2017 – Hydrogen Production and Delivery
Summary of Annual Merit Review of the Hydrogen Production and Delivery Sub-Program

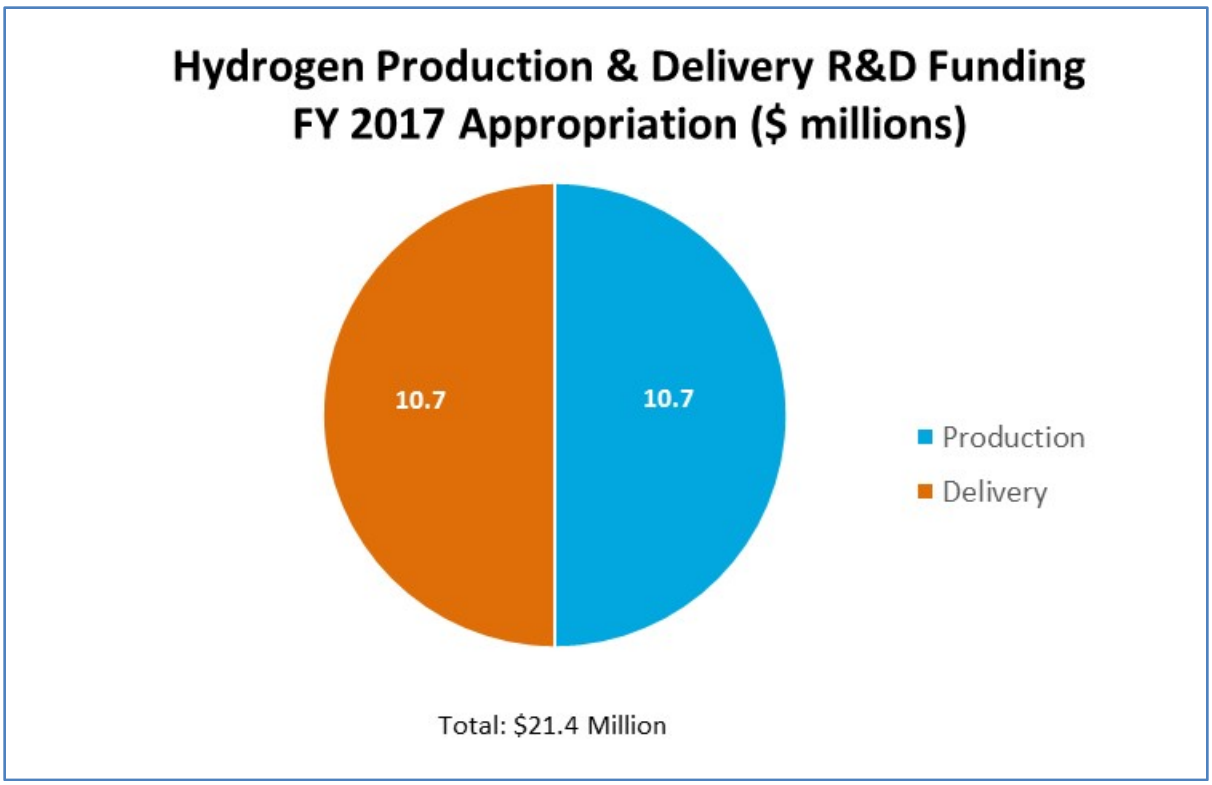
Summary of Reviewer Comments on the Hydrogen Production and Delivery Sub-Program:

The Hydrogen Production and Delivery sub-program includes a broad portfolio of technologies to produce and deliver hydrogen from diverse domestic energy resources. Production project sub-categories in 2017 included thermal and bio-fermentative conversion of hydrocarbon-based feedstocks, advanced high- and low-temperature electrochemical water splitting, direct solar thermochemical (STCH) and photoelectrochemical (PEC) water splitting, and hydrogen production pathway analysis. Hydrogen delivery projects focused on research and development (R&D) to lower the cost and enhance the reliability of technologies that deliver hydrogen to end users (liquefaction, pipelines, and tube trailers), technologies at hydrogen fueling stations (compression, storage, and dispensing) for vehicles, and technoeconomic analyses of delivery pathways.

The reviewers recognized the Hydrogen Production and Delivery sub-program as comprehensive, well balanced, effective, and well managed, with a clear strategy to achieve DOE goals and objectives. Reviewers commented positively on the relevance of the foundational research portfolio to the broader H2@ Scale initiative, and on the overarching technoeconomic framework guiding R&D priorities. They also were impressed with specific project highlights and accomplishments. The reviewers commended the sub-program's effective leveraging of cross-office and cross-agency resources (e.g., synergistic research with the DOE Office of Science and with the National Science Foundation) and strongly recommended continued expansion of such activities. The sub-program was encouraged to maintain a robust foundational research portfolio supporting important hydrogen delivery and dispensing needs to encourage early markets, as well as the needs for developing a sustainable portfolio of hydrogen production options leveraging diverse domestic resources.

Hydrogen Production and Delivery Funding:

The fiscal year 2017 appropriation for the Hydrogen Production and Delivery sub-program totaled $21.4 million. This funding was used to support foundational R&D needs identified through the H2@ Scale initiative, including early-stage R&D through the HydroGEN Advanced Water Splitting Materials Consortium and R&D to utilize advanced materials in lowering the costs of hydrogen delivery and dispensing (e.g., non-mechanical liquefaction using magnetocaloric materials, non-mechanical compression using metal hydride materials, and enhanced durability of steels using novel microstructures). The sub-program continues to emphasize leveraging of cross-program, cross-office, and cross-agency R&D opportunities and resources. The total active Hydrogen Production and Delivery R&D portfolio, including funding opportunities and laboratory call projects and joint projects with the National Science Foundation and Small Business Innovation Research program, comprises technoeconomic analysis, hydrogen materials compatibility, advanced water splitting, novel hydrocarbon reforming, non-mechanical compression, novel liquefaction, and next-generation station design. Future work is expected to continue focusing on foundational, early-stage research needs identified through the H2@ Scale initiative and the hydrogen production and delivery stakeholder communities.



Reviewer Comments and Recommendations:

Twenty-nine projects were reviewed, receiving scores ranging from 2.9 to 3.7, with an average score of 3.3. The scores indicate technical progress made over the past year across 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. There was reviewer consensus that the technoeconomic analyses performed are important to DOE objectives, particularly the identification of the long-term potential and bottlenecks of production and delivery pathways. Reviewers noted that the project has exhibited strong collaboration with DOE, industry stakeholders, and technology providers. Reviewers recommended that the key assumptions and sensitivities used in the analyses should be more transparent.

Advanced Electrochemical Water Splitting: Four 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 integrate and test renewable electrolysis systems, develop new high-temperature alkaline electrolysis, decrease the platinum group metal (PGM) loading in alkaline exchange membrane electrolysis cells, and develop solid oxide electrolyzers with a novel cell architecture. Reviewers were supportive of the projects' innovation and progress overall. In particular, integrating renewable electrolysis was praised for its enablement of clear, open, and comprehensive interaction between DOE and industry stakeholders. Reviewers commented that success in these projects offers the potential to achieve significant reduction in the capital cost of electrolyzers, as well as the cost of hydrogen production via high-temperature electrolysis and alkaline-exchange-membrane-based water electrolysis, which is critical for technology introduction on a larger scale. Reviewers suggested that enhanced collaboration would benefit the technical challenges faced by these projects and encouraged a strong emphasis on technoeconomic analyses.

Bio-Derived Feedstock Conversion: One project was reviewed in the area of bio-feedstock conversion, with a score of 3.2. Reviewers commended this project for its straightforward approach and innovative design. They noted that the project had missed scheduled project milestones but stated that the project showed reasonable progress in increasing the hydrogen production rate through improvements in both sorbent and catalyst formulations. Reviewers suggested incorporating additional cost data to better evaluate the impact of system optimization on capital cost.

Biological Hydrogen Production: Three projects were reviewed in the area of biological hydrogen production and these received an average score of 3.5. One project is focused on developing direct fermentation technologies to convert renewable lignocellulosic biomass to hydrogen, another on developing cost-efficient advanced synthetic biological generation technologies to produce hydrogen, and the third on developing a hybrid fermentation and microbial electrochemical cell (FMEC) system for hydrogen production using low-cost feedstocks. Reviewers commended all three projects on their accomplishments, progress toward goals, and significance, but they expressed some concern over practicality and ability to meet cost targets. For the direct fermentation and synthetic biosystem projects, reviewers would like to see additional cost analysis and connections to overall cost of hydrogen to better judge feasibility. Reviewers considered the cost analysis a strength for the FMEC project, and they encouraged additional work to address the electrocatalyst stability.

PEC Hydrogen Production: Three PEC projects were reviewed, receiving an average score of 3.3. The projects are investigating new materials and/or reactor designs that can operate at high solar concentration and achieve DOE efficiency goals for hydrogen production via PEC water splitting. Reviewers commended all three projects for their alignment with DOE objectives, innovation, and progress made so far. Specifically, reviewers praised the world record for solar-to-hydrogen conversion efficiency set by one project and highlighted excellent collaborations and synergies in the other two projects. The primary concern reviewers had with these three projects was that the projects might not meet all of their final targets for durability and performance.

STCH Hydrogen Production: Two projects were reviewed in the area of STCH hydrogen production, with an average score of 3.1. Both projects propose using concentrated solar power in a two-step metal oxide cycle for hydrogen production using unique reactor designs. Both projects were praised for their innovative approaches, work accomplished on the reactor designs, and progress toward meeting hydrogen production targets. For both projects, reviewers praised the collaborations, but they felt that the scope was too broad to meet all of the milestones.

Delivery Projects

Hydrogen Delivery Technoeconomic Analyses: Two projects were reviewed in this area, with an average score of 3.3. These projects included an analysis of cost drivers for fueling heavy-duty fuel cell vehicle fleets and a recently started thermodynamic analysis of liquid hydrogen infrastructure, including boil-off losses in liquid delivery infrastructure. Reviewers praised the projects for their approach, scope, accomplishments, and relevance. Recommendations included collaborating more closely with industry partners and similar international efforts, as appropriate.

Hydrogen Delivery Technologies: Two projects were reviewed in the area of hydrogen liquefaction, receiving an average score of 3.3, and one project was reviewed in the area of hydrogen pipelines, receiving a score of 2.9. The pipeline project was praised for its team composition and approach. However, reviewers questioned the delays in project schedule and ability to meet project goals. They recommended more exploration of the fundamental causes of behavior seen in experiments, leveraging industry work and collaborations to ensure relevance, and providing more information on input and contributions of collaborators, particularly the National Institute of Science and Technology. Reviewers praised the liquefaction projects for their innovative approach and progress. Suggestions to both projects included increasing industry collaboration and presenting work in a way that clearly highlights each technology's merits, potential benefits to industry, and roles of partners.

Hydrogen Fueling Station Technologies: Ten projects focused on hydrogen dispensers, compression, storage, and station operation were reviewed. They received an average score of 3.3. The three projects on dispensing hoses were praised for their technical approach, relevance, and accomplishments to date. Reviewers would like to see additional incorporation of real-world variables (such as human interaction with fueling hoses or conditions at fueling stations), exploration of the fundamental causes of material failures, partnering with additional component and/or automotive manufacturers, and collaboration with other researchers doing related research domestically and abroad. Reviewers

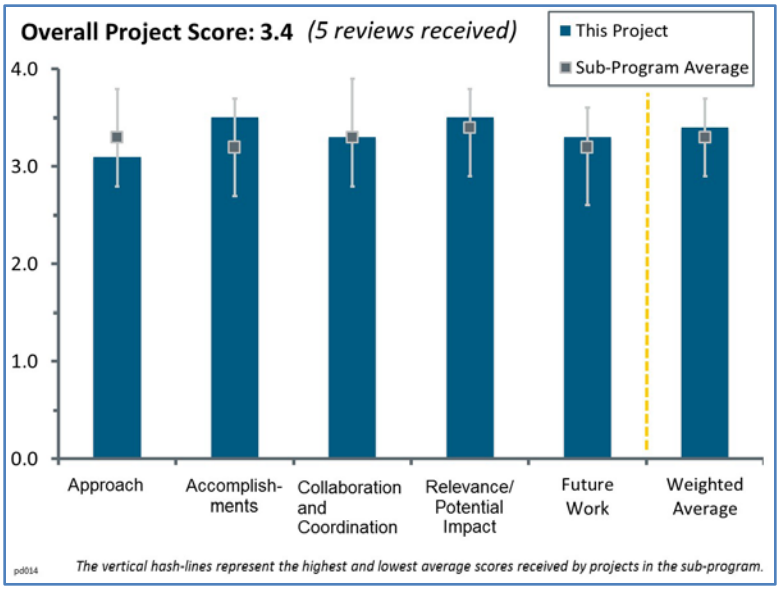
would also like the projects to communicate results to the stakeholder community (e.g., local governments, code communities). The four compression technology projects were praised for their progress to date, responsiveness to reviewer comments from prior years, and potential to lower station costs and improve reliability. Reviewers suggested technoeconomic analyses to accurately assess potential cost competitiveness of technologies being developed, and better leveraging of prior and ongoing R&D in each area. They also recommended that system-level modeling be conducted to help determine focus areas and that closer attention be paid to the specific points of failure within each technology. The project on composite vessels was praised for its progress and accomplishments. Reviewers questioned the rationale for certain design features and experimentation and recommended validation of the prototype's performance in relevant operating conditions. The two station operation projects included one on station design and one on tube trailer consolidation. Projects were highly commended for relevance to industry stakeholders, potential for lowering station costs, and strong and appropriate collaborations. Reviewers recommended that the projects share results with industry and stakeholders.

Project #PD-014: Hydrogen Refueling Analysis of Heavy-Duty Fuel Cell Vehicle Fleet

Amgad Elgowainy; Argonne National Laboratory

Brief Summary of Project:

This project will assess impacts of delivery and refueling options on the cost of dispensed hydrogen by (1) modeling refueling costs in early fuel cell electric vehicle markets, (2) evaluating the impact of design and economic parameters, (3) identifying cost drivers of current technologies, and (4) developing estimates of delivery and refueling cost reduction with market penetration. The project aims to support existing U.S. Department of Energy-sponsored tools and assist with Fuel Cell Technologies Office (FCTO) planning.



Question 1: Approach to performing the work

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

- Given that there are currently no models for medium- and high-duty fuel cell vehicles, it is reasonable to start by building a technoeconomic model from the ground up to figure out the technical/market/economic parameters that influence the industry.
- The model seems to be a very interesting model to address the lack-of-analysis barrier for hydrogen refueling stations (HRSs) to meet large hydrogen consumption requirements, such as those that serve buses. In the model, the hydrogen production cost should be included, as onsite hydrogen production using electrolysis might be cheaper than the transport of a huge volume. If this is not considered, it might lead to conclusions that are not correct, and wrong judgments might be made in terms of what infrastructure governments should support in the future.
- The task appears to have been to generate a cost model for bus-fueling scenarios. The approach is sound.
- The projects aims at the right questions and deals with important issues for hydrogen in heavy-duty fleets and the needed infrastructure. However, further activities regarding bus fleets in Europe, as well as continuous feedback from fleet owners on critical parameters, should be taken into account.
- The Hydrogen Station Cost Optimization and Performance Evaluation (H2SCOPE) model is based on passenger vehicle fuel tanks; the project appears to assume that the transition to heavy-duty bus-size tanks will show the same temperature behavior in-tank.
    - There is very limited data available from heavy-duty bus fueling. This effort should include an effort to retrieve benchmark data from operational bus fleets and verify H2SCOPE applicability.
    - Staggered fueling has an impact on the logistics of transit agencies, dependent on the transit agency. Staggered fueling may require a significant change in transit logistics similar to the change required for charging battery electric buses.
    - The project should consider including the station footprint as a parameter/variable—in most cases, this is even more of an issue than with passenger hydrogen fueling stations integrated at existing gasoline stations.
    - Fill strategy should include a “fill window,” which could be an added layer on top of a back-to-back strategy and is likely to define the number of dispensers.
    - For fleets larger than 10 buses (in the context of the next size of bus fleet being 30 buses), tube trailers are less likely to be an option, owing to logistics.
    - The larger the fleet, the narrower the fueling time window for all the buses.

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.

- Using liquid for precooling is cost-effective. Having a tap point partially through the evaporator and a second at the end of the evaporator, with both feeding a mixing valve controlled by the process temperature at the flow meter, should allow uniform precooling through the process, regardless of outside ambient temperature. This is the first modeling of multiple dispensers that the reviewer has seen—a laudable effort. Perhaps optimized multiple dispenser systems will be next.
- The team is well informed of existing refueling guidance and has talked to bus fleet owners to figure out common refueling modes, fleet sizes, and constraints. The current pathways developed seem appropriate. Much has been accomplished since the project started.
- Results are good.
    - The project should consider simulating three to four transit agencies' bus fleet operations, based on when buses return to the bus yard, to add a "one-to-one replacement of diesel/[compressed natural gas] bus fleet logistics with hydrogen" option, which will make this effort much more valuable. The staggered option can be considered only a modeling concept; otherwise, it will affect transit fleet logistics—unless it is staggered in the sense of what would happen if fuel cell buses were used as part of a larger fleet (for example, 30 buses of 100-bus fleet are fuel cell buses).
    - Fill strategy is indicative of station design.
    - Change of transit logistics equals "issues with transit unions." It is unclear how to quantify this (which does not indicate a change in approach, but it may be worth considering what the barriers are).
- In the future, heavy-duty usage of hydrogen will become very important. This study shows the refueling cost differences between various technologies. This understanding is very important for advising FCTO on what kind of infrastructure is needed and requires focus. Also, this study is very important for industry knowledge. In the presentation, the DOE targets were not shown, but DOE has some; in the future, it would be good to integrate those targets in the presentation.
- The project has a good approach to identify critical parameters for future heavy-duty vehicle station design.

Question 3: Collaboration and coordination with other institutions

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

- At this point, collaborations seem appropriate. As the models are further developed, these could be shared with fuel cell bus fleet managers such as AC Transit.
- Collaboration with fleet operators exists and should be extended throughout the project run time.
- Collaboration is appropriate for this small task.
- During the presentations, it was mentioned that there are no other studies available in the literature. This is not correct, as a big study in Europe looked at exactly the same thing and finished in March. It would have been logical for the project to examine the European Union project and use the results, if useful. Also, a question was asked about whether the tentative results from the model were cross-checked with the industry, and the answer was very unclear. The industry is not yet deeply involved in this project, and that involvement would be crucial to verifying results.
- The project needs direct involvement of a transit agency as well as a bus manufacturer and/or a company that has designed a compressed hydrogen storage system (CHSS) for buses.

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.

- Medium- and heavy-duty vehicles represent an important segment of the market that is forecasted to grow even larger in terms of contributions to overall greenhouse gas emissions and criteria pollutants. In some cases, because management of these fleets is centralized, it may be feasible to have a dedicated HRS that can serve a number of buses either staggered or back to back. Understanding the economics of this proposition and the technical barriers is key to building a business case.
- The project showed some first results on estimated costs for the various technologies, a great value in reaching DOE targets.
- Understanding the critical parameters in hydrogen supply for heavy-duty vehicles helps to improve this issue and contributes to acceptance and future deployment of infrastructure.
- This appears to be the only directly heavy-duty fueling-infrastructure-related project supported by DOE and state agencies, and while transit buses are fueled every day in the United States, DOE normally funds only medium- and heavy-duty fuel cell vehicle (non-bus) projects.
- This modeling—and more importantly, starting to look at more realistic options based on commercial volumes—is refreshing.

Question 5: Proposed future work

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

- Proposed future work items are highly appreciated. However, further parameters (e.g., on-site hydrogen production) might be interesting to consider, as would further feedback from operators to better understand critical parameters from an operator perspective.
- Future work involves the cost of liquefied hydrogen (generation, transport and road damage, pollution, and storage) versus gaseous hydrogen from on-site generation (reforming, electrolyzing). This is industrial-gas-supplied versus public-utility-supplied.
- Future work is appropriate, but there are a few more pathways/alternatives that could be incorporated in the assessment, namely:
    - Explore forecourt hydrogen production.
    - Compare results with liquefied natural gas (LNG) switching.
    - Expand assessment to other types of medium and heavy vehicles.
- Consider the non-existence of 70 MPa fast fueling (comparable to 7.2 kg/min for 35 MPa)—45-minute fueling times will not work. Liquid hydrogen boil-off losses should be addressed by industry, not by a DOE-funded project. The project should consider working with Ricardo on a fueling station cost model; Ricardo and team have a well-developed model for hydrogen bus fueling based on industry interaction.
- There was no Gantt chart available, and it seems there are no in-between confirmation points planned, such as cross-checking the results with industry. It is strongly recommended that the project make a clear plan with clear output objectives at certain times and build in some go/no-go judgment points in the planning.

Project strengths:

- This is a very important study for understanding the economics of big HRSs for buses, which is crucial for future rollout of fuel cell and hydrogen technologies. The project looks at various transport technologies and made a model to simulate each technology and change the outcome quickly when market prices are changing for a certain technology.
- The project enters into unexplored territory and makes information available for public review. This is urgently needed for understanding fueling infrastructure requirements for truck applications, and it may contribute to standardization of SAE J2601/2 fueling protocols beyond Technical Information Report levels. J2601/2 does not need to be developed in the same manner as J2601 (for cars), which is prescriptive.

- Strengths include contribution to future station design for the deployment of heavy-duty fuel cell vehicles. Identification of critical parameters for station design can contribute to decision-making for operators.
- Strengths include the start toward high-volume, multiple-nozzle dispensing and the willingness to start to look into Pandora's Box.
- Strengths include strong knowledge of the industry, strong modeling capabilities, and a good business case.

Project weaknesses:

- The project is not checking hydrogen production cost, and this is a serious weakness. For example, on-site hydrogen production by electrolysis could be more interesting economically than transporting the hydrogen on site, as bigger volumes are required. This should be addressed in the model.
- The benchmark of bus CHSS fueling temperature data is missing. There is no direct involvement of a bus manufacturer or bus CHSS supplier such as Agilent, Worthington, or Lincoln.
- Further data sets and results from other bus projects, as well as on-site electrolysis, should be considered.
- Pathways explored are limited.

Recommendations for additions/deletions to project scope:

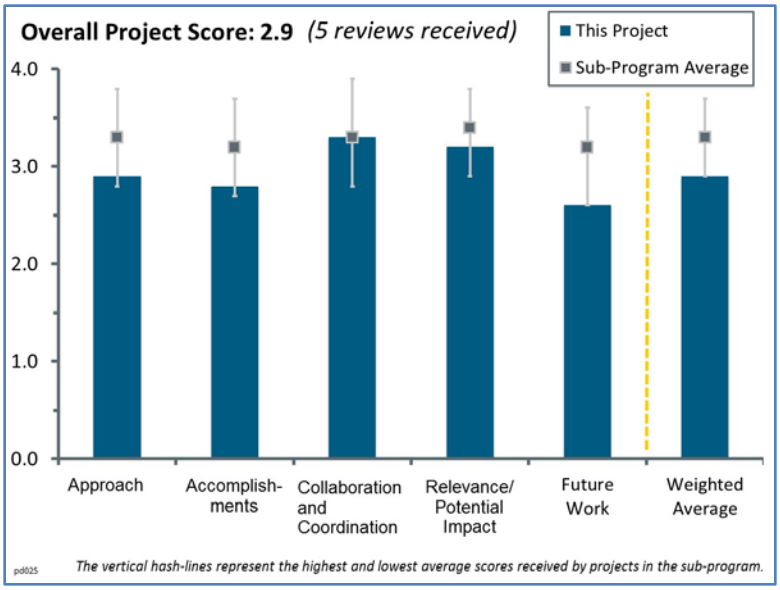
- Further feedback from fleet and station operators is crucial for relevance of parameter weighting.
  Two dispensers for 100 buses is not realistic, not even for diesel or compressed natural gas bus fleets. Take that factor out of the calculations and inquire as to what the number of dispensers is for a 100-bus fleet (conventional fuels). (For context: To fill 100 buses in 8–10 minutes with one dispenser is 800–1000 minutes, or 13–17 hours per day, which would become 6.5–8.5 hours with two dispensers—not considering the impact on logistics.) The project should include 200 bus fleet calculations to increase the value of transferability of project accomplishments to other heavy-duty vehicle applications.
- The project should:
  - Study the European project (www.newbusfuel.eu), which was published in March 2017.
  - Include hydrogen production in the model.
  - Crosscheck the results with industry and determine whether industry agrees with the findings.
- The project should explore forecourt hydrogen production and compare results with LNG switching, and the assessment should be expanded to other types of medium and heavy vehicles.
- Results should be applied to light-duty fleet vehicles.

Project #PD-025: Fatigue Performance of High-Strength Pipeline Steels and Their Welds in Hydrogen Gas Service

Joe Ronevich; Sandia National Laboratories

Brief Summary of Project:

The primary objective of this project is to evaluate the potential for modern, high-strength steels to facilitate reductions in the cost of hydrogen pipelines. Specific goals are to (1) characterize fatigue performance of high-strength girth welds in hydrogen gas and compare performance to low-strength pipe welds, and (2) establish models that predict pipeline behavior as a function of microstructure in hydrogen to inform future development.



Question 1: Approach to performing the work

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

- The project determined the fatigue behavior of high-strength pipeline steels in a hydrogen environment and found that the effect of residual stress needs to be considered. This aspect seems important when comparing welds and base metal and is a useful finding of this project. Reference was made to the development of "alternative consumable" filler metal for high-strength steel, but the background, rationale, and approach to developing this alternative filler was unclear. It is surprising that the manufacturer of the pipeline steel is not shouldering the cost of fatigue testing and weld/filler development, as it is the manufacturer's product.
- The approach to performing the work is generally good. Objectives are clear and well-thought-out, and they address a specific challenge. The test(s) chosen will yield results to address the issue, although modifications could be made to these tests to improve them.
- The objectives of the project are to determine whether high-strength girth welds are resistant to hydrogen embrittlement and develop microstructure-based predictive models of hydrogen-accelerated fatigue crack growth. High-strength gird welds are considered for cost reduction. The approach involves (1) use of the Gleeble® approach at Oak Ridge National Laboratory (ORNL) to predict/control weld microstructure, (2) fatigue testing at Sandia National Laboratories (SNL), and (3) model development at the National Institute of Standards and Technology (NIST). The overall goal is to assess the hydrogen embrittlement of X100 microstructure.
- The presentation was well defined and provided relevant testing and results to proof.
- The project timeline shows the project should be half-completed; however, nearly every task is behind in overall completion. This includes the go/no-go gate, which has passed and is projected to be completed more than six months late. The team presented no proposal for getting back on schedule so that the information developed herein can be used to inform future work.

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 project has described the importance of residual stresses in influencing the fatigue life. This aspect may have applicability to other lower-strength steels. If so, perhaps the past data on pipeline steel welds need to be re-evaluated.

- The project seems to be currently on track to achieve all the goals set.
- There is no progress at all on the development of controlled microstructure. In fact, the project has not presented any microstructure that the investigators think is going to be hydrogen-resistant (see slide 8). Fatigue testing at SNL, as described in slides 9 through 11, has revealed that X100 base metal performs similarly to the low-strength base metal (a result that was also reported by the investigators last year), but the X100 weld exhibits higher crack growth rates (CGRs). However, the presentation did not clarify the specific aspects of the microstructure that are responsible for fatigue acceleration by hydrogen. For instance, there was no justification as to why the W4 weld, a low-temperature transformation weld (LTTW), is hydrogen-resistant. As for the model development at NIST, slides 16 and 17 present no progress whatsoever. For instance, the investigators did not even discuss how the polygonal or acicular ferrite features are related to embrittlement, which is a very important ingredient in a microstructure-based model, as the project promises on slide 5.

Question 3: Collaboration and coordination with other institutions

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

- The project has a good mix of partners, including two national laboratories, a university, and an institute. These are all organizations of high repute. Work seems to have been well coordinated between these partners.
- The presenter from SNL provided evidence of collaboration with NIST and Argonne National Laboratory, which further strengthens the research work.
- The ORNL contributions to the identification of the weld microstructures can be significant. On the other hand, the project did not demonstrate what the NIST contributions are. Looking at the 2016 presentation, one cannot identify what progress has been made on model development in the last two years.

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 usage of higher-strength steel will have a significant impact on the energy cost of storing and transporting hydrogen and other energy products such as compressed natural gas.
- Reducing weld and pipeline cost is an important DOE goal, so the project's objectives are worth pursuing.
- The project goals aligns well with the transportation of hydrogen.
- While the fatigue data on X100 was presented, the presenter did not clarify whether one could conclude whether the X100 pipeline steel (at least from a fatigue perspective) was suitable material for a hydrogen pipeline. Although the project aims to reduce the as-installed cost of pipelines, it is impossible to predict the steel/pipeline industry's pricing models. For example, the industry may decide to charge a premium for X100 steel that may negate lower material cost associated with using less X100 material (i.e., thinner sections). Thus, in that case, DOE's investment in "validating" this steel for hydrogen pipelines will not have the desired impact of lowering the installation cost of pipelines.

Question 5: Proposed future work

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

- The presenter provided a clear strategy and short-/long-term planning for this project.
- Proposed future work is reasonable and will assist in meeting additional objectives.
- Regarding the Proposed Future Work slide, second bullet (Fabricate friction stir weld....), friction stir welding (FSW) is already well established as a means to generally produce better welds than fusion welds and, hence, better fatigue life. It would make sense for industry (instead of DOE) to be responsible for development of an FSW process of their products. Regarding Proposed Future Work, third bullet (Develop lab-scale high-strength steel...), there are numerous varieties of commercial high-strength steels with an

equally large variety of microstructures. Thus, the need to develop yet another steel is not clear, and it may be more cost-effective to have the industry pursue it.

- As in the 2016 presentation, it is not yet clear what the significance of the incorporation of the Gleeble approach in the project is. No significant results have been reported on the relationship of the weld microstructure to hydrogen embrittlement. The proposed future work on slide 19 is unfocused; it is designed as if the project has just started. One cannot see a systematic continuation of progress building upon the accomplishments from the last two years.
- The project failed to mention the influence of post-weld heat treatments, which are of much greater importance in high-strength pipeline steels.

Project strengths:

- The team composition is very good. The organization of the work increases the chance of success and places this team in a position to succeed. Goals are clear.
- There are very nice collaborative efforts with project partners. The relationships are clear and provide strength to the overall team.
- The project team has the capabilities to perform the proposed work and has also previously tested a variety of pipeline steels in hydrogen.
- The involvement of SNL and ORNL is a strength.
- This project is very promising in terms of solving the issues related to high-pressure hydrogen storage and transport.

Project weaknesses:

- Lack of prototypical testing is a weakness, as a better understanding of component behavior is usually achieved with well-thought-out prototypical testing. Prototypical testing is usually complicated and subject to misinterpretation if not carefully executed, but these tests yield very powerful results when carefully executed, though they are very expensive to carry out. Perhaps sponsors could consider this for future investigations. Workers may well find that predicted lifetime may change, maybe even improve, with prototypical testing. It is not certain that Gleeble-generated microstructural gradient specimens will be of much use. It might be difficult to correlate microstructure with the growth rates, especially as there may be no clear boundaries between microstructural variations. However, the thinking merits applause. It would be more useful if results could be tied to particular microstructural features such as precipitates or different phases (if they occur). It seems possible that the information already exists in literature. Investigators are encouraged not to dial out the effect of residual stresses in all instances. An attempt should be made to understand the relationship between microstructure, residual stresses, and CGRs. Residual stresses are a fact of life and can usually be linked to microstructure and then to CGR. Besides, pipes in the field will not be perfect; they will have different levels of residual stress and microstructural deformation, which will affect lifetime predictions. It is important to contribute to knowledge in this area.
- Microstructure work at ORNL and testing at SNL are not coordinated yet in a systematic way to reveal potential weld microstructures that are hydrogen-resistant. NIST contributions are not identified.
- The project needs to focus on more collaboration with the steel cylinder manufacturers and the U.S. Department of Transportation to prevent repetition of testing and evaluation.
- The project is behind schedule and is projecting a go/no-go decision six months late. Such gates are meant to be deterministic to evaluate whether the project should move forward; this should not simply be a task that can be pushed back based on changing circumstances.
- The project budget is excessive when compared to other DOE projects of similar scope.

Recommendations for additions/deletions to project scope:

- There is no explanation as to why the X100 base metal behaves similarly to low-strength steel and why the X100 weld does not. The project needs to investigate these features if it aims to develop predictions accounting for microstructural features. It is not clear how nano-indentation will produce any results useful to the J2 theory of plasticity. J2 theory is a homogenized theory with no relationship whatsoever to nanoscale. It is a collective representation of the dislocation and the broader microstructural response.

Lastly, the participation of NIST in the project should be reconsidered, as there are no tangible contributions over the past two years.

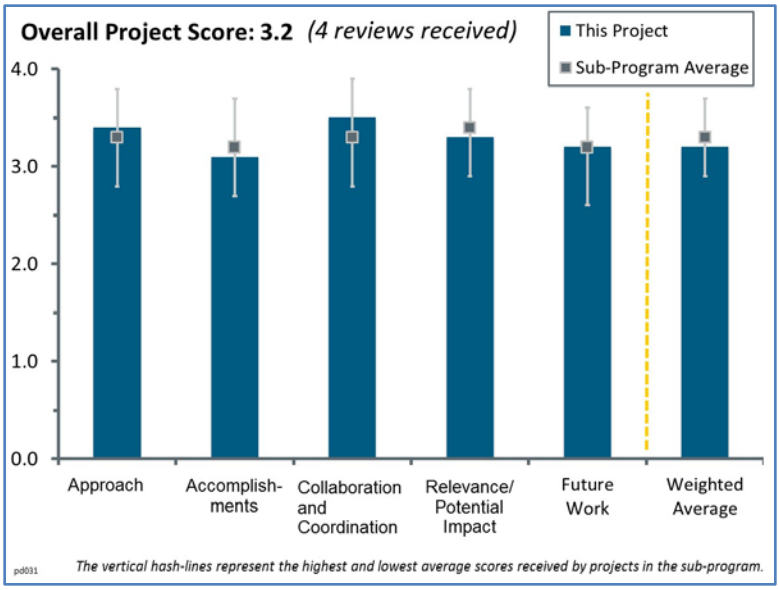
- Regarding model validation, many models fall apart when an attempt is made to use them to interpret data not used in their development. Validation of any predictive model is just as important as generating the model. The project should verify that cracks are compliant in the heat-affected zone (HAZ) measurements. HAZs are very thin, and cracks usually wander outside the zone. Investigators need to verify that what they say they are measuring is what they are actually measuring.
- This project should proceed with direct collaboration with the standards development organization, International Organization for Standardization Technical Committee (ISO/TC) 197, and ISO/TC 58 to get informed about the tremendous amount of work that has been achieved in this area.
- The team should be addressing the influence of post-weld heat treatments on microstructure. This can be addressed both in the weldments produced by ORNL and the Gleeble specimens used to evaluate the microstructure gradients.
- Development of new steel seems unnecessary, considering the large variety of steels commercially available.
- FSW development should be performed by the steel pipeline industry rather than DOE.

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

Michael Peters; National Renewable Energy Laboratory

Brief Summary of Project:

The objectives of this project are to (1) validate cell, stack, and system electrolyzer performance; (2) explore and optimize electrolyzer system efficiency and performance under varying power operation as well as integration with hydrogen infrastructure components; and (3) track the progress over long-duration testing. These objectives support the goals of integrating electrolyzers with intermittent renewable power sources and increasing the durability of electrolyzer stacks operating under variable loads while maintaining high system efficiency.



Question 1: Approach to performing the work

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

- The approach of sustained research on low-temperature electrolyzer deployment issues is commendable. Implementation of real-world systems, at increasing scale, provides much-needed public data sets on hardware performance that should help the community address technical barriers.
- The project is taking an excellent approach by assessing capital costs and improving system efficiencies and renewable system integration, which are essential areas for making renewable electrolysis a pathway for future large-scale hydrogen production.
- This is a well-designed project that addresses and validates U.S. Department of Energy goals and targets related to electrolyzer cost, efficiency, and integration into renewable energy sources. The project encompasses new technology developments in electrolysis stacks and systems over a 14-year period.
- Conducting actual testing of electrolyzer stacks is the highest-confidence pathway to full understanding and verification. The long-term nature of the project (14 years) creates complications, as the technology is constantly advancing and thus the performance of new tech stacks needs to be differentiated from older tech stacks. Investigation of separate research topics/focus areas each year is a good approach.

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.

- As this project is wrapping up, the team highlighted the accomplishments throughout the 14 years of the project duration. These have been excellent, as the team has shown continuous progress against DOE electrolyzer goals. The project has done excellent work by validating small units of 10 kW all the way to the most recent ones of 250 kW, plus looking into improving system efficiencies.
- There has been substantial and commendable progress over the years on a variety of specific performance questions: maximum power point tracking vs. direct coupling, frequency response, and decay rate assessment.
- It is not clear what accomplishments have been made since the last DOE Hydrogen and Fuel Cells Program Annual Merit Review. The score is based primarily on that. However, over its lifetime, the project clearly

has made important contributions in areas of low-temperature electrolysis efficiency, durability, and operational capabilities. Also, the project has supported development of National Renewable Energy Laboratory (NREL) infrastructure and expertise that should pay dividends for years to come.
- The project investigates and reports on the major aspects of electrolysis, including the hydrogen dryer, stack performance degradation/duration, and system efficiency.

Question 3: Collaboration and coordination with other institutions

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

- Interactions with U.S. electrolyzer manufacturers (Giner, Proton) and power providers (Xcel, SoCal Gas) have been very worthwhile. Also, involvement in the Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) initiative is important to DOE goals. As a final push on the "collaboration with others" front—particularly since the project is ending—a dedicated effort to make as much data available to the public as possible is strongly encouraged. Transfer of knowledge cannot stop at NREL doors. A good example is the best practices and lessons learned regarding balance-of-plant (BOP) maintenance, given the potential for significant adverse impacts on electrolyzer performance.
- The project seems to be linked/collaborating with numerous other DOE-funded efforts. This is commendable. Collaboration with leading electrolyzer industry members through cooperative research and development agreements and technical services agreements is a key enabling arrangement to foster clear, open, and comprehensive interaction.
- Successful collaborations with multiple stack and system manufacturers were key to validation, modeling studies, and suggested improvements to the technology.
- Working along with two excellent electrolyzer providers, Giner and Proton, clearly brings significant value to the success of this 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 **3.3** for its relevance/potential impact.

- Investigation to discern reality vs. manufacturers' claims is highly relevant. This project directly increases understanding of system operation and performance in real-world environments.
- As more renewable electricity becomes available, there is larger potential for large-scale hydrogen production via electrolysis. This project's relevance clearly supports this pathway.
- Project tasks are aligned with DOE goals, and the data collected enable electrolysis manufacturers to understand where further improvements are needed and can be implemented.
- Relevance of this work is conditional on providing unbiased, unfiltered data to the electrolyzer community. This is because the project is mostly a demonstration, problem-solving, and data-acquisition exercise. Root cause and fundamental analysis are needed to fully exploit the hard work—and that relies on publication of the data in their gory detail.

Question 5: Proposed future work

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

- The project is ending this year, so there is not much future work left, although the expertise, knowledge, and assets involved on this work will be very valuable for the initiatives around H2@ Scale.
- Technoeconomic analysis of concentrator photovoltaic/polymer electrolyte membrane (CPV/PEM) electrolysis is a good addition to the project. A final project report will be an important contribution.
- The project is nearing the suggested end date. Remaining tasks focus on technoeconomic analysis of CPV and PEM electrolyzers and continued validation of cell voltage monitoring systems developed at NREL.

Project strengths:

- Rigorous, independent assessment of technologies is a project strength. Examination of large format cells is a plus. The project is used as an assessment of technologies, and the results can influence DOE in selection of research and development investments.
- This is a sustained effort, focused on providing robust data on electrolyzer performance and capabilities. Collaboration with equipment providers and potential users is also a project strength.
- The project has good collaboration with electrolyzer manufacturers Giner and Proton. NREL's testbed provides stack and system manufacturers with a means of validating their commercial stacks and BOP components.
- The project has strong technical expertise at NREL, plus the collaboration with top electrolyzer providers.

Project weaknesses:

- There were no specific weaknesses.
- The project should assess sources of power consumption, not just report overall system efficiency/ consumption.
- There is an apparent lack of collaboration with other national laboratories, e.g., for data analysis and modeling.

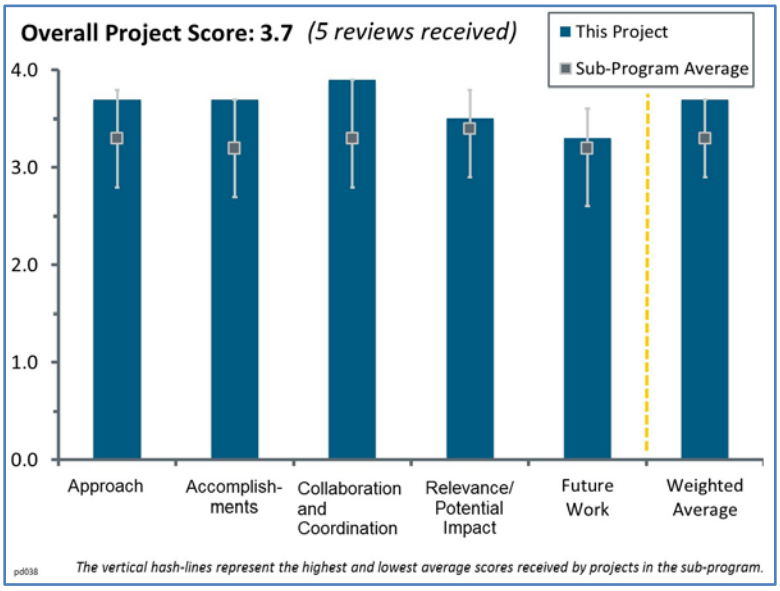
Recommendations for additions/deletions to project scope:

- Continuing this work as part of H2@ Scale is strongly recommended.
- While the project assesses various stacks, etc., it should go one step further and make recommendations as to what combination of technologies forms a pathway to achieving DOE system efficiency goals. A distinction should be made between drier hydrogen losses and drier energy inputs. Both are important. Additionally, the impact of gas pressure on hydrogen losses and drier energy should be explored.
- NREL should plan on the development of new testbeds that address emerging electrolyzer technologies such as anion-exchange membrane, high-temperature alkaline, and solid-oxide. In addition to providing operating and capital expense comparisons, the data would provide insight into the best use or selection of electrolyzer technology in various real-world scenarios.
- The project should publish learnings and make key data available.

Project #PD-038: Biomass to Hydrogen (B2H2)

Pin-Ching Maness; National Renewable Energy Laboratory

Brief Summary of Project:



The objective of this project is to develop direct fermentation technologies to convert renewable lignocellulosic biomass resources to hydrogen. The project addresses technoeconomic feasibility of hydrogen production via biomass fermentation in three tasks. Task 1 optimizes bioreactor performance, focusing on de-acetylated and mechanically refined (DMR) biomass to lower feedstock costs. Task 2 focuses on using ionic liquid pretreatment for biomass processing. Task 3 develops and applies genetic tools to modify metabolic pathways aimed at improving hydrogen molar yield. Task 4 integrates a microbial electrolysis cell (MEC) reactor into the system, producing hydrogen while cleaning the fermentation effluent to improve the overall hydrogen molar yield.

Question 1: Approach to performing the work

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

- The approach was direct to overcome the barriers of this project. First of all, the final feedstock cost was reduced by the replacement of expensive ingredients with industrial byproducts as sources of crucial nutrients for bacterial fitness, and the DMR corn stover pretreatment, a less intensive pretreatment, was efficient in releasing fermentable sugars. Second, the study of operational parameters, such as hydraulic retention time and liquid volume replacement, led to the definition of optimal conditions for higher hydrogen yield. In addition, the hydrogen yield was increased by the use of engineered *C. thermocellum*, capable of fermenting C5 and C6 sugars available in the feedstock. Third, the sequential MEC system ensures the overall conversion of biomass feedstock to hydrogen, leading to higher hydrogen yield.
- The project developed by the group is quite complete and well designed. The project contemplates some important points, such as genetic improvement and use of residues. In the case of biohydrogen production, there was concern regarding treatment of the fermentation effluent, which is rich in organic acids and was used for hydrogen production by means of MECs. The group is composed of a team of experts, and they are making progress on the stated goals while also addressing potential barriers and challenges.
- The investigators have a strong approach to addressing their proposed work. It is a good combination of innovative and sequential work. They are showing strong progress in all aspects of the work, including the logical termination of work that is not hitting go/no-go targets. This shows that their approach is actionable.
- The project has identified key barriers including hydrogen rate, final molar yield, feedstock costs, system engineering, and reactor performance, and the project uses multiple approaches to address these issues. Some aspects, such as biomass cost, are outside the scope of the project, but others are being addressed.
- The project approach seems to be well designed for maximizing hydrogen molar yield via genetic engineering. A clear example is the development of a mutant that successfully shut off two of the three competing pathways. Also, the decision to stop the fermentation of pretreated biomass using ionic liquid demonstrated serious discipline. Further progress related to eliminating costly process ingredients appears to be advancing cost reductions, although it is hard to understand how helpful those reductions are to reaching the hydrogen cost target. For next year's review, a more detailed discussion on reducing the cost

barrier would be useful. Further, it would be nice to see the conversion performance of a fully integrated system in which all of the individual components are brought together for process and cost optimization.

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

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

- The project has made accomplishments and is progressing in the bioreactor, genetic engineering, and MEC tasks. The ionic liquid task will not be continued, which is reasonable because the poor growth demonstrated would be a significant barrier to progress in that area. All tasks except for the ionic liquid task appear to be meeting their milestones. A particularly interesting accomplishment is the demonstration that an engineered *C. thermocellum* strain can co-utilize both xylose and glucose, and even seems to have a slight preference for xylose. This is unexpected, as bacteria will often preferentially use glucose, in which case long batch periods could be needed to allow the strains to first consume all glucose and then switch to other compounds. The balanced co-utilization will likely make fermentation of the mixed substrates more straightforward and allow for more bioreactor options, while increasing the portion of the biomass feedstock that can be converted to hydrogen.
- The experiments were carried out in a way to directly overcome the barriers and challenges regarding this project, which was hydrogen molar yield, feedstock cost, and system engineering.
    - Hydrogen molar yield: This barrier has been overcome by means of applying a cheap and efficient pretreatment to the lignocellulosic biomass with an increased hydrogen production rate and yield; via genetic engineering of high-rate cellulose degrader *C. thermocellum*, which led to successful hydrogen production results; and by integration with an MEC, with increased hydrogen yield.
    - Feedstock cost: This barrier has been overcome by achieving high-rate hydrogen production from a byproduct, lignocellulosic biomass; cheap and efficient feedstock pretreatment (DMR) with increased DMR loadings; and successful hydrogen production using industrial waste or industrial byproducts as a source of supplementation, replacing expensive ingredients and leading to a growth medium cost reduction of 49%.
    - System engineering: The use of a combined electrochemically assisted microbial fermentation to convert the fermentation byproducts to hydrogen gas greatly increases the overall hydrogen yield from lignocellulosic feedstocks. Achieving a high rate of hydrogen production with a non-Pt-based cathode—stainless steel wool, a cheaper material—is a great milestone for the system engineering performance.
- The principal investigator (PI) presented accomplishments for four task areas and noted good progress in all areas. The research team has produced a high-impact publication in *Proceedings of the National Academy of Sciences*. The PI noted that Task 2 was not hitting the stated milestones, and therefore, Task 2 was terminated. This was a logical action, and it was good to see resources redirected when a project is not hitting project goals. The overall progress on the project is outstanding. Each individual task has made meaningful steps forward, and all of the tasks are synergistic.
- The project seems to be making progress and is meeting milestones. The group has reached the hydrogen production rate target using DMR. The pre-treatment is an important step for project progress. However, the tests with ionic liquid did not see progress owing to the cost of the methodology. Perhaps it would be possible to test other methods of treatment.
- There were not enough data given to determine whether the cost targets can be met.

Question 3: Collaboration and coordination with other institutions

This project was rated **3.9** for its collaboration and coordination.

- The project has multiple collaborators that leverage expertise in different areas, including a long-term partnership with Pennsylvania State University (Penn State) to develop MECs to run on fermentation effluent, recent collaborations with Lawrence Berkeley National Laboratory (LBNL) and Sandia National Laboratories (SNL) to evaluate a different biomass treatment method, and a no-cost collaborator at University of California, Los Angeles (UCLA) involved in the pathway engineering work.

- This project included different approaches to achieve the goals of reducing feedstock cost, optimizing system design and operation, and increasing hydrogen yield. These approaches involved different areas, such as chemistry, process engineering, and molecular biology. Therefore, a high degree of interaction among institutions and researchers was necessary and successfully done. Each laboratory (institution) worked on its specialty to achieve the goals together.
- Collaboration appears to be excellent and includes major research groups. There is great cooperation between the partners, who have a good deal of expertise on this subject.
- The National Renewable Energy Laboratory (NREL) has a number of symbiotic collaborations including those with Penn State, UCLA, and SNL, as well as the attempt to work with LBNL.
- The PI is doing an excellent job of leveraging collaborations and other resources to maximize the impact of this 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 **3.5** for its relevance/potential impact.

- The PI has presented a logical project to improve and operationalize hydrogen production and MECs. The PI has consistently produced strong results. The relevance of the work to supporting a diverse Hydrogen and Fuel Cells Program is high. This work will likely have a moderate to high impact on the field.
- This project covers the main steps to sustainable hydrogen production from wastes. In addition to hydrogen production from wastes, the project includes steps such as genetic engineering to improve waste utilization by microorganisms and effluent reactor use. The effluent of dark fermentation, which is rich in organic acids, could be used for biogas production or, as in this case, to produce hydrogen using MECs. The design project includes cost reduction in each step, addressing the Multi-Year Research, Development, and Demonstration Plan (MYRDDP).
- Although it will be difficult for the project to meet the 2020 hydrogen cost target, the innovative genetic technology approaches will certainly help to inform other bioreactor projects using lignocellulose. For instance, the elimination of expensive enzyme cocktails and replacement of extract with industrial corn steep liquor are useful discoveries to advancing bioreactor processing in general.
- The project was carried out outstandingly. However, one of the main goals of MYRDDP is to reduce the cost of hydrogen to <$2/gge. Despite the great performance of the *C. thermocellum* mutants for producing hydrogen from hydrolysate, the use of pure culture of bacteria or pure culture of engineered bacteria systems may not be a low-cost process for practical-scale hydrogen production because of contamination issues. Therefore, this approach to increasing hydrogen production may not be the best possibility. The use of sequential fermentation–MEC systems with mixed microbial culture might be preferable for achieving low-cost hydrogen production.
- The project is making progress toward the identified goals and objectives for biological hydrogen production work and has identified additional objectives needed to support the overall goals, such as increasing feedstock loading. To understand the degree of progress, it would be useful to include information about comparable results from past years or the start of the project. For example, the bioreactor performance milestone was clearly met (and slightly exceeded the target), but the progress over past years was not entirely clear.

Question 5: Proposed future work

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

- The proposed future work goes toward achieving new milestones in increasing hydrogen production yield and efficiency, using complex substrates, and reducing the overall cost of the hydrogen production process. Tasks involve examining new alternative materials and catalysts for the cathode and increasing MEC performance.
- The proposed future work is highly relevant and will provide useful results.
    - Task 1: The optimization of hydrogen production seems reasonable.

    - Task 4: The further improvement and the tests with others materials seem reasonable. Showing the biohydrogen production long-term stability is interesting.
- The work proposed for the next steps will continue to build on the progress of the different tasks. There are several separate lines of ongoing work, and it is not clear when these improvements will be combined and tested or whether there are plans to do so at a certain point in the development process. For example, for fermentation, there are separate lines of work on bioreactor performance optimization, xylose and glucose co-utilization, media cost reductions, and the enzyme mutations; and it is not certain how the different changes will interact—for example, how the mutant hydrogen production will be affected when the strain can utilize xylose and is growing in the lower-cost media. The new proposed life-cycle analysis work with Argonne National Laboratory will provide useful information about the impacts and potential benefits of this pathway, which will help in understanding the relevance and potential impact on the overall DOE goals for hydrogen production.
- The proposed future work is logical and will make progress toward the stated project goals. It would be good to see some additional work to address the long-term stability of the engineering strains. Additionally, it would have been good to see a plan for how funds from Task 2 would be redirected (if applicable).
- In general, the next steps are logical progressions in a particular mutant that can block all three competing pathways. It would have been helpful to have Bruce Logan present to answer questions related to progress with alternative cathode materials along with next steps. It is not clear whether the stainless steel wool is a good enough replacement for Pt/C. The expected stability limitations are also unclear.

Project strengths:

- The NREL work on genetic engineering of *C. thermocellum* has resulted in strong progress in the area of metabolic engineering to improve hydrogen production. The project team is applying this to multiple areas, from addressing sugar metabolism by engineering xylose utilization to addressing redox pathways with the ferredoxin work. The decision to end the work on ionic-liquid-treated biomass in Task 2, based on the poor growth of the *C. thermocellum* in the ionic liquids, will allow the project to focus on more promising directions.
- Covering the main steps for the biohydrogen production, the project has as its main strengths:
    - The search for alternative methods for the pre-treatment of lignocellulosic biomass
    - Cost reduction using wastes
    - Redirection of the metabolic pathways, removing those that compete in hydrogen production
    - Reduction in the amount of precious metals needed
    - Utilization of organic-acid-rich effluent from the fermentative reactor to produce more hydrogen
- This is a strong project with an overall goal of improving direct fermentation technology. The PI consistently produces high-quality work. The four tasks are interrelated but not interdependent, which is an asset. Each task has a measurable goal and specific milestones. Overall, this is an exciting project, and future work is pleasantly anticipated.
- Some of the project strengths are integration among institutions and research groups to achieve the best result for each task, the complete study of main barriers for biological hydrogen production from biomass (biomass pretreatment, system design and operation, and microorganism performance) for low-cost hydrogen production, and the planning to achieve the milestones and targets.
- All of the collaborators seem to be making significant advancements toward the collective whole.

Project weaknesses:

- No significant weaknesses were noted. Additional analysis of engineered strains would strengthen the work.
- Though there has been progress in the bioreactor performance, the noted high variability of performance during use could be a problem when scaling up, both in volume and in feedstock concentration, and make it difficult to identify what issues are due to actual poor performance and what are due to variation. The source of this variation is not clear. For the work on reducing culture media costs, it would be useful to have a comparison of what existing large-scale fermentation systems use.
- The project is well designed, attending to the DOE goals. However, one of the steps of the project is cost reduction of the feedstock, which relies on the use of waste. What is not very clear is how the hydrogen

will be produced by the mutant *C. thermocellum* using wastes. Contamination probably will occur, and the production period will be reduced by the contamination. In this way, a sterilization step probably will be considered, increasing the production costs.

- The only project weakness, but an important feature, is the use of bacteria pure culture to produce hydrogen. Pure culture systems demand higher operational steps and costs in order to prevent system contamination.
- The lack of details related to overall costs of hydrogen is an apparent weakness.

Recommendations for additions/deletions to project scope:

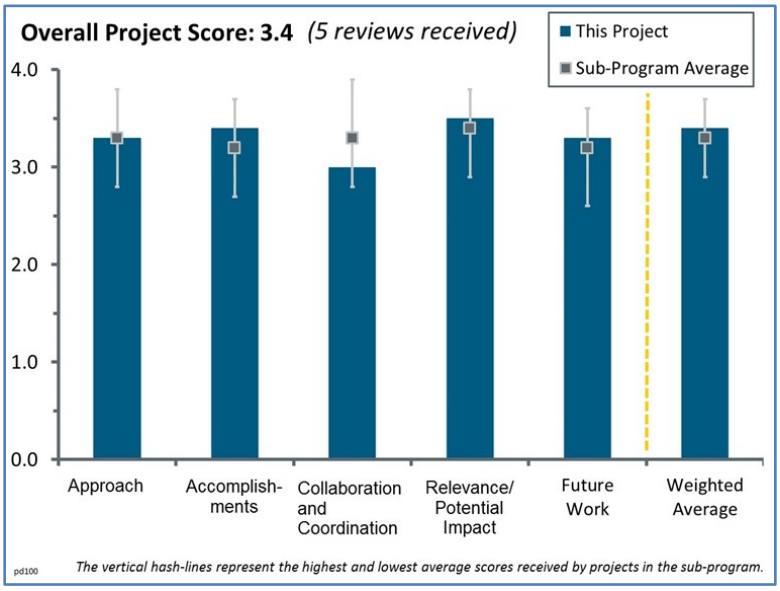
- A strong recommendation for addition to the project scope is to consider the possibility of system contamination by microorganisms present in the feedstock. The project should carry out tests without previous sterilization of the culture growth medium and feedstock to see whether there is any effect on the process performance. Another recommended addition to the project scope is to consider the use of methanogens inhibitors or shock pretreatment to the microbial mixed culture used in MECs to avoid methane production in long-term fermentation.
- Using the fermentation effluent in at least some future MEC experiments would strengthen the connection between the fermentation tasks and the MEC improvement task and provide information on how well the MEC improvements work with actual effluent. While this has been demonstrated in the past, there have been significant changes to both systems, such as the development of a xylose-utilizing strain. Though this was not listed in the slides as future work, it was mentioned in the question-and-answer session.
- Other methodologies of biomass pre-treatment should be included in the project design.
- Cost details should be included in next year's presentation.

Project #PD-100: 700 bar Hydrogen Dispenser Hose Reliability Improvement

Kevin Harrison; National Renewable Energy Laboratory

Brief Summary of Project:

The objective of this project is to characterize and improve 700 bar refueling hose reliability under fueling conditions expected at heavily utilized hydrogen fueling stations. The National Renewable Energy Laboratory (NREL) designed a test system that subjects refueling hose assemblies to pressure, temperature, mechanical, and time stresses. The high-cycling test reveals the compounding impacts of high-volume 700 bar fuel cell electric vehicle refueling, which has yet to be experienced in today's low-volume market.



Question 1: Approach to performing the work

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

- The approach is excellent, with comprehensive validation.
- The approach and amount of funding ($633,500) are commensurate with the importance of this work. The project has been ongoing from 2013 to the present. It would have been preferable for this project to be shared with state and local governments active with hydrogen refueling stations and the proposed metrics with the end-user community (station developers, both large and small and established and new). The approach included metrics to determine leak evaluation and leak locations on the hose. Risk mitigation based on comments from the 2016 Hydrogen and Fuel Cells Program Annual Merit Review looks good. The issue is that the hose passes leak checks, but upon further inspection, leaks were found. Temperature's impact on leaks was evaluated, but it is not apparent that the impacts of novice users (real people) were evaluated. (The speaker explained that for safety reasons, robots were used instead of people.) The project looked at material choices on leaks and permeation. Although 700 bar is used, 350 bar is not, and no reason was given. The approach of using graphics to communicate is very good. Perhaps the user community can be told about the issue of the leaks occurring near the crimp on the hose. Efforts are underway to share results with all of the California station operators, but it is not clear who can assist with this information-sharing process. Hose replacements are expensive if one includes the station downtime. An Energy Systems Integration Facility (ESIF) portal is mentioned, but the portal is not well-known; the project team should get the word out. Note: valve leakage is noted as important, and more information may be needed here. If the valve leakage information could also be disseminated, it would be appreciated. Some writing can be improved: "Hose consumption larger than planned with new heat exchanger." Perhaps this means "Replacing hoses can be more expensive than replacing the heat exchanger."
- This is the fourth year of this project. During this project, three hoses have been tested for reliability under conditions that would be representative of a hydrogen dispensing station. Over the past year, one hose has undergone testing. The conditions tested were pressure cycling, range of temperatures of inlet hydrogen, and mechanical stressing of the hose using a robotic actuator. The project test protocols include checking for leaks using a hydrogen sensor, leak detector (tape), pressure loss measurements, and mass flux calculations using a permeability equation. Previous tests have included mechanical testing (torsion), scanning electron microscope (SEM) analysis, and other materials characterization. Overall, the work seems to be methodical and the objectives appear to have been met. However, some questions could be raised, as this large investment in equipment and personnel costs has resulted in the testing of just three

hoses, and all from one single vendor. It is not clear why more samples were not tested. It was not stated if this is the only vendor for this type of product.
- The approach is well-thought-out. One comment would be that the fueling profile, while meeting SAE J2601, does not cover all the possible scenarios of the pressurization of the hose. It would be good to test in "most severe" situations, which might include "rapid" pressurization to full pressure in both cold and warm conditions.
- Testing evaluates pressure, temperature, time, and mechanical stresses on a refueling hose and measures the leak rate of the hose. The real learning will come with the analysis of the material post-failure. The problem is the inability to test a variety of dissimilar materials and to vary parameters. The fact that the test-stand tube has continued to operate after 4700 cycles with consistent small leakages and with California station operators revealing that tubes in service failed after 1700 and 1000 cycles indicates that, despite the effort to reproduce all these effects in the laboratory, other key influences are not included.

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.

- Overall, the work is quite solid, and the objectives appear to have been met. The principal outcome seems to be that the hoses are actually quite good. Although they all display some level of leaks, none of them can be classified as "failed." In that sense, the lessons that could be learned from this study are probably lessened; had the hoses undergone actual failure, it might have led to clear directions for improvements. The permeability measurements and calculations clearly indicate that the rate of leakage from the crimp region is much too high to be accounted for purely by dissolution of hydrogen into, and diffusion through, the polymer. It has been convincingly shown that mechanical degradation of the hose material has led to the formation of cracks or other mechanical defects through which hydrogen has found a pathway to leak instead of diffusing through the polymer material itself. This could be confirmed by conducting SEM measurements of the hose sample adjacent to the crimped end. Cross-sections of the hose could be examined at, say, one-inch intervals, away from the crimp. The samples nearest to the crimp might be expected to display the greatest density of cracks or other mechanical defects.
- A DOE goal is to make progress on performance indicators, and the speaker mentioned that the set-up "passes all [National Fire Protection Association] (NFPA) leak checks."
- Project results contribute to safety of hydrogen refueling and therefore to the acceptance of the technology.
- Progress is good, but maybe some thought is needed as to how to expedite the testing to get more cycles in a shorter period. Testing has been limited in terms of number of hoses and number of cycles.
- It is not clear what the overall project and DOE technical requirements are for the number of cycles. It appears that the cold cycle results in higher hose permeability. Operating at or near the glass transition temperature also results in high hose permeability. This should be investigated further.

Question 3: Collaboration and coordination with other institutions

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

- Overall, the most intensive collaboration has been with the Colorado School of Mines for SEM imaging and torsion rheology benchmark testing. However, this collaboration was completed in an earlier period. Other collaborations are all minor in nature.
- The project should continue to work with the NFPA, International Organization for Standardization, and Canadian Standards Association code committees to address limitations of codes for hydrogen leakage. This project could help drive these committees to develop a limit for hydrogen leakage.
- Perhaps the California governmental agencies can be integrated/included in this study/project. There are 27 retail hydrogen refueling stations in California, and the user community is disparate; users' care and handling of hoses varies, and maybe best practices could help to lower the operation and maintenance of this expensive part of a hydrogen refueling station.
- Interaction with station operators is good, although other hose producers should be included in tests.
- It would be helpful to test some other hoses, although the supplier base is limited.

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 project does address the goals and objectives of the Multi-Year Research, Development, and Demonstration Plan. This testing also provides the opportunity to evaluate valves, O-rings, connections, and break-away devices. Any failure of these components should also be analyzed to determine failure mode. Such an analysis should be done not just on the hose since these other components are also costly to replace and result in refueling station downtime.
- The goal of this project is to improve the reliability and thus reduce the cost of 700 bar hydrogen refueling hose assemblies by identifying points of failure. The goal is consistent with DOE's Hydrogen and Fuel Cells Program (the Program) goals and objectives.
- This work supports and advances progress toward Program goals and objectives in that it addresses the impact refueling station hose leakage and care and handling can have on the operations and maintenance of stations. Perhaps best practices are needed here.
- The project contributes to the safety of hydrogen refueling and therefore to the success of hydrogen as a market-ready fuel.
- Hose reliability is an issue, and this testing is important to improving safe operation of dispensers.

Question 5: Proposed future work

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

- The future work is explained, and this includes sharing the data with station developers. The future work includes the ESIF portal to get the word out. Note: valve leakage is noted as important, but this aspect is not developed in this study/paper. Including valve leakage would be appreciated. The presenters discussed the permeability experiment, which is not being run, as well as how repetitive bending causes cracks in hoses. Perhaps these can be included in future work.
- The project should evaluate the failed hoses provided by industry with the same suite of analytical methods as planned for the hose being stressed on site. The project also needs to investigate further why hoses fail significantly more frequently in actual operation as compared to the laboratory setting.
- The project should put more severe cycles on hoses to accelerate testing and/or simulate worst-case scenarios in the field. Maybe the project could work out a system to test multiple hoses at once.
- Plans to include further hose producers for testing samples are highly appreciated.
- Proposed future work seems reasonable.

Project strengths:

- The strength of this project is that the work is needed since hose replacement is an expensive part of a station's operation and maintenance. Standards compliance is key, and when the speaker was asked about the potential for leakage causing a lack of compliance with SAE J2601 (fueling protocols), the speaker commented that this effect occurs prior to a leak, causing lack of compliance with SAE J2601.
    - Another strength is that the project evaluated the hose from the station operator's point of view; the project evaluated the pin pricks required by standards (this is good).
    - Three-year-old hoses were tested, and no changes were observed. This is a practical side of this work. Some hose failures are due to internal dust, and the speaker explained how this work eliminates this as a factor.
    - The project will work with standards groups to explain the impact on standards, which is very much needed.
    - Preventative maintenance can identify hoses for early replacement, and this leads the way to providing information about the need for preventative maintenance for hoses.

    - When asked why people are not used for the testing, the speaker commented that the robot is highly accurate and measurable and that this is part of this analysis. Additionally, robots are used to protect people from accidents.
    - The speaker was asked whether the project could do some work on developing a harness to hold the hose so the fuel cell electric vehicle driver who uses the station will not have the propensity to drop this expensive part and possibly break the nozzle (and cause more cracks in the hose). The speaker agreed that would be a good future part of the project.
- A comprehensive evaluation protocol has been created to test the reliability of hoses for dispensing hydrogen. The protocol consists of pressure testing under different temperatures, mechanical stressing using a robotic arm, checking for leaks using a hydrogen sensor and leak detector tape, pressure loss measurements, mass flux calculations using a permeability equation, mechanical testing (torsion), SEM analysis, and other materials characterization. The work has been done with reasonable care and has paid good attention to the scientific method.
- There are great benefits to repetitively evaluating hoses with simultaneous pressure, temperature, and mechanical stresses mimicking real-world operations, following SAE J2601.
- The project has a great test set-up and ability to test a critical component of hydrogen fueling.
- Results of this project can ensure the safety of the refueling process for users. Therefore, the project contributes to the acceptance and success of hydrogen as a fuel.

Project weaknesses:

- In the context of operation and maintenance, preventative maintenance can be accomplished, and a hose can be replaced prior to wear and tear of the hose. The presenter touched on this, but more information here would have strengthened the presentation. The presentation does not explain how the user interface with the nozzle is important to the user interface of the hose (the speaker explicitly mentioned this is not included).
    - Some writing can be improved: “Hose consumption larger than planned with new heat exchanger.” Perhaps this means, “Replacing hoses can be more expensive than replacing the heat exchanger.”
    - The speaker did not mention the location of the station until asked; the speaker mentioned that the station is at NREL in Golden, Colorado.
    - The speaker did not explain how this project works with anticipatory changes to the standards; it would be appreciated it if more mention were made here.
    - The speaker mentioned that SAE J2601 contains no leak criteria, but it is not clear whether the speaker wants SAE J2601 to contain leak criteria.
    - The speaker did not dwell on how leaks can be mitigated for applications when people actually use the hose, e.g., whether people change the number or location of leaks.
- Only three hoses have been tested overall, and all from the same vendor. This is a rather small payoff from a large investment in personnel and equipment.
- Until now, only one sample was tested, so the relevance and reproducibility of tests for other hoses cannot be assessed.
- The experiment needs to be able to isolate and mimic the conditions in the field to better understand the failure modes that the laboratory experiment is not addressing.
- The project needs to test faster and under more severe conditions to better replicate some of the failures seen in the field.

Recommendations for additions/deletions to project scope:

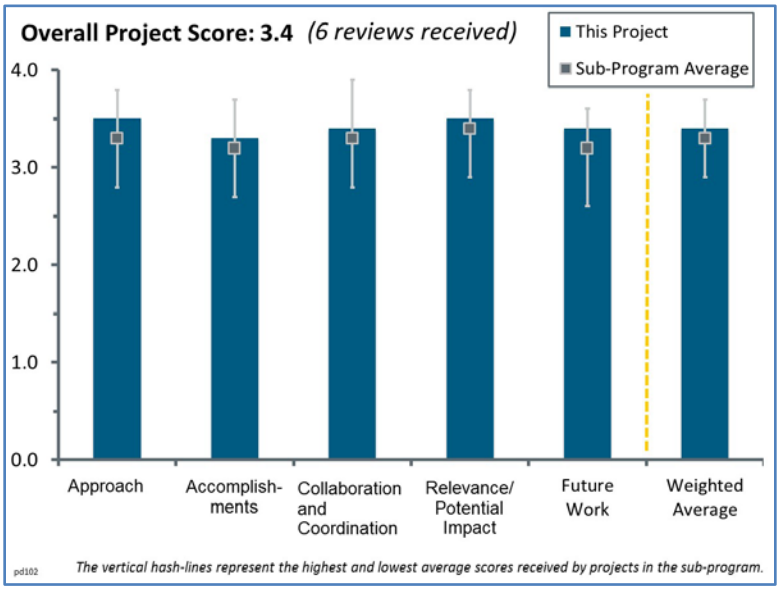
- Work is underway at other national laboratories to evaluate material permeation as a function of temperature, pressure, and frictional effects for polyoxymethylene in hydrogen. Efforts should be coordinated. A full suite of testing should be performed on the hoses that failed in the field. It appears that failure data are slow in coming for the hose testing in the laboratory. The failed hoses in the field could provide the information the project is trying to obtain in the laboratory.

- The project should broaden/widen the dissemination of this work as it continues. Hoses are expensive, and the user community needs some best practices to protect hoses that undergo wear and tear. Perhaps this work can be used to predict when the hose can be proactively replaced.
- The project should test under more rapid pressurization at both warm and cold temperatures and seek hoses that have failed in the field and apply analytical techniques to look at failure modes.
- Testing other hose samples from other producers to confirm results would be interesting. Recommendations for future design as well as testing standards should be developed from the results of the project.
- Detailed SEM analysis might provide critical evidence about the cause of leakage (mechanical damage) near the crimps.

Project #PD-102: Hydrogen Production and Delivery Cost Analysis

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 meet U.S. Department of Energy (DOE) P&D cost goals of <$4 per kg H_2 by 2020; (4) provide analyses that assist DOE in setting research priorities; and (5) apply the Hydrogen Analysis (H2A) 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 **3.5** for its approach.

- Technoeconomic analyses (TEAs) provide critical information to DOE and the broader community. Application of a consistent TEA methodology across the quite diverse set of pathways in the portfolio, such as that utilized by Strategic Analysis, Inc. (SA) for this project, is exactly what is needed. The next step—publishing those results, such as in DOE records, SA reports, and H2A model cases—is equally important, and SA does a good job. One area for improvement for SA is helping the community understand how scale-up affects the cost—not just the equations, exponents, etc. but key assumptions and sensitivities; this is the aspect of the TEA that usually draws the most questions/doubt.
- This type of work has been ongoing for many years in the Fuel Cell Technologies Office (FCTO) Hydrogen and Fuel Cells Program (the Program), and SA is the leader. While it would be nice to diversify the DOE portfolio for TEA, if the leaders can perform the work, then there is no reason to look any further. The project scope seems well defined and of interest to FCTO, and therefore, inherently it is integrated with other efforts in the FCTO portfolio. It is also nice to see TEAs of completely new designs (e.g., the REP work).
- The approach to this work is excellent: combining well-established DOE models together with SA internal models to provide a cost indication for hydrogen pathways that have been shown to be technically feasible (such as reformer–electrolyzer–purifier [REP] and WireTough's storage vessels) and that may have a direct impact on total hydrogen cost reduction.
- The approach for the cost analysis for the Hydrogen Production and Delivery sub-program is a very standardized, systematic process that is unbiased, using principles of Design for Manufacture and Assembly (DFMA) in conjunction with expert input.
- The approach is well conceived. There are no major suggestions for improvement.
- Technology readiness levels (TRLs) are part of a very good approach; new steps are an improvement for low TRLs.
    - Regarding dark fermentation, there is excellent linkage between laboratory and analysis work.
    - Regarding REP, it is not clear why a company's system is being endorsed. This is commercial technology, the data is not transparent, and there is no uncertainty analysis.

- Regarding WireTough, this does not seem to be pre-competitive technology either. It is not clear why the company is not testing the cylinders themselves and doing TEA.

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 accomplishment with regard to completing the REP analysis is excellent. It is also really interesting to see the progress being made on WireTough's storage technology. This looks like a forecourt storage technology that could be a cost-effective option. It would be interesting to see whether scaled-up designs could be assessed.
- This incarnation of the project was ongoing for only around six months prior to the due date for the Program Annual Merit Review (AMR) talk, although it does seem somewhat like a continuation from the 2016 work. Notwithstanding, the progress is strong and sufficient. This team's proven success in this work for DOE suggests the team will succeed at a high level on this project, too.
- There has been good progress in analyzing the methods suggested by DOE. There are no major concerns, although of course there are always questions about the detailed numbers that go into these types of analyses. SA is well aware of this and has a good approach to do as well as possible.
- The project investigators accomplished what DOE asked to have analyzed, which may not reflect the most high-priority technologies within the Program. The investigators updated a TRL determination methodology that should be applied across the sub-program portfolio to help prioritize costing and funding to help accelerate success of late-stage R&D.
- Regarding TRL levels, there is good progress. Results produced for dark fermentation are interesting. More needs to be done but mostly at the laboratory level. REP results are already available, but uncertainty analysis is needed. The cost assessment for WireTough seems to have already been completed.
- SA continued to do good job of fleshing out relative economics—and key R&D needs—of hydrogen production pathways by finalizing another two cases. The third case, related to compression, storage, and dispensing, is important because distribution and station costs can be on par with production costs, so it is good for SA to assess novel compression, storage, and dispensing strategies. However, the lack of transparency for the REP case was disappointing. It is difficult to discern key bottlenecks, R&D needs, and limitations; perhaps the full report will be more informative. If the lack of transparency is because the key information is proprietary, that probably means DOE should not have been involved.

Question 3: Collaboration and coordination with other institutions

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

- There appears to be excellent communication with DOE. SA clearly reaches out to stakeholders, technology providers, and industry contacts to understand technology and get the best possible input. It would be good to see direct connections made with national laboratory modeling efforts where there is overlap, for example, Argonne National Laboratory (ANL) modeling and National Renewable Energy Laboratory (NREL) analysis. This would probably need to be facilitated by DOE.
- The project is primarily a collaboration between SA (prime), NREL, and ANL to provide technical expertise on different technologies. The investigators also interact with various technology investigators to provide input on their technologies and processes.
- While SA collaborates with appropriate groups, the number of groups is small, so there may be considerable bias. Therefore, it is nice to learn that SA sends questionnaires to many experts, although indicating an average +/- standard deviation per technology would be helpful for each high-TRL and low-TRL case.
- Good collaboration has been demonstrated with both NREL and ANL. It would be good to see what the contribution has been from the industry collaborators such as FuelCell Energy (FCE) and WireTough.
- Coordination is good. There is no reason for concerns.
- Collaboration has been adequate, but it would be good to have additional feedback from industry on the REP and pressure vessel analyses to ensure that this type of analysis is not favoring just two companies.

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 analysis performed on this project is important, as it provides a cost indication for technologies that have been demonstrated to be technically feasible. This will provide DOE with an indication as to which R&D areas could be given increased focus to have a larger impact in progressing the Program goals and objectives.
- TEA is very important to DOE objectives, as it helps in identifying long-term potential (especially relative to other options) and pinch points where R&D is needed. SA does a thorough and trustworthy job of it. Transparency about key data and assumptions (where key data is measured by those with the largest impact on final results) is of tremendous value to the greater community. SA mostly meets the mark here, too. When SA cannot, DOE should be questioning its involvement in that particular technology.
- TEA is completely critical to the Program as well as any work funded by the DOE Office of Energy Efficiency & Renewable Energy or the Advanced Research Projects Agency-Energy (ARPA-E). Irrespective, if SA analyzed cases for FCTO in the past that have been around long enough now to approach future case scenarios, it would be good to know whether those technologies have been able to meet their long-term goals. The reason is that the long-term goals often seem slightly far-fetched, driven solely by DOE targets, and likely unattainable in practice.
- This project brings a level of practical assessment to the portfolio and demonstrates the potential for these technologies to meet DOE cost targets.
- Analyses provided by SA are extremely helpful for assessing various approaches and planning what areas to work on next. This clearly advances Program goals.
- A TEA of the technologies presented was needed to understand their status and feasibility, particularly for the dark fermentation technology, since it is at such a low TRL. The REP and WireTough technologies are also important to understand, but the companies should be conducting their own analyses and providing FCTO with data if they are looking to collaborate in government programs. It is not appropriate for a government-funded program to pay for all the analyses.

Question 5: Proposed future work

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

- It is a very good idea to take a second look at steam methane reforming plus carbon capture and sequestration. This is an absolutely critical baseline. The project should consider adding the byproduct hydrogen (e.g., from ethane steam cracking) and other novel compression, storage, and dispensing technologies to the list for prioritization.
- The continuation of the proposed work on WireTough's storage technology seems appropriate as a complement to the existing analysis. It is good to see that carbon capture and sequestration advances will be implemented in future analyses.
- The proposed future work is reasonable and will advance our knowledge of the status of emerging technologies.
- A good scope of ideas and critical barriers is presented. However, it is noted that a new pathway for PEC hydrogen will be conducted, yet several have already been conducted in great detail and shown that the pathway to commercialization would require several miracles. There is no mention of milestones and targets, but this project may not have them, and presumably SA will perform sufficient analyses, as they have in the past.
- Future work was well explained. There are no concerns.
- It seems as though there has been an extreme amount of effort into costing various aspects of compressed storage technologies not completely different from what is being used today. More emphasis could be placed on P&D pathway technologies that show potential for large-scale renewable production. While photoelectrochemical (PEC) is a developing technology, it is still too immature to focus on costs currently,

and there are other P&D technologies that are closer to being demonstration-ready, such as the thermochemical cycles, that could benefit from a rigorous cost analysis.

Project strengths:

- Strengths include the sustained development and application of TEA methodology, resulting in cases that can be meaningfully compared and that are published in detail to promote transparency.
- SA is carrying out important analyses using an informed approach and provides critical information on various approaches and technologies relevant for the Program.
- The team has strong knowledge of the industry and good analytical/engineering capabilities.
- Investigators and collaborators have a strong understanding of costing principles and P&D technologies.
- SA has a great track record of performing TEA for FCTO, and their current output is representative of that.
- The cost analysis process is very comprehensive.

Project weaknesses:

- There are no major concerns. A challenge with these analyses is getting enough information, especially when some of it is proprietary, as in the FCE case. For REP, as an example, getting information on the stack life when it is operated as an electrolyzer/purifier would be helpful, but that may not be available or may not be something that FCE is willing to disclose.
- There is a lack of clarity around technology prioritization by DOE for SA evaluation. There should be better identification of "so what" implications/direct recommendations by SA on R&D needs.
- A focus is needed on technologies that are more commercially viable on a large scale.
- Use of confidential data is a weakness, and there is no uncertainty analysis in the REP work.
- More transparency in milestones and the number of experts polled per technology is desired.

Recommendations for additions/deletions to project scope:

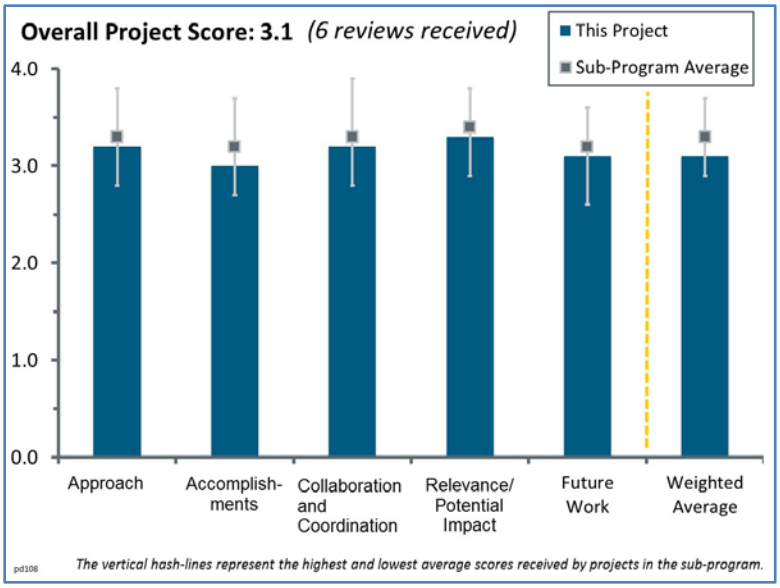
- A solar thermochemical hydrogen re-analysis would be preferred prior to a PEC analysis, based on the promising results from this year's AMR using cheap metal–oxide materials. An in-depth analysis of photovoltaics plus electrolyzer reactors and/or alkaline electrolysis alone could be conducted in place of another PEC TEA.
- The project should get uncertainty curves from FCE and have WireTough and FCE co-sponsor projects. Correlations between pentose conversion, broth concentration, and fermentation times should be investigated.
- The project team has reported that the methodology has been validated on fuel cells; another validation exercise should be considered. TRLs of technologies being evaluated should be clearly stated.

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 high-pressure hydrogen compression 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 **3.2** for its approach.

- The approach seems reasonable and innovative in terms of electric drive: improves isentropic efficiency above 95%, splits into Budget Period 1 for design and build, and conducts testing in Period 2. The testing was delayed and pushed into fiscal year (FY) 2017. Isentropic efficiency should be clearly related to current target efficiency in the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP).
- The technical approach to this project has been sound from the beginning. Project-management-wise, it seems to have tracked well, with the exception being the part delay/remake that occurred this funded year and that set the entire project back six months. This point should be of concern; given the tolerances involved on the high-pressure pistons and sealing rings, this is a very difficult part to make, and it is very easy to make it poorly and damage it during assembly, resulting in repeated setbacks.
- The combination of modeling and laboratory development work is very appropriate.
- Reducing capital cost and maintenance of hydrogen compressors is perfectly aligned with U.S. Department of Energy goals. It is not clear how the isentropic efficiency, however, aids in those goals; the team should draw easy-to-understand analogies to help show how they are tied together. The 2016 presentation presented a comparison with another compressor technology and how the linear motor reciprocating compressor (LMRC) would be greatly improved. It would be nice to read, in the 2017 report, about how this compressor will actually accomplish those improvements.
- The project is aiming for high isentropic efficiency, which apparently can also be achieved by a slow-speed mechanical compressor. It would be useful to clarify at what “speeds” the current design is able to achieve the high isentropic efficiency. Perhaps a fairer comparison with a mechanical compressor would be to state the respective isentropic efficiency at similar hydrogen flow rates and compression ratios. Since reducing the compressor cost relative to a conventional mechanical compressor is a key objective, it would be nice to compare a cost-breakdown pie chart of the proposed electromechanical compressor and a conventional reciprocating compressor (e.g., at the same hydrogen flow rate and compression ratio).
- Southwest Research Institute’s (SwRI’s) approach appears to be to follow a bulleted list. A description followed by a Gantt chart would be customary. The work itself appears to be close to schedule.

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.

- It seems from the material presented that the main accomplishments within this funding year are production of the core LMRC, dead load testing of the linear motor, and some initial gas testing of Stage 1 of the compressor. The predicted vs. actual production load of the solenoid coils is an impressive accomplishment, given that it seems to track well in a static test on a load cell. The ability to throttle the piston speed of the linear motor to minimize check valve losses during the compression stroke is a big potential benefit of the LMRC, and if it performs as the data shows it could, it is possible to realize the isentropic efficiencies noted. Other data and accomplishments from previous years (included in this year's work, backup slides) still do not clearly state how the seal design for the 1500-pounds-per-square-inch-gauge (psig) Stage 1 piston will translate to the larger challenge, which will be the Stage 3 14500 psig piston. The use of packing would not be possible at those pressures, and the leakage rate would be compounded. Progress of the project is good and methodical, and from the previous fiscal year, the LMRCs being built and ready for testing are good—and well on the way to the testing phase.
- The design appears to work. The target appears to be to generate a 3.6-times pressure raise with each stage. It is unclear whether the device meets the target efficiency levels. It would also be interesting to know the mean time between failure (MTBF) for the gaskets and seats. It would also be interesting to know if there is a preferred install origination—vertical versus horizontal. Site footprint size is always a station developer's concern.
- FY 2016 progress was shown up to some point last year owing to fabrication delays. The test stand is built, and the test article is also complete. Initial tests of the motor follow predicted performance (response of linear motor to current). The project is set for pressurization testing through the next budget period.
- This project experienced a delay and a hardware failure that put it back one year. It did not appear to result in increased project cost.
- One of the key objectives is to increase the isentropic efficiency. The team talked about reduced efficiency due to external mounting of the coils (for reduced risk). Slide 16 states "possible means to improve overall efficiency... have been evaluated." The overall presentation does not seem to lay out clearly the analysis of efficiency calculations/claims, so it is hard to know what the current status of efficiency value is. Another project objective is to reduce the capital cost—the team had to redesign the power controller; therefore, it seems to be a custom-built item (thus potentially expensive because it is not an off-the-shelf product). Further, in last year's presentation, the team realized that they needed an expensive casting material for one of the key parts. Thus, it is unclear whether the team will be able to meet the project cost objective—or at least, it would be useful for the team to lay out the cost breakdown that demonstrates that the custom controller, material costs, etc. will stay under control to meet the cost objective.
- Benchmarking against diaphragm and piston compressors (kilowatt-hours per kilogram compressed from 20 to 875 bar) would be helpful for understanding how this approach improves capital cost and/or reliability. Neither the 2016 nor 2017 slides mentioned how much energy it would take to compress a kilogram of hydrogen to fueling levels at 875 bar.

Question 3: Collaboration and coordination with other institutions

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

- The project has established a collaboration team that covers topical areas needed to improve chances of success. The team is very good.
- Collaboration appears to be appropriate. It might be helpful for SwRI to interface with International Organization for Standardization (ISO) Technical Committee (TC) 197 and the Canadian Standards Association (CSA) Group. The current product safety document is in draft form. It is expected that this device will at some time require a product listing. Here is an opportunity to address these documents prior to public review and publishing. The point of contact at ISO is Karen Quackenbush. The point of contact at the CSA Group is Sara Marxen.

- The use of ACI Services, Inc., for the detailed check valve and piston design is adequate, and the proof of good collaboration will be within this coming year's testing results and adjustments to the testing and mechanical design as leakage data and compressor performance results come in. Beyond this, there is no change from the previous year. It is suggested that the team collaborate with multiple vendors to ensure that six-month delays due to vendors scrapping parts do not occur in the future—having a sole-source part would be a big inhibitor for a commercialized product.
- The team works well with multiple collaborators and suppliers. Parts are built to specification. The project has had issues with one supplier and delays going on one year. It is too late to pivot, but the team should consider the lessons learned.
- The team appears to have in place the resources and collaborations necessary to complete the work.

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 key expectation of this project is to deliver a compressor at the stated cost target ($240,000) and with isentropic efficiency better than conventional reciprocating compressors. If such cost and efficiency targets can be met, this would be significant progress toward DOE goals.
- The second (now) most unreliable component in the dispensing of hydrogen is compression technologies. Improving reliability and reducing costs are critically important to the success of deploying hydrogen fueling technologies.
- This is relevant and may reduce compression costs and lower MTBFs for current compression systems.
- Improving the reliability, costs, and efficiency of hydrogen compression is well aligned with DOE goals and the needs of the industry. Expected performance should be compared with the project's preliminary data and commercially available data to show expected improvement in cost and energy intensity (kWh/kg).
- This project is relevant in the sense that a reduced-part-count compressor could reduce compressor capital expenses. The challenge for this team—and one for which they have provided targets without backup data—is that compressor performance projections are wildly optimistic without any high-pressure testing being completed. In this presentation, the team presented compressed power consumption value reductions of 9.2 kWh/kg (current) to 1.4 kWh/kg without having tested Stage 1 of the compressor. This is a broad assertion without technical backup. If this is truly possible, the team should present their calculations to back up this claim. Further to this point, the relevance of reduced maintenance cost is of course omnipresent; however, the presented claim says that the LMRC can reach values of 48 months without maintenance based on a 1/4 day duty cycle. When asked about exactly what this meant, the team was unable to quantify if this meant 100% run time for six hours or start–stop for some period of time. In compression equipment, this information is critical to making these assertions. After gathering one year of data, it will be good for the team to put running data to these claims to determine whether they can be backed up. This is a very important part to confirming this project's relevance.
- If efficiency can be improved through redesign, then the impact can be huge. This should be transferred to an industrial partner to do.

Question 5: Proposed future work

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

- The future work is appropriate. It is a pleasure to see that a focus on quantifying and improving first-law efficiencies has been included in this work.
- Testing is the balance of the work and must be completed. It would be good to see maintenance data to back up the operating expenses claim as well as detailed information and data sets on the kilowatt-hours-per-kilogram compressor claims made within this year's work.
- Future work comprises testing the compressor, which is highly encouraged.
- Proposed future work is appropriate.

- It seems that the current plan is to make a more efficient Stage 1, then to build Stage 2/3. The team should re-evaluate their cost and performance targets once the Stage 1 testing is complete so as to determine how close they are to their isentropic efficiency and cost targets.
- More care should be taken to improve the test facility and controls cabinet. Based on the pictures, it seems like the safety of the testing could be compromised by a lack of general housekeeping and good installation practices.

Project strengths:

- There is a possibility of a reduced-part-count compressor that achieves very high levels of efficiency and reliability. The build phase of the project—taking out the six-month delay due to a scrapped part—has gone well and is on track. The testing plan is well-thought-out. The project went through the national safety panel.
- The project claims that it will lower the capital cost of the compressor to $240,000. Presumably this cost is relative to a conventional mechanical hydrogen compressor for similar compression ratio and throughput. If this objective can be achieved, it would be a significant achievement.
- This project is flexible enough to respond to previous comments. It is good to see that the team has modified the overall project to address the efficiency point previously presented. The project has a good team and good execution and is resilient to unexpected delays.
- This project is focused on improving the cost and reliability of hydrogen compressors, which is aligned with DOE and industry goals. The LMRC approach may provide improved control of compression cycles.
- This is novel compression technology with high isentropic efficiency and potential cost and efficiency benefits.
- Novelty and potential to reduce cost are project strengths.

Project weaknesses:

- No real weaknesses are identified.
- Weaknesses include the following:
    - The project makes claims about maintenance and kilowatt-hours per kilogram, but there are no data, calculations, or evidence (even basic science) to substantiate these claims.
    - There is a lack of electrical data and surface temperature information on the linear motor. As this is pumping hydrogen, being in compliance with UL and other electrical codes and standards for explosion-proof equipment is a very big wild card for making the LMRC viable. The team must give great detail about having the coils of the linear motor (coils) rated or a possible path to UL or Nationally Recognized Testing Laboratory (NRTL) certification. It is strongly recommended that the team begin such a process. The testing matrix has a comment about the lack of seal performance, mentioning that there was a seal loss of 2% at 100 bar outlet pressure. This would be a very large red flag at this pressure and should be cause for concern about seal performance at lower pressures. It would be an exponential problem of seal losses on the higher-pressure stages.
    - While the team mentioned that 97% efficiency of the compression process was the target in the funding opportunity announcement (FOA), power put into the compression has a direct effect on overall compressor efficiency; thus, the team did not give an appropriate response to last year's comments.
- Benchmarking this compressor with commercial systems in terms of cost, energy efficiency, and reliability will help DOE understand whether this LMRC will ultimately achieve the goals of improving hydrogen compression systems. Such benchmarks are not just saying it will be better but are showing why/how the team believes these improvements will be realized—in other words, why the team believes that the seal life will last two to three times longer than the competing technology. The controls cabinet and test facility housekeeping and installation practices could be improved to reduce the chance of a mistake.
- Data on seal wear and the MTBF are weaknesses. The wear could have been independently tested. The MTBF is further down the pipeline. The four years pitched at the review is not realistic. There was no mention of field trials.
- The analysis of various efficiency numbers and cost breakdown are not clearly described. Assumptions of ceramic seal life (four years) requires experimental data to back up the life estimates.

- Weaknesses including lack of attention to system efficiency (kg/kwh); this seems to have been accounted for at the beginning of the project, possibly within the FOA. System efficiency should be looked at regardless since it is in the MYRDDP and is one of the issues with mechanical compression.

Recommendations for additions/deletions to project scope:

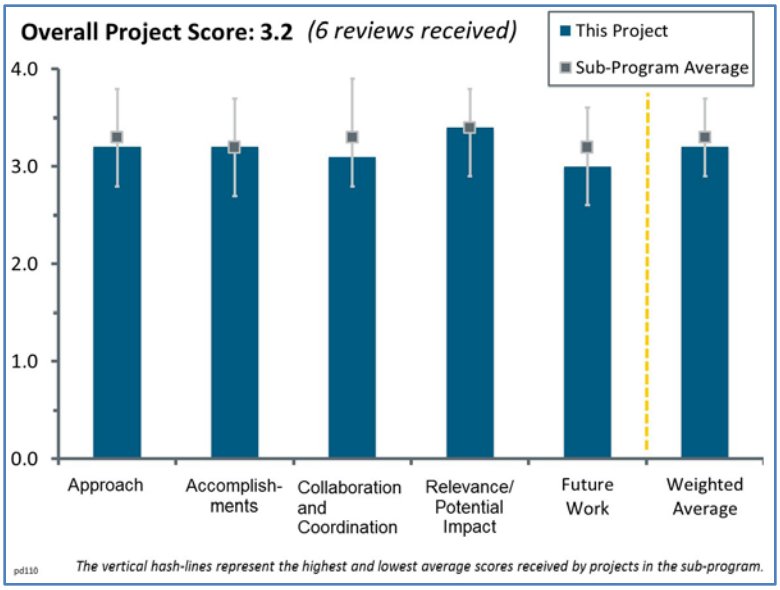
- The following additions are recommended:
    - Providing backup data and calculations for maintenance projections and kilowatt-hour-per-kilogram compressor targets
    - Ramping up the high-pressure stage seal testing, as this will be the larger project challenge
    - Finding a path to NRTL certification for explosive environments of the linear motor, without which there would be no path for the invention
- When determining the compressor's isentropic efficiency and costs, it would be useful to evaluate these aspects during both continuous and intermittent operation.
- The project should include field trials. The laboratory testing is fine. Field testing is the acid test.
- The team should consider an independent safety review of the test facility and controls cabinet to reduce the chances of an accident.
- Assessing or modeling system efficiency is recommended if project targets are met.

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 utilize innovative manufacturing technologies to develop a pressure vessel with a capacity of 765 liters that safely stores hydrogen at 875 bar while meeting the U.S. Department of Energy 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 (burst pressure to operating pressure), deliver hydrogen that meets SAE J2719 hydrogen purity requirements, and have a design consistent with relevant ASME codes.



Question 1: Approach to performing the work

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

- The approach to performing the work is very good and will undoubtedly result in the completion of the task and achievement of most if not all objectives.
- The approach involves wrapping commercially available Type I cylinders with ultra-high-steel wires for safe storage of hydrogen at 87.5 MPa. In addition, autofrettage is used to induce compressive residual stresses in the liner. Fatigue crack growth is investigated, and the results are compared with results obtained at Sandia National Laboratories with tension–compression fatigue crack specimens. The KD-10 protocol is used to assess the life of the liner under the design specifications listed on slide 9. In summary, the approach is sound and state-of-the-art. A question investigators are asked to address is why they are concerned with negative R ratios as far as life prediction is concerned. In addition, it is not clear what the contribution of the autofrettage to the fatigue life is.
- The project has performed extremely well in the short duration and is highly impactful. It is not clear how the free ends of the steel wire wrap are terminated, for example, if they are welded to the tank. If so, perhaps the weld location needs to be tested for fatigue life similar to the analysis for the rest of the steel tank. It is also not clear whether, after the hydrotest, there is any check to ensure there is no residual water left behind in the tank to avoid any potential corrosion.
- The approach employed by this project is appropriate.
- The presentation was lacking the basic testing and confirmation for the claim.

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.

- Excellent progress has been made in all areas of work execution and in addressing the challenges laid out at the beginning of the project. Cost targets have been met, and the durability target has been largely met, albeit with a 20% shortfall from target (24 vs. >30 years) in the most conservative estimation. The project will be declared a success.
- The project has made good progress toward DOE goals. The cost target of <$1,000 per kilogram of hydrogen storage was achieved, as the presenter stated that the cost was $600–$800 per kilogram. The

project has performed fatigue life calculations that show an estimated life of 24 years, which is getting close to DOE's 30-year-life goal.

- Slide 12 reports the projected life of the liner to be 24 years for pressures alternating from 89 to 90 MPa (this is a vague piece of information that the presentation did not clarify). This is a significant accomplishment, although it is not clear what the pressure variation with time is. In addition, it is not clear what the relevance of the result is to real-life application, as it was obtained for R=0.5. It is not clear what the relevance of this ratio is to the negative ratios the investigators discussed in their introductory slides. It is interesting that slide 11 reports stresses vs "distance," but no explanation is given as to what length "distance" denotes or from where "distance" is measured. In their presentation, project investigators did not elaborate what information they are gleaning from the results of this slide for life prediction methodology.
- This project is making very good progress and is on schedule.
- The project team missed their completion date by more than eight months, and the project schedule slipped from 2016 to 2017. While this can happen for a myriad of reasons, the team provided no feedback as to why this delay was required, nor did they present a clear picture of how the new date would be attained.

Question 3: Collaboration and coordination with other institutions

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

- This project has four partners, including WireTough Cylinders. This is a diverse and capable group with two national laboratories.
- Using external partners that are familiar with the code case was clearly demonstrated as an efficient and intelligent use of project resources.
- The collaborations established for this project are very good. The project might seriously consider contacting HyTrec in Fukuoka, Japan. This facility has the capability of cycle-testing cylinders this long.
- This type of work was successfully applied by the National Institute of Standards and Technology in 1970–1980, and the company shall use the results.
- The collaborations reported on slide 14 are deemed appropriate.
- The budget indicates recipient share of only $500,000 of the total project budget of ~$2.5 million. It was not clear how the remaining $2.0 million was distributed among the team so as to judge their respective contributions vs. cost to the 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 **3.4** for its relevance/potential impact.

- This project should produce inexpensive high-pressure cylinders for hydrogen transport. There is one point of curiosity, however: the lower bound on the pressure cycle is ~70 MPa, which presupposes that these cylinders will be used to top off local storage. The first thing that one will do is to empty these cylinders below 70 MPa. The cylinder should still be in compression (except the dome region). It would be good to know what the crack behavior will be at a high cycle rate (~10 cycles/day) when cycled to a lower lower bound pressure—for example, 30 MPa. This needs to be addressed.
- The project aligns well with the objectives of the Hydrogen and Fuel Cells Program in the area of hydrogen storage.
- The project meets the DOE targets with regard to both safety and cost.
- The overwhelming weight of this type of pressure vessel is impractical. Much better alternatives, such as carbon-overwrapped pressure vessels, can be used that are much lighter and not so much more expensive.

Question 5: Proposed future work

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

- The remaining scope is clear and appropriate, but no timeline was tied to the remaining tasks to determine whether the scope was achievable. With project slip already being predicted, defining the timeline of the future work is critical to a successful evaluation.
- It would be good to see cycle data to a lower pressure than the ~70 MPa design lower pressure. This cylinder design will probably be fine, but the domes are not wrapped and are not under compression, so it begs the question as to whether this region will be more susceptible to higher-frequency lower lower pressure bounds.
- The rationale for testing wires (external to the tank) in hydrogen is not clear. Even if one argues the presence of "some hydrogen" in the local environment, it is unlikely that this hydrogen will be at pressures influencing fatigue life. The team should consider evaluating the design space for further cost reduction via using alternate steel cylinders, autofrettage pressures, wire strength, etc. Such effort will move the project toward DOE's long-term cost targets for hydrogen storage containers.
- On slide 13, it is proposed that a protocol be developed on ensuring that adequate compressive stresses be developed during autofrettage. There is not enough information to assess how the project addresses the effect of the compressive stresses on the life of the liner. This is not a well-analyzed part of the project, and the investigators' presentation did not clarify it.
- The future work described will close the project out with some unanswered questions. However, given the time and resources remaining through the end of the project, not much more may be achieved in the time left.

Project strengths:

- Project strengths include the collaborative team that performed the work, the technology brought to bear in solving the challenge being addressed at a fairly low cost, and the fact that the goals and milestones reached in the work are largely consistent with what was promised at the beginning of the project.
- This project is well executed, making timely progress from last year. It looks like this will be successful in producing a low-cost, high-pressure system for hydrogen transport.
- A strength is the data-analysis-driven solution to the cost challenge of fabricating long-term hydrogen storage containers.
- Dr. Saxena's scientific stature is a project strength.
- Cost targets were clearly demonstrated to be achievable.

Project weaknesses:

- No real weaknesses are identified.
- There is a lack of prototypical testing. A better understanding of component behavior is usually achieved with well-thought-out prototypical testing. Tests are usually complicated and subject to misinterpretation if not carefully executed, but they yield very powerful results if they are carefully executed. They are also very expensive to carry out. Perhaps sponsors could consider this for future investigations. Workers may well find that predicted lifetime may change, maybe even improve, with prototypical testing. The technology highlighted depends solely on the ability to maintain the compressive stresses imposed by the wire wrap on the storage tank. If wires relax or fail, the benefit of the technology is lost (until re-wrapping is carried out). Very little testing seems to have been focused on the durability of the wire wraps when exposed to various types of service environments the tanks are likely to be in, especially as they are expected to be in service for decades. The issues of stress relaxation over long periods of time are not addressed in this presentation either. Other presentations may have addressed these issues, but this one certainly did not.
- It is not clear by how much the autofrettage affects fatigue crack growth. In fact, the elaboration on negative R ratios seems to be out of place because fatigue crack growth under hydrogen pressure takes place under positive hoop stresses and hence positive R. There is no information on what pressure profile the investigators used in their life analysis and whether the profile is relevant to real-life applications. An

overall comment is that the presentation at the 2017 Annual Merit Review lacked clarity with regard to essential ingredients of the project.

- The presentation has numerous typographical errors. In the da/din vs. delta K graph on page 7, the reasons for different trends shown by the green line, black line, and dotted black line are not clear, i.e., whether the differences are due to differences in pressure, R value, frequency, etc.
- While the project team was working to evaluate the lifecycle of the cylinders (they believed they were doing well), they still are falling short of the target 30-year design life.

Recommendations for additions/deletions to project scope:

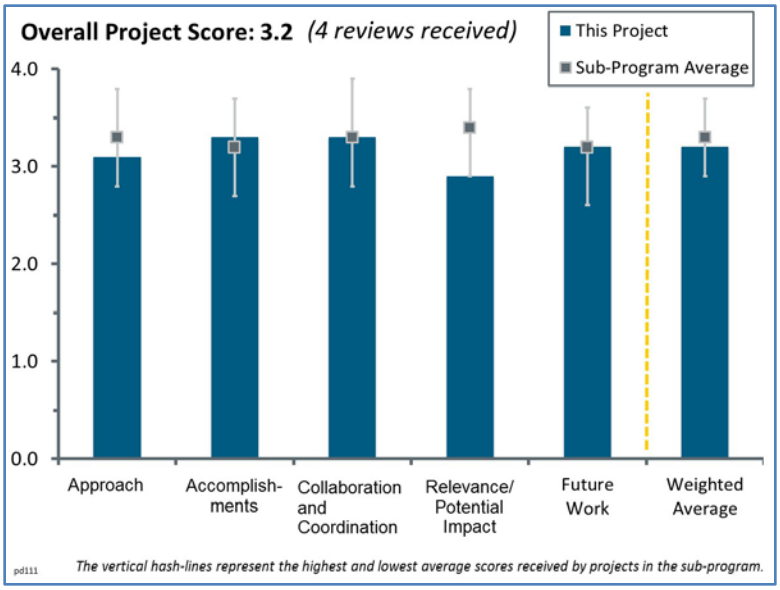
- The wire testing task seems unnecessary. Perhaps it is possible to determine whether the design space—combinations of cylinder material, autofrettage pressure, and wire strength—can be played with to further bring down the cost of hydrogen storage.
- A test facility that can cycle test these tanks under anticipated real-life duty cycles should be sought; the project should look into HyTrec in Fukuoka, Japan.
- No additions are recommended. The project is in the final stages, and any additions to project scope based on project weaknesses will require additional funds and time to complete. The project will provide useful data as planned to completion.
- Presentation slides should be numbered. The autofrettage protocol must be presented in relation to real-life pressure variations with time. The project has not demonstrated why autofrettage is needed.

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

Kenneth Rappe; Pacific Northwest National Laboratory

Brief Summary of Project:

Bio-oil reforming technology advancements are being pursued in this project. Pacific Northwest National Laboratory (PNNL) is working to (1) reduce the capital cost of hydrogen production from bio-oil through minimized unit operations, smaller pressure swing adsorption, and process simplification; (2) increase energy conversion efficiency through in situ CO_2 capture and in situ heat exchange between reaction and regeneration; and (3) increase operating flexibility and durability through reduced operations and maintenance requirements.



Question 1: Approach to performing the work

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

- The rapid swing reforming/combustion process demonstrated in this project appears to be a novel, potentially low-cost approach to producing hydrogen from bio-oil. The identified barriers are (1) plant capital costs and efficiency (unit scale of economy) and (2) operations and maintenance (O&M). To reduce capital costs, the project team minimized unit operations and simplified the process. To increase energy conversion efficiency, the team used in situ heat exchange between reaction and regeneration to minimize heat loss, and in situ CO_2 capture to push thermodynamics of reforming to higher conversion. To reduce O&M costs, the project team attempted to improve operating flexibility and durability by minimizing coking and catalyst deactivation. There are likely to be several more points of system vulnerability that could have an impact on efficiency and durability over time. The impacts of pH, temperature, and pressure cycling on vessels, pumps, valves, and sensors were not mentioned. Also, control strategies should be discussed.
- The project team has successfully addressed and made improvements to the barrier targeting hydrogen production at less than $2/gallon of gas equivalent (gge). The work shows good progress in improving the production rate of hydrogen through improved sorbent formulation and loading as well as catalyst formulation and loading. Further, the work demonstrates limited degradation of the primary components over multiple cycles, though any future work should include extended cycles in which the degradation is measured over hundreds or thousands of cycles. While optimization will also lower system costs, it does not specifically lower capital cost of the equipment, which was a targeted barrier of the work. Perhaps work was done to lower these costs, but no values were presented for estimates on the capital equipment, the catalysts, or the monolith used in the project.
- The system approach takes advantage of two sets of reactions cyclically to synergistically improve hydrogen production from a biomass-derived liquid (bio-oil), including taking advantage of coking, which is often an obstacle to performance. This is aimed at addressing multiple barriers, including improving conversion efficiently, reducing capital costs, and improving operational requirements. The approach combines development of system components such as catalysts with an integrated pilot demonstration. The demonstration step is important to evaluating the integrated impact of the developments.
- The investigators have done a reasonable job but have tested very few catalysts for this reaction. In a commercial laboratory, over a hundred different catalysts might be tested in a year. Material balances seem

to have been ignored. Analysis of coke yields and compositions, accurate analysis of light gases produced, etc. would allow the investigators to optimize the process to perhaps convert methane and other light hydrocarbons formed, or at least to get a good estimate of theoretical yield of hydrogen. The incorporation of a CO_2 adsorbent was a good addition, probably shifting water–gas shift equilibrium to give higher yields of hydrogen.

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.

- This is good work. The team reports that the addition of K and La to the reforming catalyst reduced selectivity to methane and coke formation and increased reform rates by increased metal–oxide dispersion. The sorbent was successfully optimized for scaled synthesis and stability by optimizing eutectic weight percent in sorbent, and by a method of synthesis that includes direct impregnation of Li, Na, and K carbonate eutectics on dolomite; synthesis time was reduced by 75%. The team also reports that the source of deactivation in the integrated system was identified and stable operation has been achieved. Ultimately, stable bio-oil reforming with integrated CO_2 capture was demonstrated. CO_2 capture was shown to significantly improve hydrogen yield (up to 5x). The team should consider manufacturing in further developing its design to gain efficiencies of mass production. Refined design could minimize the cost and amounts of materials used in both the primary reactors and the balance of plant, as well as thermal efficiency, durability, and long-term costs incurred in replacing components.
- The project has shown significant improvement from previous work. The work innovatively deals with the production of CO_2 and creatively uses the coke created during hydrogen production for heat generation. The work utilizes bio-oil in the form of pyrolysis oil, which is useful and in keeping with DOE goals. The group showed clear performance in hydrogen production capability and method for improving catalyst and sorbent performance.
- There has been progress in the project tasks, including accomplishments in improving the catalysts and demonstrating an integrated pilot system. The fiscal year 2016 milestone of demonstrating extended operation of the system integrated with CO_2 capture has been significantly delayed. The project identified a production target of 10% kilograms of H_2/kilogram of bio-oil. The highest status presented was around 6%, though it was noted that it was expected to increase if further optimization was done.
- The investigators have shown conversion to hydrogen, but no good heat and material balances were presented. Limited catalyst optimization has been attempted. Also, since conversion is less than 100%, the reactor would need to operate in recycle mode. The unreacted bio-oil is probably less reactive, so problems in converting this material would need to be addressed.

Question 3: Collaboration and coordination with other institutions

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

- The project has multiple partners, leveraging expertise in catalysts (Washington State University [WSU]), monoliths (Cormetech), and testing apparatus (Dason). In addition to extending the types of experiments that could be performed, collaboration was demonstrated by the troubleshooting described to identify and address the root causes of problems with the testing system, such as thermal management and opening the reactor for maintenance.
- Collaboration and coordination appear to be quite good. Team problem-solving in selecting, testing, and optimizing the catalyst is evidence of excellent coordination and collaboration. It is difficult to gauge coordination and collaboration in much detail based on the simple organization chart.
- Collaboration efforts are obvious. There appear to be some small gaps in understanding of what various groups are doing and have done, specifically regarding work done at WSU for catalyst formation. Some improved communication is advised.
- Collaborations appear to be limited to one university, a monolith supplier, and a reactor supplier. There was no attempt to work with catalyst manufacturers, bio-oil suppliers, or companies engaged in upgrading bio-oil. All of these entities could probably have made significant contributions to the success of the 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 **2.9** for its relevance/potential impact.

- The project directly relates to the DOE Hydrogen Production and Delivery sub-program goals and even utilizes use of a renewable feedstock in the form of bio-oil.
- The project identifies barriers that it is addressing, but the objectives listed (reduce capital costs, increase efficiency, increase operation flexibility and durability) do not have clear targets for progress identified. These may be built into the cost analysis, but without identifying those, it is difficult to evaluate the impact of each on the overall listed target of producing hydrogen at <$2/gge. The investigators estimate that if the project meets its technical targets, the cost for hydrogen production would be reduced to just under $4/gge, with potential for further reductions. However, the estimate with further optimization is above the $2/gge identified as the target, and there is no indication of how or whether further reductions could be made, or how much of the reduction is due to using a future pyrolysis oil cost of $6 per gigajoule (compared to a $14.1-per-gigajoule bio-oil cost in the other cases).
- Among the options for using bio-oil, upgrading for mixing with hydrocarbon fuels for internal combustion engines is a much more likely option. Given the low yields of hydrogen, upgrading to hydrocarbons is a more likely option. This technology might be utilized in small-scale remote operations to provide hydrogen for bio-oil upgrading/stabilization.
- Assuming a robust hydrogen market develops, the potential significance of bio-oil as a feedstock and this technology is unclear. Likely bio-feedstock will come from municipal solid waste, biosolids, and waste wood. It is not clear how this technology will address the energy conversion needs of those markets or what portion of the hydrogen market this technology can meet.

Question 5: Proposed future work

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

- The presenter stated that in the time since the slides were submitted, the decision was made to end the project for funding reasons, and therefore the future work is to prepare information for report-out. As the project work is ending, this is important work to ensure that the results and lessons learned can be shared.
- It is understood that the remaining time for the project will be spent on data reporting.
- The proposed final tasks are clear and appropriate.

Project strengths:

- Project strengths include the novel approach and simple design execution, as well as team problem-solving in selecting, testing, and optimizing the catalyst. The technology does not include exotic materials or extreme operating conditions, and there is a line of sight to commercial viability.
- Project strengths include use of bio-oil as a feedstock, use of CO_2 sorbent to purify hydrogen during the process, high-level collaborative partners, and a significant improvement in optimization.
- The partners were able to work together to identify real-world process issues such as opening reactor temperature control issues. In addition to allowing for the demonstrations to continue, this also will be useful information for developing designs and plans for scaling up.
- Use of CO_2 sorbent in the reactor is a strength.

Project weaknesses:

- There are not enough cost data presented to confirm the impact of optimization on capital cost. The poster presentation would be better as a poster, not a series of PowerPoint slides. The presenter seemed to have trouble presenting work in a fluid, coherent manner.

- Weaknesses include the lack of materials balance and light gas analysis of off-gas. There has been no consideration of recycle issues. The project failed to achieve targeted hydrogen yields.
- The impact of the technical accomplishments on the modeled cost is not clear. The numbers have been updated from last year, but the source of the changes is not described.
- Assuming that this technology becomes commercial, its potential impact on the supply of renewable hydrogen is unclear.

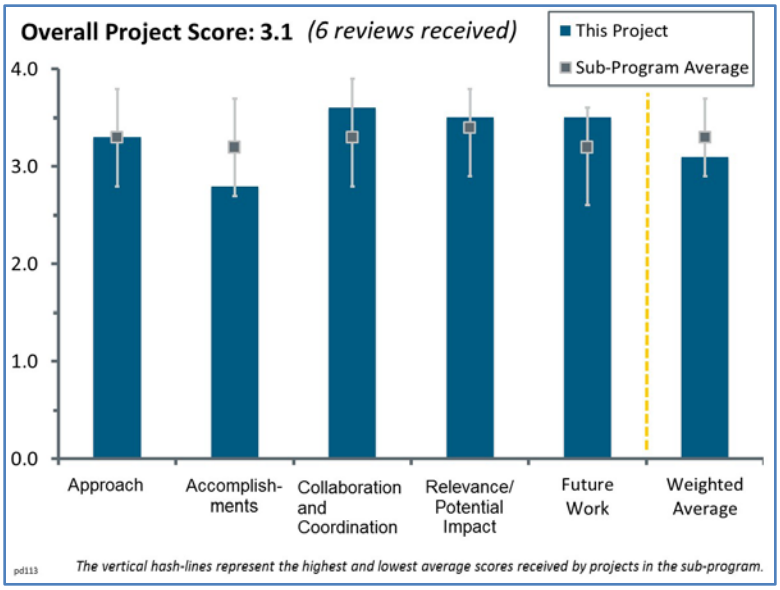
Recommendations for additions/deletions to project scope:

- Manufacturing strategies and design could be pursued to help minimize system costs. Future work could also include the development of advanced pathways for limiting CO_2 emissions. One common example might be methanol co-production. Other approaches might involve using some of the solid carbon produced by this process to make high-value materials such as carbon-based structural fiber and coatings.
- It would be useful to expand on the experimental work, specifically increasing the tests for degradation of the catalyst. Some information on capital cost and the impact of optimization on performance cost would assist in directly addressing the barriers for capital cost.

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 three liters of hydrogen by the end of the project. Fiscal year 2016–2017 objectives are to (1) discover and characterize suitable materials for two-step, non-volatile metal oxide thermochemical water-splitting cycles, (2) construct and demonstrate a particle receiver–reactor capable of continuous operation at 3 kW thermal input, and (3) conduct full technoeconomic, sensitivity, and trade-off analyses of a large-scale hydrogen production facility using a plant-specific predictor model coupled to the Hydrogen Analysis (H2A) model.

Question 1: Approach to performing the work

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

- From the onset, the team took an aggressive approach to innovating not only a complex reactor design but also novel materials. While making significant progress, choices of how to prioritize and arrange research efforts between these objectives have resulted in no clear success for either. Research is like that sometimes—big bets do not always pan out. However, the strategy of parallel materials, reactor, and analysis thrusts is commendable. Not to be endorsed is the idea of waiting until the end of a three-year project to publish all the results.
- SNL set a very ambitious and focused project scope and schedule to meet the U.S. Department of Energy (DOE) objectives, to develop materials discovery approaches for improved reaction materials for water splitting, and to design and construct a particle receiver–reactor capable of continuous operation. The approach made the most of a relatively modest amount of funding over less than three years, and the approach was high-risk and innovative. While not all barriers were anticipated or overcome, significant gains in knowledge were achieved that will allow future efforts to be even more focused and successful. This project was well integrated with the interests of the DOE Materials Genome Initiative.
- Solar thermochemical hydrogen production (STCH) is a unique process for solar water splitting that is different from photoelectrochemical (which is very similar to photovoltaics [PV] + electrolysis). The uniqueness of the reactor means that the scaling and cost-learning curves from historical technologies could be very different, and thus it has the chance to be a game-changing technology in the field of solar water splitting. However, to further support this, it should be clarified how the technoeconomics of STCH compare to solar thermal PV + electrolysis. This project is well designed and nicely complements the STCH efforts at University of Colorado Boulder.
- The investigators have combined theory, high-volume material screening, and testing to advance the science. The "small"-scale reactor work is very useful and necessary for this type of work to progress.
- With an experimental campaign that surveyed up to 200 perovskites, it seems that, using the principled approach the principal investigator (PI) describes (these experiments were not Edisonian in nature), one

ought to have validated the scientific principles the researchers set out to prove. Given the number of samples, this has the opportunity to occur to such an extent that we might argue the materials space has been "covered," in the mathematically complete sense of the word, assuming a properly executed statistical design of experiments occurred. Unfortunately, it seems we do not yet have this confidence in the materials space. It is not clear that the part of the project validating the principles set forth in the materials design was properly structured. Interesting discoveries are still happening (a new dual perovskite—right at the end of the project). While, as argued by the PI, we might not know enough design rules at the start of a project, we may certainly design an experiment to thoughtfully and efficiently cover a space, especially if the investigators bring their extensive understanding of how thermodynamic stability brackets interesting materials combinations as "possibility." Tradeoffs always happen; it is not clear that the project has been managed with such tradeoffs in mind.

- Given STCH's low technology readiness level (TRL) and long-shot prospect, the project's scope and breadth is way too broad for the project duration and funding level. The team focus and capability appear to be on reactor design and construction (which should be the primary effort) rather than on materials discovery.

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 demonstration of the operating system was very good. It is to be hoped that the investigators can get it fixed so they can complete their run. The discovery that other design considerations need to be included is important, and the team should document it well for other developers. Unless the steam requirements can be lowered, this technology will be expensive. The project should see what the H2A model costs are when the large amounts of steam are included in the system. The high-temperature "waste heat" is of extremely high value and should not be wasted. It is surprising that the waste heat cannot be used on the material and steam preheating. If it is not heated in material preheating, then it should definitely be used for other value-added product production such as electricity generation. The investigators have an impressive number of papers being written (13). In the future, they should try to publish more often to avoid having so many at one time. They identified only a handful of materials that could be used and only one or two that were better than their baseline, after screening hundreds. They need to improve the screening process.
- The primary demonstrable accomplishment was the design/build of a (mostly) operational STCH reactor. The team nearly accomplished the hydrogen production goal. It is unclear whether the failure exposed a fatal design flaw or if the team will repair and repeat the attempt. The ultimate payoff of this substantial project will be judged in the impact of the many planned publications, including use of the material design rules claimed to have been developed, and future research enabled by a functioning STCH reactor. The project also highlighted the potential for hydrogen–electricity co-generation as a significant potential advantage. This should be explored further by DOE.
- The work on cerium oxide (CeO_2) and the demonstration reactor is superb and exciting. The results and predictive capabilities from the entropy engineering have been less successful, but that is of lesser concern. Interestingly, the concept of co-generation of hydrogen and electricity is mentioned, but the global minimum for ultimate cost based on the fraction of heat going to STCH vs. concentrated solar power (CSP) electricity (followed by hydrogen via electrolysis, for example) was not mentioned. This is an important comparison that should be performed to identify the benefits of co-generation or determine whether solar thermal PV + electrolysis is projected to be less expensive.
- In collaboration with partners, SNL was able to design, construct, and test a solar simulator, reaction chambers, and the Cascading Pressure Receiver–Reactor (CPR2). Although the project demonstrated hydrogen production with this reactor, it was not able to demonstrate eight hours of continuous operation and production. The reason was not adequately explained, nor was a possible fix to the system to allow for longer-term operation discussed. It would seem that this would have to be done if the CPR2 is to be a valuable resource for future research by SNL and others and contribute to technology transfer efforts. Materials discovery work to date has not identified the optimum water-splitting material or materials, but good progress has been made in identifying design rules and protocols. This will allow future materials work to continue to make progress in this area.

- The project goals do not appear to be well defined or were too ambitious from the outset. For example, the stated goal to "develop a viable integrated solar-driven high-temperature thermochemical water-splitting process" can be subjective. It is difficult to measure the success of the stated milestones, such as "Formulate and synthesize [reduction–oxidation reaction] active oxides from $LaAlO_3$," without mention of size, properties, or performance of the synthesized material.
- The project has not achieved three liters of hydrogen in an eight-hour period. The thermally induced mechanical failure in the system is a harbinger of things to come.

Question 3: Collaboration and coordination with other institutions

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

- This project has demonstrated excellent collaboration, cooperation, and coordination between multiple partners, including several universities and a European laboratory. Participation in the newly formed HydroGEN Advanced Water Splitting Materials Consortium will provide another opportunity for SNL to shape the future of this technology.
- This project has a large number of national and international collaborators that seem dedicated to the work and the project. This project nicely complements the STCH efforts at University of Colorado Boulder.
- Most seem to have made meaningful contributions, and SNL actively engaged its partners.
- This is a very impressive national and international team. The team inputs to the project need to be better defined.
- The project appears to have key academic and international (German Aerospace Center–DLR) collaboration.
- This was done well.
- The list of collaborators is long—possibly too long, possibly resulting in high coordination/management overhead.

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 reactor system designed and built under this project should provide a valuable resource to future work by SNL or others, especially if it can be fixed to run continuously for eight hours or more. This project group has been an early leader in materials discovery research and development of metal oxides and perovskites for water-splitting applications, and their experience and results to date will serve as a springboard for further studies in this area.
- STCH needs proof-of-principle projects like this one to jump-start serious evaluation of the concept. A more conventional technoeconomic analysis (TEA) is encouraged, consistent with those done for other hydrogen production pathways. The detailed plant model developed for the project offers an excellent starting point.
- This group essentially hit their final project metric of three standard liters of hydrogen in eight hours before a catastrophic materials failure. DOE deemed this an appropriate target at this stage of development of the project and the field, and therefore this result should be commended.
- The project goals and scope are in alignment with the DOE Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan.
- STCH production is a long-range technology. It seems much of the development is low-TRL, perhaps even TRL 1. DOE Basic Energy Sciences funding should be sought for this area.
- This project cited CSP with thermal storage for heat and electricity as enabling. This has not been well appended, and claims need further vetting.

Question 5: Proposed future work

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

- The project will be ending this year, and it is not clear at this time what resources will be available to continue the investigations under HydroGEN. The investigators have identified remaining challenges and possible future work. The suggestion to combine hydrogen production with cogeneration of electricity into a hybrid system appears to be a reasonable one, and it is very important that the project team continue to make the economic argument for this route versus competing technologies, perhaps through an updated case study with DOE. Demonstration of the value proposition to gas and electricity providers is a good idea, as these may be potential partners in a higher-TRL stage of development for this technology. TEA studies of the STCH technologies would benefit from up-to-date estimates of the cost of heliostats. Perhaps in the future, the investigators or the Fuel Cell Technologies Office could obtain periodic updates on this from the Solar Energy Technologies Office. Future work in development and scale-up of the CPR2 should address design and components, which were outside the scope of this project. For example, perhaps another heat exchanger will be needed, as was suggested in the 2016 annual report, or perhaps further pressure cascade to higher vacuum than ~ 10 Pa will be required.
- In light of the fact that the project is ending in September of this year, the two stated tasks for future plans seem reasonable. Making the CPR2 available for other projects through HydroGEN to test new materials makes sense. However, considering the limited remaining resources and time, it is not clear how the project team plans to accomplish the preparation, submission, and responses to peer reviews of all 13 papers in such a short time.
- The remaining barriers for additional study are reported as being addressed by the Energy Materials Network's (EMN's) HydroGEN or are motivated by making the demonstration reactor a user-facility-based instrument for testing materials for others in the community. This seems like a very logical progression for a national laboratory. The PI mentioned a new double-perovskite materials discovery that sounds like it should be followed up on by someone in the EMN.
- The project should come to its natural, time-based end. In the PI's slides, the project's period of performance was always qualified with an asterisk. The asterisk read, "End contingent upon meeting [funding opportunity announcement] metrics," suggesting the project never ends if it keeps missing its success metrics. Sometimes it is best to wrap up, regroup and reorganize, and come again with a new, thoughtful proposal.
- This project is ending.

Project strengths:

- The reactor demonstration was fantastic. The cost analysis in the backup slides is promising, given the current state of the art (although the lifetime of materials in practice is very low), and the projected costs do not seem to constitute unreasonable advances.
- The primary strengths of the project were the excellent project team, innovation in research and design, leadership in investigations into new materials of reaction, and coordination between materials discovery work and reactor design.
- This is an ambitious, well-funded effort to demonstrate STCH—a good DOE investment with a learn-by-doing objective. The project has a strong, motivated team.
- The project has a great PI, a great team, and great resources for executing, as well as a good experimental campaign. There is a fair level of clarity in scientific conclusions.
- The project has a very strong team and is very well supported by DOE. Facilities and tools are very good.
- High-temperature reactor design and fabrication capability are project strengths.

Project weaknesses:

- It is not clear how reactor design learnings will be socialized within the STCH community. It is not clear whether this is a one-off or whether there are ways other/future designs can benefit. A large amount of "high-quality waste heat" is usually a sign that you need to go back to the drawing board. Perhaps integration with electricity generation, or integration with high-temperature electrolysis, is the answer; or

perhaps it is a serious flaw of the entire STCH concept, making the technology useful only when solar towers are commoditized. It is not clear from project analysis where on that spectrum we are sitting. We have not addressed water—quantity and quality, or how much pre-treatment is needed. These topics should be in future TEAs. The presentation, which should be to assist the broader audience, made excessive use of undefined acronyms.

- The project inadequately conveys when materials formulation is (ever) complete. Continued materials exploration has diminishing returns, barring a "Eureka" discovery moment, the pursuit of which is less investment and more like a gamble. There are concerns about mechanical robust operation of the system and how the risk register for these is established, updated, and conveyed to DOE.
- The project identified only a handful of materials that could be used and only one or two that were better than the project baseline, after screening hundreds. The project team needs to improve the screening process.
- The scope of high-temperature materials discovery should not have been part of this project. Given the early stage of STCH development, materials discovery and characterization should have been given separate and full attention by other researchers.
- The inability to achieve funding opportunity announcement goals for this project was the primary weakness. It is to be hoped that this will be addressed within the remaining months of the project.
- The entropy engineering efforts were admittedly not that informative.

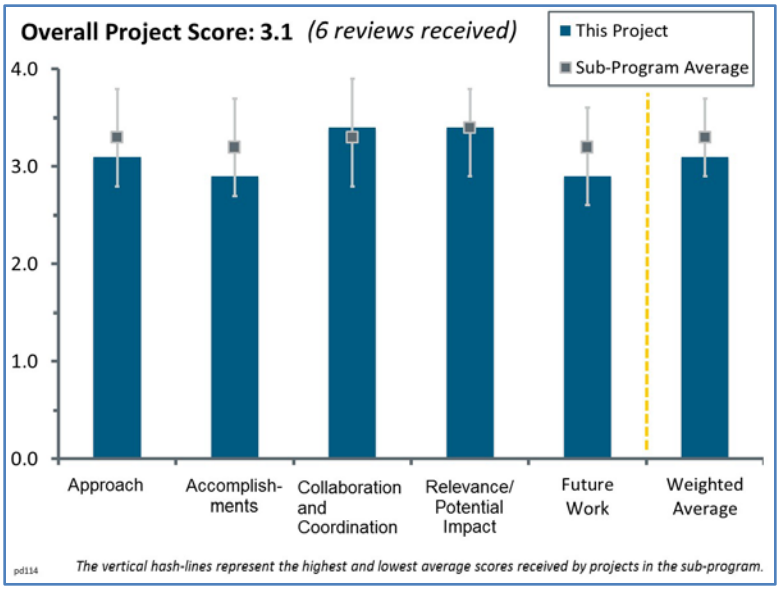
Recommendations for additions/deletions to project scope:

- As the project concludes, the investigators should:
    - Compare results of their "high-fidelity" process model to H2A's. This is the community standard and a valuable reference point.
    - Provide context for the HydroGEN effort on STCH materials. It would be good to know how and by how much materials would need to improve to move the needle on STCH capital expenditure (capex), operating expenses, and the cost of hydrogen. Perhaps other levers are more important. The water-splitting community should be helped in applying limited resources to areas with the highest impact potential.
    - The idea that each STCH plant requires a multi-billion-dollar capex investment should be concerning to the community. A roadmap should be laid out—there just may be many other lower-risk technologies to try first.
- The project has too broad a scope that demands a whole range of skill sets and expertise to expect coherent and meaningful achievements, given the resources and timeframe. High-temperature materials discovery, characterization, and testing; reactor design, construction and operation; solar field design and optimization; and TEA can be too much for one project team to do well. Given its low TRL, it may be more effective to break the tasks into separate projects, perhaps under different sub-programs. Moreover, the TEA results, assumptions, and uncertainties may need to be strengthened. This is a low-TRL technology that will demand long-term and sustained commitment.
- The project should be allowed to come to its time-based conclusion and then allowed to re-submit an application for merit review.
- The project is ending this fiscal year.

Project #PD-114: Flowing Particle Bed Solarthermal Reduction–Oxidation Process to Split Water

Al Weimer; University of Colorado Boulder

Brief Summary of Project:



The overall objective of this project is to design and test the individual components of a novel flowing particle solar thermal water-splitting system and show a pathway to a system capable of producing 50,000 kg of hydrogen per day at a cost of less than $2/kg. 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-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 and detailed process models.

Question 1: Approach to performing the work

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

- It seems this project has increased its focus on reactor design, materials, and critical components, while leaving active materials developments to a sister U.S. Department of Energy (DOE)/National Science Foundation (NSF) project. If so, this is a wise move and entirely appropriate. The team has demonstrated creative thinking in terms of reactor design options, such as integrating recuperated heat to extend operational time and exploring both fluidized bed (near-isothermal) and moving bed (temperature-swing) processes. However, the project resourcing and timeline were not built to carry along two radically different designs—there should have been alignment with DOE earlier on which one to execute. (The fluidized bed approach would have been the better choice.) The project would benefit from engaging a seasoned expert in high-temperature fluidized beds. The presenter gave a run-down of key differences between this project team's implementation of the solar thermochemical hydrogen (STCH) concept and that of Sandia National Laboratories (SNL), which was appreciated.
- STCH is a unique process for solar water splitting that is different from photoelectrochemical (which is very similar to photovoltaics [PV] + electrolysis). The uniqueness of the reactor means that the scaling and cost-learning curves from historical technologies could be very different, and thus it has the chance to be a game-changing technology in the field of solar water splitting. However, to further support this, it should be clarified how the technoeconomics of STCH compare to solar thermal PV + electrolysis. This project is well designed and nicely complements the STCH efforts at SNL.
- The project approach has been successful in addressing challenges and barriers and in meeting project goals and objectives. The project is on track to meet the DOE requirement for eight hours of continuous on-sun operation and production of greater than three liters of hydrogen.
- In general, the approach to solving any one problem is quite sound; however, the cost-effectiveness, longevity, and certainty with which each proposed/identified solution may deliver a system-wide technoeconomic advantage needs to be critically evaluated. For example, the principal investigator (PI) seems to propose 900ºC "waste" heat to run a Rankine cycle, and it is not clear why this would be

considered. The exergetic penalty would be great. Not many cycles, if any, run at 900ºC turbine input temperatures.

- The project's approach requires advances in multiple areas: identification and synthesis of efficient and robust high-temperature water-splitting catalysts, containment materials, reactor design, on-sun testing, and technoeconomic analysis (TEA) of the integrated system. Obviously, project success will depend on having multiple assumptions and diverse expertise.
- The focus has been primarily on developing the perovskite, which the project team has been working on for many years, including prior to this work. The reactor work is interesting but unlikely to work. The project is developing a coating technology.

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

This project was rated **2.9** for its accomplishments and progress.

- This project looks to focus on individual components and not a large-scale reactor, which is probably a good plan for a university lead. The collaborators at the National Renewable Energy Laboratory (NREL) High-Flux Solar Furnace evaluated the particle performance, and performance was well above the projected outcome—but for a shorter period of time than eight hours. The TEA seems very promising, notably because the sensitivity analysis showed very little deviation from $2 per kilogram of hydrogen with seemingly large parameter ranges; the reasons for that were unclear. A <$0.70 variation in hydrogen cost with >2X change in heliostat cost is unexpected. Also, the stability results are promising but not earth-shattering, but irrespective, it is not clear that the cost of atomic layer deposition (ALD) was included in the TEA. The response to reviewer comments from last year does not quantify ALD cost but simply states it is not a problem. Separation results are exciting, too, as is demonstration using hercynite and three standard liters of hydrogen in eight hours. Perhaps the solar/electric furnace (Harper International Corporation) is relevant to this project on solar hydrogen. It seems the electronic component may be just an option. If so, a global minimum for ultimate cost based on the fraction of heat going to STCH vs. concentrated solar power (CSP) electricity (followed by hydrogen via electrolysis, for example) should be analyzed to identify the benefits of co-generation or whether solar thermal PV + electrolyzers is projected to be less expensive.
- There has been excellent progress on meeting the hydrogen production target. However, it is unclear how the demonstration experiment was able to succeed if cycled between 1350ºC and 1500ºC when oxidation occurs at 1000ºC. Also, it is not clear which scheme (fluidized bed or moving bed) was utilized for this demonstration. The project demonstrated sufficient efficiency of the ion transport membrane (ITM) solid electrolyte oxygen separation (SEOS) component. However, there was no discussion of the unavoidable trade-off between efficiency and flux, and the impact that might have on the process scheme, nor considerations of the potential for scaling up this technology. This seems to be a critical success factor. The plans to identify and mitigate showstoppers were not made clear. There were positive results for high-temperature oxygen barrier coating materials, but it is unclear what the material of choice is. It is not clear how any cost/performance tradeoffs will be addressed. The publication record is commendable.
- While the project has hit some intermediate milestones and may well hit the funding opportunity announcement target of three liters of hydrogen in eight hours, a higher level of sophistication is required to gauge (1) the cost of doing this and (2) the reliability with which it may be achieved. Still, were it not for the PI and team's accomplishments, we might not even be able yet to list these concerns.
- The project team has met their goal of moving their technology from technology readiness level (TRL) 2 to 3 through their work on materials of construction and coatings, design of the reactor and other components, efficiency determinations, and TEA of costs for an integrated system. The TEA shows that realization of the DOE cost goal is heavily dependent on reductions in the cost of heliostats. Although research in this area is outside the scope of the Fuel Cell Technologies Office, it would be helpful if DOE and/or the project teams gave updates on progress in this area when possible.
- Using a sweep gas is a good approach, but it does create the need for separations. The coating may be able to inhibit gas diffusion; however, it will be in an abrasive environment, which will likely wear it out quickly. The cycle time for this system is extremely long. The project has had a good budget for three years, and the progress has been modest. The presenter started his presentation by pointing out what he

perceived as problems with another project (by name). This was inappropriate and unprofessional. It lessened the accomplishments that the presenter has made.
- So far, the project's accomplishments represent a modest contribution toward development of future high-performing STCH materials and systems. That said, it is also important to recognize that this project and any other solar thermal concepts are far from achieving water-splitting systems capable of 50,000 kilograms of hydrogen per day at $2 per kilogram, the current DOE objective.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration of this project team with NREL and the NSF project team has been excellent. It is likely that this collaboration will continue after the end of these projects. This would be very helpful for the continued development of reactor materials, as would collaboration and communication with other groups developing and modeling these materials. The participation of industrial partners (e.g., CoorsTek and Ceramatec) has been valuable to the development of the reactor coatings and components and will be crucial for the scale-up and commercialization of their technologies.
- Collaborators are mostly from industry, which is somewhat unique for solar water-splitting projects, but this shows industrial support and further validates the promise of the concept. The university and national laboratory collaboration are also strong, and the models for coating stability match well with experiments. This project also nicely complements the STCH efforts at SNL.
- The project has a well-balanced team of collaborators: a national laboratory (NREL), other academic institutions (Australian National University), industry (CoorsTek, Ceramatec, SABIC), and consultants. Each collaborator seems to have made meaningful contributions to the project.
- This was done really well.
- There are several unfunded collaborators.
- The list of collaborators seems reasonable, although it is not clear what the contribution or commitment levels are from the paid and "non-paid" partners to the current project stage.

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.

- This group has a clear path to obtaining their final project metric of three standard liters of hydrogen in eight hours. This was deemed an appropriate target by DOE at this stage of development of the project and the field, and the result should therefore be commended. Also, the TEA is very promising and should be validated by Strategic Analysis, Inc., and NREL. This effort seems game-changing in the solar-water-splitting portfolio.
- The project fully supports the DOE goal of reducing the cost of hydrogen production from renewable sources to <$2 per kilogram. The project has made progress in demonstrating the competitiveness of STCH production through the technical progress made during the project. The efficiency comparisons between solar thermochemical and other forms of solar-based water splitting (e.g., PV electrolysis) are also very helpful.
- STCH needs projects like this one to move from paper concept to testable option. The work exposes critical issues. However, this is where a detailed TEA, beyond just a Hydrogen Analysis (H2A) case, is critical. It may be that the researchers are too invested to expose fatal flaws. For example, the huge impact of active material kinetics is acknowledged but does not show up in the H2A results. If the answer is that the H2A Advanced Water Splitting Materials Consortium (HydroGEN) needs to focus on kinetics over, say, activity, that is something DOE and the community need to root out.
- The project scope and objectives are very relevant to the Multi-Year Research, Development, and Demonstration Plan in advancing research toward affordable and renewable or low-carbon hydrogen sources.

- This project cited CSP with thermal storage for heat and electricity as enabling. This has not been well appended, and claims need further vetting.

Question 5: Proposed future work

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

- The project is scheduled to end this year. Future work would presumably continue under a new project and/or through the HydroGEN consortium. The presentation provided a logical outline of what might be addressed in future work, which focused on continued modeling and testing of reactor and coating materials and components. Issues of scale-up and longer-term testing would also need to be addressed. For example, if particles of reaction were expected to last at least a year of one-hour cycles, 10 hours a day, that would be a necessary lifetime of 3650 cycles (3650 hours). Tests longer than 200 hours would be needed to adequately determine the reactivity stability and robustness of the particles. Acquisition and testing of a (>850ºC) temperature ITM SEOS membrane would allow for valuable updated estimates of the efficiency of the reactor system. A hybrid solar–electric receiver was proposed as a possible future endeavor. This sounds like a good idea, and efforts should be made as soon as possible to garner industry and utility interest in this design.
- The proposed work on detailed thermodynamic and kinetic optimization of current reactive materials is not likely to add much value, given that more efficient water-splitting materials need to be discovered to achieve the DOE goals. The materials discovery effort should be separated from the reactor or TEA work, perhaps moved to the new Energy Materials Network. If funding continues in 2018, this project should focus on reactor design and testing with existing and well-characterized reactive materials. Also, the proposed work on the hybrid solar–electric receiver could be a distraction. Although important, integrations with PV for parasitic loads or CSP storage systems are conventional engineering issues that are meaningful at large scale but not critical to the success of the STCH pathway. Besides, such considerations of heat and power integration are better handled by commercial CSP vendors.
- A few experiments could have been conducted more rigorously. For example, it was reported that SiC particles could be protected from reacting with steam by coating them with alumina. It seems that this has been reported in the literature numerous times. It would have been much more relevant to coat a slab of SiC with alumina and then allow the perovskite particles to fall against it, at temperature, followed by measurement of the persistence of the Al_2O_3 coating at the impact site. As hinted by the PI himself, we would probably have to completely throw out what is now claimed as success as actually inadequate. An alternate coating/approach would be required. Proving materials performance and lifetime/reliability is perhaps most value-added when those tests represent, as closely as possible, actual operating conditions.
- The future work is fine, but it is not certain that a larger focus on solar–electricity co-generation should be considered. Optimizing STCH alone still seems like a large challenge.
- This project is ending.

Project strengths:

- The project has a well-funded, creative team with a well-thought-out set of collaborators (e.g., NREL, NSF, industry). The focus on the reactor enabled more depth on that front, avoiding dilution by relying on the active sister materials project. Strengths also include integration of experiments and modeling across the board.
- The project strengths include the strong collaborations with the NSF materials discovery project, the innovative designs for reactor component materials coatings, and the modeling efforts (e.g., kinetics of reaction, inert gas sweep vs. vacuum).
- Strengths include an excellent PI, an excellent team, and great collaborations, as well as better use of science and theory to guide materials selection/discovery.
- Project strengths are the project team's experience with solar thermal and high-temperature systems, plus the availability of solar simulator and on-sun testing facilities.
- The reactor demonstration was fantastic.
- This was a well-funded project with a large team.

Project weaknesses:

- Any comprehensive STCH project is ambitious, so apparent lack of a project schedule (as mentioned in the last review but not addressed in this one) is disturbing. It is not certain that co-dependent milestones will be hit to ensure project completion by the end date. Also unclear is how sophisticated a process model has been developed. This would allow identification of critical challenges that may force design changes and support a detailed TEA. Of particular interest is the ability to predict/verify the 60% solar-to-hydrogen efficiency being assumed. Lack of active material attrition testing is another potential showstopper.
- The many compounded challenges, especially in integrating the discovered innovations, seems to have constrained the ability to prove viability of the innovation(s) in representative environments. This is not necessarily a consequent of funding and time alone but also planning of the experimental campaign.
- The project lacks perspective. A big-picture discussion and large-scale vision of the STCH system could be helpful to both the researchers and audiences in providing the significance of the project's effort with respect to the long-term goals.
- A strategy and/or schedule for eventual down-select of materials of reaction is not addressed. A realistic timeline for scale-up, commercialization, and deployment of the technology was never addressed.
- The TEA should be validated against practical current systems with the help of Strategic Analysis, Inc., and NREL, and bottlenecks should be more clearly identified.
- The reactor designs were overly simple. The material development was minimal. The project should have worked with SNL on material development.

Recommendations for additions/deletions to project scope:

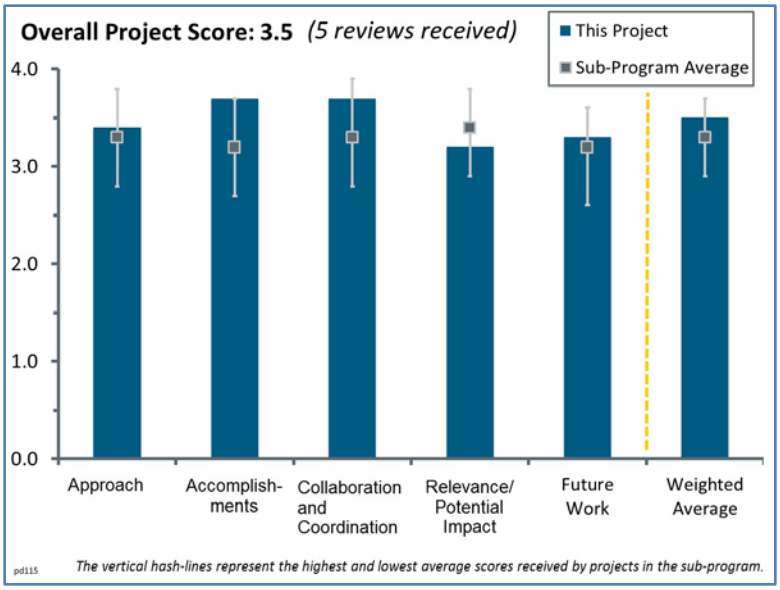
- The project should address the following big-picture questions: the conceptual solar field configuration to make 50,000 kilograms of hydrogen per day, how many acres and how many solar towers will be needed, what the output is from a single solar reactor, whether there are theoretical or practical limits to scaling up a reactor, the ideal STCH material performance vs. existing ones, etc. This is a low-TRL technology that will demand long-term and sustained commitment.
- As the project nears completion, the team should consider providing input to the HydroGEN effort on STCH materials to ensure the project's learnings are transferred to that community. In addition, the team should consider the impact of water feed—quantity and quality—so it can be incorporated into the TEA. (It would be good to know if the plant will need a sophisticated purification system.)
- The project should be allowed to complete its originally planned period of performance and then be subjected to re-compete through re-application and merit review.
- The scope is sufficient.
- The project is ending this fiscal year.

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 highly efficient, durable photoelectrochemical (PEC) reactors that can operate under moderate solar concentration and generate renewable hydrogen for less than $2/kg from PEC water splitting. The objectives for the current year are to (1) push boundaries on achievable semiconductor PEC solar-to-hydrogen (STH) efficiencies, (2) benchmark STH efficiencies for multi-junction (tandem) PEC devices, and (3) improve material durability through approaches such as stabilizing surface modifications.



Question 1: Approach to performing the work

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

- All barriers are addressed with a multi-disciplinary team that fabricates, characterizes, and tests materials with a foundation in theory. The outstanding approach—to first maximize efficiency, then increase durability, and then scale up devices—should lead to DOE cost targets being met.
- The approach has been to focus on maximizing efficiency of tandem photoadsorbers through the development of buried junction 3-5 solar cells, and showing feasibility of a number of approaches to extending the durability of the devices. The project addresses the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP) targets and leverages future cost reductions for photovoltaic (PV) fabrication based on other U.S. Department of Energy- (DOE)-funded efforts.
- This is the flagship project of the PEC solar hydrogen program. All the other project investigators look to these investigators for leadership and list them as major collaborators. Barriers are being addressed, although more weight should be thrown into durability than efficiency at this point. Nevertheless, bragging rights count for something, and world-record STH efficiency qualifies as such.
- The project has a good approach to achieving DOE efficiency goals. It seems that back illumination could have been helpful for these concentrator cells. Specifically, illumination from the back substrate could minimize parasitic optical losses associated with the protective coatings/counterelectrodes and/or liquid electrolyte. The best-performing devices have excess photovoltage, so the focus should be on increasing photocurrent to maximize STH. Additionally, back illumination would allow for thicker (more effective) protective coating and larger electrocatalytic area, which would be desirable for current densities >100 mA/cm^2.
- The approach observed is primarily related to improving the efficiency of the conversion process; while durability and material stabilization appears as an objective, it does not appear to be included in the individual partner approaches.

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

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

- The team has shown great progress throughout the duration of the project and, most significantly, by achieving the world-record PEC STH efficiency. The team is on track to meet the final project target.
- This project has achieved a world-record STH conversion record with benchmarking. Collaborative efforts are underway to improve durability, and they appear to be promising.
- DOE has recognized STH conversion efficiency as the primary driver for a cost-effective cell. The National Renewable Energy Laboratory (NREL) group appears to be well on its way to reaching the 2020 target of 20% STH.
- This project has achieved a record-setting efficiency of >16%. That approaches the DOE targets for 2020.
- The world-record STH conversion efficiency of the tandem PEC device is commendable. It was mentioned that a project goal of 875 hours of operation with <20% loss in STH efficiency was achieved, but this result was not shown in the slides. In contrast, the result on slide 16 shows ~20% loss in photocurrent of the GaInP photoelectrode after 100 hours. There are nice pictures of the PEC flow cell reactor to be used for concentrator experiments, but it is concerning that no measurements have yet been done with the cell and/or any measurements under concentration, for that matter.

Question 3: Collaboration and coordination with other institutions

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

- The project is well organized, and there is great interaction between the principal investigator and subcontractors, as evidenced by multiple accomplishments achieved through the collaborative. The investigators have reached out and gotten collaborative support from a number of non-funded groups. Examples include faradaic efficiency measurements and PEC device integration with Lawrence Berkeley National Laboratory.
- There is excellent collaboration with universities and relevant national laboratories. There are no industrial partners yet; as the results get more promising and scale-up is considered, the team will need to add industrial collaborations to ensure that the materials are truly scalable and manufacturable.
- Collaborators have included multiple organizations, some with high-risk approaches.
- It is evident that there are well-coordinated efforts across many research groups/institutions.
- There is a good balance of internal effort coupled with outside materials and expertise.

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.

- Direct PEC STH conversion has the potential to provide hydrogen at DOE cost targets; although these approaches are very much high-risk and high-reward, the payoff could be immense if they are successful.
- The project is well aligned with the DOE targets and goals for renewable hydrogen production.
- The project directly addresses the goals and targets set out in the MYRDDP goals and objectives.
- People who have worked in the PEC field have a good deal of respect for the NREL PEC program. Once you go outside the area, though, there is likely much wondering about whether it (PEC) is ever going to work. Those inside the field will say, “Of course it works!,” but until we can put a large array in the desert and have it function for several years, we have not really turned the corner. There are many ways to make renewable, sustainable hydrogen, and aqueous PEC is way down the list in terms of affordability. It is hard to envision a scenario in which PEC could make hydrogen more inexpensively than hydro electrolysis, wind electrolysis, and various types of biomass conversions. Nevertheless, scenarios in which the aforementioned do not fit may exist, and it is not really wise for DOE to force all renewable hydrogen efforts to meet the same $2-per-kilogram goal. No single technology is going to win out under all circumstances. PEC’s real competition always has been PV/electrolysis. The basic question is how much hydrogen can be made, and for how long, if the hermetic seal and front contacts are stripped from a PV cell and immersed in water. Looking at NREL’s famous PV research cell timeline, all of the better cells are based on GaAs. It is concerning to see that the NREL PEC group is thinking of getting rid of it in order to have a 1.0 eV bandgap semiconductor in tandem with $GaInP_2$. Hitting the theoretical bullseye on the

bandgap chart may open up some serious band edge matching and other recombination issues. The main problem for PEC is the need for a front coating that is optically transparent, antireflective, electrically conductive, impervious to water and acid, and catalytic for hydrogen evolution. That is a tall order, but some progress has been made. Another concerning aspect is if DOE eventually wants the sunlight to be concentrated 10x. In theory it should help, but apparently there are no recent data on this, and it will not be good for durability.

- Developments in synthesis of III-V materials for this project set the stage for high-efficiency device demonstration that is on track to hit the DOE goal of 20% STH. It is unlikely that cost goals will be met in the near future, as these hinge on many assumptions and optimistic technology developments, especially relating to the manufacture of III-V-based devices.

Question 5: Proposed future work

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

- The team has largely focused on achieving high efficiency for the PEC cell while still maintaining efforts on durability enhancement through barrier coatings, thereby showing the feasibility of generating a PEC device that can achieve the targets. The outdoor tracker and cell have been built and tested. Therefore, the proposed work to meet the final project targets has a high probability of success.
- It will be interesting to hear about the device measurements for production of three liters of hydrogen in eight hours, as this will be an important first step toward trying to scale up this technology and test under concentrated illumination in a more realistic reactor set-up (as opposed to test cells with large electrolyte volume and counterelectrodes). Although the reactor has been fabricated, it is concerning that no preliminary tests have yet been done either with the reactor or to evaluate the stability/performance under 10-sun illumination. Troubleshooting with reactor operation and product gas collection can be anticipated. Additionally, it is surprising that stability measurements were not conducted at this light intensity from the outset, since achieving technoeconomic and throughput milestones depend on operation at the high light intensity and it is possible that stability issues could be amplified under these conditions.
- Future work includes demonstration of three liters of hydrogen in eight hours. While this seems like an important demonstration, it is far from the end goal. Resources should concentrate on meeting the 20% efficiency targets and the durability/lifetime targets of 5, 10, and 20 years. In fact, it would seem that increasing the efficiency targets beyond where the efficiency currently stands is less critical than addressing the photocorrosion issues and longevity issues. Durability of 100 hours seems trivial compared to the required years of service. It seems that the durability issue is continually ignored by all projects in this area; the researchers would rather concentrate on the efficiency target, which is already much closer to where it needs to be, as opposed to durability, which is essentially nowhere.
- Even with world-record efficiency, the project still had many caveats to explore: antireflective top coatings, reflective back contacts, non-noble metal hydrogen evolution catalysts, Ni/Ti epoxy meshes, and lower-bandgap energy materials to better capitalize on tandem effect. However, a word of caution: the real action is in stabilizing the photoelectrode–electrolyte interface without substantially reducing hydrogen evolution activity.
- The team will now demonstrate that three standard liters of hydrogen can be generated with eight hours of sun illumination. While this practical demonstration will be illuminating, resources could be better spent further refining the device to overcome remaining durability and cost issues.

Project strengths:

- Gallium-based semiconductors are generally accepted as affording the highest PV efficiencies and charge carrier mobilities. At this point, it is hard to imagine a PEC program without this technology. Other strengths include a strong internal III-V PV support group and a good team of collaborators.
- The project brings together a diverse team with highly complementary expertise to develop high-performance III-V-based photoelectrodes. The team is to be congratulated on the world record.
- The strength of the project resides in the world-class team assembled and the approach taken, which has generated excellent results to date.
- The project has world-class expertise, performance, and partners.

- The project has developed materials with world-record STH efficiencies.

Project weaknesses:

- DOE studies advocate concentrated sunlight to augment device efficiency, but this group has little to no experience as to what happens when the light gets really bright. We will all find out eventually, but durability issues will likely become even worse than they are already. Steady performance for 100 hours is better than it used to be, but to attract any attention outside the immediate scientific community, it will have to climb by more than an order of magnitude, and even higher if the cells remain as expensive as they are now. There are several strategies to improve efficiency but very few in the way of reducing cost. It is a harsh reality if this is to make it to the marketplace.
- Potentially too much reliance has been placed on a final Hail Mary test that integrates multiple innovations that have been proven only separately.
- Weaknesses include lack of durability and operating lifetime, and the fact that this is not really even being addressed is likely the show-stopping issue for this technology.
- More work is needed on durability and scale-up issues.

Recommendations for additions/deletions to project scope:

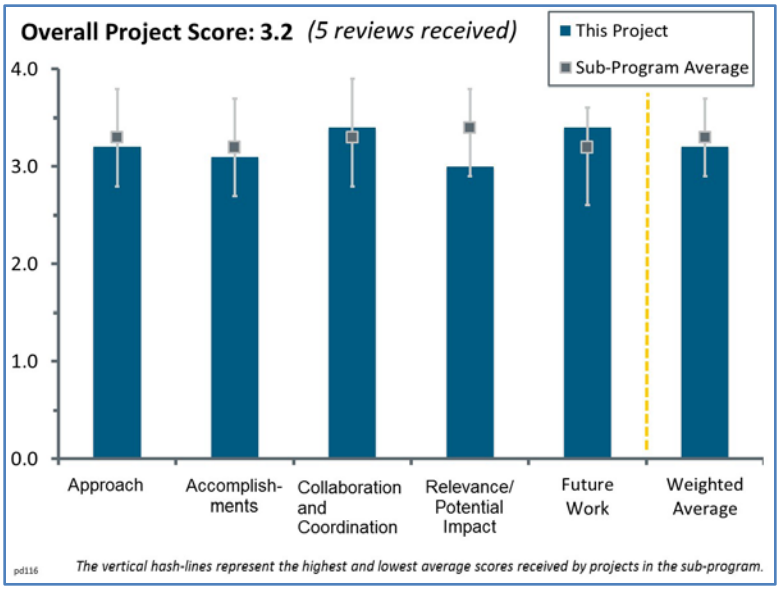
- The existing plans are fine, but it will be crucial to begin testing the reactor under concentrating conditions as soon as possible if the three-liters-in-eight-hours goal is to be achieved.
- Fiscal year 2017 durability tests seem a little short-term but nevertheless should be done if that is where the technology stands.
- This project needs to start addressing the lifetime.
- The project needs serious consideration of 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 copper chalcopyrite-based materials that can operate under moderate solar concentration and are capable of generating hydrogen via photoelectrochemical (PEC) water splitting at a cost of $2 per kilogram or less. The Hawaii Natural Energy Institute (HNEI) will (1) develop new wide-bandgap (>1.7 eV) copper chalcopyrites compatible with the hybrid photoelectrode design, (2) demonstrate at least 15% solar-to-hydrogen (STH) efficiency, and (3) generate three liters of hydrogen under ten times concentration in eight hours.



Question 1: Approach to performing the work

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

- The project focuses on low-cost fabrication of chalcogenides and alloying in order to simultaneously meet activity, durability, and cost targets. Project partners include very relevant capabilities in corrosion protection, modeling, surface characterization, and device validation. The project addresses the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP) targets for PEC hydrogen production. The chalcogenide approach leverages cheap, scalable earth-abundant materials as photovoltaic (PV) materials for the PEC devices. The research uses computational tools to tune the chalcogenide composition, then to fabricate and characterize the materials in a feedback loop.
- The HNEI group recognizes that they will have to develop a tandem cell to push chalcopyrite semiconductor performance past the 15% STH level. Given the evolution of the solid-state PV field, this effort is reasonable and complementary to the NREL effort. Both efficiency and durability issues are addressed.
- This is an attractive approach to achieving the U.S. Department of Energy's STH short-term goal of 15%. It is unlikely that the approach will achieve >20% STH based on current materials performance, especially if an all-chalcopyrite or chalcopyrite/Si tandem is used. If a III-V must be used for the bottom cell, with all of the processing/costs those materials entail, it seems like it would make more sense just to complete the monolithic tandem with a III-V top cell as well. A 900 mV open-circuit voltage (V_{oc}) (project goal) is far from where it would need to be, to be coupled with a Si-based tandem cell. It seems that perovskite would be a better approach in this respect (assuming perovskite stability issues are solved).
- The investigators are addressing the relevant technical barriers, though they seem to be fairly far from achieving them; there are many, many technical hurdles ahead. They have assembled a good team to work on the different challenges, which are daunting, from the hydrogen evolution reaction (HER), oxygen reduction reaction, and separator, not to mention the bandgap materials and corrosion/durability of all of these subcomponents, none of which have been demonstrated.
- The project has a good approach, combining synthesis, advanced characterization, and testing; but it is not clear how the Materials Genome Initiative and the Energy Materials Network are pushing toward true newly innovative approaches. It is not totally convincing that the efficiencies achieved in PV mode will translate to electrolyte exposure.

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.

- The project shows progress toward the final targets in that chalcopyrite materials with desired bandgaps have been fabricated and tested, and buffer layers and corrosion barriers have been evaluated. However, integration of both that would lead to the ultimate benefits desired have been challenging and have limited the project's capability to meet the durability targets. The team recognizes the challenges and has a path toward addressing them in order to meet the project targets.
- HNEI has worked out some pretty detailed and sophisticated synthesis and characterization methods to establish clean phases of various combinations of Cu, In, Ga, Se, and S with 1.7–1.8 eV bandgap energies, essential for tandem design. The project also examined In_2S_3 ink for the buffer layer as an alternative for CdS. The project also examined MoS_2 on TiO_2 as a catalytic top coating on the photoelectrode. A 40% increase in longevity was noted, but since baseline performance was only 250 hours, much greater improvement will be necessary.
- The investigators have shown some good improvements in current density and longevity through some truly impressive work. However, some of the numbers do not seem to add up; they have shown 300 hours of durability with a 50% drop in voltage and almost all of their Cu dissolved, yet they claim they are 45% of the way to their 750-hour 2016 goal. The metric needs to be better defined; simply running is not good enough; efficiency should come into play. The cost analysis just stretches any credibility without further demonstration. The investigators state that if they can hit their efficiency and materials cost targets, then they can generate hydrogen at $3 per kilogram, even if the system lasts only six months. This cannot include installation, service, etc. Also, they must not be considering compression, which generally costs >$1 per kilogram. The economic analysis is obviously an afterthought, but they should not present it unless they can seriously defend it.
- Good progress has been made toward increasing the performance of the chalcopyrite photoelectrodes, although significantly more progress must be made if the project is to achieve its final goals relating to V_{oc}, durability, and STH efficiency.
- Some of the new materials still seem to have issues, which are being worked out, although the team still does not know the exact cause. There has been a good deal of work achieved, but no device efficiencies under solar illumination are reported.

Question 3: Collaboration and coordination with other institutions

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

- There is abundant evidence of collaboration among the project partners. There is good use of modeling and characterization capabilities with device and structure synthesis across institutions.
- There is an excellent set of university and national laboratory partners appropriate for this project that can provide all needed skill sets.
- The project has good collaborative efforts with Stanford University and University of Nevada, Las Vegas. Other collaborators were mentioned, but connections were less clear.
- There appears to be very good collaboration and coordination among the team partners.
- The project has proper delegation of tasks and seemingly good coordination among strong partners. It is doubtful that such a large number of collaborators is the most efficient way to run a 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 **3.0** for its relevance/potential impact.

- The investigators have made good progress, but it is doubtful that they can reach the goals of durability and especially cost. A big change might occur if they improve the separator, or if durability appears to be due to

imperfections in the separator as they suspect. Previously, their goal was to move to a monolithic device, but that goal seems to be abandoned. Although they will not reach the targets, they are making impressive progress toward them, just more slowly.
- Direct PEC can potentially provide hydrogen at a cost-competitive price. This project is still a ways from addressing durability and cost, as the materials do not yet seem to be achieving desired efficiencies.
- Basing a PEC hydrogen evolution cell on thin-film PV currently best addresses cost objectives.
- This project directly addresses the efficiency, cost, and durability targets set in the MYRDDP.

Question 5: Proposed future work

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

- Future work will solve many of the remaining issues with these materials, such as durability, cost, and device integration; so while the materials are improving, further funding should really push the investigators toward device-level validation.
- Through the end of the project, the team will continue making refinements to the integration process in order to make a durable and efficient Cu-In-Ga-Se (CIGS)-based device with buried junctions and barrier coatings. The device fabrication will culminate in a final test demonstrating the capability in outdoor conditions.
- The proposed future work offers a logical progression of experiments focused on achieving project goals.
- Many photoelectrode materials were discussed in the presentation, all of which consisted of different combinations of the same five elements. Eventually, some type of down-selection needs to occur; it appears that indium and sulfur will be left out, and two different ratios of Cu:Ga:Se will be further studied. Employing them in combination with lower-bandgap but dissimilar materials such as Si or GaAs looks like a risky endeavor. The solid-state community does not seem to have had much luck in making tandem cells from CuInGaSSe.
- It is not clear how the investigators' work so far moves them closer to their monolithic goals, or why they are going through this intermediary step. There should be continued focus on durability and what is limiting that, though if the focus is working on improving durability for a system that is only temporary, it is not clear how that helps the project. It will be interesting to see what the team is using for HER and oxygen-evolution-reaction catalysts and how that effects the cost and efficiency.

Project strengths:

- The bandgap tunability and potentially low costs of chalcopyrite top cells make these materials attractive for efficiency and low-tandem devices. The team is very experienced in this area, with complementary expertise in materials synthesis, characterization, and modeling.
- The project receives good help from collaborators. Thin-film PV is thought to offer lower-cost electricity, which should ultimately translate into cheaper hydrogen. The team has well-developed understanding of a complex system (three or more major elements).
- The CIGS-based approach is relevant for achieving MYRDDP targets based on cost and scalability, with plenty of room for improvement relative to accomplishments made to date. A good team is assembled that is prepared to address and deal with any barriers that arise.
- Strengths include integration of synthesis, characterization, and theory.
- Strengths include a good team, characterization capabilities, and proper focus.

Project weaknesses:

- The system is becoming progressively more complicated (catalyst/conformal coating/buffer layer/wide-bandgap material/indium molybdenum oxide [IMO]/narrow-bandgap material/back contact), but the complexity may ultimately be justified to improve efficiency.
- The fill factor and photocurrents of the chalcopyrites being made thus far will very likely make the top cell the limiting component in a tandem device. It is not clear that there is a definitive plan in place to improve those values to levels necessary to achieve project goals. Similarly, it is not clear that the chalcopyrites

demonstrated thus far provide enough photovoltage for successful demonstration, even with a high-performing III-V bottom cell.
- The economic analysis is just not credible without more substance. If the investigators were using their system just to make electrons, it seems unlikely that they could achieve a price of $0.05 per kilowatt-hour in a system that lasts one year. If electrolysis and everything else were free, that would put them over $2.00 per kilogram, just for starters.
- The project relies too heavily on assisted PEC and PV measurements, considering the final project target.
- No real-world device testing is a weakness.

Recommendations for additions/deletions to project scope:

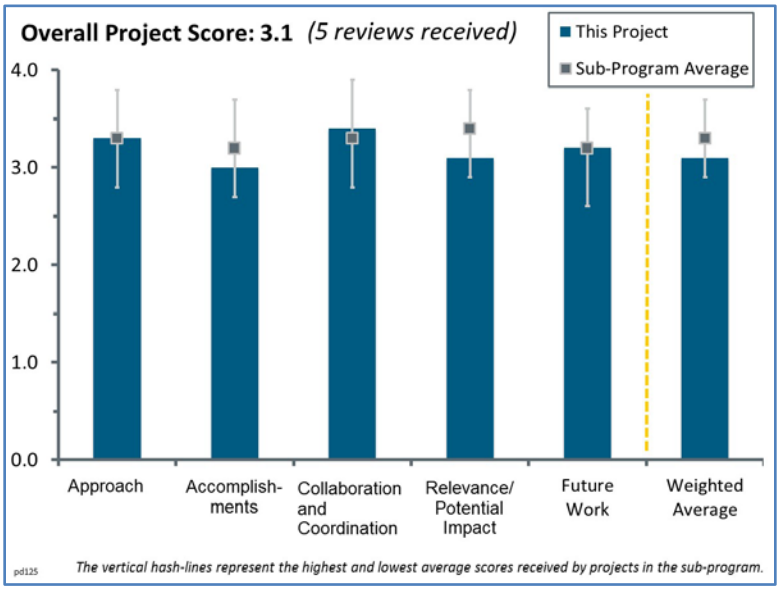
- Emphasis should be on preserving the photoelectrode–electrolyte interface. It is already known that CuInGaSSe cells produce a reasonable photovoltage and can do some electrochemistry. The more insightful question is for how long.
- Integration into devices should be added.

Project #PD-125: Tandem Particle-Slurry Batch Reactors for Solar Water Splitting

Shane Ardo; University of California, Irvine

Brief Summary of Project:

This project aims to experimentally validate a laboratory-scale particle suspension reactor as a scalable technology for photoelectrochemical (PEC) hydrogen production. The novel approach entails stacking the two slurry-reactor compartments in series instead of the more typical parallel, side-by-side arrangement to realize the tandem efficiency advantage and shorten the mass transport distance so that fewer pumps and pipes are needed. The project will perform numerical device-physics modeling and simulations of particle-slurry tandem solar reactors as well as design, fabricate, and experimentally test this reactor concept.



Question 1: Approach to performing the work

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

- The project approach is to meet Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP) targets based on a new tandem particle PEC system including chemistry and cell design using a combination of modeling, experiment, and cell design. The project is novel and unique in that it approaches the problem from a low-technology-readiness-level- (TRL)-based concept that has a good deal of potential to meet the targets. The teaming is such that it uses modeling to assist in cell design and validation prior to confirming with experiment, which saves time and allows the team to address other issues such as particle and shuttle development.
- The project is well conceived and has a nice logical approach to the design, analysis, and demonstration of particle-based PEC reactors. The experimental and modeling efforts are nicely intertwined, and there is a logical progression in the experimental work in going from thin film to particles. The project goals are feasible.
- There are many barriers to be addressed, and the principal investigator (PI) would be hard-pressed to address all of them in the same contract year. Nevertheless, there should be a logical order to it, and doing a fair amount of photoreactor modeling before having a good basic theory of photoparticle function seems wrong. On the other hand, the PI was likely under pressure to show whether photoparticle systems can be affordable under any circumstance, and so this is how it played out. Much of the experimental work was done with semiconductor electrodes in traditional potentiostated electrochemical cells. There may be a limit as to the applicability of those results to photoparticle performance. Because the other two PEC efforts are squarely in the electrode configuration regime, there is a limit as to how well integrated this effort could be with the others. On the other hand, if $GaInP_2$ and $CuGaSe_2$ make good photocathodes, maybe the PI should obtain them as powders and give them a try.
- This is a modestly funded project with very ambitious goals. The project has a large number of technical hurdles: hydrogen evolution reactions, oxygen evolution reactions, passive convection/diffusion. With this modest budget, the project might be better off concentrating on one of the challenges. In the economic analysis and U.S. Department of Energy goals, the project assumes 5% or 10% efficiency. It does not seem to be on any pathway to achieving those numbers, and the intermediate goal is only 1%. Nevertheless, the approach is very interesting and appropriate if the team is happy with 1%.

- This project looks to be successfully integrating modeling and experimental synthesis from the various partners. While two approaches are being examined, these seem to be "reactor designs." What is really needed is better photoelectrochemistry materials.

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 modeling work is on schedule or ahead of schedule. Some of the experimental work is behind schedule, especially relating to the development of the PEC materials to be used as particles. For example, the photocurrent generated by the photoelectrodes (especially photocathodes) is below target. Part of this is due to unexpected low manpower on the project, so it is feasible that this can be made up. The photoreactor to be used in this work appears to be on track.
- The project has identified hydrogen evolution photocatalysts and oxygen evolution catalysts, as well as a couple reduction–oxidation reaction (redox) shuttle pairs, that appear to easily achieve the 1% solar-to-hydrogen (STH) efficiency threshold. Showing that natural convective flow can support sufficiently different concentration ratios of redox shuttle, obviating parasitic energy losses due to pumping, is an important result. It was clear that the photoparticle reactor model is becoming increasingly more sophisticated, aided in part by collaborators from Lawrence Berkeley National Laboratory (LBNL) and the California Institute of Technology (CalTech); however, it was difficult to grasp which phenomena have and have not been brought into the model. It would help if the PI would prepare one or two slides summarizing what has gone into the model and what is left to do. Several important experimental milestones have yet to be reached, so measuring overall progress relative to DOE goals is difficult.
- The projective main objective is to experimentally demonstrate a new design for a particle-based PEC device. The accomplishments to date include numerical validation and design of a concept that will meet the MYRDDP targets, as well as materials development and characterization for the particles themselves. Significantly, the system has been redesigned to eliminate plumbing by stacking the particle beds, which was validated numerically to be feasible through diffusion of the media. Meanwhile, the materials development to make the photoactive particles is showing progress toward reaching the 1% target. The project has been extended (no cost) in order to experimentally build and validate the device.
- The technoeconomic assessment was conducted, with promising results. Coupling the physics model to demonstrate stable operation is another promising result. The tandem reactor operates at 1% STH efficiency; it is unclear how this is approaching a realistic operating efficiency related to the DOE targets.
- The accomplishments are impressive if compared against the intermediary goals of 1% efficiency, so progress toward project goals is good. When measured toward Hydrogen and Fuel Cells Program goals, the project is an order of magnitude away, and it is difficult to see a pathway for it to achieve overall goals.

Question 3: Collaboration and coordination with other institutions

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

- There is nice collaboration between experimental work at University of California, Irvine, and modeling work at LBNL. There is also an unfunded collaboration with a group in Japan.
- The project has good partners and a strong group. Collaboration is efficient in that work is centered at one group, with collaborators supplying either materials or analysis.
- There is evidence of strong collaboration and interaction between the project partners. The modeling side is communicating and interacting with the experimental side and device fabrication.
- Because the work has been mostly particle reactor modeling, it is difficult to identify common interests with the National Renewable Energy Laboratory and Hawaii Natural Energy Institute. Nevertheless, the PI has benefitted from collaborators at LBNL and CalTech.
- The project seems to be successfully integrating modeling and materials synthesis from the 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 **3.1** for its relevance/potential impact.

- This project is addressing the hydrogen production targets and could be a critical pathway leading to renewable generation of hydrogen.
- It is unlikely that this project will achieve MYRDDP STH efficiency targets, but there is much value in demonstrating this type of PEC device that could pave the way for low-cost higher-efficiency systems based on better materials in future efforts. The potential to achieve low capital expenditure (capex) is the biggest merit of this project/approach.
- Photoelectrochemistry as a whole looks so expensive in comparison to other sustainable, renewable ways of making hydrogen that some kind of "think-outside-the-box" approach for the project is appropriate.
- While the efficiencies at this TRL are low, the innovative design has the potential to lead to a strong impact, if it can be experimentally verified.
- The project goals are a little difficult to accept. The researchers want to achieve a cost target of \$20/kg H_2 while achieving a 1% solar efficiency. Put another way, if they somehow miraculously achieved 20% efficiency, their costs would be \$1/kg H_2. This is difficult to accept, especially with the short lifetimes given in the analysis. Making these very muted targets will still be extremely challenging, as they have numerous technical challenges.

Question 5: Proposed future work

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

- The proposed future work extends the timeframe of the project in order for the experimental work to catch up with the numerical models. This seems like the right thing to do in order to complete active material development and scale up the device and particles for the final demonstration.
- It is clearly time to put real particle suspensions in the baggie and see what happens. It would be best if they can find time to develop a model of the nanoparticles before they start fabricating them.
- All of the future work plans are logical and consistent with achieving project goals.
- The demonstration of three liters of hydrogen is important; however, it is unclear if the current efficiencies are relevant to long-term targets. The project is proposing the demonstration at 1% efficiency; the 2020 target is 5%, and the ultimate is 10%. It seems that increasing the photo-efficiency should be the main driver of future work, as the project is an order of magnitude away from where it needs to be.
- Working on making a unit to generate three liters in eight hours seems like more of a stunt then a relevant metric. The time and energy might be better spent on solving some of the fundamental materials problems the project has. Having said that, there is value in seeing where you are and addressing practical considerations.

Project strengths:

- Type IIB reactors offer an attractive opportunity to achieve a combination of low capex and STH efficiency that could meet DOE hydrogen production goals. Although the project does not seem to be pushing the limits of knowledge/performance on the materials side of things, the modeling work and proof-of-principle demonstrations of the reactor should be very valuable for the PEC research community. Additionally, this would be the first demonstration of a Type IIB PEC particle reactor.
- The technoeconomic assessment suggests that this could have a positive impact—and without the durability issues that plague other PEC efforts.
- It is an effective answer to those who maintain that PEC hydrogen will always be too expensive. There is some earlier work to build on, so the PI does not have to start from scratch.
- The project has a good team and has made smart adjustments along the way. This is interesting work.
- Strengths include a good team, concept, and approach.

Project weaknesses:

- Pace of materials development and iteration is slow and has led to an extension of the project. However, materials and synthesis are new (outside of scope of 3-5 and Cu-In-Ga-Se systems), and the project has reached out to external collaborators. Additionally, the investigators are addressing reactor design simultaneously.
- The photophysical model of the nanoparticles is still under development and not as well understood as electrode configurations. It remains to be seen how well results obtained with a semiconductor electrode can be translated into a nanoparticle suspension. Many fundamental materials and performance issues remain to be resolved.
- It will be a stretch to meet the STH goals. Selective catalysis (preventing back-reaction) would be very useful but may take a while to develop.
- The current efficiency is far from where it needs to be. The project seems to lack a fundamental understanding of why the yields are so low.
- The project has huge materials problems and a small budget and amount of time to address them.

Recommendations for additions/deletions to project scope:

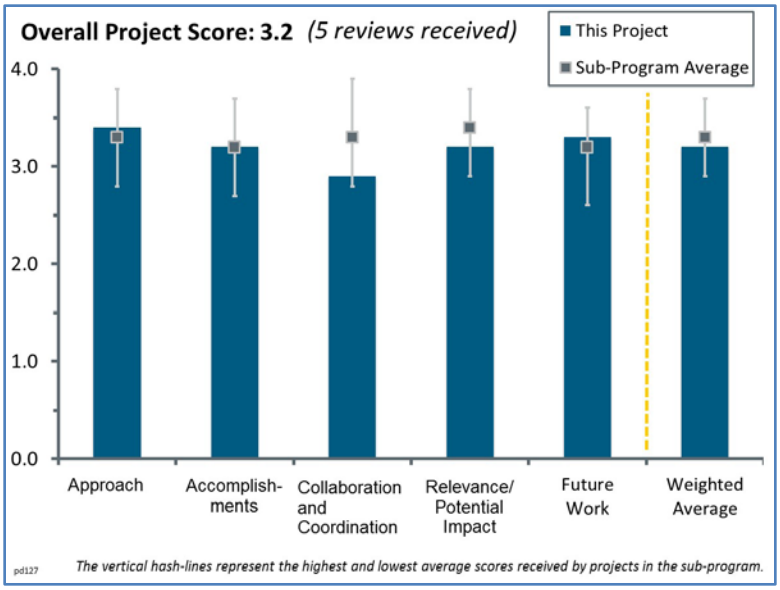
- If the investigators have not already done so, they might consider trying photocathode in the presence of the redox shuttle; this might outcompete the hydrogen oxidation reaction. Also, including a co-catalyst might be helpful, as there could be a built-in electric field across the Schottky nanojunction. The project could also consider trying the particle suspensions (just one half) in the near future, even if the photoelectrodes are not performing well; it is possible that some of the poor performance of the photoelectrodes could be related to the photoelectrode form factor (issues with carrier collection, ohmic resistances, etc.). Regarding modeling, the project might consider generating theoretical STH iso-contour plots with Eg1 and Eg2 on the x- and y-axes (see old Miller paper, Pinaud & Jaramillo EES 2015, etc.). It is not clear what the optimal Eg1 and Eg2 ranges are for these systems, especially if you consider typical particle densities/cell thicknesses and the associated parasitic absorption losses, nor is it known how much the optimal Eg1, Eg2 values differ from prior analyses for conventional tandem photoelectrode systems. This team should be well situated to generate these plots, and they would be valuable for the community, especially if the values end up being very different from prior analyses for photoelectrodes.
- Better understanding of the fundamental solar conversion is needed, with concentration on why the yields are so low. Perhaps the modeling components should be enhanced or added to increase the understanding of what materials could be developed to achieve more relevant quantum yield efficiencies. What is really needed is better photoelectrochemistry materials; concentrating effort on developing those materials rather than optimizing reactor designs around inadequate materials will achieve more valuable results in the long term.
- The PI understands that the redox shuttle electrocatalysis must have asymmetry, yet there is no obvious effort to deal with that issue. The redox shuttle will have to have more structural complexity than simple inorganic salts. Maintaining particle suspensions is going to be tough if there is no fluid flow because of pumping.
- The project might drop the three-liter demonstration if it is a large distraction.

Project #PD-127: Sweet Hydrogen: High-Yield Production of Hydrogen from Biomass Sugars Catalyzed by in vitro Synthetic Biosystems

Y-H Percival Zhang; Virginia Tech

Brief Summary of Project:

This project addresses the Fuel Cell Technologies Office (FCTO) objective of developing cost-efficient advanced biological generation technologies to produce hydrogen. Investigators are using enzyme cocktails to catalyze the production of hydrogen from renewable sugars (e.g., biomass sugars or starch) and water. This approach is expected to yield high-purity hydrogen at high rates through low-carbon production using local resources.



Question 1: Approach to performing the work

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

- The project identifies a number of barriers to producing hydrogen from biomass and uses an approach that is different from others in the portfolio and those identified in the FCTO Multi-Year Research, Development, and Demonstration Plan (MYRDDP): using an in vitro enzyme system to convert starch to hydrogen at high yields, with the approach focusing on improving production rates, increasing the volume, and showing cost-competitiveness. The approaches for improving rates and reaction volume are based on genetic engineering and molecular biology techniques; for some of these subtasks, such as shifting coenzyme preference, multiple strategies are identified. For the cost analysis, the Hydrogen Analysis (H2A) model is identified in the milestone table. The overall approach could be clearer, with a description of the expected enzyme/coenzyme combinations and reactor set-up (scale, temperatures, batch time length) for the final or modeled system. Some of this is still in progress based on the project results, but with all the different experiments in developing conjugates and biomimetic coenzymes, it is not clear what the final "mix" will be.
- The project is well designed, as demonstrated by the hydrogen production rate's doubling each year while costs are reduced by producing four enzymes in one expression. Further use of the starch feedstock results in production of high-purity hydrogen. Also, since there are no side reactions, it is easier to get close to theoretical yield. However, it should be noted that what will happen with dirty biomass remains to be seen.
- The project presents high-level scientific results in a very well-designed timeline. The approach seems reasonable to address barriers associated with an early-stage project. However, regarding the high-demanding technology of the project, the research needs to move forward to overcome challenges, such as scaling up the overall process and enzyme/coenzyme cost production, to be considered as a practical hydrogen production technology in the long term.
- This is a very innovative project. It is good to see the support for a high-risk–high-reward proposal. The approach seems to be generally strong. However, at times it was not entirely clear how all of the different project pieces fit together. While the approach may be solid, it could be beneficial to spend a little time ensuring that the full scope of the project is accessible to the reviewers.
- The synthesis of the enzymes to hydrogen production seems very interesting and novel. However, it is not very clear whether, as the project scales up, the cost targets will continue to be met and whether the benefit of this will disappear entirely once the system begins to scale.

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 principal investigator (PI) is demonstrating good progress on the stated project goals. The project has demonstrated complete starch utilization for hydrogen generation for the first time.
- The work related to improving the reaction rates and yields has shown progress, as has the work to shift the specificity of enzymes to utilize lower-cost coenzymes. The project has increased the volume of the reactor since the start, increasing from 1 ml to the 10 ml reactor size targeted by the fiscal year (FY) 2016 go/no-go milestone, but the FY 2017 third-quarter (Q3) milestone of a 1 L reactor has been delayed to December 2017. Based on the information in the presentation, the H2A cost analysis subtask has a significant amount of progress left to go and does not seem likely to meet its FY 2017 Q3 milestone deadline. All cost estimate information provided is based solely on the cost of the starches, the enzymes, and the coenzymes. The capital and operating expenditure (capex and opex) inputs are apparently in process, but these are a substantial part of the H2A model and are important for demonstrating that the system would be feasible, both from a cost and an application perspective.
- Scientifically speaking, the project succeeded in overcoming barriers such as co-expression of multiple enzymes in one host, enzyme/coenzyme production, and scale-up of enzyme production in a 1 L reactor. The hydrogen production rate is the highest ever obtained. However, although promising, these results were obtained at a very small scale. There are insufficient quantitative results to assess the price and scale in which this work can be accomplished. Moreover, not much information was given to ensure the stability of the hydrogen production rate in long-term operation.
- Reasonable progress is occurring toward the productivity target. However, it is not clear how the process will optimize to reduce costs of production from $1000 to $10 per kilogram of hydrogen between June and December 2017. There has been a nice increase of hydrogen from 200 mmole H_2/L/h in 2016 to 550 mmole H_2/L/h in June 2017, so it is conceivable that, during scale-up, the project may reach 750 mmole H_2/L/h by December 2017. However, it is not certain that the 2020 target can be met.
- The project seems a discovery-based fundamental research project. In this way, the project has completed or is on track to complete major milestones. However, sustaining peak hydrogen production may be critical for this project to be successful.

Question 3: Collaboration and coordination with other institutions

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

- The project involves work done in two laboratories: the PI's laboratory at Virginia Tech works on enzymes expressed in Escherichia coli (E. coli) and the improvements to increase rates and alter specificity, and a laboratory at the University of Georgia is doing mass production of the hydrogenase from the hyperthermophilic P. furiosus. The degree of interaction beyond sharing purified enzymes is not clear.
- There are only two universities participating in this project. It would be nice to see industry or national laboratory inclusion in the research for scale-up support.
- Very little information was given on collaboration details. An industrial partner would be important at this stage to analyze the feasibility of producing these enzymes on a large scale.
- There are collaborators in place. It was not evident that the PI is leveraging other resources to boost this study.
- There is insufficient information on collaboration details.

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 combination of the enzyme complexes and new hydrogenase SH1 (once loss of catalytic subunit is solved) may allow scaled-up hydrogen production increases that may eventually reach the DOE 2020 hydrogen volume and cost targets.
- The PI has a novel approach that could potentially have an impact on other fields. This is an innovative project, and it would be good to see it continue.
- This project does not fall into the standard pathways identified by the MYRDDP, so the MYRDDP pathway-specific goals are not entirely relevant. The project does address barriers related to hydrogen production from biomass. The project is certainly making progress toward increasing hydrogen production and yield using enzymes. The main FCTO objective identified for the project is to develop an in vitro biosystem that can produce hydrogen at a projected cost of $10/gge by 2020, but without a clear cost analysis of the entire system, not just the enzyme and starch costs, it is difficult to evaluate whether this will be successful in meeting the goals. The totals provided for just the starch, enzymes, and the lowest-cost coenzyme listed (nicotinamide riboside [NR]) add up to just over $8 per kilogram, which means the impact of the other costs may be a barrier to meeting the $10-per-kilogram target identified by the project. The proposed "sugar car" model does not appear to be relevant to the FCTO goals. Even considering the sugar conversion system as an alternative hydrogen storage system, at least five separate containers or system units are listed that would be needed to replace the hydrogen storage tank. Without compression, little hydrogen could be stored, so the system would likely need to be able to produce hydrogen on demand. The presentation compares starch to onboard hydrogen storage by noting the 14.8% hydrogen by weight, but the presentation does not take into consideration the rest of the system required to produce the hydrogen, or even the water required for the reaction to produce that amount of hydrogen from the starch (based on the provided reaction formula and cost analysis inputs).
- This is a discovery-based fundamental research project. The results, besides being outstanding, succeeded in small scale. There is a long way ahead to overcome scaling-up barriers and so achieve Hydrogen and Fuel Cells Program goals.
- The project value appears to be in a discovery-based fundamental research project. The scale-up is also important.

Question 5: Proposed future work

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

- Stabilizing coenzymes, improving expression levels and specific activity of SH1 production, and getting assistance with scale-up appear to be logical next steps.
- The project is nearing its end, and the main technical roadblock to completion appears to be issues with producing enough hydrogenase to scale up to the 1 L reaction demonstration. There is only one future step identified related to this: moving in a second copy of the full suite of hydrogenase genes. If this is not successful, it is not clear what the alternatives are, and whether this is a show-stopper or if it will simply take more time and batches to make the required enzymes.
- The proposed future work for the project's remaining six months is very challenging. The final 1000 ml volume of hydrogen reaction is still too small to guarantee the hydrogen production rate's stability using this technology.
- The cost analysis needs to be expanded, but it appears that the PI is aware of this.
- The scale-up step is important in showing the feasibility of the project.

Project strengths:

- Strengths include the following: successful enzyme and coenzyme production, complete starch utilization for hydrogen generation through enzymatic phosphorylation of starch, ultra-high-speed production of

biohydrogen gas in vitro, development of analysis methodologies, development of technology, and several publications in high-impact journals.
- The project is developing a hydrogen production system that is significantly different from others in the DOE portfolio, which if successful may be a way to avoid some of the barriers that other production pathways face. The project has made strong progress in enzyme engineering, increasing reaction rates and identifying novel ways to shift the enzymes to work with lower-cost coenzymes.
- A clear strength is the biological generation technology that allows co-expressions of enzymes that can be anchored on a substrate to increase reaction rates.
- The project is highly innovative, with a novel approach.
- Important gains in enzyme production were obtained.

Project weaknesses:

- More analysis and support are needed for the claims on costs. In addition to missing key aspects such as capex and opex, some assumptions are provided with limited or no supporting data. For example, the claim that the cost for starch would drop by a third in the future does not have supporting data, and the source for the estimate of $200 per kilogram of enzymes is not entirely clear, given the various ranges provided for industrial enzyme costs.
- Weaknesses include the small-scale reactions, use of clean sugars, and insufficient results of hydrogen production rate stability. In addition, the cost reduction strategy was not clear.
- Not much information was given to determine how long certain hydrogen productivity rates could be achieved. More detail concerning hydrogen production rates is needed.
- It was not clear that the cost of maintaining the two plasmids is included in the analysis of the overall financial cost assessment.
- The lack of a suitable scale-up partner is a project weakness.

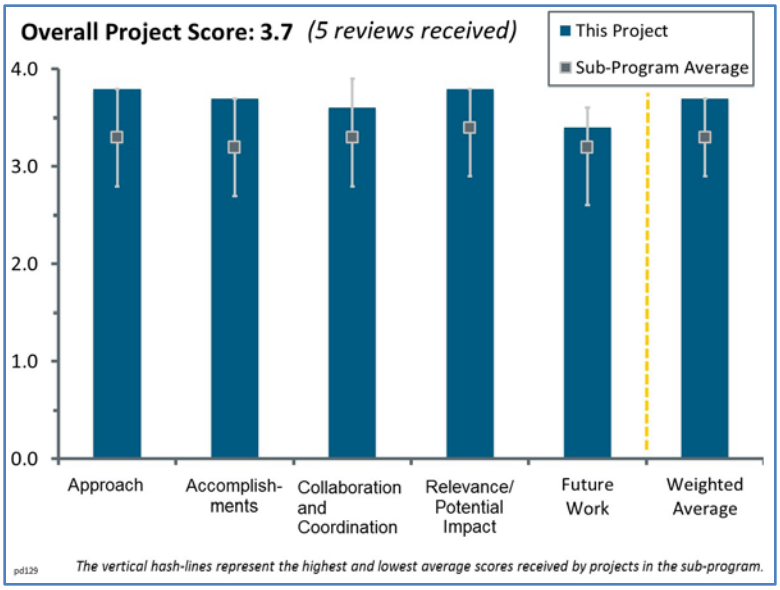
Recommendations for additions/deletions to project scope:

- The project should develop a clear cost analysis with information on all inputs and a description of expected reactor parameters (e.g., overall system size, reaction temperature, run times). In addition to their use for input to the cost analysis, the reactor parameters would be useful in understanding how feasible the system would be in different applications.
- More information about hydrogen production is needed. The approach appears to be primarily in demonstration of in vitro biochemistry of isolated enzymes and genetic tools for overexpression.
- Experiments with actual renewable feedstocks are recommended.
- The project has already noted the need for a scale-up partner.
- The cost analysis should be improved.

Project #PD-129: Novel Hybrid Microbial Electrochemical System for Efficient Hydrogen Generation from Biomass

Hong Liu; Oregon State University

Brief Summary of Project:



The project's target is to develop an efficient, hybrid fermentation and microbial electrolysis cell for hydrogen production that uses low-cost feedstocks (specifically lignocellulosic biomass and wastewater) at a cost close to or less than $2/kg hydrogen. The hybrid fermentation/microbial electrolysis cell system has the potential to be integrated with lignocellulose pretreatment/ hydrolysis or wastewater treatment processes. Specifically, the project is addressing the low hydrogen molar yield, high cathode cost, and low hydrogen production rate associated with current approaches. This is being done by (1) identifying a bacteria strain that produces >10% yield from all major sugars, (2) developing robust and low-cost cathode materials, and (3) performing system cost modeling to prioritize the critical factors and demonstrate potential to meet DOE cost goals.

Question 1: Approach to performing the work

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

- The project approach starts with an ambitious target, developing a microbial electrochemical system that can produce hydrogen from biomass sources at or below $2 per kilogram. The approach is to use a hybrid fermentation–microbial electrolysis cell (F-MEC) system, working the two systems separately first to improve performance and gather data, and then use analysis to develop and then demonstrate a reactor that is planned to produce 24 liters of hydrogen per liter of biomass per day. The strong focus on using cost and system analysis to guide later steps, including the design of the hybrid reactor, is a good approach. The approach of evaluating the viability of both biomass hydrolysates and wastewater increases the feasibility of the project—wastewater may be an attractive feedstock because it is low-cost and can even provide an avenue for revenue generation, but wastewater also has more challenges. Hydrolysates are more expensive but are a more consistent and less challenging feedstock. Evaluating both means that even if the wastewater challenges prove to be too high a barrier, hydrolysates may be used, and if both options can be demonstrated, the technology will have more potential applications.
- The project approach is excellent, despite the fact that it was not clear how the F-MEC system will be designed so that, optimally, both processes occur simultaneously. Having multiple teams has significantly improved their results to address the barriers, considering feasible strategies that target scale-up of the biomass-to-hydrogen technology, such as the use of mixed bacterial culture from environment sources, the use of a low-cost methanogens inhibitor at low concentration, and the study of a low-cost catalyst to the MEC system.
- The principal investigator (PI) has an innovative approach, and the use of wastewater is an asset. The work on the inhibition of methanogens is strong and will have a medium to high impact on the field. The work evaluating different catalysts is also strong and will complement other studies in the field.
- The project is well designed, as it starts with the evaluation of simple sugars processed in individual steps of fermentation, followed by MEC processing. Additionally, the use of wastewater in cost projections

allows one to see the impact of a feedstock credit that brings the hydrogen production cost close to the project target. However, actual testing using wastewater still needs to occur.
- The group is making progress on the stated aims while also addressing potential barriers and challenges.

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

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

- The research groups involved in this project have had several accomplishments.
    - First of all, they successfully produced hydrogen from pretreated lignocellulosic biomass using mixed bacterial culture derived from environmental sources, which certainly has higher robustness and feasibility to grow at large scales. A hydrogen production rate of >10 liters of hydrogen per liter of biomass per day is a satisfactory result for continuous-flow hydrogen production, despite of the use of mixed pure sugars. However, it is necessary to apply the actual biomass hydrolysate to verify whether the hydrogen production rate will be maintained, since the hydrolysate is more complex than pure sugars.
    - Second, the MEC culture work was also successful, with hydrogen production from liquid fermentation products. However, it was possible to observe an increase in acetic acid concentration, despite the organic acid consumption. The strategy to inhibit homoacetogenesis in the system is unclear.
    - Third, the team synthesized low-cost MoP catalysts with low overpotential that demonstrated high activity and durability under ex situ testing, similar to Pt catalyst performance.
    - The cost performance modeling for hydrogen production from biomass hydrolysate and from wastewater completes the technical achievements of the work.
- The project milestones are being met on time; Milestone 1, Phase II, was completed since the slides were submitted. The project tasks are showing progress and building on the results; for example, the team demonstrated that their existing lab culture grown in the MEC can break down almost all the fermentation products. The team has also identified homoacetogenic bacteria growth as the likely cause of the significant acetate remaining and has started considering methods to suppress this activity.
- Developing a hybrid nonprecious metal (MoP) electrocatalyst for MEC with performance similar to Pt is a major accomplishment, as is the identification of a bacterial culture capable of producing hydrogen from all major sugars. Further, using mixed sugars, the fermentative hydrogen production rate of 8–10 liters of hydrogen per liter of biomass per day over an 80-day period shows noticeable progress towards the 24 liters expected from the hybrid system. However, the ultimate challenge is production from mixed waste, which has yet to be tried.
- The progress of the project is within what is expected for fiscal year (FY) 2016–FY 2017. Progress includes the optimization of fermentation and the MEC cathode development. The steps scheduled for FY 2017 are also in progress, with some results already presented. Some of the challenges listed in 2016, such as inhibition of methanogenesis, were also overcome.
- This is the first year that this project has been reviewed, and the progress in that year has been strong. The research team has demonstrated good progress on all of the stated tasks and has a clear path forward.

Question 3: Collaboration and coordination with other institutions

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

- The project shows significant collaboration, both with other groups at Oregon State University (OSU) for biomass treatment and microbial community characterization, and with two laboratory staff at Pacific Northwest National Laboratory (PNNL) for work on the MEC cathode and cost analysis.
- Collaboration and coordination between OSU, PNNL, and Oregon Nanoscience and Microtechnologies Institute are clear and effective.
- Coordination is evident between partners. All collaborators seem well suited to their divisions of the work.
- The collaboration includes three of the major entities: a university, national laboratories, and DOE. It would be beneficial at some point to have industry input on the commercial feasibility of the technology.

- There seem to be strong collaborations in place.

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.

- As the PI noted in the presentation, the project has set ambitious targets. If successful, this would result in a pathway that could meet the DOE cost goals while using renewable biomass feedstocks; if the wastewater feedstock could be used, the analysis indicates that the ability to treat wastewater could be an added value equivalent to $10 per kilogram of hydrogen and open the system to applications in food processors and similar industries that generate and have to pay for water treatment. The project's achievements in developing the different components of the hybrid system indicate that there will be progress toward the targets, though the full impact will not be clear until the demonstration reactor is designed and tested and a full cost model developed.
- The project aims seem to be well aligned with the DOE goals. There are several advances that have been made that aim to move toward scaling up the process.
- This project meets the cost and performance requirements of the Hydrogen and Fuel Cells Program. A number of advances have been made that may also be applicable beyond this work.
- This work has direct, real-world applicability.

Question 5: Proposed future work

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

- There is a clear path to keep addressing the barriers and challenges. It is important to obtain the stability of the cathode catalyst performance and to obtain stability of the hybrid reactor in continuous operation, which may be the main challenge of this project.
- The optimization of the individual steps, followed by building of the hybrid system, appears to be a logical set of next steps. However, the researchers should probably consider optimizing the bacteria and electrocatalyst for converting waste to hydrogen prior to building the hybrid system.
- The information about future plans is limited, but the plans are logical extensions of the current work. The project has identified barriers to the work and has noted plans to address these.
- The proposed MEC optimization, the hybrid system evaluation, and the model of costs seem reasonable for this project. It is important to test the long-term stability of hydrogen production through the hybrid system.
- The presenter responded well to questions in the room. The presenter was aware of possible concerns with the work and had a logical path to address the concerns.

Project strengths:

- The project provided particularly strong support for cost assumptions for the wastewater feedstock, based on data from 19 communities and interviews with 17 food and beverage companies to provide clear support for the credit for wastewater treatment. The work on different aspects of the system, for example, utilization of sugars or impact of flow rate, is aimed not only at improving the performance in isolation but also in gathering data to feed into the system and operation parameter analysis and design, increasing the likelihood that the hybrid reactor will be successful.
- One of the main project strengths is the approach to directly reducing the hydrogen production cost, such as the use of mixed bacterial culture as inoculum, which greatly simplifies the overall operating process, making it more feasible to scale up the system. In addition to that, the use of low-cost feedstock and the study of low-cost cathodes enable the use of MEC as an additional process to improve final hydrogen yield.
- The project is technically well designed and logically planned, and there is excellent expertise from the partners. Progress has been made in system design and analysis, as well as in investigations and consideration of designs and innovations that combine the strengths of dark fermentation and MEC processes. The tests conducted using mixed culture are also great strengths of the project. The use of mixed

culture is a major advance for the reduction of costs in the production of biohydrogen, since it allows the use of several sources of wastes that do not require the sterilization step.
- Strengths include real-world applicability (use of wastewater) and research output that could have medium to high impact on the field.
- The progress with the fermentative hydrogen production from multiple sugars using a single bacteria proves the concept is promising.

Project weaknesses:

- No weaknesses are noted at this time. The concerns that were raised during the question-and-answer session were already known to the PI, and there was a logical path to address the concerns.
- There are no apparent project weaknesses at this stage.
- Some of the information on the Hydrogen Analysis (H2A) analysis is confusing, particularly all the different options tested, making some comparisons difficult. In particular, the tornado chart assumptions do not seem to match any of the scenarios shown in the bar charts. This is at least partly because of the large number of options being modeled. The wastewater option is interesting, but few data are provided related to it.
- The identified weak point is very timely and refers to the demonstration of the fermentative hydrogen production stability results, which should be better presented with a larger number of production data.
- Rigorous MEC electrocatalyst stability work needs to be done.

Recommendations for additions/deletions to project scope:

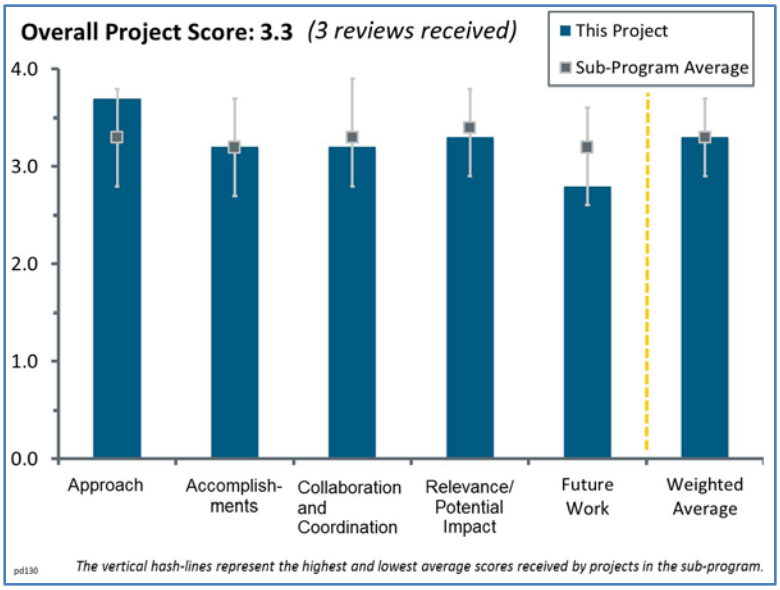
- The project covers all the important aspects to be considered in a low-cost hydrogen production target project: technical and operational improvement of engineered systems for hydrogen production from low-cost feedstocks; the use of low-cost and renewable feedstocks, such as biomass and wastewaters; the mixed bacterial culture performance optimization; and cost performance modeling. No addition or deletion to the project scope is recommended. However, there is a strong recommendation: to obtain experimental data of continuous hydrogen production using the actual feedstocks and to stabilize/optimize the hybrid system using the actual feedstock instead of synthetic wastewaters. This brings reliability and robustness to the project.
- The project is now on the right track. It would be important to aggregate other partners to conduct further system testing using other sources of waste.
- Clarification is needed of what, if any, work will be done using wastewater feedstocks.
- It would be good to test the robustness of the individual stages by using waste as the feedstock.

Project #PD-130: Improved Hydrogen Liquefaction through Heisenberg Vortex Separation of Para- and Orthohydrogen

Christopher Ainscough; National Renewable Energy Laboratory

Brief Summary of Project:

This project aims to utilize the endothermic conversion of hydrogen between isomers (para vs. ortho) to aid in cooling hydrogen. Researchers will develop vortex tubes to aid in hydrogen liquefaction, moving them from Technology Readiness Level (TRL) 2 to TRL 4 such that the technology can be commercialized to units that are 5–30 metric tons per day (MTPD) in size. Exothermic ortho–para conversion results in significant refrigerant use, whereas the vortex concept leverages catalysts for reverse endothermic reaction. The endothermic reaction is expected to cause bulk cooling of the mixture. This concept is expected to improve liquefaction efficiency by >40% by minimizing refrigerant use.



Question 1: Approach to performing the work

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

- Focusing on the vortex fundamental principles is the best approach. The team is using both experimental and simulation tools to that end. Once understood, the vortex needs to be optimized for this application, and the team seems to be doing just that. Looking at the integration of the vortex in the plant is critical too. The project is doing a good job.
- This project has a clear academic flavor. The thorough literature/experience history search, combined with detailed fluid dynamic/thermal analysis, including the development of relevant equations of state (EOS) is refreshing. This is excellent.
- The activity is logically structured, with relevant milestones and a clearly identified methodology. The vortex technology is a geometry-based thermofluidic device that can be complicated as it scales. A more clear identification of the actual technique for increasing the technology scale would be helpful. The activity expressly named partner organizations and identified a range of potential entities with which it could collaborate. However, no other formal U.S. Department of Energy program was explicitly identified as a customer or future potential partner. Based on the early presentation timeslot and answers provided to audience questions, it is likely that this was an oversight.

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.

- An unanticipated “near-miss” event generated an atypically long facility delay, but not an unreasonable one. The steps taken during this schedule shift reduce the risk for future facility-related delays on any project using this facility. Once the facility issues were resolved, the activity demonstrated proof of concept and clearly identified the design parameters necessary to drive the next stage of development.

- It looks like a fair amount of progress was made, especially regarding measurements of pressure ratio and outlet temperatures under different circumstances. Task 2 seems to be completed. It is not clear how the computational fluid dynamics work was used for fiscal year 2017, nor is it evident how the results on slides 19 and 20 were computed. The project made a good effort toward trying to quantify the performance regarding DOE targets (slides 21 and 22). However, the numbers do not match up exactly between the two slides. Also, the numbers on slide 21 are confusing: a value commonly accepted for work of liquefaction is 12 kWh/kg, assuming plant inefficiencies and all (ideal work is ~4 kWh/kg). However, the author claims numbers between 20.4 and 30.8 kWh/kg for the Linde–Hampson (L-H) cycle; these values look as if they were artificially "bumped." It seems the L-H cycle is more inefficient than a Claude cycle, although it is not clear that this is so. Perhaps the project could look at integrating the vortex in a Claude cycle and see the comparison with the baseline. On slide 21, the author wrote, "Vortex tube…decreased the work of liquefaction by ~25%." It would be helpful if the two numbers used to support this claim (perhaps 30.8 to 23.7 kWh/kg) were highlighted. It is unclear why only 16.7 kWh/kgLH_2 and 7.4-12 kWh/kgLH_2 are used on slide 22. The project did a good job on slide 25.
- The accomplishments and progress suffered from the power outage experienced by this project. The power outage was a random event for this project; however, regarding the response of the equipment to such an event, the effects can be mitigated by planning with a focus on safety—as evidenced by the suggestions of the Hydrogen Safety Panel and subsequent system modifications. This should have been done before the experiment was initiated.

Question 3: Collaboration and coordination with other institutions

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

- This presentation cited informational sources well and clearly gave credit where it was due. While this particular activity formally lists few collaborators, the interplay between the multiple presenters in the room and answers provided to reviewer and audience inquiries strongly suggest an informal but strong collaboration among many entities in this technical area.
- The collaborators make up the essential talent and guidance needed to make this project relevant to the needs of the liquefaction industry and to potential end users.
- The relationship with Washington State University (WSU) seems to be working well, although it looks like the National Renewable Energy Laboratory's and WSU's tasks are complementary and do not rely on each other. Praxair's role is not clear; it seems possible that Praxair is giving directions as to where the research is going. Not enough information was given on the quality of the collaboration(s); hence, this category earns a "satisfactory" grade.

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.

- Assuming this project is successful in meeting its performance goals (and it is meeting or exceeding the goals), this technology will make a significant impact on cost and reliability of liquefaction systems.
- Reducing the system-level energy to liquefy hydrogen for general consumption is strongly in line with the Multi-Year Research, Development, and Demonstration Plan. Eliminating rotating components reduces parasitic power, increases system reliability, and reduces leak rates. The path to scaling up to meet the stated 5–50 MTPD is poorly communicated by this activity. The actual work of scaling up is clearly identified as a future activity, but having at least a conceptual plan would illustrate the team's thought process.
- The project supports the Fuel Cell Technologies Office goals and objectives by trying to improve the efficiency of liquefaction. The objectives for the life of the project are clear (to increase TRL), but the objectives for the current year are not mentioned, although this was required in the presentation template. The impact on the barrier is not mentioned either.

Question 5: Proposed future work

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

- Proposed work is well-thought-out and will keep this project on a track for success.
- The future work represents a logically laid-out plan of action. One micro-nit would be to include a tolerance on empirically validating calculations demonstrating "endothermic para/ortho conversion of [>] 5% of a stream." Depending on the labor distribution within the activity, it may be feasible to simultaneously pursue the empirical validation and portions of the technoeconomic analysis.
- The proposed Future Work slide is identical to last year's. Although the four-month delay is understandable, the duplicate future work is troublesome—and disappointing if it is not a typographical error. The same remark applies to the Summary slide and the Remaining Challenges and Barriers slide.

Project strengths:

- It is interesting to see how the concept has evolved from being based solely on the vortex tube (VT) technology to being integrated in existing L-H and Claude cycle-based designs. The characterization of the vortex through experimental and numerical investigation is appreciated. The project has done great work on developing the EOS for the refrigerant. The presenter has great oral presentation skills.
- The team's technical competence is the dominant advantage of this activity. All activities are clearly communicated and apparently laid out in a logical progression. The network supporting this area of research illustrates active collaboration among multiple projects across agencies and countries.
- The careful establishment of previous work (literature and hardware) is a strength, and careful development of models (fluid dynamic and thermal [EOS and transport]) to guide the development is very good.

Project weaknesses:

- There is no real weakness.
- There were difficulties presenting the results and relevance in a useful and clear manner. This makes it very challenging for the reviewer to evaluate the project's progress and relevance, and the evaluation then leans toward negative. For example:
    - On the summary slide, a claim such as "Increase efficiency" in "Relevance" does not appear to be accurate anymore (slide 22 shows lower efficiency).
    - A claim such as "world's leading researchers" in "Approach" may be a bit of a stretch, especially since this claim is not backed up throughout the presentation (no publications, no technology transfer other than a two-year-old patent application, etc.) and does not carry any information for the project in question (and to be honest, this claim could be made by 80% of the projects covered in the DOE Hydrogen and Fuel Cells Program Annual Merit Review [AMR]).
    - The "Accomplishment" claim is very vague. Besides, it may be the House of Quality and a proven concept (although both were already achieved last year).
    - The "Collaborations" claim is not backed up by any mentions throughout the presentation.
    - The "Future Work" is very vague and could be applied to 100% of AMR projects. In addition, this slide is 100% identical to last year's.
    - Slide 12 contains a good deal of information, but it is not clear where we want to be to have a successful working technology.
    - Slide 18 shows how to integrate the VT in the liquefaction plant, but it is not clear what the VT is replacing and what performance the VT should have (upstream pressure, outlet cold and hot temperatures) for this integration to be successful.
    - The last five slides of the presentation were 100% identical to last year's.
- Weaknesses include the impact on the system level from a unit cost basis. Range was mentioned as a key discriminator for the eventual system but ignored within this particular segment of the activity. It would be helpful to elucidate system parameter sensitivities later in the activity.

Recommendations for additions/deletions to project scope:

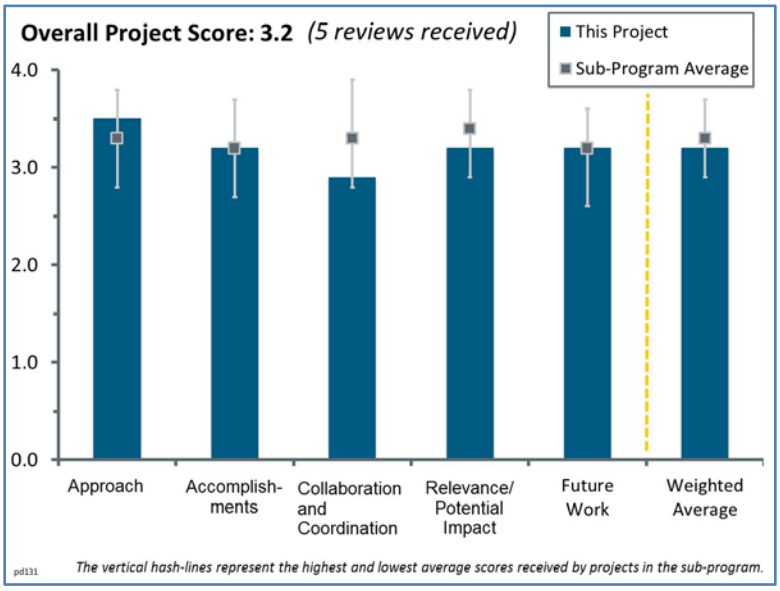
- The project should focus on the VT technology itself: how to improve the ΔT and what performances are needed to use this technology in a liquefaction plant. The project should also be boiled down to what the vortex technology replaces in a liquefaction plant and how it compares in terms of inlet and outlet pressure, temperature, and flow rates. It looks like the nationwide technoeconomic trade study for optimal placement has been dropped, which is great if it is the case. No more work on ternary refrigerants should be done at this point. The project should focus on Tasks 1 and 3 only. There is no need to talk much about para–ortho (cf. slide 5) since no para–ortho conversion is shown throughout the result, only speculated. The conversion would be a nice benefit, but it looks like wishful thinking at this stage. Although the presenter does a great job in front of an audience, the slide deck does not do justice to the project (confusing numbers, complicated figures [such as on slide 10] with no lines or legend for acronyms, lack of big picture, copy/paste from the last AMR, etc.). Much improvement is expected in that area for the next AMR.
- While it is tempting to recommend additional scope to this particular activity, the success to date results from the activity management and the development pace. Rather than risk upsetting the apple cart, no scope change is recommended.

Project #PD-131: Magnetocaloric Hydrogen Liquefaction

Jamie Holladay; Pacific Northwest National Laboratory

Brief Summary of Project:

The Pacific Northwest National Laboratory (PNNL) magnetocaloric hydrogen liquefaction system is expected to be considerably more energy-efficient than the Claude cycle. At 30 tonnes per day (tpd), the latter shows 40% efficiency, while the former is projected to be 70%–80% efficient. In this project, investigators will demonstrate the PNNL system liquefying ~25 kg/day. At industrial scales, the concept is expected to have a figure of merit (FOM) >0.5 (as compared to the Claude cycle system's FOM of <0.3). The project will also identify a pathway to a larger-scale system with an installed capital cost of less than $70 million.



Question 1: Approach to performing the work

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

- This project is on its second year, and the principal investigator has clearly demonstrated a strong focus on the barriers needed to be successful. The key approach is to increase efficiency by using bypass flow (which reduces ΔT in the process heat exchanger) and layered materials in the hope of increasing the FOM from <0.4 to >0.75. The U.S. Department of Energy target is 0.5.
- The approach appears to address all of the critical barriers in a methodical way. The project team has taken lessons learned from earlier phases of the project and acted on them in an organized fashion. The project team adequately makes attempts to predict future barriers and prepares for alternate approaches. The project has taken the necessary steps and applied resources to increase project scope when it is critical to success, e.g., atomized sphere generation.
- Although this process is in the early stages of development and more complex than existing processes, the work is well-thought-out, and the approach seems to be as practical as possible.
- This is a great incremental approach with high potential. The topic is highly complicated. The targets are ambitious.
- No doubt the team is working very hard to prove the concept and overcome barriers, but the presentation gave the sense of a wide gap between laboratory activity and the end-game objective of having a technology that delivers the product in a competitive manner as measured in terms of capital and energy consumption.

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 project progress is impressive and clearly demonstrates targets and pathways toward achieving those targets. The project has successfully addressed setbacks and recommendations from earlier project phases.
- Given the complexity and some of the hurdles faced (e.g., powder production techniques for spheres), the project seems to be on track and moving toward the established goals.

- The accomplishments and progress toward the overall project and DOE goals are good, but the project is behind schedule. The milestones for the first quarter (Q1) are at only 75%. The quality of the work is high, and the investigators are on the right path. The presenter claims the team will be able to get back on track with respect to the schedule, but a strong case was not presented that they would be able to.
- The team came up with an interesting layered design, identified materials, and validated their model. Milestone progress on slide 7 is disappointing, but it has to be balanced with the fact that the slides were due in April (2017 Q2). Reports on milestone progress since the last time the slides were due (April 2016—i.e., fiscal year [FY] 2016 Q2, Q3, and Q4 and FY 2017 Q1 and possibly Q2) are needed for better progress assessment.
- The team members are extremely impressed with their accomplishments. The volume of data presented was awe-inspiring; however, it was not clear how this data demonstrated progress to an end goal as measured in energy consumption and specific capital (dollars per tonne). The 25-page presentation covers magnets, particles, heat capacity, etc. but fails to bring it together to show that the technology is progressing to the project's original goals.

Question 3: Collaboration and coordination with other institutions

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

- Collaborations with Ames Laboratory and Emerald Energy NW (Emerald) look strong, as do interactions with potential future partners and patent applications.
- The project team has reached out to outside institutions. As the technology readiness level increases, this will become increasingly important, as the project team has recognized in their presentation. That said, the project has progressed nicely with the current level of coordination with other institutions.
- This is one area in which the project could benefit from more industrial interactions. The project is mostly interacting with other national laboratories. This was a comment from the previous year and was addressed in the comment section, but the project needs more collaborations than just Emerald. The investigators should continue their discussions with several industry groups that expressed interest, including those that are interested in magnetocaloric hydrogen liquefier (MCHL) systems.
- This area is very weak. Emerald is the concept originator and current consultant. Ames Laboratory is working on materials, and PNNL is doing shape separation. None of these partners serves to challenge the project or assess its commercial value. If funding continues, DOE needs to ensure the concept is exposed to critical thinking, commercial evaluation, and alignment with the original project goals.
- Perhaps the team should explore other metal powder production processes (e.g., hot gas atomization). It is not clear if the spinning disk process is the most practical.

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.

- This project is focused on addressing the barrier of "High-Cost Low Energy Efficiency of Hydrogen Liquefaction." The project supports and advances progress toward the Hydrogen and Fuel Cells Program goals and objectives delineated in the Multi-Year Research, Development, and Demonstration Plan (MYRDDP). The only issue is that the DOE efficiency target for 30 tpd (for a small facility) is 85%, and this effort will get you to 70%–80%. However, the installed capital cost, operations and maintenance cost, and energy input numbers for this technology are very favorable and potentially could make this technology very successful.
- The potential to have an impact on MYRDDP targets is tremendous. Measuring the degree of impact remains difficult to predict; however, future work (laboratory demonstrations on the kilogram-per-day level) will shed more light on proposed capital, operating, and energy cost reduction for hydrogen liquefaction.

- Low-cost hydrogen liquefaction is a key to expanding hydrogen and fuel cell adoption to achieve scale. The efficiency goals, if achieved, will drive liquid hydrogen (LH_2) costs down if the capital expenditure does not negate the efficiency benefits.
- The project is very relevant to addressing the high cost of liquefaction. Questions remain concerning the claimed benefits (huge if accurate), given that the application relies heavily on very specific materials.
- The presenter was not able to advise on the forecasted energy requirement to liquefy a kilogram of hydrogen. This metric left the impression that the project is a science experiment, rather than an early-stage concept that has commercialization potential.

Question 5: Proposed future work

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

- The project has highly effective planning, impressive progress, and logical approaches. The project is extremely complex, and the accomplishments to date, given the funding level, are impressive.
- The proposed future work is very much focused on the final target (i.e., demonstrating hydrogen liquefaction).
- This is very complex project with many moving parts. It would have been good to see a table with all the key assumptions and how risk is being mitigated for each step. It is true that the investigators have focused this year on the demonstration of two critical aspects—bypass operation and multilayer design—but next year, more focus is required: refining the third generation (GEN III) design, designing/building/testing the GEN III system, characterizing the active magnetic regenerator liquefier (AMRL) for 1–25 kg/day LH_2, documenting the AMRL, and evaluating at least two non-rare-earth-based magnetocaloric materials and completing a cost analysis. These are complex tasks, and the future milestones planned for FY 2018 were not shown.
- Based on slide 22, the project wants to demonstrate some theoretical accomplishments (eight-layer operation) but is having a hard time translating activity to a real end game. A clear objective of work in 2017 is to determine whether the concept has potential to liquefy hydrogen and to determine the specific energy to do this. If it is a long shot, the project should focus on another objective that may be more attractive.
- The team needs to continue to address the impact of scale-up on the mechanical component reliability and maintenance costs. The sealing system should be a key focus. Some benchmarking should be performed on extrapolating the reliability and maintenance data gathered in the GEN III system versus current liquefaction plants.

Project strengths:

- This project has many strengths:
    - A principal investigator who is very strong in this field
    - Interactions with other researchers
    - An innovative approach and concept
    - An approach involving the elimination/reduction of the intrinsic irreversibility of the active magnetic regenerator cycle of magnetic refrigerants
    - 88% reduction in magnetic material
    - Increasing the FOM from <0.4 to >0.75
    - An approach involving characterizing Cp, Tc, and magnetic moment for each material
- The project addresses LH_2 production efficiency improvements using the bypass method, which seems compelling. The methodology and testing leverage some previous work on liquid petroleum gas. There seems to be a nice synergy with liquefied natural gas (LNG) to reduce capital expenditure. The cost benefits of a combined production facility—steam methane reforming ---> LH_2 and LNG—should be explored further.
- The technology has high potential and may be the only candidate susceptible to disrupting the state of the art. The project is very relevant to DOE's portfolio—and the rest of the hydrogen community, for that matter. The team is highly skilled, with great expertise in the process and the materials.

- Strengths include the project's potential, innovativeness, and the project approach in terms of both basic science and engineering.
- The team members are passionate.

Project weaknesses:

- The project would benefit from improved presentation skills. Granted that this is a complicated topic with many results, but an effort should be made to convey the technology, the challenges, and the progress to a non-MCHL audience. For example:
    - Slide 6 has too many layers of text.
    - Slide 8 has no takeaway message. It makes sense that heat capacity is temperature-dependent, but it is not clear whether the presentation is saying that low fields are good or bad. A few words on Curie temperature would also help.
    - Slide 9 is too wordy. Maybe a drawing would help.
    - On Slide 10, the equations may be acceptable, but the takeaway message is that the model was updated, which is not obvious from the slide.
    - It is not clear whether Slide 11 shows the temperature of the fluid or the magnets. There is no need for such a long legend, and no need to repeat the dotted line vs. the continuous line.

  Also, much of the work has been done in the high-temperature region, but surprises may show up in the sub-100 K zone. Lastly, some background about availability of materials is lacking.
- This is very complex project, with many moving parts and many key assumptions. It would be good to see a list of all of these key assumptions, either verifying them or confirming their sensitivity to the targets as being low and not on the critical path.
- It is not clear that the team has identified and addressed all the issues encountered with the Prometheus design. It is not certain that the powder production process is practical at scale. Other more common metal powder processes (e.g., inert and hot gas atomization and plasma atomization) have seemingly not been explored.
- Weaknesses include understanding scale-up and determining market potential/commercialization (more of a challenge than a weakness).
- It is not obvious that the team members can determine whether the project has any LH_2 value, nor that they really want to know that answer.

Recommendations for additions/deletions to project scope:

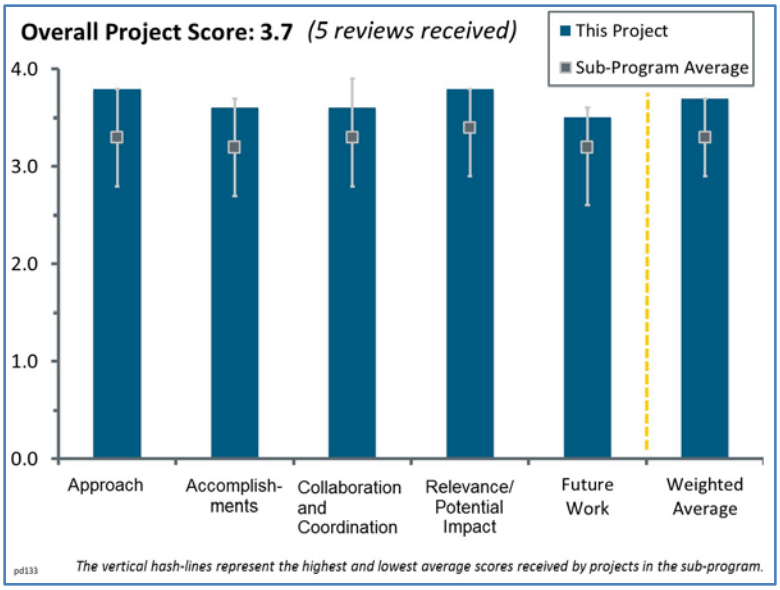
- There are no recommendations for changes to the project scope. This is excellent work.
- This is a very complex project with many moving parts. It would have been good to see a table showing all the key assumptions and how risk is being mitigated for each step. It is true that the investigators have focused this year on the demonstration of two critical aspects—bypass operation and multilayer design—but next year, more focus is required with respect to major steps. If the project is not able to meet its milestones for FY 2017, then the project's focus and scope should be reduced to address the challenges that are slowing the project down. This is a strong project overall.
- Milestone progress since the last time the slides were due (April 2016) should be shown. The project should try to simplify the slides for a stronger message. The presentation should give information about the availability of materials:
    - Which industry is typically using them today
    - How much they cost in dollars per pound today
    - How many pounds of materials are needed for an X-tpd liquefaction plant
    - Among the 10 different materials, which one is the most rare/expensive
    - Which of the alloys are already available today
    - Which alloys quickly reach <100 K conditions
- If funding continues, the project needs to include real partners to critique the technology, performance, and potential.
- The investigators should determine the importance of identifying the right magnetic powder production process and its impact on costs and operation.

Project #PD-133: Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) – Consolidation

Christopher Ainscough; National Renewable Energy Laboratory

Brief Summary of Project:

This project aims to reduce at-the-pump hydrogen prices by decreasing the cost contribution of fueling station capital, specifically compression costs. Currently, compressors for large stations can cost up to $1 million. The project is designing and demonstrating a hydrogen station based on a tube–trailer consolidation concept that will increase compressor throughput and reduce compressor size.



Question 1: Approach to performing the work

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

- The approach to this "consolidation algorithm" is well designed, feasible, and integrated with other efforts. The approach includes the cost of compressors and their influence on overall station costs. A number of partners participated (companies that deliver hydrogen using tube trailers are needed). The set-up is extensive. The approach addresses how to succeed at peak fueling by keeping some of the pressure at the tube trailers. The approach used is to develop an algorithm to reduce the contribution of compression to the overall hydrogen cost per kilogram, in this case by 50%. The mean time between failures (MTBF) is increased, and back-to-back fills are maximized. A smaller compressor can be used. The analysis shows how a four- to five-time increase in the number of complete vehicle fills is possible. Achieving the SAE International J2160 standard's T40 (fuel delivery down to -40°C) rating is a goal of five 4 kg fills. The Hydrogen Infrastructure Testing and Research Facility (HITRF) is used and set up for two-way flow of hydrogen to/from storage tanks at three pressure levels. The hope is to make this algorithm implementable in everyday retail hydrogen stations. The approach is to allow the station compressor to deliver high throughput during peak refueling demand by supplying the compressor with a high-pressure stream of hydrogen from the storage tubes (consolidated during off hours). The hydrogen in the tubes of a tube trailer (during off-peak hours) can restore the high-pressure supply to the compressor, improving the refueling capacity during peak demand hours. The approach, after the work is refined/done, should be rolled out into the existing hydrogen refueling stations; the approach uses existing hydrogen refueling station equipment.
- This project has a good balance of analysis/modeling, along with the field testing. In a relatively short project, the researchers have made impressive progress in each area. This is the type of project that is most useful to the industry in that it produces relevant results in a timely fashion and demonstrates a usable technology that can be broadly applied by gas suppliers, station owners, component developers, etc.
- This project is the experimental validation of a consolidation algorithm/scheme developed by Argonne National Laboratory (ANL) (Amgad Elgowainy, specifically). This is important to note. The approach for this experimental validation is good, particularly the state of the experimental development. While it will not really affect the overall findings from this work, it is a bit surprising that no energy balance on this overall process has been performed. Unless heat is transferred out of the cascade tanks used to simulate the vehicle, the tanks will heat up from cycle to cycle. It would be good to see an energy balance analysis performed and/or temperature monitoring of the "vehicle" tank simulators. Should this be an issue, it will likely not have a significant influence on the overall conclusions of this work; hence this issue was not sufficient to significantly "ding" this project.

- The approach has been excellent, with the National Renewable Energy Laboratory (NREL), ANL, and PDC Machines (PDC) all contributing to the conceptualization and the method of demonstrating the benefits of the concept.
- As usual, the first steps are to question the assumptions. It was surprising to see a cost of $4.5 million for a reformer skid for 300 kg/day. In the 1990s, United Technologies Corporation was building phosphoric acid fuel cell power plants that generated 15 parts per billion (ppb) of hydrogen (~160 kg/hr) with a clean-up of CO down to 0.5%. The total power plant sold for $0.6 million. The reformer and gas shift reactor together ran about $75,000. The project team should speak with Doug Wheeler about the United Technologies Corporation PC25 fuel cell. Tailoring the duty cycle with a smaller compressor that is active more frequently is one solution. However, MTBF may become an issue. This is often more dependent on run time than on the number of start/stop cycles. Another option might be to generate a higher-efficiency, higher-MTBF, and lower-capital-expenditure (capex) design. The polymer electrolyte membrane (PEM) electrochemical design is intriguing. The PEM concept matches steam methane reforming and electrolyzers (PEM and alkaline). The pump is also a filter for all but water, and water is easier to remove at higher pressures. This project is not novel but is demonstrating the idea of expanding the boundaries of thought at the national laboratories while supplying PDC with a test stand.

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 has produced the following accomplishments:
    - Equipment is installed.
    - Station upgrades are complete.
    - Buffer storage requirements have been established.
    - Baseline and consolidation tests have been started.
    - Testing has started on the quantity of possible tank fills, evaluating the state of charge after the fills are complete, documenting the station storage pressures, and determining the energy use of the compressor.
    - The vehicle simulator is assembled and working.
    - Work has been started to determine the consolidation operation and how the station capacity can be extended.
- This project has been underway for about 1.5 years. Given that at this point the project is still in an experimental build-out, which is a time-consuming, difficult task, the accomplishments are quite impressive. Building a high-precision experimental facility to produce high-quality data is not an easy task. The project is doing a good job.
- To upgrade the test platform, install and test the new compressor, and validate the models in the short period of project time is impressive. It would be good to see the project continue beyond its end in that it is not certain that all of the cost and reliability issues will have been adequately addressed by the project's conclusion. It would appear that there is considerable potential value to be gained in the project by continuing to look into this topic.
- The project team has made good progress after some procurement delays and some unanticipated hospitalization of a key contributor because of some long-term back problems. The testing system is ready to go.
- The rationale of trading compressor size with storage size is interesting. The key point being missed is land cost as a key capex entry. This also needs to be factored into the cost. It is unclear whether doubling the footprint and doubling the storage hardware will offset the cost of a second compressor.

Question 3: Collaboration and coordination with other institutions

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

- This project is the experimental verification of modeling and algorithm development from ANL. The collaborators are ANL (very appropriate) and a well-established compressor firm that is well known for exceptional efforts and unique compressor hardware. Collaborations are very appropriate—this is excellent.
- The collaboration between NREL, ANL, and PDC has been excellent, with all parties contributing to realizing the concept of consolidation and developing the test method to demonstrate the benefits of the concept.
- The project has a good mix of modeling and experimental work. The compressor partner is obviously key to project success. The project would be strengthened by adding a tube trailer supplier or (probably better) a station owner or industrial gas company with experience in managing stations and trailers.
- It is not certain that partnering with only one other national laboratory is enough. Further, partnering with a subcontractor can be an echo chamber. Perhaps the project could bring in other members of the H2FIRST organization. The presenter mentioned that optimization of the algorithm will occur after collecting test data. There are plans to integrate the companies that deliver hydrogen by tube trailer.
- Collaboration and coordination is typical of this type of simple exercise.

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.

- This project shows how station developers/operators can optimize existing hydrogen refueling station hardware. Potentially, station operation can extend the station capacity for full vehicle fills (with back-to-back capability) by a factor of 400%–500%. The relevance and potential impact are enormous since some retail hydrogen refueling stations require truck fills throughout the day since the station nameplate capacity is exceeded by the demand from the cars. Station operators think they need station upgrades that can cost as much as an original station, but if what is needed is a "consolidation," that certainly must be less expensive.
- Presuming that this algorithm works as intended, it will have significant impact on compressor reliability and overall throughput size, which goes to cost. This is elemental, and the impact is very high.
- Station owners see direct value in this evaluation and will be able to apply the learnings to both existing stations and the design of future stations.
- This concept will enable the lowest possible cost of compressed delivery to fueling hydrogen vehicle fueling stations.
- This project should have some impact on the DOE goals.

Question 5: Proposed future work

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

- Although this project has ended, the speaker/author explained the need for more future work in this area. Specifically, there is a need to integrate companies that deliver hydrogen using tube trailers into this work and to optimize the algorithm. This project shows promise for the refueling industry.
- Proposed work is very relevant to the goal of this work. The project will finish the experimental hardware build-out and run the experiment. This is exactly what is expected.
- The future work is well planned, and the team has been developing many other parts of the test system to adequately test the system over the remaining months in fiscal year 2017.
- The value of the project is to be determined by the successful testing over the coming months, so a successful conclusion to the project is happily anticipated. At the presentation, a reviewer commented on taking steps to ensure that the vehicle simulator is able to meet the SAE International specifications for repeat fills in ambient tanks. This is an important step to ensure. Very little was presented on how the costs and reliability would be determined for the various operating scenarios, yet this is where the value in the

project resides. Based on the state of the project and results to date, it seems likely that there will not be sufficient time in this project to fully evaluate these parameters and that this evaluation is left as an important area for further future project work.
- The future work is to follow and complete the construction schedule.

Project strengths:

- The strengths of the project team include:
    - The concept of demonstrating the benefits of consolidation (ANL and PDC)
    - The PDC team that has developed the tri-mode compressor (Stage 1, Stage 2, or both stages at the same time)
    - The concept of how and where to do the demonstration (NREL)
    - The development of the sequential fueling "vehicle simulator"
    - The test plan
    - Preparation of the NREL HITRF facility with an upgraded dispenser chiller block and larger cooling machine
    - The project team
- Among the project strengths are the following:
    - The project uses existing equipment at a hydrogen refueling station in a new way.
    - The high capital and operating costs of hydrogen refueling station components (the compressor) may decrease as a result of this work.
    - The project is grounded in the supervisory control and data acquisition (SCADA) system at the HITRF.
    - The self-contained system that simulates vehicles is unique, needed, and can perhaps be used on a fee-for-use basis for station developers/operators to conduct tests.
    - Finally, the author has the motivation to refine and optimize the consolidation algorithm once this project ends (September 2017).
- Project strengths include the balance of analysis/modeling and experimental work. Also, the project has made impressive progress on the test location and the compressor design/build/implementation.
- This is the experimental verification of ANL work. This is well-thought-out and appropriate for the desired goal. This experimental team and NREL test facility are well suited to perform this work.
- Strengths include the project's simplicity and the presenter's skill; Mr. Turlip gave an excellent presentation.

Project weaknesses:

- At this stage of the project, no significant weaknesses were identified, with the exception of a missing energy balance to determine whether the system can indeed be operated for an infinite number of fills (whether the tanks heat up over time, which is a small issue).
- There are no obvious project weaknesses.
- The weaknesses of this project were the long time for contractual negotiation and some production delays at the compressor manufacturer, which leads to a shortened test program this summer.
- To date, there has not been enough emphasis on evaluating the effects on cost and reliability of the systems. The project needs a partner with experience in managing tube trailers.
- This project does not advance the art; it validates a station optimization of a compressor. The cost does not properly address capex.

Recommendations for additions/deletions to project scope:

- Reliability and cost considerations need to balance capex versus complexity. It is not obvious that the net result of this process can achieve the 50% cost savings mentioned. Cost savings are primarily in the reduced size of the compressor, but it is not clear that this balances the added cost due to complexity of valving and flow path additions. The cost impact on cycling the tubes more often (hence shortening their lifetime) is unknown. Reliability issues have similar concerns. While we may experience better reliability

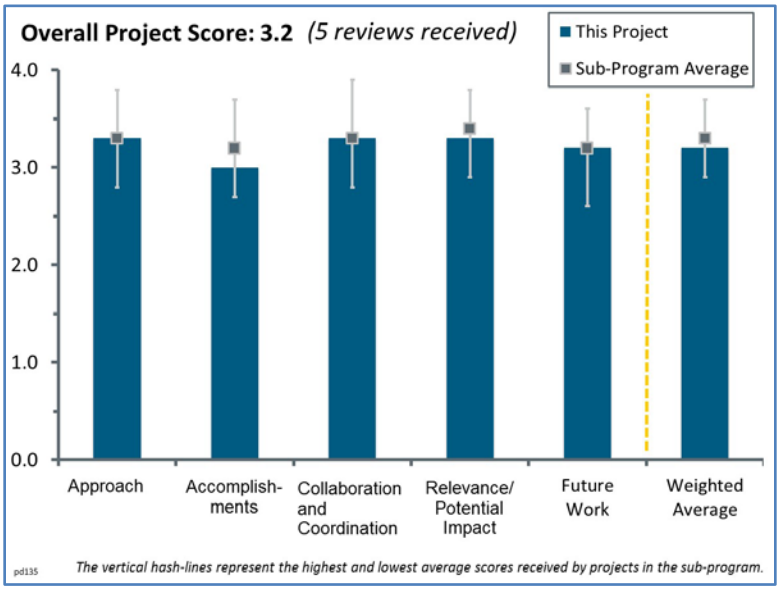
on the compressor, all of the valves and flow path complexity must also be considered. It is not clear that the system overall is more reliable; perhaps compressor reliability has been traded for system reliability.

- The project should perform an energy balance over many "vehicle" fill cycles to determine the temperature time history of the "vehicle" fill tanks.
- The cost estimate should be reworked.
- This project ends September 2017, so there will be no more additions to the existing project scope.

Project #PD-135: Liquid Hydrogen Infrastructure Analysis

Guillaume Petitpas; Lawrence Livermore National Laboratory

Brief Summary of Project:



Liquid hydrogen has many benefits for the hydrogen infrastructure, especially at large scale. Two main technical barriers to using liquid hydrogen were addressed in the previous year: (1) "lack of hydrogen and infrastructure options analysis" and (2) "reliability and costs of hydrogen pumping." To accomplish this, the project simulated the liquid hydrogen pathway (from liquefaction plant to end use) using a thermodynamic model to estimate, then mitigate, the transfer and boil-off losses that occur. Real-life driving and parking data were collected from a large population to use as an input for the model. The project also identified the major hydrogen boil-off sources and investigated potential recovery technologies/processes (technical and cost) to eliminate/reduce these losses.

Question 1: Approach to performing the work

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

- Lawrence Livermore National Laboratory (LLNL) and partners have a well-thought-out approach to evaluating boil-off losses. The approach is comprehensive with respect to evaluating every transfer point. Simulation is a low-cost option to assess losses at both stations and onboard vehicles. Task 3 covers boil-off recovery and other mitigation options. The entire pathway approach is required to get a full view of all losses rather than at one location that is easiest to measure. It will be very important to look at reusing NASA's code and to not reinvent analysis that has already been done. Because of the strong effect of temperature on the equation of state and Z-value roll-off, the real gas equation of state is required to carefully consider the two-phase transition. The project approach of using real-world data rather than choosing arbitrary driving distances, as considered in the past, is important. By carefully reviewing past liquid hydrogen work from NASA, the investigators have access to a comprehensive list covering the main sources of loss and several different mitigation strategies. More quantitative details could be provided for mitigation options; however, those should be provided when the work is complete.
- The approach to performing the work is science-based, is clearly broken down into tasks, and addresses the critical stages of the liquid hydrogen (LH_2) delivery chain where losses occur.
- The concept appears to be relevant, especially for specific vehicle applications. The approach to performing the work appears to be scattered and lacks a clear destination of the end game. It may be too early to select a specific end game, but the project needs to have relevant goals.
- The project identifies the significant barriers, then limits its focus to contain the project scope. From the outside, the activity appears to focus almost exclusively on the thermodynamics of the cryogens and appears to treat heat transfer issues lightly. Including a list of assumptions would help to reduce the number of questions as the team restricts its scope to fit available resources.
- While the approach and analysis are reasonable, the approach of using liquid or cryo-compressed hydrogen (CCH_2) in vehicles appears to be a technological dead end. The authors should address this concern by asking what it would take for CCH_2 or liquid to be a more favorable solution than the current standard of 700 bar compressed gas systems. Without addressing this point, the value of this work is limited.

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 did a nice job presenting an understanding of cryo-compressed dynamics in a vehicle. The use and refinement of the NASA code is very convincing.
- Progress against goals has been good. Perhaps the project can develop a metric—perhaps a hydrogen efficiency measure or dollars-per-mile standard—to compare directly against the 700 bar standard.
- Boil-off losses are a key element of the overall energy efficiency and well-to-wheel energy consumption and emissions, and progress was made in this area toward meeting DOE's efficiency goals. Progress was made toward gathering data and finalizing sources of data. The project is 5% complete, so more details about additional accomplishments and progress should be available next year.
- The project is of a relatively short duration and has been underway for only a few months. The accomplishments and progress toward overall project and DOE goals will be highly dependent on the results the project delivers and how the results can be turned into action toward reducing sources of loss.
- The activity obviously faced some initialization issues. However, this presentation reported 5% completion and, along with the updated information during the review occurring at the halfway point in the project, suggests that there are underlying issues to resolve. Of particular concern are the validation of changes to the NASA model, compensating for differences between American and European driving habits/siting issues, and heat transfer issues contributing to boil-off. Validating changes to the NASA model is likely to be difficult and time-consuming. These issues are not insurmountable, but they merit increased monitoring.

Question 3: Collaboration and coordination with other institutions

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

- This is the strongest advantage for the activity. International collaboration with the right array of partners provides fertile ground to yield accurate results. The principal investigator is well connected and would do well to take advantage of these connections to mitigate the issues occurring in other areas of the activity.
- Linde is the premier supplier of cryopump technology and LH_2 delivery. This collaboration is key to understanding the pump and potential mitigation strategies.
- Linde and BMW are the right industrial partners for this project, as they are the commercial leads for implementing these technologies.
- This project will require significant levels of interaction with other institutions, specifically gas companies. The project and the principal investigator have a long history of interaction with these institutions. However, this will require a deeper dive into industry practices upstream of the final point of delivery. In the next phase of the project, it will be very important to interact with key individuals in these institutions to understand standard practices, non-standard practices, regulatory requirements, and business-driven practices. The effectiveness of the project results will depend on the science-based calculations of the various losses but also, perhaps more importantly, on characterizing the input parameters of those losses that stem from industry practice, which may vary among institutions.
- This area seems weak, probably because cryo-compressed is not widely accepted, but effort needs to be given to draw in more collaborators. The concept might be interesting to large-scale fuel cell vehicle manufacturers (buses, trucks, etc.).

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 LH_2 supply chain will become an increasingly important option for hydrogen delivery to stations, especially for larger-capacity stations, e.g., truck and bus stations. Losses during the LH_2 delivery chain are a major opportunity for fuel cost reductions, efficiency gains, etc. LH_2 may also play a role in securing low-

cost renewable hydrogen sources and expanding the economic delivery range of the hydrogen, via liquefaction. Minimizing losses will play a role in meeting DOE targets for hydrogen cost at the pump.
- After liquefaction energy, losses are the biggest concern for LH_2 pathways. This topic is very important for success of the technology and can have a significant impact on advancing the Hydrogen and Fuel Cells Program and hydrogen infrastructure efforts.
- Many reviews from the industrial and commercial perspectives have been completed. A complete evaluation of the entire LH_2 market from the residential market perspective is both warranted and necessary. This activity is a great start, but being limited to a single year imposes scope restrictions. Depending on the results from this stage, a subsequent assessment delving more deeply into the assumptions is likely advantageous.
- The presentation made a convincing argument that there are likely applications in which this approach to fueling vehicles makes the most sense. During next year's presentation, it would be helpful to understand those applications and what the economic benefit of the technology for those applications is.
- While having good data and models for these approaches is important from a benchmarking perspective, it would appear that the industry has moved away from LH_2 and CCH_2 onboard.

Question 5: Proposed future work

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

- The project has effectively planned for future work with clear targets toward reaching the project objectives. It will be important to emphasize gathering information from industry on practices in the supply chain and using that information as inputs into the simulation to determine opportunities for improvement.
- The next steps include verifying the thermodynamic codes and beginning to run typical cases. The key work after this analysis is to determine where the most effort should be placed for mitigating boil-off losses and what technologies might be used.
- One goal for this year is to verify that the codes(s) are appropriate for the analysis being considered. The criteria for answering this—the quantitative measures used to determine code applicability/accuracy—should be more detailed. Secondly, there is no mention of a backup plan if the codes are deemed unsuitable. It seems that the project is working under the assumption that the codes will be deemed suitable in some fashion.
- The stated future work, both documented in the presentation package and expressed during the oral review, fits within the schedule and budget for this activity. It is likely that the validation effort for the NASA software modifications is underappreciated, as this validation is crucial to lending credibility to any results based on the modified model. Remarkably little was proposed for subsequent work beyond the end of this period of performance. This absence suggests a level of confidence in the results not fully substantiated from an external perspective with the provided documentation.
- It seems the project should be able to demonstrate the concept in a setting that replicates filling, fuel use, ambient conditions, and other environmental factors.

Project strengths:

- The project has the right partners. Unlike some projects that "reinvent the wheel," this project is leveraging the years of work done by others in developing the models and the building upon them and applying them to new areas.
- The project has a clear, science-based approach and a strong commitment to reach out to industry and institutions. The project is highly relevant to Multi-Year Research, Development, and Demonstration Plan (MYRDDP) targets. Progress to date is good.
- The project strengths include close collaboration with Linde and a deep technical knowledge of cryogenic hydrogen. The additional data analysis around real-world drive cycles is an important complement to the technical work and next steps.
- This project leverages many external partners with representative skill sets. From this network, the project is positioned to recover lost ground and meet activity objectives within a moderate tolerance.
- The project is reminding the community that this approach to fueling should not be overlooked, especially for large-scale segments of application.

Project weaknesses:

- Weaknesses include the limited commercial viability of the onboard LH_2/CCH_2 approach. Many assumptions are being made regarding parking and conditions under which the cars are used. Customers will cover the entire spectrum of usage options (not just some kind of average usage) and will need to understand the effects of all these extremes. There does not appear to be any prioritization of the loss mechanisms or their mitigation. Perhaps adding a more comprehensive review of the impacts leading to prioritization would be helpful.
- The project should show how industrial gas companies can adopt the analysis and what market effects, such as low-cost energy, lead to such large amounts of boil-off. Current industry practice does not prioritize reducing LH_2 boil-off because of how prices are negotiated and because of feed stock prices.
- The activity faced initialization issues that are apparently not fully resolved. The subtle differences between thermodynamics and heat transfer are poorly communicated and minimized, as are the behavioral differences between American and European drivers. These are not insurmountable issues, as team members are quite capable of resolving rather than mitigating these issues.
- There is an unclear degree of focus on LH_2 supply chain losses versus cryo-compressed park/drive/fill pattern analysis (i.e., which project objective will have more impact on MYRDDP targets). More emphasis is needed on results delivery and recommendations for improvements.
- More vision and physical action are needed regarding how to demonstrate the concept to prove the theoretical work.

Recommendations for additions/deletions to project scope:

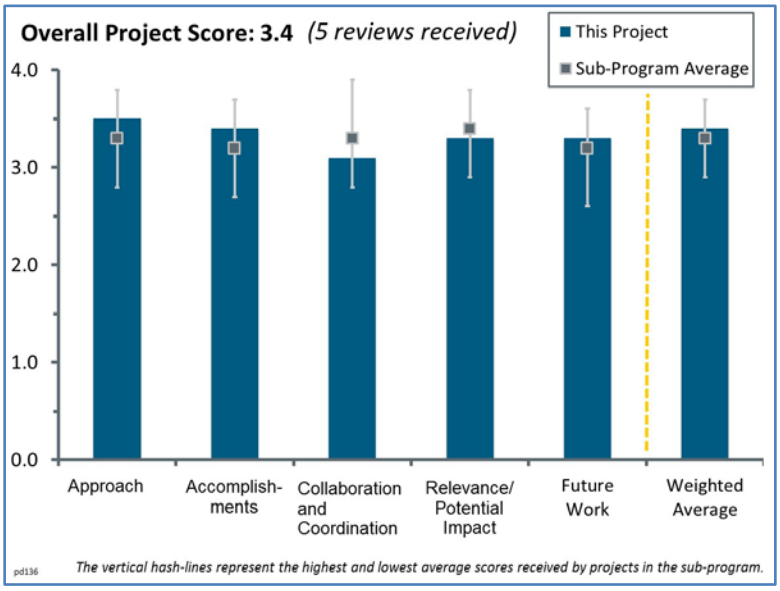
- The project should make a more direct comparison to 700 bar and develop a criterion by which the performance of the two systems can be directly measured. This comparison and analysis should be used to determine when/where LH_2 or CCH_2 would be a better solution than 700 bar. To make the work more relevant to today's and future infrastructure solutions, the project should start to focus less on the onboard storage losses and start to look at the LH_2 supply issues and mitigation strategies.
- The project should clarify an approach for results delivery in the form of a paper or other method to ensure widespread distribution to institutions and industry (specifically with regard to the LH_2 supply chain). The project should also ensure all uses of LH_2 at the station are considered in the pathway: low-pressure pumping, high-pressure pumping, vaporization/compression, cryo-compressed filling, etc.
- The activity has correctly identified what needs to be done and a methodology to complete it. It would impose programmatic risk to expand the project scope with the current state of progress. Simultaneously, descoping the activity would belittle its merit to DOE. At this time, this activity would benefit from additional monitoring temporarily as a mechanism to provide support until it is back on track.
- If the initial analysis is promising and specific technologies or industry best practices are recommended, the technologies and hardware should be tested at LLNL.
- More action is needed.

Project #PD-136: Electrochemical Compression

Monjid Hamdan; Giner, Inc.

Brief Summary of Project:

This project will develop and demonstrate an electrochemical hydrogen compressor (EHC) that is lower in cost, higher in efficiency, and more durable. Specifically, the project will (1) fabricate hydrocarbon membranes (HC) with enhanced properties for use in EHCs, (2) improve EHC water and thermal management, (3) optimize stack hardware and demonstrate cell performance, and (4) build a prototype system. Development of reliable and low-cost, high-pressure hydrogen systems is needed to enable market penetration of fuel cell electric vehicles.



Question 1: Approach to performing the work

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

- This project started in fiscal year (FY) 2017 (despite a confusing note that said FY 2016–2017). Considering the short time, the project has already achieved some important milestones. Barriers are well addressed, and the project seems to be well designed and well executed.
- Mr. Hamdan presents a concept for an electrochemical compression system using an HC membrane and a water management membrane. The use of an HC membrane could lead to lower-cost systems. This seems to be a feasible approach with merit.
- A substantial amount of work has been completed for a small percentage of the budget—indicative of the foundation on which Giner, Inc. (Giner) is building. Leaving a milestone at a lower-than-actual completion percentage to enable additional materials tests is an excellent contingency approach, given the early stage and ambitious goals. Cost will always be the driver.
- This project is very well designed and feasible. It aims at developing a reliable compressor with low maintenance cost. It fits very well with the other alternative projects, PD-137 and PD-138.
- Giner has taken a well-reasoned and -planned approach to the task and has made outstanding progress in Year 1.

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.

- This project is in an early stage, but it seems to be capable of addressing Hydrogen and Fuel Cells Program (the Program) goals and DOE targets. In the first several months, the principal investigators (PIs) made substantial improvements and demonstrated that the approach in this project is carefully planned and executed. Task 1 is fully accomplished, while Task 2, which is dedicated to membrane fabrication, is 60% accomplished, which is significant progress considering the short timeframe in the first year. Benefits from Task 2 are in utilization of cost-effective materials and improvement of mechanical properties while maintaining a high conductivity of 0.106 S/cm (the target was 0.100 S/cm). In addition, the cell performance was improved with an HC membrane to 0.110 V/cell. The PIs also reported progress in stack hardware development and initiated targeted evaluations at 350 bar, as well as stack design at 875 bar.

Overall, based on the progress in the first fiscal year, this project might be capable of addressing DOE targets in the future.

- If successful, this project could lead to hydrogen compressors without rotating parts; therefore, this project could lead to the development of novel high-pressure compressors with much higher reliability and eventually lower investment cost. It fits very well with the aims and goals of DOE.
- Giner has demonstrated a substantial jump on the progress to date for a small percentage of the budget. The project is consistent with the DOE goals and, based on the preliminary data, is on track to hit the technical performance targets.
- Giner has made great progress toward the targets. Economics are still an issue, but capital expenditure (capex) has not been addressed yet. The rational approach to membranes and stack has yielded outstanding results so far.
- During the presentation, the speaker was fairly confident that the project had overcome the technical barriers and was on its way to a successful completion. However, it is not clear how the goals on slide 2 are fully supported by the results on slide 12. On slide 12, the cell is operating at a voltage of 0.255 V/cell (from Nernst) and a current density of 890 mA/cm^2. The ideal cell voltage, per Nernst, should be 0.07 V/cell. Therefore, there is an overpotential of around 200 mOhm cm^2. This works out to around 5 kWh/kg required for hydrogen compression, which is very far from the DOE goal of 1.16 kWh/kg and does not match the project's reported status of 2.7 kWh/kg on slide 2. It is not clear if the team's indication that the project was on track to meet DOE goals was by extrapolation of slide 13. If indeed the project results are four times the power consumption goals, the team has a very long way to go to meet DOE's goals.

Question 3: Collaboration and coordination with other institutions

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

- Giner's experience in cell, stack, and system development would not technically require much collaboration to complete the milestones for this project. That said, Giner has nonetheless assembled a strong team—the National Renewable Energy Laboratory (NREL) (cell to stack test and optimizations), Rensselaer Polytechnic Institute (HC membranes), and Gaia Energy Research Institute LLC (technoeconomic analysis and life-cycle analysis)—that uses each partner for its strengths. As usual, coordination for a timely execution will be the challenge going forward.
- Giner has great internal talent and capabilities but has gone outside of the company and identified appropriate partners to complement the internal expertise where needed.
- Giner has worked out a good collaboration with an HC membrane supplier. The project has the right team members to get the job done.
- Giner is collaborating with a national laboratory, academia, and a private business. The division of work seems to be logical (development of membranes, development of stacks, testing, and technoeconomic analysis) and enforces the collaboration between these entities. Further exchange of information with other domestic or international groups is not clear.
- The team is carefully assembled, and it seems to be well coordinated. The roles of participants are well defined; however, it was difficult to delineate contributions among collaborators. In the future, it would be beneficial to assign the results to each participant that contributed to progress and accomplishments.

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 high-pressure hydrogen compressors are the most expensive part of a hydrogen fueling station, and they cause high maintenance costs for the station. Reliability so far is suboptimal. This project can significantly help to overcome these challenges if the possibility of membrane rupture at the envisioned high pressures could be excluded.

- This multiyear project aligns very well with the Program goals. This project is aimed at addressing critical barriers in deployment of fuel cell technology. In the first year, the PIs demonstrated the significant potential of this project to address limitations in hydrogen compression.
- The project targets lower cost, higher efficiency, and improved durability in a focused manner—aligned with the Multi-Year Research, Development, and Demonstration Plan. If the targets are hit, it will open the door for an additional technology candidate for forecourt hydrogen compression applications.
- Electrochemical compression is poised to be an important component of an energy-efficient hydrogen delivery system.
- This "no-moving-parts" compressor could be a real game changer, if successful.

Question 5: Proposed future work

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

- The proposed future work is expected to expand current project status by getting closer to the outlet pressure capability of 950 bar. The strategy of future work is well assembled, with the ability to deliver alternative options to the current state of the art. The PIs based their work on the metrics and proper decision-making points.
- The plans for the membrane, cell, stack, and system are spot on. Hitting performance at cost will be a challenge. The logistics of ensuring endurance and the tradeoff against repeated cell purges will need attention.
- Lifetime, scale, and cost reductions are all targeted for future work.
- The most critical aspects have been identified and will be addressed.
- Giner believes that the project is well through its technical hurdles, but this is not clear. The investigators should go back and double-check all of their assumptions about energy efficiency before proceeding further. A 1,000-hour test is impressive, but a longer test should be performed to be more compatible with a realistic equipment maintenance schedule.

Project strengths:

- Strengths include Giner's state-of-the-art status, the depth and breadth of their foundation in design-through-delivery of ultra-high-pressure electrolyzers, and NREL's Energy Systems Integration Facility testing and optimization capabilities. Rensselaer Polytechnic Institute and Gaia Energy Research Institute LLC are used for their key strengths also. Giner staff's ability to translate polymer-electrolyte-membrane-based electrolyzer system experience into high-pressure compression will be a key benefit for the project.
- This is a strong and experienced team that is capable of executing challenging issues in hydrogen compression. The approach is science-based, and it is being executed in a highly technical manner. The project offers alternative and innovative solutions for hydrogen compression.
- The project is run by a very experienced team. Different polymers and concepts are being tested. Results so far are promising. The contact and collaboration with the TÜV Rheinland Group in such an early stage is appreciated.
- The project looks at using HC membranes and an advanced water management membrane to improve the performance and lower the cost of electrochemical compression.
- Strengths include strong expertise, a well-planned approach, and a well-executed project.

Project weaknesses:

- There are no significant weaknesses.
- The project reports on progress made in FY 2017. However, considering timeframes for submission of this report, it would be premature to assign weaknesses to what has been achieved. The only obvious flaw is a discrepancy between the DOE target of 950 bar for the outlet pressure capability versus the 350 and 875 bar that are listed in this project.
- At this early stage of the project, these would be more accurately called challenges than weaknesses: as always, individual technical barriers (for example, specific energy) can be overcome—but the coupling of this and the simultaneous completion of other technical barriers is key, e.g., hitting $170,000 uninstalled

capex, plus a 10-year life, with a new cell/stack design that also enables 950 bar (along with a possible scale-up in active area). The effects of that cumulative challenge will become revealed only in the coming year.
- The team might be overconfident about their ability to reach DOE goals and their ability to deliver a working, useful prototype.
- Compared to the other projects using metal hydride compressors, such an electrochemical system has a high inherent risk of failure (membrane rupture), especially if operated under the high-end pressure. The risk assessment has to be started very early.

Recommendations for additions/deletions to project scope:

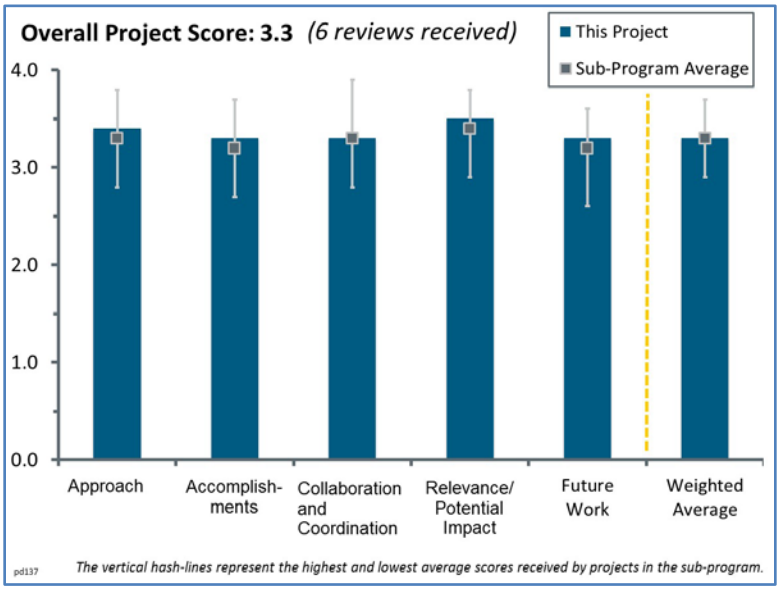
- The project scope seems to be well aligned with ongoing progress and proposed activities. There are no recommendations for addition or removal of existing efforts.
- At this time, there are no recommendations for additions or changes. The usual vigilance on the next scope-set is needed: HC membrane evaluations at 5,000 psi; 1,000-hour-duration test at low decay rate (especially needed); new stack design/fabrication/verification at 12,688 psi; prototype system design; and develop specifications/procure Class 1, Division 2, Group B components.
- The researchers should review their energy balance equations and take an honest look at whether the project is able to meet the DOE goals at a system level.
- Risk assessment for membrane rupture has to be done early. It should be determined whether other parts of the compressor would also fail in the case of a membrane rupture.

Project #PD-137: Hybrid Electrochemical–Metal Hydride Compression

Scott Greenway; Greenway Energy, Inc.

Brief Summary of Project:

There is a need to increase the reliability, reduce the cost, and improve the energy efficiency of gaseous hydrogen compressors. This project seeks to address this challenge by developing a hybrid electrochemical–metal hydride (EC-MH) compressor. The project will analyze and screen potential hybrid compressor systems and materials, conduct experimental tests, develop a hybrid compressor system model, and build a prototype unit.



Question 1: Approach to performing the work

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

- The project takes an excellent approach, combining EC compression and MH hydrogen compression to reduce total investment cost, reduce operation cost (by using waste heat), and avoid the risk of possible membrane failure at high pressures.
- The main goal of this project is to propose and deliver a hybrid EC-MH system for compression of hydrogen to be used in fuel cells. The approach is carefully made and seems capable of addressing the barriers. During the initial period of the project, principal investigators (PIs) have demonstrated that the approach is feasible and well aligned with other efforts.
- The presenter did a nice job of describing the potential advantages and disadvantages of three non-mechanical hydrogen compression schemes directed at improving the reliability, improving the energy efficiency, and reducing the cost of gaseous hydrogen compression. The three approaches described were (1) compression via staged electrolysis, (2) staged compression via MH adsorption–desorption, and (3) the subject of the presentation, compression of hydrogen via a "hybrid" EC-MH dual-stage process. This approach may take advantage of relatively inexpensive low-pressure electrolysis coupled to a high-pressure MH stage, if successful. Given that low-pressure and -temperature electrolysis is fairly well known, and that MH compression has been around for several decades (including greater-than-laboratory-scale demonstrations), perhaps the approach could be improved by increasing the focus on the technoeconomic analysis of the hybrid approach to discern at an earlier stage in the project whether the hybrid approach can be cost-competitive and meet the specific energy target.
- The approach taken to store hydrogen via a hybrid scheme using EC hydrogen compression followed by MH compression is a novel spin on EC hydrogen compression. The project is not necessarily clear on what its specific objective is. From the presentation, there appear to be four research tasks:
(1) polybenzimidazole (PBI) EC cell, (2) metal hydride selection/development, (3) EC-MH hydrogen compression demonstration, and (4) system modeling. The researchers should focus on their efforts on Tasks 3 and 4. Task 1 should not be researched, because EC hydrogen compressors are already demonstrated and commercialized (although the demand and cost are not currently favorable). It is recommended that the researchers partner with a company to supply the EC hydrogen compressor. The researchers are not in a position to provide significant improvements or advancements in the area of EC hydrogen compression. For example, slides 16–18 are fundamental studies on PBI-based EC cells that have been heavily researched in the past. Task 2 has the flavor of MH development/discovery (slide 19). The models developed for the Hydrogen Storage Engineering Center of Excellence (HSECoE) and modified for this project should be used to identify the most appropriate and readily available MH and used as the

surrogate to demonstrate the system. The researchers must resist the temptation to turn this project into a materials development and characterization effort. The researchers are encouraged to pick an MH and go with it, for they will never have the ideal MH.

- The combination of EC compression and MH compression appears to be a reasonable approach, which could be an interesting way to eliminate mechanical compressors. The main issue is in the cost of the EC compressor. Accomplishing 10–100 bar compression at the rates proposed should be based off of electrolysis stack costs plus additional costs for the exotic metals required for phosphoric acid compatibility. At that point, it becomes more expensive than commercial 30 or 60 bar output polymer electrolyte membrane electrolysis without the need for management of phosphoric acid and drying hydrogen from water and acid. Nevertheless, there may be a niche where this approach may work. The cost analysis should be expanded to look at water electrolysis with MH compression.
- The development of a hybrid EC-MH compression system is attacked in the project with a reasonable approach to address the barriers. While it is early in the project and a substantial amount of work has been accomplished, the overall scope, schedule, budget, and targets of the work are ambitious.
    - Slide 8: the U.S. Department of Energy target (100–875 bar) is identified as "= 1.6 kWh/kg", and the graph for Nafion™ indicates that for the highest number of cells/lowest current density, the lowest/best total power is ~2.1 kWh/kg. It is not clear what the approach is for reaching 1.6 kWh/kg.
    - Slide 14 identifies that 2.5 A/cm^2 is required to match temperature and energy requirements. Slide 16 identifies that testing will be conducted at 1.5 A/cm^2 and that the inlet pressure is 1–30 bar. It is not clear what the approach is to hit 2.5 A/cm^2 at the earlier specification of an inlet of 10 bar.

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.

- This is a new project that started in fiscal year 2017, and the submitted report reflects the progress made over the first six months of this effort. During that period, PIs managed to launch the project and started to address barriers by implementation of what has been proposed for hybrid EC-MH compression of hydrogen. A technoeconomic evaluation has been fully executed, and the most promising systems have been selected based on Ti-MH and PBI membrane. It is estimated that waste heat from PBI-EC hydrogen compression, which operates at higher temperature, is supposed to provide enough energy to drive the MH compression. In addition, a new heat transfer for MH compression is proposed, which can diminish the heat transfer area and reduce the cost. Small-scale testing of MH systems is complete, and a new hybrid system is under development. The progress made is well aligned with milestones. On the other hand, the PIs did not provide direct comparison with the DOE barriers; however, considering that it is the initial phase of this project, that should be addressed in future reports.
- Very nice results were already obtained for both the EC compression system and the MH compression system. The accomplishments so far are promising. The chance to develop a highly reliable compressor system seems very good, considering the achievements reached so far.
- Given the ambitious combination of scope, schedule, budgets, and milestones, the accomplishments to date are commendable.
- The project appears to be on track, judging from the milestones achieved to date. While the project is making reasonable progress on the screening of materials for the EC stage, this team could perhaps accelerate progress by teaming/collaborating with other projects that are focused on low-temperature and -pressure electrolysis for hydrogen production to arrive more quickly at a possible configuration of the EC stage. Similarly, there is a good deal of published work on MH compressors up to and including near-commercial units; exploring the existing state of the art could accelerate this team's progress. If the experimental portion of the project was streamlined, perhaps the team could get at the crux of the problem (defining the cost and determining whether it can be competitive with that of mechanical compression). There is concern that the overall system efficiency can be met, given that slide 8 indicates that at low current density, the Nafion-based membrane achieves around 2 kWh/kg at 10–100 bar. Good progress is being made on the system model development (Task 1.4). Consideration should be given to accelerating

and enhancing this effort, as a good system model may help to reduce the amount of experimental work outlined in Tasks 1.1–1.3. Water and heat management issues are known to the researchers, and the team should provide updates in future presentations on how or if these can be handled economically and efficiently.
- This project is a new start, and the researchers are just getting their feet wet. However, the researchers would be better served to focus their efforts on the demonstration unit and modeling. The modified models generated in the HSECoE used in this project position the researchers to optimize operation and costs associated with the hybrid hydrogen compression approach.
- This project started recently. The team should revisit the cost analysis.

Question 3: Collaboration and coordination with other institutions

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

- Besides the collaboration between the direct project partners, there seems to be a nice collaboration with other partners both at the MH side (Bob Bowman and Craig Jensen as well as Terry Johnson) and on the EC hydrogen compression side (Advent Energy).
- The team listed on slide 25 is excellent for this work: Oak Ridge National Laboratory/Bob Bowman for material property; University of Hawaii/Craig Jensen and Sandia National Laboratories/Terry Johnson for MH system and compressor modeling; and Advent Energy for membrane materials.
- Researchers should not be focused on the design of EC hydrogen compression hardware or MH development. The researchers are urged to collaborate with companies that already produce and/or have expertise in this area. Given the short time frame of the project, the focus should be on demonstration and modeling. The researchers should be talking with fuel cell researchers (national laboratories or companies) about the advantages and disadvantages of PBI- versus Nafion-based EC cells, for example, cost, platinum loading, and thickness.
- For a new project, the level of collaboration is adequate. It may help the project to develop additional collaborations with low-temperature and -pressure electrolysis projects to accelerate progress and to help populate the systems model with relevant information. Collaborations on the MH compressor portion of the project are appropriate and of high quality. It is nice to see that this project is collaborating with the two-stage MH project. This is a good indication that both teams want the best technology to emerge, and by working together, this is more likely to happen earlier rather than later.
- The participants in this project are clearly listed; however, the PIs should more precisely delineate the roles and contributions from each participant in accomplishment slides. In addition to participants, the project also interacts with a network of suppliers for materials of interest.
- There was not much interaction between the two partners until way late in the process. The difference between Milestones 1.1.2 and 1.6 are unclear.

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's Hydrogen and Fuel Cells Program. The approach and deliverables are well structured and are capable to address the barriers in hydrogen compression, which has been identified as one of the bottlenecks in the wide spread of hydrogen infrastructure and fuel cell technology. If successful, this project would deliver a new hybrid EC system for efficient compression of hydrogen and lower cost.
- The relevance and potential impact of non-mechanical gaseous hydrogen compressors may be high. Reducing operating costs with potentially enhanced reliability and energy efficiency is clearly beneficial to the bottom line of delivering hydrogen inexpensively at the fueling station.
- The approach is brilliant, and if the project is successful and cheap MH alloys can be identified, it can mean a breakthrough to improved reliability and reduced maintenance cost of hydrogen fueling stations.

- The concept of low pressure by EC compression and high pressure via MH is worth pursuing and is consistent with the Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP). The challenge is hitting the overall power goal and cost.
- The relevance of the project is rated as moderate not because of the proposed application of hydrogen refueling stations (which is a hard sell), but because of the intended use of this approach on a much, much broader and bigger scale (e.g., H2@ Scale applications). The researchers should take the time to perform a cost comparison of the current mechanical compression strategies to those of the project. In particular, the cost of maintenance, downtime, etc. of mechanical compression should be used to make the argument for using EC-MH hydrogen compression. The cost of ownership may be a suitable metric to use for this comparison.
- The project addresses some goals from the MYRDDP. It is unclear if efficiency goals optimistically appear to be three times worse than DOE targets: leakage rate MYRDDP target versus compressor leak rate through hydrogen diffusion.

Question 5: Proposed future work

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

- Considering this is an early stage of the project, future work will be critical in evaluating the success of this effort. The proposed activities, along with go/no-go decision steps in addressing the barriers, are carefully planned and seem to have logical flow as the project evolves.
- At the next review, it would be beneficial to hear more about the systems modeling effort and the detailed inputs to the model. Overall, the proposed future work followed logically from the current status.
- Proposed work appears to be adequate. It is recommended that the team focus on the demonstration and model validation efforts. System demonstration and model validation will allow the overall system to be further optimized. It is also recommended that the team collaborate with vendors or companies already producing Nafion-based EC hydrogen compression for bench-scale testing. The efforts in developing and testing membranes are severely diluting the project's primary research focus and should not be pursued. Savannah River National Laboratory was the MH lead in the HSECoE and performed some great modeling work on MH systems and the identification of MH material characteristics, which makes it difficult to understand the extended efforts in identifying a suitable MH for this demonstration. It is also recommended that the team use the highest-quality hydrogen to prevent contamination and complications. In the end, this is a demonstration/"proof-of-concept" effort in which the durability of the EC hydrogen compressor and MH compressor are not the focus, not to mention that the contaminants for Nafion- and PBI-based EC cells are known. The team should add purifiers to scrub the hydrogen of these contaminants to demonstrate and validate the models. There is interest in seeing the effects of a shifting plateau region of the isotherm (i.e., non-ideal) on the overall system efficiency, operation, and cost.
- The planned work is on the right track. For the high-pressure MH compression part, it is very important for the partners to measure the isotherms (plateau slopes and hysteresis) at the final temperatures and envisioned pressures. Looking at cheap alloy alternatives might be important for the economical evaluation.
- Emphasis is to be placed on hitting the energy target and evolving from the initial test conditions used for early verification and validation to the targeted values. The approach to running at 2.5 A/cm^2 is not clear It is also not clear what the tradeoff is between minimizing sensible heating, thermal losses, and void volume while also maximizing hydrogen flow rate. Finally, it is not clear what the anticipated decay rate is (percentage of initial capacity over ~1,000 cycles).
- The approach of separate EC and MH development leading to the Year 2 prototype design relies too heavily on both working without any prior knowledge. The cost analysis should be expanded to look at water electrolysis with MH compression.

Project strengths:

- The project is aimed at addressing the barriers in hydrogen compression by implementation of hybrid EC-MH systems that would enable more effective hydrogen compression and lower the cost. The initial phase of the project demonstrates positive development and reflects promise that this approach might be

successful. The main strength can be viewed in already identified components that would become integral parts of the future hybrid system.

- This is a very competent group that follows a highly interesting alternative route for high-pressure hydrogen compression. Combining both low-pressure EC hydrogen compression and high-pressure hydride-based hydrogen compression is a very smart idea. The team has put together competence both from membrane/stack development and from the MH side.
- The team is exceptionally well qualified to perform the demonstration and modeling efforts. Using the "pioneering" foundations from the HSECoE modeling efforts, the team is well positioned to push the hybrid EC-MH hydrogen compression to the next level of operational understanding and development.
- This is a good team with a good approach to the critical engineering tasks. The team is working well with a "competing" team. The work is highly relevant and impactful to DOE targets and the potential for future commercialization of low-maintenance gaseous hydrogen compressors.
- The project jumped on some early EC compressor data and sensitivities. The project must continue to probe and understand the true potential amenability of phosphoric acid materials for this application; the early MH data indicating the near-feasibility of hitting the targeted pressure is also an excellent start.
- This project has taken an interesting approach.

Project weaknesses:

- There are almost no weaknesses to this project at all. To argue for this alternative route for hydrogen compression, it seems very important to estimate the costs of the whole potential design as soon as possible. For this estimate, the cost for actual MH alloys may be taken into account, as well as the assumption to shift to cheaper alloys. The team should be able to answer the question about the costs of such an alternative.
- The primary weaknesses of this project include the research efforts focused on membrane electrode assembly (MEA) development and MH identification. The MEA effort is a distraction, and the team is encouraged to contact companies with expertise in EC compression, or perhaps even acquire an EC hydrogen compression unit from them to include in the demonstration unit. Another notable weakness is the technoeconomic analysis (TEA) of Nafion- and PBI-based EC cells. For PBI-based systems, the membrane is typically thicker (increasing the overpotential) and has higher platinum loadings (~two to three times higher) than for their Nafion-based analogs; these differences are not reflected in the TEA analyses. The researchers should consult with the appropriate companies or researchers working on fuel cells to ensure realistic TEA analyses are performed.
- The PIs should demonstrate more clearly the individual roles of participants in accomplishment slides. Also, DOE technical targets should be listed versus the current status of this project. The progress that has been made should be listed as a percentage of accomplished work. Considering this is a new project, these comments should be addressed in next year's report.
- Given the significant amount of work in both EC compression and particularly MH compression, perhaps more emphasis should be placed on systems modeling to accelerate the project to developing a good cost model of the hybrid approach.
- The project's weaknesses are hitting the efficiency/power target and still being viable on cost.
- It is unclear whether the goals can be met, even theoretically. Electrolysis to hydride compression seems to be a more logical approach.

Recommendations for additions/deletions to project scope:

- At this early stage, there are no additions or deletions recommended. Vigilance will be required that the materials, stack design, and MH design enable testing and evolve to hit the operating window required for thermal alignment as well as the best/lowest power requirement. Of course, cost/utilization of the MH will be a driver.
- Considering project duration, it would be premature to suggest additions or deletions of activities.
- It is recommended that the team focus on demonstration and model validation of the EC-MH system and eliminate the work scope associated with EC hydrogen compression (i.e., hardware development and testing). It is also recommended that the PIs of PD-137 and PD-138 both use the same modeling framework

and collaborate in choosing the high-pressure MH. This approach allows for easy comparison of the two different hydrogen compression approaches.

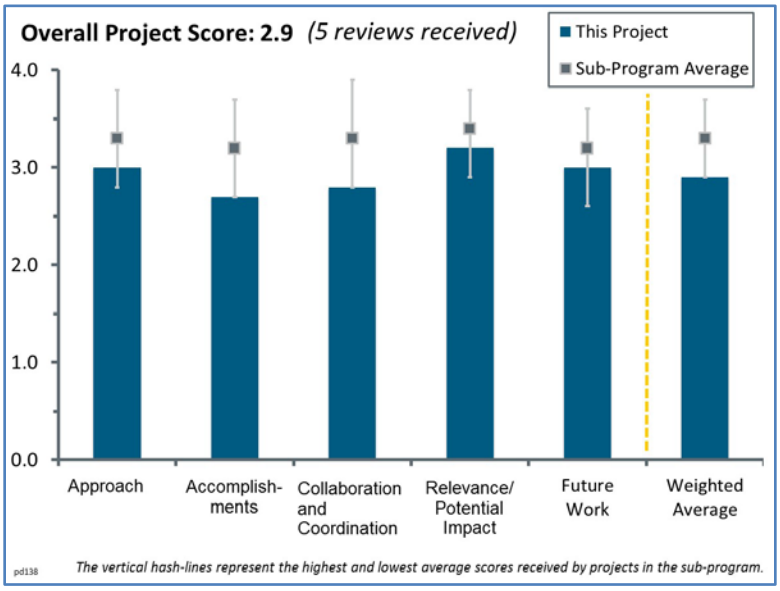
- A rough cost estimation should be performed to answer questions from the industry. Hysteresis and plateau slopes of the potential MH alloys must be measured at final temperatures and pressures.
- An earlier go/no-go based on cost or ability to meet the efficiency target is recommended. The cost analysis should be expanded to look at water electrolysis with MH compression.
- An added emphasis to cost modeling is recommended.

Project #PD-138: Metal Hydride Compression

Terry Johnson; Sandia National Laboratories

Brief Summary of Project:

The objective of this project is to demonstrate a two-stage metal hydride compressor with a feed pressure of 50–100 bar delivering high-purity hydrogen gas at an outlet pressure of 875 bar or more. At least two candidate alloys will be identified for both the low-pressure and high-pressure stages, a detailed design for the compressor completed, and a prototype compressor built. The developed technology seeks to address the need for less expensive and more reliable compressors for hydrogen fueling stations.



Question 1: Approach to performing the work

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

- This approach dovetails nicely with the other two non-mechanical gaseous hydrogen compressor projects, which are a two-stage electrochemical compressor and a "hybrid" electrochemical–metal hydride two-stage approach. The approach is highlighted by a rational down-selection process of two metal hydrides that have the appropriate hydride/dehydride properties to combine into a system that can compress hydrogen to 875 bar at an acceptable energy efficiency and overall cost. An experimental approach is balanced by a system model that will be used to demonstrate feasibility and assist in final bed design and demonstration.
- Metal hydride compressors are a sensible approach to reducing the maintenance and improving the reliability of compressors. However, the ability to meet the cost target is highly dependent upon the heat source—either waste heat (from somewhere) or incorporating a low-cost combustion (natural gas) system. Not much can be done from the material side because the thermodynamics are set.
- In a nutshell, the project is using metal hydrides to store and pressurize hydrogen by way of heat management. The researchers state that the main objective is to demonstrate a two-stage metal hydride compressor with a feed pressure of ~50 bar delivering high-purity hydrogen gas at 1 kg H_2/hr at an outlet pressure of 875 bar. However, demonstration can occur only if the appropriate metal hydrides for each of the two stages are identified. The approach to using metal hydride compression is not novel, but the overall challenge with the high pressures required is namely the development or identification of appropriate metal hydrides. The project's efforts related to system modeling and cost analyses should be leveraging the extensive and thorough outputs of the Hydrogen Storage Engineering Center of Excellence (HSECoE), now available on the web. It seems that the project is attempting to reinvent the models. The team is strongly encouraged to contact the HSECoE metal hydride modeling team to take advantage of what has already been done at the system level, cost, and material properties. The primary issues identified with metal hydrides (aside from the gravimetric capacity) are kinetics and thermal management. The additional issue with this project is the absence of suitable high-pressure metal hydrides within the operational constraints. If the primary objective is to demonstrate metal hydride compression, then the effort should focus on off-the-shelf, readily available metal hydrides, even if the overall delivery pressure is less than the 875 bar target (within reason). This approach offers a proof-of-concept deliverable with model validation (in collaboration with HSECoE or using modified HSECoE web models), whereby the shortcomings of heat management, metal hydride performance and development, system operability, durability, and cost can be identified.

- The approach focuses on the development of a two-stage metal hydride compressor that has advantages over current compressor technology, such as reduced operating cost, low maintenance, passive (powered by waste heat), etc. The approach involves a trade study to evaluate bed designs, development of a system-level model to assess feasibility, and the identification and testing of candidate metal hydride materials capable of operating at appropriate conditions of temperature and pressure in the two-stage compressor. The approach is straightforward and clearly stated. However, there are two important issues that should be addressed. First, metal hydride compressor technologies have been described in the literature for decades. A compelling and definitive statement concerning the advantages of the technology being developed in this project is needed (there is concern the project is "reinventing the wheel"). Second, a detailed cost analysis is needed. A principal motivation for the work is that compressors dominate station costs in existing systems. The estimated cost for the more complex two-stage metal hydride compressor system proposed here is not known, nor how that cost compares with the conventional compressor cost (parts, fabrication, operation, etc.). It is understood that the cost of the metal hydride materials has yet to be determined. However, it seems reasonable that cost boundaries for materials could be established. A solid cost estimate is needed before it even makes sense to consider taking this approach further.
- Theoretically, the approach is sound. The implementation will be the challenge, as usual. Especially the simultaneous matching of thermals, the synchronizing of the hydrogenation and dehydrogenation cycles, and sizing and engineering the number of beds for continuous pumping at the desired pressure with *minimal* heat input. Consistent with the discussion after the presentation, it is known that metal hydride has been a candidate for replacing a mechanical compressor for a few generations, and it was not clear what specific uniqueness (e.g., newly established materials family of compositions and/or canister design with integrated thermal energy storage) sets this project apart from the past concepts that never made it in terms of the utilization of canister, performance/thermal shutoff limits, and cost, especially given that the initial modeling identifies an energy usage for heating of 12.5 kWh/kg H_2 (before an improved recuperator design could bring it down to 10). While it is true that the compressor cost dominates capital expenditures and operations and maintenance of a station, that nonetheless defines a "benchmark, or baseline budget," that alternative technologies are required to show a potential to beat. It is unclear what the value proposition is, i.e., what the benchmark cost is, and where the two-stage metal hydride system can land relative to that benchmark.

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

This project was rated **2.7** for its accomplishments and progress.

- Good progress has been made on the development and application of the dynamic system model capable of predicting metal hydride system performance. Likewise, useful background work has been presented on the identification and preliminary testing of several metal hydride alloy candidates capable of operating at temperatures and pressures compatible with the compressor design. A high-pressure testing (cycling) apparatus has been designed, and testing is in progress. A detailed cost analysis and comparison with the cost of incumbent system components is a noteworthy omission.
- The project's progress was good. There is a fair amount of work still ahead toward screening the candidate low-pressure and high-pressure materials. It is preferred that for the materials analyses, the technical performance characteristics be integrated with cost to understand the trade on both metrics up front. This would force a more in-depth analysis on this trade up front (the relationship between the metal hydride materials costs as a percentage of overall metal hydride system cost would also need to be roughly estimated up front). The availability of the material, in the preferred as-fabricated shape, also appears to be a parameter on which materials may also receive at least a qualitative ranking.
- This is a new start. That said, the project's progress has been very limited in meeting the main objective. Significant efforts were placed on metal hydride selection (critical for the high-pressure stage). It seems this effort could have been expedited by using the available HSECoE materials property tools. The development of the high-pressure pressure–composition isotherm (PCI) for materials characterization (although necessary) diverted the focus from the main objective. Lastly, the system modeling efforts lacked significant development/maturity because the team is not leveraging the existing models developed in the HSECoE. The team and partners have a wealth of metal hydride knowledge and system experience, which was not reflected in their progress to date. No mention of system costs or efficiencies was presented. The

team will proceed with great success if they maintain focus on the main objective and resist the urge to turn this into a materials development and characterization effort.
- Progress to Milestone 2.2 (characterization of materials) is "delayed," and there must be some concern that there may be an impact on the milestones coming up in July 2017 and the go/no-go decision in October. The go/no-go decision includes demonstration of two-stage compression and exercising of the systems-level compressor model in the next four to five months—a schedule that seems daunting.
- Only one of two milestones has been met. The high-pressure system is delayed (no measurements yet) and expected by the end of the fiscal year. It is not clear why low-pressure measurements are incomplete. Alloy selection is based on literature data, with no experimental verification. This is probably okay, but it would be good to confirm. Vendors claim to be able to produce alloys, but this should be checked. Material purity and phase composition may not be exactly what is claimed.

Question 3: Collaboration and coordination with other institutions

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

- Close and beneficial collaborations among the Sandia National Laboratories (SNL) team (lead, management, bed design, and compressor development), Oak Ridge National Laboratory (ORNL) (hydride identification and characterization and support bed design), and Hawaii Hydrogen Carriers (hydride characterization and prototype fabrication) are evident. This cooperation is resulting in more rapid progress in all aspects of the project. However, the team is strongly urged to include an additional team member with expertise in cost and efficiency analysis (e.g., Strategic Analysis, Argonne National Laboratory) to assist in making accurate cost projections.
- The team itself is quite good, with Bowman and Jensen as team members. So perhaps the team is adequate to handle the laboratory aspects and demonstration of prototype, and the systems-level compressor model is in good hands at SNL. Where there may be a gap is in the cost modeling aspect, and perhaps a collaboration in that area would be a good addition to the skill set of the team.
- This is an experienced team with good coordination between team members, but there is no obvious external collaboration yet. One wonders why the project does not collaborate with a group that has an existing high-pressure Sievert's rather than building a new one.
- The teaming of SNL, ORNL, and Hawaii Hydrogen Carriers is a reasonably strong one, technically; however, the presence of a commercialization team member would be preferred for even such an early-stage project.
- The project is not using the system models and material property guidelines developed in the HSECoE. The project is focusing on the development of a two-stage metal hydride compressor that has the potential to be extremely valuable (especially for the H2@ Scale concept). This was clearly evident based on the cost and maintenance hours associated with mechanical compression of hydrogen. It is recommended that the project leverage existing models and focus on demonstrating the two-stage metal hydride compression concept. The principal investigators from PD-137 and PD-138 are encouraged to collaborate in choosing the same high-pressure metal hydride and use the same materials data, keeping in mind that this project is not a materials discovery project but rather a demonstration 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 **3.2** for its relevance/potential impact.

- Reducing the operating costs and improving the reliability of gaseous hydrogen compression at an acceptable capital cost is central to reducing the cost of hydrogen delivery to the consumer. Therefore, this project is very impactful and relevant to Fuel Cell Technologies Office goals, particularly in the context of the two companion projects, each seeking to find which of these three concurrent approaches may be most advantageous.
- The team did a good job highlighting the need for replacing the traditional mechanical compression approach by pointing out the current cost and maintenance hours associated with mechanical compression.

It would be worthwhile for the team to compare the cost of ownership of a mechanical compressor as compared to the proposed two-stage metal hydride compressor. The potential impact could be large, assuming an H2@ Scale economy.

- The development of an improved hydrogen compressor system with reduced operating costs, low maintenance, and higher reliability is an important development that could have positive impacts on hydrogen filling station cost and efficiency. In that sense, the project definitely has potential impact on advancing progress toward DOE goals and objectives. However, the cost and performance advantages must be stated up front in a more compelling and detailed way before the impact of this project can be fully assessed.
- The project holds strong relevance as an alternative to addressing the reliability versus mechanical compressors. However, it is not clear whether the project also serves as an alternative to mechanical compressors on the cost metric.
- The project addresses reliability and maintenance issues associated with hydrogen compression.

Question 5: Proposed future work

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

- The proposed future work looks good. There is a need to demonstrate how the system will ultimately meet energy and/or cost targets. Currently, the energy requirement is approximately six times too high, so this should be a priority. Long term, there is a need to determine the regeneration capability/cost to determine a full system life cost.
- The team has identified that the path forward must address the effective utilization of “waste” or low-cost heat to drive the process. The researchers also identify that durability of the metal hydride beds while maintaining maximal hydrogen flow rate is of course crucial to a successful outcome of this project. So they are focused on key challenges and barriers as they move forward. Their upcoming go/no-go decision point is looming rapidly, and so their work in the immediate future must be efficient to achieve the intervening milestone(s).
- The future work follows straightforwardly from the current studies. An important component in the work plan is the characterization of at least two alloys for each pressure stage. In some ways, it is surprising that PCI data are not already available on the AB_2 systems being investigated. However, additional information is apparently required. The proposed future work should include a more detailed cost and efficiency analysis.
- At a high level, the plan for fiscal year 2017 (FY 2017) and FY 2018 are reasonable. In FY 2017, the characterization of “at least two alloys for each stage” would need to include testing over the full operating window, with as much system-like integration as possible. Also, laying off the subsystem-like testing and 1,000-cycle duration testing for FY 2018 appears to increase schedule risk, reducing opportunities for contingency and backup.
- The proposed work appears to be adequate; however, the cost analysis effort is not called out for either FY 2017 or FY 2018. Given that the cost analysis can be performed in parallel during FY 2018, it is recommended that this task be undertaken.

Project strengths:

- A strong project team has been assembled. The team comprises recognized experts in compressor system design and prototype development, metal hydride bed design and testing, and metal hydride materials and characterization. The work plan is logical and addresses the critical obstacles.
- This is an excellent team with a long history of working in this area. The work is highly relevant to DOE goals and objectives and to potential future commercial fueling station operation.
- This is a team of respected and knowledgeable metal hydride researchers that can achieve the project’s main objective. The project relevance was clearly identified.
- The project has a strong technical lead/project manager, a strong SNL/ORNL technical background, and a good work breakdown structure/teaming.

- This is a strong team with considerable experience. The approach is reasonable. The metal hydride system could be a simpler, more reliable alternative to mechanical compression. There is a nice plan for follow-on prototype demonstrations.

Project weaknesses:

- Metal hydride has been a candidate for replacing a mechanical compressor for a few generations; it was not clear what specific uniqueness (e.g., newly established materials family of compositions and/or canister design with integrated thermal energy storage) sets this project apart from the past concepts that never made it in terms of utilization of canister, performance/thermal shutoff limits, and cost, especially given that the initial modeling identifies an energy usage for heating of 12.5 kWh/kg H_2 (before an improved recuperator design could bring it down to 10). While it is true that the compressor cost dominates capital expenditures and operations and maintenance of a station, that nonetheless defines a "benchmark, or baseline budget," that alternative technologies are required to show a potential to beat. It is unclear what the value proposition is, i.e., what that benchmark cost is, and where the two-stage metal hydride system can land relative to that benchmark.
- The project lacks a solid and persuasive argument about why this technology is an improvement over the metal hydride compressor systems that have been proposed and developed over the last several decades. In addition, a more comprehensive cost and efficiency analysis that focuses on how the proposed system compares with the conventional compressor technology in terms of cost, reliability, efficiency, maintenance, etc. is needed.
- The characterization of low-pressure and high-pressure hydrides is only 10% complete (milestone due April 2017). The selection of two alloys for each stage is based only on literature value; there is no experimental confirmation yet. There is a need to understand how much of a savings is possible with a hydride compressor versus a mechanical one.
- The project's primary weakness is not using the preexisting models developed within the HSECoE for system analyses, system cost, and material properties. Using these models will also allow a direct comparison to the current hybrid electrochemical–metal hydride approach and potentially future hydrogen compression designs. Leveraging the existing models affords the team the opportunity to focus on system designs, cost analyses, and system demonstration.
- The team may wish to consider moving some cost modeling forward in the project. If the project is technically successful but the capital costs are far too high, that would be good to know sooner rather than later.

Recommendations for additions/deletions to project scope:

- Projects PD-137 and PD-138 are both strongly encouraged to coordinate and collaborate on their research efforts. It is recommended that both projects employ the same high-pressure metal hydride and share materials data (thermal conductivity, isotherms, etc.) to ensure both approaches can be compared. In addition, the modeling efforts can also be coordinated between the two projects to ensure direct comparison and continuity. The team must resist the urge to turn this project into a materials project that will ultimately detract from the main objective. A sensitivity analysis on the effects of non-ideal plateaus and hysteresis on the overall system efficiency and cost would be worthwhile. The team should focus on demonstrating the concept with an off-the-shelf, readily available metal hydride (projects PD-138 and PD-137 should use the same metal hydride), even if the overall delivery pressure falls below the target value (within reason). The overall modeling of the system will point toward the necessary improvements in the overall system design, operation, and material characteristics that are required to meet the DOE targets.
- The addition of a systems analyst to the team is recommended, an individual or group capable of rapidly conducting a cost/efficiency analysis of the proposed system and comparison with existing (conventional) compressor approaches. It is recommended that this be done as soon as possible so that a solid foundation for conducting further work is in place. This does not have to be particularly time-consuming or costly; it simply needs to provide the basis for further project development.
- There is no cost analysis yet. Metal hydride materials will be a cost driver, and there are questions about how much they degrade and whether they can be regenerated (and at what cost). Researchers are

encouraged to look into full system cost, including energy/cost associated with each cycle, along with material and regeneration costs.

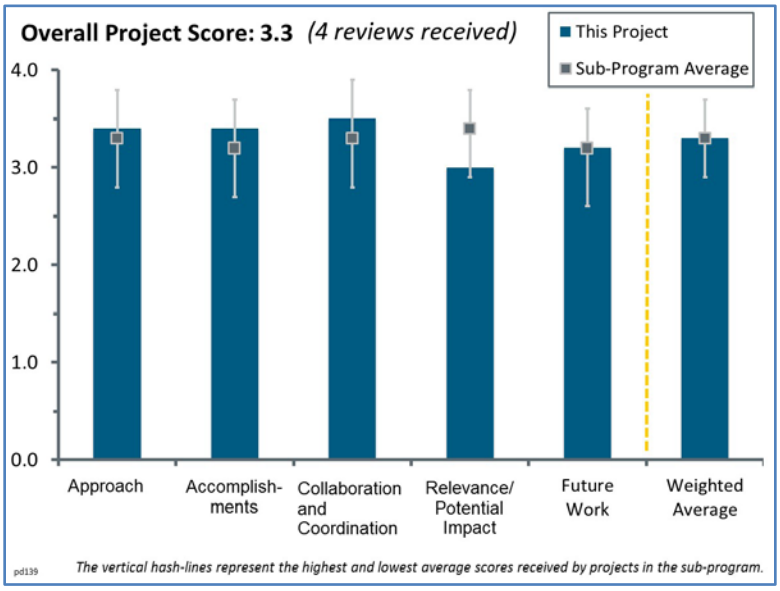
- The team should consider whether some small schedule/approach refinements are possible. Laying off the subsystem-like testing and 1,000-cycle duration testing for FY 2018 appears to increase schedule risk, reducing opportunities for contingency and backup.
- Cost modeling or an independent assessment based upon the systems-level compressor model output would inform as to the economic feasibility of this approach.

Project #PD-139: Reference Station Design, Phase II

Ethan Hecht; Sandia National Laboratories

Brief Summary of Project:

This project will provide publicly available details of different hydrogen fueling station equipment, designs, and economics. An "apples-to-apples" cost comparison of the entire hydrogen delivery chain with various station scenarios using data from industry collaborators will be developed. Evaluation includes stations supplied by centrally produced, delivered hydrogen and those with hydrogen produced on site. Some of the station designs are to be conventionally assembled on site, while others utilize a modular (containerized) construction approach.



Question 1: Approach to performing the work

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

- The approach to this project covers a very good and fair range of hydrogen station costs and architectures. This project addresses central production, distributed production, and the impact of throughput on station costs. As such, this selection is what needs to be evaluated/studied. The approach of including the piping and instrumentation drawings for station components works well to communicate. Others may use the piping and instrumentation diagrams from the National Renewable Energy Laboratory (a neutral source) to explain/describe stations. The approach of showing the relative power needs of the system components is also appreciated. This is needed for the investment community so they can "pick up the project" quickly.
- Exploring the five design options is a reasonable approach to establish general parameters. Engagement with partners and collaborators to ensure the analysis will be most useful to industry in infrastructure rollout was performed very well. The project is to be commended, particularly for its success in obtaining data typically considered priority. The effort is broken out into reasonable phases that can be built upon, and there is hope that the design tool will be made available to industry with training, so that further refinement of assumptions could be made on an individual basis moving forward.
- This project demonstrates excellent modeling of the impact of station design.
- The chosen capacities from Phase I of this project are a weakness of this project because they rely too much on the previous projects. The usability of these outcomes loses value over the years because of market and technology developments. What applied for stations at the start of the project does not apply when the project is done (from a capacity perspective). It would have been better to look ahead to what was likely to be needed in three years (300, 400, and 500 kg/day instead of 100, 200, and 300 kg/day), which would have added much more value because current size stations are already in place and operational.

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 accomplishments for this project include the development of a new Python model to evaluate/calculate the cost of hydrogen for station developers/operators and to show break-even over seven years based on delayed station rollout. The project team applied the model with industry surveys on station component

costs with optimized station design principles; benchmarked against other models; and completed breakdowns of labor, insurance, and delivery costs, among other costs. The value of this project toward meeting DOE goals is high because this project articulates to the hydrogen community, including potential investors/station developers with a material interest in knowing, the various costs of equipping and building a station. Further, this project addresses the impacts of delivered hydrogen on overall station operation costs and the footprint required for tube trailer storage. The impact on overall costs of using a modular station design with conventional equipment is also covered. The modular station is integral to hydrogen refueling network planning, for it presents an alternative to stations designed and built with components that assembled together can take on any non-standard configuration.

- The project results improve available data on station design and therefore provide better understanding for future station deployment and investment decisions.
- This project is an essential step in identifying key cost drivers and beginning to understand the implications of key assumptions.
- There is some question as to whether DOE/Hydrogen Fueling Infrastructure Research and Station Technology (H2FIRST) should spend its effort and funding on these hypothetical stations. It is not clear what happens to stations where the demand ramp rate does not increase as modeled. In addition, only a small percentage of all stations will not be in urban areas; the assumptions used for this effort were too generous.

Question 3: Collaboration and coordination with other institutions

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

- This project demonstrates a comprehensive collaboration between laboratories as well as stakeholder exchange.
- The list of collaborators shows that good effort has been taken to get market stakeholder feedback.
- The list of collaborators is impressive. The team is encouraged to add an asterisk on the Overview slide describing the partners, to reflect a much broader set of collaborators.
- The project's collaboration and coordination with other institutions is extensive, but it does not include utility representatives. More detail as to the collaborations (i.e., what happened) would have strengthened this presentation.

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 **3.0** for its relevance/potential impact.

- This project provides a valuable tool to facilitate design trade-off analysis, which contributes directly to the barriers identified in the presentation.
- Project results target future station design and deployment and therefore contribute to future hydrogen market development.
- This project is relevant and has a potential impact. This project contains details of various stations, their equipment, designs, and economics for stations of 100, 200, and 300 kg/day (stations operating today), with the idea to meet the DOE target: $2/kg of dispensed hydrogen. This includes the tube trailer lease costs for delivered hydrogen. This project supports and advances Hydrogen and Fuel Cells Program goals in that it addresses some of the issues associated with larger-scale stations (a part of the Multi-Year Research, Development, and Demonstration Plan), but it could have greater relevance and potential impact if it had included representatives from the utilities. The contribution to the cost of delivered hydrogen is relevant. This project summarizes that delivered gas is the most economical, but the footprint required is larger than a station with on-site production, and this presents issues in urban areas where space is at a premium.
- There are questions about whether this is real research, development, and demonstration or market assessment, and how this topic was prioritized over other topics.

Question 5: Proposed future work

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

- The future work proposal targets interesting questions with additional value.
- The plans to evaluate methods to reduce footprints are worthwhile. It is suggested that the project also consider putting the tool in the hands of station designers/developers who can then tease out potential scenarios, possibly identifying variables in assumptions made. It is also recommended that the project clarify on the Overview slide that the presentation includes plans for future phases of the project (under Future Work), as this puts the overall effort into a clearer context. As presented, the Overview slide gives the impression the project (rather than this phase of a larger project) is complete.
- The identification of footprint reduction for stations in urban locations as a high priority is good, but it is questionable whether developing a reference station for urban sites is the most effective use of funding.

Project strengths:

- The topics addressed in this project (delivered gas versus on-site hydrogen production) and the related costs are important to the public sector and the investor community, and they are presented in a logical approach. The topics address station capital and installation costs for modular hydrogen stations and those based on central fill. With modular stations, higher station capital costs are offset by lower installation costs. This project began to address the cost of power for stations: buy off-peak and use grid load balancing. This project assumes the highest cost of hydrogen comes from a small station, and this is likely true. This project provides detail on a modular station design (300 kg in a 20-foot container) and points out that such designs/ systems have firewalls already built in, which could be a cost saver. The topics also include the cost of delivery, on-site storage, and on-site production and how these contribute to the cost of delivered hydrogen. The conclusion explains that replacing conventional station equipment with modular station equipment does little to change station economics or layout.
- The phased approach makes the effort easier to manage. The project makes it possible to compare trade-offs between conventional and modular hydrogen refueling stations and on-site production vs. delivery. The collaborations named in this project are appropriate to achieve project goals.
- The project contributes to a better understanding of different station designs and their strengths/ weaknesses.
- This is an effort to understand how stations are built and attempt to make this information publicly available.

Project weaknesses:

- There is a real industry value of the outcomes. The market continues to develop rapidly—every year the context is different—and this may speed up even more within the coming three years, making results obsolete. There is no concrete plan to share this tool at this point, but funding is needed to continue the effort.
- The following are project weaknesses:
    - This project covers up to 300 kg/day stations (to address today's station), whereas somewhat larger stations are planned: 360 kg/day. This seems like a very minor point, but stations are expected to increase in size further (this is proposed to be covered in a potential Phase III: 600 kg/day). Attribution to the station developer and the location of the station shown on slide 15 would have been appreciated, as the network is at its infancy and attribution would be great.
    - The topics of station cost and the impact of on-site versus delivered hydrogen are complex and difficult to present in 18 slides, at in-depth technical levels that advance the state of the art, and the presentation is somewhat weakened by this.
    - This project/presentation explains that the modular station costs decrease because of standards but does not explain how.
    - Although this presentation mentions that the modular station design uses less square footage, the problem is that the modular design cannot be changed in its configuration, by definition. A project

weakness is that a "factor" was not assigned to this issue. It could be that a modular station design does not fit into, potentially, as many as 30% of the potential gas station sites.
    - This project assumes that the highest cost of hydrogen comes from a small station, but this hinges on the ability of the station to operate at capacity, and that was not factored into this study.
    - No possibility of a double-decker station was included.
    - Plans to put the tool into the community were not presented.
- As the project identified new barriers and challenges of the current station infrastructure rollout effort, it would be helpful to see some future effort dedicated to working through some of those challenges. Liquid hydrogen was excluded from this phase of the project.

Recommendations for additions/deletions to project scope:

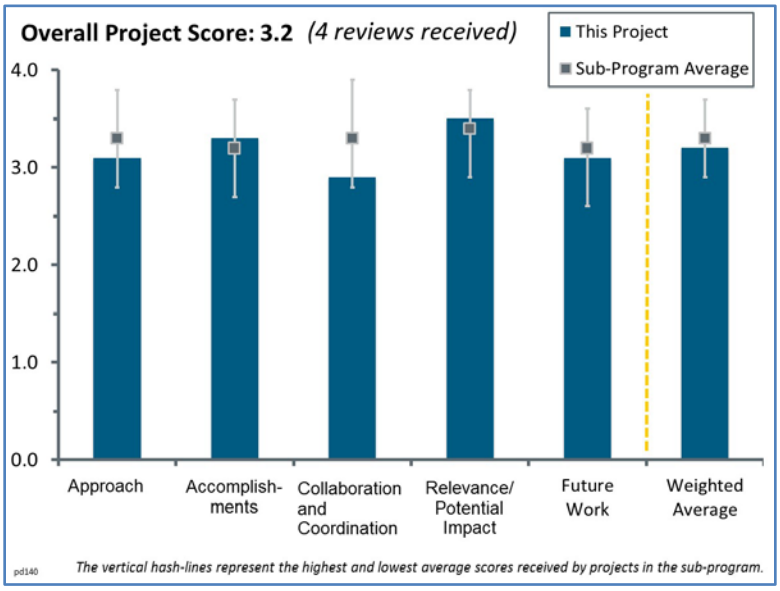
- Because this project is complete, there are no recommendations for additions/deletions to the project scope.
- Industry will be taking over, making its own decisions based on internal market assessments. Because this project does not contribute to the goal of $4–$7 per kilogram dispensed, it may be wise to discontinue this effort to find a more effective use for overall DOE funding.
- Liquid hydrogen delivery and storage compared to the alternatives has already been explored.

Project #PD-140: Dispenser Reliability

Christopher Ainscough; National Renewable Energy Laboratory

Brief Summary of Project:

Hydrogen fuel dispensers are a top cause of maintenance events and labor time at hydrogen fueling stations (HFSs). This project seeks to identify the proper balance between dispenser costs—both capital and operations and maintenance (O&M) costs—and performance. The project consists of three major tasks: (1) a technoeconomic analysis (TEA) of capital and O&M improvements to the chiller/heat exchanger, (2) reliability testing of dispenser components, and (3) development of an open-source and free hydrogen fueling model.



Question 1: Approach to performing the work

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

- Understanding the reliability of components is critical to identifying cost-limiting elements in an HFS. The TEA performed to date and the development of a "fill model" will prove to be very important tools/ suppliers of critical information on what needs to be addressed to cost-effectively advance HFS deployment. There is concern that the "hardware" testing of components by accelerated cycling of different components (such as valves and hoses) may yield misleading information. However, it may be the best that can be done at this very early stage of deployment, where actual field data are missing. The project is advised to use caution in the interpretation of results from the hardware part of this activity. There is a question about the need for 11 components per level, which results in an extraordinarily large number of components and tests, making this part of the project prohibitively expensive. There is also a question about the flow rates of 2–3 kg/min as a set of testing requirements. The SAE International J2601 standard is designed to fill a ~4 kg tank in four to five minutes, which makes the flow rate on the order of 1 kg/min. The rates specified of 2–3 kg/min seem high and require justification. The proposed modeling effort is very good and is needed to accelerate and verify fill protocols and characterize the behavior of the tank during a fill. This team is cautioned not to develop protocols but instead to use this validated capability to identify critical elements in need of attention during a fill, as well as those elements that can be identified in need of pre-normative research and development that currently result in rate-limiting and/or safety issues during a fill.
- The project approach is sound. Seeing the fuel dispenser as being the weakest link is a surprise; the compressor would be expected, followed by the refueling hoses and breakaways. The dispenser is a simple device and should have a high mean time between failures (MTBF). Having a low MTBF indicates a poor or incomplete design, poor construction process, or both. A visual design inspection of the device might be useful in identifying poor practices. Failures ranked by MTBF (frequency and life), repair cost, and lost time due to repair are metrics that would help prioritize development activities. These parameters could be compared to field data.
- The work plan shows a comprehensive approach to improve reliability and cost.
- Comments are provided by task:
    - The goal of Task 1 is to determine how dispenser reliability affects the fuel cell electric vehicle customer. The project's approach is to use a TEA to determine the cost to the consumer. However, cost is only part of the customer impact; station availability and uptime are more important in the early stages of introduction, as there are few stations, there is little redundancy, and the fuel costs

are included in the vehicle payments. It is not expected that the TEA will fairly reflect the impact on the consumer.
  - The goal of Task 2 is to improve the reliability of the dispenser. The project's approach is to test component reliability. The methodology outlined for the tests seems flawed. Component reliability testing is best done when there are known failures that are identified and tested. Putting the entire system through a cycle test will only identify failure modes exposed in the test. However, it is not clear that cycle failure is even a mode of concern, nor whether the team has done a failure mode and effects analysis (FMEA) or root cause analysis (RCA) of any field failures or designs to identify the key component weaknesses. Without doing this evaluation on each component and then testing against each failure mode, this testing is not likely to be a robust mechanism to identify or improve components.
  - For Task 3, it was not made clear how a fueling model would improve dispenser reliability. There are questions about what problem is being solved or investigated by the model and what kinds of solutions can be identified.

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.

- This project is fairly young but is still making good progress. The TEA performed to date has already highlighted critical cost elements of HFS operation.
- The project improves knowledge about reliability issues and cost.
- The accomplishments in Task 1 are interesting. It appears that the dispenser includes a heat exchanger and chiller. These should be off-the-shelf items. The coolant could be glycol water. The working fluid could be ammonia at these temperatures. Low-volume usage of the chiller will offer lower operational cost savings. The costs are actually heat loss. There is a question about what (if anything) is being done about proper insulation. For stations with liquid storage, this issue might be resolved by better energy utilization integration. For temperature, the team could have two tap points from the evaporator, one at ~–100°C and one at <10°C below ambient temperature. The taps would be controlled by a mixing valve (motorized three-way ball valve) controlled by the fuel temperature measured at the dispenser. This would potentially exchange the chiller and heat exchanger for a one-inch valve. Using the storage boil-off as a pressurizer would also help. Sandia National Laboratories (SNL) had been referencing valving reliability standards for its safety analyses. This may be a source of information. The project should also investigate whether the military generated a mechanical handbook comparable to the Department of Defense, Military Handbook: Electronic Reliability Design Handbook (MIL HDBK 338B), and whether American Petroleum Institute has a similar document. Shell might know.
- It looks like very little progress has been made to date on any of the tasks.

Question 3: Collaboration and coordination with other institutions

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

- Industry participation includes relevant stakeholders and therefore improves the relevance of the analysis.
- Collaboration is pretty good on the "want" side, but it seems lighter on the "component manufacturers" side. The component manufacturers could help with failure modes, MTBF, and remove-and replace-costs.
- Without a partnership with the component manufacturers, it is not obvious how this project can succeed. Component manufacturers have the ability to provide design reviews/information and perform RCA/FMEA on their components. This information could then be used to design experiments to validate/test the failure modes and component designs.
- Critical elements missing in the collaboration are the Joint Research Centre (JRC) and Argonne National Laboratory (ANL). JRC developed a very impressive tank fill model (fully validated computational fluid dynamics), and ANL developed station behavior (optimization) for the station side.

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 project, if done carefully, could be very helpful in reducing component costs. It could also validate the product safety testing standards.
- This project has the potential to provide significant insight and identify critical elements of HFS operations.
- This project can contribute to the needed balance between performance and cost.
- The project topic is very important, as dispenser reliability is becoming a bigger and bigger issue. It is not clear, however, that the national laboratories should be taking this on as a task. Instead, it should be led by the component manufacturers (perhaps partnering with the national laboratories to develop test capabilities and standards).

Question 5: Proposed future work

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

- The proposed future work is good. This project really needs to help define, measure, and/or establish (to the best of its ability) the MTBF of components.
- The future work plan develops project-specific and reasonable testing and modeling to improve and validate the interim results.
- The proposed future work seems fine.

Project strengths:

- This topic area is important. Testing and performance standards are needed to ensure component reliability at stations. This project has a broad, involved team from across the national laboratories.
- There is a good focus on TEA and modeling to understand the weak/strong points for HFS operations.
- The project improves knowledge about cost and reliability. It demonstrates excellent collaboration with industry stakeholders.

Project weaknesses:

- There is concern about the accelerated component testing. It is not clear that mechanical cycling will “mimic” the wear experienced in the field. Also, attention to MTBF as data needed for HFS deployment is lacking.
- The project tasks are not aligned with the goals of the project. Deliverables are not clearly defined, nor is it clear how the tasks will meet the deliverables. Partnerships with component manufacturers are needed.
- The project does not appear to be following standard reliability test protocols.

Recommendations for additions/deletions to project scope:

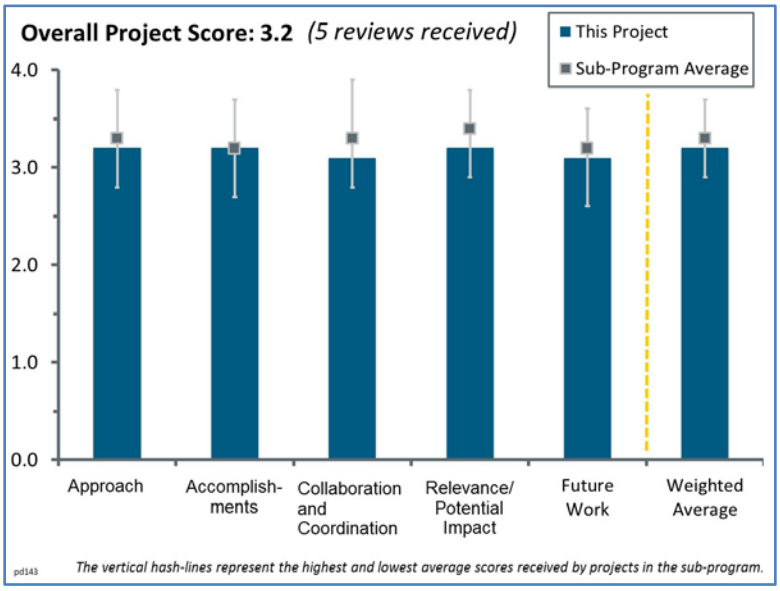
- It is recommended that the project work to keep the component reliability testing as representative to actual field data as possible and work to get the MTBF for these components. The project should not get caught up in designing filling protocols but should instead develop defensible, validated models of the station/ vehicle fill physics. It is recommended that the project team with JRC and use SNL to provide validation of the fill model physics proposed as part of this project.
- The project might target a set of recommendations for future component development and station design.
- It is recommended that the project benchmark against other reliability programs (perhaps “Energy Star”).

Project #PD-143: High-Temperature Alkaline Water Electrolysis

Hui Xu; Giner, Inc.

Brief Summary of Project:

This project aims to develop high-temperature molten alkaline electrolyzers with improved electrical efficiency and reduced cost. The electrolyzer will operate in the temperature range of 300ºC–550ºC. Specific project tasks include (1) development of porous ceramic oxide matrices, (2) incorporation of molten hydroxide electrolytes into the porous matrices, (3) selection of anode and cathode catalysts, (4) assembly and testing of single cells, (5) construction and testing of a 1.8 kW electrolyzer stack, and (6) system and economic analysis.



Question 1: Approach to performing the work

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

- The approach is innovative. It is not clear whether the researchers will be able to achieve the high efficiency that they are claiming. The system will be extremely corrosive, and it is not clear whether the seals they are planning on using will resist the corrosion in this corrosive environment. If the researchers can develop the seals, there is a good chance they will be successful in demonstrating a device, but it is not certain if they will achieve the high efficiency they are targeting.
- The focus is on developing new high-temperature alkaline electrolysis. The approach to do this, developing tape-cast porous membranes, is appropriate and well designed. The primary issue is that system efficiency needs to be addressed. Using an OH^- transport membrane means that $2H_2O$ in the feed stream has to be heated to the vapor phase for every hydrogen molecule produced. These 2X O_2^- or H^+ transport membranes, given the high heat of vaporization, are a potentially high energy penalty for the process.
- Intermediate-temperature electrochemical technologies can benefit from moderate heating requirements and therefore facile kinetics. However, they then require advances in electrolytes to remain stable, often necessitating solid electrolytes while adequately conducting ions. The proposed solid/molten hydroxide conductors seem like a logical approach with clear barriers and a pathway forward, in a space where often $[O]_2^-$ or H^+ conductors are the norm.
- The approach to performing the work is feasible. However, it is not clear that the lessons learned and experience obtained from molten carbonate fuel cell systems (similar to this system) have been incorporated in this project.
- The approach to performing the work plan as currently defined is fine, but there needs to be more of a focus on identifying the key challenges and designing the work plan toward addressing these challenges.

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 accomplishments and progress are notable, especially given that the project started approximately only four months prior to the deadline for the DOE Hydrogen and Fuel Cells Program (the Program) Annual Merit Review slides. However, all of the reported information is structural. An upcoming challenge will be

the stability of the solid/molten electrolyte in a moist atmosphere, where the hygroscopic hydroxide materials will likely have an even larger dynamic restructuring. Another challenge may be the compatibility of layers and interfaces.
- This is a new start project that began only a few months ago. Given the short time that this project has been occurring, the team has made good progress. Ten-hour tests are not sufficient to state that the material is stable. Corrosion protection for the interconnects and cell assembly need to be demonstrated.
- This is very early (beginning in Quarter 2) in the project, but the Quarter 1 milestones appear to have been met, and progress made on other milestones was demonstrated.
- The project started only recently, and progress appears to be commensurate with the expenditures.
- Reasonable progress has been obtained since the project just started in January 2017.

Question 3: Collaboration and coordination with other institutions

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

- Few collaborators are mentioned, but the scope of the work seems to be appropriate for a small team, and the lead principal investigator has a strong and proven track record for advances in the DOE Fuel Cell Technologies Office (FCTO) Program.
- The project needs to add a partner with experience in designing new seals and corrosion protection films. Having an unpaid advisor, while helpful, may not give the support that is needed.
- It is important that the team include a company with ceramic manufacturing experience because the concept requires a ceramic matrix material with a very challenging set of attributes.
- Expanding collaboration with FuelCell Energy (not just on stack assembly) is strongly recommended.
- There is planned collaboration with ZIRCAR Ceramics and FuelCell Energy.

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.

- This has good potential to develop a technology that has a higher efficiency than the current state of the art. It is unlikely to achieve the >90% efficiency target, but it still has potential to achieve high efficiency. The durability of the approach is not clear and will need to be tested extensively.
- Intermediate-temperature electrolyzers can be game-changing for large-scale electrolysis applications, and it is good to see that DOE's Office of Energy Efficiency and Renewable Energy (EERE) FCTO is funding this work.
- If successful, the project has potential to reduce the cost of hydrogen production via electrolysis.
- The project fully supports the Program. It is too early to evaluate its impact.
- The previously stated thermal efficiency of OH^- transport membranes was concerning.

Question 5: Proposed future work

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

- Since it is early in the project, proposed future work is the majority of the project and appears to be well planned.
- The metric of 1.0 A/cm^2 at <1.5 V seems unnecessarily ambitious, but it would be impressive if it can be met. The reviewer looks forward to learning more from the efforts of this group.
- The presenter was asked a number of questions about the technical challenges associated with his approach, and the responses indicated that the presenter had not previously considered these challenges. It is important that a critical technical review is performed so that the challenges can be identified and the work plan adjusted so that these challenges can be addressed sooner rather than later (with a go/no-go decision based on whether these challenges have been successfully retired).

- The work plan is extremely aggressive. The team is underestimating the sealing challenges. Traditional seals will not work in this environment. New seals will be needed, and this development is not in the work plan. The efficiency tests are not in the plan until the Quarter 10. The project should have some intermediate milestones. The cost analysis should specify a hydrogen cost target using the Hydrogen Analysis (H2A) model. As currently planned, an H2A analysis is not required.
- Identification of risks and issues to provide background/justification for future work is lacking.

Project strengths:

- While it is unclear whether molten hydroxide conductors will be successful for this proposed application, and therefore this may be too high-risk for DOE EERE FCTO, the team and project are well proposed, and the project has a chance for very high impact.
- This is a clever idea. Giner, Inc., has a long history of success. The team has made good progress in the short time that the project has been active.
- This is a novel concept with the potential for reducing electricity costs associated with electrolysis.
- The project is developing a new approach to electrolysis using high-temperature alkaline membranes.
- The approaches are feasible.

Project weaknesses:

- The project is too early to speculate on many weaknesses, but critical aspects will be electrolyte and interfacial stability. The efficiency target seems to be a little bit of a reach. The project seems a little long, but the budget is appropriate.
- The seals are not sufficiently developed, and there is no mention of how sealing will be done. The team lacks expertise in developing seals. The project has not done a sufficient thermodynamic analysis to see whether the 90% efficiency target is feasible.
- The thermal (latent heat of vaporization) energy penalty of using an OH^- conductor for hydrogen generation is a weakness.
- There is insufficient consideration of technical challenges in the design of the work plan.
- The identification of risks and issues is lacking.

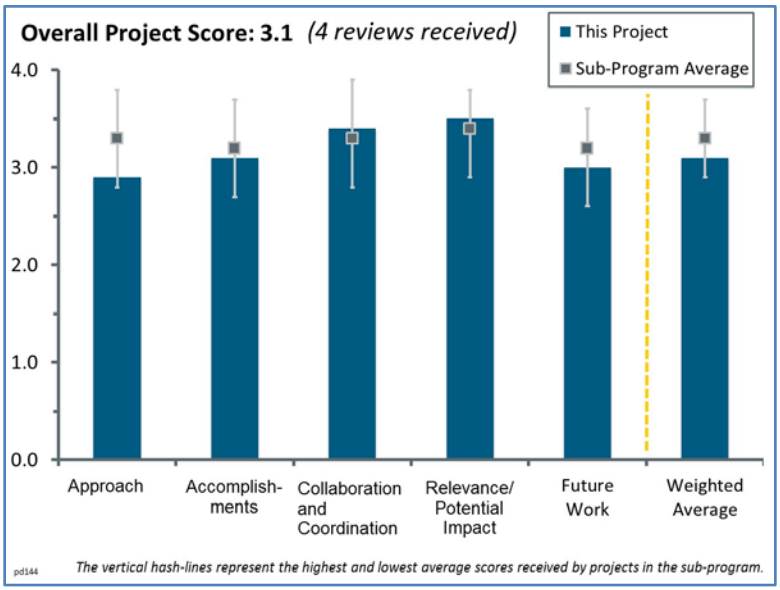
Recommendations for additions/deletions to project scope:

- The proposed historically low-temperature electrocatalysts seem to be warranted, but their stability at higher temperatures is likely poor. Evaluating electrocatalysts from the solid-oxide conductor field may be a better approach because it is unlikely that their kinetics at these reduced temperatures will be that poor.
- The researchers should do an H2A analysis now to see if they have a chance of achieving the cost targets. They should do a thermodynamic analysis to see if they can achieve the 90% efficiency (lower heating value).
- Recommendations include a critical technical review, identification of technical challenges, and a revised plan, as appropriate.
- Longer-term testing on material stability (only 10 hours to date) is recommended.
- The project should do a complete energy balance for the proposed process.

Project #PD-144: Multiscale Ordered Cell Structure for Cost-Effective Production of Hydrogen by High-Temperature Water Splitting

Elango Elangovan; Ceramatec

Brief Summary of Project:



The objective of this project is to develop and test a novel high-temperature electrolysis stack design with an advanced honeycomb-based cell architecture. The stack will combine advanced materials including stable electrode compositions for improved lifetime, a unique set of fabrication options, and cell performance improvements via the multiscale ordered cell structure design. The result will be a highly efficient advanced high-temperature water-splitting stack demonstrating a potential pathway to meet the U.S. Department of Energy Fuel Cell Technologies Office (FCTO) cost goals for hydrogen production. The ability of the stack to operate on intermittent energy sources, such as for energy storage and grid ancillary services applications, will also be tested.

Question 1: Approach to performing the work

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

- The solid oxide electrolysis cell (SOEC) approach to hydrogen production is excellent with potential high efficiency, especially when combined with a source of heat. Novel three-dimensional (3D) electrode structures have been proposed to address the gas diffusion limitations of conventional random porous electrodes. However, there is concern that 3D printing of a thick major structural support, versus a thin added layer, will not scale well or be cost-effective.
- Overall this seems like a reasonable approach, but there are a few concerns.
    - Scandium is rare and expensive. The team should calculate the cost and impediments to scale-up presented by Sc. Sc metal costs around $5,000/kg, and on the order of 10–15 tons/year are produced.
    - The goal is to operate at 1.2 V and 1 A/cm^2. That is about 0.3 V of overpotential, corresponding to an area-specific resistance (ASR) goal of 0.3 ohm-cm^2. However, the presentation mentioned an ASR target of 0.4 ohm-cm^2. There may be a disconnect here.

  Overall, this is a good team with an interesting approach. It is very early in the project, so there are not many results yet, as expected.
- Based on the proposed work plan, the approach is sound. Ceramatec is well positioned to be successful since the company is leveraging a considerable amount of previous work in this field. The concept has a number of extremely difficult technical challenges. Fortunately, the team is aware of these challenges and has identified approaches to address them. The following are the primary technical challenges for this project:
    - Achieving the required flatness of the multilayer structure will be difficult, especially as the area is increased to that required for stacks to produce meaningful hydrogen production levels.
    - It will be essential to reduce the resistance contribution of the honeycomb support layers. Ceramatec is aware of this challenge, and the team's calculations suggest it is possible. That said, experimental validation of the low support resistance levels is required.

    - The honeycomb support layer adds complexity to the sealing challenge, especially since it will be really difficult to make the unit cells with ultimate flatness.

  Future reviewers of this project should assess the progress toward retiring these technical challenges.
- The cells will be very complex to assemble. They need a flat surface for the seals, but the electrode fabrication will likely create a surface that will not be smooth and flat. This is a significant risk. The durability testing is not clear. There is no lifetime test, and there are no thermal cycling tests. The researchers need to set degradation standards and power and efficiency targets.

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.

- Ceramatec has extensive previous and ongoing work (e.g., the NASA Mars Oxygen In Situ Resource Utilization Experiment, or MOXIE), so the researchers could hit the ground running. Therefore, they are on track with their first milestones, although it is very early in the project.
- This is a new start, and the project has made good progress so far. The initial ink patterning is very encouraging.
- Progress is commensurate with project expenditures.
- The project is just getting started. There are no major results yet, as expected.

Question 3: Collaboration and coordination with other institutions

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

- This project is a collaboration between two highly capable research and development companies (Ceramatec and Palo Alto Research Center [PARC]) and an experienced technoeconomic analysis firm (Gaia Energy Research Institute). A high level of collaboration between Ceramatec and PARC will be essential for success, given that different manufacturing processes for the unit cell structure will be performed at two different facilities. Their respective process development work cannot be performed successfully without a lot of back-and-forth communication.
- There is collaboration with PARC on 3D printing and Gaia Energy Research Institute on technoeconomic analysis. The question will be how well PARC's 3D electrode process matches with the rest of the cell by Ceramatec.
- The team has well-defined roles and responsibilities. The team has the skillset to accomplish the tasks.
- The effort appears to be well coordinated with the partner organizations.

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.

- If successful, this project has the potential to reduce the cost of hydrogen production via electrolysis.
- High-temperature SOECs should play a significant role in the DOE Office of Energy Efficiency and Renewable Energy Hydrogen Production and Delivery sub-program.
- High-temperature water splitting is relevant to the FCTO.
- The team is targeting an aggressive and relevant cost goal of $2/kg of hydrogen.

Question 5: Proposed future work

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

- The team has established acceptable plans for achieving performance demonstration at the button cell level (the first go/no-go decision point metric) in the current project year. If feasible, some work aimed at retiring

technical challenges (mentioned previously) earlier rather than later would be good; some of these can be retired only after scale-up to larger cell areas is achieved (which is scheduled to be completed in the following project year).
- The proposed future work is consistent with the goals. The primary question is in the integration of thick 3D-printed structural support with the rest of cell manufacturing.
- The team appears to have a good plan for future work.
- The researchers need to include metrics for durability, power, efficiency, and an early, preliminary hydrogen analysis to show they have a pathway to achieve the cost target of $2/kg H_2.

Project strengths:

- A potential high-efficiency SOEC approach with unique architecture to address gas diffusion limitations is a strength.
- This is a good team and an interesting approach, with significant previous experience on which to build.
- This is an exceptionally strong technical team.

Project weaknesses:

- Scandium is very expensive and rare, calling into question whether achieving cost targets and scale-up is feasible. It is not clear that the ASR target is aggressive enough to meet the 1.2 V and 1 A/cm^2 goal.
- The project is aimed at an SOEC stack that will be extremely challenging to make, and it will take a lot of time and funding to prove ultimate feasibility.
- There is concern over how that thick 3D layer integrates with the rest of cell fabrication in large-scale manufacturing.

Recommendations for additions/deletions to project scope:

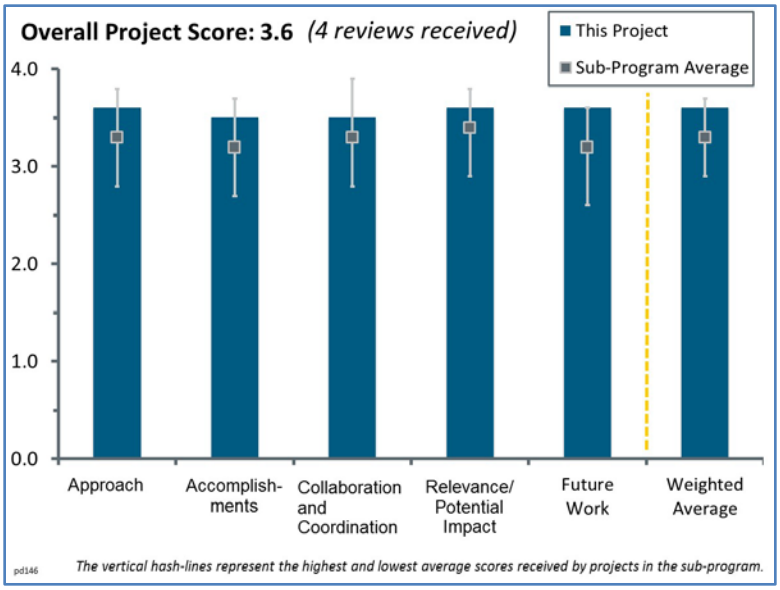
- Anything that can be done to prove ultimate feasibility of making the unit cell architecture with ultimate flatness and required large area should be done earlier rather than later in the project.

Project #PD-146: Advancing Hydrogen Dispenser Technology by Using Innovative Intelligent Networks

Darryl Pollica; Ivys Energy Solutions Inc.

Brief Summary of Project:

The primary objective of this project is to develop a robust and cost-effective system for dispensing and measuring hydrogen that further enables widespread commercialization of fuel cell electric vehicle (FCEV) technology. Key project activities include (1) development of robust sensor hardware and algorithms that improve accuracy based on empirical testing and enhanced meter temperature measurement; (2) development, testing, and demonstration of the use of dedicated short-range communications (DSRC) for use in vehicle refueling; and (3) simplification and cost reduction of flow control and hydrogen pre-cooling systems.



Question 1: Approach to performing the work

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

- In terms of dispenser build and flowmeter testing, this team is well on track and focusing on the right areas. The DSRC will rely on original equipment manufacturer (OEM) participation for success. More feedback from OEMs presented to reviewers in the project may shed light on barriers to commercialization that remain unclear. The overall approach is promising.
- The project team presented a concise overview and clearly presented the technology objectives (metering and communications). The team also focused on the economic benefit of the project to demonstrate the relevance. The team did a nice job with highlighting the challenges (i.e., thermal fluctuations) to be overcome and showed interim physical progress.
- The Ivys Energy Solutions Inc. (Ivys)-led team is doing an excellent job of managing this project and has brought some very good creative thinking to an area where improvements are very much needed.
- The project is well organized and focused.

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 accomplishments and progress toward project objectives are outstanding. The project team is correctly addressing known issues with dispensing and is using standard automotive equipment to improve vehicle communication systems. The progress is measured clearly, with specific targets for completion of critical milestones. Cost reduction progress deserves more focus.
- The project is doing nice work with customizing the Coriolis meter development for thermal stability, understanding the IEEE Wireless Access in Vehicular Environment (WAVE) protocol, and articulating the related economic savings from simpler nozzle designs. The concise project schedule for tasks and milestone completion is appreciated.
- The project was slow to start but is on track to accomplish its first phase successfully. The approach and selection of DSRC and Coriolis flowmeter were consistent with the DOE goals and objectives.

- Except for a contracting delay, the team has made good progress.

Question 3: Collaboration and coordination with other institutions

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

- The team seems to have a very close relationship with Rheonik, where they are providing technical assistance to overcome specific application challenges (i.e., thermal). The team is laying the groundwork to conduct field testing with Air Liquide and to advise on enclosures and dispensing locations. And naturally, the team is working with the National Renewable Energy Laboratory (NREL) to conduct testing. All are good steps at this point. To accelerate progress, the project should expand collaborations to include other dispenser organizations (e.g., First Element), work with Toyota or others on the WAVE protocol, and maybe add a software developer to develop a robust interface using the WAVE protocol.
- The partnership with ATLAS as a future industry collaborator for retail demonstration is a project strength. The partnership with Rheonik as a supplier represents a focus on a major flowmeter supplier in the industry and has already demonstrated value with controlled testing of the meter and improvements to the meter that can make immediate impacts on operating retail fueling stations. Areas for improvement include finding an FCEV OEM partner to ensure the vehicle communications approach mitigates barriers to commercial adoption.
- The collaborative work with NREL, Air Liquide, and Rheonik has been excellent. This is a great project for Ivys and will help solve a major problem for Air Liquide. It appears that one result of this collaboration will be a lower-cost, higher-performance dispenser design.
- Collaboration with other industry partners and contractor share funding are excellent.

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.

- Metering hydrogen is going to be very important, and helping to drive down the cost and maintenance of a dispensing station is also very important. The project team presented a convincing presentation on the efforts to achieve these goals.
- The results of this project are in line with the DOE Hydrogen and Fuel Cells Program goals and objectives. Its potential impact will be significant in the development of the infrastructure.
- This project is critical in making an effort to develop lower-cost fueling station dispensers.
- The alignment with the objectives of the DOE Fuel Cell Technologies Office Multi-Year Research, Development, and Demonstration Plan deserves a high rating. However, the project results to date do not seem to show a pathway toward significant cost reduction of either dispensers or pre-cooling systems. The costs of the systems remain a significant barrier to FCEV adoption, and while this project does show a clear pathway toward reliability and accuracy improvements, it does not appear to address cost in a significant way. The project approach shows a low-volume dispenser cost target of $150,000 and a high-volume cost of $40,000, yet those targets are relatively unsubstantiated. Furthermore, these costs are for a dispenser with single dispensing capability, while dispensers with simultaneous dispensing capability are showing increasing demand in the market, as station developers try to maximize the fuel delivered in small footprints and during times of the day with high vehicle traffic.

Question 5: Proposed future work

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

- The project clearly identifies the future work and predicts future barriers and mitigations. They are well documented and well presented, especially the scheduled targets. The project success relies on a strong and dedicated team at Ivys and at the NREL Hydrogen Infrastructure Testing and Research Facility. More

information about the project team individuals and their roles/responsibilities would be appreciated in future reviews.
- The plan for the rest of the project is well thought out and should proceed as soon as possible.
- The proposed future work is in line with the DOE goals and objectives.
- The schedule summary was clear and concise. It seems possible that the research team will fall behind schedule once they immerse themselves into the technical details of both the metering and the protocol. A schedule reassessment should be performed by the fourth quarter of 2017 and published.

Project strengths:

- The project team is very strong on technical competence, with a deep history of relevant work in the area of hydrogen vehicle fueling systems. The collaboration with the manufacturer of the Rheonik flowmeter is excellent, as this is the primary flowmeter used in this application. The focus on using the DSRC communications link for SAE International (SAE) J2799 is quite novel and has great potential.
- Project strengths include overall subject matter knowledge, having a retail fueling station partner, the flowmeter manufacturer partner, flowmeter testing and development, and the organization and schedule.
- There was excellent technical knowledge and coordination with other partners. The ability to adjust and meet the schedule and milestones is a strength.
- The project is focused. It has good partners who are engaged. The team has set clear goals (technology and economics).

Project weaknesses:

- No project weaknesses were noted.
- Cost reduction for the dispenser requires focus. Cost reduction on the precooling system received limited focus in the project. The lack of an FCEV OEM partner is a weakness. There is a lack of focus on (or at least presentation of) other component reliability (i.e., dispenser valves, sensors, and fueling hardware).
- There is concern about being adequately resourced to achieve the milestones presented. This should be reassessed on a quarterly basis.

Recommendations for additions/deletions to project scope:

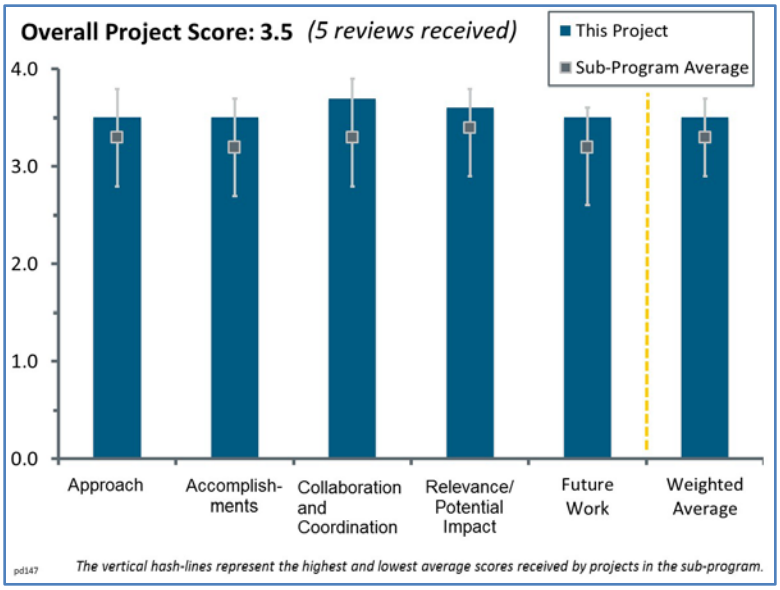
- In terms of additions, the proposal for using DSRC for SAE J2799 needs to be brought to the SAE Fuel Cell Interface Task Force for comment and feedback. In terms of deletions, the suggestion to use a radio frequency identification tag to identify the car at the dispenser nozzle is not needed, as the pressure pulses at the beginning of the SAE J2601 fueling process will identify which vehicle within wireless range is connected to the nozzle.
- More information should be included on project roles and responsibilities of the greater project team. For example:
    - Include more information on dispenser component selection and reliability considerations (e.g., valves, sensors, and fueling hardware). Detailed science on these components may be outside the scope of the project, but proper selection of these components and design of the dispenser to comply with fueling protocols will be critical to project success.
    - Include more information on cooling system design and design/reliability considerations (same reasoning as above).
- The project should continue to design and develop DSRC and improve the Coriolis meter. Field validation should be performed in the next steps.
- More collaborators would be a good addition.

Project #PD-147: Economical Production of Hydrogen through Development of Novel, High-Efficiency Electrocatalysts for Alkaline Membrane Electrolysis

Kathy Ayers; Proton OnSite

Brief Summary of Project:

This project aims to produce a high-performance anion-exchange membrane water electrolyzer (AEM-WE) with low-platinum-group-metal (PGM) electrodes. The anode ionomer will be optimized for improved stability and conductivity, and the lead ruthenate catalyst synthesis process will be refined and transferred to a commercial partner. A 10- to 12-cell prototype AEM electrolyzer system will be designed and built.



Question 1: Approach to performing the work

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

- The approach was excellent. Three barriers, a catalyst, an ionomer, and a cell stack design were selected toward the overall goal of system cost reduction for the AEM technology. Each barrier was addressed in a logical and feasible way, and the technical accomplishments show this.
- Developing AEM electrolyzer systems with low-PGM-loading catalysts is an attractive approach to bringing down the costs of producing hydrogen from water electrolysis. This is a well-rounded team with expertise in catalysis, ionomers, membranes, devices, and systems that is well equipped to achieve project goals.
    - There was very little information on the AEM itself and whether this is an impediment to achieving system goals. Some comments after/during the presentation imply that the membrane does indeed limit operating conditions (i.e., current densities, likely temperature) far below what an AEM electrolyzer would operate at in an economically competitive commercial system. There could be concern that these ionomers and catalysts are being developed/optimized for non-optimal conditions. Rotating disk electrode (RDE) experiments could still be useful in this respect.
- Using AEM is a significant step toward achieving the 2020 electrolyzer system capital cost targets of $0.5/kg and $300/kW by significantly reducing membrane electrode assembly (MEA) and flowfield material costs. Identifying the highest-activity catalyst and transferring the catalyst synthesis to Pajarito Powder's (Pajarito's) commercial facility enables validation in a prototype stack test.
- This project addresses the barriers for capital cost of electrolyzers by examining lower-cost anode catalysts and looking at alkaline technology that allows for the switch from Ti to stainless steel plates. The anode catalysts still contain PGM (Ru).
- This work is in its first year of the project, and it seems like a new direction that has little overlap with other projects in the U.S. Department of Energy's Office of Energy Efficiency and Renewable Energy portfolio. However, while the need for advances in this research and development space is high, it is unclear whether the approach taken is a pathway toward this. The rationale for alkaline electrolyzers is sound, but it is unclear how beneficial ruthenates are as a cost-effective alternative to acid-stable catalysts. A plot suggests that even replacement of Ir with Ru would constitute a large cost savings (>80% of polymer electrolyte membrane [PEM] oxygen evolution reaction [OER] cost). It is unclear how this affects the total cost as used in the motivation slide.

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 has made progress toward the project goals and has demonstrated lower-cost lead ruthenate catalysts with good activity. The OER catalyst composition has been selected, and scale-up has been initiated at Pajarito. Initial scaled-up material provided similar activity to the small batch material made at Washington University in St. Louis (WUSTL). The ionomer degradation rate for the block copolymer appears to be lower than that for the Tokuyama AS-4 ionomer. However, durability testing has not been sufficient in length yet. The stack and system concepts have been developed.
- There has been excellent progress by Pajarito in scaling up and reproducing/refining the catalyst synthesis. Promising initial ionomer stability should be validated in longer-term tests (>1,000 hours). This project integrated all new materials in an operational test in a small single cell.
- The plan is clear and feasible, although it could be argued that a 0.5 A/cm^2 current density is modest in comparison to the operational current density in acidic electrolyzers. Notwithstanding, most tasks are close to their expected targets by their expected due dates. The results show promise for this design in terms of stability, efficiency, etc., and this work will answer key questions about the suitability of the development of alkaline electrolyzers.
- It appears that the researchers are making good progress in hitting all of their targets, although the presentation did not include many technical performance milestones (e.g., target device efficiency at a given current density, stability level at given current density).
- The project is on target, and the milestone progress was clearly indicated.

Question 3: Collaboration and coordination with other institutions

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

- The collaborations on this work are not as large as other projects, but the team is well justified and appropriate. Each team member brings unique expertise that is required for the overall success of the project, and therefore, a full team effort is required to advance this work. While a few hiccups were noted toward the beginning, the work and team seem to be on track now.
- The collaboration is appropriate in that each partner adds value. There are not too many partners, which can muck up some projects. Georgia Institute of Technology's and WUSTL's roles of providing ionomer technology are fine for this project. Ideally, it would be nice to see a company filling that role, as a company would stand to gain more value than a university should the technology be commercialized. However, it could be that no company exists to provide that step in the value chain at this time.
- There was good interaction between the partners, with WUSTL's catalyst being scaled up by Pajarito and then characterized at WUSTL and validated at Proton OnSite in cells and stacks.
- There seems to be good collaboration between the partners on this project. A single-cell device was tested that used components/contributions from all of the team members.
- Collaboration between partners appears to be working well. Hand-off of the synthesis process to Pajarito for scale-up has been successful.

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.

- The project is relevant and supports DOE goals to reduce costs of hydrogen by reducing costs of alkaline membrane electrolyzers. The reduction in PGM by going to lead ruthenates and the reduction in plate cost by going to an alkaline system offer large potential benefits. However, durability of these systems must be demonstrated, and alkaline membrane durability remains an issue.

- This project is a completely critical step toward overcoming the barriers associated with acidic electrolyzers and historical alkaline electrolyzers. Even the current project goals to replace one PGM (Ir) with another (Ru) have the opportunity to be hugely impactful in terms of the cost of hydrogen, but more detail on this should be shown in terms of technoeconomics.
- This project certainly shows progress toward lowering costs and expanding hydrogen production options. Demonstration at scale will be important if it will be relevant to reaching the desired 60 million tons/year.
- The overall approach sounds reasonable to achieving the stated goals, although it is hard to tell without more detailed technoeconomic analyses.
- High-performance, lower-cost electrolyzers can help meet the hydrogen production cost targets for small-scale distributed hydrogen production.

Question 5: Proposed future work

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

- The down-selections have been rational, and the project plan moving forward seems sound and logical. Although risk mitigation was not directly spelled out, the lead principal investigator has a proven track record of succeeding in this type of project and organizational structure. Therefore, the risk in this work is low, but potential for impact is high.
- The proposed future work is appropriate and addresses remaining questions. The cost estimates will provide guidance for potential production. Testing in 1.0 L/min stack prototypes is appropriate and will provide needed system durability data as well as identify problems.
- The future work is well laid out to complete the remaining milestones.
- The proposed future work tasks all seem reasonable.
- Within the project, the future work is laid out very clearly. It is not clear what the next step would be beyond the close of this project.

Project strengths:

- This is an interdisciplinary and experienced team with the combined expertise to achieve the stated project goals. Exploring multiple catalyst and ionomer compositions helps reduce risk. The combination of component/RDE experiments and cell/MEA testing is also valuable for deconvoluting the degradation/loss mechanisms.
- This work is much needed in the DOE Fuel Cell Technologies Office portfolio and specifically for hydrogen production. It is to be hoped that DOE funds additional efforts in this project space by seriously considering the near-term potential.
- The electrolyzer experience of Proton OnSite and the staff's knowledge of system requirements for the market are major strengths of the project.
- This is a strong team with good capabilities for catalyst scale-up, in-stack validation, and prototype system design.
- This project is very focused on barriers and goals and has very good technical understanding.

Project weaknesses:

- It is hard to find any weaknesses with this project, really. Beyond the scope of the project, it is not clear how the technical lessons may be translated to other projects, but that is not really the fault of this project, just something to think about.
- It is a little concerning that many tests are being performed at low current density (e.g., 200 mA/cm^2). That would make this technology uncompetitive. If this technology cannot get up to 500 mA/cm^2 or greater, it seems that it will have trouble competing with conventional alkaline electrolyzers in terms of cost, since they can, in principle, make use of the same catalysts but with simpler device structure. It is important that controls be included that are based on state-of-the-art RuO_2 and/or ionomers to serve as a basis for comparing the novel catalysts/ionomers being developed through this project. While present in some cases, it is not evident that these are always being included.

- The replacement of one PGM with another is admittedly not a long-term solution for a game-changing technology, and it would have been nice to see more in terms of the future plans for use of first-row transition-metal electrocatalysts. If not, it is unclear why they were not of interest.
- Ionomer and catalyst long-term stability and durability have yet to be confirmed.

Recommendations for additions/deletions to project scope:

- The project should consider moving up the technoeconomic analysis tasks to better inform the necessary operating performance metrics (e.g., current density, temperature, and lifetime).
- The project should consider adding a side-by-side performance comparison of AEM with PEM, using the new AEM technology developed in this project.
- Parallel assessment of state-of-the-art first-row transition-metal electrocatalysts, such as $Fe_xNi_yO_zH$, would be a useful addition to the project scope.
- If short-term tests continue to show promising results, longer-term in-cell tests (>1,000 hours) should be performed.