2016 — Hydrogen Production and Delivery
Summary of Annual Merit Review of the Hydrogen Production and Delivery Program

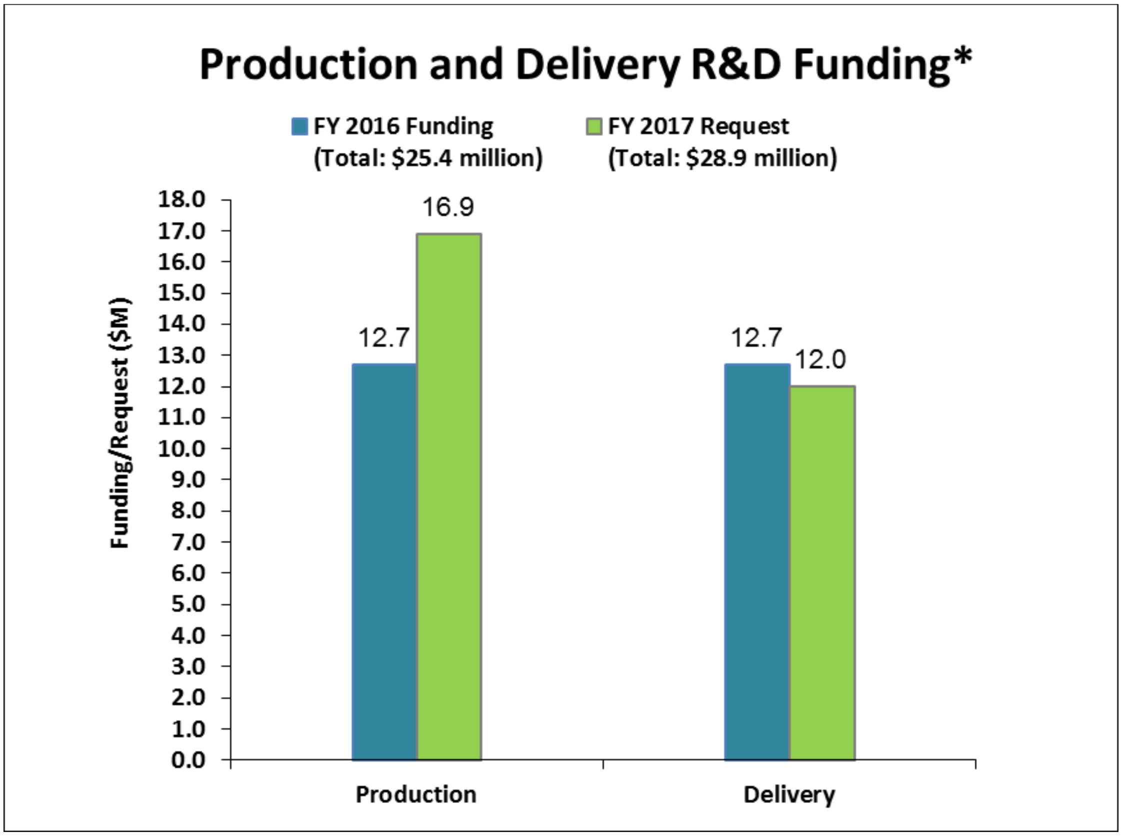
Summary of Reviewer Comments on the Hydrogen Production and Delivery Program:

This review session evaluated hydrogen production and delivery research and development (R&D) activities in the U.S. Department of Energy's (DOE) Fuel Cell Technologies Office (FCTO) in the Office of Energy Efficiency and Renewable Energy. The hydrogen production projects reviewed represented a diverse portfolio of technologies to produce hydrogen from renewable energy sources, as well as an overarching analysis of hydrogen production pathways. Production project sub-categories included thermal and thermo-electrochemical conversion of bio-derived feedstocks, advanced water splitting, direct solar thermochemical (STCH) and photoelectrochemical (PEC) water splitting, biological hydrogen production, and hydrogen production pathway analysis. The hydrogen delivery projects reviewed included R&D for low-cost, reliable delivery technologies (pipelines and tube trailers), hydrogen fueling station components (compression, storage, and dispensing), novel liquefaction technologies, and strategic delivery techno-economic pathway analysis.

The reviewers recognized the Hydrogen Production and Delivery program as focused, effective, well managed, and having a clear strategy to achieve DOE goals and objectives. Reviewers commented positively on the relevance of delivery projects to both near- and long-term priorities and needs, and they commended the achievements of production projects in innovative systems design and use of techno-economic analysis. Reviewers encouraged more detailed studies of both near-term and future costs of production and delivery technologies, especially those at lower technology readiness levels (TRLs); reviewers also recommended continued leveraging of relevant industry-partnership opportunities and greater collaboration with other domestic and international government agencies. Continued and strengthened emphasis on industrial collaboration and stakeholder engagement was strongly recommended.

Hydrogen Production and Delivery Funding:

The fiscal year (FY) 2016 appropriation for the Hydrogen Production and Delivery program was $25.4 million, with funding distributed approximately evenly between hydrogen production and hydrogen delivery technologies. The production portfolio funding focus in FY 2016 was on advanced water splitting pathways such as STCH, PEC, and other electrolysis technologies, as well as the addition of new fermentative hydrogen production projects competitively selected in FY 2015. This emphasis will continue in FY 2017 with the addition of a newly organized advanced materials consortium and additional high temperature electrolysis work. FY 2017 planning is based on a $28.9 million budget request (~$16.9 million apportioned to production R&D). The consortium approach to identifying and utilizing resource nodes to accelerate development of renewable hydrogen production pathways will continue to be developed. The delivery portfolio emphasis in FY 2016 was on reducing station technology costs, such as those associated with storage vessels and dispensing hoses, and on advanced compression and liquefaction technologies for the mid- to long-term. The portfolio also includes funding for hydrogen fueling infrastructure analysis. This emphasis will continue in FY 2017 with ~$12.0 million apportioned from the budget request with an additional focus on the reliability of critical components, such as forecourt compressors, meters, and other dispensing equipment.



* Subject to appropriations, project go/no-go decisions, and competitive selections. Exact amounts will be determined based on research and development progress in each area and the relative merit and applicability of projects competitively selected through planned funding opportunity announcements.

Majority of Reviewer Comments and Recommendations:

Twenty-nine projects were reviewed, receiving scores ranging from 2.30–3.50, with an average score of 3.15. The scores are indicative of the technical progress that has been made over the past year in the hydrogen production and delivery R&D portfolio.

Production Projects

Hydrogen Production Pathway Analysis: One project was reviewed in the area of hydrogen production pathway analysis. The project received a score of 3.1. Reviewers recognized the high-impact and usefulness of the techno-economic analyses performed by the project team, as well as the team’s expertise and experience in this area. They would have liked to have seen more information from relevant industry partners in the development of the case studies, though they acknowledged the challenges presented by the low TRL of the dark fermentation and solid-oxide electrolyzer cell (SOEC) cases presented. Reviewers would also like to have seen further details on the technical and economic assumptions of the analysis. They specifically expressed concerns about the aggressiveness of some of the assumptions presented for the future cases studies.

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 decrease the platinum group metal (PGM) loading of the proton exchange membrane (PEM) electrolysis cells; efforts to completely eliminate PGM materials and enhance membrane stability in anion exchange membrane (AEM) electrolysis cells; efforts to understand electrolysis under variable electrical load; and the development of SOECs operating at extremely high current density. Reviewers praised the progress on low-PGM PEM and non-PGM AEM electrodes while maintaining performance and durability compared to commercial baselines with higher-PGM electrodes. They also praised the progress on the demonstration of current density exceeding $3A/cm^2$ in SOEC cells. They commented that success in these projects offers the potential to achieve significant reduction in the capital cost of electrolyzers, which is critical for technology introduction on a larger scale. All projects in the category have completed or are on track to complete major milestones. Reviewers recommended continuing work to better understand mechanisms of the electrochemical processes in the electrolyzer stack, further optimize operating conditions for each technology, and better evaluate the long-term hydrogen cost ramifications of technologies improvements.

Bio-Derived Feedstock Conversion: One project was reviewed in the area of bio-feedstock conversion, with a score of 3.0. Reviewers commended the project team for its progress in developing and testing innovative catalysts and carbon sorbent materials as well as for its strong, multi-partner collaborations. They expressed concern over the significant operational challenges facing the swing reactor system integration and control. The reviewers also questioned the assumptions of the reactor's fuel flexibility and bio-oil feedstock costs in terms of the system's performance and potential to meet the hydrogen production cost goal.

Biological Hydrogen Production: Two projects were reviewed in the area of biological hydrogen production and these received an average score of 3.16. One project is focused on microbial processes, pairing fermentation of biomass with Microbial Electrolysis Cells (MECs) to produce hydrogen from the waste effluent. The other is an in vitro process using isolated enzymes to convert starches to hydrogen. Reviewers commended both projects on their accomplishments and making progress toward their goals, but they expressed some concern about overall feasibility of biological conversion of biomass to hydrogen. The reviewers noted that the in vitro biosystems project was pursuing a novel pathway and commended the progress toward protein expression and peak production rates. Reviewers questioned the practicality of the project, noting that the techno-economic analysis presented was mostly qualitative and may not have considered all costs. Reviewers complimented the fermentation and MEC project for its effective partnerships. Reviewers questioned the value of studying parallel feedstock pretreatment methods in the fermentation project and requested additional details and quantitative results for some tasks.

PEC Hydrogen Production: Three PEC projects were reviewed, receiving an average score of 3.33. Reviewers felt that projects in this area were well aligned with DOE objectives, with a focus on developing the most-promising PEC material systems and prototypes, such as those based on highly efficient III–V semiconductor and chalcopyrite thin-film materials. Projects were rated highly for advancing the efficiency of PEC devices through improvements in the interfaces between materials. Reviewers highlighted the use of qualified collaborators who contributed unique expertise and capabilities to the projects. Recommendations for future work included placing stronger focus on increasing the stability and durability to meet upcoming milestones.

STCH Hydrogen Production: Three projects were reviewed in the area of STCH hydrogen production projects, with an average score of 3.0. Two of the projects focus on two-step, metal-oxide-based reaction cycles, and the third addresses a hybrid sulfur (HyS) reaction cycle, which includes an electrolysis step. Reviewers praised the innovative approaches and achievements in all three projects, including the following: (1) combined experimental and modeling efforts for materials discovery; (2) reactor design and prototype builds that will allow for hydrogen production demonstrations; and (3) the screening and characterization of advanced membranes and a system design that allows for 24-hour operation for the HyS cycle. Reviewers also recognized key challenges in all three projects, such as their potential to meet the hydrogen cost goal and, again, they recommended continued efforts on the techno-economic analysis for these technologies. They specifically recommended including realistic capital costs; better definitions of assumptions; and an effort to establish materials discovery approaches, testing protocols, and reporting standards for the STCH community.

Delivery Projects

Hydrogen Delivery Techno-economic Analyses: Three projects were reviewed in this area, with an average score of 3.1. These projects included updates of the Hydrogen Delivery Scenario Analysis Model (HDSAM) to version 3.0, analysis of the energy and cost of hydrogen precooling systems, and techno-economic analysis of the cryo-compressed delivery pathway. Projects were praised by reviewers for their technical robustness and relevance to DOE objectives. Recommendations were made for projects to collaborate more closely with industry partners and to more clearly explain the basis of the assumptions presented.

Hydrogen Delivery Technologies: One project was reviewed in the area of hydrogen pipelines, receiving a score of 3.1, and two projects were reviewed in the area of hydrogen liquefaction, receiving an average score of 3.4. The pipeline project was praised for technical robustness (testing in high-pressure environments, studying welds, accounting for residual stresses, and testing multiple types of fiber-reinforced pipeline). Reviewers suggested that the steel pipeline project collaborate more closely with industry to ensure real service conditions are represented. Reviewers praised the liquefaction projects for using novel approaches that could have significant impacts on hydrogen production costs, as well as their accomplishments to date. Suggestions included increasing industry collaboration, characterizing cost, and focusing on scale-up potential.

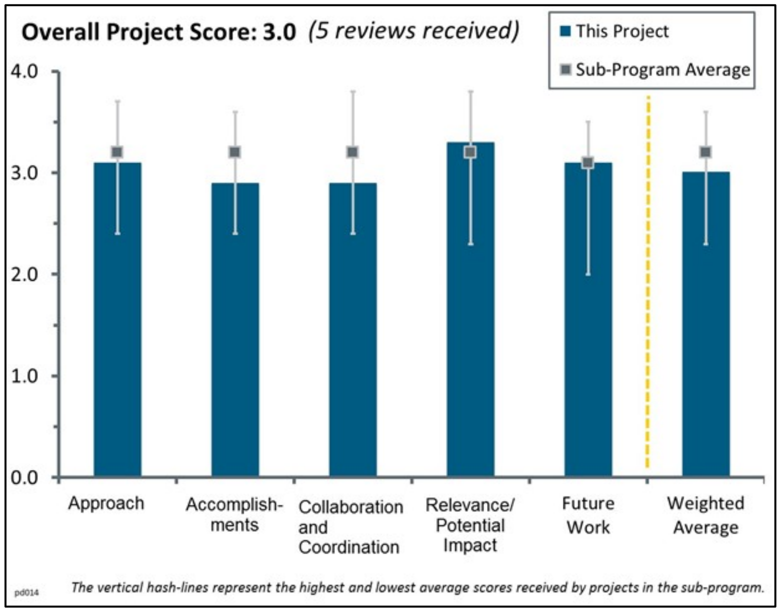
Hydrogen Fueling Station Technologies: Nine projects were reviewed on hydrogen dispensers, compression, storage, and station operation. They received an average score of 3.0. The projects on dispensing hoses were praised for their technical approach and relevance, which included providing accelerated cycle life testing for key components of the hydrogen station, including fittings. The projects on compression technologies were praised for the potential to lower station costs and improve reliability if successful. Reviewers expressed concern over the project's thermodynamic efficiency compared to incumbent technologies and suggested that the team obtain guidance from experts in electric motors and compression. Reviewers expressed concern with the ability of the steel concrete composite vessels (SCCV) to compete with incumbent storage technologies. They commented that the design of the high-pressure (875 bar) SCCV has not been optimized for fatigue life and that the costs of transporting and installing the SCCV technology may negate any cost savings associated with the vessel. The hydrogen storage project on wire wrapping Type I vessels was commended for its approach and promising burst test results. Reviewers suggested that the cost of this technology be assessed in greater detail. The project on tube trailer consolidation strategy testing and development was praised for its potential for near-term cost reduction, but reviewers suggested that additional industry collaboration would be helpful.

Project #PD-014: Hydrogen Delivery Infrastructure Analysis

Krishna Reddi; 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 (DOE)-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.

- The approach appears to cover all the major delivery methods. However, the presentation did not make clear how the cost impact of variations within a delivery method was considered. For example, tube trailers today carry hydrogen at 200–350 bar. Some gas merchants have or are planning 400–450 bar trailers as well. It is unclear whether the scope of the H_2A Delivery Scenario Analysis Model (HDSAM) is meant to model transportation costs of hydrogen or the entire delivery cost, i.e., including the capital and operating expenses (capex and opex) of the hydrogen refueling station (HRS) itself. The project references "Delivery Infrastructure Analysis"; however, it appears only transportation costs are considered. In considering the total delivery cost of hydrogen from terminal to end user, a particular delivery method can affect the capex and opex of the HRS itself. This is a strategy being considered by gas merchants that control the transport and HRS portions of hydrogen delivery. Again using the example of tube trailers, by using higher-pressure tube trailers, the cost of terminal compression and trailer capex would increase, but capex and opex of the HRS would decrease—a smaller or more efficient compressor could be used, owing to higher pressure to the inlet of the compressor. Another approach could see the tube trailer being used as the "low bank" in a cascade system in addition to supplying hydrogen for compression to the "high bank," reducing the amount of storage capex for the station (in other words, moving HRS capex to transportation capex). This would have advantages in the near term when a singular capex in the trailer would increase the asset utilization while decreasing capex costs across multiple HRSs.
- The overall approach with detailed scenario modeling is reasonable and appropriate. The challenge is to properly account for variable input data and uncertainty of assumptions with regard to key parameters. The investigators are continually refining the model to improve on the quality of the output.
- The model is still limited by the use of cost estimates vs. after-turn-key cost checks. The source of the cost reduction factors is unclear (this was answered late in the presentation but is still not very clear or, at a minimum, is not transparent or substantiated). The presentation should make the data sources more transparent and referenceable/reviewable, and allocate some additional time to ensuring the appropriate or reasonableness of the inputs. The work is limited by the lack of data. The approach needs to evolve to accommodate these limitations to better inform DOE's research direction. It is hard to have confidence in the results.
- How far industry really uses the results for their research, development, and demonstration strategy is questionable.

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.

- The team made good progress in introducing various technology states into the model and further tuning the existing consideration factors.
- Version 3.0 of the model appears to be a step change improvement over the previous version (2.3). Significant progress has been made in examining various scenarios and options since the last Annual Merit Review. However, some of the results presented appear to be unclear and confusing. It is unclear whether sufficient data were available to project the component cost trends presented. Proper representation and better clarity would be helpful. The results are shown to project delivery cost for the long-term, high-volume case via all the three options—gaseous tube trailer delivery, liquid hydrogen (LH2) delivery, and pipeline delivery—to be about the same at $4/kg. It is not clear how this is possible. Some pathways are just not feasible or practical for a given scenario; e.g., a tube trailer cannot deliver to multiple large stations at 1,000 kg/day capacity (slide 8). It is not clear whether the tube trailer is assumed to be at current 200 bar pressure or at 500 bar or some other pressure—that will also have an impact. The baseline assumptions appear inconsistent; e.g., while cost comparisons show HRS capacity of 1,000 kg/day, the emissions results show capacity of 500 kg/day (slide #13). There are inaccuracies in the data presented that should be corrected. Market penetration numbers are different in different slides (2% vs. 10%), which makes it confusing to understand and compare the results. The conclusion on slide 17 does not seem to be consistent with the graph shown, if it is for the 700 bar case.
- Both main delivery pathways (gaseous hydrogen (GH2) tube trailer and LH2 truck) increased by roughly 10%. This would not be considered a "small increase," as mentioned on slide 8. It is not clear what the consequences are or how this is fed back to industry. Results on slides 14–16 are not very surprising. Slide 17 shows an overall trend in the wrong direction. Again, the consequences and how this fed back to industry are unclear.
- The model, or the results as they were shared, is limited. The lack of sensitivity and uncertainty consideration limits the usefulness to use for a key objective in identifying cost drivers. The large price differences between HDSAM 2.3 and HDSAM 3.0 should serve as a warning and lesson on the dependence of discrete price quotes.

Question 3: Collaboration and coordination with other institutions

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

- Good collaboration efforts and partner participation are indicated. Additional input from component manufacturers, gas suppliers, and station operators would be helpful.
- Collaborations exist but do not seem to have addressed concerns.
- As of the presentation, there is a huge lack of industry collaboration. It is not clear how much industry players such as Linde, Air Liquide, and Air Products are contributing. Maybe the project could elaborate more on that in next year's 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 **3.3** for its relevance/potential impact.

- This is a critical activity. The results should help determine key cost factors on which to focus as well as suggest commercial feasibility of different pathways under different conditions. The results presented make an impact on outsiders' views of the feasibility of hydrogen fuel cell vehicles. Therefore, it is important that the data are truly representative and that the results are accurate.
- The work being done is necessary to understand the current and projected costs in the hydrogen infrastructure space.

- The project is of high value for FCTO to set priorities and cost and performance targets.
- The work, as it is presented, is too high-level.

Question 5: Proposed future work

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

- Everything seems to be covered.
- The future work addresses essential key parameters and updates. The addition of a cryo-compressed pathway would be useful.
- Future work captures some important areas in which data or information is needed to improve usefulness of the modeling. However, the proposals seem focused more on precision than on accuracy.

Project strengths:

- With so many hydrogen transport technologies being explored and considering the variations within those technologies, the project team has tackled a challenging problem with good success. The approach appears to be sound—just further honing of the model is needed.
- Strengths include a strong modeling background, well-thought-out selection of pathways and corresponding parameters, and deep understanding of fundamentals and key issues.
- The project is trying to focus on empirical data collection and expert/industry input.
- The project provides an in-depth analysis tool for FCTO.

Project weaknesses:

- If the researchers have collected information on the range of costs, both from the production/equipment side and regional/local issues, it seems to have been condensed into a single value rather than used as a range of values (i.e., 90% range and median or mean). Without that range, it is not clear how a practitioner should or would use the model to evaluate an actual station design. This was effectively brought up in the prior year review, and it does not seem to have been addressed. Data sources, references, etc. need to be more transparent. At a minimum, a summary of the input data needs to be provided. The reliance on a single individual for the cost reduction values is concerning, especially since there is little to no evidence provided on the validity.
- Data for some parameters are insufficient, and there is a lack of consistency of assumptions and clarity in presentation of results.
- As of the presentation, there is a lack of direct industry involvement.

Recommendations for additions/deletions to project scope:

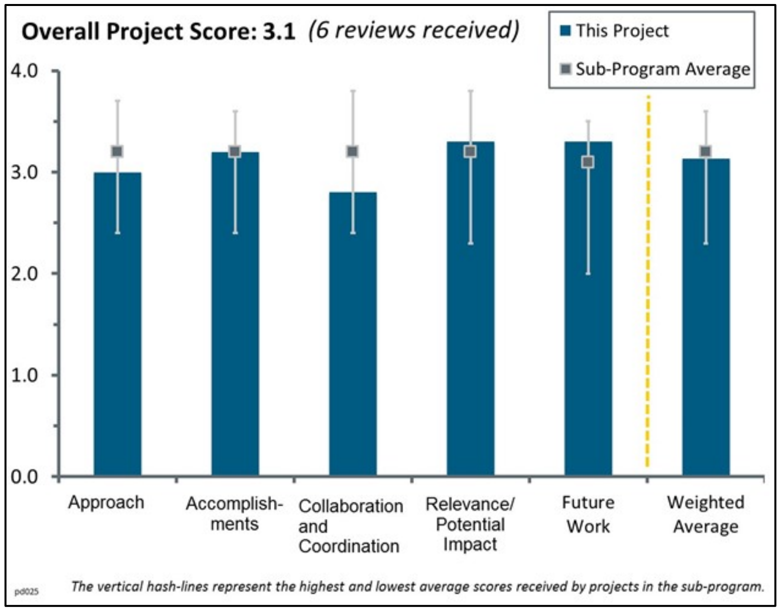
- It would be helpful to add sensitivity analysis and show range bars indicating uncertainty. Additional realistic scenarios and refinement of current pathways with technological advances and improved understanding as well as data availability would be helpful.
- Output next year needs to emphasize the range of real-world costs and emphasize the range of outcomes.
- Regular reviews with industry should be added.

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 enable deployment of high-strength steel for hydrogen pipelines to facilitate cost reductions. Specific goals are to (1) demonstrate 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 **3.0** for its approach.

- It appears that the original schedule and scope of work are being followed according to plan. The approach and methods proposed continue to be followed in the early phases of this work.
- The experimental approach followed by Sandia is the best there is. Assessing the fatigue crack growth resistance of low and high strength welds is the safest way to go if we are to increase pipeline strength to reduce cost. In addition, the proposal that crack growth through base material, the heat affected zone, and weld be investigated under constant stress intensity factor range (see slide 12) is also a well-thought-out and targeted approach to save time and resources. However, there was no information about the National Institute of Standards and Technology (NIST) approach on predictive modeling, nor about the Oak Ridge National Laboratory (ORNL) work on promising microstructures for enhanced hydrogen resistance.
- In general, the approach was sound. The principal investigator (PI) discussed that cyclic loading is the core difference between station pipelines and regular industrial pipelines. It did not seem that the PI performed cyclic testing, and this prevented the project from being rated a 3.5 for its approach.
- The approach is mostly centered on quantifying the effect of the microstructure on the fatigue performance and crack propagation in hydrogen gas environments. The team focus on samples extracted from different regions near and at the welds is well-thought-through. However, it is not clear how the team will utilize the data from the experiments, or how the data will help in developing the models to predict pipeline behavior as a function of microstructure. The presentation gives a sense that the approach is mostly empirically based, with no significant feedback between modeling and experiments.
- The results are communicated to relevant code committees, but the approach does not seem to include a review process with industry.

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 demonstrated that friction stir welding (FSW) and gas metal arc welding (GMAW) welds experience almost identical hydrogen-induced acceleration of fatigue crack growth. This is an interesting result by itself, as it holds promise for higher-strength pipelines. The identification of crack pathways for specimen preparation and subsequent testing is also a well-thought-out accomplishment, indicative of a systematic plan.

- Consistent results prove the testing protocol is good and repeatable. The protocol has a great ability to provide results that can be used to calculate wall thickness using measured crack growth laws.
- This project started less than a year ago. The team has already conducted experiments in both air and in hydrogen for a couple of different pipes.
- The preliminary microstructural evaluations, specimen preparation, and techniques are being conducted according to the proposed plan. Future tests appear to be on schedule.
- Promising basic findings have been achieved, and good testing facilities built up. There is no transparent documentation of potential and achievements compared to DOE targets.
- Well-defined standard development.

Question 3: Collaboration and coordination with other institutions

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

- Good, clear collaboration with industry and national labs.
- The collaborations between ORNL and NIST have been clearly articulated. The extent of the involvement of the Colorado School of Mines (CSM) and ExxonMobil is not yet clear. It is not clear if their roles will increase in years 2 and 3.
- The collaboration with partners appears to exist, particularly with ORNL. However, it is yet to be seen how effective collaboration with CSM will be. The Gantt chart references NIST, but the work scope appears to be from CSM.
- Collaboration with ORNL holds promise because of ORNL’s capabilities in assessing weld microstructures. The reviewer could not comment on the collaboration with NIST because their approach was not outlined in the presentation.
- On slide 14, it is mentioned that the industry collaboration means that they (in the past) supplied pipes. It is unclear if the collaboration is limited to that or if there are review loops with industry. It is unclear how project results are fed back to industry.

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

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

- Quantifying microstructure-fracture relationships in hydrogen is very important to aid in the development and validation of physics-based predictive models. It is clear that this has the most potential impact.
- The project can be impactful on cost reduction of pipelines. In addition, the project has the potential to impact understanding of weld response to hydrogen-induced fatigue. The profession’s understanding is not very advanced in this area
- The understanding and relevance of work to program objectives are excellent and well-aligned. It is expected that the results of the project will be relevant.
- The opportunity to use high pressure pipelines is good, although somewhat down the line. New and expensive infrastructure would be required, along with expensive and lengthy permitting.
- There is no transparent documentation of potential and achievements compared to DOE targets.
- Currently, composite overwrapped pressure vessels (COPVs) provide much more relevant promise than these pressure vessels for storage of hydrogen.

Question 5: Proposed future work

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

- Reviewing new generation steel is a long-term possibility and is a needed and good approach.

- This project started nine months ago, so the bulk of the work proposed has yet to be completed. The reviewer feels the work appears well-planned and understood, and believes the PI is well-prepared to complete the scope of work.
- The proposed future work seems adequate towards the goals of the project. However, some emphasis on quantifying the role of microstructure on crack initiation in hydrogen would also be of significant importance for the predictive models.
- The reviewer assumes the use of Gleeble aims at developing specific microstructures that will replicate the transitioning of the microstructure from the weld to the baseline material. This is a nice approach, but no details have been given as to whether this replication of the microstructure is relevant to that in real-world welds.
- Everything seems to be covered.

Project strengths:

- The capabilities and tests planned appear to align well with the original proposed scope.
- Good systematic approach and proposed way forward.
- Well-thought-out and relevant in the long term.
- The involvement of Sandia and ORNL.

Project weaknesses:

- None.
- No weaknesses were identified.
- Nothing has been outlined on the contribution from NIST, and only limited information regarding the contribution from ORNL.
- No benchmark to DOE targets. What is the potential contribution of the project? Industry collaboration.

Recommendations for additions/deletions to project scope:

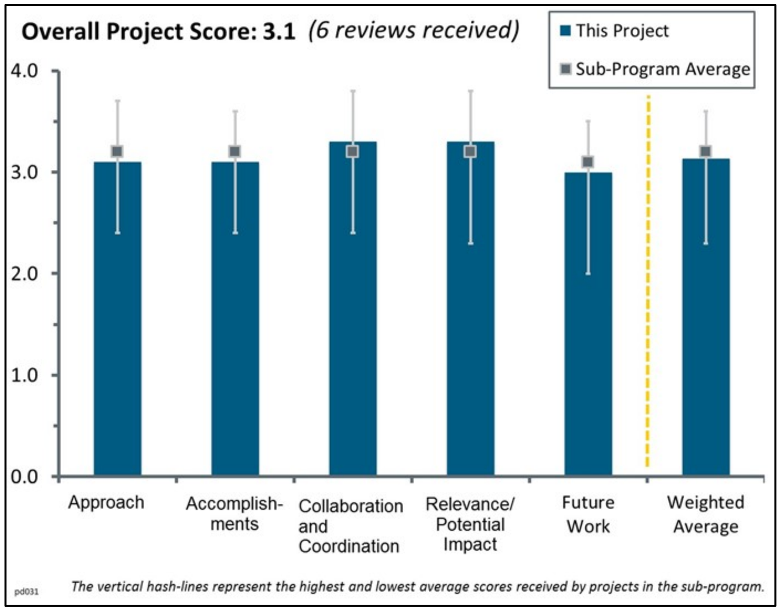
- The project demonstrated that FSW and GMAW welds experience almost identical hydrogen-induced acceleration of fatigue crack growth. This is an interesting result by itself as it holds promise for higher strength pipelines. However, this conclusion needs to be explained mechanistically based on the microstructures involved. In fact, this conclusion may change depending on the frequency used at testing. It could be that at lower frequency and higher R ratios, the higher strength weld can be more susceptible to hydrogen accelerated fatigue crack growth. In addition, the project should test the threshold stress intensity factor range for hydrogen-induced accelerated fatigue. The reviewer believes the two thresholds for the low and high strength welds will be found to be different. It is the reviewer’s understanding that acicular ferrite microstructures are the most resistant to hydrogen-induced failure. This is a general belief that to the reviewer’s knowledge has not been tested experimentally against fatigue crack growth. It is unclear that Sandia and ORNL plan to explore this issue.
- Include some efforts targeting the role of microstructure on crack initiation.
- It is recommended that the role and relationship of CSM with NIST be more clearly identified. Related to this, having CSM identified in the Gantt chart seems appropriate, as it seems their participation is significant and on the same scale as NIST.

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 as well as 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 **3.1** for its approach.

- The approach is well organized, and the objectives address Barriers G, I, J, and L well. The project does a good job at evaluating the effect of variable operation with renewable energy systems (photovoltaic [PV] solar and wind) on stack durability. The hydrogen savings through the variable drying and the direct current (DC) balance of plant (BOP) power that will be explored is also a positive aspect of the project.
- There is excellent integration of hardware demonstration with industry collaboration.
- The approach is relevant, including investigating effects of variable loads and variable versus fixed drying, and how both affect efficiency and hydrogen purity; however, the project scope seems somewhat small.
- This seems to be a well-thought-out approach. Slide 3 is not appropriate; it states that renewable hydrogen is necessary “to make a significant impact” on greenhouse gas (GHG) emissions. Coal-based integrated gasification combined cycle (IGCC) and carbon capture and storage (CCS) can make significant reductions in GHGs: in the electrical sector, hydrogen from IGCC and CCS would reduce GHGs by a factor of 12.8 compared to a coal-ST plant and by 5.8 times compared to a natural gas combined cycle plant. In the vehicle sector, steam methane reformation (SMR) to produce hydrogen for a fuel cell electric vehicle (FCEV) reduces GHGs by 22% compared to a gasoline hybrid and by 52% compared to a conventional gasoline internal combustion vehicle. These reductions are significant. The project should be careful not to let the perfect (renewable hydrogen) be the enemy of the good (SMR hydrogen).

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 experiments showed that variable and constant drying show no major differences in terms of degradation. Hydrogen savings were demonstrated through the variable drying approach. These contribute to the reduction of costs of hydrogen produced from renewables, which contributes to the target of $2 per gasoline gallon equivalent by 2020.
- Demonstrating that electrolyzer performance is not degraded by variable inputs is a very good achievement.

- A project cannot do much better than this one did; all milestones in fiscal year (FY) 2015 and FY 2016 were complete, and future milestones are on track. Also noted is a new advance in which dried hydrogen was fed to linear actuated valves to control hydrogen loss and DC/DC converters were used to run pumps. There were significant hydrogen savings from the variable drier. The only major concern is that specific Multi-Year Research, Development, and Demonstration Plan (MYRDDP) targets were not explained in great detail. Also, the high silica content found in the stack water should be addressed in the next iteration.
- Multiple parallel investigations show considerable progress. The team seems to draw relevant and insightful conclusions from data as opposed to just reporting numerical results. Analysis of decay rates seems to show a wide range of results depending on stack and testing protocol. It is not clear that meaningful conclusions are drawn from the testing (or can be drawn from the data). Replacement of the constant hydrogen dryer losses with a variable loss (due to a variable valve) is a clear improvement. What is not clear is why Proton Onsite has not done this already on its own. It seems like a basic systems integration and optimization task. Examination of the all DC BOP architecture is a task well suited to the National Renewable Energy Laboratory (NREL) with consultative input from Proton OnSite.
- Quantifiable DOE goals for this project were not apparent, but the importance of performance assessments to advance general DOE goals is understood.

Question 3: Collaboration and coordination with other institutions

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

- Clear work involves Xcel Energy, Proton OnSite, Giner, and others at NREL.
- Collaboration with Proton Onsite seemed particularly beneficial with respect to evaluating electrolyzer degradation.
- The partners in this project all have valuable expertise and seem to be working well together.
- The project is teamed with very appropriate and experienced 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 **3.3** for its relevance/potential impact.

- The project focuses on tasks well aligned with DOE goals. Understanding and tracking real-world stack decay rates is vital to the long-term success of electrolysis. BOP reliability and loss minimization is also vital to DOE goals.
- This project is essential to enabling production of large quantities of renewable hydrogen from solar PV and wind.
- This project can facilitate an increase in the share of renewables by providing storage opportunities, as well as grid services through frequency regulation. The hydrogen savings and increased efficiency through the DC BOP can also contribute to DOE goals.
- It is unclear to what extent this work supports progress towards MYRDDP targets because of the terse description of indicators and the lack of tie-in to those targets in the slides.

Question 5: Proposed future work

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

- The proposed future work is essential to showing the potential of this off-grid approach.
- A future focus on power regulation and variable voltage integration with renewables is appropriate.
- Emphasis on direct coupling to renewables is appropriate.
- Planned advances include a voltage measuring device for each cell in the stack so that voltage can be measured at any point in time. One additional barrier is that MATLAB is needed for the full computational analysis, yet the project does not have money for it.

Project strengths:

- The project is evaluating DC/alternating current (AC)/AC/DC, which is how the grid would supply power, and not DC/DC, which would be the case with a standalone integrated renewable source.
- The project is addressing the barriers very well and has been well organized. The partners are experienced and have very good expertise. The innovation potential with all DC BOP components is very good.
- NREL is well suited to analyzing the long-term collection and analysis of data and is uniquely qualified to conduct variable power load testing and modeling.
- The collected data could be of value to various stakeholders in fine-tuning their energy storage designs.
- There is good cooperation with the electrolyzer manufacturer.

Project weaknesses:

- The reviewer did not see any specific weaknesses.
- It would have been better if the original hours of the first two stacks were known.
- The project should make sure to critically assess the feasibility plan of the proposed research and development and expound on how it relates to MYRDDP targets.
- The decay rate data appear to be quite variable, with insufficient analysis to discern the causes of the variation.
- The project is not deriving mechanistic understanding that would be needed when translating the results to industrial scenarios.

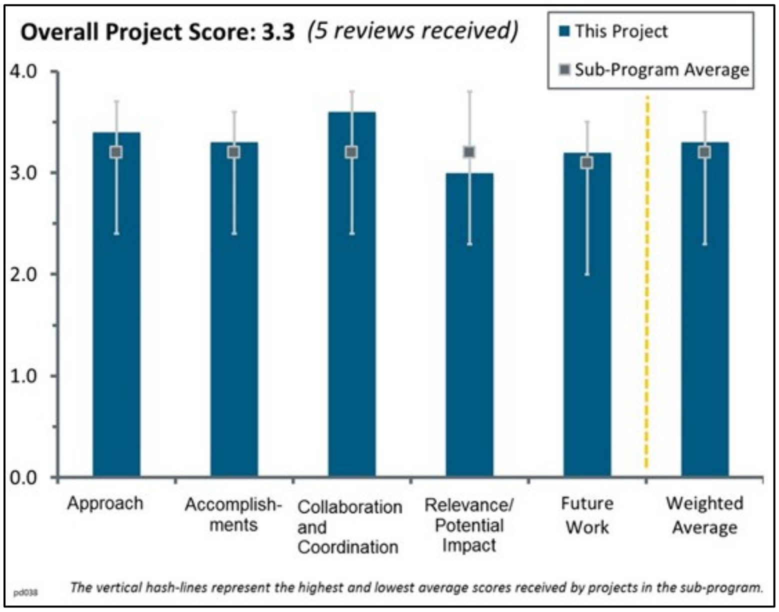
Recommendations for additions/deletions to project scope:

- The project should make the data publicly available so that different users can come up with their own conclusions.
- It is not sufficient merely to measure and quantify performance. That is a necessary and appropriate first step but must be followed up with additional analysis to draw conclusions from the data.

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 techno-economic 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 (IL) pretreatment for biomass processing. Task 3 develops and applies genetic tools to modify metabolic pathways aimed at improving hydrogen molar yield. Task 3 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 **3.4** for its approach.

- The team of experts, from multiple national labs and universities, has significantly improved hydrogen production and production rate while reducing production costs through advancing multiple experimental components, including the bioreactor batch process, use of DMR processing of corn stover, and the double elimination of the two pyruvate pathways.
- The approach is sound. The group is making progress on the stated aims while also addressing potential barriers and challenges.
- The engineering of *C. thermocellum* to overcome the hydrogen molar yield barrier seems reasonable. The knockouts are a good approach to change hydrogen production. The engineering of the microbial electrolysis cell is innovative and complements the genetic work nicely. The reasoning for using IL-derived sugars is not clear.
- Fuel Cell Technologies Office (FCTO) goals for biomass to hydrogen are identified well and addressed by this project.
    - Taking advantage of the National Renewable Energy Laboratory's (NREL's) pretreatment expertise to leverage Bioenergy Technologies Office (BETO) funding is laudable.
    - It is not clear that the necessary increase in solids loading for the DMR stover is achievable without significant advances in reactor and process engineering. Utilization of xylose by *C. thermocellum* is likely to be diauxic, so incorporating this into the already problematic high solids, long retention time fed-batch fermentation will be a large process engineering challenge.
    - It is not clear what the ILs task adds to this project. Increasing the IL tolerance of *C. thermocellum* is interesting but no analysis was presented to say that ILs would be a better pretreatment fit for this process than DMR or acid pretreatment. In fact, given the BioEnergy Science Center's (BESC's) extensive work with acid pretreatment and *C. thermocellum* as well as *C. thermocellum*'s tolerance to inhibitors produced during this pretreatment, it is not clear that DMR is superior to acid pretreatment for this process.

- In Task 3 it is unclear how novel the mutations are completed or how they compare to the work done at BESC on this organism. Overexpression of Hyd2 seems like a productive area of research, and the approach here is good.
- Task 4 seems promising in several aspects, including stainless steel fiber felt (SSFF) replacement. Utilization of the effluent from NREL is a good tie-in process-wise, but the fate of the more problematic compounds here seems potentially difficult (e.g., lignin monomers) but is a good strategy for the organic acids.

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 has come very close to meeting the hydraulic retention time (HRT) target for hydrogen production. For it to become commercially viable, the concentration of hydrogen will need to improve substantially. This may be accomplished through adding the elimination of ethanol production, which would require further funding to complete.
- Given the FCTO goals, this project seems to be making progress and is meeting milestones, although it seems that a lot of the most difficult steps are proposed for the following years (e.g., increased sugar/solids loading).
- The group has had a number of accomplishments. This is a new project that has not been previously reviewed. It was stated that this work is leveraging previous work. It was unclear which results (if any) presented were from previously funded work. Having reviewed presentations by this research team in the past, there were some findings that seemed similar to past work, although this may have been for a different system.
- The engineering of *C. thermocellum* in order to overcome the hydrogen molar yield barrier seems to be going very well. The knockouts of the lactate and formate pathways seem to be going well and both show an impressive change in hydrogen production. Assuming this scales up well, this mutant could provide a solid supply of biohydrogen. The electrolysis cell has made improvements and appears to be on track and functional. The IL work shows that the cells are fairly intolerant to IL-derived sugars. The reasoning for using these sugars (which are pretty toxic to the bugs) is not clear and perhaps not as good as just using some standard acid pretreated or DMR hydrolysate. Trying to acclimate *C. thermocellum* to this particular hydrolosate, which is not widely used, seems to be a questionable effort in this project considering how low the technology readiness level of IL-sugar is compared to other cellulosic options. Exploring the usefulness of IL as a path to cellulosic sugars is definitely something that needs to be done, but this may not be the project in which to be doing it.
- The slide 3 table indicates a yield of hydrogen that is 29% (2011), 33% (2015), and 50% (2020) of the maximum theoretical yield per glucose equivalent from cellulose. These yields need to be supported by data indicating the time period to obtain this yield. The microbial electrolysis cell (MEC) production rate is given in the slide 3 table in volumetric units of hydrogen and is 1 L/L-reactor-day (2015) and 4 L/L-reactor-day (2020). This should be normalized to the dry weight of biomass used, not the volume of the reactor. The rate needs to be specified if initial rate (day 1) or peak rate (which days) or average rate (which days). The amount of energy input from the electrolyzer needs to be stated and the net energy yield of hydrogen produced needs to be stated. In slide 4, the percentages after treatment do not add up to 100% for pretreated corn stover and DMR treatments—this should be explained.
    - In slide 5, the average hydrogen rate in data shown as 200 mmoles/96 hours for 4 sequential feed cycles (days). It is unclear what portion of each component (there are three listed) in the DMR precursor is converted to hydrogen. It is unclear what portion remains and could be recycled. It is unclear what portion is lost. It is unclear how much cell dry weight is used relative to the DMR weight (5 g/d). It is unclear what the rate of cell replenishment is needed. Figures show that after four sequential feed cycles the rate of hydrogen production falls off significantly—this needs to be explained. The hydrogen production vs. time is not stationary. It is not clear that it is understood what is responsible for the growth and decay kinetics and how this affects future needs.
    - In slide 6, the bar graph shows no error bars and no indication of the number of replicates. These data do not meet the confidence standards of customary scientific evaluation. Medium costs data set refers to a small reactor volume and a single day trial. There is no determination of the medium

costs based on a multi-day trial at a scale applicable to the pilot scale. There is some conflicting information; the MOPS buffer is important for cell pH and fitness, but performance without MOPS results in not heavily diminished (i.e., less than 10% loss) hydrogen production. In slides 7, 8, and 9, which report on IL extraction of untreated corn stover, data is presented in such a way as to mask the results of conversion yields.

- Slide 9 indicates that all three ILs produced insufficient extraction of cellulose components. No clear conclusion was given. This appears to be a lost effort. It is unclear if this project was initiated based on solid theoretical or experimental expectation of high yield.
- In slide 10, task 3, mutagenesis, descriptions of the single gene alterations are missing. Comparisons to known mutant strains in other bacteria are not stated even though these gene knockouts are widely tested in other bacteria and yeast. Growth rates of the mutant cell lines are not given and should be included so the consequences of the genetic change can be assessed. Viability of the mutant cell lines in normal conditions encountered in the bioreactors over the course of useful lifetimes is not given. This section needs more in-depth characterization.
- In slides 11 and 12, relative hydrogen production rates are given for a single hour, but no description of the conditions required to observe this rate or longer periods is provided. Absolute hydrogen production rates are not given. Longer-term measurements and alternative conditions are not given. In slide 14, task 4, it is unclear why the data in the figure have such wide swings in current density. The data are presented without adequate description of the experiment. It is unclear what the rationale was for using a smaller cathode vessel volume. It seems that the rate of hydrogen ion production and subsequent saturation of the cathode with adequate hydrogen ion concentrations for hydrogen evolution reaction (HER) had a very strong impact on evolved hydrogen, but the reasons were not explained. It is unclear if the rate of hydrogen ion production varied due to aerobic/anaerobic, light/dark, or proton gradient conditions. These are important experimental controls that must be applied to give valid data. Non-Nerstian pH dependence and activity increase with alkalinity. System performance appears not to conform with any standard form.
- In slide16, it is unclear in task 4 whether the membrane is polymer electrolyte membrane (PEM) or anion exchange membrane (AEM). It seems it should be PEM. Regardless of which membrane was used, it is unclear whether there has been analysis of the off-gas and the electrolyte from the cathodic reduction of hydrogen ions to hydrogen. It is unclear if there is natural organic matter cross-over from the bio side of the device to the cathodic side of the device, through the membrane or otherwise.
- In slides 13–18, task 4, it seems that performance improvement upon decrease in cell volume goes in the opposite direction than that desired for a commercial process. The overpotential requirements for lower-cost stainless steel electrodes vs. platinum electrodes offsets value of this approach. Data are not explained. Engineering changes have resulted in minor changes in performance and in some cases within experimental errors.

Question 3: Collaboration and coordination with other institutions

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

- The partnerships are effective.
- Coordination is evident between NREL, Sandia National Laboratories, and Lawrence Berkeley National Laboratory.
- There are a number of strong and long-standing collaborations with both domestic and foreign investigators.
- If the IL task is to continue, more discussion between those teams on the feasibility and the direction of the work should occur. It is unclear that if *C. thermocellum* can be engineered to be tolerant to ILs whether the MEC can tolerate those concentrations.

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.

- It does appear that this project would lead to a process in which the hydrogen molar yield is higher than the current standard. The reduction of the growth medium cost is very promising and suggests that this bacteria could be generated cheaply. If this is coupled with an effective MEC, the ability to generate biohydrogen at reduced cost is there.
- The stated aims seem to be well-aligned with the DOE mission. There are a number of advances that have been made which may also be applicable beyond this work. Some questions remain about the long-term stability and viability of the engineered organisms.
- It is not clear how the project will ultimately meet the cost targets.
- Insufficient quantitative results to assess progress towards achieving benchmarks. Unclear timeline to achieve solar-to-hydrogen (STH) goals.
- Generally speaking, making hydrogen from biomass seems like a non-optimal use of the carbon in biomass. The one real advantage of biomass as a feedstock—aside from the fact that it is renewable—is that it contains carbon in organic form, which can be transformed relatively easily into easily transportable liquid fuels and chemicals. Because biomass is highly oxygenated, making relatively reduced hydrocarbon fuels necessitates the input of reducing power or the rejection of carbon dioxide, but this problem is even larger with hydrogen production in which all carbon must be rejected as carbon dioxide. Many technologies are looking at making organic molecules from sunlight, carbon dioxide, or water because of how central these carbon energy carriers are to the U.S. economy. Given that, the state of technology for electrochemical water splitting for hydrogen production, and the massively falling costs of renewable electricity from wind and solar, it is hard to see what role biomass to hydrogen could play in the renewable energy future.

Question 5: Proposed future work

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

- There is a clear path to complete the stated research goals within the time frame allotted.
- Task 1 and Task 2 of the future work do not seem to be synchronized. It is unclear why it is necessary to go back to the drawing board to a different feedstock.
- Techno-economic analysis (TEA) is qualitative, based on early stage technologies, and is highly approximate. Primary data lack error analysis and evidence of replicates. Assessment of near- vs. far-term prospects could benefit from greater quantification.
- Task 1: “Pretreatment” with a pre-made celluosome cocktail takes this project one step further from consolidated bioprocessing (CBP), as it will now include DMR and enzymatic hydrolysis, which is very similar to other proposed biological processes. That said, it will likely be necessary to achieve 175g/L loading, and thus achieve project milestones.
    - Task 2: The potential advantage of ILs over DMR for this process needs to be better articulated for this to be a useful task. Even if it is possible for *C. thermocellum* to grow with 10% [Ch][Glu] it is not clear why this would be the preferred pretreatment method without some sort of TEA or explanation of how this pretreatment is specifically better for *C. thermocellum*.
    - Task 3: This task seems slightly risky and hard to optimize but very worthwhile. Some metabolic modeling may be helpful in the redox balance optimization.
    - Task 4: Proposed work seems reasonable.

Project strengths:

- Mutations of *C. thermocellum* seem promising, and the targeting of hydrogenase 2 is a sensible approach. The engineering team has created an impressive MEC utilizing the engineered biocatalyst. It has the potential to innovate the generation of biohydrogen.

- Project strengths include improved technique to increase sugar production as well as the collection of all sugars in one fraction; deletion of competing pathways to increase hydrogen production; progress towards adaptation of organisms to 10% IL; and reduction in the amount of precious metals needed (use of fibrous felt instead of platinum).

Project weaknesses:

- Weaknesses include the potential long-term stability of the engineered organisms, both due to the deletion of the competing pathways and the over-expression of the hydrogenase 2 system.
- The program should consider if they would like to pursue either IL-derived sugar or more common acid-pretreated or DMR sugars. Task 1 under proposed future work focuses on optimizing *C. thermocellum* to consume DMR corn stover, yet task 2 focuses on adapting it to survive 10% IL. The project may be better served by simply narrowing down the type of sugar the investigators want to use.

Recommendations for additions/deletions to project scope:

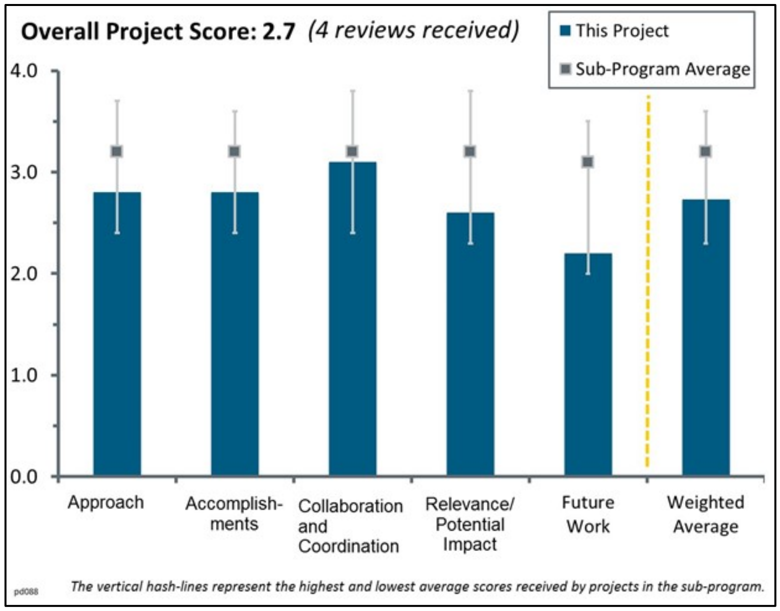
- The project team needs to conduct a go/no go and TEA around use of ILs in task 2 very soon.
- The team should seriously question the relevance of working with multiple sugar feedstocks, especially one that needs a lot of optimization.

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

Zhili Feng; Oak Ridge National Laboratory

Brief Summary of Project:

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



Question 1: Approach to performing the work

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

- The approach the researchers took to develop and demonstrate the feasibility of a steel–concrete composite vessel for high-pressure hydrogen that meets DOE technical and cost targets has been well-planned and well-executed. The only concern is whether the choice of steel–concrete composite is the most appropriate approach. While the principal investigator (PI) did explain his viewpoints on utilizing experience from concrete industries, it is not clear that there have been any efforts by the team in assessing other viable options/designs.
- The initial project concept was interesting, considering the potential to truly locate this storage underground, as opposed to an underground room containing conventional storage. However, it is not apparent why a subscale vessel was not considered to demonstrate proof of concept first. It would have been prudent to demonstrate a proof of concept for some of the novel design approaches before making the leap to viability of manufacturing. It would have facilitated the construction of the vessel, allowed the project team to fabricate a number of vessels to address the multiple design challenges, expedited testing, and possibly introduced some additional decision gates to effectively steer the project. The PI admitted during the presentation that concrete may not have been the best choice as a reinforcement material—something that would have been apparent from subscale testing. There are questions about the effectiveness of the multilayered steel and hydrogen vent channel approach to mitigating hydrogen embrittlement (H_2E) effects. There are also questions about the ultimate fatigue life of the vessel, considering the design calls for the use of concrete in tension (not appropriate for concrete). These questions could have all been answered had numerous subscale vessels been available for testing. If the intent of the project team was to use this vessel at hydrogen refueling stations using cascade filling, the industry norm for efficient gaseous delivery, the team missed the need for multiple tanks being located onsite. This point is relevant because it appears the economic feasibility of the vessel makes sense only on a very large scale, and the PI was claiming the objective was for one massive vessel to be located onsite. While the vessel itself could meet the DOE cost reduction targets, as claimed by the PI, any transport, handling, and site preparation costs of using this vessel would negate any savings when compared to existing technology today. These are non-trivial costs that should be factored into the overall vessel cost per kilogram since these are costs unique to this vessel design when compared to existing technology. It is also not clear how H_2E performance will be effectively evaluated. A sound test methodology for H_2E was not proposed in the presentation. The number of pressure cycles conducted seems insufficient to simulate any appreciable hydrogen service timeframe. It is also not clear that any of the accepted H_2E evaluation methodologies could be employed on the metallic elements of the pressure vessel—it is widely believed by the industry that H_2E performance tests must be conducted in

a hydrogen environment to be effective. Any approach that relies on exposure of a material to hydrogen, then removing it from the hydrogen environment for testing, is seen as problematic since hydrogen rapidly escapes the material after removal from the hydrogen environment.
- For the most part, the project was well-designed and well-coordinated. The PI has done a good job of trying to meet the barriers identified by industry and the targets set by DOE. However, it is apparent that this project is going to be labor-intensive and will most likely not meet the manufacturing schedule required to meet the demand for 1000s of hydrogen stations.
- Stainless steel and concrete for this application is not efficient. The only benefit of this design over carbon composite is the cost.

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 overall seems to have progressed well. There are a few shortcomings—in particular, measuring and quantifying H_2E and fatigue life.
- While the accomplishments in fiscal year 2017 align with the goals of the project team, they do not adequately address several design or practicality concerns with this vessel. It would have been good to see a validation of the fatigue life of the vessel, validation that the multilayer steel walls with “vent channels” was effective in mitigating H_2E, and validation that the vessel was cost-effective compared to today’s available technology. It is not clear that any of these concerns will be addressed by the conclusion of the project.
- It is difficult to discern the level of improvement between this project and the initiation of the new project. DOE should have ended this project or simply combined the efforts prior to awarding very similar efforts.

Question 3: Collaboration and coordination with other institutions

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

- The team comprised a list of individual service providers that had appropriate expertise to accomplish the job, and the project seemed to have adequate coordination between the groups.
- A number of institutions/companies have been actively participating in this project. These contributions have been articulated by the PI.
- Since a large portion of the project appears to center around the novel multilayered steel approach, it was disappointing to see that Sandia National Laboratories—a key member of the effort to establish an international consensus for a H_2E test method—was not a collaborator in this project. A sound plan is needed to evaluate the effectiveness of the hydrogen vent channels.

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

- This project meets the cost and performance requirements of the Program; however, what is not taken into account is the ability to manufacture and transport these vessels. These structures will most likely require manufacturing onsite, so lowering costs through economies of scale will most likely not come to fruition. The materials are low-cost but highly labor-intensive and not easily manufacturable/transportable.
- This project presents one approach to achieving the DOE goals for high-pressure hydrogen storage. However, still more work is needed to prove the feasibility in terms of fatigue life and H_2E.

Question 5: Proposed future work

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

- This project is scheduled to end in September 2016, and many of the lessons are being incorporated in a follow-on project addressing higher-pressure operation.
- The project is in its final six months. The PI has another project that seems to be tailored toward further enhancement of the design. However, it is not clear what the team plans to do in the remaining time of the current project that does not necessary overlap with the other project.
- This project is ending.

Project strengths:

- Project strengths are the demonstration of the steel–concrete composite vessel and coupling between finite element analysis and tests.

Project weaknesses:

- Project weaknesses are lack of (1) fatigue life and (2) H_2E assessment.
- Novel design aspects addressed in this project do not have a robust evaluation scheme. A number of conclusions were to be expected from the project. However, it does not appear the project will demonstrate the following: vessel demonstrates minimum fatigue resistance using concrete as a reinforcement material; use of hydrogen channels to vent hydrogen demonstrates mitigation of H_2E, thus allowing for high-strength steels to be used; with those proven features, this vessel meets or shows promise in meeting the DOE storage vessel cost target; and vessel demonstrates features are superior to existing technologies currently employed in the industry.

Recommendations for additions/deletions to project scope:

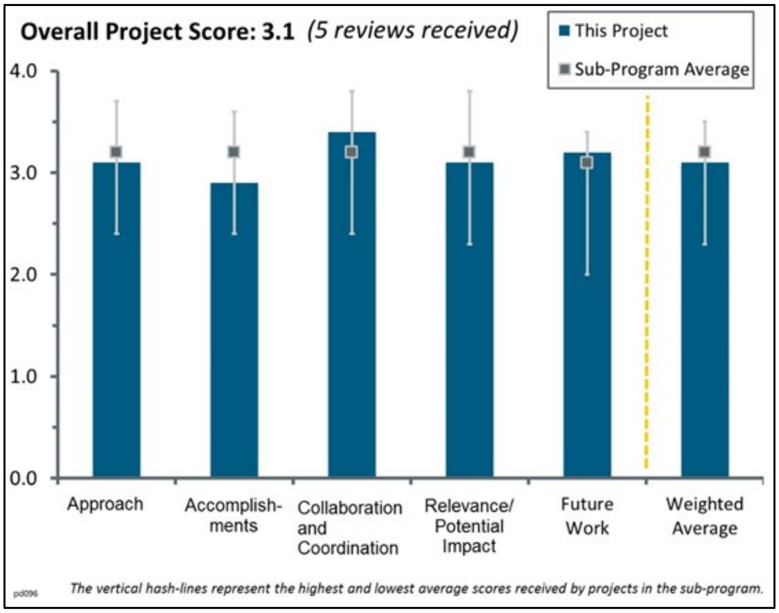
No responses entered.

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

William Summers; Savannah River National Laboratory

Brief Summary of Project:

The objectives of this project are to (1) develop highly efficient process designs for coupling the hybrid sulfur (HyS) thermochemical process with a concentrated solar energy system; and (2) demonstrate SO_2 depolarized electrolysis (SDE) using improved electrocatalysts and high-temperature polymer electrolyte membranes (PEMs) that permit high-efficiency hydrogen production. Fiscal year 2016 objectives are to (1) analyze and select a baseline plant design that utilizes high-temperature solar heating, (2) develop process flowsheet models and calculate plant performance and efficiency, (3) estimate operation and production costs for a commercial plant, and (4) test candidate high-temperature PEMs and demonstrate SDE performance improvements.



Question 1: Approach to performing the work

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

- This project leverages past Hydrogen Fuel Initiative investments in hydrogen production at Savannah River National Laboratory (SRNL) and past and continuing U.S. Department of Energy (DOE) investments in fuel cell component development. The process was originally funded through the DOE Office of Nuclear Energy to develop hydrogen production through a two-step HyS process (an electrolysis step combined with a high-temperature thermochemical step) using heat input from nuclear reactors. The project is building on this past investment, and on DOE-funded development of advanced membranes and catalysts, to address the coupling of the HyS process with a concentrated solar power system. The immediate objectives of the project are to demonstrate the electrolysis step using high-temperature membranes and improved electrocatalysts that permit stable, high-efficiency hydrogen production. It is very important that the project team continue to focus on the high-temperature membrane demonstrations in order to prove viability of a hybrid approach to solar thermochemical hydrogen (STCH) production. Specific barriers limiting progress were addressed, and a reasonable plan was proposed for the remainder of the project and for future work.
- The technical approach taken by the project team to work on the main tasks of this project seems reasonable: focusing on the electrolyzer component development, include polymer electrolyte membrane and electrocatalyst, as well as on the system design so as to have a more accurate cost analysis.
- SRNL has multiple variations on a sound base design. Each variation addresses a potential barrier in a logical and practical manner.
- The HyS cycle is interesting in that it is at a lower temperature than some of the other cycles and does not have the attrition issues that a solid-based system has. The researchers need to clarify many of their Hydrogen Analysis (H2A) tool assumptions, such as electricity costs. Thermal storage at >850°C is difficult, and it is unclear whether the system currently does this. Sand is typically used, and it is good to up to 600°C. The Solar Energy Technologies Office (SETO) is doing work to increase it to 800°C–900°C. It is unclear how the associated costs were included in the H2A analysis.

- The project is addressing key barriers that are relevant to the Hydrogen Production and Delivery program and has already shown, through a techno-economic analysis, a pathway to the goal of $3.7/kg. The biggest problem is the fact that a large proportion of the cost is associated with the solar capital under the assumption that the SunShot Initiative achieves its 2020 goals. This is certainly something that can be considered an external risk.

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.

- Considerable progress has been made in plant, process design, and cost analyses for an STCH production process utilizing Aspen Plus process flowsheet models and the H2A tool. Candidate high-temperature membranes (polybenzimidazole [PBI], sulfonated Diels-Alder poly(phenylene) [SDAPP], and sulfonated perfluorocyclobutyl [S-PFCB]) have been identified, and one membrane (SDAPP3) was shown to meet the go/no-go criteria of a 50 mV improvement at 91°C. The project team has selected baseline designs for the solar thermochemical plant, solar receiver, and sulfuric acid decomposer.
- The project team presented good progress on this work, including the solar HyS process design and the H2A tool for the 2020 design case.
- Generally, all barriers are addressed in this work. The method for thermal energy storage is particularly interesting, as are the methods of heating. Also, the acid decomposition work in the bayonet heater shows promise, as does the identification and potential solutions to sulfide build-up on the cathode. The H2A results appear feasible and accurate. Further detail could be provided regarding how to achieve the high temperatures stored in the sand.
- The results on the membrane electrode assemblies (MEAs) using the Pressurized Button Cell Test Facility (PCBTF) are encouraging and show promise; it would be very interesting to see the results at 130°C. The work done on the falling particle receiver has provided a solution in terms of diurnal operation. The progress of the project is substantial, but it is still very early to say whether it will have a substantial impact on DOE goals.
- Modeling the reactor is a good start. The bayonet reactor design has been around for a long time, so it is surprising that this has not been done before. A 50 mV improvement by increasing the temperature is not a surprise. This does show improvement over the 2014 Nafion data. The projected 150 mV reduction at 130°C seems very unlikely. The project seems very dependent on others for success. The majority of the cost savings are associated with the assumed SETO successes.

Question 3: Collaboration and coordination with other institutions

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

- The project team has benefited from including membrane and solar reactor and receiver experts from Sandia National Laboratories and the University of South Carolina. Industrial partners in the area of membrane development and fabrication would add additional perspective to the project, particularly in the area of fabrication options and costs. The project team continues to have excellent collaboration with modeling experts at SRNL and valuable information exchange with the German Aerospace Center (DLR). Additional industrial participation for development of the electrolysis technology using advanced membranes and catalysts (ideally for both STCH production and other end uses) will be needed to accelerate scale-up of the technology and to fully realize the potential for a two-step hybrid process.
- Collaboration appears to be excellent and includes major research groups and industry. All collaborators seem well suited to their division of the work.
- The collaboration and the expertise among the individuals of the entities working together on this project are very robust.
- There is great cooperation between the partners, who have a good deal of expertise on this subject. Further collaboration with the European project Solar to Hydrogen Hybrid Cycles (SOL2HY2) through DLR could be beneficial.
- Collaborations are very good.

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

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

- The project is directly related to and contributes to the goals of the Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan.
- The project supports the FCTO goal of reducing the ultimate cost of hydrogen production from renewable resources to <$2/kg as well as the objective to verify the competitive potential for STCH production by 2020. Meeting the cost goal will depend both on the successful completion of this project and on technology development beyond the scope of the project and FCTO, e.g., lowering the cost of heliostats.
- This is a renewable-powered water-splitting project for hydrogen production, so it is relevant to FCTO's goals. The potential impact is difficult to assess, as the technology is very early-stage and has many issues to solve. The cycle has the advantage of not needing to move tons of solid materials. However, the project plans to use thermal energy storage, which will require moving large amounts of sand as a thermal storage medium and may negate this advantage. In addition, the high-temperature sulfuric acid will be a materials compatibility challenge.
- This project definitely supports FCTO's efforts to develop a technology that will enable low-cost production of renewable hydrogen.
- There has been substantial progress in the project, but its impact is doubtful. The main reason for this is that the cost is highly dependent on the SunShot project.

Question 5: Proposed future work

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

- The proposed future work is highly relevant and will provide useful results.
- The proposed demonstration of SDE performance using high-temperature MEAs seems reasonable for this project.
- The proposed work for continued research and analysis is reasonable. Particular attention should be focused on detailed cost analysis and on demonstrating the performance of high-temperature membranes in order to show that a hybrid STCH cycle can compete with other solar hydrogen production methods. H2A results should be compared to those for high-temperature STCH and updated analyses for photovoltaic electrolysis and solid oxide electrolyzer cell electrolysis.
- All of the plans for future work can directly address the remaining barriers for this system. Some comparison work for the various methods of heating and storage (sand, helium, etc.) would be useful.
- It is important to test the long-term stability of the sulfonated PBI (s-PBI) membrane. This needs to be done under real conditions. The project needs to look at cycling as well as constant current.

Project strengths:

- The thermal storage design is promising, and it is encouraging to see that the system can be used even during non-sunlight hours. The testing plans, pinch point analysis, new MEA material, and bayonet design modeling contribute strongly to this work.
- The project has an experienced and skilled project team with historical knowledge of the area and expertise in the technologies critical to success. The project is leveraging past work funded through the Hydrogen Fuel Initiative. Progress has been made in system design and analysis and in investigations and consideration of designs and innovations for membrane materials, solar receivers and reactors, and sulfuric acid decomposition.
- This is the one cycle that does not require movement of tons of solid materials. This is a huge advantage.
- There is excellent expertise from the partners, and testing facilities are good.

Project weaknesses:

- The project presentation claims that a clear path to $2/kg hydrogen was defined. However, significantly more detail is needed regarding assumptions made and specific process and capital cost improvements that will lead to the cost goal; otherwise, the H2A analysis showing that the cost target can be met is not convincing. (Reviewers must rely on information presented, not more detailed reports or analyses that are not part of the Hydrogen and Fuel Cells Program Annual Merit Review.) It is concerning that the PBCTF still cannot achieve the desired test conditions. This needs to be resolved as soon as possible.
- The H2A analysis was nondescript in critical components. While there are many parameters that contribute to an H2A analysis, some key parameters (total capital costs, installation factors, etc.) should be listed. Steps in the waterfall chart should indicate directly what changes lower the total cost.
- The cycle requires extremely high-temperature sulfuric acid, which will cause materials compatibility challenges.
- The dependency on the SunShot Initiative's success is significant.

Recommendations for additions/deletions to project scope:

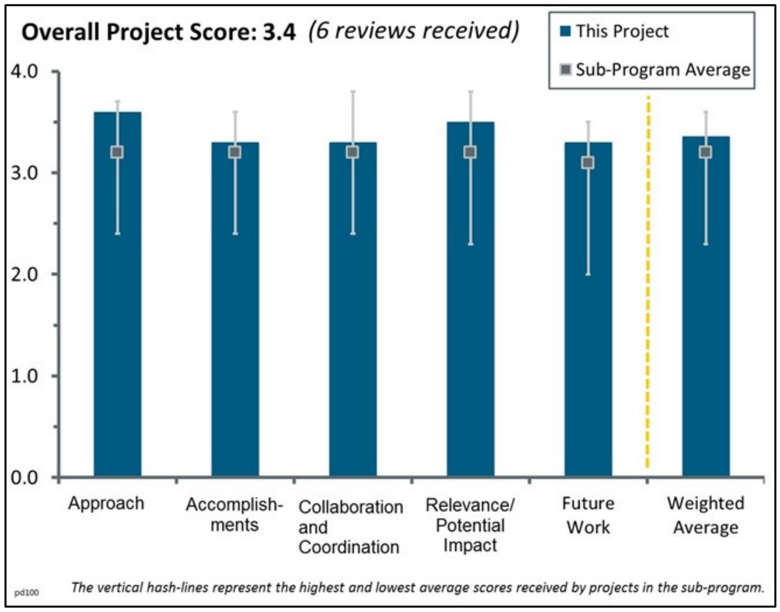
- The project team has done a good job analyzing options and approaches for the whole hybrid HyS thermochemical cycle, and has identified challenges to be addressed in future work. Time and funding limitations, however, demand that research tasks be prioritized going forward. It is crucial that the project team give the highest priority to demonstrating long-term performance and durability of s-PBI membranes as soon as possible, including SDAPP and other high-temperature membranes if time/resources permit. Advanced catalysts, optimization of the acid decomposition step and the falling particle receiver, and design and operation of an integrated system are important but will not be needed if sulfur crossover and deposition is not eliminated and an appropriate low-voltage, high-temperature membrane developed.
- In next year's presentation, it would be very good to show the progress achieved by the SunShot Initiative to see if it is reasonable to assume that the 2020 targets will be achieved.

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 upon 700 bar refueling hose reliability under mature market conditions. 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 **3.6** for its approach.

- This is a great project to provide accelerated cycle-life testing for a key component (dispenser hose) that has few vendors, high failure rates, low mean time between failures, and weak technical standards. The project provides support in all areas of deficiency.
- The project is well-designed to address most barriers, especially the critical ones.
- The project has thought through the test setup carefully.
- This is interesting work. The approach seems appropriate.
- The approach is good. The system is nicely automated, programmed to replicate human motion, and providing 24/7 operation. While the principal investigator (PI) has more than one hose, all are from the same manufacturer; to understand whether there is a systemic problem among different hoses and with the technology in and of itself, similar hoses from different manufacturers need to be tested. The PI did mention that, after this project is over, the facility will be available to test other components on request, not just hoses. That is good, but as part of this project, hoses from different manufacturers should be tested to look for systemic problems.
- The approach is technically sound. The tolerances on the robot making and breaking the nozzle receptacle connection result in tighter alignment than encountered in the real world. It would have been nice if the alignment tolerances were looser to see whether the current receptacle design properly addressed the brinelling issues previously seen on the N25 and N35 nozzles in natural gas service.

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.

- It is good to see that hydrogen supply limitations have been addressed at NREL and that laboratory safety systems are now in place to allow around-the-clock cycle testing.
- The project has demonstrated many accomplishments in terms of identifying the leakage of hydrogen before, during, and after the system has been depressurized.
- Progress is good so far.

- The progress to date is interesting. It is not clear whether the magnitude of the leaks has been determined and, if so, what the sizes of the Class 1 Zone 1 and Zone 2 volumes are.
- Recognizing that these experimental campaigns take a long time to execute, it still seems that the quantity of output is low, the number of publications is weak, and the location of the publications is weak. Presenting to a technology team and producing a YouTube video do not count. This work needs to be published in a refereed journal or refereed conference/symposium or suitable trade journals. A comprehensive general-distribution NREL report would be acceptable as a precursor to a refereed article. This project has been funded since 2013, and after three years, the team should have more to show. This work is useless unless it gets put in the public domain where it is readily available.
- The presentation is not very clear about what the main results of the work have been up to this point. It is not clear what the consequences are or how the results are fed back to industry.

Question 3: Collaboration and coordination with other institutions

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

- The project is designed in a way that allows a well-coordinated and logical interaction with other collaborators who are considered experts in the field.
- The collaborators are okay (except that NREL is not a collaborator), although a broader set of samples from other manufacturers is needed to understand any inherent systemic problems. With only one sample, no systemic problems can be identified. The testing and results are valid for only the one.
- This type of work is usually done by a commercial laboratory or a Nationally Recognized Testing Laboratory (NRTL). The fact that no NRTL is involved is surprising, as is the fact that the breakaway manufacturer and the nozzle manufacturer are not on the team.
- It seems that the industry partners provided only samples. It is not clear that a review process has been set up and, if so, that it influences the industry partners' research, development, and demonstration strategy.

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.

- Delivering hydrogen fuel to the end users is an important task and critical to the success of the Hydrogen and Fuel Cells Program. This project has a potential impact on the program, as it is well-designed to aim sharply at effectively improving the reliability of hydrogen dispenser hose under high-pressure operating conditions.
- The relevance of this work is excellent. Reliability of stations is a real Achilles heel for the fueling industry. This work needs to expand the hardware being studied so there is an "industry-wide" relevance.
- Understanding these leak behaviors is interesting and should be continued as new hose materials are introduced to the market.
- The hose accelerated cycle testing has potential for significant impact on current infrastructure problems with poor hose cycle life.
- This activity is highly relevant.
- No explicit benchmark to DOE targets was mentioned. Nonetheless, station reliability is a major issue. The project provides a useful independent test facility for hoses and receptacles.

Question 5: Proposed future work

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

- Increasing the number of cycles is critically important. Reliability at the station is probably the most important factor in customer acceptance as hydrogen technologies are deployed. A fully functional station should see on the order of 100 fuelings per day; over a year, a dispenser may see 36,000 fuelings. The service interval of only six months is way too short to be cost-effective. This technology needs to be made

more robust. The future proposed work is headed in the right direction. However, hardware from other manufacturers must be included in this work, as it becomes available, to understand industry-wide systemic problems. Also, this work needs to include contaminants leaching into the fuel supply with cycling and age. We are finding that fuel quality control is a real challenge.
- The proposed work seems appropriate. It would be a comfort to learn that a NRTL, a breakaway manufacturer, and a nozzle manufacturer were included in this effort. Additionally, it would be good to hear that the tooling designs are available for the NRTLs to copy if they so choose.
- The proposed future work is logical and technically sound. However, it is lacking the risk mitigation plan for each decision point.
- The future work makes sense, although it does not seem systematically planned. It is not clear how the results will be shared with industry, not is it evident who is driving this process.
- Future work is pretty clear.

Project strengths:

- The project is technically well-designed and logically planned; it is well-coordinated with other collaborators; and it has been carried out in a timely manner, is on schedule, and is making good progress.
- It is very helpful to have 24-hour-per-day cycle testing during this time of hose standard development at the International Organization for Standardization (and thereafter at the CSA Group).
- The PI is talented and hard-working. There is good automation in the laboratory.
- The industry need and the laboratory’s expertise are project strengths.
- The project presents a useful independent test facility for hose/receptacle testing.
- Early findings are interesting, and the reviewer looks forward to more understanding of key mechanisms/patterns as the work proceeds.

Project weaknesses:

- This project is lacking a risk assessment plan, which is an important factor to consider. The risk assessment plan is necessary to allow the project to mitigate risks related to safety, reliability, cost and performance effectiveness, system limitations, and project schedule (i.e., project downtime).
- A NRTL, a breakaway manufacturer, and a nozzle manufacturer were not included in this effort. They would lend credibility and industry acceptance of the results.
- Perhaps industry is not sufficiently involved.
- The project needs to get additional hose collaborators and/or hoses for testing.

Recommendations for additions/deletions to project scope:

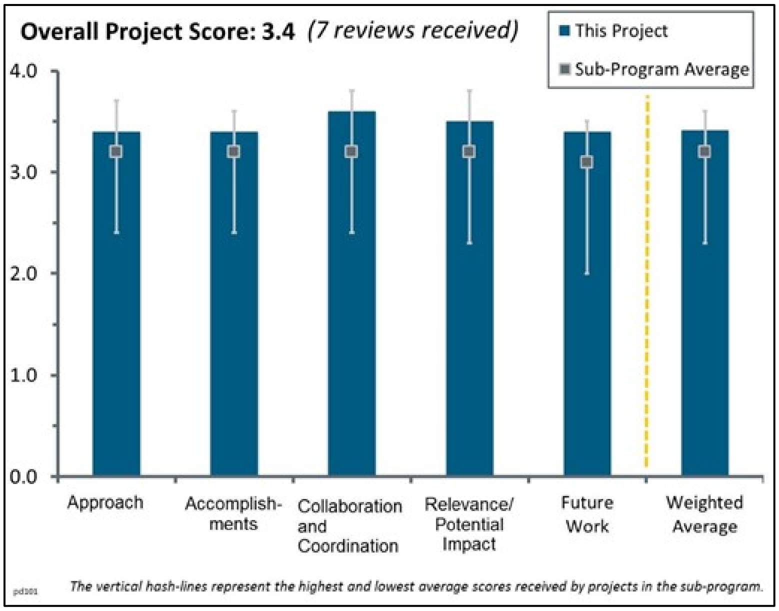
- A NRTL, a breakaway manufacturer, and a nozzle manufacturer should be included in this effort. The project should consider making the tooling designs available to the public for copying. The project should also consider amending the test plan to generate meaningful data to address other issues such as brinelling concurrently.
- The project should hold supplier workshops and involve the big station technology providers such as The Linde Group, Air Liquide, and Air Products.
- The team could work with more hose manufacturers and could add a second robot station to work on two hoses at the same time.
- The project needs to get additional hose collaborators and/or hoses for testing.
- It is recommended that a risk assessment plan be added to the project.

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

Jennifer Lalli; NanoSonic, Inc.

Brief Summary of Project:

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



Question 1: Approach to performing the work

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

- The team members clearly know their polymer science and have produced and are producing very attractive hose products. Their Achilles heel has been in fastening the ends to their hoses (crimping problems). This has been an issue for a long time and has been noted and articulated to this group in several review venues. It was great to see that the project was successful with at least one hose and end going to 60,000 psi without separation, which is outstanding. It seems certain that the project will be successful in designing an end that will be rated to the appropriate pressures; this is good work. The team members need to pay a bit better attention to the contaminant issue—100 ppm is a big number. While they have the appropriate standards (J2719 and ISO 14687-2), they did not seem to understand these standards in detail. They need to pay better attention to those standards—and unfortunately for them, the standards are undergoing revision, so the current tolerance numbers will change. The team also needs to pay attention to the species that might contaminate the fuel and any particulates that might flake off.
- The result of this project will be to have a new, qualified supplier of hoses for hydrogen station implementation. This is a good example of a funding opportunity being used to benefit a technology development while also serving the broader industry by introducing a new supplier into the mix. The awardee has done a good job of addressing the primary concerns from the previous year, expanded the scope to include fittings, and with this has demonstrated some good progress.
- This project addresses important barriers and contributes significantly to the improved technology and economic cost challenges of the component, which is an enabler for the hydrogen infrastructure.
- The approach is sound. It is unclear as to when material compatibility with polymer electrolyte membrane fuel cells (PEMFCs) is to be conducted. Considering this has the potential to be a showstopper, sooner would be better.
- The approach seems logical. Having to change the project a bit in regard to the terminations/fittings is interesting but understandable.
- The project is well designed to overcome the barriers.
- This is a really good project with potentially great promise that has suffered from lack of partners or collaborators with prior experience in the hose industry.

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 are excellent, and the recognition and the reporting of the remaining challenges are even better. This "eyes-wide-open" approach pays dividends on the chances of success.
- This project has made great progress since the last time the reviewer saw a report (only a few months ago). The success in crimping the end to the hose is recent and very significant—the project has done a good job.
- The scope expansion to include fittings is significant, and the awardee has done a very good job of taking on this scope while continuing to deliver on the overall project. This is not necessarily something to which other awardees would have agreed.
- Good progress has been made in down-selecting the final hose materials with the best performance.
- The team has continued to make progress, and the results have great promise.
- The project seems on track. Hose costs are not a major issue, but something cheap and very durable would be helpful.
- Much progress has been booked so far, and already it can be stated that the project is a success. However, considering the work still to be done (for example, testing and qualification) with an end date of 7/27/2016, it is not credible that the project can be completed in all aspects without an extension.

Question 3: Collaboration and coordination with other institutions

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

- This is a good team/collaboration. During the question-and-answer session, the principal investigator was asked about Lillbacka participation in this project. Indeed, the involvement is that NanoSonic purchased the now in-house crimping tool. That is the extent of the involvement—no consulting, no collaboration. The principal investigator explained that Lillbacka referred to crimping as "black magic," and NanoSonic wanted/needed to make the hose entirely in-house. This explains a bit why NanoSonic was not more aggressive in seeking consulting advice from Lillbacka or other crimping companies. In the end, the project team was successful. It seems the team will be able to continue to refine and continue with successfully assembling the entire hose, with metal ends on NanoSonic's polymer hose.
- The awardee has formed a strong team for the evaluation of the hose performance. National Renewable Energy Laboratory (NREL) testing will be an excellent next step to validating the performance. It is unfortunate that the fitting manufacturers have not been more cooperative, as it seems that this has caused considerable problems for the project team. In spite of this, the project team seems to have taken on this scope and is producing good results.
- The project is designed in such a way that it allows a well-coordinated and logical interaction with other collaborators.
- The selection of partners is outstanding.
- There is nice collaboration between industry and the national laboratory.
- Sound interfaces are present with testing laboratories and industries.
- This is a really good project with potentially great promise that has suffered from lack of experience in the hose industry.

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

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

- Recognizing that delivering hydrogen fuel to the end users is an important task and one critical to the success of the Hydrogen and Fuel Cells Program (the Program), this project has demonstrated that it aligns well with the Program and DOE research, development, and demonstration objectives. The project has the potential to advance progress toward improving the performance of hydrogen dispenser hose under high-pressure and low-temperature operating conditions.

- Developing a delivery hose that can withstand 25,000 to 30,000 H70 fills without failure would go a long way toward enabling a robust reliable hydrogen fueling infrastructure. Also, this technology has the potential to cut the costs of these delivery systems significantly.
- Being constrained to a single supplier for high-pressure hydrogen hoses has been a continual problem for station designers. While the potential cost impact is small when compared to overall station cost, this will benefit the market.
- The lack of a reliable, cost-effective fueling hose is making an adverse impact on the industry. This effort has the potential to remove this obstruction.
- This project can deliver a strong contribution to the technical and economic progress of a critical component of the hydrogen infrastructure.
- This is not a major issue, as hose technology seems pretty good, but it is worth working on to be sure.

Question 5: Proposed future work

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

- There is a clear plan for future work, which looks good.
- The proposed future work is logical for the stage of deployment of new hose materials.
- The proposed future work is rational.
- The reviewer looks forward to the next steps in testing. The test program could include a side-by-side comparison with the existing hose manufacturer whenever possible. Performance data are very difficult to obtain, and having side-by-side comparisons would benefit both the future users and the awardee with insights into strengths and weaknesses in each design. Testing for leaching/contamination seems weak. It would be good to see a more thorough understanding of the testing and specifications that need to be met in this area—in particular, if there is a test standard or an acceptance criterion.
- The project is addressing the crimping problem in the future work proposal, which is good. The project is addressing fuel quality, which is also good; there has been an expressed concern about whether these hoses will evolve contaminants into the fuel, so fuel quality testing, with use and in time, is critical. However, it was not clear that any attention was given to particulates. It is conceivable that, with time and use, this material might fatigue and give off particles into the fuel supply. Micron and sub-micron particulate sampling really needs to be done.

Project strengths:

- This team is very strong with the polymer science, the facilities are good, and the team is clearly able to experiment with different hose configurations. In so doing, the project is clearly making progress in developing a hose with the desired properties. This is excellent.
- Hose cycle life in H70 dispensers is a tough duty cycle, and long life is essential. This project is proposing new hose liner technology that warrants continued support.
- The team has shown good adaptability and has done a very good job of addressing the concerns raised in 2015 with regard to the fittings.
- The following are project strengths: the project has coordinated well with other collaborators, has been carried out in a timely manner, is on schedule, and is making good progress.
- Project strengths include the "eyes-wide-open" approach and the willingness to find partners to help.
- The project has demonstrated that it possesses the required excellent technical and management competences to overcome the many challenges found in its path.
- This is an interesting investigation of new hose materials/manufacturing—and "manufacturability" should be a key element on which to focus.

Project weaknesses:

- The lack of a solution for the metal fitting has probably caused delays. The problem seems to have been overcome now, and a full component will be available for testing.

- The presentation needs to show/explain in more detail how to mitigate the difficulty of crimping the Ceramer coupling to the hose for high-pressure application.
- The team is new to making hose assemblies and has a steep learning curve.
- Weaknesses include the delay in addressing potential showstoppers such as incompatibility issues with PEMFCs.
- Better attention to fuel quality and the relevant standard specifications needs to be paid.

Recommendations for additions/deletions to project scope:

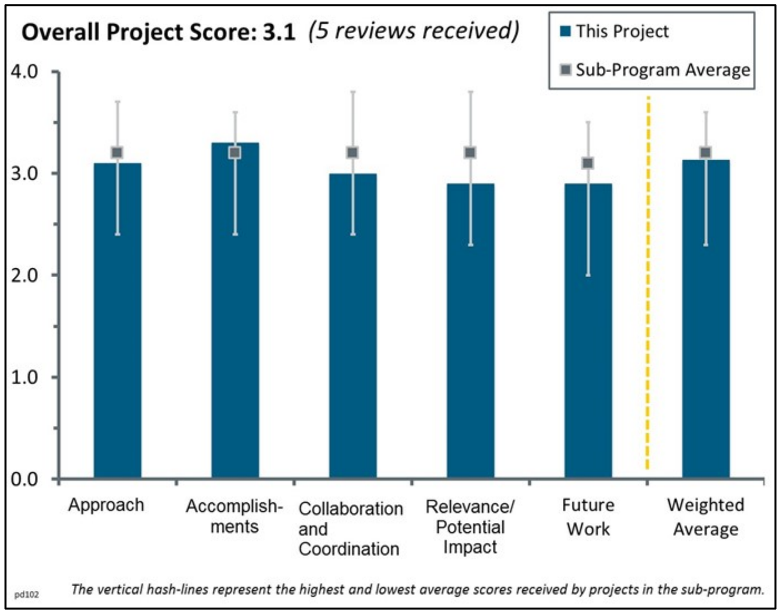
- The project is now on the right track. It is hoped they continue to have success with what used to be a serious crimping problem.
- The project should continue per its plan and determine what compounds may leach out and/or off-gas from the liner materials, and at what rate.
- The project is a very good one. However, its presentation does not clearly explain/identify the essential effort offered by this project to improve the hydrogen fuel-delivering system. It is suggested that the objectives or project scope be clearly defined and presented in such a way that the contribution of this effort can be recognizable.
- Side-by-side comparisons with the current industry standard should be made. An industry benchmark data set would be valuable for this product and for any further developments in this area.
- The project needs more funding and more partners.
- Given somewhat of a work scope change, it is not clear what the company’s plan is, i.e., whether to produce complete systems (with end terminations) or just the raw hose material. Since leaks seem associated with the fitting/crimping sections (based on other NREL research), it is not clear how we best address this.

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

Brian James; Strategic Analysis, Inc.

Brief Summary of Project:

The objectives of this project are to (1) analyze hydrogen production and delivery (P&D) pathways to determine economical, environmentally benign, and societally feasible paths for the P&D of hydrogen fuel for fuel cell electric vehicles; (2) identify key bottlenecks to the success of these pathways, primary cost drivers, and remaining research and development (R&D) challenges; (3) assess technical progress, benefits and limitations, levelized hydrogen costs, and potential to U.S. Department of Energy (DOE) P&D cost goals of <$4 per kilogram hydrogen 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 **3.1** for its approach.

- Strategic Analysis, Inc., (SA) and its partner national laboratories have taken a clear and detailed approach to modeling hydrogen production. The project has done a good job predicting costs for very early-stage technologies. Projections of pioneer and nth plants are a valuable consideration in evaluating technologies.
- This is a well-established approach that provides a degree of "consistency" across many different technology platforms and feedstocks (biomass, electricity, natural gas, etc.) for estimating the cost of producing hydrogen. Given that many of these technologies are far from market-ready, it is not clear how accurate the cost estimates for "large-scale" production are.
- The assessment seems technically sound, but some of the key underlying assumptions are overly aggressive and have not been properly vetted. In addition to "today at production volume" and "future at production volume," it would be good to see the "fabricated today at today's volume" case to understand where the technology is, followed by an explanation of how learning by doing and higher production volume will reduce cost. Regarding "fermentation results, future case," the assumptions of broth density seem very aggressive given what has been achieved in the laboratory so far. Given that the corn stover concentration is based on DOE Bioenergy Technologies Office (BETO) goals, it would be material to vet this assumption with academia and industry and assess the likelihood of achieving this concentration level. Regarding solid oxide electrolyzer cell (SOEC) results, the current central case is based on assumptions of technology at scale. It is misleading to call this case "current" given that the data do not reflect current technology costs. Rather, they are based on assumptions of future potential volumes of production.
- The main sources for technical information have been research organizations rather than industry; the reasons for this are unclear. How the analyzed pathways are selected is not very transparent.
- It is not clear why the nuclear coupled thermochemical process is not selected for a review.

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.

- Completion of the SOEC (including degradation) and dark fermentation cases is a significant accomplishment. For the dark fermentation case, the high density is a big leap from current technology. Perhaps an intermediate case should also be considered. Bio-oil reforming should move quickly away from fatty acids to real bio-oils.
- The project has made excellent progress toward meeting project objectives for technologies being evaluated as defined in the presentation. It is difficult to assess how well the project has made progress toward meeting DOE goals since the results and outcomes of this project are used by DOE to define its R&D priorities and performance/cost targets. Therefore, it is difficult to assess how well the project has progressed toward meeting DOE goals, particularly from the perspective of how much impact it has on the R&D community; however, the significance and importance of this project to DOE are fully recognized.
- The project has made excellent progress toward identifying cost drivers, assessing Technology Readiness Levels (TRLs), and integrating results into the H2A model. It is unclear whether the team has already identified all technical and economic bottlenecks.
- The results seem mature and helpful for further Fuel Cell Technologies Office (FCTO) work.

Question 3: Collaboration and coordination with other institutions

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

- SA works to gain input from all stakeholders. There is good collaboration between SA and national laboratories.
- BETO is pursuing numerous thermal chemical and bio-based routes for converting biomass to biofuels and the idea that many of these technologies could be modified to produce hydrogen as an end product. Given these facts, perhaps it would be beneficial to evaluate a broader range of biomass-based processes for producing hydrogen than what could be called the “niche” projects being evaluated. It would provide a broader perspective of the cost competitiveness of biomass-based technologies for producing hydrogen at the projected DOE target.
- The project should ensure that assumptions are harmonized with Argonne National Laboratory’s (ANL’s) analysis work since ANL is working on the same cases to assess the lifecycle footprint of the technologies. Some of the assumptions appear different, such as the stack replacement schedule of the SOEC and the process flow of the fermentation pathway. Perhaps researchers in academia are working on similar technologies. It would be good to get their perspective on the TRL of the technologies and to compare results from this work against published literature.
- It is not clear whether there are any industry collaborations other than the one with FuelCell Energy, Inc., as no others are mentioned. It seems the analysis would not be possible without industry input. Perhaps there is a review process of the results with industry 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 **2.9** for its relevance/potential impact.

- The major benefit to DOE and the R&D community at large from this project is identifying key bottlenecks to reducing cost, which is critical for DOE and companies in setting R&D priorities.
- These results are vital for defining appropriate areas for future research.
- Analysis results could help FCTO to focus on pathways with high potential.
- It is necessary to understand the status and the potential of hydrogen production pathways but the assumptions for future progress appear highly optimistic, which makes these pathways less relevant. At this

point, given the low TRL of these technologies, these cases are not very relevant to industry, but they can be relevant to guiding DOE funding decisions if the right assumptions are made.
- It is not clear what impact this study will have on the production pathway. DOE's selection criteria are also unclear.

Question 5: Proposed future work

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

- The analysis of the FuelCell Energy work is appropriate and should be interesting.
- It is somewhat difficult to evaluate the proposed future work since the future work presented focuses solely on cost projections for FuelCell Energy's Reformer-Electrolyzer-Purifier (REP) technology. During the presentation, it was stated that SA had been awarded a new contract, so presumably there is a broader scope of work going forth, but since that work will be conducted under a new contract, future work outside of the scope of this contract is not being considered in this review.
- Molten carbonate fuel cell technology being used in the REP system is still at an early stage of R&D but shows significant promise for utility-scale operations.
- It is not clear whether any other pathways are planned or how the analyzed pathways are selected.

Project strengths:

- The project is highly relevant to future FCTO priorities in research, development, and demonstration (RD&D).
- The project team has a good understanding of processes and excellent analytical capabilities.
- There is a strong, experienced team with a proven track record.
- The project builds on proven expertise and experience.

Project weaknesses:

- It is not clear why the projected cost of hydrogen produced by solid oxide electrolysis increased from $4.21/kg in 2015 to $4.95/kg in 2016 for the Current Central case and from $3.68/kg in 2015 to $3.83/kg in 2016 for the Future Central Case. The electricity costs seem to be the same for both the 2015 and 2016 analyses. With a drop in the price of crude oil, one would assume that there would be a slight decrease in price. The factors that are leading to these cost increases are unclear. The future cost projects are based on high-volume production. It is not clear what constitutes high-volume production, either in terms of the total amount of hydrogen produced across the United States or the number of plants of a given technology required to produce the projected volume demand.
- Data and information appear to be sourced from single sources but have not been validated through collaborations with other researchers working in the same or similar fields. Technologies are assumed to operate at scale when none of their TRLs are higher than 5. Assumptions appear to be overly optimistic.
- A project weakness is lack of experimental data for emerging systems.
- Industry collaborations are a weakness.

Recommendations for additions/deletions to project scope:

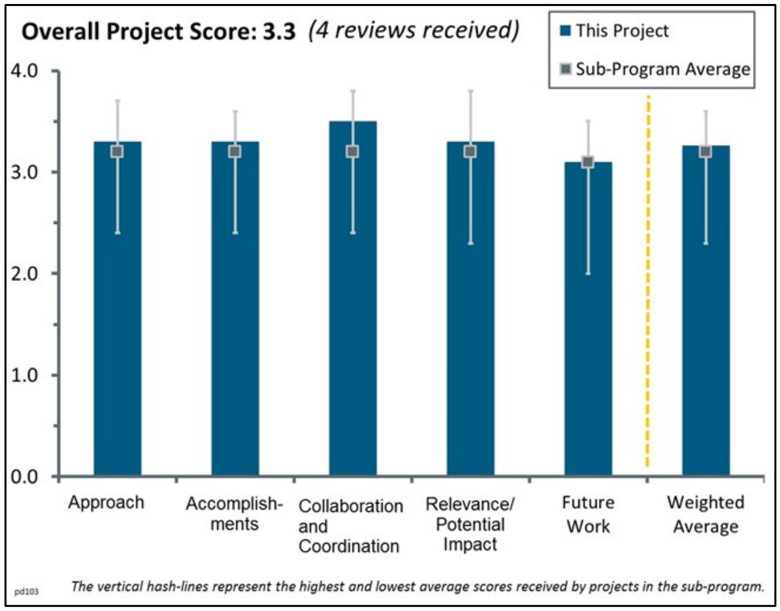
- The project should ensure that assumptions are harmonized with ANL's analysis since the project team is working on the same cases to assess the lifecycle footprint of the technologies. Some of the assumptions appear to be different, such as the stack replacement schedule of the SOEC and the process flow of the fermentation pathway. Perhaps researchers in academia are working on similar technologies. It would be good to get their perspective on the TRL of the technologies and compare results from this work against published literature. In addition to the "today at production volume" and "future at production volume," it would be good to see the "fabricated today at today's volume" case to understand where the technology is and then explain how learning by doing and higher production volume will reduce cost. Regarding "fermentation results, future case," the assumptions of broth density seem very aggressive given what has been achieved in the laboratory so far. Given that the corn stover concentration is based on BETO goals, it would be material to vet this assumption with academia and industry and assess the likelihood of achieving this concentration level. Regarding SOEC results, the current central case is based on assumptions of technology at scale. It is misleading to call this case “current” given that the data do not reflect current technology costs. Rather, they are based on assumptions of future potential volumes of production.

- “Future” is a somewhat ambiguous term since it could be 5 or 25 years or more before the technology is implemented at the projected scale. Given that future cost projections are based primarily on DOE R&D targets, it would be good to know at what point DOE goes back and reevaluates the current cost projections of the technologies that have been considered under this project to see whether the predicted cost reductions are on target or have been met and to determine whether there are different cost drivers that would require refocusing R&D priorities.
- Industry workshops to derive RD&D projects with industry based on the results would add value to the project.

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

Hui Xu; Giner, Inc.

Brief Summary of Project:



The objectives of this project are to (1) scale up and commercialize low-platinum-group-loading oxygen evolution reaction (OER) catalysts using the Giner, Inc., (Giner) polymer electrolyte membrane (PEM) electrolyzer platform and (2) evaluate the impact of newly developed catalysts on PEM electrolyzer efficiency and cost.

Question 1: Approach to performing the work

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

- The project is well designed and feasible. It is integrated with other efforts, including using Giner and National Renewable Energy Laboratory (NREL) standardized protocols for electrochemically active surface area (ECSA) with Hg underpotential deposition (UPD) on Ir vs. IrO_x, and transmission electron microscopy (TEM) at Oak Ridge National Laboratory (ORNL) before and after evaluation under harsh conditions.
- The project is using a good approach to replace expensive platinum-group-metal (PGM) catalysts with less-expensive catalysts with equivalent performance. The project should do a techno-economic analysis (Hydrogen Analysis [H2A] model) to determine the cost savings the project technology will achieve.
- The focus on durability test protocols is particularly encouraging.
- The project addresses the barriers of electrolyzer cost by lowering PGM loading. The project addresses the electrolyzer performance barrier and high anode overpotential. It is not clear what the strategy is to mitigate Ir dissolution and migration.

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 nanostructured thin film (NSTF) and Giner anodes demonstrated superior performance compared to the standard baseline anode. The Giner catalyst had comparable performance to the baseline with one-fourth the PGM loading. Giner and 3M successfully scaled up catalyst production. The project identified Ir migration from anode to cathode as a degradation path and potential shorting mechanism. The team has developed an Hg UPD protocol for determining surface area on oxides and metals.
- This project demonstrated progress against Fuel Cell Technologies Office (FCTO) Multi-Year Research, Development, and Demonstration Plan (MYRDDP) performance indicators where the performance of a small batch of Ir/W_xTiO_{1-x} OER electrocatalysts was inferior to a large, scaled-up batch. This activity difference was due to Ir having fallen off after voltage cycles and was found more on the membrane and no longer on the carbon support and/or was aggregated in some other measurements. Moreover, the activity of the scaled-up catalyst, while excellent, was not stable on the two-days-and-longer timescale. This, however, was not a problem because another batch, synthesized in March (only discussed during the presentation, i.e., no slides), exhibited excellent performance and was stable by rotating disk electrode (RDE). However,

the project should be cautioned about use of certain materials as standards when the electrodes are fabricated by Giner and not purchased as full membrane electrode assemblies (MEAs) because the performance may suffer (e.g., Johnson Matthey Ir Black). Along this line, the reproducibility of the materials and electrodes seems poor. Also, hydrogen crossover was small when mitigated membrane decals were used. During the presentation, it was mentioned that F content in solution was measured using inductively coupled plasma (ICP) to assess stability, but the data and results were not included in the report or talk; they should be included next time.

- The materials showed good initial performance, but the durability tests show that improvement is still needed. The development of standard testing protocols was very interesting. It is not clear how the researchers will engage the electrolyzer community to try to get acceptance of their protocols. The accelerated testing is interesting but needs to be validated against real-life data.
- It would be helpful to better understand the "mitigation" process and its impact on the 3M membrane since that appears to be critical to the membrane's hydrogen crossover performance. Perhaps there is something general to be learned here.

Question 3: Collaboration and coordination with other institutions

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

- ORNL analysis has been able to identify Ir migration from anode to cathode. All partners are involved with the work, with 3M and Giner supplying catalysts, ORNL performing microscopy, and NREL studying MEA degradation. A method was developed to measure ECSA, but the correlations between SEM/TEM microscopy, measured surface areas, and performance need further investigation.
- There is a healthy "competition" between Giner and 3M for the best lead catalyst concept, and the project is leveraging expertise at national laboratories. Microscopy done at ORNL was critical to understanding aging behavior.
- The work is nicely collaborative and involves interaction with NREL, 3M, and ORNL.
- There is a strong team that has well-defined roles.

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.

- This project exhibits progress toward MYRDDP targets where Phase IIB funding (Small Business Innovation Research program) resulted in two OER catalysts, Ir/W_xTiO_{1-x} and Ir-NSTF, with much lower PGM loading, although arguably Ir may be more costly and resource constrained than Pt.
- The work is relevant and should lead to decreased PEM electrolyzer costs through lowered catalyst costs. Lowered electrolyzer cost can have an impact on hydrogen cost, especially in situations such as using stranded renewables where the electricity cost is very low.
- It is appropriate for DOE to support continuous incremental improvements in electrolysis through both fundamental and applied research.
- Low-temperature water splitting for hydrogen generation is very relevant to the FCTO. The development of the testing protocols is needed. How the project will get the protocols accepted is not clear. Getting stakeholder acceptance is beyond the scope of this work. Without a techno-economic analysis (H2A), the potential impact of the work is not clear. The project needs to do the H2A to understand the potential impact of the work.

Question 5: Proposed future work

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

- The future planned work is logical, including placing the developed electrocatalysts in a sub-megawatt stack, although "sub-megawatt" should be more precisely defined. It is less clear if there are decision points. It does not seem so based on the milestone and delivery chart.
- It is not clear what the strategy is to mitigate Ir dissolution and migration.

Project strengths:

- Giner is a leader in the electrolysis field. Other strengths include attempts to develop accelerated stress tests for electrolysis and a method to measure ECSA for metals and oxides.
- Project strengths include the ability to do scale-up and testing with a clear path to commercialization of promising leads.
- This is a strong team working on interesting problems.
- This is an interesting project and idea.

Project weaknesses:

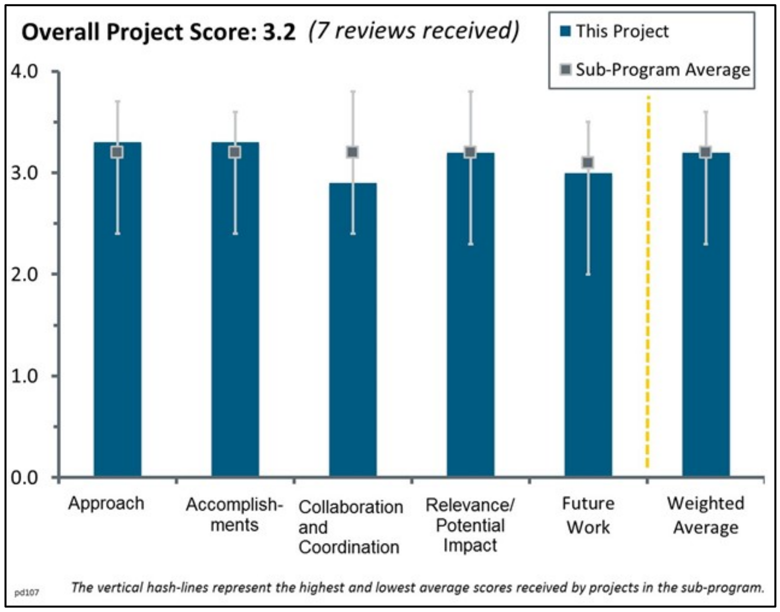
- The researchers need to work on the durability of their catalyst. They need to validate the accelerated testing protocols. They should look to the PEM fuel cell protocols for any additional durability tests. They need to do a techno-economic analysis (H2A) to determine the projected impact of their work on hydrogen cost.
- It would be beneficial to perform an H2A model analysis and compare results with other similar technologies because, notably, the use of Ir seems prohibitive in practice.

Recommendations for additions/deletions to project scope:

- Understanding the role of particle migration/sintering upon cycling (and dependence on size and initial dispersion) for these Ir systems would be helpful for this project and others with similar PGM minimization objectives. The national laboratories might be engaged for this.

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

Amgad Elgowainy; Argonne National Laboratory



Brief Summary of Project:

The objectives of this project are to (1) evaluate theoretical precooling requirements at hydrogen fueling stations; (2) collaborate to acquire information on refueling operation and review results; (3) examine current precooling equipment design and cost; (4) identify major drivers for precooling cost and energy consumption; (5) analyze tradeoffs between different design concepts; and (6) vet analysis results and findings.

Question 1: Approach to performing the work

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

- The approach is based on a single refrigeration cycle that is presumed to be representative of the industry. In reality, each of the station suppliers has a very different approach to solving this problem and this program does not cover the variety of market solutions very well. The analysis of the hydrogen halide (HX) effects on back-to-back (B2B) fills is interesting and valuable. Developing an improved methodology to determine the HX design parameters and to provide guidance to the refrigeration design is valuable. Similarly, the effects on Joule-Thompson (JT) expansion and HX performance are new and interesting.
- The project is well-defined with a targeted approach involving modeling of pre-cooling that takes into account appropriate variables and different fill scenarios. The tasks are feasible and integrated well with other efforts.
- The project is clearly defined, well-designed, and presented well.
- As the principal investigator (PI) noted, it is difficult to obtain pre-cooler designs from hydrogen station providers. There are some pre-coolers with designs that differ from those presented. However, in principle the PI has a sound understanding of their design and operation. The approach is robust and comprehensive.
- Very focused approach including review with industry.
- Interesting work—the approach is pretty well explained.
- The approach seems excessively focused on exactly what is done today instead of looking at better alternatives. Seems to be that the process lends itself to optimization, and it would be important to understand why things aren’t done better. The presenter stated during the presentation that, perhaps, energy efficiency does not matter—this poses significant ramifications for future delivery technologies.

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.

- Good accomplishments and significant progress are clearly presented. This project is sharply focus on improving the performance of hydrogen refueling stations (HRS) in terms of thermal loads, cooling system size, and associated costs.
- Good in-depth analysis of refrigeration process.
- Some good initial findings -- looking forward to more results from the project.

- The analysis is articulated well with relevant results showing correlations of key operating parameters as well as scope for improvement with respect to cost and energy usage for pre-cooling. It would be helpful to point out more clearly potential benefits of this exercise on the overall cost and energy consumption for dispensed hydrogen, and how it is helping progress towards DOE goals.
- Many good calculations for a small and brand-new project. I would, however, question some of the results. It seems to me that J-T expansion heating is overestimated. Running Refprop, the reviewer obtained a maximum J-T heating of about 30 K, for a maximum temperature downstream of the expansion valve equal to 25+30=55 °C, not 70 °C as shown in the figure. Also, Figure 8 shows a very sharp change in slope for the heat exchanger cost. It seems that heat exchanger cost should continue to gradually decrease as cooling capacity increases.
- While the reviewer did not see much value in the assessment of the refrigeration loop, the reviewer perceived the work that describes the operating limits on the HX as it pertains to J-T valve placement and B2B fills is valuable and suggested that it should be the focus of this effort. The reviewer also noted that the costing studies do not appear to be consistent with the limited cost data seen in other station designs and is of less value.

Question 3: Collaboration and coordination with other institutions

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

- Collaborations with other institutions are well-coordinated and effective.
- Good coordination of efforts. It is noticed that Linde was dropped as a collaborator from FY 2015 to FY 2016. A station operator is important to have as a partner to verify realistic data and to ultimately implement any improvements resulting from this exercise.
- Good collaborations and sets of partners, but the project may benefit from further interactions with Linde, Air Products, Praxair, etc., to figure out what they envision for the future. If this is an important problem for gas dispensing, these companies may have insight on possible approaches and they may even be willing to share their knowledge.
- The biggest weakness in the program is the lack of design data and information from existing cooling systems. Without this, it is difficult, if not impossible, to provide a good assessment of the station designs. A partner who is able to bring this information to the project would strengthen the team's capability considerably.
- No direct collaboration with station technology providers and operators. It would seem they would be interested in the cost and energy efficiency of their systems.

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.

- Since HRS requires significant effort in designing a system that could deliver hydrogen fuel under low temperature and high pressure safely and effectively, this project certainly provides the good solution for that. It has a potential impact on the success of hydrogen fuel delivering systems. The project fully supports and advances the progress toward DOE Hydrogen and Fuel Cells Program goals and objectives.
- This is an important and increasingly relevant topic of study—how to reduce station costs and improve efficiency of operation.
- Compressed gas dispensing is plagued by many issues, cooling being one of them. This is relevant to hydrogen-fueled transportation at least in the near term while a better approach to refueling and dispensing gains prominence.
- The development of a tool set that allows for better design of the cooling systems remains a critical weakness in station design and a gap that needs to be addressed for next generation station designers.
- While the quality of work done is very good, its importance and potential impact is questionable. As shown in slide 3, pre-cooling cost is only 10% of the installed station equipment cost. Thus, of the total cost of

dispensed hydrogen, it is a small portion. Moreover, the project is not targeting elimination of this cost, but simply optimization of it. So, the cost savings are expected to be a small fraction of the overall cost. While any cost savings are useful, it is a question of prioritizing in light of limited funding available. The analysis done to date is useful, but it may be reasonable to expect station designers to implement improvements and optimize operations to minimize cost.

- No high impact on capital cost. Higher potential to decrease operating costs through higher energy efficiency.

Question 5: Proposed future work

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

- The proposed future work is logical and technically sound.
- It seems that an important task may be finding a plan to eliminate station cooling. It seems that this effort would be well justified to avoid all the issues presented here.
- I would like to see an emphasis for the remainder of the project to be on the effects of the HX design on the ability to conduct B2B fills. If we had a better understanding on how B2B fills and heat exchanger properties affected the refrigeration requirements for the system, this would be a big help.
- I would suggest looking at the worst-case SAE J2601 scenarios to ensure the pre-cooler is adequately sized for “peak load.” This is typically the 50 °C ambient top up fill. Otherwise, proposed future work is a good summary of action items to enhance the current level of analysis.
- Very design focused. Since Honda is a collaborating partner, the reviewer suggests checking to see if results from the European project HyTransfer or other alternative approaches like this are taken into account for future work.
- This was a bit rushed in the presentation and could be made a bit more clear, but the project seems to be on track.
- Future work in the last Annual Merit Review (FY 2015) had the first item as “Design & develop new design concepts” (e.g., carbon dioxide, R507, etc., as well as relaxing the SAE J2601 30-second window). These do not seem to have been addressed. While the project has made significant progress in improving fundamental understanding of pre-cooling options, associated costs, and efficiencies and scope for improvement, continuation of this effort is questionable. It is not clear if it would add much value given the future work plan.

Project strengths:

- This project strengths are as follows: technically well-designed and planned logically; well-coordinated with other collaborators and institutions; and has been carried out in a timely manner, on schedule, and making good progress.
- Excellent analysis of pre-cooling requirements with good understanding of fundamental concepts. Adequate attribution of practical operating parameters and tangible outcomes.
- Experienced researchers.
- Systematic approach and relevance of station cost barrier.
- Exploring in detail the issues associated with hydrogen pre-coolers, which is a relatively poorly studied area.

Project weaknesses:

- Technically, there is no weakness in the design and analysis of this HFS pre-cooling analysis.
- Industry collaboration.
- The project is not expected to make a major impact on the overall cost of dispensed hydrogen. Although potential improvements are identified, the implementation strategy is not well defined. Efforts should now focus on field demonstration and manifestation of benefits.

Recommendations for additions/deletions to project scope:

- It is suggested that:
  - Consider using double pipe heat exchanger installed between the variable area control device (VACD) and HX
  - Locate the VACD further upstream of HX, not by using straight pipe, but by using winded-coil so that the VACD can be physically installed close to HX when it is necessary due to space constraint.

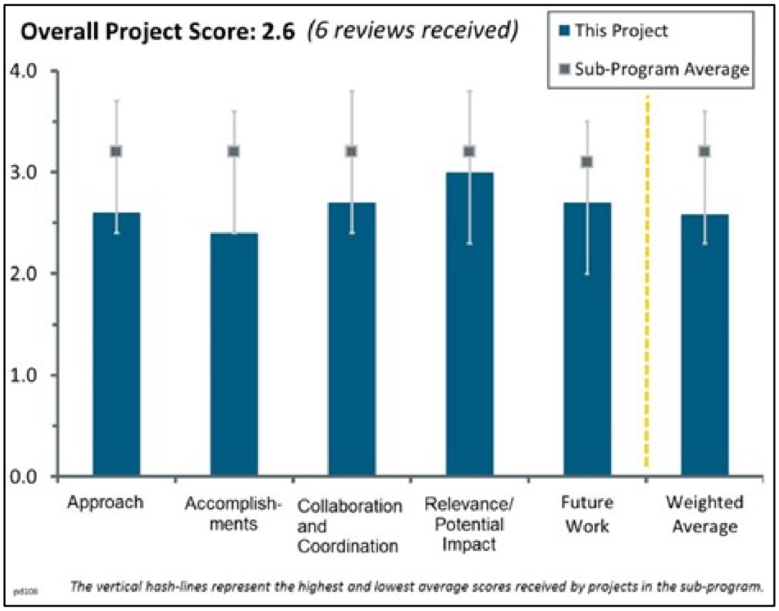
  Or item 1 and 2 can be combined to dissipate the extra thermal energy before hydrogen enters the pre-cooler HX.
- The project may be concluded with concrete suggestions to station designers and operators to reduce cost and energy consumption.
- Perform industry workshops and derive research, development, and demonstration projects with industry based on your results.
- Investigate and explain why things are done better. It is unclear if it just an effort to minimize initial cost, considering that energetic cost has little effect on the bottom line.

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 above 95% by minimizing aerodynamic losses, (2) reduce capital costs to half those of conventional reciprocating compressors by minimizing part count, and (3) reduce required maintenance by simplifying the compressor design to eliminate common wear items.



Question 1: Approach to performing the work

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

- The approach appears to be sound. The design seems to be progressing. There are many computer models and much theory, both based on simplifying assumptions. Very little validating data are shown.
- The novel compressor design is interesting. The project objectives seem ambitious, especially the reduction of cost statement. From the presentation, it seems that the project team was proceeding to build a compressor using this novel compressor design. It is suggested that the team build and test one stage first and determine that the proof of concept works before moving to building an entire compressor. The presentation made many performance claims based on theory, calculations, and predictions. The assumption of 100 bar inlet restricts the usefulness of this compressor in hydrogen station applications.
- There are three areas of concern with the approach:
  1) Major design changes, such as the switch from a sapphire to a ceramic piston—although it was probably necessary—do not engender much confidence that the team has thoroughly thought through the design issues with this machine.
  2) The team is not planning any reliability testing despite the importance of compressor reliability issues in industry. Given the novelty of the design, significant reliability issues would be expected with the first version.
  3) The footprint of the machine as laid out is likely impractical for a commercial hydrogen station. This could be remedied and is not a showstopper, as this is just a prototype, but the team should put some thought on how to shrink the overall package.
- The drive mechanism is novel, but it is unclear whether it is more efficient than existing technology. The investigators propose using superconducting magnets to increase efficiency. It is not clear how superconducting magnets would work in this application.
- It is not clear whether the cost barrier will be overcome through the proposed approach. No information is provided on the new design's effect on part-count reduction. In addition, it is not clear whether the efficiency claims can be experimentally verified until the end of Year 3. Go/no-go gate criteria have not been provided. It appears that the project will spend two thirds of the budget without any experimental verification of the assumptions that need to be proved to overcome the barriers. Thus, while the work seems technically sound, the overall approach could have been better.
- The approach to this phase of the project is fair, primarily owing to the balance of the reporting at the AMR and within the reviewer information provided. Beginning manufacturing without a proper risk mitigation plan seems unreasonable.

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

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

- This project is fundamentally a product design exercise and, as such, has multiple parts, including design of the magnet area mechanism, dry running piston section, and check valves given the high compression ratio, speeds, flow, etc. Regarding magnet assembly, accomplishments as presented display good engineering principles for magnet theory and good understanding of how the actuator should respond and act. The key area for concern is the extremely high cost per kw/kg compressed with this arrangement (fundamentally due to the power required to actuate the actuator), which far exceeds current compression technology. Further, the presentation states that the project is able to achieve 1.3 kWh/kg (using an advanced linear motor reciprocating compressor [LMRC]). This claim is presented with no base-level backup analysis and is simply unreasonable and unbelievable. Proving that significant reductions could occur should have been a go/no-go gate. Perhaps a conclusion could be drawn between the papers the project team members have read and their assumption, but it was not presented. Additionally, on the linear actuator point, the use of neodymium magnets is a cost-volatile choice. Neodymium rare earth magnets are subject to wild price swings, as the majority of this material can come from conflict zones, and magnet prices sometimes swing up to five times the normal market price. This should be taken into account for long-term cost analysis. Efficiency calculations were also extremely low, and no hard analysis was presented to explain the principal investigator's 70% possible improvement on efficiency. In looking at the piston section design, it seems that there is an unusually high dead volume at the top of the stroke of the actuator, which would severely limit the efficiency possibilities of the machine. Seal life and maintenance life values are not substantiated—data must be presented as to the baseline for these assumptions—especially because of the real fact that there is no standard for a 900B check valve/seal design for dry-running compressor technology. Additionally, it is not clear that the project has considered how brittle ceramic is or its possible effect on performance and actual function. The test loop is reasonable.
- The design seems to be progressing. The material selection is spotty. Some materials are defined by composition, form, and heat treatment (ferritic ductile iron casting ASTM A536 grade 60-40-18), some are defined by composition and heat treatment (Aluminum 6061-T6), and some are defined by composition only (Incoloy 903). Proper material definition would indicate a deeper understanding. It would have been nice to see some preliminary test data. To date, only theory and computer models have been provided, both based on simplifying assumptions.
- The project seems to be progressing according to the milestones listed. The full-scale design estimates of 930 cycles per minute (cpm) operation seem to be significantly higher than the 360 cpm (or 6 cycles per second [cps]) scheme that is listed in slide 12—thus, the full-scale design isentropic efficiency might be lower than the listed 99% efficiency at 6 cps operation. The definition of an "advanced LMRC actuator" is not given, and no justification is provided as to how this "advanced LMRC actuator" will achieve a specific energy of 1.3 kWh/kg.
- The energy usage of 9.2 kWh/kg is more than an order of magnitude above the DOE target of ~0.8. As this number does not even include the cooling load, the issue is even more problematic. When asked how the team had confidence that it could move its efficiency from 20% to 90%, it did not have satisfactory answers. It is questionable whether, in the absence of evidence to the contrary, a reasonable efficiency can be achieved. This is a serious problem and should kick off a detailed project review to determine whether, and if so how, the project can be saved.
- Increasing current observed efficiency from 20% to 90% seems a daunting task. Some of the suggested approaches, such as superconducting magnets, seem too complex and costly for forecourt use.

Question 3: Collaboration and coordination with other institutions

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

- The project has multiple industry participants that are all pertinent to the success of the project. It is not clear whether the participants (other than SWRI and ACI) are providing only parts/design or in-kind

support as well. The issue of cost increase due to wrong materials selection (high-strength–low-alloy steel vs. superalloy) seems to indicate weakness in the project team.
- The collaboration within the team seems adequate. However, the team could benefit from additional feedback from station operators and designers. Had that collaboration existed, the advisor could have noted the low efficiency as a non-starter early on in the project.
- The collaboration is present, but moving ahead with a build of the machine without coming close to project goals of efficiency and other targets should have been more closely considered between the collaborators ACI Services Inc. (ACI) and Southwest Research Institute (SWRI). Everything is a creation on this project, and as such, there seems to be no collective plan to handle it if something does not work.
- The collaborators appear to be suppliers, not collaborators. It is not clear whether they had input into the design, selected and designed their components, or only supplied a catalogue component. Some evidence, even anecdotal, would help to show that the collaborators are contributing.
- The project needs input from a compressor manufacturer. Having a component manufacturer is valuable but not as good as a partner that makes actual compressors (Hydropac, for example).

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 project objectives are relevant to Fuel Cell Technologies Office goals in developing a low-cost compressor with reduced overhead and maintenance cost and high efficiency. If successful, the project has the potential for significant impact. However, budget issues and lack of experimental verification of various assumptions early in the project presents a risk that the objectives may not achievable.
- Because compressors are such a reliability and cost issue, the project could potentially have a great impact. This is provided that the team can overcome the very low efficiency of the machine and demonstrate adequate reliability and cost.
- If the project can turn it around and prove performance far exceeding what was presented, its relevance could be prominent. A good deal of additional analysis and design iterations must be made to reach potential cost targets.
- If successful, this has the potential of reducing the operating and capital expenditures for hydrogen compression systems.
- If successful, the project could have a significant impact on forecourt costs.

Question 5: Proposed future work

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

- The next steps are logical.
- The proposed future work is good in terms of proceeding according to the plan. It seems risky and very challenging to leave all the following to budget period 3: the fabrication of stages 2 and 3, their integration with stage 1, performing all the data analysis, and possibly resolving any potential issues that invariably crop up in any experimental/demonstration work.
- It is this reviewer’s opinion that a design iteration to improve efficiency would be needed; however, the project is moving ahead to fabrication.
- It is not clear why we are proposing to build a 20% efficient compressor. Construction on the first stage has apparently gone too far to be stopped, but work on the second and third stages should be stopped and design changes made, verified, and implemented before construction is considered. There is skepticism regarding the ability of a ceramic–ceramic sliding seal to contain high-pressure hydrogen. This should be verified on a small-scale prototype before proceeding with full-scale.
- Future work information provided is too vague to evaluate with any depth, but it does not appear to address major risk issues with the project.

Project strengths:

- The concept is a strength, as it is a way to reduce moving parts and reduce footprint. The control scheme and test loop considerations are all sound and are strengths for a product.
- The project is investigating a novel design that has the potential to effectively address major compressor issues.
- The project hopes to overcome several barriers with its innovative design of a reciprocating compressor, such as through the use of a ceramic piston to reduce the coefficient of friction and maintain tight seals.
- The project presents a novel approach to compressor design.
- This is a somewhat novel approach.
- Theory and computer modeling are project strengths.

Project weaknesses:

- Fundamental concepts and analysis are missing/not considered. Cost analysis for rare earth magnets should be considered in long-term analysis. Seal clearance (in the technical backup slides) is extremely close, and for easily scratched parts (sapphire/ceramic), it could be a huge challenge to ensure keeping a seal at the high-pressure stage of compression. Low pressure could be fine. Additionally, at low speeds, this seal will have a tendency to leak faster that when at higher speeds. It seems a good deal of analysis went into the frame structure and not enough into the internal piston parts (for example, the real leak rate could have been tested statically).
- Current power consumption is greater than that of existing compressors, and ideas for improvement have not been carefully developed.
- Stage gates were not defined; experimental validation of the assumptions was not performed early in the project.
- The goal of 9.2 kWh/kg is not viable. This number should prompt a detailed project review and accounting for how or whether the project should go forward.
- There is a lack of bench data.

Recommendations for additions/deletions to project scope:

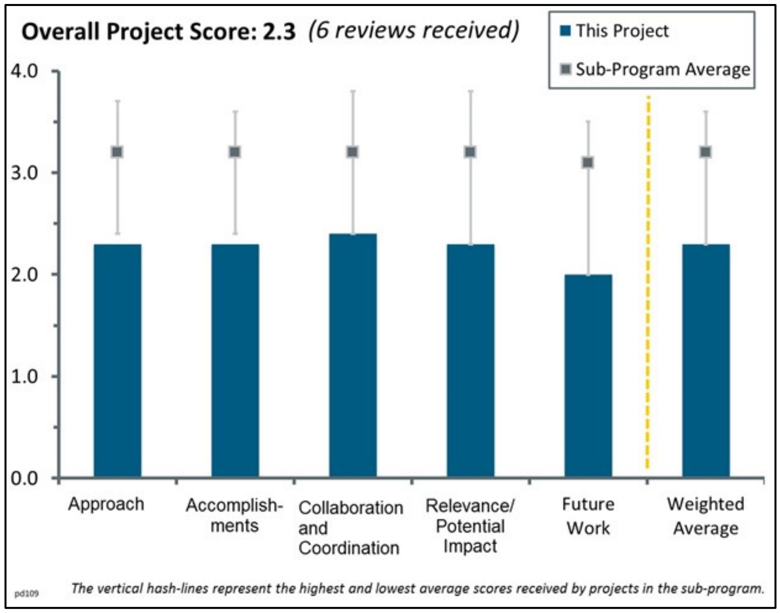
- The project should focus more on kilowatt-hours per kilogram of hydrogen rather than isentropic efficiency. The former is the metric that really matters. Investigators need to work with compressor manufacturers to validate assumptions and approaches.
- The project must regroup to analyze the piston area and seal on a smaller scale before the full build. Efficiency improvement would be a go/no-go gate to continue or, by analysis, determine the real possible improvement level.
- The project should describe the “part counts” of an LMRC vs. a conventional reciprocating compressor and provide the data that experimentally verify the life of seals (48 months) and valves (4 years).
- Reliability and efficiency testing should be added.

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

Zhili Feng; Oak Ridge National Laboratory

Brief Summary of Project:

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



Question 1: Approach to performing the work

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

- The approach seems reasonable, but the project should look at other options to ensure funds are not spent pursuing a non-optimum solution, as happened with the concrete reinforced vessel. Costs should be benchmarked with competing technologies—not just the U.S. Department of Energy (DOE) goals since other technologies are advancing quickly.
- The team's approach is to build on a previous Generation I (Gen I) steel concrete composite vessel to reduce the cost of hydrogen stored at pressures of 875 bar. The first set of analyses performed by the team was mostly focused on cost optimization. However, it was surprising that the team has not yet performed any structural analysis (e.g., finite element method analysis) or embrittlement analysis that would guide/support the cost optimization.
- The approach seems to be primarily an extension of the lower-pressure approach project. Some of the lessons learned, such as load-sharing wrapping, are being incorporated, although the movement toward replacing the pre-stressed concrete starts to point toward a reasonable design that might meet needs for both manufacturing and transportability. It would have been good to have seen more of a multivariate materials screening methodology applied to the project to ensure the best of all the appropriate materials are being utilized.
- The principal investigator (PI) was looking at a stainless layer inside the high-strength-steel shell, which would then be reinforced with concrete. Now the project is looking at no layer and steel-wire-wrapped high-strength steel. The approach seems to be all over the spectrum. The PI proposes to "replace the stainless steel inner layer with low-cost materials as [a] hydrogen permeation barrier," but there is no clue as to what that barrier might be.
- The intent of the project was to use low-cost concrete to build a low-cost vessel. At this point, the design does not incorporate concrete in any appreciable way. Barriers to the use of concrete have not been overcome.

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

This project was rated **2.3** for its accomplishments and progress.

- Current techno-economic analysis is revealing a new direction in tank design, and screening high-tensile steels could be very beneficial across the Hydrogen and Fuel Cells Program.
- There is a certain irony to this project. It began as a steel vessel reinforced with concrete, and it has evolved into a steel vessel wrapped with steel wire—eliminating the concrete. While this is an accomplishment, it might pay to consider how this outcome might have been foreseen, thus avoiding the expense of the concrete-wrapped tank that was essentially non-productive.
- It is not clear that there is any need for a large-volume 850 bar vessel, as this would not be very helpful in cascade for an SAE J2601 compliant dispenser. It was not clear why the PI switched from the previous concept of concrete reinforcement to wire-wrap reinforcement, and achieving success with the new approach does not look any more feasible.
- Given a start date of October 2014, it seems that the main accomplishment by now is the cost optimization beyond the Gen I vessel. Given that the project has only another one and a half years remaining, it is not clear how the team will be able to assess fatigue life, assess hydrogen embrittlement, and design and build the Generation II vessel.
- There are other pressure vessel technologies, such as composite overwrapped pressure vessels (COPVs), that provide a much more efficient method for hydrogen storage and transport.
- Although the project is still proceeding, it has not validated its original premise for use of concrete. It would be better to end the project and let other projects carry the work forward.

Question 3: Collaboration and coordination with other institutions

This project was rated **2.4** for its collaboration and coordination.

- The collaboration between tank designers, cost analysts, and materials experts is a very strong aspect of this project.
- A big team is involved; however, the activities of the different team members are not yet clear, given that most activities have focused on cost optimization.
- Reliance on ASME codes will be the downfall of this project and any innovation on steel tanks for hydrogen service. Already there are European Pressure Equipment Directive tank designs that are welded steel, are 50 bar working pressure, and contain ~375 kg in 90 m^3 transportable vessels (the largest tanks that can be shipped by regular lorry over most roads) that are priced at $320/kg. Collaboration with Pressure Equipment Directive standards at the Compressed Gas Association level has been blocked for years, and the United States has weak standards for welded tanks. Tanks built for 50 bar service in the United States to ASME standards will be at least 50% thicker wall and have 50% greater materials costs, but these tanks will still beat DOE cost targets.
- Other collaborators are listed, but the work seems to be focused mostly at Oak Ridge National Laboratory.
- It is not clear that adequate interaction with fuel station providers is sufficient to warrant production—i.e., it is not clear that there is a need for these vessels.

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

- If the goals are reached by the end of the project, the potential impact would be strong. It is just not clear that this will be attainable in the remaining timeframe.
- This project could potentially provide a vessel that could meet the current cost targets while being manufacturable and transportable, thus supporting the growth market for hydrogen stations across the nation.

- The work is relevant, but DOE should make sure that competing technologies are not on target to equal or surpass the cost/performance of this material.
- The pressure rating of the vessels is not particularly useful for fueling. The pressure rating of the vessel is roughly equal to that required for fueling, leaving little usable quantity. Hydrogen fueling stations do not have a need for such large vessels at this high of a pressure. The cyclic life is not likely to be sufficient for a fuel station. There is virtually no mention of cycle life or a plan to address testing cycle life. It will likely be impossible to fill the vessel while it is being used because it will be producing a large number of cycles, even if partial. It is not clear that the "cost" would end up being a real sale "price" until someone is willing to build and sell for the cost listed. There would be other corporate overhead costs and the profit margin to apply.

Question 5: Proposed future work

This project was rated **2.0** for its proposed future work.

- The focus on health monitoring and inspection is good and could be cross-cutting. It would be good to see more of a multivariate screening of materials.
- The focus should be on the use of Type 4 polymer-lined tanks for high-pressure hydrogen storage. Steel liners are not the best for high-pressure storage. The PI proposes to "replace the stainless steel inner layer with low-cost materials as [a] hydrogen permeation barrier," but there is no clue as to what that barrier might be.
- Weaknesses include the lack of complete structural analysis, fatigue life assessment, and hydrogen embrittlement assessment.
- This work should not proceed until this approach has been compared to other storage technologies to ensure that money is not spent on another suboptimal approach.
- The use of concrete has not been proven to provide value; therefore, the project may as well be stopped. There are other projects already working on high-pressure wire-wound vessels of the same type.

Project strengths:

- The project has proven that concrete is not going to be a viable pathway.
- The project builds on previous experience.

Project weaknesses:

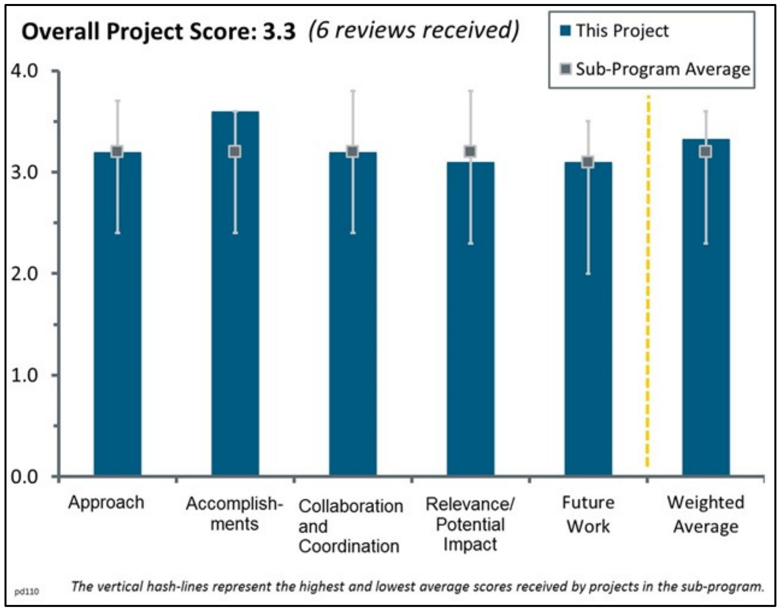
- ASME codes are not being aggressively revised to support hydrogen infrastructure needs—in fact, the team seems to be dragging feet.
- Failure to benchmark with competing technologies is a project weakness.
- Concrete has not proven to be a viable material, but the project is continuing.

Recommendations for additions/deletions to project scope:

- The project should coordinate with codes and standards to confirm that cylinder lengths will not compromise station design with setback distances. An independent benchmark analysis of this and competing technologies should be added.
- The project should eliminate the remote sensor technology cost optimization and focus on fatigue life and hydrogen embrittlement assessment and proof of concept.
- The project should stop progress and not proceed to the manufacture stage. There is already another project designing comparable vessels of the type to which this project has evolved.

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

Amit Prakash; Wiretough Cylinders



Brief Summary of Project:

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

Question 1: Approach to performing the work

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

- The overall approach is well planned and should lead to the development of a high-pressure vessel for hydrogen storage that satisfies DOE targets.
- Use of commercial cylinders should minimize costs. Project is well defined and described. Investigators are looking at several methods to improve design and performance.
- There seems to be progress in pursuing an alternative technology to composite wrap. It would be good to see a direct comparison of cost between wire and composite to understand the full, long-term benefit.
- The project seems to be moving along with the ASME stamp of approval for vessel design. The focus on fatigue crack growth rate (FCGR) is appropriate to answer many of the anticipated issues with the vessel and will provide a good basis for future design efforts. It is suggested that the FCGR studies be extended across both temperature and pressure cycling regimes, although doing so may be a difficult task.
- The approach based on assessing FCGR in the liner material starting from a flaw that is 3% of the thickness is a proper one. Autofrettage is used to reduce the tensile stresses, but stresses become positive upon the application of the load, so the purpose of exploring negative R ratios for liner fatigue is not clear.

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 accomplishments and progress of this project to date have been outstanding. The team has been able to approve an ASME code case for a Wiretough design, perform detailed fatigue tests, and plan for different design improvements.
- The project has progressed well and seems to be on target for producing workable cylinders. ASME certification is a great accomplishment.
- The ASME certification of the project results is a good progress indicator. The results shown on slides 9 and 10 constitute significant progress toward the analysis reported on slide 11. For the results shown on slide 11, it would be good if the team showed the magnitude of the hydrogen pressure reported in the first column next to the magnitude of hoop stress. The calculation of the cycles to failure also requires

knowledge of the threshold stress intensity factor range. It has not been reported what this threshold value is.
- These pressure vessels are way too heavy and impractical. Composite overwrapped pressure vessels could provide a much more efficient method for storage and transport of high-pressure hydrogen.

Question 3: Collaboration and coordination with other institutions

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

- Continued collaborations with the Oak Ridge National Laboratory (ORNL) on the fatigue studies will definitely benefit the project. Also, a number of other collaborators are involved in the different tasks.
- There is good work with ASME to certify cylinders. The project appears to be utilizing ORNL's and Sandia National Laboratories' resources effectively.
- Collaboration with Dr. Kevin Nibur is a unique strength for the project. Dr. Nibur is a world expert on experimental measurement of fatigue crack growth rates.
- Collaborations between Wiretough, ORNL, N&R Associates, CP Industries, Structural Integrity Associates, and Hy-Performance Materials Testing seem to be adequate to meet the project demands currently. The project might need a new partner that could possibly address cyclic fatigue across the actual pressures and temperatures.
- The stainless steel liner is a concern for hydrogen embrittlement.
- There is no collaboration with fuel station providers to understand whether the resultant product will meet industry technical needs, or whether there is a market for the vessels.

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.

- If successful, the project will have a significant impact on reduced-cost storage of hydrogen at high pressures.
- The project has the potential to decrease costs for station storage.
- The project aims to deliver a safe and reliable storage vessel at 875 bar hydrogen pressure on the basis of wrapping the cylinder with ultra-high strength fibers. The reviewer cannot assess the project's effectiveness in achieving its objective because there is no comparison between cycles to failure in the presence or absence of wrapping.
- The tank design shows potential to meet the cost targets. However, it is not certain that the principal investigator is considering installation in the model since it is not part of the target metric, but installation should definitely be considered when understanding costs.
- The pressure rating of the vessels may not be particularly useful for fueling. A statement is made that the pressure rating might be in the 10,000–15,000 psig range. To be useful, the pressure must be at least 14,000 psig, and preferably 15,000 psig. The cyclic life is not likely to be sufficient for a fuel station. There is mention of cycle life and material testing, but no details on final pressure rating, cycle range, and cycle count of final design. It is not clear that the "cost" would end up being a real sale "price" until there is someone willing to build and sell for the estimated cost. There would be other corporate overhead costs and a profit margin to apply.

Question 5: Proposed future work

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

- The project appears to be on track for successful completion.
- The hydrogen embrittlement and fatigue performance is great.
- Future plans are adequate. However, some focus on crack initiation and not just pre-existing defects would strengthen the design and safety of the vessel.

- There still seems to be a number of challenges remaining for this project to be successful; it is not certain that the researchers can address everything. The project should focus on a few of the challenges that would provide useful data to DOE for future efforts in the materials design space.
- The approach to reduce the yield strength of the liner seems to be moving in the right direction, but the gain due to autofrettage needs to be ascertained given that the compressive stresses will also be lower in that case.
- The project needs to perform actual cycle testing on the prototype vessel. The project should complete a cost analysis with detail earlier in the project than shown.

Project strengths:

- The project is pursuing the metallurgical testing for the base materials. The project is trying to demonstrate a new technology for vessels and is making progress.
- There is a good blend of practical and theoretical work to optimize cylinder performance and costs.
- The credentials of Dr. Saxena and Dr. Nibur are project strengths.

Project weaknesses:

- It is not clear 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 (slide 11) and hence positive R.
- There is insufficient discussion about fatigue life at a given pressure cycle range to understand whether these vessels will meet the intended service. There is no support for the basis of meeting the storage tank cost of less than $1,000/kg, other than statements that it will be met.

Recommendations for additions/deletions to project scope:

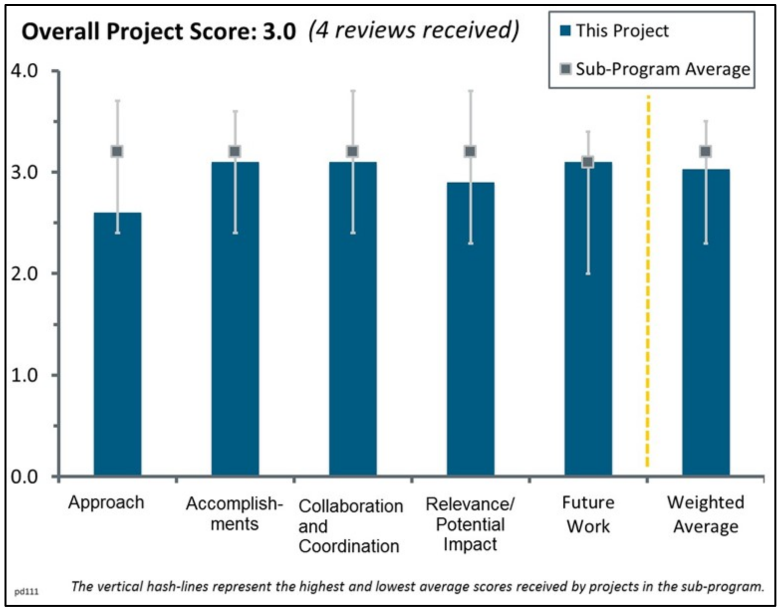
- The project should compare projected costs to competitive technologies in addition to DOE goals. DOE should set a recommended standard pressure cycling regime (cycles/day and pressure swing) to be used to estimate cycling effects on vessel lifetime.
- The effect of the axial stress needs to be investigated. In fact, possible failure scenarios due to axial stress need to be envisioned and outlined.
- The project should perform some crack initiation studies as well.
- There should be additional support for the contention that the sub-$1,000/kg storage tank cost goal can be met. Better define cycle range and number of cycles are needed. There should be a step to actually test and demonstrate the resultant vessel to these design parameters. This does not appear to be included.

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

Wei Liu; Pacific Northwest National Laboratory

Brief Summary of Project:

Bio-oil reforming technology advancements are being pursued in this work. Pacific Northwest National Laboratory (PNNL) is working to (1) reduce the capital cost of plants through minimized unit operations, smaller pressure swing adsorption, and process simplification; (2) increase energy conversion 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 **2.6** for its approach.

- It is good to see the focus on kinetics and heat and mass transfer. The project should accelerate this work and develop a basic process model to predict hydrogen production and bed temperatures as a function of feed and regeneration conditions. (Leveraging Dason Technology here would be a good idea.) Also, efforts should be focused on understanding the impact of coking and sulfur on catalyst performance and ability to regenerate; these responses have the potential to be killer variables. The techno-economic analysis shows a key feature of this project is a substantial reduction (50% assumed) in bio-oil feed to achieve the same hydrogen production. It is not clear how to assess the work being done against that goal. Also, it would be good to see the explicit relationship between regeneration conditions and hydrogen production cost.
- The project team is taking a good approach by starting with looking at materials innovation for both the sorbents and the catalysts, followed by demonstrating the reactor innovation with actual bio-oil, and then integrating the system and developing an innovative process.
- The early focus on identifying reforming catalyst materials, followed by optimizing reactor and process systems, is reasonable. However, the project should also provide the assumed mass and energy balance for the sorption and regeneration reactor systems. The stated improvement in smaller bio-oil usage per unit hydrogen produced may be explained by the fact that a significant percentage of the hydrogen is coming from steam. As such, it is not clear if the calculated cost reduction from this work accounts for the higher energy consumption due to steam production and high-temperature regeneration. The Hydrogen Analysis (H2A) model analysis is overly simplistic. It is not clear what the basis was for the assumed over 50% reduction in future bio-oil feedstock cost compared to the H2A default. Similarly, it is not clear why, in slide 4, the future capital and operations and maintenance (O&M) costs—the two identified barriers in the Fuel Cell Technology Office Multi-Year Research, Development, and Demonstration Plan (MYRDDP)—are assumed to be constant relative to the H2A default.
- In principle, the proposed approach is interesting and attractive: addressing bio-oil conversion issues by utilizing coke formation to its advantage. However, there are significant operational challenges, especially with operating a dual bed swing reactor system at high temperatures. Thermal management and cost of materials are some of the key factors. The investigators should refer to previous efforts to develop similar technologies for natural gas reforming, none of which have been successful; bio-oil is even more challenging.

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.

- Impressive progress has been made on identifying lead catalyst and sorbent materials and proving out the catalyst/monolith system (albeit on a simulated bio-oil, which avoids coking and sulfur complications). The reviewer looks forward to seeing the results with both catalyst and sorbent loaded onto a monolith.
- Accomplishments and progress are excellent on the sorbent and catalyst portions of this project, which are key aspects to the success of this work.
- Good progress has been made in developing novel catalyst/CO_2 sorbent materials. The monolith base has benefits with respect to pressure drop, catalyst distribution, and uniform gas flow. The critical step is operating a dual bed system continuously with thermal stability and consistent output. More time should be devoted to operational aspects; if successful, catalyst/materials can be further improved later. Based on prior experience with similar systems, the viability of this approach in the long run is questionable. While switching between reforming and regeneration steps, there will be some residual gas left in the system that will need to be purged to avoid contamination. This step can cause instability and added cost that should be addressed. It is essential to have mass and energy balance for the cyclic process to know how much coke must be deposited for adequate heat generation. The cycle time may need to be adjusted accordingly to make sure the system is thermally balanced, including the purge step.
- The catalyst performance and stability results look promising. In addition, the reported catalyst improvement over packed bed by use of a monolith reflects nice progress. That said, the project team should carry out additional parametric tests to pin down the mechanism for this observed improvement, namely the quantitative impact of changes in weight hourly space velocity or steam-to-carbon ratio.

Question 3: Collaboration and coordination with other institutions

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

- A very strong team and collaboration are covering the main aspects of this work: materials innovation (PNNL), catalysis (Washington State University), monolith support (Cormetech, Inc.), and process design (Dason Technology). There is a very good mix of industry, national laboratories, and academia.
- There is a good mix of industrial and academic partners.
- Selected partners bring necessary skills and capabilities. Nevertheless, a major industrial partner with significant operational experience could provide valuable feedback on critical challenges of high-temperature cyclic reactor operation. This is a deficiency in the current team.
- Overall, team collaboration is reasonable, although the project could benefit from partnering with a feedstock/bio-oil supplier.

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.

- This project will support DOE's goal of reducing distributed hydrogen production costs, and the H2A model analysis presented by the project team showed that this technology could potentially meet the production cost targets.
- Reducing the cost of bio-oil conversion to hydrogen is important for cost-competitive renewable hydrogen production. As projected in slide 4, the proposed method can result in a significant cost reduction. However, the assumptions need to be verified under realistic conditions. For example, there is a need to explain and verify a 50% reduction in bio-oil usage, as it contributes significantly to the projected cost reduction. The investigators should make sure that some of the critical issues on approach and progress are adequately addressed and properly accounted for in the cost calculations, for example, purging step operation and associated equipment, as well as high-temperature switching valves and other materials.

Although some CO_2 is removed in situ, based on the results presented and configuration shown, this step does not eliminate water–gas shift and pressure swing adsorption equipment. Thus, capital cost reduction is not obvious.
- It would be good to understand the potential for this technology vs. electrolysis in forecourt applications. The project should consider adding quantitative technology goals that target key barriers, in addition to 80% conversion efficiency and hydrogen production cost.
- The project is fairly aligned with the MYRDDP and aims to address key barriers to commercialization. However, the benefit of this approach over steam methane reforming is not obvious and needs to be made up front, including quick greenhouse gas (GHG) life-cycle analysis.

Question 5: Proposed future work

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

- The proposed work looks good and is aligned with the approach presented for this project. A key aspect for the success of this project is the demonstration of the integrated reactor system, and the team is planning to complete this task.
- The stated milestones and the proposed approach to achieve them are aligned with the overall objectives. The project needs to make the transition from simulated to real bio-oils before the end of the project period.
- Proposed future work addresses key areas but fails to adequately address cyclic operation in detail. Investigators should carefully consider thermal management, purging requirements, high-temperature switching valve selection, and durability of the high-temperature components.
- It would be good to see a vision for (1) how to deal with bio-oil composition variability and (2) how to scale up to approximately a ton of hydrogen per day.

Project strengths:

- The project team has strong catalyst material screening and conceptual reactor design capabilities.
- Project strengths include knowledge and expertise in catalyst and sorbent development.
- The partnership with Dason Technology is a strength.

Project weaknesses:

- It is ambitious to start with bio-oil as the feed.
- Not enough consideration is given to potential non-matching conversion and kinetics of the competing reactions for both reforming and regeneration conditions, which are likely to have significant impact on overall system design and O&M costs. Although not the primary goal of the project, there was not much discussion or analysis of the in situ CO_2 capture portion of the system.
- Project weaknesses include the team's experience with high-temperature cyclic operation and its challenges.

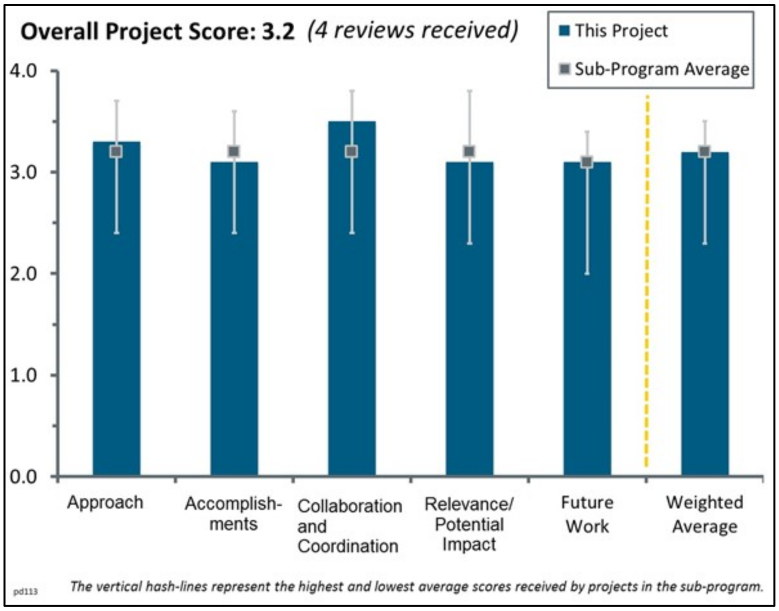
Recommendations for additions/deletions to project scope:

- The role of coke burn-off must be better understood. Questions include how much coke is ideal and whether the feed composition can be related to the amount of coke that will be deposited; the latter will be critical for bio-oil since the composition will vary considerably depending on how it is made. The project should consider modeling (or even testing) the use of bio-gas instead of bio-oil.
- The project should address the challenges of matching the reaction kinetics of the various reforming and regeneration reactions within the temperature and pressure operating envelope. The project team needs to address the likely sensitivities of the reforming catalyst performance with respect to temperature and the likely seasonal and regional feedstock variabilities. The 2006 reference to the National Renewable Energy Laboratory report and cost numbers looks outdated. There has to be a more recent and relevant reference.

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 greater than 3 L of hydrogen by the end of the project. Fiscal year (FY) 2016 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 techno-economic, sensitivity, and trade-off analysis 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.

- The SNL team has outlined an aggressive but reasonable approach for meeting project objectives by the end of 2016. The SNL team made the correct decision to focus on the demonstration of hydrogen production to meet U.S. Department of Energy (DOE) requirements. SNL continues to improve on timeliness and effectiveness of communications and coordination with the entire project team on component design and fabrication. The team also continues to improve on materials characterization (e.g., through a "virtual lab" using the stagnation flow reactor to investigate materials kinetics and thermodynamic properties). Although finding optimal reaction materials will require more time and resources than the current effort allows, it is anticipated that these investigations will continue in the future.
- The team has identified key barriers in developing this technology. Objectives are being met in a timely manner and directly address the pathway barriers. Further work is still required to finalize models and conduct the complete techno-economic analysis.
- The team made a good decision to settle on CeO_2 and focus on demonstrating the key aspects of the reactor design.
- The researchers have a very large collaboration that combines experimental work, theory, and techno-economic analysis. They need to state how they will characterize the materials in terms of redox potential, cycle life, mechanical strength, etc., or at least put in a reference where the characterization procedure can be reviewed. They need to define their target metrics in terms of materials performance—moles hydrogen/gram material, cycle time, cycle life, etc. This is missing and is very important to assessing progress and feasibility.

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.

- Excellent progress has been made in completing and validating the Cascading Pressure Receiver-Reactor (CPR2) design, in designing and fabricating reactor components in preparation for testing, and in combining modeling and experimental investigations to accelerate material discovery for materials of reaction. The project team continues to devise novel and creative testing methods (e.g., stick slip measurements to determine flow rates of the reaction particles) in preparation for full-scale testing of the CPR2. Materials discovery work has yielded promising results, but it will take extended investigations beyond the scope of this project to find an optimal material of reaction having the thermochemical and performance parameter values desired for solar-to-hydrogen (STH) efficiency >20%.
- The work in question has made significant progress in demonstrating the target technology. Obviously, great strides have been made in the solar receiver technology. Specifically, many materials have been analyzed for effectiveness. The study could still benefit from a better understanding of the problems regarding the synthesis of $CeFeO_3$. This may prove to be a critical issue because $CeFeO_3$ seems to be the preferred material for this technology.
- While the team is taking a rigorous approach to modeling the process, the lack of techno-economic analysis at this point in the project is disappointing. The techno-economic analysis should have been an earlier objective, even if subsequently revised, because it helps to identify the critical technical challenges, which might have changed the project strategy.
- It is good to see that the researchers have validated the theory by finding and testing some potential compounds. Whether the compounds perform as well as predicted is not stated in their slides, however. This project is very well funded, so the progress on constructing the test system is expected. They reported screening 50 new compounds, but it is unclear whether these compounds were the same ones reported in 2015, and the results of that screening were not shown. It was also unclear whether the compounds identified in the density function theory (DFT) modeling performed as predicted. This finding should have been reported because it would validate the DFT modeling task. In addition, how the screening was done is not discussed. For example, they do not disclose how many cycles compounds were tested for or how well the experimental work compared to what was predicted. This is extremely important. They are spending a lot of effort on DFT modeling to direct their discovery. They need to show that the modeling is accelerating the discovery, which from the data is not known. It is difficult to see what was changed and why on the MATLAB work from FY 2015. They were debugging the model last year and are now ready for exercising it. This was done on a sub-contract, which the presenter said they were not happy with and that they are changing contractors. Based on the results presented, a change in sub-contractors is a good decision, and the principal investigator/project manager should be commended for making the change. There do not seem to be any journal publications, which given the nature of the work, the funding level, and the length of time the project has been active is surprising.

Question 3: Collaboration and coordination with other institutions

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

- An excellent project team has been assembled for this project, which leverages materials experience and reactor design capabilities at SNL Livermore and Albuquerque, respectively, and academic materials discovery capabilities at several key universities, as well as access to DOE Office of Science facilities. The interaction with the German Aerospace Center will allow significantly more progress to be made in this effort than would be the case if the U.S. team were working alone. The new “virtual lab” capability for the stagnation flow reactor will facilitate further collaborations with project partners. It would have been useful to hear if any progress has been made (as in the photoelectrochemical [PEC] community) in coordinating across the research community to establish conventions for analysis, best practices, and key measurements, and in coordinating and communicating materials discovery approaches, testing protocols, and reporting standards.

- There seems to be sufficient collaboration between various groups. The groups also include both research and corporate aspects, which inspires confidence that production goals will be met. There is some indication from the presenter that there was a previous problem with the timeliness of work. This issue was also stated to be resolved, but careful team member selection should be maintained in the future.
- The project has a lot of collaborations.

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

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

- The project supports the Fuel Cell Technologies Office (FCTO) goal of reducing the cost of hydrogen production from renewable resources to < $2/kg, as well as the objectives to verify the competitive potential for solar thermochemical (STCH) cycles for hydrogen in the long term and to develop this technology to produce hydrogen by 2020. Meeting the cost goal will depend not only on the successful completion of this project, but also on technology development beyond the scope of the project and the FCTO (e.g., lowering the cost of the heliostats to the DOE Solar Program SunShot target).
- The project goals align well with DOE targets for water-splitting technology. While the work is only laboratory scale, at least some thought should be given to large-scale operational considerations such as heat recovery and equipment size for full operations.
- This technology is in its infant stage, so it has potential to make an impact, but until it is further developed, the impact is unclear. The system is extremely complicated and still has many technical challenges to be solved before its impact can be assessed.
- This technology does not have a convincingly reasonable potential to achieve $2/kg hydrogen. Multiple breakthroughs are required (e.g., materials, reactor design, and thermal management).

Question 5: Proposed future work

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

- Information provided on future work addressed the remainder of FY 2016 and included plans for an on-sun test, full techno-economic analysis, and publication of results. The planned future work is totally reasonable and as much as the remaining time allows. Additional discussion of next steps after the conclusion of this effort would have been helpful, even though they are, of course, contingent on the results of the 2016 tests and analysis. Continued cost analysis should take place. Heat recovery in the receiver-reactor will need to be more fully addressed, and additional materials discovery work will be needed to identify the optimum materials of reaction. Going forward, coordination with the University of Colorado and others on materials of discovery work would be beneficial.
- The approach taken to plan the project timeline, as shown in the Gantt chart, is appreciated. The project should develop a concept for particle illumination at scale and determine the implications for time on sun. The project should identify key challenges associated with heat management and determine implications for economics.
- The work is promising, but foresight should be given beyond laboratory-scale demonstration, when possible.

Project strengths:

- The project team and progress to date are excellent, with the project team focused on demonstrating hydrogen production by the end of FY 2016. The coordination between materials discovery work and component and reactor design and analysis is noteworthy and should serve as a model for future efforts of this kind.
- The project has demonstrated significant work in materials studies as well as manufacturing techniques. Further, the modelling work is well supported, and the amount of collaboration for the team is good.
- This is a well-funded, long-term project that has a very strong team.

Project weaknesses:

- No significant weaknesses were apparent; however, updates to H2A were not provided or discussed. No information was given on recent publications, patents, or collaborations with the greater research community on establishing testing protocols and coordination of analyses.
- Careful selection of team members needs to be maintained consistently to ensure timely work responses. Thought should be given to how large-scale operations will function, especially with regards to heat recovery and equipment size.
- The response to the second comment listed on slide 15 was poor. The team should provide data to address the challenging technical issues—redox material performance, circulation of high-temperature materials, high-temperature structural materials challenges, radiative heating challenges, and material durability challenges (added here because the active materials must go through significant stress in this process). The team should address these issues with data. The team also states that STCH has the potential to be more efficient than PEC and photovoltaic (PV) plus electrolysis. For low-temperature electrolysis, this may be true; however, high-temperature electrolysis has the potential to be more efficient than STCH. The comparison to low-temperature electrolysis is not fair since PV plus electrolysis is at a high Technology Readiness Level (TRL) compared to the low TRL of STCH. In addition, as PV technology improves, which it will, the PV plus low-temperature electrolysis may become more efficient than STCH. Finally, while STCH may have the potential to be more efficient than PEC, PEC is so much simpler that it may be preferred over STCH. The team should not disparage other technologies to try to justify its research.

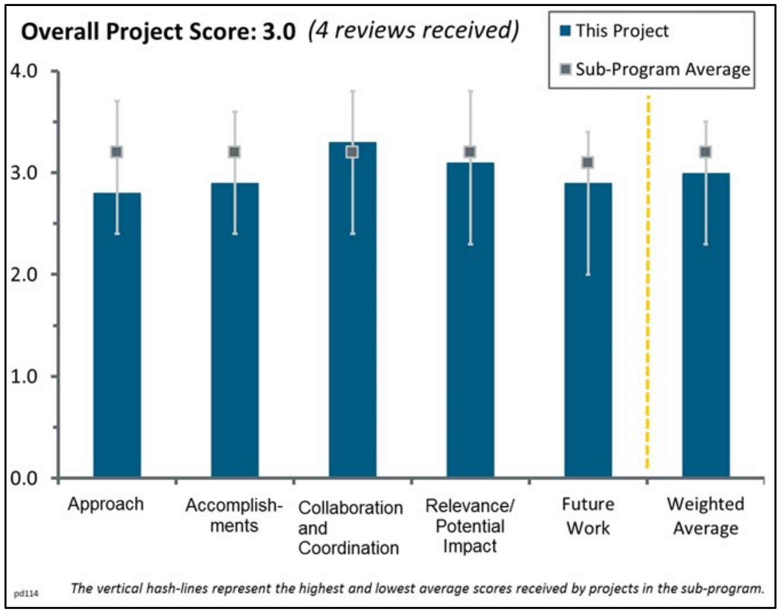
Recommendations for additions/deletions to project scope:

- The project should work to indicate how effectively a column of particles will support a pressure differential. The project should identify the critical particle properties (e.g., size, packing characteristics) and determine how the pressure drop depends on column height.
- The project should continue to update H2A of costs for hydrogen production and compare it to updated analyses of PV electrolysis and high-temperature solid oxide electrolyzer cell electrolysis. The project should also encourage the materials community involved in high-throughput computational materials screening to coordinate and collaborate on establishing materials discovery approaches, testing protocols, and reporting standards.
- The project should define target metrics in terms of materials performance—moles hydrogen/gram material, cycle time, cycle life, etc. This is missing and is very important to assessing progress and feasibility.

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

Al Weimer; University of Colorado

Brief Summary of Project:



The overall objective of this project is to design and test the individual components of a novel flowing particle solarthermal water-splitting system 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 **2.8** for its approach.

- The comprehensive approach is laudable. However, the effort is spread across an extremely wide range of objectives. Prioritizing the list of objectives is suggested. Then the project should concentrate resources on those near the top of the list that can be realistically addressed in the remaining project time. A specific recommendation is to de-emphasize improvement of the redox material since the hydrogen production target has been met and focus on other critical aspects—especially since the stated objective is to move this technology from Technology Readiness Level (TRL) 2 to TRL 3.
- All of the barriers presented as a part of the approach are clear and directly related to water-splitting. However, there are a great many barriers listed, and the project should take care that there is sufficient time to address all of the barriers. Further analysis and details may be required for details of particle flow and entrainment in the system. Hydrogen Analysis (H2A) results and key input parameters—not just sensitivity results—should be supplied if possible.
- The project approach is to test and validate the performance of components of the project's reactor design to move the technology concept from TRL 2 to TRL 3. This involves synthesis of reactive particles, design and demonstration of high-temperature containment materials, performance and attrition resistance testing of materials of reaction and construction, iterative updating of an Aspen process model and H2A, and collaborations with partners for identification and design of materials of reaction (University of Colorado) and "on-sun" testing (National Renewable Energy Laboratory [NREL]). Challenges and barriers were adequately addressed.
- The project approach was outlined and is logical; however, a project schedule for meeting project goals and U.S. Department of Energy (DOE) requirements was not given. A timeline for achieving all the go/no-go decision points and the DOE requirement for eight hours' continuous on-sun operation and production of greater than 3 L of hydrogen is needed in order to determine whether the project approach is appropriate. Preparing for on-sun testing and demonstration of hydrogen production requires working on many technical

fronts simultaneously, and it is not clear that all technical issues will be addressed sufficiently to meet the requirements in the project time remaining.

- The approach of experimental work coupled with theory closely resembles work done by Sandia National Laboratories. They should collaborate. The researchers need to define their anticipated system operating parameters such as cycle time for the materials. It is not clear whether this will this be 24/7 operation or only when there is sun. If it is 24/7, it is not clear whether they will use thermal energy storage. They plan on depositing SiC onto the reactor using atomic layer deposition. Given the proposed size of the reactors, they need to present compelling evidence (examples) that atomic layer deposition (ALD) can be economically done on a system of this size, shape, etc.

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.

- The project has an excellent approach to modeling and extensive laboratory work with clear forward progress in several areas. Given the wide range of barriers addressed, some priority or indication of the primary barriers was expected, as well as detailed progress showing the scale and effect of those improvements.
- The modeling of the multi-phase reactor is a good start to show the viability of the approach. It is not clear where this reactor is in the concept shown on slide 5. The materials require extremely high temperatures (1500°C) for long periods of time (60 minutes). This seems like it may be a problem for the system. The targeted cycle time is not clearly defined and needs to be defined. The researchers have screened many materials using theory. They have identified around 15 materials for further testing. The results of the testing will be interesting. It is unclear whether the coating of the very large reactors using ALD is economically feasible; it is not clear whether there are examples of this. In fiscal year (FY) 2015, the heat exchanger was a major emphasis; in FY 2016, this was essentially not mentioned. The reason for this was unclear. Solid–solid heat exchangers are very challenging, especially at the large flow rates required. The researchers indicated that particle size is important and the desired size is 1 mm. In their fluidized bed system, it is likely that attrition will occur. It is not clear whether they are testing their materials' durability under the very challenging conditions they will be operating. The principal investigator said that the cycle time was 40 minutes to oxidize and 40 minutes to reduce for a cycle of 1 hour 20 minutes (though slide 17 shows a cycle time of 1.5 hours). Using the average production (482 micromoles hydrogen/gram, slide 17) and assuming 50,000 kg hydrogen production per day, this means the system is moving approximately 2,900 tonnes of material per hour. This seems like a lot of solid material to move. If the cycle time can be decreased to 15 minutes, the amount of material decreases to ~540 tonnes. It is recommended that the researchers improve the kinetics to decrease the cycle time.
- The comparison evaluation of vacuum cascade and recycled inert gas sweep was interesting. It was not clear that the transport membrane for inert gas sweep would be capable of performing at the temperatures required or would be available for future scale-up testing. Lower reduction temperatures may be needed for use of inert gas sweep (if that is selected over the vacuum cascade method). Significant progress was made in modeling for coating stability of barrier material coatings on SiC, with candidate materials identified, and in synthesis of spherical particles of reactor materials. Materials discovery work continues to make progress and yield interesting results. The comparison between Sr- and Mn-doped $LaAlO_3$ (SLMA) perovskite, hercynite spinel, and ceria was interesting. A more detailed analysis of relative advantages of these classes of materials and the prioritization of peak production rate, cycle time, and production for system efficiency and production volume would be helpful.
- The H2A analysis in 2015 indicated that heat exchanger effectiveness was by far the most critical factor in the final economics. This technical challenge seems to not have received attention. Furthermore, its impact on the economics no longer appears in the H2A tornado diagram. This should be explained.

Question 3: Collaboration and coordination with other institutions

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

- The synergy and interactions between the Office of Energy Efficiency and Renewable Energy (EERE) project team (University of Colorado and NREL) and the National Science Foundation (NSF)/DOE project team are excellent. The participation of CoorsTek, Inc./Ceramatec, Inc., in the project provides valuable industrial input into development and vetting of reactor system components.
- There is a good balance of national laboratories, companies, and foreign collaborators.
- Several new collaborators appear to have been added from previous years, addressing concerns about having large-scale engineering companies involved in the project.
- They have added some collaborators (CoorsTek, Inc./Ceramatec, Inc.) that are good additions to the team.

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

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

- The project supports the EERE Fuel Cell Technologies Office (FCTO) goal of reducing the cost of hydrogen production from renewable resources to <$2/kg and the objectives of verifying the competitive potential for solar thermochemical cycles for hydrogen in the long term and of developing this technology to produce hydrogen by 2020. Meeting the cost goal will depend on the successful completion of this project and on technology development beyond the scope of the project and FTCO, e.g., lowering the cost of the heliostats to the DOE Solar Program SunShot target.
- The project directly supports the plans and targets of the Multi-Year Research, Development, and Demonstration Plan.
- This technology is in its infant stage, so it has potential to make an impact, but until it is further developed, the impact is unclear. The system is extremely complicated and still has many technical challenges to be solved before its impact can be assessed. Renewable hydrogen production from water splitting is relevant.
- It is not convincing that this technology has a reasonable potential to achieve $2/kg hydrogen. Multiple breakthroughs are required (e.g., materials, reactor design, and thermal management).

Question 5: Proposed future work

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

- All future work is in line with project barriers and goals. There are a great many barriers and issues to be addressed. Sufficient time and expertise will be required to complete all of these tasks.
- The project team has outlined future work in reactive and containment materials development, reactor design, and modeling and prediction in a logical manner, with milestones and go/no-go decision points identified. In the future, activities and plans for scale-up of fabrication technologies to apply materials of containment coating to full-size components should be addressed. Also of interest would be information on heat recuperation/recovery requirements for an inert gas sweep system (if that is what is selected) and heat exchanger requirements necessary for reactor success.
- It would to beneficial to see a Gantt chart describing the timeline that will achieve on-sun production in 19 months (before the end of the project).
- The researchers need to reduce the cycle time and the average production of their materials in order to decrease the solids handling required.

Project strengths:

- The project has an experienced team collaborating and leveraging resources from DOE and NSF. Excellent work has been performed on the development and production of materials of reaction and construction.
- Strengths are the extensive collaborative teams and the strong experimental and modeling work.

- The project's vision and collaboration are strengths.

Project weaknesses:

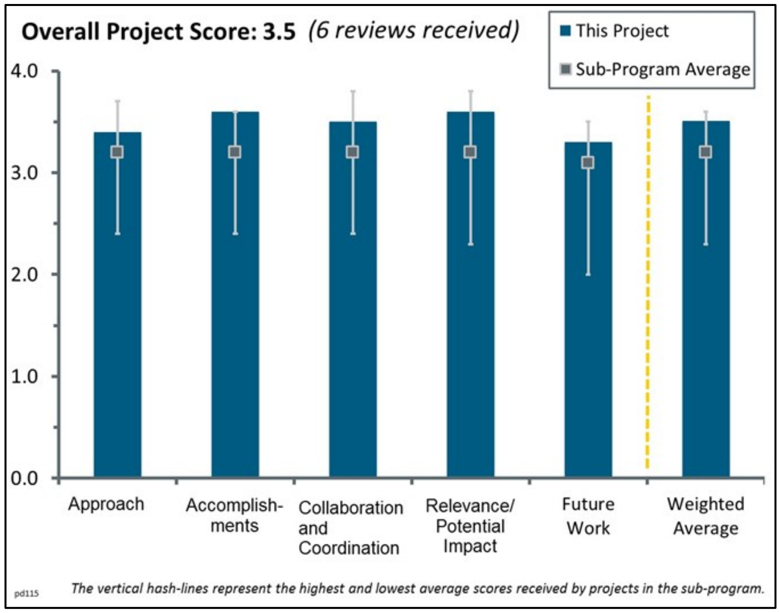
- The presenter would benefit from describing the connection of the disparate parts of this research in such a way as to give a complete understanding of the system and its operation. This information would be best presented early in the discussion. More H2A details indicating what the key baseline parameter values are should be provided. There are a significant number of barriers to address in this project. Further work to examine and report the details of the particle fluid dynamics is encouraged.
- Breadth and depth are project weaknesses.
- No schedule or strategy was discussed for meeting project requirements in the time remaining.

Recommendations for additions/deletions to project scope:

- The project should engage the Saudi Basic Industries Corporation (SABIC) more and get its perspective on the challenges associated with very large high-temperature moving bed reactors.
- The project should encourage the materials community involved in high-throughput computational materials screening to coordinate and collaborate on establishing materials discovery approaches, testing protocols, and reporting standards. The project should also identify an eventual downselect strategy for materials of reaction.
- The project should continue to update H2A analysis of hydrogen production costs. The project should identify an overall solar-to-hydrogen efficiency metric for a given reactor system/materials combination and compare the efficiency of the reactor system with photoelectric-powered water electrolysis and high-temperature solid oxide electrolyzer cell electrolysis.

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.

- Barriers are addressed with careful analysis of reported efficiencies (as published in *Energy & Environmental Science*) as well as effects due to optical absorption by water. The project is well designed and includes the use of inverted metamorphic multijunction (IMM) cells and graded junctions so that lattice matching is not required between the components in the tandem design (which is included in a non-provisional patent). The project is feasible, assuming that in time the expense of III-V compounds drops greatly. This work is highly integrated with other efforts as noted on the Energy Materials Network slide and extensive collaborations list.
- It is a successful tandem junction solar cell approach based on an IMM cell of $GalnP_2$/InGaAs. The investigators grew a very high-efficiency tandem junction that produced a high-voltage output that drove the electrolyzer. In summary, it is a successful engineering device.
- The approach is a reasonable response to DOE objectives. The project continues to exploit the National Renewable Energy Laboratory’s (NREL’s) leadership in the III-V semiconductor field. Techno-economic analysis aside, deterioration of the photoelectrodes has always been the Achilles heel of semiconductor photoelectrochemistry, so the project should pay more attention to it.
- The approach to the project and tasks within are clearly planned out in a manner to achieve Hydrogen and Fuel Cells Program milestones and targets. Partner involvement is clearly defined, and the pathways to addressing specific targets are well addressed.
- This approach is feasible only if solar concentration can be used. The principal investigator (PI) stated, “Solar concentration is the major lever.” The approach is based on the assumption of scaling solar intensity by 100x to meet cost goals to reduce material usage. To this point in time, the project has focused on other aspects than materials suitability at required solar concentration. This could become a significant roadblock to success if unanticipated barriers are encountered that require more than engineering solutions.

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.

- There was huge progress against DOE performance indicators with all milestones completed or on-track for completion. There was a series of recent world-record STH efficiencies using the buried-junction concept and IMM cells to alleviate lattice-matching problems.
- The project has shown significant improvements in STH efficiency per DOE targets. No guidance is given for the duration of the test or ability to sustain this efficiency.
- Modeling efforts have provided a realistic view into how the PEC water electrolysis cell of the future will be configured. Several insights and developments, such as electrolyte depth, back reflective contact, concentrated sunlight, circulation of electrolyte, and IMM, have all gone together to make >20% efficiency cells appear to be in the realm of possibility.
- The 16.3% efficiency is indeed impressive, and it shows the soundness of the approach. However, this world record should be considered along with durability. It is unclear whether stabilization of the p-$GaInP_2$ surface for 60 hours through MoS_2 is a successful milestone or a promising future approach. Further, the prospects of improving the efficiency of the device are unclear. It seems that the team needs a better junction to do that; it is unclear whether there is a plan for this.

Question 3: Collaboration and coordination with other institutions

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

- NREL has put together a good team with the University of Nevada, Las Vegas (UNLV); Lawrence Livermore National Laboratory (LLNL); the University of Hawaii; Stanford University; and others.
- The project partners and their involvement are shown as highly engaged. Tasks are segmented to appropriately leverage the strengths of each contributing institution.
- The large group of engaged collaborators establishes the credibility of these results.
- This work is highly collaborative.
- It seems that UNLV is an important contributor to the project, but its role should be better defined through a well-thought-out plan, i.e., how microstructural characterization can help the project achieve higher efficiencies. The way the collaboration was presented made it seem that materials are chosen first and then tested at UNLV. For instance, on slide 24 it is stated that new materials will be investigated with no reference as to how the new materials will be chosen in relation to promising microstructural features.

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 extremely relevant and provides a sound approach toward achieving the DOE targets.
- DOE has many goals and objectives, and it is hard for any one project to satisfy all of them. At some level, PEC hydrogen has to be compared with all other means of producing hydrogen using renewable energy.
- This work is potentially impactful and supports great progress toward the Multi-Year Research, Development, and Demonstration Plan targets given the series of new reported world records (16.3% STH, not on slides); addition of the window layer, which is new; etc. However, it is unclear whether this technology path will prove fruitful because of costs.
- The project meets or approaches near-term efficiency targets. Durability is still nonexistent. It is difficult to see how $/kg targets are achieved with no method demonstrated that shows an ability to incrementally show progress toward increased run time.

Question 5: Proposed future work

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

- The future work is planned in a logical manner with decision points including the use of ambient pressure x-ray photo-electron spectroscopy (XPS) built by the Heske Group at UNLV and GW approximation theory measurements with the Ogitsu Group at LLNL. However, there are some significant barriers to the goals, including (1) the cost of the wafer in the context of the new techno-economic analysis, (2) champion electrodes have been hard to reproduce, (3) the need for a thin water layer, and (4) the mention that 10x concentration resulted in >100 ºC operation for 12 hours per year from JCAP-North work, and so one cannot likely go much higher in optical concentration. Notwithstanding, the durability is being addressed and alternative pathways to mitigate risk are present, including down-selected materials, metamorphic geometry, protection layers with the Jaramillo Group at Stanford, and investigation of work using increased pressure to mitigate bubble formation.
- Future efforts are detailed well with focus on continued improvement toward DOE targets. Barriers are acknowledged, but the specifics of what are required to meet the $/kg targets are still significantly far away on all aspects.
- It is good that durability will finally be getting more emphasis. The various modeling exercises where the lifetime of an array is put at 5–20 years are somewhat disconnected from where the technology presently is (which seems to be hundreds of hours).
- Materials and device lifetime studies of solar concentration were raised as a need in the previous year's review. The reliance on one class of materials is a potential weakness.
- The proposed work is directed to efficiencies greater than 15% and durability over 875 hours. Although this year's accomplishments ensure better results, it is not clear that the suggested three encapsulation pathways (see bottom of slide 24) will yield better durabilities. In other words, the mechanism-based approach to understanding degradation is missing.

Project strengths:

- NREL and collaborators demonstrate unrivaled competency in developing $GaInP_2$-based multijunction photoelectrodes and likely PEC water-splitting technology in general.
- The group uses capillary mass spectrometry to evaluate the 1:2 ratio of hydrogen to oxygen and aims to grow materials on silicon substrate to mitigate some of cost because the current substrate is prohibitively expensive.
- Progress on multiple fronts has been made. The new world record for solar PEC efficiency is 16.3%.
- The project has come up with a world-record STH efficiency, which is excellent.
- The collaborative nature of the project and contributions from UNLV and LLNL are strengths.
- The team and focus on fundamental research are strengths. Detailed plans for current and future work are strengths.

Project weaknesses:

- It would be essential to determine what is the actual device (or interfacial) lifetime of materials at the full (100x) solar concentration intensity stated to be essential for meeting deliverable STH. It is unclear whether there is a different intensity inactivation curve for each material. It is unclear whether it is interface specific. It is unclear whether the lifetime is predictable from fundamental physical and chemical properties of materials. The decision to eliminate the (lower cost) nitrides materials subtask because of inadequate performance further reinforces the dangerous reliance on one class of ternary III-IV materials, which has not been tested at 10x or 100x solar intensities.
- In the broad scheme of things, it continues to look like a very expensive way to produce renewable hydrogen. Setting efficiency records is good in that it captures headlines, a non-trivial consideration when funds are being distributed among disparate agency branches and technologies. Nonetheless, such accomplishments will invariably be followed by demands for scaled-up demonstration projects. It is unclear how much longer NREL will be allowed to work with 0.25 cm^2 electrodes.

- The high cost of material prohibits use of significant material for stability tests under 10x and larger optical concentration. Also, more needs to be known about stability, which is part of the scope of work.
- The project still has not addressed the photocorrosion aspect, which is critical to the progress.
- The lack of a mechanism-based approach to understanding photocorrosion is a weakness. It is unclear why the electrolyte is in contact with the tandem junction.
- A weakness is the project's inability to show any durability for this technology.

Recommendations for additions/deletions to project scope:

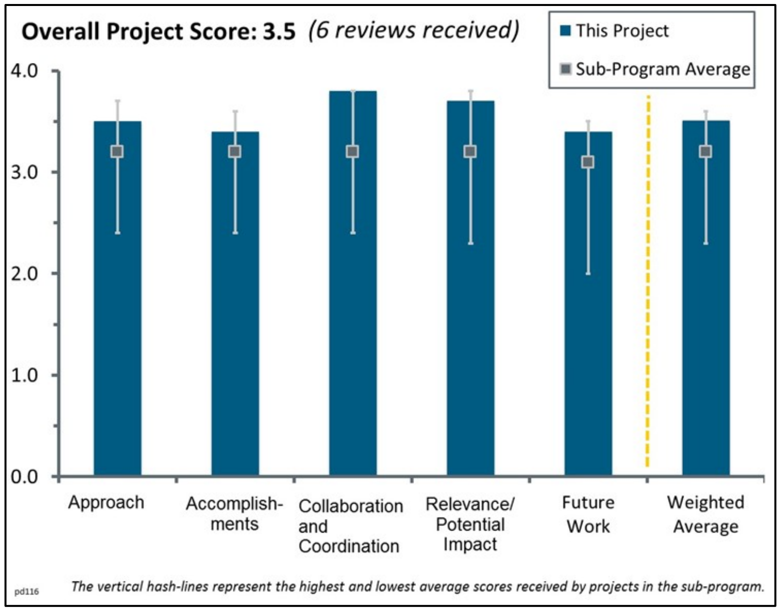
- The entire PEC hydrogen research area needs to carve out a niche for itself that defines a possible set of parameters in the future world energy economy that would make photoelectrolysis the preferable approach to renewable hydrogen. In other words, PEC hydrogen has to elbow its way between hydroelectric- and wind-powered electrolysis, maybe even certain types of biomass conversion, and ultimately solid-state photovoltaic arrays coupled to dark electrolysis. All of these technologies can be done presently on a massive scale if a demand for renewable hydrogen were to arise.
- The project should use a voltmeter and an ammeter to assess the actual short-circuit condition.
- The project will benefit if the PIs put more focus on the lifetime of the device at this time.
- It is stated that the approach used minimizes interface defects, but no proof was given. However, it is stated on slide 15 that this task will be undertaken with UNLV in the near future.

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 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 efficiency, and (3) generate 3 liters of hydrogen under 10 times concentration in 8 hours.



Question 1: Approach to performing the work

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

- The project addresses barriers, including the need to widen bandgap to make a tandem using a sulfur/selenium mixed material with single phase by X-ray diffraction (XRD). The University of Nevada, Las Vegas (UNLV) identified an oxygen vacancy and they are exploring if this is a major issue. The project is well-designed using photovoltaic (PV)-grade materials that are bandgap tunable with cost-effective processing. The project is feasible and the techno-economics are promising. The group accomplished the go/no-go decision for year 1 already, via four pathways (three different materials; one of them measured using outdoor testing in Hawaii and got 0.6 V (V*oc*) and decent fill factor with rather long-term stability). This work is integrated with other efforts as it is largely collaborative, including with the National Renewable Energy Laboratory (NREL) and the University of California, Irvine (two other presenters of photoelectrochemical [PEC] work at the Annual Merit Review [AMR]).
- The project goal is to identify low-cost materials for tunable bandgap absorbers and investigate their suitability for preparation of photoanode and photocathode interfaces using catalysts and/or passivation of the electrolyte interface. Copper chalcopyrites and copper indium gallium (di)selenide (CIGS) alloys will be evaluated for optical absorption properties of relevance to use in PEC tandem cells.
- CIGS has been around for some time but the investigators have found some productive avenues to pursue. Widening the bandgap with the idea of eventually producing a tandem device is worthwhile. They are using the same partners as NREL to modify the electrode surface to slow deterioration.
- Efforts appear to be well-coordinated and supported by a solid team that has done a good job of addressing barriers. Progress on a durability barrier is still way behind and it is difficult to see that any success will be accomplished here since 2015 milestones are still only 25% complete.
- The approach aims at PEC water splitting at low cost and solar-to-hydrogen (STH) efficiencies greater than 10% by using wide-bandgap copper chalcopyrite based materials. The approach is focused more on the photovoltaics aspects such as optical absorption than surface chemistry and catalysis, which are given less attention. It was also stated that the Materials Genome Initiative will guide the choice of new wide band gap materials and then be tested at UNLV, but it was not shown how the process of down-selection will take place.

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 progress against performance indicators is excellent including meeting the go/no-go decision in several ways. It was unclear which demonstrations used buried junctions, PV-bias, and electrocatalysts (and what they were); this should be made clearer next year. This work is also demonstrating progress toward DOE goals by notably working on the protection issue.
- The principal investigator (PI) has synthesized a range of copper chalcopyrites and CIGS alloys and evaluated optical absorption properties of relevance for use in PEC tandem cells. They have identified compositional requirements for achieving wider spectral range and met appropriate goals. The team has moved on to examine how to stabilize the interfaces and how to avoid interfacial problems. Data appears to show identical sub-band-gap absorbance and scattering for a wide range of different materials. Actual absorbance spectra appear to have been normalized (no data for absorbance [Abs] were given). Impurities were identified in the synthesis and work towards removal has progressed. Heterogeneity at the compositional level may contribute to spectral band gap structure and to sub-band gap absorbance scattering and will be investigated.
- The evolution of bandgaps presented on slide 8 over the duration of the project shows cohesion and concerted effort. The photocurrent density of 10 mA/cm^2 seems to be a promising result. Oxygen and sulfur identification of impurities by X-ray photoelectron spectroscopy (XPS) (see slide 10) is an important accomplishment but it was not mentioned how this information will be used to achieve better efficiencies or even guide choice of new materials. Studies on corrosion resistance are significant (see slides 14 and 15) but again they should focus on mechanistic understanding rather than on observational conclusions.
- Manufacturing and efficiency targets are being met. It is difficult to see a pathway to achieving durability targets within this program since the current trajectory looks like another miss in 2016. All other goals look to be in a good position to be met for the balance of the program.
- Gallium, aluminum, and sulfur have been identified as potential dopants to adjust (widen) bandgap. Most of the work was done on sulfur, which has been used in that regard on copper indium selenide PV cells for some time. Nonetheless, HNEI continues to accrue improvements in open circuit voltage and conversion efficiency. Use of MoS2 and Pt-TiO2 as surface modifiers provided significant improvements in durability although there's a long way to go to reach DOE cost targets.

Question 3: Collaboration and coordination with other institutions

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

- Team interaction appears to be well-managed. Contributors are being leveraged well to play to their strengths. This has been key in meeting the majority of targets thoroughly and in a timely manner.
- This project has a huge list of active and influential collaborators.
- The project demonstrates effective collaboration among partners in identifying contaminants and preventing electrode corrosion.
- The project enjoys a wide range of effective collaborations.
- It seems that UNLV is an important contributor to the project, but its role should be better defined through a well thought out plan, i.e. how microstructural characterization can help the project achieve higher efficiencies. The way the collaboration was presented, it seems that materials are chosen first and tested at UNLV next. Perhaps the collaboration between UNLV and Stanford can advance the identification of the source of impurities at the interface.

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

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

- The work is in alignment with the goal of producing cost-reduced hydrogen efficiently. The goal of producing a cheaper chalcopyrite material achieved and provides a path to meeting long-term goals.
- The work is excellent and complementary to the NREL work.
- Project relevance fits well within the scope of the entire PEC program; however, the reviewer is concerned about the fit of the PEC program with DOE hydrogen.
- It is an engineering-driven project that would have more impact if there was more focus on catalysis and surface chemistry.

Question 5: Proposed future work

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

- The future work is planned in a logical manner with decision points but, as in any PEC approach, there are many barriers to the goals, which are addressed directly (i.e., bandgap, purity, stability, and benchmarking). The group has also explored several alternative pathways to mitigate risk, e.g. the Ogitsu Group at LLNL did modeling to identify a window layer that has a good energetic match and identified ZnOS; corrosion protection with Jaramillo Group at Stanford because they had done physical vapor deposition, (PVD) but this material is rough and so now they are doing MoS2 by atomic layer deposition (ALD) and demonstrated a doubling of the lifetime (they also looked at Pt/TiO2 [50 nm] and got 250 hours stability)
- Intriguing and innovative work with dopants, surface modifiers, and tandem cell fabrication was outlined.
- Proposed future work is set up to address project barriers.
- Although a solid approach toward future developments is described, the specifics of the required fundamental science are missing. For instance, on slide 19 it is stated that the optimum protective material for durability will be identified but no specific pathway is outlined.

Project strengths:

- The project represents a good set of back-up photoelectrode materials should the III-V semiconductor tandem cell work become stymied for some reason. The chalcopyrites might actually have a better chance of reaching long-term cost goals for renewable hydrogen.
- The project demonstrates excellent recent progress.
- The project presents well thought-out, systematic study on the variation in the elemental composition of the film.
- The collaborative nature of the project and contributions from UNLV and Lawrence Livermore National Laboratory are strengths.
- The project enjoys strong team collaboration.
- The project has an excellent team integrated well within the user community.

Project weaknesses:

- This is significantly farther down the power curve (literally) than the GaInP2 photoelectrodes. The lower efficiency may cancel out the advantage of lower material cost.
- The work has too much emphasis on efficiency. This does not mean a lot if you can't get the device to run for any reasonable period of time.
- The project does not address whether the proposed MoS2 or TiO2 films could provide long-term protection even when pin holes are removed.
- There is a lack of a mechanism-based approach to understanding bottlenecks.

Recommendations for additions/deletions to project scope:

- Oxygen was found in the bulk of the selenium/sulfur semiconductor. Its origin should be identified and its concentration controlled. Its presence in small amounts might be beneficial.
- The project will benefit from more coordination on fundamental science between UNLV, Stanford, and LLNL. The below answer by the project PI to reviewers' comments on page 17 indicates that the focus of the project needs to be expanded:

  > *Our project aims to develop wide bandgap chalcopyrite photocathodes. A mechanical stack approach will be used to pair these electrodes with existing high efficiency PV drivers to form a complete high-pressure electrolysis device (proof of concept). However, our technoeconomic analysis indicates that this approach is not economical for large scale PEC hydrogen production. To be economically viable, a commercial device should be made of two absorbers monolithically integrated on the same substrate, with hydrogen and oxygen evolved on opposite sides of the device. For this reason, our team has chosen to study some key components of the monolithic structure (e.g. In2O3:Mo as intermediate transparent window layers) to identify possible pitfalls.*

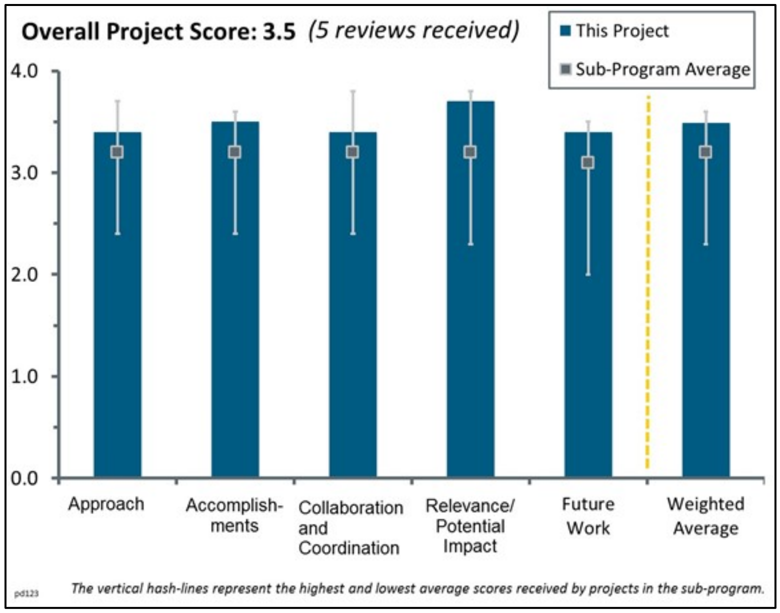
  Indeed, this answer shows that the project is mainly focused on PV and its direct link to hydrogen production is not clear.

Project #PD-123: High-Performance Platinum-Group-Metal-Free Membrane Electrode Assemblies through Control of Interfacial Processes

Katherine Ayers; Proton OnSite

Brief Summary of Project:

Anion-exchange-membrane (AEM)-based electrolysis enables the elimination of most expensive cell materials. This project will further AEM electrolysis through three objectives: (1) demonstrate feasibility of non-platinum-group-metal (non-PGM) catalysts in AEMs, (2) enhance membrane and ionomer stability to achieve long-term cell operation, and (3) demonstrate 500 hours of stable operation at <2 V for a fully integrated AEM cell at 500 mA/cm^2.



Question 1: Approach to performing the work

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

- Barriers are addressed, including increased polyphenylene oxide (PPO) membrane stability through the use of hydrocarbon cation spacers to decrease benzyl attack. This cannot withstand hot press conditions like cation exchange membranes, so spray deposition of catalysts on the membrane was used to circumvent this. This is a well-designed project, especially given that electrolysis is the most mature technology to bridge hydrogen to market. The project is feasible using Ni-based oxygen evolution reaction (OER) and hydrogen evolution reaction (HER) electrocatalysts, AEMs, and stainless steel flow fields, which are cheaper when at scale than polymer electrolyte membrane (PEM) Ti. This work is integrated with other efforts at Proton OnSite and collaborators at Northeastern University, Pennsylvania State University (Penn State), and the University of New Mexico.
- The project addresses barriers of electrolyzer cost, durability, and performance. Cost is addressed by investigating PGM-free catalysts for HER and OER in alkaline-exchange membrane electrolyzers. Durability is addressed by enhancing alkaline membrane durability. Though Los Alamos National Laboratory has shown aryl ether backbones can be susceptible to ether cleavage, the dimethyl substituted aryl ethers pursued here should be kinetically stabilized because of steric hindrance provided by the methyl groups. The choice of longer side chains for the ammonium group provides greater hydroxide stability. The sacrificial support method should enable a porous electrode with efficient mass transport. The approach to address catalyst, membrane, and ionomer should lead to an optimized MEA.
- This project has a structured approach that should lead to good results. The team is also working on novel tunable membranes for use in non-PGM electrolysis systems. Technical targets seem to focus solely on catalyst activity without paying much attention to durability. To make a viable system, both are needed. That the addition of carbonate to the system has such a profound and unexplained effect is a bit of a concern. This issue warrants more attention.
- The project is following a logical approach. Alkaline electrolyzers are lower-capital-cost; however, they are lower-efficiency. This means that this technology may be applicable for smaller systems in which capital costs dominate, not the larger stations in which electricity costs dominate. It would be good if the project identified its target market for the project’s applications. The project should include analysis conducted using the Hydrogen Analysis (H2A) model to be able to track the impact of the project work. The project needs to include work on more traditional baseline catalysts to show improvement.

- It is not clear how choices were made for particular catalyst and polymer membrane compositions. Addition of potassium carbonate has a huge impact on membrane electrode assembly (MEA) testing, but there is no apparent plan to probe why.

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.

- While IrO_x is not a great OER catalyst in base, the new Ni-based OER electrocatalysts had similar surface areas, electrical conductivities, and OER onsets to IrO_x (a decent standard). The project also demonstrated progress toward DOE goals including membrane stabilization (and 400 mV better performance) with use of carbonate and demonstrated five-year stability at 2 V, including addition of cross-linking in the AEM to enhance performance and properties.
- Progress has been excellent so far in the year since this project launched. The team has already down-selected OER and HER catalysts based on technical milestones and is progressing on membrane stability.
- The project has identified PGM-free catalysts with OER activity of 20 mA/cm^2 at <1.55 V with 0.1 M KOH. HER catalysts with less than 200 mV overpotential at 20 mA/cm^2 have also been identified. The project has shown a full PGM-free electrolyzer cell with stable operation at 500 mA/cm^2 for five hours. On slide 10, the project states it will use ammonium groups with longer spacers to improve stability, but the cross-linked structures utilized have the ammonium group directly attached to the backbone, which should result in ammonium groups, and the data shown on slide 26 show problems with stability. Stability is still an issue.
- The initial performance tests are very interesting. The durability tests will be useful to determine whether the catalyst and membrane performance is good enough. The PPO-MEA + catalyst tests do not show good stability, even at the low current density. However, it is very difficult to determine from the data whether the degradation is due to the catalyst or the membrane. It is hard to determine the accomplishment significance since there is no associated H2A analysis to show progress on reducing the cost.
- Heavy use of acronyms and industry parameters made it difficult for a non-expert to critically assess.

Question 3: Collaboration and coordination with other institutions

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

- The team appears tightly integrated and focused on results. Penn State is working on the ionomer, with the University of New Mexico working on high-surface-area fabrication of catalysts developed at Northeastern University.
- The project is highly collaborative.
- The team is strong.
- There is a nice set of academic collaborators, each with clear responsibilities, but it is not clear how integrated the academic teams are with each other or with Proton OnSite.
- Collaboration among partners is good. All partners have an active role. Collaborations with those outside the project are not apparent.

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

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

- In order to see hydrogen production at large scale, it will be necessary to reduce the capital cost of electrolyzers significantly. Developing non-PGM systems is critical to that goal. This project, though focused initially on laboratory-scale systems, could have the potential for scale-up to industrial sizes.
- Alkaline electrolyzers have a lower capital cost than PEM electrolyzers, so companies that are very concerned with initial capital costs may prefer this approach. In addition, for lower production rates, capital cost may surpass electricity costs for the largest part of the hydrogen cost.

- Proton OnSite is the leader in PEM electrolyzers for hydrogen and is now taking the same prominent position in the AEM electrolyzer space. Progress is excellent.
- It is appropriate for DOE to support an AEM alternative to PEM through both fundamental and applied research. The project has a good set of quantified goals.
- Alkaline electrolyzers have the potential to significantly reduce the capital expenditures for electrolyzers. The project addresses major cost components for alkaline membrane electrolyzers.

Question 5: Proposed future work

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

- The planned future work is logical with decision points (some materials were already down-selected), including scale-up of cross-linked PPO membranes, mechanical reinforcement to improve durability of membrane and water transport rate and PPO with >25 mS/cm, tests on effects of carbonate (K_2CO_3) using half-cell and full-cell electrochemical impedance spectroscopy (EIS)measurements, and catalyst performance and interfacial stability.
- The project has a clear timeline for addressing milestones.
- It is recommended that two components be added to the future work. (1) The project should add a lifecycle cost assessment evaluating the trade-off in life-cycle cost between capital outlay and system efficiency. The motivation for this analysis is that a non-PGM system is likely to be less efficient than a PGM system. On a life-cycle basis, the cost of electricity is the most important factor in the cost of hydrogen. This consumption is directly affected by efficiency. The question is how much efficiency loss capital cost reduction can buy. At several reviews, the principal investigator has said that her customers care about capital outlay, but it is not clear to what extent. (2) The effect of carbonate on the system is profound. It is important to understand the mechanism by which carbonate improves the system stability and performance. It is unknown whether the addition of carbonate is viable in the long term. Is it not certain that carbonate can be engineered out.
- The plan to perform more durability testing is good. The team needs to include cycle testing as well as lifetime. It is recommended that the project compare its data with some more traditional baseline alkaline electrolyzer catalyst data (such as a Ni catalyst).
- The proposed work addresses mass transport in the electrolyte layer, and optimizing the hydrophobic properties is needed. Transport issues are likely to be important.

Project strengths:

- The project is actively focused on capital cost reduction from the ground up. The team is taking an almost blank slate approach to what it means to build an electrolyzer.
- Alkaline electrolyzers have clear entry points into the market with increased materials development and could conceivably be plug-in replacements into PEM cells/stacks.
- The project has a good team and is doing a thoughtful, methodical approach.
- Proton OnSite is a leader in the electrolysis field.

Project weaknesses:

- The approach may not be helpful for DOE to achieve its long-term, high-volume production (>1000 kg hydrogen/day) cost targets.
- There are questions about carbonate.

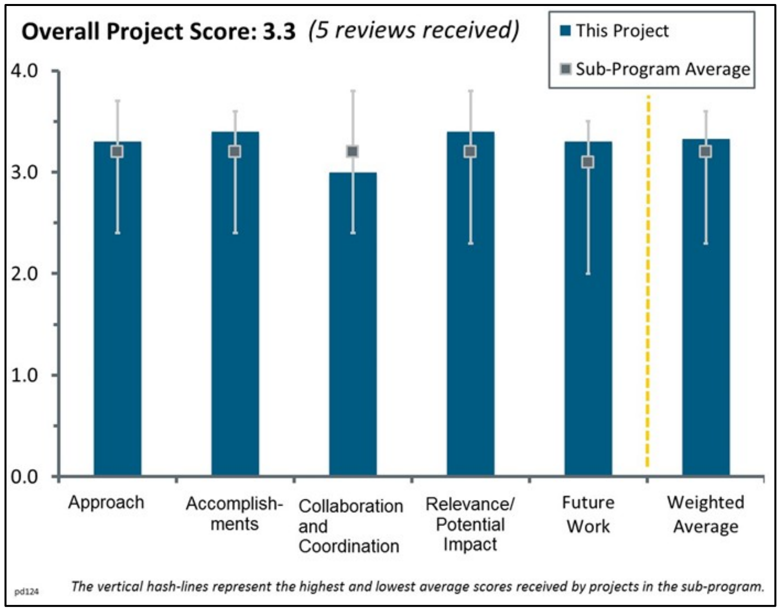
Recommendations for additions/deletions to project scope:

- It would be interesting to see H2A model analysis at lower hydrogen production levels, such as 100 kg/day. There are some very old H2A model analyses at this production capacity that can be considered. The project should consider testing against baselines other than Ir. The required cost for the carbonate addition is unknown.

Project #PD-124: Solid-Oxide-Based Electrolysis and Stack Technology with Ultra-High Electrolysis Current Density (>3A/cm^2) and Efficiency

Randy Petri; Versa Power Systems

Brief Summary of Project:



This project is researching and developing solid oxide electrolyzer cell (SOEC) technology capable of operating at ultra-high current density with reasonable efficiency. Project objectives are to (1) develop a solid oxide electrolysis cell platform capable of operating with current density up to 4 A/cm^2 at an upper voltage limit of 1.6 V, then demonstrate cell operation with high current density of more than 3 A/cm^2 for 1,000 hours; (2) design a solid oxide electrolysis stack platform capable of operating with 3 A/cm^2 current density cell technology at an upper voltage limit of 1.6 V, then demonstrate stack operation with high current density of more than 2 A/cm^2 for 1,000 hours; and (3) complete a solid oxide electrolyzer process and system design that accommodates the ultra-high operating current density platform.

Question 1: Approach to performing the work

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

- Details on specific approaches were not provided, but the results suggest that the technical approach was solid. There is a more global question, to be addressed in future work, of whether operating the SOEC at high current density is actually preferable. Ultimately, stack size and cost (which are lower at high current density) need to be traded against power requirements (which increase with higher current density) and stack lifetime (which is likely reduced at higher current density). The analysis to be performed by the University of California, Irvine (UC-Irvine) should shed some light on these trade-offs.
- It is not clear why someone would want to operate at >1.6 V. The advantage of SOEC over low-temperature electrolysis is increased efficiency. At the high current and voltage, the efficiency is low. It would make more sense to operate closer to thermoneutral and higher efficiency. The project really needs to do an analysis using the Hydrogen Analysis (H2A) model to show that, should the project be successful, it will reduce the cost.
- The innovation being pursued by this project is unclear, beyond testing existing materials at higher voltage and the design of the appropriate SOEC system.

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.

- Versa Power Systems made considerable progress toward its stated goals. The demonstration of SOEC stack operation at such high current density—with minimal degradation—is an important accomplishment.
- The project appears to be ahead of schedule.
- Getting to >3A/cm^2 in the single-cell testing was impressive. The degradation rate is too high. The project really needs to do longer durability and cycling tests at the high rates. Operating an SOEC at or above

1.6 V causes severe irreversible degradation due to microstructural changes both in electrodes and at the active interfaces. The researchers are seeing that in their system. In addition, they are generating a good deal of excess heat. Their stack model shows a large temperature gradient that will impact stack durability, especially for cycling. The advantage of operating at relatively high voltage is not clear.

Question 3: Collaboration and coordination with other institutions

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

- It seems the collaborations were primarily in the past on other projects. The project does not seem to be collaborating with others. It is recommended that the project bring on a partner to aid in the analysis. The Pacific Northwest National Laboratory (PNNL) has developed a lot of solid oxide materials, seals, and coatings and has plenty of experience doing failure analysis. The project should consider reaching out to PNNL. The National Renewable Energy Laboratory (NREL) has done techno-economic analysis for Versa Power Systems in the past and could help the project team understand the value, or lack thereof, of operating at high voltage in terms of hydrogen cost.
- It was not apparent that there has been any collaboration to date on this project (aside from Versa Power Systems' parallel work with Boeing and the Solid State Energy Conversion Alliance), although this could be related to the proposed project plan. It seems likely that this will change in the future since UC-Irvine is tasked to complete a techno-economic analysis. NREL's role on the project was not clear from the presentation—perhaps NREL is supporting the techno-economic analysis.
- It is not clear how NREL is involved or whether other partners are still engaged.

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.

- Solid oxide electrolysis represents an important approach for hydrogen production in water-abundant locations, especially if integrated with "free" sources of power (e.g., wind and solar) or "free" sources of heat and steam (e.g., nuclear power plants).
- Solid oxide electrolyzers have the potential to lower hydrogen cost since they use electricity more efficiently than low-temperature electrolysis.
- It is appropriate for DOE to support SOEC development. There is a good set of quantified technology goals in support of higher-level cost and efficiency goals.

Question 5: Proposed future work

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

- The future work outlined in the presentation is fine. It is recommended that UC-Irvine's techno-economic work be initiated and completed as soon as possible so a decision on the target current density for the SOEC stack can be made. Based on these techno-economic results, the technical targets for the work might be revised. That said, any work that enables higher current density to be achieved with lower degradation will still be germane for SOEC stacks operating at lower current density. Thus, the work proposed toward improving SOEC stack performance need not be delayed.
- The clear project plan and timeline are appreciated. The project should consider engaging academic or national laboratory expertise to develop an approach to stabilizing Ni loss from the cathode.
- The project is doing the techno-economic analysis at the end of the project. The researchers should be doing the analysis now and using it to direct their work. They need to do durability and cycle testing.

Project strengths:

- Versa Power Systems is a world leader in solid oxide fuel cell (SOFC)/SOEC technology. The company has a stack platform with high technical maturity and has demonstrated world-leading SOEC performance. The results obtained to date on this project are outstanding from a technical perspective.
- This is very interesting work, and the project has made good progress.
- The project has made excellent progress.

Project weaknesses:

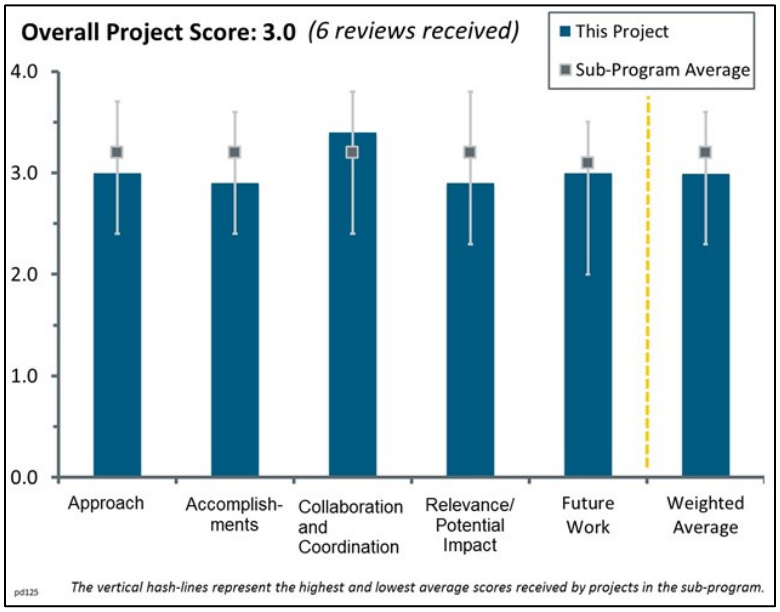
- A techno-economic assessment is needed to determine the optimum current density for SOEC stack operation.

Recommendations for additions/deletions to project scope:

- It is recommended that the project perform durability testing and cycling testing, as well as examine the interconnects for corrosion.

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 **3.0** for its approach.

- This type of photoreactor is likely viewed by many as the contrarian approach to solar water splitting, so starting with an increasingly sophisticated modeling effort to show that >1% efficiency is even possible is a worthwhile approach. A good deal of theoretical modeling was done in the early days of photoelectrochemistry, so it is appropriate to do some upfront modeling of particle slurries as well. Actually, these types of systems received much attention as water purification/pollutant removal devices in which the redox transfer agent was instead the targeted pollutant. There may be some chemical/civil/ environmental engineering studies that would be of benefit, at least in terms of what reaction rates have already been achieved. Trying to apply a tandem configuration to particle slurries will not be easy but is novel nevertheless.
- The Type 2 system (suspended particles) has been considered in prior techno-economic analysis evaluations for hydrogen production. This project aims to model the standard configuration and some variations on this configuration. The project's value will be in providing a further assessment of this system's limitations in terms of geometry, plumbing, diffusion, etc. The project could provide further boundaries for defining its possible contribution to a real Type II system if the costs can be achieved. The U.S. Department of Energy (DOE) Office of Energy Efficiency and Renewable Energy (EERE) is fully responsible for selecting projects based on real or anticipated contributions to achievable designs—those with the greatest potential for translation to practical applications.
- The general modeling and experimental approach to evaluating the proposed "new reactor design" of overlaying the bags is fine and should accomplish project objectives. Given that particle (i.e., photocatalyst) development is not a focus of this project, it is unclear how much of the boost in efficiency and projected cost reduction can be attributed to the "new design" and how much would be attributed to having better-performing photocatalysts. (The considerable number of materials contributors suggests that the project is targeting the best available photocatalyst technology, but even that may not be adequate to meet performance targets.) It would be beneficial for judging the potential for this technology to significantly reduce cost if the benefits could be quantified on a "photocatalyst"-independent basis.
- The iterative research and development approach, between computational and experimental, presented by the project lead seems a very effective way to achieve the main goals and objectives of this project.

- This is a "bag approach" aiming at >1% solar-to-hydrogen (STH) efficiencies at reduced cost. However, there are a number of issues that need to be further explored before the approach can be deemed sound and promising. Among these issues are the degree of particle suspension and its effect on quantum yield, as well as the stability of the particles.

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.

- The project lead presented significant progress on this work, including the electrolyte stability demonstration through the project's modeling efforts, the electrocatalyst deposition through bipolar electrochemistry, and the synthesis and characterization of the photoelectrodes.
- Progress appears to be adequate to date relative to milestones based on the percentage completed reported, especially for a new startup project. However, insufficient information was presented to evaluate whether projected completion percentages were truly representative of whether the milestones were on schedule. For example, the D1.1.1.1 milestone to reduce pipes and pumping energy demand by 80%, which is due in less than two months, is reported to be 50% completed; however, there was no information presented to support this level of projected completion.
- The effort just started late last year, and the calculations successively incorporate more and more dynamic aspects of the ultimate particle slurry configuration. Therefore, at this point, the best that can be said is that the project is making progress, but many more aspects need to be built into the model to make realistic conclusions. There is some concern that the model will become so complicated that simplifying assumptions may have to be made, which will then cast a shadow on the final results. It looks like the project already has candidate oxygen- and hydrogen-evolving particles identified.
- Early-stage results (nine months) are approaching targets.
- The accomplishments so far are only numerical related to transport issues among the various "bags." It was mentioned that Rh-modified $SrTiO_3$ and $BiVO_4$ were chosen, but this choice seems rather ad hoc with no clearly underlying relation to the milestones and targets of the project.

Question 3: Collaboration and coordination with other institutions

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

- The collaboration on this work is outstanding. The primary project team is collaborating with a significant number of experts in particle material synthesis.
- The project team has found it necessary to go outside the DOE PEC program, even internationally, to get help on some aspects of the work. That shows some initiative on the team's part.
- Excellent leveraging of expertise is available through the Joint Center for Artificial Photosynthesis; however, it is not clear what impact these collaborations have had to date on advancing the project. There is a wide and diverse range of materials contributors. It is not clear what, if any, impact these contributors will have on the project. The presentation gave the impression that materials development for photocatalysts is not a focus of this project.
- The names of the collaborators and partnering institutions are impressive. However, the presentation did not make clear what the collaborators' contributions are. For instance, the role of Professor K. Domen in this project could not be identified.
- There are many prospective collaborators, but few real partners were listed. Engagement of others that could offer experience with this Type 2 system would further benefit task completion.

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.

- Solar water splitting could be a potential enabler within the Fuel Cell Technologies Office's main goals to achieve low-cost renewable hydrogen production.
- It is a worthwhile project, providing some balance within the PEC program. The issue is how the entire PEC program stacks up against all the other approaches to renewable hydrogen.
- This comment is directed at EERE, not the principal investigator. DOE needs to establish its priorities in terms of Type 2, Type 3, Type 4, or other configurations. The funding of research on Type 2 PECs that is likely not to lead to a practical implementation is not advised. EERE is fully responsible for selecting projects based on real or anticipated contributions to achievable designs—those with the greatest potential for translation to practical applications.
- It is not clear what impact the new design concept being pursued will have on enabling tandem particle slurry batch reactors for meeting DOE hydrogen production cost targets, given that the project's success lies in identifying materials with relative high solar efficiencies, which is outside the scope of this project.
- The project is still at an initial stage. Further developments, in principle experimental, are needed before the relevance of the project can be ascertained.

Question 5: Proposed future work

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

- The proposed experimental work for fiscal year 2017 will be essential for the validation of the modeling work. Future work is very well defined.
- The project is well planned.
- Proposed work is based on meeting project milestones. Task D.1.1.1 is a go/no-go decision point that should demonstrate certain, but not all, proof of concepts related to the new design. It is not clear what the benefit of task M.1.1.3 is—this task leads to an "improved model" by considering more process parameters—especially since it is not clear how the improved model will benefit the experimental development. It is not clear how the project progresses, as a material that operates at a rate consistent with an efficiency of >1% STH cannot be identified.
- Since the project just started up, most of the work statement is future work.
- The approach continues to rely on an incremental addition of device physics to the numerical transport model. This is not a sound approach. The project needs to focus first on how a transparent membrane is going to remain transparent over a long time (not just a few days) or how the particles are going to remain stable over that timeframe, etc.

Project strengths:

- The project provides some balance to the PEC program, where low-cost and likely lower-efficiency STH materials and configurations are employed to eventually produce large-scale photoreactors. The tandem adaptation is a novel idea that may enable a significant jump in efficiency.
- Proposed "stacked bag" reactor design does appear to have some benefits over "side-by-side bag" design in terms of better utilization of solar flux. The benefits in this design with regard to fewer pipes are not as clear.
- The presented theoretical calculations shed a clear light on the project's feasibility.

Project weaknesses:

- The particle slurry reactor represents a challenging modeling task whose ultimate conclusions may or may not be credible. Most of the experimental work is still ahead.

- It is difficult to evaluate how the results presented to date demonstrate progress toward meeting milestones and deliverables, especially since this is only a two-year project.
- Project weaknesses include a lack of important understanding of particle stability, membrane integrity, and particle quantum yield.
- Feasibility of the efficient ion transport aspect is still not clear.

Recommendations for additions/deletions to project scope:

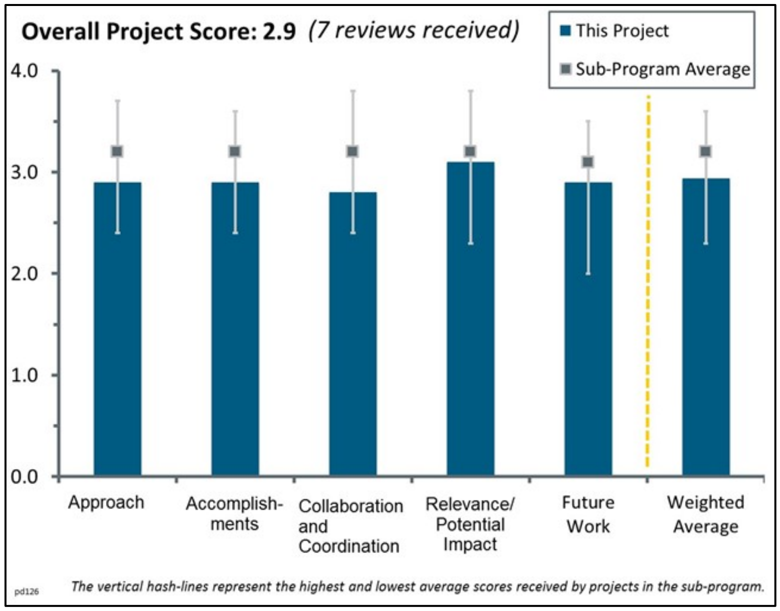
- A reference point for “80% less pipes, 80% less pumping” is needed. If 3 L of hydrogen is a deliverable, then 1.5 L of oxygen should be also—or at least the project should demonstrate somewhere along the way that it is splitting water and making the gases in a 2:1 ratio and not forcing something in the photoreactor to function as a sacrificial reagent. The top layer of the tandem slurry photoreactor will have to be optically transparent to long-wavelength sunlight. Even if the bandgap of the semiconductor is large, there are still scattering effects that have to be minimized to provide any radiation to the lower chamber.
- A comparison of the overall gain in solar efficiency and decrease in parasitic energy consumption for this proposed “stacked bag” vessel compared to the conventional “side-by-side bag” vessel using the same photocatalyst should be conducted. Such a study would provide a basis for evaluating the merit of the proposed reactor design concept. Right now, it seems that identifying/developing a better catalyst would have significantly more impact than developing this new stacked bag design.
- Experiments should be conducted.

Project #PD-126: Compressorless Hydrogen Refueling Station Using Thermal Compression

Kenneth Kriha; Gas Technology Institute

Brief Summary of Project:

This project is demonstrating the technical and economic feasibility of the thermal compression concept for 700 bar hydrogen fueling stations. Compared to conventional liquid hydrogen fueling stations, thermal compression is expected to minimize energy loss and eliminate the need for compressors and refrigeration chillers. Investigators will use modeling to establish and optimize the concept design, evaluate cryogenic pressure vessel options, and compare costs of traditional and proposed stations.



Question 1: Approach to performing the work

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

- The project approach is excellent; the flow chart describing the modeling and demonstration, and the integration are laid out clearly. The project is addressing important barriers, especially the reliability and cost of hydrogen compression.
- The baseline proof of concept for this project is sound and well-thought-out at this stage. Based on the project results, it will influence future work. The testing approach is right-sized and appropriate at this stage of development. The proper steps to achieving this concept were well-thought-out (although practicality is another consideration).
- The technical approach seems comprehensive. However, perhaps a more practical approach would have been to evaluate the economic feasibility of this concept earlier in the project. Granted, the thermal compression would eliminate the compressor; however, the increase in the number and complexity of the pressure vessels seems to indicate very early in the project that demonstrating economic feasibility will be a challenge. The high-pressure storage is among the most expensive items in a hydrogen station. The addition of the cryogenic service to high-pressure storage would be assumed to be more expensive than the existing high-pressure storage today. In the reviewer's experience, the "smaller-and-more-vessels" approach seen on slide 10 of the presentation typically results in higher costs, not in reducing material costs as claimed. While the elimination of the chilling system would be a cost savings, it provides a means of controlled cooling of the gas. It is not clear how the project team means to control temperature of the hydrogen gas in accordance with SAE J2601 with this system.
- Several major barriers have been identified. The following are some thoughts:
    - The design of an ASME cryogenic vessel capable of the thermal and pressure cycling is mentioned, but this issue should not be minimized since this concept is impossible without that capability. There are major technical hurdles on this item.
    - The amount of piping and valving that needs to be both cryogenic and high-pressure is substantial. It is not clear that this heat leak has been factored into the analysis.
    - There may be several potential hazards that need to be addressed, including potential high-pressure back flow and leakage due to cold-temperature equipment cycling.
- So far it has been mostly modeling, with many test cases and results. However, there is no recommended approach yet, just generalized trends such as desiring more, smaller bottles. At the go/no-go point, there should be one recommended design, and that recommendation should state exactly how much hydrogen is

lost and at what step in the process. It seems likely that anything with more than 10% hydrogen losses will be too expensive. The test demonstration in Period 2 should consider using at least two or three bottles to simulate the cascade operations. It is not clear what the source is for all the thermal energy needed to heat up the hydrogen. Slide 6 could have shown some basic energy balances to verify.

- The approach for this project seems to be disconnected, as it does not seem to capture a good deal of the experience from previous cryo-compressed work at Lawrence Livermore National Laboratory (LLNL) by the same team. A tremendous amount of time was wasted on creating an Excel spreadsheet model that did not function well and had to be recoded in Fortran. Having recoded the model, it seems that the project is making adequate progress but is very much behind schedule. The U.S. Department of Energy (DOE) should reassess this project and possibly tune it toward more of an analysis project, screening the materials and determining an optimum design prior to moving forward toward a demonstration that looks to be in jeopardy of not being completed by May 2017.
- The concept is extremely interesting, but it is too early to determine the validity of the modeling or whether the thermal swings are too challenging for a single cryogenic vessel.

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.

- The project has made excellent accomplishment toward the overall project—the key was developing the model and programming it in Excel/Fortran. The limits of the model variables have been defined, and preliminary data has been generated. Some limitations of the model have also been identified, though the reason has not been described. The project has identified that the vessel cost using ASME-allowed materials will be above the 2020 DOE cost target, while a "low-cost" steel could be a potential candidate. However, in the absence of clear understanding of the properties of the "low-cost" steel, it seems premature to make any decisions. In some instances, the boil-off percentage exceeds 100%, which seems to suggest more hydrogen is boiling off than was originally put into the dewar. It is not clear if the >100% boil-off is a bug in the software, was calculated on a different basis, or has some specific implications.
- A good modeling approach has been made, and software has been developed to design the system. The project's initial first analysis came up with using 25–30 300 L cryo vessels. The following should be first-level questions and considerations: what footprint this would take at a station and whether it would even be possible at any level; whether it would work in terms of operating expenditures (OPEX) without a boil-off recovery compressor, as the boil-off losses are huge and unpredictable (this would be considered from a cost analysis perspective, taking into account the number of vessels); and the fact that, combined with the space consideration, a 700 bar cryo tank design with up to 300 L water volume with *foam* insulation and minimal losses is a *very* long way away (i.e., this is long-term and high-risk).
- The early modeling work and concept layout reflect very encouraging progress. The absence of thermal data associated with cryogen vaporization is baffling. Future reviews need to present a convincing slide that accounts for various ambient conditions. Early economic analysis is interesting, but there is some concern that pre-mature economic evaluation may lead to an early death of the concept.
- The project has made good progress on the modeling but not much progress on the hardware. The hardware is the key challenge and needs most of the additional work. Also, it is not clear that the fills are J2601-compliant since the graphs show non-full fills.
- Not much seems to have been done in the area of reducing the risk of finding suitable materials for these vessels that have so many thermal cycles at high pressure. There should be more specific information than simply citing "low-cost steel." More details should be provided as to how and when ASME will allow these material specifications and for what thermal cycling conditions. Several test cases were run using the model, and sensitivity analysis gives trends on what variables are important.
- No complete vessel designs were described, although several parameters were screened. Tremendous modeling, design, and materials challenges lie ahead for this project, and accomplishments do not seem to support meeting the go/no-go.

Question 3: Collaboration and coordination with other institutions

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

- The listed collaborations seem appropriate and relevant. Not much was said about what Royal Dutch Shell has contributed.
- It is a bit unclear who was doing what work. However, all team members seemed to have good knowledge and responded to questions and concerns well.
- It too early to be critical on this point. However, the project needs more collaborators. In particular, the team should collaborate with experts in modeling, vessels, heat exchangers, and material thermodynamics. Also, perhaps the concept could benefit from employing ceramics for the purposes of strength or insulation. Lastly, the team should be encouraged to search for other work that has explored the concept or portions thereof. Time and energy can be saved by building off others' research. The concept seems too practical to believe others have not investigated.
- Several collaborators were mentioned, but it was not very apparent what they are contributing or have contributed thus far in the project.
- The project partners have been identified. It seems there is an overlap of several activities, e.g., station design and cost analysis are being done by several partners. Thus, while the partners may all contribute to a given activity, it is not clear who is leading it.
- It would be beneficial to have partners with more substantial cryogenic and liquid hydrogen handling experience. They may have some insight that could help the overall system.

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.

- If technically and economically viable, the concept will have a huge impact on all fuel cell applications. The elimination of high-pressure pumps, compressors, and receivers will have an impact on capital outlays and eliminate significant OPEX. The concept also reduces the technical competency required to manage and maintain the system in commercial applications.
- The relevance to being able to recover some of the energy required for hydrogen liquefaction is promising. Theoretically, it should be simpler and less expensive. However, adding many storage bottles—each with its own sensors, valves, and relief valves, plus a control system—is complex and may be expensive and difficult to operate. Diagrams show five active valves per cryogenic vessel (CV), not including relief valves. This could require several hundred cryogenic valves, which is expensive. It is not clear how they are actuated or how much area is required for all these bottles.
- If the number of vessels at high pressure can be significantly reduced, it could be a long-term approach to high-volume station design. Many barriers exist (lack of material for cyclic conditions at cryogenic pressures, insulation to limit boil-off, footprint, and cost).
- The project is relevant to Hydrogen and Fuel Cells Program (the Program) objectives and has the potential to make progress toward lowering the cost of hydrogen compression. The key challenge to the concept appears to be the limited material selections for sustaining the CV operation over its service life.
- If successful, this technology could provide substantial ability to improve fueling capability. However, the capital cost does not appear as if it would scale well for larger stations. The capital cost might appear almost linear as the system scales in size.
- While this technology could ultimately deliver some advantages for large-scale storage at stations, it does not seem certain that this project will identify the designs that will demonstrate those impacts. The Gas Technology Institute might consider making this a pure analysis project prior to moving toward a demonstration that seems likely unachievable, given the schedule.

Question 5: Proposed future work

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

- The team has a good go/no-go gate based on cost analysis vs. the baseline, and it is sound and reasonable within the funding period.
- The proposed "future work" reflects the standard format expected in the Program's Annual Merit Review presentations. The team should be encouraged to focus more on proving the concept and less on analyzing the economics or physical footprint. If the concept is viable, many organizations will provide the funds to streamline and remove costs.
- The proposed future work is reasonable. The challenge in the "demonstration" phase is not clear: it is obvious that vaporizing liquid hydrogen at constant volume will raise the pressure, and the duration over which this pressure increases is a function of the rate at which the hydrogen is heated. Therefore, the value of data generated needs to be brought out.
- The proposed future work is correctly identified, but it is not evident that the work can happen within the schedule of this project.
- Demonstrating a single vessel may not be indicative of a successful larger future system since the demonstration will not address cycling of vessels or the balance of system issues (piping, valving, and heat leaks). Also, the test vessel should be 1000 bar, not 700 bar, in order to do complete fills. If not already done, an analysis should be completed for the heat leak of the entire system (piping, valves, etc.), including during dormant periods, to see the impact of overall losses.
- It is suggested that this concept's economic feasibility be moved ahead in the project timeline to demonstrate that this system has a potential cost advantage over today's conventional station design.
- There should be a firm proposed design that has estimated hydrogen losses characterized for a variety of usage scenarios before the go/no-go decision is made.

Project strengths:

- Theoretically, this approach should require less energy than hydrogen compression. Some exergy is recovered from the liquefaction process. Thermal compression may reduce station maintenance costs and downtime due to compressor failures.
- This is an interesting and unique concept, and there is good collaboration between partners. The approach is novel. Technical back-up material was presented well.
- The concept has few moving parts, lowers required operating competency, and utilizes ambient energy to capitalize on cryogenic density.
- The project has proposed a simple and innovative concept to compress hydrogen and has developed a model to evaluate a host of process and station parameters to optimize cost and hydrogen boil-off losses.
- The project demonstrates a compressionless system. The concept can eliminate the need for dispenser cooling for a fuel station.

Project weaknesses:

- The major weaknesses of the project include lack of a suitable vessel and the inability to scale well past 400 kg—there is not much in the way of savings per kilogram for larger stations since equipment need is virtually linear. Also, it is not clear whether the losses are acceptable based on perception, cost, and safety.
- Weaknesses include the high boil-off rate, the unpredictability, and the associated hydrogen OPEX this boil-off would affect. The large number of vessels may not be a weakness but simply a result.
- The design requires a large number of yet unproven cryo pressure vessels with associated valves, sensors, and controls. There are potential high hydrogen losses.
- The value of the demonstration task is not clear. While it may provide some thermodynamic data, it would have been better if the demonstration was designed so that it could validate assumptions used in the model.
- There are not enough collaborators. There is potential for economic analysis to stifle a great idea.

Recommendations for additions/deletions to project scope:

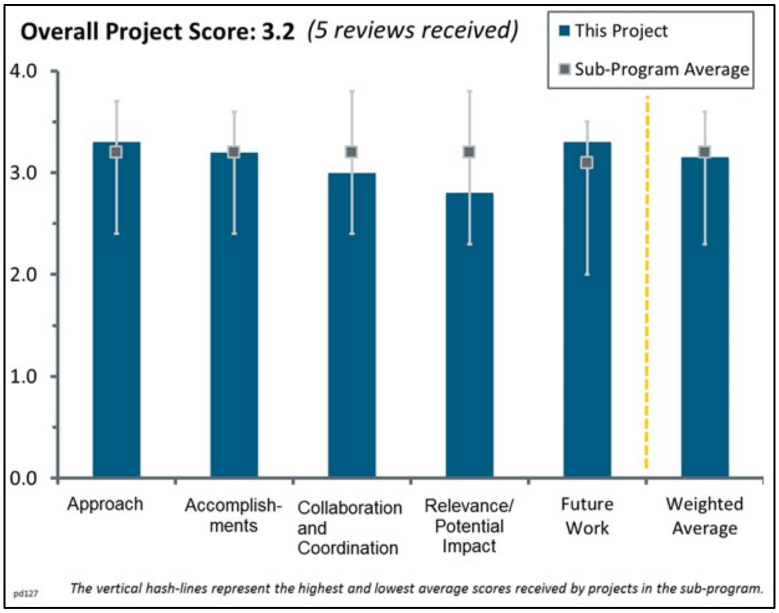
- The project should look into whether the excess hydrogen at 150 K can be economically recovered, to be either compressed by a smaller compressor or re-liquefied and returned to the storage tank. Venting cold parahydrogen seems to be a waste. The model input variables should be separated by what can be controlled by design (number and sizes of vessels, etc.) and what the requirements may be (fueling profile). Then the project should consider worst-case requirements. For example, the project should consider what happens if 10 cars in a row come in and need to charge only from 400 bar to 700 bar. In that scenario, the lower-pressure bottles might have to be vented at a higher pressure, resulting in a higher percentage of hydrogen loss.
- Boil-off rate should be considered in cost analysis. The project should also consider how to reduce the footprint to make it reasonable/similar to existing technology. In addition, the project should consider liquid hydrogen setback distances and the effects that cryogenic liquid hydrogen at 700 bar would have on these at actual sites. This would feed into the footprint discussion. Finally, the project should consider additional thermal modeling to understand possible reduction of the footprint.
- The team can consider replacing the demonstration task with machine learning aspect described on slide 12. The ability to evaluate a host of variables and determine the most sensitive input parameters would be beneficial for any future modeling activities for any other concepts. The team can consider leveraging machine-learning capabilities within the group of collaborators or in the DOE national laboratory network.
- Modeling, vessel options, and demonstration should be added; economic analysis should be pushed to the next phase.
- A separate project is recommended to resolve the pressure vessel issue.

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 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 **3.3** for its approach.

- The approach to the work seems reasonable and aims to address barriers.
- The overall approach seems reasonable for a project at a very early Technology Readiness Level. Much of the science seems very interesting and is novel.
    - The use of starch vs. monomeric sugars is very good in this context, although it does add to the complexity of the system. It should be noted, though, that this in vitro approach will likely be effective only for very clean sugars/starches and so will have somewhat limited greenhouse gas benefits when compared to a lignocellulosic system.
- It is unclear why expressing multiple enzymes in one *E. coli* host is useful for this project, other than to minimize the volume for very small-scale assays. The benefit of this will quickly disappear entirely once the system begins to scale.
    - The work on cofactor replacement is interesting and ultimately very necessary for an in vitro system to function and is one of the most innovative areas of the proposal.
    - Overall, the argument that an in vitro system is the best approach for hydrogen production is not entirely convincing, given the complexity of the hydrogen evolution machinery (eight accessory enzymes), but if robust variants can be found for all elements of the system, this limitation has the potential to be overcome.
- With the synthesis of the enzymes and identification of challenges/solutions with the cofactors, the project appears to be off to a promising start in terms of meeting the hydrogen volume objectives. It is still not clear how the project will go about meeting the cost targets, as it is not clear that all of the capital costs have been fully considered. An industry partner might be helpful for testing the commercial viability of the approach.
- From a practical point of view, this approach based on in vitro enzymatic reconstruction of a complete degradative conversion of cellulosic feedstock to hydrogen is among the most risky, high-cost, and impractical approaches conceived.

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 protein overexpression seems to be working very well, and the enzyme production work so far is solid. However, the price and scale at which this work can be accomplished is questionable. Specifically, the bridging electron mediators seem to be fairly expensive. Also, there was not much information given to determine how *long* certain hydrogen productivity rates could be achieved—whether it was just a few moments or sustained.
- Many of the preliminary results are promising, but sustaining peak hydrogen production will be critical for this project to be successful. If there are currently no milestones around sustained production vs. peak production, it may be useful to add some. Additionally, running the system in semi-continuous mode, even at very small scale, seems much more useful than scaling to 1 L.
- There are insufficient quantitative results to assess progress towards achieving benchmarks. Techno-economic analysis (TEA) is qualitative, based on early-stage pre-technologies, and highly approximate.

Question 3: Collaboration and coordination with other institutions

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

- The work is shared among a small set of collaborators who contribute useful expertise. The extent to which the principal investigator (PI) utilizes that expertise is not known.
- There is reasonable collaboration between the University of Georgia and Virginia Tech, but the role of the University of Georgia is unclear beyond supplying *Pyrococcus furiosus* enzymes.
- If it were economically viable, the team should be expanded to include a national laboratory and industrial partner at this stage.
- Very little information was given 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 **2.8** for its relevance/potential impact.

- This is a discovery-based fundamental research project that has some overlap with the Office of Energy Efficiency and Renewable Energy mission for hydrogen production. The value in this approach appears to be primarily in demonstration of in vitro biochemistry of isolated enzymes and genetic tools for overexpression.
- Generally speaking, making hydrogen from biomass seems like a non-optimal use of the carbon in biomass. The one real advantage of biomass as a feedstock, aside from the fact that it is renewable, is that it contains carbon in organic form, which can be transformed relatively easily into easily transportable liquid fuels and chemicals. Because biomass is highly oxygenated, making relatively reduced hydrocarbon fuels necessitates the input of reducing power or the rejection of CO_2, but this problem is even larger with hydrogen production, in which all carbon must be rejected as CO_2. Many technologies are looking at making organic molecules from sunlight/CO_2/H_2O because of how central these carbon energy carriers are to the U.S. economy. However, given the state of technology for electrochemical water splitting for hydrogen production and the massively falling costs of renewable electricity from wind/solar, it is hard to see what role biomass-to-hydrogen could play in the renewable energy future.

Question 5: Proposed future work

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

- Decreasing enzyme production costs in an *E. coli* host is not terribly useful. As the PI noted, this would not be the ultimate expression host for the process, and so this optimization of multi-enzyme expression plasmids will not be useful for the ultimate process. The TEA discussion around expression in alternative hosts is convincing; maybe a single proof of concept in a filamentous fungi and then extrapolation to the rest of the enzymes would be sufficient. Cutting this scope would call for more focus on the enzyme engineering/enzyme stability/cofactor replacement. In addition, it is unclear what the scale-up past 10 mL really shows, considering how many elements of this system are still in development. A focus on the other tasks with the resources dedicated to this scope would potentially accelerate the crucial unknowns in this project, such as cofactor switching/enzyme stability.
- The scale-up is appealing if tied to more information on how long the project can achieve its productivity rate.

Project strengths:

- Enzyme production appears to be going well.

Project weaknesses:

- TEA efforts appear to be lacking and could better assess the practical needs for impact.
- More detail concerning hydrogen production rates is needed.

Recommendations for additions/deletions to project scope:

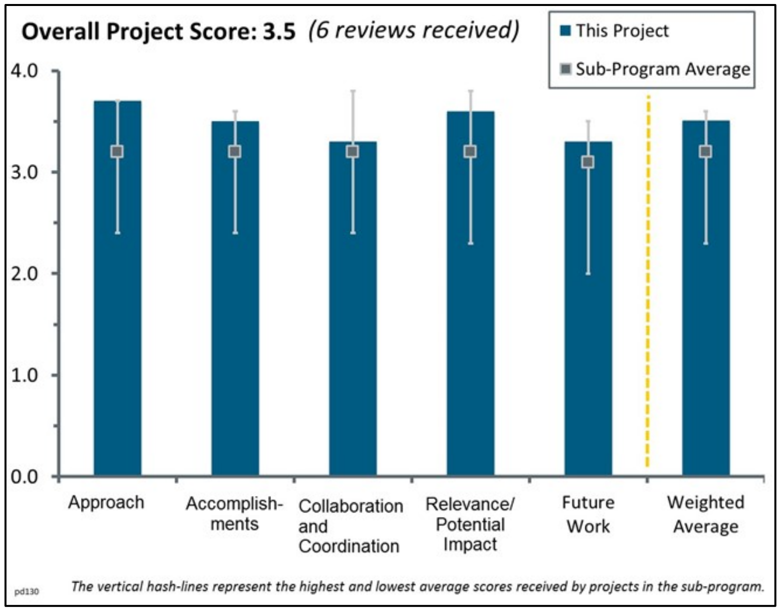
- More detail concerning hydrogen production rates is needed.
- DOE should assess whether this research project will benefit its biological hydrogen program.

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 further the first concept in history that directly uses ortho–para conversion to aid in cooling hydrogen. Researchers will develop vortex tubes for 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 vortex motion cools parahydrogen for subsequent liquefaction. 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.

- This initiative represents a new and original approach to liquefaction of hydrogen. It could simultaneously make liquefaction more efficient, reduce the scale of balance of plant and therefore the amount of investment needed in the plant, and increase the flexibility to where liquefaction plants may be located. This would permit locating liquefaction near sources of alternative energy (solar, wind, hydroelectric, etc.).
- A novel concept is proposed that is focused on an important issue with hydrogen liquefaction. If successful, it could have significant impact on the economics of hydrogen production.
- On the Overview slide, it is not clear what is meant by lower liquid hydrogen delivery cost ($4–$15/kg). It is unclear whether this is the reduction of the current cost. Nearly all liquid hydrogen in North America is delivered for $4–$7/kg. A specific cost-per-kilogram savings target should be provided in the overview. For example, a reference to how much reduction in specific power (in kilowatt-hours per kilogram) could be provided, along with specific reduction in dollars per kilogram based on expected power cost. It is also not clear what the total reduction in capital is expected to be. Making this information available will make the value of the project clearer.
- The approach to vortex tube work seems good. The project uses modeling first with testing to validate the model. More details on specific test parameters should have been given. More details on modeling assumptions should be given. The link between the vortex tube work and the work with the refrigerant composed of helium, hydrogen, and neon is unclear. Characterization of the mixed gas refrigerant is also valuable. The cycle as drawn does not work. With 25%–75% hydrogen gas coming out at 30 K and 14 psi, it is unclear how the remaining gas gets liquefied. There is not enough energy for a final Joule–Thompson (JT) expansion.
- The approach to liquefaction is highly original and seems brand new. However, it is unclear whether the approach is feasible. The aspects of the vortex tube operation and how the hydrogen in the periphery may heat up and convert to orthohydrogen by interaction with the catalyst are clear. It is, however, unclear how cold hydrogen in the center of the vortex tube may convert to parahydrogen so rapidly and without being in contact with a catalyst. If it does not convert to parahydrogen, it is not clear that the approach still makes sense.

- It is not clear how lowering the cost and achieving commercialization of units that are 5–30 MTPD can be achieved when the anticipated TRL of the vortex tubes is expected to be 4 at the end of the project. A stage that will bring the TRL of the vortex tubes to at least 7 is missing. Maybe the stated goal should be realistically redefined or clarified.

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.

- It seems like a good deal of good work has happened in just seven months. This includes two different thermodynamic models, a computational fluid dynamics model, and two different types of tests.
- The Status of achieved modeling and analytical and experimental development are outstanding.
- Excellent progress has been made in the limited time available, with a focus on the key aspects of the technology and an appropriate go/no-go decision point. Since the success of the concept is ultimately dependent on the net energy usage, the amount of cooling required for stream E on slide 7 (returned to the source) is critical. A mass and energy balance should be presented for the entire system, not just the vortex tube.
- The analysis and initial experiment at Washington State University (WSU) seems appropriate. However, the helium–hydrogen–neon refrigerant task seems to have little relevance to the overall effort. It is unclear that this task is needed to solve the main issues with the technology.
- It is too early in the process for much input, but the project should be encouraged to get to actual vortex tube experimentation quickly. Pre-work is good, but the project needs to demonstrate actual technology.
- This is the first year of the effort, and it is perhaps too early to comment on progress.

Question 3: Collaboration and coordination with other institutions

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

- The team has all the necessary technical capabilities. In addition, having Praxair as a partner provides the required commercial input.
- Collaborations seem to be balanced between academia and industry.
- The concept was developed by WSU, which has a well-established industry partner, Praxair. If subsequent development is successful, it might be useful to introduce more collaboration.
- Working with Dr. Leachman at WSU and leveraging his hydrogen test apparatus is a good partnership. More details on Praxair involvement would help. The Praxair facility in Tonawanda, New York, has good mixed-gas characterization capabilities and may be able to assist in that task.
- It seems that Praxair is having minimum participation other than to review the relevance of the approach. This relationship should be enhanced, and Praxair should contribute to all aspects of the problem, including cost analysis and comparison with other existing approaches.
- There is only one partner other than the prime technology provider. Input from additional industrial gas companies that currently operate liquid hydrogen plants would be useful, in addition to at least another organization that can provide feedback on the core technology. In addition, potential collaboration with the magnetocaloric liquefaction project could be useful. It is possible that each technology could benefit from collaboration.

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 problem of small-scale liquefaction is important for future hydrogen-fueled transportation, and it seems that this concept is scalable to small sizes, even though the analysis presented seems directed to large sizes.

- Liquefaction plants are large and expensive, and there are few in the United States and Canada. This technology could revolutionize the scale-up of infrastructure to help meet hydrogen supply needs as automotive demand materializes. If the proposed benefits are realized, another aspect of revolutionizing infrastructure would be the reduction in capitalization necessary to build the hardware.
- The project clearly addresses a key barrier to a liquid hydrogen pathway in terms of high energy use and cost. It is well aligned with DOE goals and targets.
- The project's success could be critical to achieving DOE goals.
- This is solid, basic research that could lead to potential improvements in technology to lower the cost of liquid hydrogen production. There may be challenges and it may not work, but it is worth continuing.
- Increasing the figure of merit (FOM) of hydrogen liquefaction is an important goal. If successful, the vortex tube cooling has the potential to do that. The project emphasizes scaling up, but the technology may also make small-scale liquefiers competitive. More details on the relevance of the mixed-gas refrigerant would help. It is unclear whether the refrigerant is to be used for isothermal cooling at 20 K.

Question 5: Proposed future work

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

- The future work plan adequately covers the key aspects and is appropriately focused on the go/no-go step. It is critical to understand the potential impact of the technology on the net energy usage and cost of a liquefaction plant. Hence, the planned techno-economic analysis that will indicate the impact of the proposed work on the overall cost is essential and appropriate.
- The scope of the proposed future work seems to be appropriately defined.
- The proposed work appears to address research goals. However, the technology may have application in solving different problems that are not in the scope defined for the present work.
- It seems that a good idea for a future task would be comparing this approach to liquefaction with today's power plants under a variety of performance conditions. For example, it would be good to know what happens if no ortho–para conversion occurs in the middle of the tube and the result is 50% parahydrogen at the cold end. It is unclear whether this will destroy the concept. It is unclear how this would affect the results, considering that this conversion would have to occur outside the tube.
- The project should focus on technology of liquefaction, not location of hydrogen plants. Location of plants will be dependent on the market and feedstock costs, not on the location of wind power. Distribution costs from small, remote plants will likely outweigh the potential for savings.
- The proposed go/no-go test is for 500 g/hr flow of hydrogen with 5% para–ortho conversion. However, more than composition should be measured; the temperature drop and the warm gas bypass rate should also have some metrics.

Project strengths:

- The FOM computation indicates good promise for improvement of liquefaction efficiency over standard methods.
- A new technology to lower the cost of ortho–para conversion by performing at a higher temperature is interesting and potentially valuable.
- If successful, the vortex tube has the potential to reduce hydrogen liquefaction costs and meet DOE targets. The mixed-gas refrigerant work is interesting and will provide new data on possible refrigerant mixtures.
- This is a novel concept with high potential impact. The project has a solid technical background, as well as good planning and focus.
- Modeling, analysis, and experimental capabilities are project strengths.
- The approach is highly original.

Project weaknesses:

- This is not strictly a project weakness, but it would be helpful if comparisons to current liquid hydrogen production methods were expanded upon, and an effort made to predict the potential for cost savings. In addition, the benefits of a smaller balance of plant could be expounded upon in greater detail.

- Scale-up must be validated. The project needs to develop specific cost savings per kilogram to compare to existing liquefaction (e.g., specified power and capital reductions) to show value. In particular, the cost savings of the new technology might be overwhelmed by the worse economics of small-scale production; i.e., it is best to focus on large plants.
- The system requires approximately 40% of the flow to be diverted back to the compressor, increasing compressor size and operating cost. The cycle as shown requires two cold compressors. The project may want to add a recuperative heat exchanger and use warm compressors. It is unclear that the vortex tube will work as advertised.
- There is potential over-optimism regarding the commercialization impact.
- It is unclear that the approach is feasible.

Recommendations for additions/deletions to project scope:

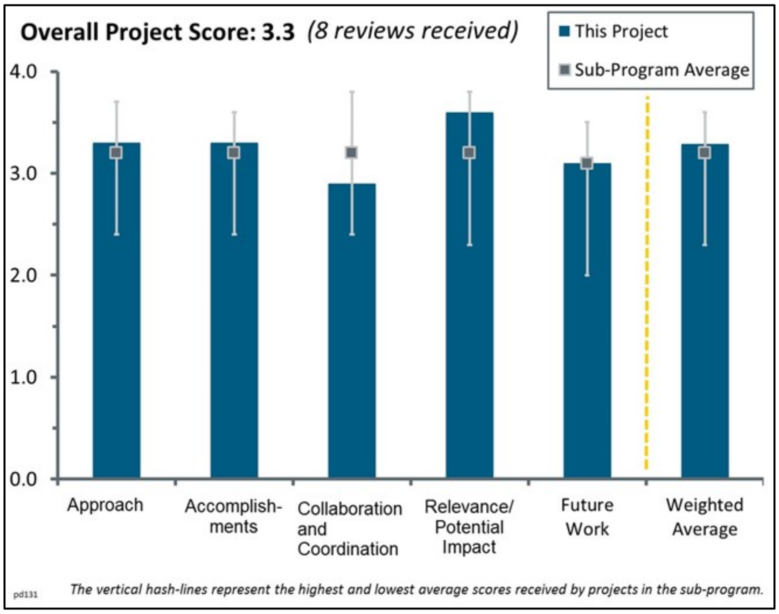
- Apparently the technology could play a role in boil-off recovery from existing storage systems. This capability would be of great value to liquid hydrogen users with large storage systems and the need for long product "hold" times. It may prove that some combination of vortex separation and magnetocaloric cooling can yield even greater system efficiencies and cost reductions for production. Magnetocaloric cooling might be a good "front end" for the vortex separation. Initial development should proceed as planned to validate these technologies, but some thought should be given to seeing whether the two processes are complementary in some way.
- The project should consider cooperation with a magnetocaloric project. Focusing on liquefaction technology exclusively is recommended, regardless of the location/type of feedstock or power. The liquefaction technology is the breakthrough, not the location of small plants, etc. It will likely be easier to transport renewable power than ship liquid hydrogen over long distances.
- The proposed process is not entirely clear. It appears the project is using liquid hydrogen plus a catalyst to convert warm equilibrium hydrogen to cold equilibrium hydrogen, removing the exothermic heat of conversion of 25% of the gas. Then the vortex tube converts a certain mass fraction of that back to normal hydrogen at warm temperatures, and the endothermic heat of reaction is used to cool the remaining hydrogen. It is unclear whether the advantage is just being able to remove less heat of conversion at higher temperatures to create para, then reconverting the para back to ortho, removing a higher heat of conversion now at a lower temperature. It is unclear that if there were no ortho–para conversion back and forth, the vortex tube would act as an isenthalpic expansion process to cool the gas. It is unclear whether the vortex action helps make it closer to isentropic instead. It is unclear why the cold gas flowing through the center of the tube does not exchange heat with the warm gas flowing around the outside of the tube. More details should be given in future presentations, including mass and energy balance of the vortex tube.
- The project should focus on the vortex tube operation and performance and eliminate side tasks that do not contribute to this effort.

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 with 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 **3.3** for its approach.

- The approach, successively testing different prototypes, is strong. The project starts with an existing Generation (GEN) I testbed, then designs a new GEN II prototype to build and test. The team then applies these lessons to the final GEN III system. If all that is successful, then this project may have a huge impact on future hydrogen liquefaction.
- It is good that previous project results (i.e., from Prometheus Energy Group, Inc., [Prometheus]) are followed up. The approach seems systematic. It is unclear how deeply involved large gas companies are, or whether they are interested in the technology.
- The project appears to be very successful in proving the technology and refining the efficiency of the dual active magnetic regenerator (AMR) design; however, there was little to no discussion about the complete liquefaction system. It was unclear how the ortho-para (O-P) conversion will be performed, what the overall energy consumption will be, and how many dual regenerative AMRs will be required for a 30 tpd liquefaction plant.
- The capability of 100°C temperature reductions by the proposed magnetocaloric cooling system will be a breakthrough in hydrogen liquefaction, if achieved. The introduction of this technology would reduce hydrogen liquefaction costs at a time industry needs to increase production. There is no question that this research directly addresses important barriers of the cost of producing liquid hydrogen.
- Magnetic refrigeration is not a new concept. Development of this concept was previously funded by U.S. Department of Energy (DOE) at Prometheus with Dr. Barclay, who is a consultant on this project, as the principal investigator (PI). It is not explained how the proposed approach is different, whether it is an improvement over the previous work, or what specifically was changed. While the concept is interesting, the approach needs to identify key novel features properly and explain why these would lead to a successful development given the critical barriers.
- The project is focused on creating a magnetocaloric liquefaction machine that operates from ambient temperature all the way to 20 K. This seems rather difficult to accomplish. Magnetocaloric materials operate best near a phase transition temperature, especially if this is a second order transition. The authors would, therefore, need to select a cascade of materials with appropriately located phase transition temperatures to make this work. It is unclear whether these materials have been selected and properly characterized. It is also unclear whether the performance predictions take this into account.

- There is no mention of either O-P conversion or the energy required to make this conversion, so it is not clear whether that conversion is included in the 6 kWh/kg target for liquefaction energy or in capital cost estimates. Barriers are addressed, but there appear to be too many for one project.
- The demonstrated approach and projected targets are sharply focused on critical barriers.

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.

- Achieving a world record on ΔT for He is very impressive.
- There are a lot of accomplishments and good early results. It is good to see that the existing machine was refurbished so rapidly and that it produced many good results.
- Given that the project is 10 months into a four year effort, the progress is good, but it is important to keep in mind that in previous funding DOE spent $2 million on the technology that facilitated much of the progress in this project. By the next DOE Hydrogen and Fuel Cells Program (the Program) Annual Merit Review, it is hoped that the project will have progressed on technology development and will also have provided equal attention to the balance of plant to enable reviewers to fully appreciate a production design that accounts for all energy demands (O-P conversion, cooling, heat transfer fluid management, etc.).
- It appears there was a lot of progress in the first part of the project, but the starting point based on past work was unclear. Characterizing the temperature spans and heat lift in the GEN I system was a good accomplishment as well as figuring out to add the bypass flow. The propane liquefaction demonstration offers a preview of the potential for other gasses. In addition, a good start on the GEN II design was accomplished.
- The project seems to be on schedule.
- While a lot of details are provided, a more concise description would be helpful to clearly understand what is the background material (e.g., from PD-019 from fiscal year 2011/2012) and what is accomplished in this project. It is unclear whether this project is a continuation of the previous project. It would be useful to elaborate lessons learned from the previous work and see whether and how they are guiding the project plans. Based on the description provided, significant progress seems to have been made toward the goal. However, since it was previously attempted to develop this concept at Prometheus with DOE funding, it is critical to reference the work and to specifically point out differences, improvements, etc., and whether there were any shortcomings or hurdles before, which are being addressed in this work.
- The project has demonstrated liquefaction with propane, which is good for demonstrating initial promise, but propane may be a relatively easy fluid liquefaction compared to the liquefaction of hydrogen. While the magnetocaloric cooling properties were explained, the "bypass" design, a key element in the system to achieving the large temperature reductions, was not covered as clearly. The project has just begun, so accomplishments with hydrogen are limited at this point.
- The presentation was not concise or clear as to progress and barriers. There was too much technical detail in some areas, but not enough explanation of how the pieces fit into the "bigger" picture. The main benefit of the liquefaction technology is stated as reducing the compression energy for the Claude cycle to get to liquid nitrogen (LIN) temperatures. However, if this is the case, the technology might be better proven by making other cryogenic products first, instead of going to the more difficult step of liquid hydrogen. For example, the technology might be more cost effectively used to liquefy air, which is a much larger industry. While the test on propane was interesting, liquefaction of air would be a more impressive accomplishment to test the technology.

Question 3: Collaboration and coordination with other institutions

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

- The project has good coordination with Ames National Laboratory and its regenerator material group. It appears the materials database shows many magnetocaloric materials over the necessary temperature span to choose from. It is unclear what happened to past collaborators in this area such as John Barclay and Prometheus.

- The project partners appear to be appropriate and well-coordinated.
- There are a limited number of participants, but the partners are experts.
- For the work performed to date (i.e., refurbishment of Prometheus equipment), the collaboration appears to be well-targeted. Going forward, the team needs to expand to include individuals and organizations that can help determine whether the technology can be scaled up in a manner that retains the stated targets of $70 million in capital and 6 kW/kg. This support has to come from organizations that have experience in process and plant design and safety standards, and the support has to account for maintenance and operations.
- The project has good collaboration efforts with partners having necessary skills and capabilities. It would be helpful to have an industrial partner to assess commercial feasibility of the technology, if successful.
- The project should include a potential future user (e.g., Linde Group or Praxair) in the team to provide industry perspective and cost evaluation. Magnetic refrigeration is commercially available for ultra-low temperature. The project team could investigate whether any of these magnetic refrigeration companies would agree to contribute to this project.
- There are no industrial gas companies collaborating on the project, nor anyone actively liquefying hydrogen for validation of technology. Including these partners is recommended, especially since it is not clear that the project participants have any direct experience with liquefaction of hydrogen.
- It is unclear how deeply involved, or even interested, gas companies such as Linde Group, Air Liquide, or Air Products and Chemicals, Inc., are.

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.

- Active magnetic refrigeration has great potential to reduce liquefaction costs and reach DOE goals. The preliminary cost analysis shows the feasibility.
- This work directly supports DOE Program goals for development of a national hydrogen infrastructure. The technique, already demonstrated to have import for the propane industry, may have similar implications for production of liquid nitrogen, liquid helium, and other cryogenic fluids.
- Hydrogen liquefaction is an important area, and any reduction in energy usage and cost for this step would be helpful. The proposed approach is claimed to have a potential major impact on the overall energy and cost. The proposed new concept would replace liquid nitrogen cooling section, including associated compressors. This is a part of the overall liquefaction system. It would, therefore, be helpful to illustrate what portion of energy and cost will be impacted and to what extent, along with cost/benefit analysis, to clearly point out the benefit of the work.
- The project has good relevance as liquefaction (whether on site or not) is sure to play an important role in the future of hydrogen-based transportation.
- If the project objectives can be achieved in a functional plant, the relevance of this project is very meaningful.
- There is no question that the success of this project could be critical for achieving DOE market deployment goals.
- If the projected values can be reached, this will be an important step to reach the DOE target.
- It could be a breakthrough technology, but at the current development stage, it is unclear whether the cost estimates can be credibly assessed. For this reason, it is not clear whether it will be able to support DOE efforts.

Question 5: Proposed future work

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

- The proposed future work is excellent. More details should be given on the GEN II prototype. It is unclear whether the GEN II prototype is just one stage of a multistage device for GEN III. The temperature span is also unclear. The work done on manufacturing different materials for the regenerator heat exchanger use is critical.

- The future work described is comprehensive and covers all essential features.
- Proposed future scope seems appropriate to meet the project objectives.
- The authors should develop a systems model that may predict liquefaction efficiency based on what materials are being used at what temperature levels and how these will perform. This is key to determine how likely this approach is to reach the efficiency and cost targets presented here.
- The plan for a GEN II system seems to be the correct path to take, but more needs to be accomplished in defining a full-scale system (30 tpd) and evaluating its financial viability.
- The challenges faced by this project are significant and range from manufacturing the base material up through the process cycle itself. It is likely best for the project scope to be narrowed to some specific near-term (and simpler) objectives, which can then lead to a future hydrogen liquefaction system.
- The project should incorporate gas companies unless Emerald has the potential to industrialize such a technology.
- At this juncture, the proposed work has just begun. While the proposed work looks logical, this reviewer is not sufficiently expert to comment on the proposed development plan.

Project strengths:

- The investigators have made good progress in a short time. They have identified key issues and have designs and mitigations in place. The idea of magnetic refrigeration has great potential to simplify and reduce the cost of hydrogen liquefiers.
- This could provide a technology breakthrough, if successful, and significantly lower cryogenic processing cost.
- The project undertakes a novel concept compared to state of the art for hydrogen liquefaction. The project has strong team expertise and fundamental understanding of technical details as well as existing equipment, materials, and background work to build on.
- The project has high relevance and has made excellent progress with many accomplishments in very little time.
- The project has a strong knowledge and expertise base as well as appropriate partners.
- Optimizing the AMR technology and demonstrating design improvement in the GEN II stage is a project strength.
- The project directly addresses issues of cooling efficiency for hydrogen liquefaction. The development of this capability requires advances in several areas: extending the magnetization cooling properties with layered materials optimized by the use of different rare earths; demonstrating the “bypass” technique; and expanding the technology capability of rotating disk atomizations to work with rare earth alloys.

Project weaknesses:

- The project needs more, and better, partners within the cryogenic and liquid hydrogen industry. The project needs to address O-P conversion. If it is included in process calculations and capital, the project should provide the details. The project has too many risks and assumptions for one project and should be narrowed. There are many intermediary steps between proving the refrigeration capability of this technology down to liquid hydrogen temperatures. It would be best to work on those intermediary steps first and prove it to be a viable refrigeration technology at warmer cryogenic temperatures first.
- The project is ambitious in that it will pioneer several advances simultaneously. The PI had difficulty explaining the various aspects of the “bypass” innovation that was critical in achieving the cooling gains advertised. It remains to be seen whether progress will be straight forward in achieving cooling at the lower temperature regimes required for liquefaction of hydrogen. A lot rests on the cooling effectiveness of the multi-layer magnetic regenerators for the GEN II system.
- The lack of a concise description of a new concept is a weakness. The lack of focus and the absence of go/no-go decision points are weaknesses considering that this project is high risk.
- There is no set plan to fully quantify the long-term goal (capital and performance) for a 30 tpd design.
- These ideas have been proposed for years, but practical designs have not been achieved.
- The feasibility of the approach remains questionable.

- The lack of industry collaboration is a weakness.
- Magnetic liquefaction has been promised to work “tomorrow” for the past 40 years.

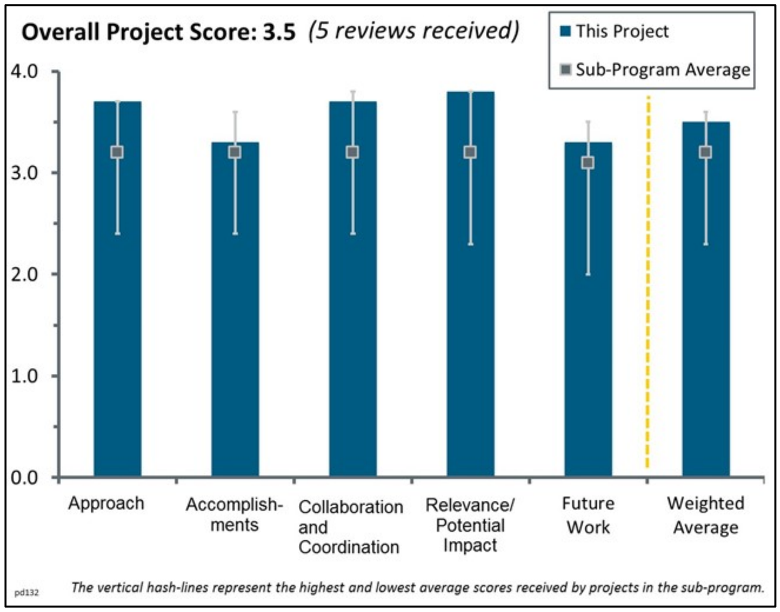
Recommendations for additions/deletions to project scope:

- Consider collaborating with the vortex tube liquefaction project. Maybe magnetocaloric hydrogen liquefaction can do refrigeration to -320°F, and the other project can refrigerate to -420°F. The project should delete any consideration of assistance from liquefied natural gas liquefaction. This is known technology and obfuscates the benefit of the magnetocaloric hydrogen liquefaction technology. For the next step, focus on demonstrating better performance than traditional refrigeration to -320°F LIN temperatures, rather than liquid hydrogen.
- There are several areas that might be noted. This technology stands to be synergistic with the vortex flow technology, and this synergy should be pursued. This technology, or some element of it, may prove useful in recovering cold gaseous boil off from storage vessels. More important, this technology does not simply apply to liquefaction of hydrogen, but would have application in the production of other cryogenic fluids.
- The investigators should clearly identify key challenges of magnetic refrigeration that were not attempted or satisfactorily solved before and focus efforts on those. A critical path analysis would be helpful to present with well-defined go/no-go decision points.
- It seems that, given the low level of technological maturity, it may be premature to do a cost analysis, as the analysis is likely to change as the system definition improves.
- The project should include more collaborators. Inclusion of an assistance PI responsible for evaluating the viability of a full-scale plant is also recommended.
- The project should perform industry workshops.

Project #PD-132: Advanced Barrier Coatings for Harsh Environments

Shannan O'Shaughnessy; GVD Corporation

Brief Summary of Project:



Plastic and elastomeric seals are integral to all areas of hydrogen compression, storage, and dispensing (CSD), and seal failure is a major contributor to hydrogen compressor maintenance, adding significant downtime and cost to operation. This project is developing two types of coatings to improve seal life from <1,500 hours to >8,000 hours: flexible barrier coatings that mitigate hydrogen ingress into the seal, preventing premature failure; and low-friction coatings that reduce wear of rigid seals, extending seal life significantly. Using polymer vapor deposition will result in uniform coatings that are conformal down to the nanoscale.

Question 1: Approach to performing the work

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

- The technical approach of the principal investigator (PI) was soundly presented as a way to improve seal performance in non-lubricated compressor seals. The work has primary relevance to moving seals. The approach to application of coatings to a variety of elastomers and rigid seal parts is good. Uniform deposition and depth of impregnation is a key challenge for most coating technology.
- Great work on polymer layer impregnation with physical vapor deposition (PVD); this looks very promising. The vacuum tumbler is great idea on how to develop a new manufacturing method.
- This project is focuses well on its target—producing a low-cost, low-friction, high-sealing material for compressors. They are spot-on in their approach.
- The approach is defined well and well thought-out, considers key barriers and issues with the state of the art, and demonstrates good understanding of fundamental problems. The science behind the proposed solution is explained well and covers the necessary details. The efforts are focused on solving the specific issue with compressor seal failure.
- GVD Corporation (GVD) understands the problem well and has demonstrated that their well-thought-out approach can overcome the barriers.

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 vacuum tumbler is great idea on how to develop a new manufacturing method.
- For a project that is only one year along, their progress is excellent. They have optimized the chemical vapor deposition (CVD) process and are on track for scale-up.
- For the one style of coating and deposition presented, the PI had good, presentable results. Something missing was a risk mitigation plan if this particular coating and deposition did not work as planned and alternate depths, materials, or processes of deposition could be used to plan for unforeseen real world

results. Further, it is not clear if there are other applications and if they would be effective in other industries.
- GVD is making good progress toward overall project and DOE goals. Their accomplishments have been demonstrated by their good barrier coating technology, which shows significant reduction in helium and hydrogen ingress via permeation through elastomeric seal materials.
- A systematic development and testing plan is in place with attention to detail. Given that the team has already spent little more than a year on the project, slides 14 and 15 do not appear to reflect adequate progress. It is unclear which portion is background GVD work and which is performed under this contract. It would be helpful to more clearly point out the accomplishments during the project time period.

Question 3: Collaboration and coordination with other institutions

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

- The team has assembled the right set of talent in their collaborators—an advance seal manufacturer, a manufacturer of compressor equipment, an industrial user, a hydrogen fuel system (HFS) designer, and a national laboratory—to understand and perform fundamental permeability measurements; this is excellent.
- GVD has demonstrated good collaboration with other partners, with whom well-planned coordination as well as participation has been a key for their success.
- This is a strong team of collaborators, and their work plans are sound.
- Nice collaboration with industry partners that really need some help with seals to support linear hydrogen compressor technology.
- The partners selected are appropriate to cover all aspects of technology development and commercialization.

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.

- The relevance of this work is spot-on. Hydrogen fuel cell (HFS) reliability is by far and away the biggest Achilles heel for the early deployment of HFS; this goes right to customer satisfaction. Compressor failure is the biggest factor in HFS reliability, and this project addresses that issue.
- This work on PVD layers for polymers promises a potentially major impact on reducing compressor maintenance costs.
- The project aligns well with the Hydrogen and Fuel Cells Program and DOE research, development, and demonstration (RD&D) objectives, and has potential to advance progress toward DOE RD&D goals and objectives. The project has a significant impact on the success of delivery hydrogen fuel to the end users. Besides the fact that it could reduce the cost of delivered fuel, the safety as well as environmental impacts are also important factors to consider.
- With a good risk mitigation plan, this project could have significant impact on station reliability and stations' operating expenses.
- The project addresses an important, known practical issue with the high cost of hydrogen compressor maintenance. It is well-aligned with the DOE goals and program plans. A techno-economic (cost/benefit) analysis showing the potential impact of successful implementation of the proposed solution would be helpful.

Question 5: Proposed future work

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

- The vacuum tumble PVD system will hopefully demonstrate seals with new properties optimized for high pressure hydrogen service.

- The only thing preventing this project from being rated "good to excellent" is identifying the metrics for what is acceptable compressor testing wise; permeability is straightforward and good. The PIs must specify how often they will inspect seal wear, and what patterns they will look for. This type of testing approach was not presented.
- The proposed future work covers essential aspects necessary for commercial implementation of the technology. It may be helpful to conduct accelerated durability testing of the coatings in a simulated environment before testing in a compressor under actual operating conditions. Also, a cost-benefit analysis would be helpful.
- The proposed future work is effective. However, it lacks a risk management strategy for scaling up the design to accommodate large-scale operation with high-throughput manufacturing. Mitigating risk is particularly crucial in making sure that the project is executed smoothly and effectively as planned.
- The proposed future work is good; however, this project really needs to start concerning themselves with possible contaminant poisoning of the fuel, through both outgassing and friction degradation. Concentration levels on the order of parts-per-billion (ppb) are required to maintain the fuel quality needed for fuel cell electric vehicles. Also, maybe this is a bit unfair, but the fuel quality specifications are being revisited and may go lower than currently published standards (SAE J2719, and ISO 14687). This is a serious enough omission in their future plans that it cost them an entire rating point.

Project strengths:

- The project enjoys good scientific and technical background for the proposed solution, excellent capabilities and qualifications, and appropriate partners.
- The project is technically sound and well-coordinated with well-known collaborators who are considered experts in the field.
- This work is very relevant, and employs a good approach to basic science. This style of station presents an opportunity for reducing operating expenses.
- The team is excellent, as are the facilities and program plan.

Project weaknesses:

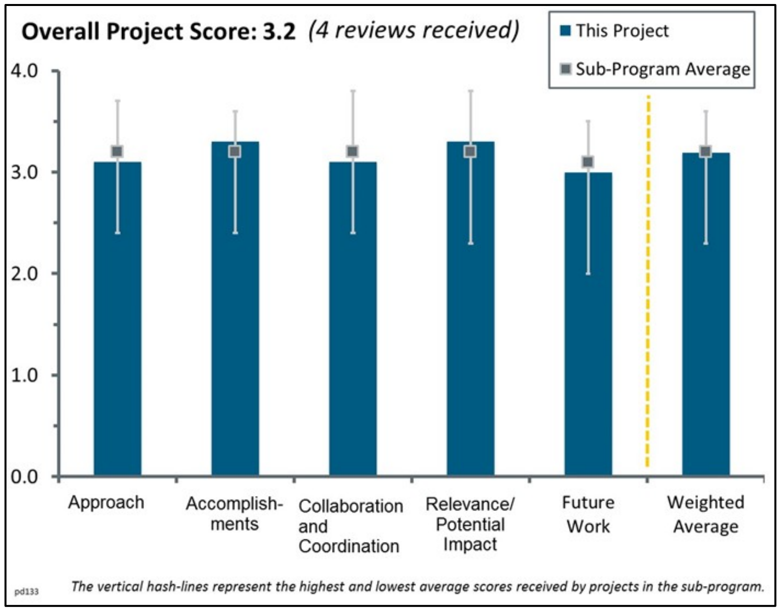
- The project lacks risk mitigation plans, should this style of coating and deposition not work.
- The project lacks a risk management strategy, which could cause many failures such as project delay, costly repair, and unsafe operation.

Recommendations for additions/deletions to project scope:

- This project really needs to start concerning themselves with possible contaminant poisoning of the fuel, through both outgassing and friction degradation. Concentration levels on the order of ppb are required to maintain the fuel quality needed for FCEV. Also, maybe this is a bit unfair, but the fuel quality specifications are being revisited and may go lower than currently published standards (SAE J2719, and ISO 14687).
- It is highly recommended that a risk management strategy be added to the project scope to ensure its success.
- The PIs should create alternate methods to achieve project goals/have a risk mitigation plan.

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 will design and demonstrate 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 **3.1** for its approach.

- The project's approach sounds promising. It is unclear why there is a lack of refueling infrastructure data with approximately 20 stations in operation and the data being acquired by the National Renewable Energy Laboratory (NREL).
- It is unclear if any benchmarking has been done based on the different systems available. It is also unclear if there are noise-related issues with the compressor, which could potentially limit hydrogen fueling station (HFS) installations.
- The presentation did not make clear how this novel compressor design was an advantage over two compressors achieving the same objective. A more practical approach would be to move the proposed "first stage" of the novel compressor to the terminal compression location and use 400–450 bar tube trailers to deliver hydrogen. This concept is currently being developed by some gas merchants, and achieves the same objective as this project without the need to develop this technology for the station itself.

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.

- No benchmark against DOE goals is provided. Overall project progress is good. As mentioned in the presentation, a vehicle simulator could be helpful for station providers and car original equipment manufacturers (OEMs).

Question 3: Collaboration and coordination with other institutions

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

- The project should incorporate additional collaborative partners (industry) and a more diverse stakeholder set and inputs.
- Even though this is a broad approach on increasing performance and decreasing cost of stations, the spectrum of partners is very small. It is unclear if major station providers are involved.

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.

- If ambitious objectives are reached, the project could have a large impact on station cost reduction. The relevance for major industry players is not clear.

Question 5: Proposed future work

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

- Elaboration on future work is not very detailed. It is unclear what will be tested and when it will be tested.

Project strengths:

- The project offers a large potential impact on station cost.

Project weaknesses:

- There is no collaboration with station system technology providers.
- The presentation did not make clear how this novel compressor design was an advantage over two compressors achieving the same objective. A more practical approach would be to move the proposed "first stage" of the novel compressor to the terminal compression location and using 400–450 bar tube trailers to deliver hydrogen. This concept is currently being developed by some gas merchants. This achieves the same objective as this project without the need to develop this technology for the station itself.

Recommendations for additions/deletions to project scope:

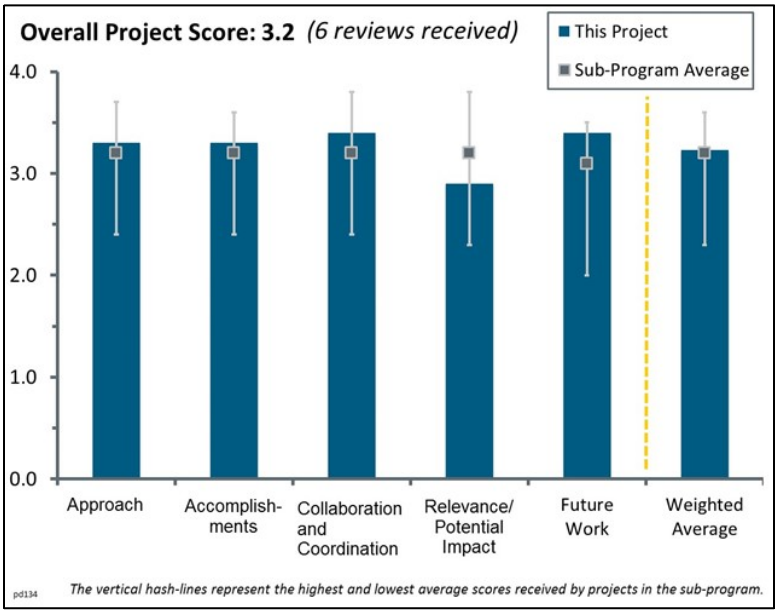
- The project leads should convene industry workshops.

Project #PD-134: Cryo-Compressed Pathway Analysis

A.J. Simon; Lawrence Livermore National Laboratory

Brief Summary of Project:

This project is developing well-to-wheels (WTW) cost and emissions estimates for cryo-compressed hydrogen (ccH2) pathways. Investigators are building physics-based and industry-guided estimates of system and cryopump performance and cost into the Hydrogen Delivery Systems Analysis Model (HDSAM). The project furthers the ability to identify the cost-effective options for hydrogen delivery by enabling analysis of infrastructure trade-offs through an investigation of key parameters associated with liquid hydrogen.



Question 1: Approach to performing the work

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

- The approach is appropriate to estimate future costs and greenhouse gas (GHG) emissions for ccH2. This will be helpful to establish technical targets for components, but it is not useful to understand current technology status. The study relies on models that assume high-volume liquid hydrogen (LH2) production and high market penetration of fuel cell electric vehicles. Both sets of data are needed to trace a pathway from today's cost, GHG emissions, and technical performance to their future, desired values. Today's WTW values are presumably very different from what they will be in the future, so it would be helpful to know how different they are. Low station utilization will have a significant impact on the WTW results. It is good that some of the hydrogen losses will be actually measured. If more money becomes available, the next step should be instrumenting the facility to validate station engineering calculations for the losses that have not been physically measured.
- It appears to present a reasonable approach to collecting information about barriers addressed by this project. It appears that the project is estimating "potential" boil-off and not necessarily estimating "actual" boil-off. It is not clear whether the initial description of ccH2 is 350 bar or 700 bar.
- The approach is pretty clear—the project wants to better understand the thermal connection (if any) between the two dewar systems.
- It is a small project with clear focus.
- First, the title of the project appears misleading. The project as described is about LH2 pathways, not cryo-compressed pathways, as is generally understood, and this is correctly stated in the objective on the Summary slide. Secondly, although the objective is stated as pathway analysis, the work described is mostly about estimating boil-off losses. If the project is defined as focused on LH2 boil-off losses, then the approach is adequate. The total liquid hydrogen pathway analysis would also involve estimating costs and emissions of the major contributing step, namely liquefaction.

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

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

- The project has excellent results on the cryo-compressed pathway.
- There is nice work/progress for a small project—it is interesting stuff.
- Progress is good. It seems that the team may be able to finish ahead of schedule.
- It is not clear why "losses from vehicles" after dispensing are included in overall boil-off loses. The project should clarify what assumptions are made for cryopumping and the effect on vehicles, e.g., whether the station dispense 35 MPa or 70 MPa CcH2.
- It would be helpful if the methodology used in estimating the losses were more clearly described. The results as presented are somewhat confusing. For example, on slide 7, Total Delivered and Total Dispensed bars have the same value; it appears that the losses are not subtracted. It is unclear as to how the analysis results presented can be used to understand boil-off losses, reduce losses, or overcome associated barriers. Decreased losses with larger stations, higher demand, and better delivery logistics are obvious and likely well understood in the industry. The value of the work needs to be better explained and represented. It is not clear how variation in demand during the day (with number of cars) would impact boil-off.

Question 3: Collaboration and coordination with other institutions

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

- The right institutions have been approached to provide feedback. Shell is not mentioned as one of the collaborators; it would be useful to get its input on the analysis, given its experience operating a LH2 station. Granted, the hydrogen is re-gasified and delivered in gaseous form, but its expertise with production, delivery, and storage could be useful.
- Relevant partners are onboard.
- Most appropriate partners are selected, although their specific contributions are not obvious.
- The project should consider involving Washington State University Professor Leachman to discuss project accomplishments from an academic perspective on LH2 boil-off losses.

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.

- Given the lack of data in this area, it is critical to understand the potential impact of ccH2 if this technology is to be deployed at large scale in the future. Also, the relevance of the study would be greater if WTW parameters were calculated/measured for current technologies as part of the project's scope.
- It is unclear how this ccH2 will go, but it certainly is an interesting and significant line of research.
- The project contributes to understanding of LH2 boil-off losses, which is good for more accurate hydrogen delivery modeling. However, at currently observed (potential) boil-off loss rates and trailer capacity, it may not make economical and logistical sense to consider alternative distribution methods and/or invest in improving (reducing) boil-off rates.
- Hydrogen boil-off losses are important in the LH2 delivery pathway. While some of the losses may be reduced with better equipment and delivery logistics management as well as higher usage profile, some of the losses may be unavoidable. The impact of these losses on the overall cost and emissions appears minimal.
- It is uncertain how much cryo-compressed systems will play a role in future because of manufacturing issues with the storage system.
- ccH2 does not seem to be a realistic future pathway. Only a single original equipment manufacturer is onboard and collaborating.

Question 5: Proposed future work

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

- The proposed future work is nice—there is a clear plan for project completion.
- The proposed future work is appropriate. Actual measurement of various losses would be useful, and it is important to compare them with the estimates to confirm the methodology.
- Given the budget limitations, the proposed future work is adequate, but it would also be helpful to: (1) estimate/calculate WTW parameters for today's operation conditions, (2) work with an instrument facility to validate engineering calculations for hydrogen losses, (3) ensure that the cost analysis is in alignment with financial models such as the Hydrogen Financial Analysis Scenario Tool (H2FAST).
- The project should stay focused on LH2 delivery losses and LH2 cryopumping. It should not expand too far outside of these areas, because there is plenty to learn in these areas.
- With regards to the project objectives, the proposed final steps make sense.

Project strengths:

- The strengths include the facilities, expertise of Lawrence Livermore National Laboratory staff, and collaboration with partners with expertise in delivery and dispensing of LH2.
- The strengths are the new territory for understanding boil-off rates of LH2 and involved partners.
- The project is interesting and cutting edge—ultimate applicability may be more for medium- to heavy-duty and large marine applications, but this is interesting stuff.
- The understanding of fundamentals of liquid hydrogen boil-off behavior under different conditions is a project strength.
- The project has a very good and detailed analysis of the pathway.

Project weaknesses:

- Venturing into vehicle-side loses is a project weakness. It is unclear what the state of hydrogen is out-of-cryopump and on what infrastructure/type of vehicles this applies, e.g., 350 bar hydrogen, 700 bar hydrogen, 350 bar CcH2, or 700 bar CcH2.
- The weaknesses are the narrow focus within the given pathway and the lack of clarity.
- The relevance of the project is uncertain.

Recommendations for additions/deletions to project scope:

- The project seems good.
- The project should take feasibility of vehicle storage into account if possible.
- The project should be appropriately titled to indicate the focus area. Given the small budget allocated, the scope cannot be increased to cover the entire pathway.
- The project should expand the scope to 1,000 kg/day capacity stations to have broader applicability in the future. The current inclusion of an upper limit of 800 kg/day capacity station can be expected to be too low for future usage (particularly in medium- and heavy-duty vehicle fueling settings). The project should consider assessment of mitigation strategies for boil-off (in addition of what was presented in reviewer-only slides).
- It would be interesting to also calculate the Levelized Cost of Energy of the full pathway and compare it against other hydrogen pathways. The project should ensure that the cost analysis is in alignment with financial models such as H2FAST