to develop cost-effective and efficient PEC materials.
- Given the technoeconomic analysis upon which this project is based, this is the logical approach to the problem of hydrogen production.

Question 5: Proposed future work

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

- The principal investigator (PI) is pressing toward the right endpoint—getting something that works and pushing it to the prototype stage. That is when the next round of challenges becomes apparent.

- The project has a defined path to improve PEC cost, efficiency, and durability.
- The STH and durability next-step targets are good to excellent, although the presentation did not describe the future work to understand the treatment processes for durability enhancement. Use of lower bandgap configurations to achieve higher current density is not accompanied by adequate description of means of retention of water splitting capability, so it is not clear what value should be given to this future work task. The statement in the presentation that “Multi-physics” modeling will be used to evaluate limits on achievable concentration is unclear. It is also unclear whether there is concern about photoactivity saturation effects or about thermal management issues. The latter seems more likely to be the controlling factor but requires significant concept design detail for evaluation. The photoreactor prototyping task seems to precede significant effort in concept design and performance simulation. The proposed order seems backwards. Whereas the presenter stated that work is already proceeding in new photoreactor concept development, the task of simulating performance under operational environments is significant and should precede prototype development. The presentation showed little concept design beyond the original general concept, so it seems that significant concept design work remains before performance simulation can proceed.
- It is apparent that long-term stability tests are a challenge for the small photoelectrode sizes, which necessitate the use of surfactant to avoid bubbles but create fouling of the counterelectrode. If at all possible, larger electrodes should be made and/or different sealing methods should be employed to avoid this issue and unambiguously demonstrate stability targets. For some of the same reasons that the team is presently having issues with its long-term stability measurements, a real PEC reactor would not likely operate with surfactant present. Previous results of MoS₂-coated p-n Si photocathodes show that linear sweep voltammetry (LSV) curves are shifted to significantly more negative potentials (100–200 mV) compared to using Pt. It is unclear whether it is feasible to achieve efficiency targets with MoS₂-protected photocathodes. On the topic of light concentration, it is not clear how sensitive the H₂A analysis is to light concentration. Although Pinaud’s *Energy and Environmental Science* article compares Type 3 reactor @ 1 sun and Type 4 reactor @ 10 suns, this reviewer has not seen an H₂A analysis (i.e., Tornado plot) showing how sensitive the Type 4 hydrogen cost is to light concentration. This would be valuable to know.
- The team will attempt to demonstrate increased efficiency (i.e., >15% STH) with lower bandgap configurations. The researchers plan to demonstrate 875-hour durability under 1 sun in outdoor conditions. Photoreactors will be designed and built with low optical concentration and low electrolyte penetration depth.

Project strengths:

- Important strengths are the project team’s quality, in-house facilities, and open teaming and collaboration approach to satisfying other facilities and expertise requirements. Another strength is that its relationship with the Advanced Materials Manufacturing (AMM)/Materials Genome Initiative (MGI) efforts helps to populate the third approach to meeting the Program goals by identifying promising third-generation materials.
- The DOE Hydrogen and Fuel Cells Program Annual Merit Review (AMR) presentation was excellent. The work is very promising, but challenges remain in the areas of cost, durability, and materials. Overall, this technology appears to have great promise.
- The project’s strengths include its good approach and the team’s expertise. The team has a great likelihood of meeting world-record efficiency targets.
- Strengths of this project include the collaboration among team members with experienced backgrounds in PEC synthesis and characterization.
- The project features an excellent technical staff and facilities, as well as an experienced group of collaborators.

Project weaknesses:

- There was no mention of plans to scale up to larger electrode areas. A detailed plan for reactor design is missing from the presentation. Even if the reactor design is not demonstrated in this project, it would be great to include this plan to illustrate the vision of what a commercially viable product/reactor would look

like. A complete, explicit list of milestones was not provided. It would be useful to include this for the next AMR.

- A large cost may be incurred in the fabrication of a complete PEC system (i.e., integration of liquid/gas plumbing and electrical connections), and it may be difficult to achieve DOE's 2020 future cost targets. Although it is early in the project, a discussion of scale-up challenges should be presented.
- The technology as presented uses sputtered-on PtRu (with N) as a catalyst durability layer. This approach may have significant cost implications. The team is also evaluating non-platinum group metal (e.g., MoS₂ and atomic layer deposition TiO₂) surface treatments.
- The projected need of electronic-grade materials requires guidance from a physicist/device physicist, which leaves the research group open to being blind-sided by chemical problems. The PI needs to compose a chemical "think tank" from among collaborators to cover this condition.
- The project is complex, and the team must overcome many obstacles to meet the project objectives and/or the Program goals.

Recommendations for additions/deletions to project scope:

- The project scope is mostly appropriate.
- Having a significant solar concentration level is a central ingredient of this project. In this regard, thermal management is likely to be a limiting feature for photoreactor design options. The project might be improved significantly by including design and engineering work to evaluate thermal management issues.
- The team should add additional details regarding the cost analysis related to scale-up.

Project # PD-116: Wide Bandgap Chalcopyrite Photoelectrodes for Direct Solar Water Splitting

Nicolas Gaillard; University of Hawaii

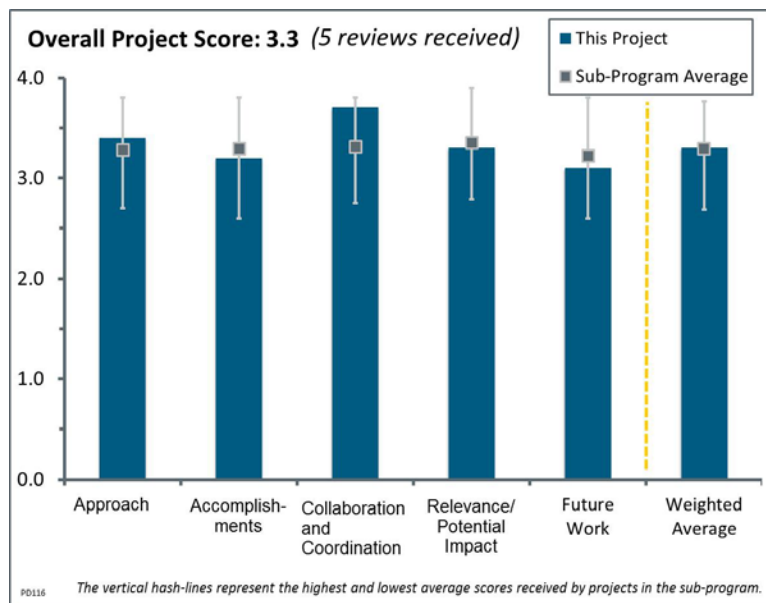
Brief Summary of Project:

The long-term objective of this project is to identify efficient and durable chalcopyrite-based materials that are capable of generating hydrogen via photoelectrochemical (PEC) water splitting at a cost of \$2/kg or less. Leading efforts involving theoretical modeling, synthesis, and advanced characterization, the Hawaii Natural Energy Institute (HNEI) will (1) develop new wide bandgap (>1.7 eV) chalcopyrites compatible with the hybrid photoelectrode design, (2) demonstrate at least 15% solar-to-hydrogen (STH) efficiency, and (3) generate 3 L of hydrogen under 10 times concentration in 8 hours.

Question 1: Approach to performing the work

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

- HNEI is exploring potentially low-cost chalcopyrite photoabsorbers with tunable bandgaps, which represent a very promising class of materials to achieve high STH conversion efficiencies with a tandem device. The materials development approach includes systematic evaluation of the solid-state properties of the chalcopyrite absorber layers separate from the electrochemical environment. This is a useful means of deconvoluting catalysis and corrosion effects from the performance of the chalcopyrite absorber. The researchers are spending early efforts exploring new emitter layers for the new chalcopyrite absorber layers, which is a well warranted and important task if they are to achieve the performance milestones. The project milestones are clearly presented.
- The project features a well-defined approach that focuses on accelerating material development through modeling, synthesis, and characterization of chalcopyrite materials. The approach addresses barriers of efficiency to final synthesis and manufacturing.
- This project aims to identify and develop new wide-bandwidth (>1.7 eV) copper chalcopyrites (CuInGaSe₂) for PEC water splitting using an integrated experimental, computational, and theoretical approach. The cells under consideration are “hybrid photoelectrode” (HPE) cells. These cells include high-current-density photovoltaic (PV) “drivers” with wide-bandwidth PEC cells.
- This work is one of three general approaches to PEC performance achievement. It seeks new materials with characteristics that hold promise for meeting the U.S. Department of Energy (DOE) Hydrogen and Fuel Cells Program (the Program) goals in 2025 and complements the second approach of seeking to stabilize and improve on existing materials with high STH efficiency. The general approach of this project is excellent. It builds effectively on earlier work and is sharply focused on barriers to achieving the Program goals. The selection of copper chalcopyrite alloys for bandgap management has proven effective in identifying potentially satisfactory candidates for meeting STH goals. The project suffers most from requiring PV-grade materials and device fabrication and integration. The cost of materials and device fabrication will likely prove seriously challenging. Integration, coordination, and collaboration with other work (e.g., device testing, materials characterization, process execution, and evaluation) are outstanding.
- The project is well conceived, given the goals. The development of new absorbers is commendable, yet risky, because one must learn new surface chemistry with each new compound. Given the use of S and Se



compounds, the principal investigator (PI) should consider assembling the data on the stability of these compound solids as a class and not analyze each electrode's stability as an individual case.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The progress is good to excellent, as exemplified by the conclusion that alternative copper chalcopyrite alloys are necessary to meet project goals. The identification of three potentially successful wide bandgap candidates is outstanding. Evaluation of sulfurized conventional chalcopyrites and their photoactivity is an important step that allows follow-up investigation of improved alloys with appropriate tandem band levels for water splitting. The role of sulfides in both interface durability and buffer performance in conventional copper chalcopyrites is an important advancement. It is important to follow up with identified alloys to demonstrate that these conclusions are not contradicted by alloyed characteristics. Exploration of new material surface energetic performance (such as ZnO) is crucial to continuation of progress. Interface stabilization with moly-sulfide demonstration is good but insufficient to meet the Program durability goals.
- Three promising high-bandgap, PV-grade absorber compositions (i.e., $\text{CuIn}_{0.4}\text{Ga}_{0.6}\text{S}_2$, $\text{CuGaSe}_{0.7}\text{S}_{0.3}$, and $\text{CuIn}_{0.2}\text{Al}_{0.8}\text{Se}_2$) have been identified. PV-grade absorbers have been demonstrated at 1.55 eV. The crucial role of surface Cd doping has been demonstrated. Chalcopyrite photocorrosion has been quantified, and advanced characterization methods have been developed to identify mechanisms. The HPE system has been simulated to identify solid-state requirements. The team also investigated a PEC reactor with a 10x solar concentrator.
- Overall, the team has made good progress toward project and DOE goals. The early work has demonstrated the team's ability to precisely tune the bandgap of the chalcopyrite materials of interest and reported promising solid-state and PEC performance (current density and photovoltage) for some of these alloys.
- The team is on track to complete milestones for 2015. The performance of chalcopyrite photoelectrodes that can generate 12 mA/cm² will be a key go/no-go decision in the near future.
- The progress has been extremely good—not so much regarding the number of compounds created, but in terms of the establishment of an integrated approach to theory, modeling, and synthesis to create new absorbers.

Question 3: Collaboration and coordination with other institutions

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

- This project is inherently outstanding in collaboration and coordination, partly due to its strong presence within the PEC Working Group and partly due to its incorporation of testing and evaluation collaborations with material experts in related but different lines of research. These are shown in the presentation element "Project-specific collaborations."
- The project features excellent collaborations, including theoretical and surface science support in addition to tie-ins with the Materials Genome Initiative.
- The project features a well-established team capable of addressing DOE's cost and performance barriers for PEC water splitting.
- Collaborations have been established with The Swiss Federal Laboratories for Materials Science and Technology, Columbia University; the University of Los Andes (Colombia); the National Institute of Advanced Industrial Science and Technology (AIST, Japan); the University of Bordeaux (France); and the University of California, Irvine.
- It is disappointing to hear the PI has had no input from the Stanford group at this stage of the project execution.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.3** for its relevance/potential impact.

- The relevance of the project to the Program objectives is excellent to outstanding. The potential impact is outstanding, largely because of the incorporation of this work in the PEC Working Group, arguably the best example of expert team collaborations in response to huge technical and cost performance challenges. The PEC Working Group investment in performance metrics and testing protocols has significantly improved the reporting basis for the entire PEC community.
- Overall, the project is relevant because it involves the development of efficient, direct STH PECs. This project aims to create the first all-chalcopyrite HPE device to achieve PV-grade performance with thin-film materials to meet DOE goals.
- The discovery of new semiconductors for PEC work is always impactful, especially those that can be synthesized differently from the standard electronic-grade solids. These semiconductors provide opportunities for future device development.
- The objectives of the project are in line with DOE's targets regarding the development and fabrication of cost-effective and efficient PEC materials.
- The project is relevant to DOE goals and has a high potential impact if a tandem chalcopyrite-based device can be demonstrated with STH >15%. The viability of a mechanical tandem for a commercial device and the challenges of manufacturing a high-performance monolithic device could limit the potential impact.

Question 5: Proposed future work

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

- The future work is well defined. A critical junction in this project will be whether the team can demonstrate chalcopyrite photoelectrodes capable of 10–12 mA/cm² with bandgap greater than 1.7 eV. It may be early in the project, but it would be good to see additional discussion on the cost of a full-scale system.
- The proposed future work is good. The future work plan is abbreviated, although the intense focus on quantitative targets is outstanding. The plan does not include investment in a general design for supporting the long-term DOE Program goal.
- A device design of a monolithic tandem is illustrated on slide 8, but it would also be useful for the team to show a schematic that illustrates the team's vision for what the complete, integrated device for real-world application might look like. For example, it is not clear how ion transport between the front and the back of the device would be achieved. This is not shown in the current side-view schematic. In situ x-ray emission spectroscopy (XES) measurements of CuGaSe₂ are proposed (or were already performed). From slide 13, it is implied these are to be done for direct exposure of the chalcopyrite to the electrolyte; however, it seems obvious from electrochemical measurements (and common sense) that the photocathode will require a protection layer. The team might consider doing the in situ measurements on the photoelectrodes containing protection layers instead. For the development of new buffer layers for p-n junctions, it would be great to see some more direct experimental measurements of energetics/band alignment across the emitter/absorber interface (perhaps using techniques at the University of Nevada, Las Vegas).
- The proposed future work includes a continued search for PV-grade wide bandgap absorbers, evaluation of surface passivation treatments, the use of MoS₂ for corrosion resistance, and device certification.

Project strengths:

- The project is focused on a very promising class of materials for water splitting, and the team is very well qualified to carry out the upcoming tasks, although hitting the ultimate milestones (e.g., 15% STH efficiency) will be very challenging. There are nice collaborations between team members and outside collaborators.

- Important strengths are the project team's quality, in-house facilities, and an open teaming and collaboration approach to satisfying other facilities and expertise requirements. The team has a record of identifying no-go metrics and implementing transitions to more promising lines of effort.
- The new solids this project is creating for PEC use are very important in their own right, not just in this application.
- The project features strong team members who are well versed in PEC material development and characterization.
- This work represents an interesting alternative approach to PEC solar hydrogen production.

Project weaknesses:

- No transmittance data for the wide bandgap energy top cells was provided, which is very important for device design (current matching) and thus the ultimate STH of the tandem devices. It is not clear how the optimal bandgap combination changes as the average transmittance of the top cell is varied. Based on the best performance of world-record chalcopyrite PV cells, there will be very little photovoltage to spare if the STH targets are to be met (especially 15%). For this reason, it is unlikely that MoS₂ can be used as a catalyst coating for high-efficiency devices based on chalcopyrites. Still, the MoS₂ coatings might be useful for other materials and/or if one of the chalcopyrite layers is combined with a higher open circuit voltage cell. The researchers will focus on mechanical tandems, which is reasonable, given their limited resources. Monolithic tandems would have a big advantage from the standpoint of manufacturing and the achievable capital cost, but it was noted that significant processing challenges exist for processing both absorber layers on the same substrate.
- The project is complex, and the project team must overcome many obstacles to meet the project objectives and/or the Program goals. The design of an operational photoreactor concept is unclear.
- This technology does not appear to be less mature than the National Renewable Energy Laboratory metal oxide semiconductor approach.

Recommendations for additions/deletions to project scope:

- Complementing the planned work with some initial evaluation of Al alloyed selenides would be interesting. Whereas some efficiency reduction might be expected, that could possibly be offset by likely reduced fabrication cost, which is one of the outstanding barriers to this hydrogen production method.
- Analysis of scale-up and the associated cost would be an interesting focus in PEC development programs.
- On-sun technology demonstration should be performed as soon as possible.
- The scope is suitable.

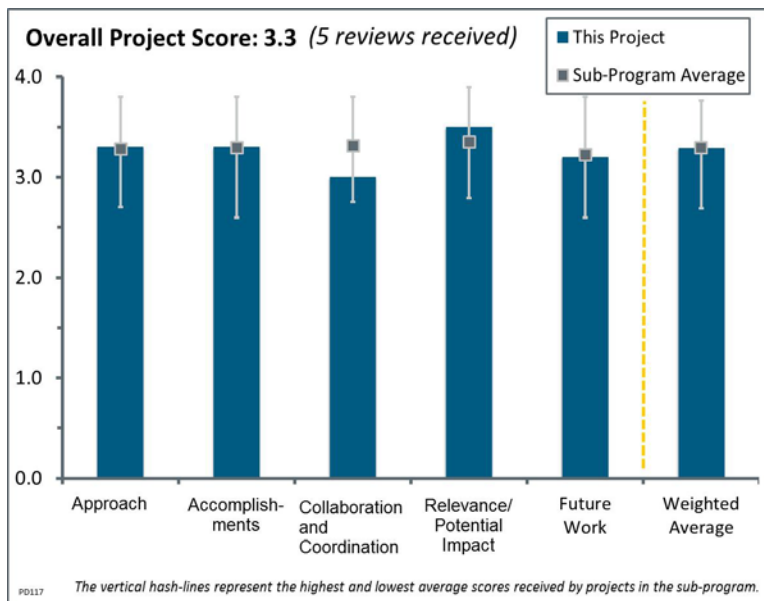
Project # PD-117: High-Temperature, High-Pressure Electrolysis

Cortney Mittelsteadt; Giner, Inc.

Brief Summary of Project:

The overall objective of this project is to reduce the cost of polymer electrolyte membrane (PEM) electrolysis by developing membranes capable of operating at a higher temperature and pressure with improved efficiency and durability. High-pressure delivery will simplify the systems and reduce the overall capital expenses. High-temperature (HT) operation will increase efficiency, reduce operating expenses, and reduce the balance of plant (BOP). Long-term durability will reduce maintenance and capital expenses.

Question 1: Approach to performing the work



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

- The overall project objective is to reduce the cost of PEM electrolysis for energy storage by improving membranes for high-pressure operation, high efficiency, and durability. High-pressure hydrogen is produced using electrochemical compression, so there is a large pressure difference across the membranes. This will require development of special membrane materials (e.g., hydrocarbons [HCs]). The cells are also operated at relatively HT (95°C demonstrated) for more favorable thermodynamics and to improve kinetics. High temperature operation will challenge chemical durability; additives will be required. The conductivity to permeability (C/P) ratio must be increased for high efficiency.
- Increasing the C/P ratio is key for achieving high efficiency and operating pressure over a large operating range, which is important for integration with renewables.
- The approach is effective and contributes to overcoming most barriers.
- The project's relevance is strongly explained and justified. Selection of the four types of membranes and the corresponding theory of how they were selected (and how they work) is not well explained. While the C/P ratio is good for screening, it is also desired to simultaneously have high conductivity. Thus, screening for conductivity might be an important adjunct to the C/P metric.

Question 2: Accomplishments and progress toward overall project and U.S. Department of Energy (DOE) goals

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

- The testing station for the new types of membranes has been built, and four different types of perfluorosulfonic acid (PFSA) and HC membranes have been developed. Three membranes demonstrated a C/P ratio of greater than 2. Membranes showed long lifetimes (i.e., 30,000 hours), and catalysts were stable.
- A 5000 psi test stand (not funded by this project) is ready. This test stand has been modified for detailed diagnostics. Four PFSA membrane types were tested with additives to minimize crossover and degradation. Several HC (sulfonated aromatic HC polymers) membranes have also been developed. C/P ratio was identified as an important metric and was measured for many membranes, with two HC membranes exhibiting a ratio >2. Testing was completed at 95°C and 1000 psi.

- The project has made good progress in screening candidate membranes for the ratio of conductivity to permeability.
- The accomplishments were generally effective but could be improved. This project contributes to overcoming some barriers.

Question 3: Collaboration and coordination with other institutions

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

- There is good collaboration; partners participate and are well coordinated.
- Giner, Inc. is working with Virginia Tech (alternative membranes) and 3M (ionomer).
- The coordination between partners seems adequate, but there is little basis for assessment. Further reviews could more fully explain the collaboration.
- The project collaborates with only one partner.

Question 4: Relevance/potential impact on supporting and advancing progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan

This project was rated **3.5** for its relevance/potential impact.

- The project aligns well with the DOE Hydrogen and Fuel Cells Program and DOE research, development, and demonstration (RD&D) objectives and has the potential to advance progress toward DOE RD&D goals and objectives.
- The project is perfectly relevant to increasing the potential of integrating a PEM electrolyzer with renewable energy sources, while at the same time increasing efficiency and operating pressure.
- Direct electrolytic production of hydrogen at high pressure using electrochemical compression reduces downstream compression energy costs. Efficiency is enhanced with high temperature because of the lower thermodynamic voltage requirement and better kinetics (similar arguments can be made for HT steam electrolysis). Cost is reduced because of the lower precious metal catalyst requirement.
- The project's relevance, in terms of various parameter trade-offs, was very nicely explained. However, the potential impact could be better quantified in cost terms for the specific project goals. For instance, the cost curve shown is unclear and does not indicate the expected cost reduction if the project is successful.

Question 5: Proposed future work

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

- The future work will be performed to mitigate crossover seen with some alternative PEM materials. Testing will be completed at 1000 psi and 95°C, followed eventually by testing at 5000 psi and 95°C. Catalyst formulations will be developed for lower platinum group metal (PGM) loading. Tests will be performed on HC-based membranes. Short stack tests will be performed. Long-duration tests will be completed up to 5,000 hours.
- The future work is well planned out. It would be useful to show how the efficiency of the electrolyzer varies with temperature because variable operation will lead to temperatures lower than the nominal 95°C.
- Additional testing of the alternative membranes is both necessary and planned. Actual validation at 5000 psi (with comparison to the baseline membrane) is critical.
- The project is generally effective, but it could be improved; it contributes to overcoming some barriers.

Project strengths:

- This work has great potential for direct high-pressure hydrogen production using PEM technology. Delivery of high-pressure hydrogen reduces BOP costs because most applications require high-pressure hydrogen. Higher-temperature operation increases overall efficiency. The membrane development work is innovative.

- The approach was very well laid out. The completion of Tasks 1 and 2 in the short time since project initiation should be commended.
- The industrial partner has the expertise to understand the project and to implement improvements in its product line.

Project weaknesses:

- It is not clear whether the proposed process to directly generate high-pressure hydrogen is more economically valuable than the existing techniques, which generate hydrogen at low pressures and then use compression.

Recommendations for additions/deletions to project scope:

- It would be interesting to see measurements of efficiency at temperatures lower than the nominal 95°C.
- The team should add an actual conductivity metric (in addition to using a C/P goal).