

## **Hydrogen Storage**

### **Summary of Annual Merit Review Hydrogen Storage Subprogram**

#### **Summary of Reviewer Comments on Hydrogen Storage Subprogram:**

Reviewers considered hydrogen storage R&D to be critical to the President's Hydrogen Fuel Initiative. The projects were considered to be appropriately diverse and strongly focused on addressing technical barriers and meeting targets. The Center of Excellence approach was praised as an example of effective R&D collaboration between National Laboratories, industry and academia.

The major criticism by the reviewers was the redirection of funds to Congressionally-directed projects and its impact on the program. As a result, none of the Grand Challenge awards were made in 2004 and a shift toward more exploratory research, a recommendation in the National Academies' Report, could not be implemented.

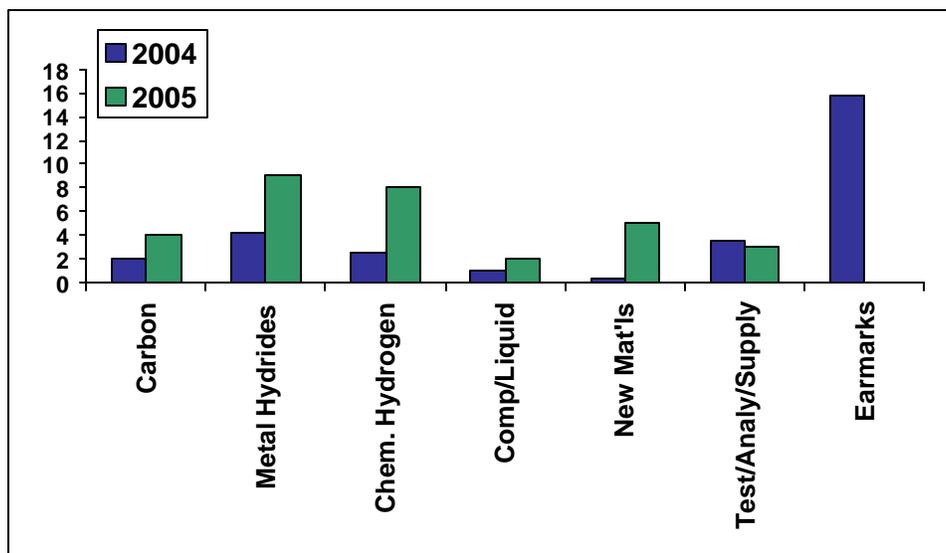
Additional recommendations were to conduct earlier analyses on alternate options and place greater emphasis on go/no go decision points. Most comments related specifically to continue funding of carbon nanotube R&D. DOE's position is that it is premature to eliminate or scale back work on carbon nanotube materials. Adequate research funding and time are needed to address reproducibility issues and to provide closure on controversial issues surrounding past work on carbon nanotubes in the technical community. A milestone is in place to reproducibly demonstrate 4 wt% storage in carbon nanotubes in 2005. A go/no-go decision point based on achieving 6 wt% storage is scheduled in 2006.

There was also a request for DOE to justify its emphasis on metal hydride R&D. Research on complex metal hydrides is part of DOE's hydrogen storage portfolio largely due to recent breakthroughs, particularly with sodium alanates. Although this material, with a less than 6 wt% theoretical capacity for the *material alone*, will not achieve the 6 wt% *system-level* target, a better mechanistic understanding of hydrogen storage in sodium alanate will likely advance the design and development of other higher-capacity complex metal hydrides.

Finally, the reviewers also commented on the need for cost reduction in the area of compressed hydrogen tanks. It should be noted that the National Academies' Report recommends that DOE terminate work on compressed and liquid hydrogen tanks for on-board storage because these technologies are near commercialization. However, since early hydrogen-powered vehicles will likely use high pressure tanks, the major barrier of cost must still be addressed. The DOE plans to shift away from funding work focused only on weight and volume targets for tanks. Future work on compressed/cryogenic tanks will focus on novel approaches for cost reduction and conformability, as well as on R&D for off-board storage applications. Advanced concepts for tanks such as heat dissipation in solid-state systems will also be explored.

### Hydrogen Storage Funding by Technology:

The funding portfolio for hydrogen storage addresses primarily long-term materials R&D to meet 2010 and 2015 targets for on-board applications. The requested FY2005 funding profile, subject to Congressional appropriation, addresses the National Academies' Report recommendations and provides greater emphasis on new materials and concepts. Focused R&D through Centers of Excellence in metal hydrides, carbon-based materials and chemical hydrogen storage will also be implemented subject to congressional appropriations.



### Majority of Reviewer Comments and Recommendations:

In general, the reviewer scores for the storage projects were average or low (the highest scoring project received only a 3.32 score). In many cases, this is because the projects are new and have minimal progress or technical accomplishments to report. Key recommendations are summarized below. DOE will act on reviewer recommendations as appropriate for the scope and coherency of the overall hydrogen storage research effort.

- **Tanks:** Focus on cost reduction. Define component volumes in the calculation of system volumetric capacity.
- **Chemical Hydrogen Storage:** Address life-cycle efficiency, particularly for off-board regeneration of spent fuel, early in the program. Emphasize go/no go decision points in the project research plans.
- **Metal Hydrides:** Refocus efforts on new materials with potential to meet the long term storage targets.
- **Carbon:** Implement go/no-go decision points. Shift emphasis from single walled carbon nanotubes and address the broader area of carbon-based materials and other high surface area adsorbents.
- **New Projects:** Some new projects need more focus and coordination. Greater emphasis on new approaches to meet long-term targets is positive in the DOE plan.

- **Testing and Analysis:** This is a critical area and there is a need for rapid storage capacity evaluation methods. Achieving reproducible and accurate results, accepted by the technical community, is an important first step.

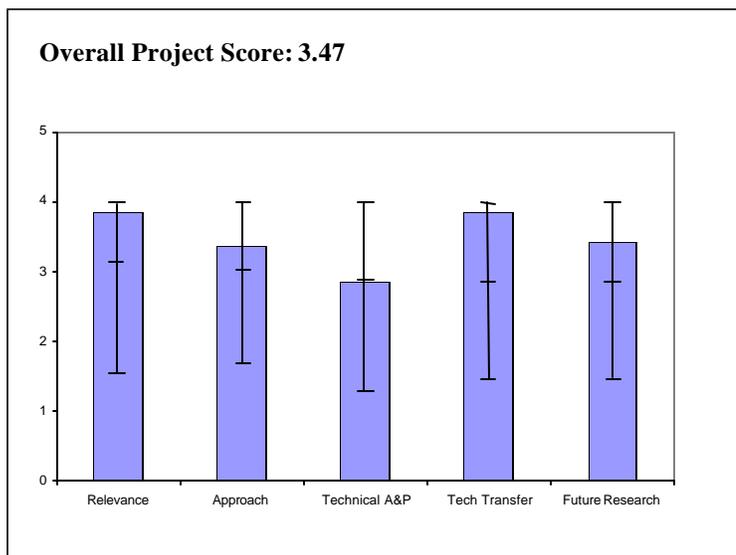
For essentially all areas, defining a list of system components and their respective weights, volumes, and cost, is recommended.

Project # ST-1: Hydrogen Storage Subprogram Overview  
 Milliken, JoAnn; DOE

### Brief Summary of Presentation

The purpose of this Storage Subprogram Overview is to: describe goals/objectives; budget; barriers/targets; approach to R&D; technical accomplishments; interactions and collaborations; solicitations and awards; and future directions. As such, it sets the stage and puts into context the R&D and analysis projects which will be presented in this subprogram area during the Annual Merit Review.

### Question 1: Relevance to overall DOE Hydrogen objectives



This presentation earned a score of **3.86** for its relevance to DOE objectives.

- This Subprogram is extremely important to allow the President's Hydrogen Fuel Initiative to be realized. The projects included are diverse in their approaches to storing hydrogen (primarily on-board a vehicle).
- Program status presented clearly.
- Nice overview of future program directions.
- The presentation gave a good description of the program.
- Most of the funded projects have merit and align with the Hydrogen Fuel Initiative and Multi-year R&D Plan.
- On-board H<sub>2</sub> storage is absolutely critical to the President's program work. Without it, the mobile hydrogen economy will never come to fruition.
- Generally very good overview, but some further justification would have been useful, e.g., why the heavy emphasis on metal hydrides.

### Question 2: Approach to performing the research and development

This presentation was rated **3.36** on its approach.

- This H<sub>2</sub> storage program is very focused on the technical barriers and stresses that all projects keep focused on the targets.
- New ideas/approaches are given fair attention.
- Early evaluation of the projects vs. targets would be beneficial considering redirection of resources if no-go.
- The approach of sponsoring a wide range of programs that study many different techniques is perfect.
- Seeking many inputs on what to work on and what goals to set is important too.
- Funding work that is too risky for others, but could serve if it works is even appropriate.
- I have doubts that carbon nanotubes will have chances of reaching the 4 wt% storage density target.
- May need to be slightly more ready to drop projects with serious doubts.

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

This presentation was rated **2.86** based on accomplishments.

- Low score due to funding issues and timing.
- Congressional earmarks affected the output.
- Progress in this program is very good. The nearer term technologies are fairly close to meeting 2010 targets. No technologies are close to meeting 2015 targets and capacity and conformability still remain as difficult barriers.
- Although, individual projects show different progress towards targets, overall accomplishments stated very clearly, progress towards goals is significant.
- Progress is really quite good.
- Nearing 2010 goals for physical storage methods in many dimensions, a significant advance.
- While solid storage materials are behind the more mature methods, there has been significant percentage increases.

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

This presentation was rated **3.86** for technology transfer and collaboration.

- This program demonstrates an outstanding level of collaboration with many industry, academic, and government participants. Examples, such as Quantum, are working towards technology transfer for implementation in vehicles.
- Centers of Excellence is a good example how research collaboration should be done.
- Center approach will ensure collaboration, cooperation and coordination.
- Very good collaboration shown by the contractors and the DOE team as well.

### **Question 5: Approach to and relevance of proposed future research**

This presentation was rated **3.43** for proposed future work.

- Can implement earlier go/no-go decision point?
- The Grand Challenge should provide significant increases in quantity, quality and speed for advancing research in H<sub>2</sub> storage.
- Excellent focus on new storage materials to meet the targets.
- Future work is well distributed with emphasis in many important areas.
- There is a need for more work on cost of compressed and liquid storage methods to declare victory. While these were down played in the NRC report, it is hard to argue with the idea that most FCVs will initially use one of these methods if the cost can be reduced to an acceptable level.

### **Strengths and weaknesses**

#### Strengths

- Strong leadership.
- Strong vision.
- Integrity.
- The DOE team is qualified, coherent, well-managed and doing a great job.
- Most funded projects have merit.
- Vital and well informed team of sufficient size to handle the job.

- Diverse approach.
- Go/no-go decision points to "window" programs based on promise.
- Ambitious customer oriented goals.
- Excellent program plan.
- Presenter appeared to be an exemplary program manager.

#### Weaknesses

- Be more flexible.
- Be more agile.
- Adopt the "fail-fast" principle.
- Available funds not enough to start new projects.
- Earmarks have a large negative impact.
- Projects chosen may not be the best ones.
- Some projects will have trouble moving forward.
- Material safety should be a major concern.
- Funding paced to government budget cycle.

#### Specific recommendations and additions or deletions to the work scope

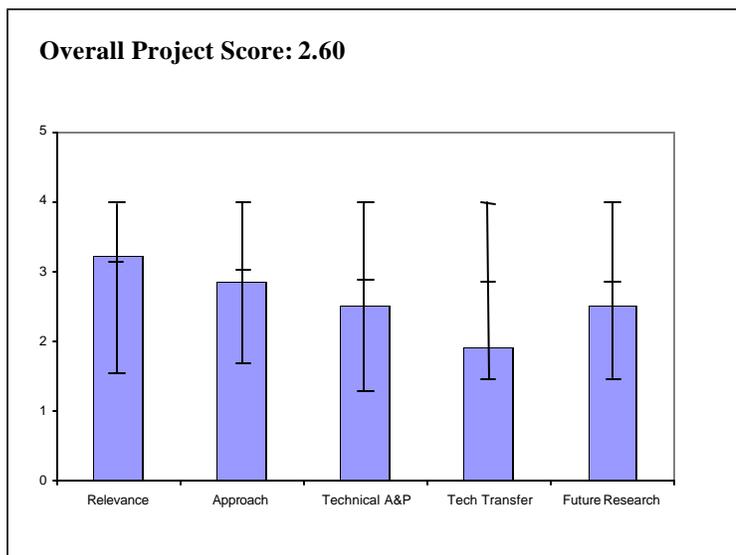
- Start the analysis of each alternative as soon as possible.
- The go/no-go decision should be made at earlier stage.
- Conserve resources.
- Spread the resources to a wider group.
- New technology routes may need to be assessed earlier in their development against their theoretical ability to reach technical and economical targets, and terminated if the answer is negative.
- More new-materials projects are needed.
- Funds on carbon nanotubes can be reduced.
- Work on sodium borohydride regeneration will not be fruitful. So, go/no-go decisions need to be set earlier.
- Need more money.
- Carbon Center and carbon nanotube work is weak. The progress in this area has been particularly disappointing, and the material themselves are all quite limited even in terms of their potential to meet volumetric density goals. This area should be a small effort, but does not warrant a separate Center of \$4M in funding. Physical storage is proposed to go from \$1M to \$2M. Where is the extra \$1M going to go? I'm very surprised to see no mention of ammonia in any of the existing projects, new projects, or the chemical hydride center.

## Project # ST-2: Low Cost, High Efficiency, High Pressure Hydrogen Storage

Newell, Ken; Quantum Technologies, Inc.

### Brief Summary of Project

Quantum Technologies' project goal is to deliver a cost-effective and safe high-pressure hydrogen storage system that will meet DOE targets. The technical plan entails a three-pronged approach: lowering the cost and weight of the storage system (via material optimization, process evaluation and use of lower cost carbon fibers); reducing the amount of material required through the use of sensor technology to monitor storage system health; and increasing the density of hydrogen by filling and storing at lower temperatures. This is a new project, started in 2004.



### Question 1: Relevance to overall DOE objectives

This project earned a score of **3.23** for its relevance to DOE objectives.

- Unclear, particularly given NAS report and DOE goals if this R&D is a priority and will yield breakthroughs.
- Compressed gaseous storage is the only short or mid term technology known today that will support the successful introduction of hydrogen powered vehicles. More research into compressed gaseous storage systems is needed to address remaining issues such as cost and volume.
- This project provides a near term, on-board storage solution and supports activities in demonstration programs. It does have some issues in meeting longer term (2015) objectives.
- At best, compressed hydrogen will not achieve more than part of the 2010 targets. Safety and cost remain major issues.
- This work has some importance since compressed hydrogen is the main way for on-board storage currently.
- Objectives are on target.
- Methods are appropriate to study.
- This is the likely technology for first commercial vehicles so improving the technology is key to the President's plan.
- Good fit with near term goals for storage.
- Cost reduction is key for early success.
- Clearly relevant as a near term option for FC test vehicles; may not be the answer in the long run. Quantum seems to understand the implications and significance of balance of plant (BOP) considerations.
- Cost, specific volume and weight of the H<sub>2</sub> storage system are highly important.
- Will be near term technology.
- Critical to short term application.
- May be applicable to other storage methods.

- Obviously, this work can never achieve 2015 (or likely even 2010) goals. Also, there seems to be little progress on achieving even the 2005 cost goals. Nevertheless, this is currently the most-mature technology, and therefore compressed storage has a place in the DOE portfolio.

### **Question 2: Approach to performing the research and development**

This project was rated **2.85** on its approach.

- Not clear why EIHP is reduced to 1.8 (sp). What is the cooling/refrigeration needed? What is "low cost?"
- Not clear: Fiber translation - how is it done?
- Consider measuring and modeling the effects that embedded sensors may have on fiber translations and other potential localized effects or defects.
- EIHP is okay, but what about US-DOT? Need to conduct future testing.
- Promising results in early evaluations.
- Getting a 3X increase in capability over a couple of years demonstrates that this is a good approach. The project seems to be quite focused on addressing the technical barriers. However, it isn't clear if the approaches outlined in the project will get close to meeting cost targets in the near term and density and volumetric targets in the long term (2015).
- Not clear how cost target can be achieved.
- Not clear whether or not storage system is compared to targets (or just tank?).
- Quantum is trying to improve density and reduce cost.
- This work is not integrated with other research.
- Technical barriers should be addressed in more depth.
- Studying various sensor technologies to improve safety.
- Attacking the right problems.
- Cost is clearly key at this point, and both carbon and processing costs are good ways to approach this.
- Sensors are also a good approach to help on cost.
- Good spread of different approaches for cutting costs/increasing H<sub>2</sub> capacity.
- Lower burst specs + sensors for tank health may have problems with codes & standards.
- The approach is right for what Quantum is trying to do.
- Are burst tests done with hydrogen?
- Nothing really exciting or new, rather a consequently continued development.
- What exactly are "low cost fibers"?
- Work is clearly structured.
- Focus on materials is good.
- Good approach at using lower cost fiber.
- Good consideration to BOP.
- Cost seems to be the largest problem, so this work should be more focused on it.
- The largest potential for cost reduction is in the area of lower-cost carbon fiber, but there seems to be little work in this regard in this project.
- Clearly explain how much potential there is for cost reduction and what types of processing routes there are (and new routes that might be explored) to produced lower-cost fibers.

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

This project was rated **2.50** based on accomplishments.

- Stated that they started in Jan '04 but showed considerable previous work, so what actually was done in '04?
- How is it different from old work?
- Not enough details provided in presentation to be able to accurately assess accomplishments or progress.
- Status was reported for the tank itself but not for the entire system.
- Cost reported was not the DOE-recommended volume levels.
- PI did not seem to understand (or want to understand?) the criteria under which DOE requested that data and results be presented. Thus, it is difficult if not impossible to assess progress toward DOE technical goals - disappointing for a system that is the nearest to being production ready of any storage system.
- Close to 2005 interim targets except for cost (by order of magnitude +). 700 bar progress is encouraging.
- They met the 2005 density targets.
- It is a great challenge to meet the 2005 cost target.
- They claimed they met the 2015 fill rate target and at the same time they are still working on fill rate improvement.
- The presentation combined accomplishments and technology status in several slides. This was confusing.
- It is simply bad that they continue to quote numbers without the regulator as being system numbers. This is now unacceptable.
- Still, probably the best system to date as far as meeting 2005 2010 goals.
- Much better cycle durability understanding than anyone else.
- CoolFuel method appropriate and valuable.
- Cost improvements nice, though still not there.
- Reasonable progress.
- Low cost fiber results promising.
- Seem to be making good progress in what is clearly an uphill struggle to get close to DOE storage targets. Getting the burst pressure criterion reduced to 1.8-X would narrow the gap.
- Unclear if the status numbers include valves, regulators, PRDs, etc. My guess is that they do not.
- Had to realize a significant improvement.
- Insufficient information provided.
- Recently started, 2 months ago.
- Good analysis of cost drivers.
- Good progress on regulatory approvals and validation tests.
- Some reduction in cost achieved.

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

This project was rated **1.92** for technology transfer and collaboration.

- The reason for low grade: it is a private company, so it is hard to share information but need to tap into other DOE-run projects.
- No discussion of collaborations or plans to technology transfer... other than a brief mention in passing that they are working with GM on its fuel cell program.
- Mentioned working with GM, but other collaboration not discussed. Is there additional collaboration with academia?
- No collaboration with other organizations?
- There was no indication of cooperation or technology transfer.

- Cooperating or collaborating with suppliers and customers is critical to realization of the initiative and 2010 goals..
- Limited collaborations.
- Minimal collaboration; substantial cost share by Quantum.
- Not presented. Quantum is very close to commercialization.
- No collaborations identified.
- None identified.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.50** for proposed future work.

- Not sure what they are, other than the targets.
- Need to elaborate more.
- Lowering the safety factor may be possible with embedded sensors, but must to done in context of real world cylinder issues: such as crash and impact damage (one time event) and long-term fatigue.
- Provided list of things to be accomplished but no details about approach or specificity of plans.
- Future work is focused on the most significant challenges, but timeline to overcome barriers (cost) not clear.
- This project looks more like engineering development than research.
- Focus on cost target needed.
- They need to show more information on the CoolFuel concept.
- No contingencies consideration.
- Focused in the right areas.
- Reasonable approach.
- Aggressive schedule for technology selection.
- Planned follow-on development work should provide a benchmark for pressurized H<sub>2</sub> storage.
- Rather a development plan than future research.
- Whose are new materials and new designs?
- What can be expected in the next 2-3 years?
- Too soon.
- Cover all aspects of.
- Very little specific detail in this regard.

### **Strengths and weaknesses**

#### Strengths

- Good project, in use in demonstration vehicles. Appears to be able to get to 2010 interim goals except for cost.
- Appears to be close to meeting technical targets.
- Have the technology.
- Achieved some targets.
- The only project on compressed hydrogen.
- Multiple strategies for achieving improvements.
- Good/balanced program; Quantum isn't trying to deceive anyone about what the prospects are. Only near-term technology that could enable the introduction of fuel cell vehicles into the marketplace.
- Solid company with a broad background.
- High tech composite company.

### Weaknesses

- It is commercially available now.
- Technology has reached its physical limits.
- Explain sources of data and hidden assumptions.
- Appears to be moderately mature relative to the rest of R&D efforts of DOE.
- Does not allow for much packaging and design flexibility. Looks more like development than research.
- Materials cost!
- Cost.
- Safety.
- Physical limits to gravimetric and volumetric densities.
- Status numbers do not appear to reflect the total system weight and volume.
- Everything seems to be proprietary.
- Did not include BOP in weight and volume claims.
- "Smart tanks" and "CoolFuel" not adequately described.
- The confusion surrounding the current status numbers is inexcusable. Quantum, of all the presenters, should understand the important difference between a system and a tank or a fuel. What's more, the volumetric density status is equal to that of 10000 psi hydrogen itself. This causes much skepticism of all of their claims. Also, the DOE status numbers that Milliken showed were all from Quantum, but Quantum's presentation had significantly different numbers. Again, this doesn't inspire confidence in their claims.

### Specific recommendations and additions or deletions to the work scope

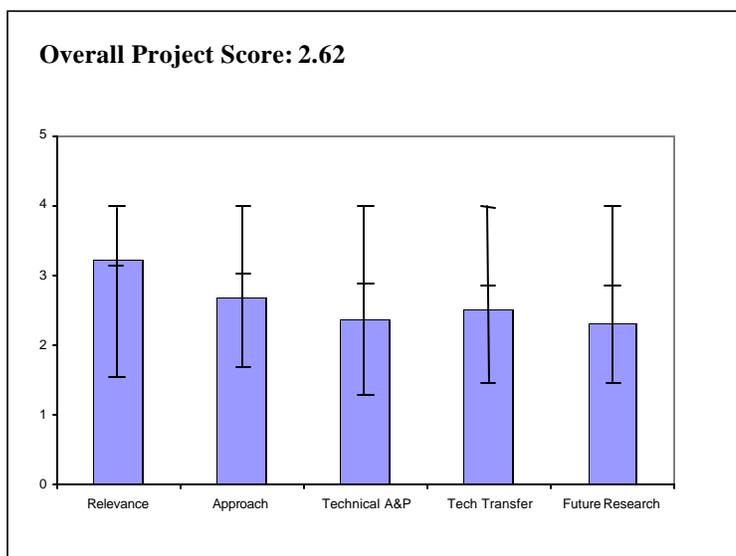
- Targets vs. status - should be evaluated on the basis of the system!!
- Could benefit from looking at manufacturing issues to ensure high volume reliability and consistency of technology.
- Work should focus on sensors also.
- PI needs to follow DOE guidelines for reporting status consistent with goals of program, e.g., costs at high volume, efficiency for entire systems (not just for tank) etc.
- Continue working on getting closer to the cost target. This is the best challenge for research.
- More information on the CoolFuel concept is needed to evaluate it.
- More work on safety is needed.
- Provide at least an estimate of the projected total storage system volume and weight per kg of H<sub>2</sub> at the next review.
- Consider pathways for solving the fiber supply issue and evaluate costs for large volume production of storage systems.
- Have safety review. Safety slide addressed their product - not their in-plant safety.
- More focus on 10K tank performance and cost reduction.
- Should develop target cost / strength /modulus properties for carbon fibers that would enable the 2005 and/or 2010 DOE targets. This information could be cascaded to other researchers exploring new processing routes to lower-cost fibers.

## Project # ST-3: Optimum Utilization of Available Space in a Vehicle through Conformable Hydrogen Tanks

Aceves, Salvador; Lawrence Livermore National Laboratory

### Brief Summary of Project

Lawrence Livermore National Laboratory's (LLNL) project goal is to develop conformable hydrogen tanks that will meet DOE targets. Conformable tanks have the potential to optimally utilize available space in a vehicle and thus greatly improve volumetric efficiency. Since conformable vessels are subjected to bending stresses, their approach entails two techniques (tank structures) for reducing these stresses, continuous fiber vessels and vessels made of replicants. This is a new project, started in 2004.



### Question 1: Relevance to overall DOE objectives

This project earned a score of **3.23** for its relevance to DOE objectives.

- Offers substantive approach to compressed or liquid H<sub>2</sub> storage.
- Compressed gaseous storage is the only short or mid term technology known today that will support the successful introduction of hydrogen powered vehicles.
- More research into compressed gaseous storage systems is needed to address remaining issues such as cost and volume.
- Project is very important for allowing the necessary flexibility in vehicle packaging and design.
- Conformable tanks provide better packaging options than non-conformable tanks.
- A conformable tank may not hold pressure as much as a non-conformable tank.
- Good alignment with needs of storage. Especially deals with the boil off issue. Also innovative method to change the cost and production methods.
- Important objective for real world application.
- Addresses multiple targets.
- Novel approach.
- Addressing an important issue; could lead to design concepts that reduce the volume of the H<sub>2</sub> storage system.
- The subject of transformable (conformable) high pressure tanks is very important.
- Worthwhile goals.
- Complimentary to room temp, compressed gas development.
- It is difficult to see how the claims of achieving 2010, and even 2015 density goals can possibly be realized.
- Compressed hydrogen will not be the answer to on-board storage.
- Don't think it will meet the targets.

## **Question 2: Approach to performing the research and development**

This project was rated **2.69** on its approach.

- Not sure how they could meet 2015 targets.
- Good analytical approach.
- Look forward to see how challenges of implementations are addressed.
- Novel approach to compressed gaseous (mostly) storage which addresses the issue of conformable tanks. Conformability is critical in vehicles for maximum packaging space efficiency.
- Evaluation of three approaches and down select to two is good. The project seems to be working to specifically address some of the technical barriers, but cost is a question and how to handle stresses on outside ribs of "bucking" design is not clear.
- Project desperately needs someone experienced in pressure container design.
- The timeline of the project is extended over 5 years. Most these are to do analysis. Building and testing is in 2009. I am not sure if this time is needed before testing.
- Little information was presented.
- The barriers were discussed as if they were solved.
- The novel theoretical work and the clever cryo-compressed work are both good approaches.
- Reasonable mix of theory and practice. Some consideration for manufacturability should be included.
- Should verify lap joint approach with composite producers if it hasn't been done already.
- Don't understand how much strain will be present at rib-skin joints.
- Need to do non-FEA modeling of buckling design. End effects will be significant.
- Lacking in thorough engineering detail, but the approach does project reasonable design ideas that accommodate shaping of the H<sub>2</sub> storage container.
- Leveraged with DARPA funds. Replicant approach could lead to breakthrough.
- Should be more focused --> "The project addresses most technical barriers."
- Focus on the subject of safe and transformable tank.
- Innovative.
- Low temperature compressed gas systems allow higher volume density to be achieved.
- Replicants approach still to be validated.
- Approach is certainly novel, but the motivation for this project should be more clearly spelled out. Seems like quite a few disjointed ideas.

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

This project was rated **2.38** based on accomplishments.

- Good theoretical work but far from real life application.
- Moving well against own timeline.
- Could benefit from an accelerated rate overall to be viable by the 2015-2020 timeframe.
- Conformable vessel analysis was promising and has allowed the PI to narrow the range of alternatives. However, no details about the results were presented, possibly because the project has just started.
- Need more work to confirm that there is an ability to meet the targets (as reported) with conformable tanks.
- Making good progress, but still haven't totally figured out the bonding issue for the ribbed approach or how to handle stress on outside ribs of "bucking" design is not clear.
- Results presented are misleading.
- The project is new. No accomplishments yet.

- Accomplishments have been slow over the last year.
- FEA results are interesting.
- Reasonable progress for limited time project has been funded.
- Project began in FY 2004.
- The results are interesting and they do make sense, but a prototype(s) needs to be built to prove feasibility of the concept.
- Very early in the overall program.
- Just a few results can be seen.
- Results do not really show something outstanding; rather textbook-knowledge.
- Just only the "bucking" system is a bit disappointing.
- Relatively new project.
- Did eliminate one option - good.
- All results shown are modeling and analysis. I would like to see experimental results as well.
- Project is new, but even still seems to have been very little progress.

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

This project was rated **2.50** for technology transfer and collaboration.

- Active association with others is minimal on this program even though they have collaborations with others on similar topics.
- Collaboration with pressure container designer(s) needed.
- Collaboration with pressure container fabricators needed.
- High level of collaboration, including OEMs (ACC), academia (several) and a prototype supplier, as well as other government activities.
- There was a slide on interaction and cooperation.
- Is a project with DARPA on carbon fibers to be considered cooperation?
- Potentially very important to realization.
- Good mix of collaborators.
- Might try to work with commercial polymer manufacturer or good polymer chemists. Don't know relative value of prototypers vs. resin manufacturers.
- Collaborations with Purdue, Univ. of Calif., and Spencer Composites.
- Not shown.
- One patent and two scientific publications are too few.
- Several organizations identified.
- Very good collaborations.
- Good connection with ACC and DARPA.

#### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.31** for proposed future work.

- Practical issues are not addressed.
- Modeling and analysis could benefit by including liner/permeation barriers in modeling before transition to build. Particularly with fatigue/pressure cycling.
- Moving in right direction. Could benefit from a transition to physical testing.
- Would like to see more detailed future plans.
- Future research plan should continue as stated but address source of issues mentioned above.
- Need to get on track with designs that might work.

- Need to get on track with designs that can be fabricated.
- Contingencies not considered.
- The plan does not seem to be impressive.
- Would like to see more actual testing to substantiate the theory and see it sooner in the plan.
- Future plans need to target the whole H<sub>2</sub> storage container and interfacing parts of the associated storage/delivery system.
- Analysis of concepts needs to be finished in '05.
- Covered FY 05.
- Needs more focus on experimental work.

### **Strengths and weaknesses**

#### **Strengths**

- Innovative concept using old technology.
- Work uses a more unique approach vs evolutionary improvement.
- Important concept to packaging the fuel container similar to today's gas tanks. OEM's should be quite interested.
- Good concept to explore.
- May be unique in working on conformable tanks.
- Novel and innovative approaches.
- Novel approach.
- Potential to increase the conformability index for pressurized storage tanks.
- Innovative ideas.
- Innovative.
- Important work.
- Project is high risk, high payoff.

#### **Weaknesses**

- What are the strengths of LLNL?
- Domes are not built because of any weakness but because of high cost, you haven't addressed the cost of your approach as such, it's a weak concept!
- Not clear if this concept could ever be affordable or would overcome physics issues.
- Apparent lack of pressure container design experience.
- Not eliminating impractical concepts.
- Not enough details- cost, plan.
- No partners to enhance or supplement DOE funding.
- Long range. Fabrication costs may be large.
- Need to integrate experimental and theoretical approaches.
- High risk - but worth trying.
- Not clear that potential barrier and failure point have been identified with pathways to address them as R&D moves forward.

### **Specific recommendations and additions or deletions to the work scope**

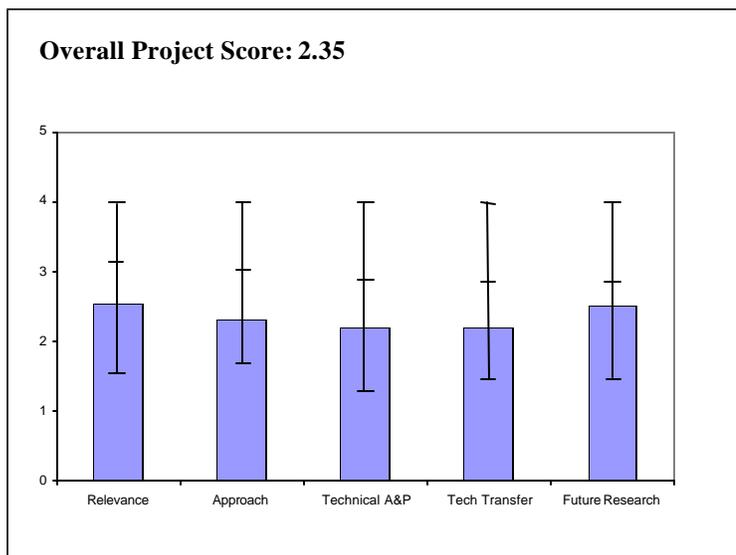
- Need to look at the cost issue.
- Should bring up the go/no-go decision to an earlier date.
- Budget information for the project life needs to be included.
- Need to show some progress in determining if these methods work in the real world, and need to show complete system capabilities of cryo systems by measurement.

- Need to present exactly what is included in the estimated weight and volume density.
- Need to demonstrate that the concept is feasible in prototype hardware.
- Thiokol and/or another tank manufacturer should be involved in building and testing prototypes.
- Clear targets and a go/ no-go milestone for the replicant "technology" should be considered.
- Pursue collaborations with other organizations (e.g. Quantum, Dynatech) to understand the needs more clearly.
- Have independent verification of his claim that the cryo-pressure concept will meet 2015 targets.
- Would like to see more actual testing to substantiate the theory and see it sooner in the plan.

**Project # ST-4: Radiolysis Process for the Regeneration of Sodium Borate to Sodium Borohydride**  
*Wilding, Bruce; Idaho National Engineering and Environmental Laboratory*

**Brief Summary of Project**

Idaho National Engineering and Environmental Laboratory's (INEEL) goal is to develop a viable method for regenerating sodium borohydride from sodium borate, to meet DOE's fuel cost target of \$1.50/kg H<sub>2</sub>. Specific objectives are to: demonstrate radiological methods of converting borate to borohydride; validate earlier observations and results showing borate conversion; initiate processes for identifying, qualifying and quantifying conversion mechanisms; and estimate production capability of the process.



**Question 1: Relevance to overall DOE objectives**

This project earned a score of **2.54** for its relevance to DOE objectives.

- This sort of program is needed if a BH<sub>4</sub> based system is to be useful.
- It is not clear if BH<sub>4</sub> systems are viable, but this method might make it possible.
- May have use in production of hydrogen.
- Uses a domestic energy source, presently under-utilized, toward hydrogen storage problem.
- Truly independent of fossil energy.
- Compressed gaseous storage is the only short to mid term technology known today that will support the successful introduction of hydrogen powered vehicles.
- More research into compressed gaseous storage systems is needed to address remaining issues such as cost and volume.
- The radioactive energy is not free, even if it came from waste.
- Sodium borohydride can't be regenerated directly from sodium borate.
- There are several processes that can be used. Which one is adopted here?
- I could not see the relation between generating sodium borohydride from sodium borate and hydrogen production mentioned in the presentation. The regeneration process consumes hydrogen.
- Little information is given.
- Any progress in hydride regeneration will be significant.
- Very speculative work - relevance is tentative at best.
- Willing to give this a chance to show relevance since it is a very new project.
- Not one of the better Storage projects in terms of relevance.
- There will be many roadblocks to coupling H<sub>2</sub> production/distribution with nuclear facilities of any kind.
- Important because of promising H<sub>2</sub> storage densities (on the material level).
- Unlikely to produce anything useful.
- Are people really going to want to rely on spent nuclear fuel to regenerate storage material?
- Approach is specific to borate regeneration. However, it is important to study this method.

## **Question 2: Approach to performing the research and development**

This project was rated **2.32** on its approach.

- Not sure if the barriers were explained.
- Early in project but it appears to have an interesting proposal/approach worthy of further evaluation/research.
- No evidence of the formation of borohydride via radiolysis.
- The project does not seem to be well designed.
- No integration to other research.
- No indication of technical or financial feasibility.
- How was the 53% conversion efficiency measured?
- The approach is not especially robust, but given the extremely low budget they may have little choice. The concept is interesting but they need to run more definitive experiments soon.
- Project is unique; INEEL uniquely qualified.
- Potential impact on chemical storage solution to hydrogen storage problem is immense.
- First priority should be verifying formation of borohydride.
- Need to form enough material to isolate and perform XRD or other unambiguous method.
- Lots of possible chemistry occurring. You're hitting the system with a huge hammer - lost of ways for things to shatter to produce hydrogen! Is boron catalyzing water decomposition? Are there other gas phase products?
- I am very skeptical about using such a high energy process to drive the reaction since all the chemicals (both products and reactants) get destroyed to some extent at these energies.
- Not obvious that the analytical methods are providing interpretable information. I'd like to see error bars on the production data.
- Approach is too broad. Lots of work already done on additives.
- Focus on radiation.
- Approach not clearly explained - new project.
- Don't understand experimental set-up.
- Very important to determine viability of this method.
- Need better analysis and measurement techniques.

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

This project was rated **2.19** based on accomplishments.

- May be too early but they have to improve their analytical techniques.
- Need to confirm that borohydride is produced and that conversion is possible.
- The project is new.
- Very hard to assess progress, especially given the small time they have had to do it. They certainly can make hydrogen.
- Calculation of throughput per reactor is valuable progress.
- Spectroscopic evidence of  $\text{BH}_4^-$  in solution huge step forward.
- First synthesis of  $\text{BH}_4^-$  without electrochemical basis.
- Must replicate the result. Reproducible regeneration would be outstanding.
- Too early to evaluate.
- Too early to hold this against the project since it is very new.
- The NMR data were poorly presented and not clearly described.

- What happens when sodium borohydride (wet or dry) is irradiated? Basic chemistry is not well understood.
- Even though the project is new there is more knowledge on sodium borohydride publicly available than INEEL indicates to know.
- Trouble finding borohydride and replicating earlier results.
- Barriers identified - new project.
- What are the options/alternatives?
- Promising results.
- Good progress for new project.
- Most important to determine whether borohydride is actually being formed (e.g., with XRD or other characterization tools).
- Also, need to be clear about forward/reverse reaction: in what conditions is the reaction expected to go each direction?
- Why use water as the solvent when this is known to promote the reaction to borate?

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

This project was rated **2.21** for technology transfer and collaboration.

- Need to find a good partner with strong analytical chemistry background.
- Collaboration with industry leader is good. Question whether collaboration with Idaho State University is adding any value.
- Little collaboration planned.
- Little opportunity for collaboration but they have paired with two logical partners, aligned with needs of program in  $BH_4$  recycling.
- Must strengthen collaborations. Some problem with measuring yield/detecting product likely solved by others in chemical hydride field.
- Pursue Millennium Cell collaboration as well as collaboration to verify  $NaBH_4$  formation.
- Making appropriate commercial contacts.
- Collaboration with Millennium Cell and Idaho St. Univ.
- The quality and depth of the chemistry being done on this project could be enhanced by a stronger analytical component.
- Only Millennium Cell and Idaho State University.
- Where are other valuable cooperation on chemical background knowledge?
- Collaboration with industry and university.
- Should couple more closely with other organizations to expand measurements and analysis.

#### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.50** for proposed future work.

- Proposed focus on aqueous media will not address the stabilization of borohydride issue.
- Correct factors to look at.
- All effort must be made to reproduce spectroscopic proof of  $BH_4$ .
- Identification of mechanism also important.
- No contingency if result is not reproduced.
- Supposedly it's to perform duplicates; test catalysts; study conversion mechanisms; explore other chemistries; test different radiation sources. More specifics on some of these planned directions would make it easier to judge the prospects for success.

- The proposed future work appears to be unfocused.
- Improving efficiency and yield, catalyst effect, conversion mechanisms.
- Future work is the majority of the project - especially impact of different mechanisms.
- Good focus on improved efficiency and yield.
- Good to expand to other materials.
- Not clear how catalyst would be involved.

### **Strengths and weaknesses**

#### **Strengths**

- Interesting approach that can have applications in other areas.
- Intrinsically inexpensive way of converting borate to borohydride.
- Uses essentially waste energy.
- Novel approach that may work for other materials.
- May have identified first non-electrochemical route to borohydride - a noteworthy accomplishment.
- Identification of direct conversion of nuclear energy to hydrogen storage solution critical step in hydrogen economy.
- Novel approach.
- May have some value in use of reactor 'waste'.
- Reactor types - use of waste.
- Very unique approach.
- Unique capability.
- Approach offers alternative to chemical and electrochemical process.

#### **Weaknesses**

- Limited resources to do the right analytical chemistry.
- Unknown cost or commercialization viability.
- Intriguing proposal, but can it deliver? Need to confirm generation of borohydride and if so, then need to confirm repeatability in number of cycles that might be seen in use in vehicles.
- Use of aqueous solutions is a barrier towards stability of borohydride.
- Poorly equipped to analyze results.
- At least 3 ways  $\text{BH}_4^-$  could be destroyed.
- Collaboration may increase rate of research.
- No obvious contingency plans.
- Not broadly applicable to hydrogen economy. Only successful if borohydride specifically is proven out.
- Chemical selectivity using the proposed radiation approach may be difficult. That's why incontrovertible proof of  $\text{BH}_4^-$  formation is crucial.
- EERE should raise the expectation bar with respect to the quality of the science coming from this project.
- Regenerating spent borate into borohydride requires the addition of hydrogen.
- It appears that the hydrogen comes from the radiolysis of water.
- Funding level not addressed.
- Poor presentation. Little information. The presentation was difficult to follow.
- Did not show which chemical and nuclear reactions produce borohydride.
- Did not show cross section and mean free paths.
- Experimental setup unclear.
- Chemical reactions involved need to be mentioned.

- There is a fundamental gap on knowledge about sodium borohydride.
- Seems like a real long shot.

### **Specific recommendations and additions or deletions to the work scope**

- This project depends on sodium borohydride being a storage alternative. It should be therefore directed to work with other projects that work in this area.
- Should be subject to an early go/no-go decision.
- Re-direct or terminate the project.
- Research should focus on the objective.
- Work on getting the equipment they need to analyze output solution.
- Focus on understanding the chemistry.
- I concur that stopping back reaction is key - but as there are three ways this may happen (radiation makes excited  $\text{BH}_4$  that reacts faster than ground state, second photon breaks  $\text{BH}_4$  created, radiation converts water to highly reactive species that react with any existing  $\text{BH}_4$ ) this will be challenging. Identifying sodium borohydride is crucial.
- If this is not a vital way to recycle  $\text{BH}_4$  at low cost, then consider trading it to the production team because they clearly make hydrogen, seemingly very effectively.
- Broaden scope of project to other solvent/hydride systems. All hydrides are expensive, but can often be interchanged. Example: A cheap synthesis of  $\text{LiH}$  leads to cheap  $\text{MgH}_2$  and  $\text{NaBH}_4$ . Thus, all chemical hydride technologies would benefit from success of this program.
- Need clear statement of what is being attempted; what general mechanism is being used; and some model to predict costs/efficiency.
- Project needs serious go/no-go milestones.
- Strengthen the physical and analytical chemistry aspects to include a broader range of measurement tools to sort out the mechanism.
- Molecular spectroscopy might be useful.
- Run borohydride solution as a blank to determine the effect of radiolysis on the borohydride.
- Might fit better under production and delivery rather than storage.
- Achieve more know-how on sodium-borohydride (solubility and other topics).
- The project title indicates regeneration of sodium borohydride, the presentation is about the  $\text{H}_2$  release.
- Bring this work closer to the Center of Excellence and clearly focus.
- Must strengthen collaborations. Some problem with measuring yield/detecting product likely solved by others in chemical hydride field.

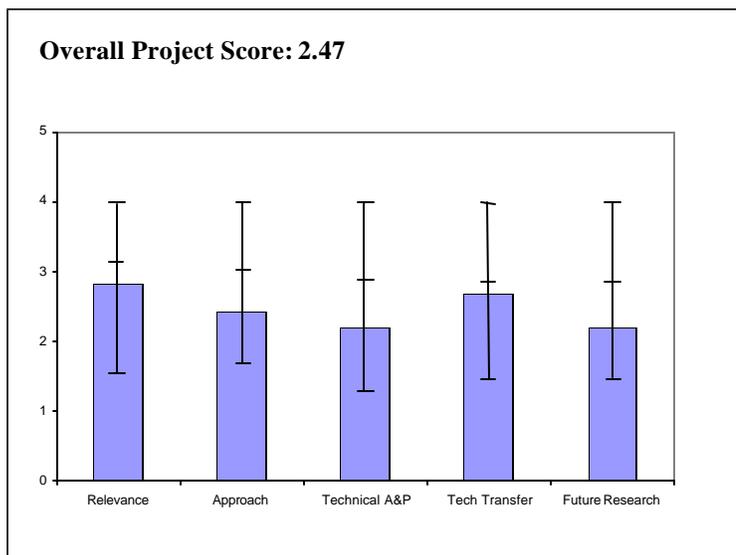
## Project # ST-5: Low Cost, Off-Board Regeneration of Sodium Borohydride

Wu, Ying; Millennium Cell, Inc.

### Brief Summary of Project

Millennium Cell's project is focused on the development of a reliable regeneration process for sodium borate ( $\text{NaBO}_2$ ) to sodium borohydride ( $\text{NaBH}_4$ ) that meets DOE cost targets. The technical approach is to identify electrolytic processes that reduce cost, focusing on direct borate reduction and high efficiency sodium reduction. A key tool is to use hydrogen gas to reduce cell voltage and improve regeneration efficiency. **This is a new project, started in 2004.**

### Question 1: Relevance to overall DOE objectives



This project earned a score of **2.83** for its relevance to DOE objectives.

- Chemical storage is one of several possible storage technologies that will meet the targets necessary for the successful introduction of hydrogen powered vehicles.
- No prior knowledge if electrolysis can be used to regenerate sodium borohydride from sodium borate.
- How to stop the spontaneous back reaction?
- Information on the kinetics of the reaction, thermodynamics?
- The only barrier they can overcome, if successful, is the cost barrier.
- This project does not address other targets.
- Potentially could meet 2010 goal; however I would note the recycle proposed seems very unlikely.
- Regeneration is crucial for metal hydrides.
- Why plan on 7.5 kg stored  $\text{H}_2$  in slide 5?
- $\text{NaBH}_4$  seems to be a very questionable solution to  $\text{H}_2$  for cars.
- Unclear whether a sodium borohydride-based storage system can meet DOE's 2015 weight and volume goals for on-board  $\text{H}_2$  storage and delivery "systems."
- Chemical borohydrides promise decent storage efficiency (on the material level).
- Unlikely to be successful- overall energy efficiency likely to be poor.
- Regeneration of borate critical to use if borohydride for storage.
- Unlikely to meet cost goals due to life-cycle analysis.
- Projections about gravimetric densities for future systems seem overly optimistic.

### Question 2: Approach to performing the research and development

This project was rated **2.42** on its approach.

- They understand/explain the barriers well.
- Major barrier is the cost of electrolysis.
- Energy efficiency is an issue.

- The cost forecast in slide 4 is not realistic at all.
- Material safety is not addressed.
- System density is optimistic.
- The electrolysis approach is not very likely to be energy efficient.
- While reducing the number of steps is a good idea, the use of electrochemistry as a tool seems unlikely to be efficient.
- Typically electrochemistry is powerful but inefficient.
- Electricity is generally an expensive "reagent" for fuel production due to generation efficiency. H<sub>2</sub> assisted voltages are interesting. Have they all been verified experimentally? Are all chemical paths verified? Is energy to melt NaOH included in energy cost? What will capital costs be for operations involving corrosive materials?
- Need to look at process designs and preliminary energy and costs.
- Electrolytic approaches to chemical processing are generally inefficient. I'd like to see some efficiency predictions for this concept.
- Clearly formulated approach.
- There is no way to achieve \$1.50/gallon gasoline equivalent with sodium borohydride by 2010.
- Cost roadmap is very questionable.
- Very questionable that electrolysis leads to more efficient Na - production.
- Electrolytic approach has potential for reducing energy costs of regeneration.
- Unclear on cost of H<sub>2</sub> used in process.

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

This project was rated **2.19** based on accomplishments.

- Too early in project to be able to assess if project has potential to be successful.
- Evaluation of status vs. targets - system basis -- very good!
- Still energy efficiency/cost is a challenge.
- A new project.
- Have reduced voltage by 4. However they do not understand the actual energy efficiency including hydrogen consumption.
- Too early to evaluate.
- Early in project.
- What voltage gives a sufficient reduction current density?
- How many hydrogen energy equivalents go into the regeneration process?
- Too early in the program.
- Recently started.
- New project but some electrolytic results.
- New project; no progress yet.

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

This project was rated **2.68** for technology transfer and collaboration.

- Reasonable variety of collaborators in industry, research and academia.
- Superb collaboration team.
- Project is not critical but off board regeneration to enable the potential of chemical hydrides is important to the plan.
- May need additional consultants outside Princeton community.

- Good start - appropriate for this stage of project.
- Collaboration with Air Products, INEEL, Princeton Univ., and Ionotec.
- The cooperation partners are not well-linked into the research.
- Air Products.
- Would be desirable to have a major NaBH<sub>4</sub> supplier.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.21** for proposed future work.

- Do life-cycle energy calculations . . . it's not that difficult.
- Use the most optimistic assumption and tell us what the efficiency is.
- "Tweaking" the electrolytic regeneration would not address the energy efficiency cost issues.
- It looks as if few electrolysis paths will be evaluated.
- The one step reaction is not very likely.
- The plan to improve Na production more probable to work technically, though as noted, economic success is less likely.
- Reasonable approach.
- Don't understand "Rule out false positives" slide 14.
- Very strong statement for electrochemical research.
- A project like this could go on forever. We definitely need a go/no-go timeline here.
- Too unfocused and unspecific.
- Too early.
- Score based on effort towards regeneration. Should include work on onboard system density as well.

### **Strengths and weaknesses**

#### Strengths

- Consideration of actual gravimetric capacity is based on entire storage system.
- Millennium Cell has a strong background in sodium borohydride for vehicle operation.
- Strong on use of output of the program.
- Some improvement in Na processing is likely.
- New approaches to difficult problem of borohydride regeneration.
- Team is very familiar with basic chemicals being studied.
- Know NaBH<sub>4</sub>.

#### Weaknesses

- Uphill thermodynamic barriers.
- Unlikely to achieve cost targets.
- No discussion of onboard problems (precipitation, corrosion).
- Focus on electrolytic regeneration.
- Not sure if they have enough resources to perform sodium borohydride regeneration.
- Highly speculative one step process efficiency.
- The company is too small to accomplish the required fundamental research (i.e. find a better way to generate Na metal - other firms have worked on this for many years).
- Not a big chemical process company.
- Storage efficiency is still an issue.
- Use of average system weight not good. Should use highest weight/volume condition.

- Electrochemical efficiency evaluation should have been done before project started.

**Specific recommendations and additions or deletions to the work scope**

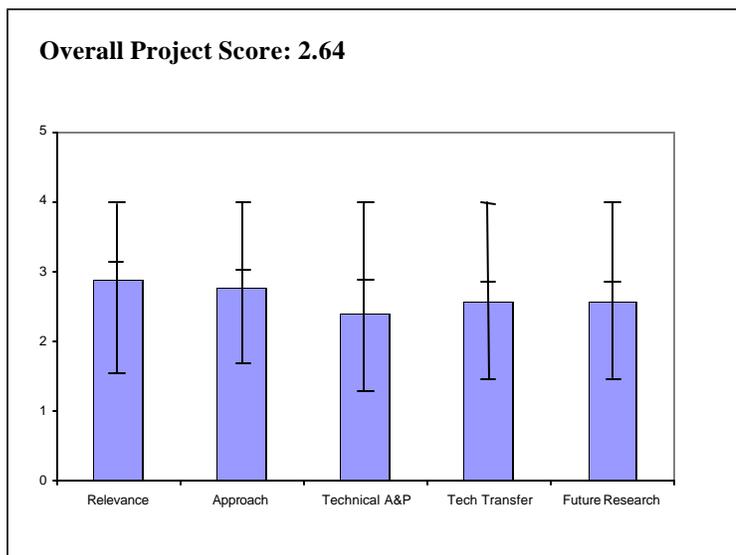
- At last review meeting, PI was asked to provide specific estimates on overall energy efficiency of regeneration cycle: theoretical and measured - none provided. Must provide estimates at next review.
- This program should be subject to closer review.
- Push the go/no go decision forward.
- May need to consider changing the direction from electrolytic regeneration of borate to borohydride to using a completely different approach for the regeneration.
- They need to have partners whose core business is in NaBH<sub>4</sub> regeneration from sodium borate.
- Perform experiments that might disprove the one step process early so resources are not wasted if this is indeed an unlikely path.
- Figure out the energy cost of hydrogen in the Na process so you know exactly where you have to lower the voltage to equal the existing process.
- Need a project plan with milestones.
- Need to demonstrate direct reduction in one step.
- Need to develop an accurate estimate of the full fuel cycle efficiency and cost.
- Need to include the energy contained in the H<sub>2</sub> at the anode in the efficiency calculations.
- Might fit better under production and delivery rather than storage.
- Consider realigning this program with the work executed at the Chemical Hydride Center of Excellence.
- Needs overall energy balance - need to include energy required to make input H<sub>2</sub> and electricity.

## Project # ST-6: Chemical Hydride Slurry for Hydrogen Production and Storage

McClaine, Andy; Safe Hydrogen, LLC

### Brief Summary of Project

The objective of Safe Hydrogen's project is to demonstrate the viability of magnesium hydride slurry as a cost effective, safe, and high-density hydrogen storage medium. The pumpable, high density slurry offers infrastructure advantages, and high system energy density with high vehicle range. The focus of the project will be on regeneration of the spent slurry and conversion of magnesium hydroxide to magnesium hydride to meet DOE cost targets for off-board regeneration. Work will also be done on mixer, slurry, and system development to meet DOE capacity targets. **This is a new project, started in 2004.**



### Question 1: Relevance to overall DOE objectives

This project earned a score of **2.89** for its relevance to DOE objectives.

- Chemical storage is one of several possible storage technologies that will meet the targets necessary for the successful introduction of hydrogen powered vehicles.
- An attempt to develop a storage material and system.
- Concept looks good and has advantages.
- Preliminary density information not provided.
- Efficiency of the processes needs to be described in a better way.
- The project sounds good and worth pursuing.
- Cost estimates could be optimistic.
- While a safe system is useful to the plan, this material has almost no chance of making the target for 2010 and so its relevance is limited.
- Reasonable applicability to chemical hydride goals.
- Key is regeneration costs.
- Very relevant - success in the project would be a big step to get to the President's hydrogen economy objective.
- Safe Hydrogen projects that the slurry can make cost targets, but is this based on total system volume and weight, including the companion water system?
- Clear presentation of the estimated total system weight and volume is needed to determine whether the technology can meet the DOE targets.
- May turn out better than Na.
- Not clear if method will reach any of the DOE targets.
- Should not quote these "eye-popping" numbers for "slurry" density. This is misleading; at the least should always include water with these numbers, and at the most should include the entire system.

### Question 2: Approach to performing the research and development

This project was rated **2.78** on its approach.

- Intriguing approach but concerns about magnesium hydride slurry being viable from a cost perspective and from feasibility of coating to preclude exposure to water.
- Ought to continue to determine if there may indeed be a way to make the system viable.
- The project is designed reasonably.
- Some of the technical barriers are addressed.
- No integration with other research.
- Reasonable timeline and milestones.
- In general the process followed is good: they are looking at the function on the vehicle and recycle. Only the end product efficiency is not appropriate.
- Would like to see details on economic estimates for H<sub>2</sub> costs.
- Preliminary estimates are rarely too conservative.
- Project is well-focused and relies on a significant data base of known chemistry.
- What volume and weight percent of the slurry is oil?
- Cost share by Safe Hydrogen is about 30%.
- On-board system has been demonstrated; off-board regeneration is the focus of the effort.
- Carbothermic process could use coal as the energy for regeneration.
- High energy costs of regeneration. Proposed process still too high.

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

This project was rated **2.40** based on accomplishments.

- New project. Too early to assess properly.
- New project.
- Have not started the program so hard to appraise.
- Too early to evaluate.
- Early in project but it appears well-organized and prepared to work on barriers.
- Just getting started. It will be interesting to see what they accomplish by next year's review.
- Just started.
- New project.
- New project; no progress yet.

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

This project was rated **2.56** for technology transfer and collaboration.

- Limited collaborations, per se. Lists various activities that will be doing testing or evaluation.
- Coordinated with logical partners.
- Given that the parent process to use the slurry is unlikely to meet consumer energy density requirements, it is hard to say this is essential to the plan.
- Should partner with commercial Mg producers. They must be looking at SOM process as well.
- Fairly adequate for this stage of work.
- There are four strategically selected collaborators that reinforce the project in crucial areas.
- Good team.
- Add large scale Mg supplier as consultant.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.56** for proposed future work.

- Would like to see more specificity and details of future plans for research and how challenges will be met such as: oil coating the material, retention of the coating through the life of the system, etc.
- No consideration for contingencies.
- Reasonable timeline and milestones.
- Research plan is suitable for goals.
- Carbothermic reduction would be great. Could you share literature on earlier use with Tech Team? Why was it abandoned?
- Makes pretty good sense.
- The future plan is very general. It just shows that they plan to cover all the bases with minimal detail about how that will happen.
- Just started.
- Critical issue is regeneration. Not enough emphasis on this.

### **Strengths and weaknesses**

#### **Strengths**

- A good concept.
- Using a fluid fuel is attractive.
- Significant experience with end product and needs.
- Interesting approach.
- Relies on relatively well-known chemistry.
- Builds on previous DOE work that demonstrated the feasibility of the on-board H<sub>2</sub> generation process with LiH-oil slurry.
- Mg should be a more tractable system.
- Good team.
- Turned in safety 1-pager.
- Generally good presentation.

#### **Weaknesses**

- Question whether the system will be able to meet the goals for the DOE program despite the claims of the PI. Will need to see data.
- The main issue for this approach is the need for liquid water as a second fuel. This makes it more difficult to achieve the density targets.
- Need a waste tank on board.
- Thermodynamics and kinetics are not addressed.
- Little experience in the recycle chemistry business.
- Parent system to use slurry unlikely to go on a vehicle.
- As with all chemical hydride systems, regeneration costs will be the critical value.
- Didn't get the feeling that this project has a whole lot of science attached to it.
- Don't see how you can get to any of the density targets onboard or meet cost targets for system or for fuel.

### **Specific recommendations and additions or deletions to the work scope**

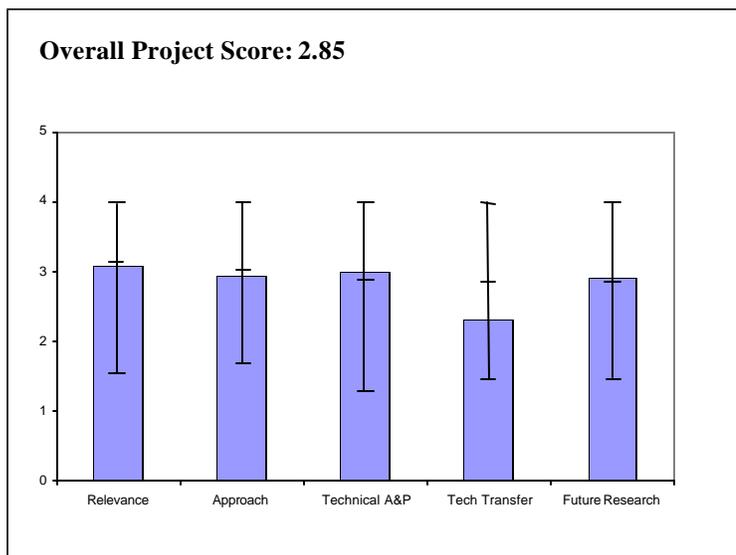
- Need to make a list of system components and materials along with their masses and volumes.

- Need to look at the usage system early on. If this cannot work, then not much point in going further.
- Add some meaningful milestones to projects timeline slide 9.
- A full systems analysis is needed here that includes all the parts of the fuel storage and delivery infrastructure for a passenger vehicle.
- Consider partnering with commercial Mg producers. They must be looking at SOM process as well.

**Project # ST-7: Hydrogen Storage by the Reversible Hydrogenation of Liquid and Solid Substrates**  
*Pez, Guido / Cooper, Alan; Air Products and Chemicals, Inc.*

**Brief Summary of Project**

The overall project objectives are to develop carbon-based solid and liquid hydrogen storage materials with material-based capacities of >6 wt% and >45 g H<sub>2</sub>/L and a hydrogen storage system prototype with 6 wt% and 45 g H<sub>2</sub>/L capacity in the range of -40 to 90-120C and less than 1000 psi H<sub>2</sub> pressure. Primary technical objectives are the discovery of novel materials using experimental and computational methods and demonstration of reversible hydrogen storage with liquid substrates. **This is a new project, started in 2004.**



**Question 1: Relevance to overall DOE objectives**

This project earned a score of **3.09** for its relevance to DOE objectives.

- Chemical storage is one of several possible storage technologies that will meet the targets necessary for the successful introduction of hydrogen powered vehicles.
- Materials with more than 6% are very valuable to meeting plan goals.
- Promising approach to utilize existing infrastructure. Could minimize transition costs.
- The only storage project that I would call critical to Initiative.
- Very relevant and being carried out by a company that understands the business of bulk chemicals.
- Difficult to evaluate all merits since the liquids are not disclosed.
- This is still a material discovery system.
- Material and system targets are the same (slide 2).
- Confusing (claiming they have the material and at the same time saying they will work to select it).
- Liquid fuel has advantages.
- Still need a waste tank.
- Ambitious about many things.
- Is it that easy to recover the regeneration heat and also to use the fuel cell heat to release hydrogen?
- Little information was provided.
- Almost no data.
- Material safety?
- At best, this project (if fully successful with known LQ\* materials) will only meet half of the 2015 targets; also it is probably true that a full system approach to figuring out the actual storage values will leave LQ\* H<sub>2</sub> at a level well under half of the 2015 target.
- 6-7 wt% good for 2010 target.
- Liquid-based system at low pressure should not add too much.
- Interesting concept; unfortunately, without knowing more, it is difficult to accurately assess the relevance of the work.

- Assuming that I can guess the types of liquids that are being considered, it seems unlikely that they will ever achieve more 6 wt% or so. (In a system, this certainly would not satisfy the 2010 goal.)
- What about volumetric densities of these fluids?

### **Question 2: Approach to performing the research and development**

This project was rated **2.95** on its approach.

- Appears to have a unique approach to a total hydrogen fuel delivery system. It will be interesting to see if it proves out.
- Potentially low cost and safe media for H<sub>2</sub>-storage.
- Good mix of experiment and computational approaches. Use of exothermic hydriding step gives a real advantage over other materials. How big is the synthetic challenge?
- Only chemical hydride project that avoids huge regeneration costs in both \$ and energy.
- Talk was slightly compromised by company protection of proprietary data but approach came through as well planned.
- Not sure what research objectives are.
- Not sure how it meets 2010 or 2015 targets . . . need component list.
- No information on cost.
- Can utilize commercially available hydrogenation process.
- No evidence of technical feasibility.
- Secret materials claims not supported.
- Barriers addressed vaguely.
- They understand the needs, but they are not very clear about exactly what they intend to do.
- Hopefully the confidence they have in their predictive methods are valid in this instance, for the moment it is hard to rate them.
- I'd suggest more emphasis on the search for the best possible LQ\*. None of the ones we know about are good enough. Hopefully, we'll hear a success story about the new "bent bond" material at the next review.
- Analytical capability is impressive.
- Well focused but includes solids as well. Best success possible with liquid case.
- Again, without knowing more, cannot really assess.

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

This project was rated **3.00** based on accomplishments.

- Considering short period, showed good progress.
- Although early in the project, early results seem promising.
- New project (results not expected to date).
- Good upfront estimates of potential performance limits and material/process costs.
- New project.
- Described material has moderately good properties, which is good progress in a short time.
- Clearly the material needs to be brought to a lower release temperature.
- Very good progress for a new project.
- Early in project but a number of chemicals which might work have been identified.
- Timelines have real decision points!
- Too early to tell if substantive progress towards a practical system is being made in this project.
- Recently started.

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

This project was rated **2.32** for technology transfer and collaboration.

- Need to bring in a catalyst company.
- Collaborations and technology transfers seem limited.
- Appear to be mostly preliminary discussions with no substantive commitment for support. But it is early in the program.
- The collaboration slide show limited contacts.
- Could be important if the materials are cheap and work at low temperature.
- The collaboration team is largely predicted, may be due to the fact they are not started, but one would hope they have this wrapped up by now.
- Reasonable collaborations for project involving proprietary material.
- Tech transfer is fully adequate for their stage.
- A strategically chosen set of collaborators, including fuel cell manufacturers, SWRI, and the C-H Storage Materials Working Group.
- Just started. Limited so far.
- Could use more interactions with other chemist and engineering groups.

#### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.91** for proposed future work.

- Need to move quickly and efficiently through the downselect process in the first year to zero in on the most viable technical approach(es).
- Project is vague.
- No contingencies considered.
- Rating based on timeline, because we have no idea if they are really approaching the problems on a chemical level in a systematic and optimized way.
- Good approach.
- Very exciting project.
- Company knows what it is doing.
- The future work summary was too vague.
- Air Products needs to be forthcoming about the specific kinds of LQ\*s and catalysts they are looking at.
- Just started.
- Productive modeling to lead experimental efforts.

#### **Strengths and weaknesses**

##### Strengths

- Potentially low infrastructure cost but we don't know what it is.
- Safety.
- New material discovery.
- Potentially low cost and safe material for storage.
- Possibility of use of existing infrastructure for refueling.
- Possibility of regeneration (hydrogenation) using existing commercial facilities.
- Having a liquid fuel is desirable.

- Experienced team with good tools.
- Appear to have a line on a material.
- Chemical hydride project that uses existing infrastructure with minimal changes.
- Overcomes recycling dilemma that plagues other hydride systems.
- Outstanding- company knows what it is doing.
- By far the best of the Storage projects.
- Successful completion would result in a useful product that could be put into use at a reasonable cost and is likely to be accepted by the public.
- A 30% cost share by Air Products shows some commitment.
- Competence of Air Products.

#### Weaknesses

- Unsubstantiated claims on efficiencies - why can't you estimate them?
- Unclear on net cost of H<sub>2</sub>/kg and assumptions
- Difficult to meet 2015 target as this approach is at near H<sub>2</sub> capacity of petroleum-based materials.
- Liquid not disclosed.
- Generation of waste streams.
- Secret material. If they discovered the material or the process, they can file a patent which would protect their rights.
- Little data and information provided.
- Not clear if they found the material or they are still searching.
- Materials are not known so safety of the material(s) may be an issue that needs to be assessed.
- Waste tank is needed (this waste seems to be solid which would not be easy to remove.)
- Does not seem to suitably use the \$1M per year.
- Possible synthetic challenges.
- I'll believe the cost projections when I see performance given in terms of "net" energy per unit of system weight and volume.
- Very expensive project. Presentation does not adequately describe what DOE gets for the cost.
- Heat available from the fuel cell is limited (T=80 degrees C). Even if this can be utilized, this will result in condensing the water in the exhaust stream, which can be an issue.

#### Specific recommendations and additions or deletions to the work scope

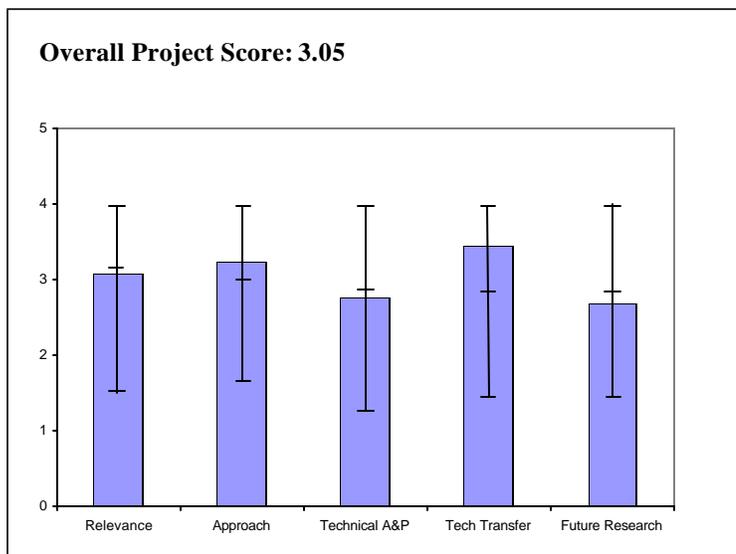
- Continue with the plan.
- Should have different density and cost targets for the material and for the system.
- Should give detailed account to DOE team so they can evaluate the chances better than we can.
- Need to consider the purity of H<sub>2</sub> out of the system.
- Deselect to one or two systems after ~ 1 year.
- I'd suggest more emphasis on the search for the best possible LQ\*. None of the ones we know about are good enough. Hopefully, we'll hear a success story about the new "bent bond" material at the next review.
- Need to move quickly and efficiently through the downselect process in the first year to zero in on the most viable technical approach(es).
- Provide estimates on efficiency.

## Project # ST-8: Doped Sodium Alanates: Fundamental Studies and Development of Related Hydrogen Storage Materials

Jensen, Craig; University of Hawaii, Manoa

### Brief Summary of Project

The University of Hawaii is developing catalytically enhanced hydrogen storage materials capable of being used in vehicular applications. The objectives of the research are to determine the chemical nature and mechanism of the species that is responsible for the enhanced kinetics of Ti-doped NaAlH<sub>4</sub> and to apply the insights gained from fundamental studies of Ti-doped NaAlH<sub>4</sub> to the design and synthesis of hydrogen storage materials that will meet DOE hydrogen storage system targets.



### Question 1: Relevance to overall DOE objectives

This project earned a score of **3.08** for its relevance to DOE objectives.

- The use of reversible hydrides is important to the Hydrogen Fuel Initiative.
- Metal hydrides are one of several high potential storage technologies that would support/enable a hydrogen transportation system.
- The PI has unique knowledge and experience in this area.
- The PI's claim of a new material with 7% wt density is not supported with data.
- System targets are not addressed. All research and numbers are material based.
- I believe it will be difficult to reach the DOE targets with these materials (alanates).
- Material safety (except lab safety) is not addressed.
- This work is aimed straight at a major need in the program.
- Good progress in addressing important issues to realize the goals of Hydrogen Program.
- Innovative and thorough.
- Relevant to alanates and other potential complex hydrides.
- The focal point for this research is another system that has no credible chance of meeting appropriate 2015 target for weight and volume.
- The justification to continue this work is to develop an understanding of a "model" system that could be applied to different materials.
- Contributes very well to the basic understanding of NaAlH<sub>4</sub>, the state-of-the-art H<sub>2</sub>-storage hydride.
- Some chance of 6 wt% - not 9 wt%.
- Work could lead to higher capacity materials.
- Pure sodium alanate unable (or at least unlikely) to achieve 2010 goals; thus, improvements on this material are key.
- Unlocking key to the dopant material in NaAlH<sub>4</sub> may give clues about making other materials reversible.

## **Question 2: Approach to performing the research and development**

This project was rated **3.23** on its approach.

- The work is innovative.
- Progress is being made.
- Failure to address specific barriers related to the use of sodium alanate with respect to 2010 or 2015 targets.
- Approach appears to be a bit scattered. The results are confusing and interpretation of the data a stretch.
- PI seems to take great care in trying to understand and to explain the results of the analysis and to leave no stone unturned in identifying and evaluating alternative compounds.
- Too strong a focus on characterization of materials that do not meet targets (e.g. 2010 or 2015).
- A significant number of studies on doped alanates have shown little promise to date.
- Technical barriers were addressed vaguely.
- Technical feasibility is not guaranteed.
- Did not address the cost of alanates (doped and undoped).
- Safety of these materials will be a major concern even if other obstacles were overcome.
- Using many techniques to evaluate the material.
- Asking appropriate questions and getting answers experimentally. Not much PI could improve on here.
- Excellent example of a program developed to address the programmatic goals using the best available techniques and outreach with collaborators that can bring the experience needed to get at the core issues.
- Mechanistic information important to understanding and developing other complex hydrides. Group has developed good understanding of this system as well as improving performance.
- Seems that the state of mechanistic understanding is actually more uncertain than presented last year (May 2003). That is not a negative, but points to the complexity of the material system and the need for rethinking and new experiments.
- Leaves few stones unturned in the choosing of measurement and characterization techniques.
- The detailed battery of experimental techniques employed is highly commended.
- Going for fundamental understanding.
- Great diagnostic instrumentation.
- Application of wide array of analytical methods to understand mechanisms in material. Good capability for synthesis and material treatment.

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

This project was rated **2.77** based on accomplishments.

- It is hard to determine new work from previous work or literature data.
- Lots of data, but very confusing results and interpretation. Need to get all data/results gathered and formulate consistent theory and test theory.
- PI is learning more about the chemistry and physics of metallic lattices and storage materials but seems to be making slow progress toward a technical solution that can meet the DOE storage targets.
- I did not understand why a patent on a model is filed.
- It seems that it is not known which model is more accurate to represent the doping chemistry. What is the plan to resolve this issue?
- Some results were not reproducible. Need to know why.

- Answers to some reviewer questions were not solid, such as "it likes to be there" in response to one.
- The preliminary 7 wt% storage capacity with the new discovery needs to be supported by evidence.
- Why are different measurement methods needed?
- Creation of the Ti-substituted material is a major advance IF it has good properties. The method alone is of value; though we cannot evaluate that as it was not discussed.
- Diagnostic results are not as definitive as might be wished; hard to tell for certain what is happening to the titanium as far as its true location or its actual molecular function. Still, a lot of new results that may someday help answer these questions.
- Progress is clearly being made; but it is unclear whether key barriers will be overcome, as in all storage programs due to the level of technical achievements needed.
- Good work. A little disappointing that mechanism is not clearer, but that is the way nature works sometimes.
- Lots of data but no convincing answer to the "Ti" question.
- The project appears to be a "good" return on investment.
- Tying the "mechanistic" studies and various experimental data together has been a little slow.
- After almost 4 years, project is barely into second objective "development of related hydrogen storage materials."
- Gained large amount of information from wide array of experiments. Initial results on "7 wt%" material promising.

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

This project was rated **3.46** for technology transfer and collaboration.

- Lack of a partner with practical/end-use experience.
- Work with international and lab partners shows collaboration, but no tech transfer. Collaboration appears to focus on data, not on interpretation.
- The PI seems to be doing extensive collaboration and exchange of ideas to try to confirm or to better understand his experimental results.
- There is wide cooperation and technical publications.
- Well aligned and well connected.
- The outreach has been excellent and this PI is a highly sought after collaborator.
- Great utilization of resources to attack problems.
- Replete with collaborations - too numerous to list. That is the most positive aspect of this project.
- A highly detailed and valuable spectrum of international scientific collaborations. Transfer of information and practical implications to industry not quite as clear.
- Lots of high quality collaborators.
- Effective interaction with collaborators has resulted in good data and a number of publications.

#### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.69** for proposed future work.

- As the speaker stated, it appears that this technology has reached its end.
- Unsure of continuation. Need to analyze where project stands and make a go/no-go decision now.
- Future direction needs to be clarified further. What other options are available if the model fails?
- Correct research to be doing.
- Have very specific plans for the next 2 years.

- The future focus should be on systems that do have a credible chance of meeting appropriate 2015 targets for weight and volume.
- Some hint of a better performing material but no details – “trust me” approach.
- Need to complete mechanistic understandings and move into new materials development. Seems to be understood.
- Not very detailed description beyond FY-04.
- Much research has focused on applying a wide variety of experimental techniques to determining the role of Ti. The evidence is contradictory, but the bulk vs. surface role of Ti would be immediately settled if the higher-density brine-free material could be synthesized and storage measured. This would also be working toward the DOE goals. Therefore, synthesizing and testing these higher-density materials (based on the hypothesized bulk substitution) should be the absolute top priority for this project. This definitely should be sorted out before next year's meeting.

### **Strengths and weaknesses**

#### **Strengths**

- Lots of data and empirical observations.
- Research is world class and well respected.
- Good experimental and testing techniques.
- Significant knowledge on doped alanates.
- Unique knowledge in this area.
- Well conceived. Innovative. Focused.
- This project has been and continues to be at the forefront of important scientific questions that get at the heart of the technical issues that need to be addressed for the hydrogen storage problem to be resolved. The value of this project per dollar invested is extremely high.
- The presentation included technical aspects and technical questions that got to the core barriers that need to be addressed for this problem to be solved.
- Good work to attack mechanism and go after improved materials.
- Collaboration structure is a strength.
- Excellent scientific base and collaborations.
- Diagnostics excellent.

#### **Weaknesses**

- Failure of the material. Poor on application or storage device development.
- Low credibility: the speaker should not use the forum to propagate unsubstantiated claims.
- Interpretation of data is confusing. May need to rethink research plan to arrive at conclusive results.
- The PI tantalizes us with statement that he may have a storage media with 7 wt% capacity but does not provide enough detail or information to understand and to confirm or refute the results. We need to find a way around any confidentiality or proprietary issues to allow the results to be confirmed or excluded.
- The doped alanates will not meet the capacity and storage targets. Storage capacity presented for material (not for storage system).
- Results of proprietary methods were not verified. Non-supported claim of a new material with 7 wt% storage capacity.
- No system-based numbers. Safety and cost are not considered.
- Substitution model not proven.
- The presentation was difficult to follow.
- The PI may be a little too enamored of the Na substitution theory of Ti.
- The systems chosen for study represent a weakness.

- Resolution of exact mechanisms perhaps slower than expected. Not clear how this will quickly lead to DOE goals.

**Specific recommendations and additions or deletions to the work scope**

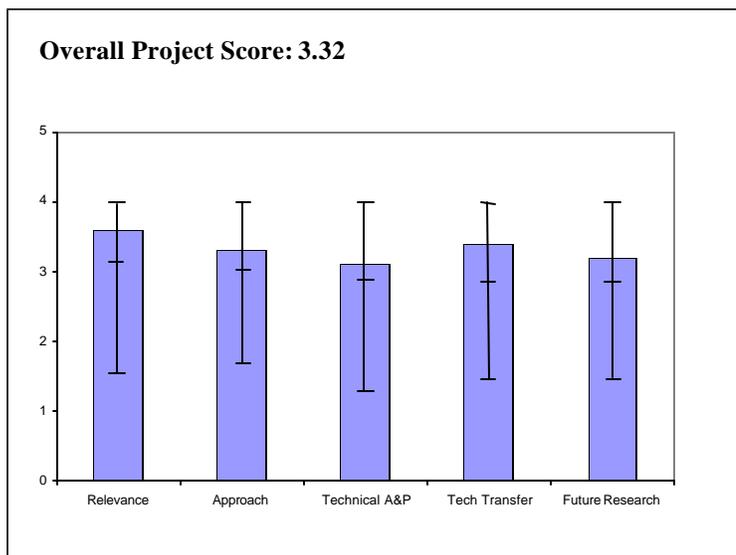
- Examine data in more detail to look for consistencies. Also, consider effects of ball milling. Per PI, ball milling enhances initial absorption but not reverse. Could "dopants" simply stabilize physical structure toward hydride/dehydride rates.
- Consider re-focusing from alanates towards the discovery of novel "breakthrough" materials (other than alanates).
- Find ways to confirm results. Add system perspective. Add cost and safety information.
- Fully characterize the new material if it shows decent gravimetric capacity and release temperature. Definitely prove where the Ti goes.
- Either prove that doped alanates have a chance of meeting meaningful (auto industry accepted) targets for H<sub>2</sub> storage or move on to systems that do. The sooner the better.
- Closer collaboration with commercial hydride producer.
- Focus on the new materials.
- This program should end as quickly as possible.
- Synthesizing and testing the higher-density materials (based on the hypothesized bulk substitution) should be the absolute top priority for this project. This definitely should be sorted out before next year's meeting.

## Project # ST-9: Hydride Development for Hydrogen Storage

Wang, Jim; Sandia National Laboratories, Livermore

### Brief Summary of Project

Sandia National Laboratories (SNL) is seeking to develop new complex hydride materials capable of achieving at least 6 wt% system hydrogen capacity, to improve kinetics of absorption and desorption and thermodynamic plateau pressures, and to improve processing and doping techniques that will lower cost. Current materials under study include: advanced complex hydrides; destabilized binary hydrides; novel intermetallic hydrides; and other reversible hydride-based materials. In addition to new materials discovery, work continues in fundamental modeling, materials synthesis and modification, testing of hydrogen storage and delivery characteristics and engineering science and process development.



### Question 1: Relevance to overall DOE objectives

This project earned a score of **3.60** for its relevance to DOE objectives.

- Important project to look for solid-state chemical hydrides. Need to investigate these classes of materials thoroughly.
- Metal hydride research is one of several possible high potential storage technologies that would support/enable a hydrogen transportation system.
- The research programs cover various chemical hydrides and new materials. Structured presentation with useful information. Amides may be the way to go. Safety and cost issues need to be identified. Having SNL to lead the Complex Hydrides Center of Excellence was the right choice.
- This project provides an important component towards meeting the capability to develop a hydrogen storage material that meets the targets and is contributing towards identifying what material will eventually meet those criteria.
- Good match to DOE goals.
- System with 6 wt% stored reversibly doesn't meet the DOE performance targets. New materials are needed.
- Good focus on new materials.
- Focused on the right problems. This is a high-resource project but is likely the best program in the entire DOE portfolio.

### Question 2: Approach to performing the research and development

This project was rated **3.30** on its approach.

- Good systematic approach backed by careful scientific thought.

- Evaluating a large number of complex hybrids - which is likely to be required to find the optimum few for further assessment.
- Good focus on new material discovery. Good testing facilities. Good upfront estimates of the potential of new materials (amides, etc.).
- Projects are well-designed to answer important questions. A system approach has to follow material discovery.
- The new facility they are building/have built is very good as are the approaches towards identifying new storage materials.
- MgLi amides promising new materials. Did they come from LANL or Singapore? Need to ensure no duplication of work at other labs. Mechanistic work in progress for amides? Ti-alanate calculation promising result for mechanism elucidation.
- A program this size ought to be able to make meaningful progress in a couple of years. The approach is well orchestrated.
- Careful. Safety excellent. Excellent facilities.

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

This project was rated **3.10** based on accomplishments.

- Promising results of Mg-Li-amides. Reasonable model for Na-alanates; further test this model with appropriate experiments.
- Difficult to assess technical accomplishments because insufficient analytical or experimental data was presented. Presentation was too superficial for valuable assessment of this area.
- Potential of new materials has not been demonstrated yet. Good system engineering estimates.
- Gravimetric density seems to be the metric used to measure the storage capacity of materials.
- This project has a lot accomplished with a lot of people contributing to it - the difficulty here is how to measure accomplishments of over 20 people and \$1.7M per year with projects of \$200K and a couple of graduate students.
- Reasonable progress. Good discovery on amides. Limited mechanistic progress on alanates given size of effort. Engineering work with conductivity is important for device fabrication.
- Major effort expended in setting up state-of-the-art measurement facilities (e.g., an in situ XRD system). They seem to have what they need in place to get going at a respectable pace.
- Good start.
- Li amide work is very interesting, as is the work on the Ti mechanism in alanates. Future progress along these lines is highly anticipated.

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

This project was rated **3.40** for technology transfer and collaboration.

- Lots of collaborations; and research exchange with Univ. of Singapore very good.
- Numerous collaborations were cited but it is unclear to this reviewer how extensive or in-depth the collaborations actually were.
- Excellent collaborations and teamwork.
- The number of interactions and collaborations that they identify is quite impressive; this reviewer hopes they are as intensive as they are numerous.
- Good work with others.
- Lots of strategically chosen collaborators. UTRC prototype demonstration project should be incorporated into the Center.

- Visiting scientist. Good list of collaborations.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **3.20** for proposed future work.

- Overall program plans look promising. Still too early in the project to evaluate technical feasibility of many of the proposed ideas.
- I am expecting good results from the DOE Center of Excellence and various collaborations led by this lab.
- Proposed work is a new project. A center of excellence that has all the appropriate milestones, etc.
- Would like to have more information on approaches planned to carry out 2004 plans.
- The plans for future work seem to be clearly directed at meeting the DOE targets for "weight" and "volume."
- Good plan for the next few years.

### **Strengths and weaknesses**

#### Strengths

- Strong engineering discipline combined with detailed scientific rigors.
- Systematic approach backed by good scientific approach.
- Excellent comprehensive approach to new storage system design including discovery of new materials via the second project.
- The presentation was well prepared and easy to follow. It looks like they have enough resources to get a job done. Unique in working on the amides. Accomplishments are easy to see.
- This project involves a lot of people with diverse backgrounds that provides a broad scientific base that can be brought together to solve new problems and explore new opportunities.
- Good team. Excellent facility development.
- Broad focus that includes "nano" concepts and embraces a wide variety of systems.
- Competent organization. Large group of in-house and outside participants. (Turned in safety 1-pager.)

#### Weaknesses

- The output of a team is compared with resources that they receive. Not enough progress made toward development of an onboard storage module.
- PI did not allocate his time well. My perception was that the PI spent far too much time talking about safety, organization/staffing and test facilities and equipment plans that there was insufficient time for detailed discussions about the content related portions of the report.
- Primarily empirical approach to new material discovery. Need more systematic approach.
- Material target only. It looks that hydrogen release from the Li amide requires high pressure. Cost estimation is not covered.
- Difficulty of geographic separation. Collaboration and may result in duplication of effort and facilities.
- Thermal conductivity of the alanates is low, making for difficult heat transfer to release hydrogen.

### **Specific recommendations and additions or deletions to the work scope**

- Work with U. Hawaii (C. Jensen) to evaluate alanate results. Sandia model seems reasonable. Is it consistent with all data? What experiments are needed to evaluate materials versus this model?

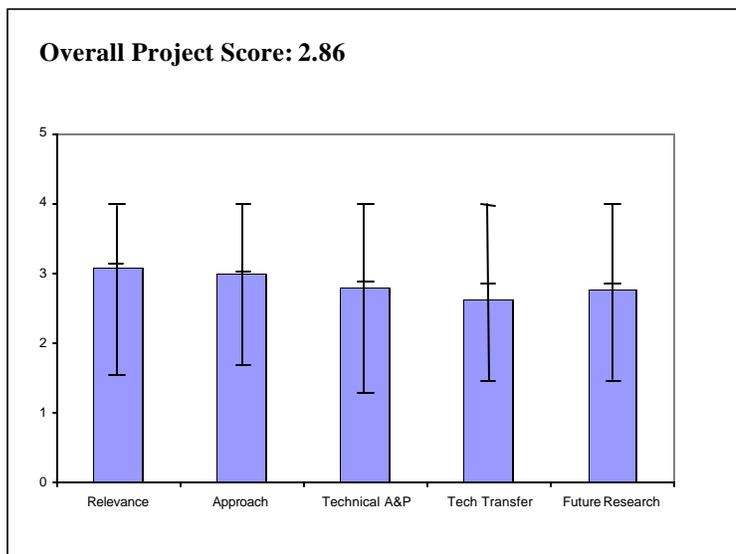
- System-based studies are needed.
- This project should definitely continue.
- Make sure the performance metrics include considerations of (1) "whole storage system" weights and volumes and (2) "net" energy delivered to the vehicle.
- Schedule down select of materials and no-go for those that will not meet targets.
- Should consider investment in Na-alanates given uncertainties in this approach. Continue work in this area, but do not sacrifice other ideas for this chemistry.

## Project # ST-10a: Development of a High Density Hydrogen Storage System Prototype Using Doped Sodium Alanate Complex Hydrides

Anton, Don; United Technologies Research Center

### Brief Summary of Project

United Technologies Research Center (UTRC) is working to design, develop, and evaluate a prototype hydrogen storage system using NaAlH<sub>4</sub>-based complex hydrides with an ultimate goal of 5-kg hydrogen capacity. The hydrogen storage system will be suitable for a PEM fuel cell powered mid-size automobile application. UTRC's approach is to develop improved sodium alanate based materials and to design and fabricate a 1-kg hydrogen system for initial testing. UTRC will attempt to improve the charging and discharging rates of the NaAlH<sub>4</sub>-based materials by increasing the reversible weight fraction of hydrogen stored to 7.5%, enhancing the hydrogen evolution rate to meet steady-state demand, and increasing the regeneration rates to achieve the five-minute refill requirement. Based on the 1-kg prototype's performance, a decision will be made with regard to building and testing a 5-kg hydrogen prototype. They will also determine the safety and risk factors associated with the enhanced material compositions.



### Question 1: Relevance to overall DOE objectives

This project earned a score of **3.08** for its relevance to DOE objectives.

- Metal hydride research is one of several possible high potential storage technologies that would support/enable a hydrogen transportation system.
- No need to develop a system without having a basic material meeting the targets.
- The only team working on developing a storage *system* (currently). Good and informative presentation. Honest about data, without making unsupported claims.
- Engineering work is needed to show what tank "overhead" truly is.
- Device construction is crucial to identify issues involved with alanates and future hydrides. Information is probably available within auto makers, but needs to be public.
- Lots of money is being spent on a system that can not meet meaningful weight and volume target values for H<sub>2</sub> storage. However, the lessons learned from engineering a prototype system will be very valuable to the program.
- Good and detailed focus on a practical vehicular (PEMFC) H<sub>2</sub> - storage tank. Science and engineering good.
- Good to do a full system even though it will not meet goals.
- The only project in the portfolio focused on system issues relevant to solid-state storage. This is an important issue that deserves more attention from the DOE.

## **Question 2: Approach to performing the research and development**

This project was rated **3.00** on its approach.

- Not sure how they will overcome the barrier. Not clear why they want to continue when their own analysis- shows short-term failure.
- Noteworthy that project is conducted by a commercial company that plans to build a near-production size system. This differentiates this project from others in the area.
- System cannot meet the targets.
- Approach remains good.
- The approach to study catalyzed aluminum hydrides and design systems/approaches that can handle any hydride is very good.
- Comprehensive approach to multiple problems. Use of commercial H<sub>2</sub> is good to approximate real world. Would like to see high purity for comparison. Integrated approach to building device is strong.
- Methodology is standard; the project could use a stronger science component and a broader range of physicochemical measurement methods.
- Good combination of modeling and experimental work. Detailed safety studies are a plus. First effort to develop a full-scale (5-kg H<sub>2</sub>) composite vessel.
- Good careful job.
- Broad approach using experiments, modeling, safety tests, and engineering.
- Some flaws in their approach. Modeling work seems very weak, and does not bode well for future project (complex hydrides materials discovery project). For example: The "prediction" from modeling the efficacy of catalyst additions is based on many underlying assumptions which were not mentioned (the idea of bulk substitution being most obvious). And, the most important thing is that the predictions do not work (again, not mentioned). Cerium (Ce) is not a better catalyst than Ti. And, from their model, the impurity with the highest heat of solution would produce the highest energetic effect, and therefore be predicted to be the best catalyst. However, it would also be the most energetically unlikely to form (again, not mentioned).

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

This project was rated **2.81** based on accomplishments.

- Difficult to evaluate the accomplishments because the PI moved so quickly through the presentation. He skipped several key slides such as gravimetric and volumetric efficiencies. PI should not assume that the reviewers attended other technical sessions and should report relevant results here; sacrificing other portions of the presentation to include key results, accomplishments and status.
- The slide showing barriers and targets should also include a column for performance to those targets as well as the go/no-go.
- Much work is being done, question is will it meet goals. Goals seem unlikely to be met even though they are less than DOE current goal for 2005. Some of the progress shown was also shown last year, so I do not count that this year. Improved materials are not as good as those demonstrated previously by other teams. Significant progress on engineering though.
- The accomplishments may be better than the score of fair but it is not possible to tell from data that is withheld from the review team - this is a basic flaw in the review process and a strategy for the project management.
- 1-kg hydrogen system progressing well.
- Most of the progress made is not encouraging for alanates. The matter of storage medium degradation due to persistent impurities in the reload hydrogen is not a trivial one.

- Extensive progress on many fronts (especially engineering/system). Safety studies excellent (but a bit intimidating). Tank design done well, with very important heat transfer modeling. About to begin testing.
- Good progress toward classifying safety properties. Developed new doping process although material performance not generally improved by new process.

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

This project was rated **2.62** for technology transfer and collaboration.

- While difficult, there should be more collaboration with Sandia National Labs.
- Certainly a critical program. Connections are good.
- This project has good collaborations and interactions with others, - academic and industrial.
- Over a half dozen collaborators - mostly industrial types. If the project is moved into the Sandia center, the science component should be improved.
- Many good collaborations with supporting organizations. Good team.
- Too much proprietary (data and results).
- Thermodynamic databases they are developing are important and this type of heat management that they enable will be important to future efforts in the area. DOE is paying for this development, and needs to insure that these databases will be available to others in the field. (This is not proprietary fuel cell related information.)
- System-level modeling is quite important, but was described as proprietary IP. Again, this will be important work that the DOE will have to re-fund over and over in the future if every company declares this as proprietary. Perhaps these researchers could extract the sensitive fuel cell information from their systems model and leave these as inputs to the model and provide a non-proprietary version of this systems model?

#### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.77** for proposed future work.

- The future plans do not address the challenges.
- Timeline chart was not readable.
- Plans look suitable. Hope they pan out.
- Approach of foregoing 5-kg hydrogen system and improving the 1-kg program is a good modification of the work plan.
- It's not clear how catalysts are going to help alanates meet DOE targets for 2015.
- Good plan to finish work.
- Revised plan to redesign 1-kg hydrogen bed is a good idea. Premature to build 5-kg hydrogen bed.

#### **Strengths and weaknesses**

##### Strengths

- Very honest presentation - this project should be supported.
- The only team building and testing a hydrogen storage system. Safety issues are considered.
- Only real engineering program currently active. Powerful team.
- The capability to handle any hydride is important and well thought out. The safety experiments are excellent and help everyone.

- The redirection towards new work to take advantage of new materials and recognition of the need to redesign the vessel to enable the gravimetric requirements to be met is completely appropriate.
- Good, well-integrated engineering approach. Repeating 1-kg hydrogen device allows learning from v1.0 device.
- The team assembled to engineer and build the prototype system is impressive.
- Very systematic and professional, combining modeling, engineering, and experimental.
- UTRC is a quality outfit.

#### Weaknesses

- Experimental approach - why a composite tank? Unclear what the new targets are when they admit they will not meet some of them.
- To reiterate, the PI should size the presentation to fit the time allocated and should not move quickly past key status or results on the assumption that reviewers were at other meetings where the results were able to be discussed in more detail.
- How to design the system such that it can be used with other metal hydrides?
- Will not meet 2005 targets. However, the project is a good exercise.
- Why are the results of the system level modeling sensitive IP? I assume the models and the details are proprietary. If this is an internal effort, it does not need to be mentioned here.
- Approaches taken on material do not seem to equal those taken by others.
- Non-disclosure of new materials and their structures inhibits advances of the scientific community as a whole and especially in such an important area where the state-of-the-art is still a pretty long way from reaching the goals. Such non-disclosure only suppresses the ideas of others who could come up with "the magic bullet."
- Alanates probably have limits and may never make DOE (FreedomCAR) goals. But should try.

#### Specific recommendations and additions or deletions to the work scope

- Re-scope the project by setting new priorities: Science objectives (e.g., material, transport phenomena); engineering objectives; storage module/vessel development objectives.
- Suggest redirecting resources towards the development of other reversible  $H_2$  sorbents that can potentially meet targets.
- Focus on improving the engineering aspects.
- Continue project with redirection efforts.
- Computational details need to be examined - UTC vs. Hawaii results needs to be rationalized.
- This is a "demo" project; anything that could be done to put the project on a more scientific approach might get the demonstration closer to something that auto makers will consider attractive. Inlet  $H_2$  purity should be studied further to determine the effect on durability over repeated cycles.
- No changes.
- Try to get system to not be proprietary.
- This project needs "competition." Right now, they are the only ones working on this, and are going to be unable to meet the (very conservative) go/no-go decision points. However, without some other group working on this, it is difficult to tell whether this project simply is not executing a clever engineering design, or whether this is a more fundamental problem which will befall anyone in the field. A second project in this area would at least provide a point of comparison.
- Thermodynamic databases they are developing are important. DOE is paying for this development, and needs to insure that these databases will be available to others in the field.
- System-level modeling is quite important, but was described as proprietary IP. Again, this will be important work that the DOE will have to re-fund over and over in the future if every company declares this as proprietary.

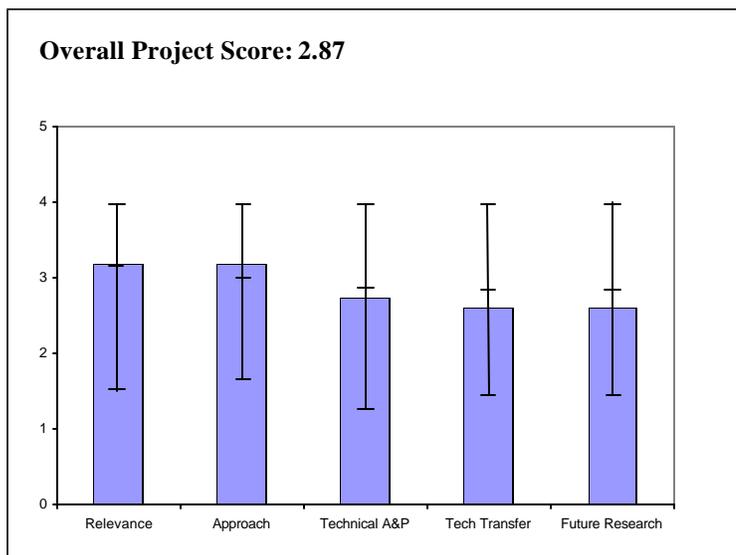
## Project # ST-10b: Complex Hydride Compounds with Enhanced Hydrogen Storage Capacity<sup>1</sup>

Anton, Don; United Technologies Research Center

### Brief Summary of Project

United Technologies Research Center (UTRC) will develop a new complex hydride compound(s) capable of achieving greater than 7.5 wt% hydrogen capacity and 500 cycle-reversibility with 100% efficiency. The focus will be on  $\text{Na}_y\text{M}_{+ix}(\text{AlH}_4)_{y+ix}$ , in the quaternary phase space between sodium hydride (NaH), alane ( $\text{AlH}_3$ ), transition metal or rare earth (M) hydrides ( $\text{MH}_z$ , where  $z = 1-3$ ) and molecular hydrogen ( $\text{H}_2$ ). The team will accelerate the discovery of new complex hydride compounds and guide experimentation with first principles modeling. The team will conduct three

levels of performance evaluations to select compositions for further development, optimize dehydrogenation and hydrogenation catalysis with spectroscopic mechanistic studies and first-principles screening simulations, develop manufacturing processes to reduce cost and scale-up production, and develop business analyses for the commercialization of hydrogen storage systems integrated with fuel cell power plants. **This is a new project, started in 2004.**



### Question 1: Relevance to overall DOE objectives

This project earned a score of **3.20** for its relevance to DOE objectives.

- This project is an important component in a program to find and develop materials for storing hydrogen and supports the Initiative's goals and objectives. This is true of both the old project and the new one.
- Good project in terms of looking for hydrogen storage materials.
- Need to find better materials.
- New materials important. Obvious match.
- Fundamental work is also useful both to them and others.
- Logical survey of "new" alanates.
- This project is an important component in a program to find and develop materials for storing hydrogen and supports the initiative's goals and objectives.

### Question 2: Approach to performing the research and development

This project was rated **3.20** on its approach.

- Project has just started. High wt% storage material is important; not enough progress yet to determine if this project will get there. Does show potential.

<sup>1</sup> Note: In some cases, reviewers evaluated both ST-10a and ST-10b on the same evaluation form.

- The approach to study catalyzed aluminum hydrides and design systems/approaches that can handle any hydride is very good. (Both the old and the new projects).
- Good target set for basis system (not just material).
- Good discovery approach.
- This project attempts to bring the science component to the project in searching for enhanced performance (of the materials).
- Approach looks great. Good mix of theory and experiment. Molten state synthesis is interesting. Has it been tried for NaAlH<sub>4</sub>?
- "New alanate" search will be quite systematic.

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

This project was rated **2.75** based on accomplishments.

- The discovery project just started so no progress to report.
- New project's strength is that it is built on the experience from the old project and would be rated very good.
- Since project has just started, it is difficult to assess progress.
- Not enough innovation in selecting new materials for year 1 evaluations (still alanates).
- The slide showing barriers and targets should also include a column for performance to those targets as well as the go/no-go.
- Materials development program is a new start, so no accomplishments to report.

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

This project was rated **2.60** for technology transfer and collaboration.

- This project has good collaborations and interactions with others.
- Probably will be proprietary problems here.
- Good team- exploits strengths of each partner.
- Reasonable mix of universities, research facilities, and industry.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.60** for proposed future work.

- Future plans continue on the past successes and are quite good.
- Time line chart was not clear. Future plan was discussed briefly.
- New program is ambitious.
- Seems logical and well thought out.
- Continues with previously successful combination of modeling and experiment.

### **Strengths and weaknesses**

#### Strengths

- Systematic approach to new material search. Good synthetic capabilities already in place.
- UTRC is a quality place.

### Weaknesses

- Not clear how this program differs from other projects in the field. Widen the search beyond alanates.
- Intrinsic costs of materials not considered. Still focused on alanates.
- From the presentation, it appeared that the modeling efforts were independent of experiment in terms of some kind of verification that the modeling data is predicting properties that are correct. If this is not included it needs to be included, or you could wind up chasing non-existent artifacts.

### Specific recommendations and additions or deletions to the work scope

- Approach of doping sodium aluminate material is consistent with other efforts at University of Hawaii and Sandia National Labs. DOE should work with contractors to promote synergy and minimize overlap.
- Consider broadening to include non-alanate materials.
- Material cost needs to be addressed.
- This project seems to rely heavily on atomistic modeling capabilities, and this group simply does not have the relevant expertise. (For instance, it is clear that they don't even know how to spell "first-principle" - this is not a quibble, but just an indication that this group is not expert in this topic.) DOE should consider having them work with established experts in this area, or possibly changing the research direction of their new project.
- DOE should consider how this project relates to or coordinates with the Sandia Complex Hydrides Center of Excellence.
- Need to ensure that modeling efforts are not independent of experiment- need validation that the modeling is predicting properties correctly.

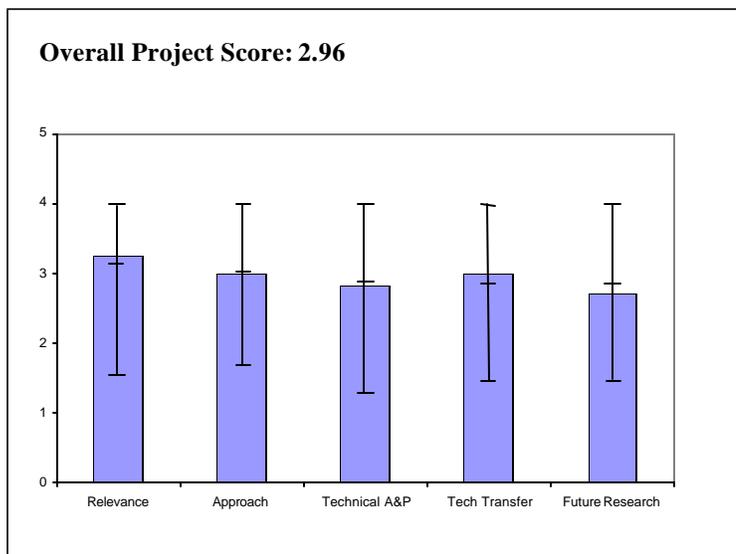
## Project # ST-11: Discovery of Novel Complex Metal Hydrides for Hydrogen Storage through Molecular Modeling and Combinatorial Methods

Lesch, David (PI); Sachtler, Adriaan presenting; University of Pennsylvania

### Brief Summary of Project

The objective of the proposed project is to discover novel complex metal hydrides for hydrogen storage that contain 6 wt% or more of hydrogen and can reversibly desorb hydrogen between -40 and 90C. The project will combine molecular modeling with high throughput combinatorial material synthesis, testing and characterization, large-scale materials testing, and system engineering studies to produce prototype, commercializable hydrogen storage systems.

**This is a new project, started in 2004.**



### Question 1: Relevance to overall DOE objectives

This project earned a score of **3.25** for its relevance to DOE objectives.

- Metal hydrides are a possible high potential opportunity for hydrogen storage that can support the hydrogen transportation vision long-term.
- Has potential to be more relevant if project progresses as expected by UOP.
- Good technique and balanced program to achieve what is desired.
- Well aimed at critical problems.
- The goals as outlined, support the hydrogen objectives and contribute a very important discovery/computational aspect to address this initiative.
- Good match.
- UOP seems to understand the "target" issues in terms of "system weight," "system volume," and "net" energy delivered. Go/no-go decision making is in the plan.
- Aiming at DOE targets, as required.
- Need better materials.

### Question 2: Approach to performing the research and development

This project was rated **3.00** on its approach.

- Appear to be taking a logical approach to addressing the issues.
- Need to be more specific than "2010" targets as objectives.
- Having go/no-go points is a good step.
- Good focus on new materials search.
- Balanced approach of experiment and theory.
- Combinatorial on large scale samples is good.
- Need better definition of how they will analyze at high rates.

- Cost is identified as a technical barrier - it's not - it's an economic barrier that results from some underlying technical barriers that should be clearly identified.
- Combinatorial approach is promising. Would like more info on screens. Development of classical computational methods that yield useful results is challenging, but will be a great aid in rapid computational screening. Will combinatorial approach be used for synthesis and screening, or just screening?
- The approach looks efficient - combinatorial synthesis, TPD based screening, and theory coupled in to help elucidate results and define pathways for further research. More details on the high-throughput combinatorial screening technique are needed to assess the efficacy of this approach.
- Modeling/combinatorial dual approach good.
- Safety and environmental matters well-covered.
- Vague on materials to be studied. A few alanates at first. Not clear about possible overlaps with other DOE projects (e.g. ST-10).
- Screening - mass production.
- Will the combinatorial approach lead to rapid development of new material?

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

This project was rated **2.83** based on accomplishments.

- Program just started so difficult to evaluate technical accomplishments or progress.
- New project. Good plan overall.
- Project is new but they had the labs ready and began modeling work.
- New program. Too early!
- Good progress has been made for being in existence for about 3 weeks.
- Just getting started with this big program. Large cost share by UOP shows commitment.
- Just starting this month but experimental equipment is already put in place.

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

This project was rated **3.00** for technology transfer and collaboration.

- Appears to have a good mix of industry, university and research facilities.
- Project has a good level of collaboration by including National Labs, academia and industry.
- Good set of partners.
- Program is critical to realization of goals.
- The partnerships that are established are quite good and all should contribute towards its success.
- Good combination of participants.
- Collaborating with Ford (an auto company - that's good), UCLA, Univ. of Hawaii (a major university player), and Striatum.
- Good experienced partners.
- 4 on team - limited now.
- Good team assembled but no interaction outside of team to draw on years of experience/results of other teams.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.70** for proposed future work.

- Lays out comprehensive research plan for each year accompanied by go/no-go criteria for assessing value of research and whether or not to proceed to next phase of research.
- Project is just starting, so can't really evaluate future research based on past progress. May want to include more contingency planning if research doesn't yield expected results.
- Good balanced plan.
- New project so it has to be watched to see how it develops - while no past progress in this program it is based on progress in other areas.
- Still building facilities. Future plans lacking in detail for such a large project.
- This reviewer finds the future (especially materials) rather vague. Presenter apparently not very experienced in hydrides. Not well thought through (materials and methods).
- New start.

### **Strengths and weaknesses**

#### **Strengths**

- Approach sounds good.
- Good cross-section of expertise and collaborators.
- Systematic approach to new materials discovery. Development of predictive tools for new materials properties.
- Good team. Technical approach is well-defined. Cost estimates to be performed.
- Balanced approach.
- Coupling discovery science and computation is fundamentally a good idea and an important component of a technology discovery/development program. One question that comes up is whether the discovery method they are designing can "discover" known existing systems.
- Ambitious project. Challenging tasks in early years to develop combinatorial and computational methods.
- Substantial in kind contribution and an auto manufacturer for a partner.
- Marriage of modeling and combinatorial experimental work.
- They have auto manufacturer (Ford) involved.

#### **Weaknesses**

- It may not be possible to actually synthesize many of the predicted materials.
- Too early to tell - but the need for high throughput screening needs to be carefully thought out because that is both needed to test the high throughput preparation and couple it to the human interface. This can be and often is the downfall to many discovery science activities and it was not clearly defined - at least at this early stage in the project.
- Will the science base be strong enough to push this project past the obstacles limiting the search for a usable hydrogen storage material that meets automotive industry requirements?
- More work on alanates! This seems to be an overworked field. Possible duplications with other DOE projects (e.g. ST-10).

### **Specific recommendations and additions or deletions to the work scope**

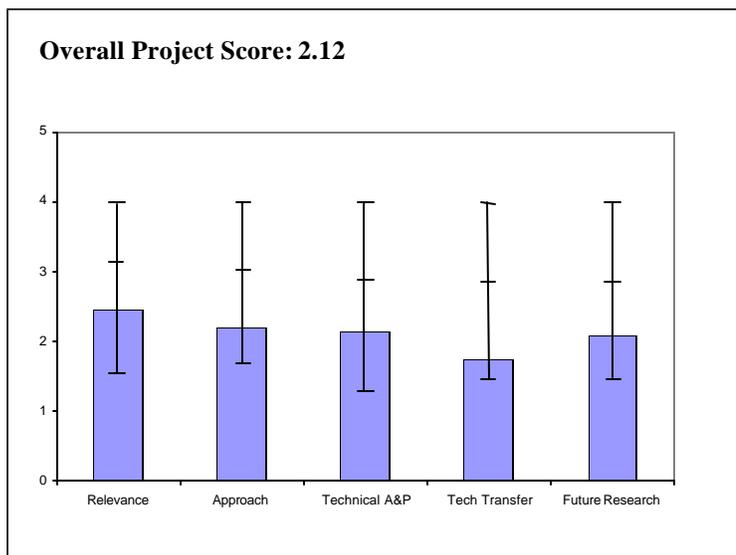
- While not possible this year, for next year's review, show progress towards targets and goals.
- This is a new program that should be continued.
- Come to the FY 2005 Review with clearly laid out plans for FY 2006 and beyond.
- Consider immediate move to year 2 materials.
- DOE should consider how this project relates to or coordinates with the Sandia Complex Hydrides Center of Excellence.

## Project # ST-12: Sub-Nanostructured Non-Transition Metal Complex Grids for Hydrogen Storage

Talu, Orhan (PI), Surendra, Tewari Presenting; Cleveland State University

### Brief Summary of Project

The objectives of this project are to grow sub-nanostructured metal grids (less than 1 nm thick) with about 50% microporosity (less than 1 nm wide), measure hydrogen uptake/release, and evaluate these novel metal grids for hydrogen storage applications. The premise is that the overall hydrogen dissociation reaction rate will be higher (because the external metal surface area is enhanced), and the diffusion time constants will be lower (because the diffusion path is greatly reduced). In addition, conduction in the highly interconnected metal grid and convection in the micropore space may enhance heat transfer. The nanostructured metal grids will be grown from pure and alloyed non-transition metals and the phase diagram is anticipated to be different from the bulk phase diagrams because of the quantum effects that may arise at these length scales. **This is a new project, started in 2004.**



### Question 1: Relevance to overall DOE objectives

This project earned a score of **2.46** for its relevance to DOE objectives.

- Innovative concept but not sure how it will address and support the Hydrogen Initiative.
- Metal hydrides are a possible high potential opportunity for hydrogen storage that can support the hydrogen transportation vision long term.
- It is not clear that this project directly aligns to supporting the President's Initiative.
- Benefits not clear. Claims in slide 2 are not supported. No scientific evidence it can be done. May not be successful. Will not meet volumetric targets. What is the cost of making the structure? This concept may be useful for other applications.
- Potentially relevant in areas other than storage, but would be a major contribution if it works for storage - opening a new "front" in the effort to achieve the goals.
- The goal of building metal grids to reduce the diffusional barriers associated with transport of hydrogen has the potential to overcome or help overcome some of the issues with implementing the President's goals.
- Novel approach for making micrometals.
- This team really hasn't done its homework. Calculations should have been presented to show what the possible storage volumes for hydrogen might be for the 3-D grid structures. Stable grid structures may not be possible and may not come close to meeting DOE volumetric storage goals. Calculations would have been helpful. Despite the negative sounding comments, the project is far out and probably needs some time before a judgment can be made.
- Does not give any theoretical estimate as to what level of gravimetric and volumetric hydrogen storage capacity might result, vis-à-vis DOE targets.
- Trying new approach for high performance materials.

- Initial work is long way from developing storage material. This is really a material science project to develop a metal grid based on zeolites. Not really a hydride project.
- This idea seems unlikely (in the extreme) to work. We do need ground-breaking new ideas admittedly, but it is unclear how such an approach will help enable DOE goals, even if it works.

### **Question 2: Approach to performing the research and development**

This project was rated **2.19** on its approach.

- Interesting approach to try to grow film on a metal substrate. Looking forward to future work and results to see if theoretical concept will bear fruit.
- Approach and tasks identified, but with the project just starting, it is difficult to evaluate feasibility and whether or not the expected results are possible.
- No evaluations of theoretical capacity, intrinsic costs. Approach is unlikely to work.
- Interesting approach. Many ways it could fail do not seem adequately addressed (e.g. grid collapse when zeolite removed, pore plugging too early, poor substrate zeolite contact, grid made but H<sub>2</sub> adsorption insufficient). Need to plan work around. May be under-staffed - remains to be seen.
- The approach proposed appears reasonably sound; however, since this is a new project, we'll have to wait and see how things go. The big question here is whether these metal grids even have the possibility of meeting the volume and weight targets set out for the storage needs of industry.
- Initial synthesis will be challenging. Maintaining structure will be even more difficult. Should consider mesoporous (MCM-41, etc.) materials in addition to zeolites. High risk project. Needs appropriate go/no-go decision points. Gas adsorption and XRD may be better techniques for characterization than HRTEM. They work much better for zeolites and should be better for the inverse lattices as well.
- Based on all the "hoping" and "trying" statements in the review presentation, I sense that the investigators for this project might be flying in the dark. It's not likely that a few metal atoms per faujasite super cage will connect up into a 3-D grid when the support is dissolved away. Also, zeolites are not good electrolytes or electrodes, so I'm not sure what kind of electrochemical exchange methods one could use effectively with them.
- New approach. New ideas needed. Details are lacking. I suspect only kinetic improvements can be expected, unlikely to improve capacity.
- High risk, high payoff.
- Unlikely that structure would stay intact with cycling.

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

This project was rated **2.14** based on accomplishments.

- Project just funded. Too early to assess technical accomplishment or progress. Will want to watch closely if the researchers will be able to create the structure they think they can.
- Project is just starting, so can't really evaluate progress.
- New project. Too early.
- Some progress has been made since the project's inception and the milestones are reasonable.
- This is a new start - no results yet to provide any confidence that the methodology will work.
- New project. No progress to report.

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

This project was rated **1.73** for technology transfer and collaboration.

- Need to work or find a partner with end user or an application in mind.
- No discussion of collaboration in presentation.
- From the presentation, this project appears to only include work at Cleveland State. If this is a collaborative project, it should be identified.
- Not clearly connected to other work. Partly supports the plan.
- This area of the project needs strengthening that should occur as the project matures, but at this point there are no apparent collaborations.
- Should work with Nenoff of Sandia or Pall Corporation for help with metal supported zeolites. Could cut months off the project and focus on goals rather than substrate preparation.
- No meaningful collaborations. Recommend someone with experience in the manipulation and modification of molecular sieve structures. The general approach has been used by other investigators going back to the 1980s.
- No collaborations cited by PI. Need help with practical hydride materials.
- Few if any partners identified.
- None identified.

#### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.09** for proposed future work.

- PI describes detailed research plans; but question whether the first year's goal is sufficient/adequate.
- Not clear that the future work identified will provide the intended results. Does appear to be basic research.
- The plan looks adequate for now, if there is weakness it is on handling surprises that may arise in terms of grid collapse.
- This is a recently funded project and builds on the previous experience of the investigators in adsorption phenomena.
- Not optimistic. Wait and see what they present at the FY 2005 review.
- Some plans in place. Almost nothing on practical hydrides to be put in grid form. I get the impression presenter has very little background in state-of-the-art hydrogen storage materials.
- Has proposed plan.
- New start.

#### **Strengths and weaknesses**

##### Strengths

- Innovative novel concept.
- Good method for electrode development.
- Bright idea of making a "micro porous hydride material" to overcome diffusional barriers in the hydrogen storage system.
- Innovative.
- Novel approach to highly dispersed metals.
- New idea. Should improve kinetics and heat transfer.
- New approach.

### Weaknesses

- Lack specific H<sub>2</sub> storage application.
- The metal grid is potentially subject to H<sub>2</sub> attack, thus collapsing the grid.
- Not enough focus on goals and targets.
- Metal grids (if Li, for example) would leach out together with zeolite matrix. "Microporous hydrides" will get converted to bulk crystalline material (thermodynamics).
- Highly speculative. Unlikely to make mass target.
- The lack of recognition (response to a question) that Pd has been ion exchanged into faujasites and reduced is surprising since this is the basis of probably 50% of the world's hydrocracking catalysts and a good percentage of its aromatic hydrogenating capacity.
- High risk. Need collaborations with people skilled in characterizing microporous materials.
- The presentation was not very clear about what underpinned the thinking that the proposed approach had validity. We know what the structural dimensions of the faujasite structure are and how many metal atoms we are likely to pack into that structure. Surely, some projections could be made from this information.
- Sub-nano grids seem unlikely to improve hydrogen storage (gravimetric or volumetric). Unlikely to reach DOE capacity goals.
- May not work.
- Don't see any potential for achieving high capacity material or even better understanding of storage materials. Small structure only impacts kinetics. Difficulties in depositing multi-component alloys. Appear to be limited to conducting systems.

### Specific recommendations and additions or deletions to the work scope

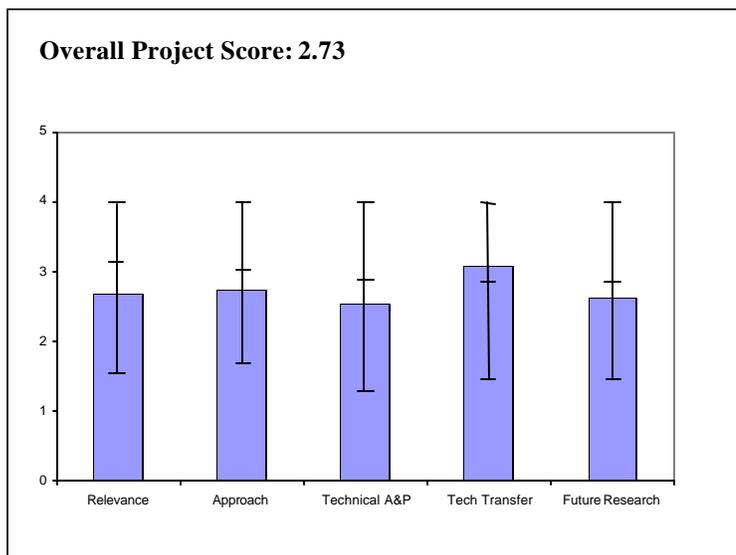
- Should be considered for funding in a different program, not H<sub>2</sub> storage. Need to look for end application as a scope.
- Should work with Nenoff of Sandia or Pall Corporation for help with metal supported zeolites. Could cut months off the project and focus on goals rather than substrate preparation.
- While not possible this year, for next year's review, show progress to targets and goals.
- Quickly do thermodynamic calculations to see if "hydride grids" will get converted to bulk upon recycling. Redirect or terminate the project scope.
- Focus as soon as possible on light materials. Note: this would be a great project for NSF/BES to fund. May have a role in battery materials; plan a route to transfer the work to the appropriate part of FreedomCAR or DOE if storage of hydrogen is small.
- This is a new project and should continue.
- It would be interesting and possibly of value to the project staff to see the results of a literature search that probes the prior work done by others to use molecular sieve materials as a means of making nanoscale materials/structures, such as quantum dots.

## Project # ST-13: Hydrogen Storage in Carbon-based Materials

Heben, Mike; National Renewable Energy Laboratory

### Brief Summary of Project

The focus of National Renewable Energy Laboratory's (NREL) work is on hydrogen storage in carbon-based materials such as single-wall nanotubes (SWNTs). The objectives are to determine the extent to which hybrid SWNTs can reversibly store hydrogen, to discover the mechanism of hydrogen storage in these materials, and to develop low cost, reproducible, and potentially scalable processes for producing SWNTs. The main focus in 2004 was on *reproducibly* measuring hydrogen storage capacity in hybrid SWNTs at room temperature and pressure.



### Question 1: Relevance to overall DOE objectives

This project earned a score of **2.69** for its relevance to DOE objectives.

- Addresses 2010 target. Broad potential impact for both vehicle and stationary applications, but significant hurdles.
- Good science. If successful the program is well aligned.
- Carbon nanotubes are a possible storage technology that has been subject to much publicity and hype.
- Satisfies the need to explore and develop advanced materials for solid-state storage.
- Worth a thorough exploration of carbon.
- Class of materials may be applicable to storage applications.
- There is a critical need for the DOE storage program to determine whether there is indeed any commercially viable potential in nanotube technology. If so, the rating would be significantly higher, but as long as there is skepticism within the technical community, the rating needs to be lower given the definitions herein.
- Are carbon nanotubes capable of being a cost-effective way of storing hydrogen?
- High storage density may not be achieved.
- What is the plan to achieve the 4% SWNT capacity?
- What about system-based density?
- Odds of this being possible are not great anymore, which detracts from the alignment rating.
- Targets for carbon materials seem to be falling short of DOE storage targets.
- Hard to be positive about this project. Most observers have little or no confidence that carbon-based materials will come close to the DOE targets for "system weight," "system volume," and "net" energy delivered. This is, in effect, a last chance for carbon.
- Unclear at this point how this work will ever achieve DOE goals. This is basic science work that should probably be in the BES portfolio, but probably not in EERE focused on achieving more applied goals.

## **Question 2: Approach to performing the research and development**

This project was rated **2.73** on its approach.

- Fundamental approach to understand adsorption/desorption mechanisms and apply to materials design is good.
- Measurement technique development and independent validation of storage results are extremely important to success of project and technology transition.
- Consideration of all carbon-based materials is good.
- There is a significant discussion of measurement accuracy and analysis.
- All the characterization work and lab verification work is top notch. The problem is that with good technique they do not make good material.
- Focus on TPD is a good idea.
- Computational studies should provide useful insights, e.g., about consequences of molecular strain.
- Looks good.
- Effort has broadened from earlier work.
- The researchers in this area are clearly top caliber and creative.
- Accuracy of measurement seems to be established.
- It had to take outside influence to address the problem.
- The technical approach has been good but apparently has recently been confined to confirming past results.
- Not clear in this project presentation on how it will get to 4 and 6 wt%.
- Intrinsic cost of nanotubes may be a "show-stopper."
- No evidence this will be technically feasible.
- There has been an avoidance of making samples that could be externally verified if successful. While tedious, in an environment where most scholars doubt the effect is real, this is a requirement.
- Some of the critical parameters for hydrogen sorption behavior in SWNTS could have (and should have) been identified earlier in the program; for example, the stochastic nature of the alloy doping. The approach toward addressing these issues is now on the right path forward and have confidence that significant fundamental understanding will be achieved.
- Real focus needs to be reproducible production of large (gram) quantities of material that adsorb significant quantities of H<sub>2</sub>.
- Should also verify production of "good" nanotubes using low cost synthesis method.
- Laser synthesis will never make cost targets.

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

This project was rated **2.54** based on accomplishments.

- Good validation of measurement reliability.
- Recommendation of low T/ low P options for new materials a great idea.
- Computational focus excellent, and interesting initial results for Fe role in H<sub>2</sub> spillover.
- The reproducibility is addressed. The results are disappointing.
- It is critical that results (especially claims of 8 wt-% storage) be reproduced and independently verified.
- Work done by the lab and the PI appears to be technically sound and rather rough, but the need for reproducibility and independent verification is critical.
- While equipment accuracy appears to be well-established, project is still yielding less than 3 wt%.

- Arguments on technical credibility of the measurements and reproducibility are not entirely convincing.
- Not much technical progress since last year given funding level. Lab review was essential, but not lengthy.
- Calculations are interesting though not yet compelling for explaining SWNT results.
- Now that the analytical capabilities have been validated and the reported results for hydrogen capacity are credible, the important materials-related efforts in this program can now proceed with diligence.
- Good work in resolving measurement issues.
- Need to go after materials synthesis in earnest.
- Need to make better progress in the area of reproducible synthesis.
- The role of catalysts/dopants needs better understanding. Is any of the information from the alanate work transferable to carbon?
- Still do not understand conditions for high capacity H adsorption on nanotubes.
- Demonstrating the reliability of their measurements was key; however, it is disappointing that all samples where reliability was demonstrated fall at or below the storage level of the alloy itself.
- How long has it been since the NREL group has made a sample that stores more than 4 wt%? Their milestone to "reproducibility" demonstrate this capacity or greater by FY 05 seems to be a crucial point at which a decision about the viability of this topic should be made.
- Why 2 of 9 samples showed high H<sub>2</sub> intake in SWNTs?

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

This project was rated **3.08** for technology transfer and collaboration.

- Broad range of university collaborations with disparate opinions on best methods to make nanotubes with desired properties.
- Excellent leadership in organizing symposia.
- If successful, industry collaborations will occur to transition technology.
- Extensive collaboration through debate and discussion with other activities trying to reproduce or verify results obtained by the PI.
- Appears to be highly collaborative with academia and other National labs, but not with industry.
- If this project is to go further, it should also start to address how the technology could be transferred to industry.
- Alignment and collaboration adequate.
- Since the capability for synthesizing and catalyzing SWNTs is fairly unique to NREL, collaboration at this level is understandably weak.
- However, it is apparent that a substantial amount of outside collaboration has transpired relative to analytical measurements, theoretical chemistry, and characterization of materials.
- Sharing of candidate SWNT materials for verification of sorption behavior needs to occur.
- Great utilization of external resources.
- Many collaborations.
- Spearheading "carbon materials working group" and closely tied to SwRI - that's a plus.

#### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.62** for proposed future work.

- Specific follow on areas identified for each class of materials they are working in based on last year's results.
- Plans seem reasonable, but future progress will depend mostly on ability to repeat and then independently verify past claims/results.
- The current plans are not justified by current results.
- It is not clear how the future research identified will meet targets.
- Put more focus beyond carbon nanotubes.
- Tests are nicely planned, but the focus on reproduction seems weak: this needs to be the over-riding focus.
- New focus on cryo is interesting and should also be pursued as the secondary line of work.
- Technical challenges are now clearly understood and a plan to move forward with the business of materials science has been outlined.
- Other methods of catalyzation need to be explored.
- Hopefully, TPD development work will be a reliable screening tool.
- Would like to have heard more about what NREL has in mind for research on "other" carbon-based materials, such as MOFs.
- Could be speeding up.

### **Strengths and weaknesses**

#### **Strengths**

- Good science and team at NREL.
- Top notch team.
- Hydrogen storage center award- good news. Will be great to finally get independent validation of results reported by various groups for storage.
- Storage under mild conditions (low PT).
- Leading-edge research to develop advanced nano-scale materials for solid state hydrogen storage.
- Experimental approach should provide sufficient data to gauge whether or not SWNTs will be efficacious in meeting DOE performance goals.
- Technical expertise.
- Validated methods.
- Strong collaborations.
- Interactions with other researchers in the field have increased the confidence level in the NREL findings.
- Quality lab.
- Good strong team.
- Computational calculations of hydrogen spillover on carbon are generally good; but materials are too heavy and bulky for storage.
- High funding.
- Have responded to many recommendations.
- Very large effort.
- Program has already changed direction away from nanotubes (SWNT).

#### **Weaknesses**

- Lots of work going on in this program, but it was barely covered since so much time spent on other required slides (safety, tech barriers and targets, etc.). Need to streamline so more results can be presented.
- Failed material.
- Little or no potential for significant improvement.

- Project presentation is not clear how it will meet targets for wt%.
- Intrinsic material cost may be too high.
- Storage capacity is too low for a commercially viable device.
- Need to show the spill over work actually applies to SWNT.
- No progress on reproducing samples that store meaningful amounts of hydrogen when most of the world no longer believes this is a real effect.
- There seems to be a reluctance to face the fact that at some point it makes sense to pull the plug.
- Will the 4% externally verified go/no-go be enforced?
- Challenges are fundamental in nature, yet goals are very applied.
- Need clear path to reproducible, affordable nanotubes with good adsorption properties.
- The goals for "system weight," "system volume," and "net" energy delivered are too low.
- Little progress for such a long term program.
- Still no correlation between hydrogen uptake and material method or material structure.

### **Specific recommendations and additions or deletions to the work scope**

- The go/no-go decision should be moved to 4Q '04.
- 4 wt% capacity as a target is inadequate - should be revised to at least 8 wt% for any chance of success.
- Try to get industry involvement in collaborations.
- Scope should be refocused beyond carbon nano-materials.
- The program should be stopped as soon as possible.
- Need to list what a system based on carbon materials would contain (including masses and volumes).
- Cost needs to be assessed.
- Focus totally on making a sample others can measure 4% storage in.
- Cryo work is an appropriate addition.
- This would be much better funded by NSF/BES where the horizon is longer and the deliverables need not be so focused. See Special Report of DOE-EERE written by SwRI: Carbon Nanotube Sorption Science - External Review.
- Don't burn up too much of your research base on the Yaghi-type MOFs, they only look interesting on paper (volumetric storage goals will be hard to meet).

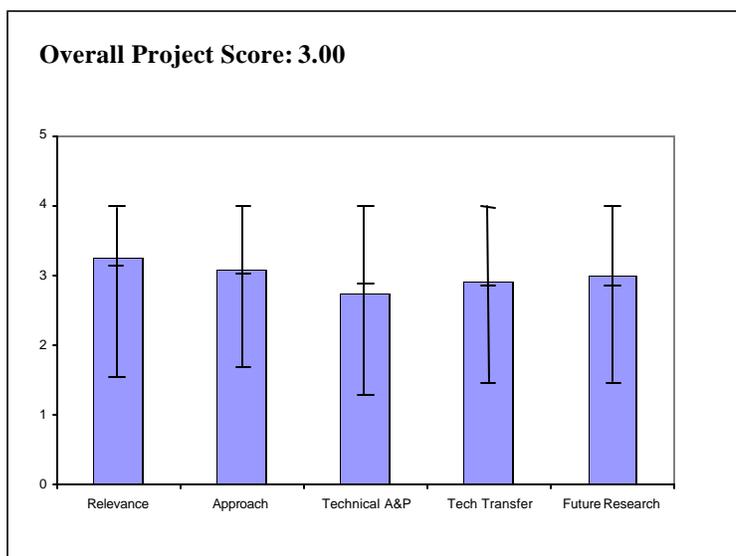
## Project # ST-14: Standardized Testing Program for Chemical Hydride & Carbon Storage Technologies

Page, Richard; Southwest Research Institute

### Brief Summary of Project

In this project, Southwest Research Institute (SwRI) will develop and operate a standard testing and certification program aimed at assessing the performance, safety, and cycle life of emergent solid state (e.g., metal hydride and carbon) materials and systems. As part of this project, SwRI will work with industry, DOE and other stakeholders to develop an accepted set of performance and safety evaluation standards.

### Question 1: Relevance to overall DOE objectives



This project earned a score of **3.25** for its relevance to DOE objectives.

- The relevance of such an activity is quite important to the overall DOE objectives. This project should facilitate quick, unbiased measurements of the sometimes contradictory storage claims. Also, it will enable "materials discovery" labs to concentrate on discovery, without having to build the equipment for measurement capabilities.
- Important project if solid state hydrogen storage is considered to be the ultimate solution.
- Having the ability to independently test materials in the program is essential.
- Establishment of facility serves as a safeguard against catastrophic distractions caused by "false positives."
- Possibly the most critical program in the portfolio. This has to be right and it has to be good enough so that when samples are judged as high or low capacity there is faith in the external community that this is correct. Hard to administer the program without a good testing facility.
- Testing program represents a necessary facility.
- This project will provide the Hydrogen Program with the credibility it needs to prove real progress toward meeting H<sub>2</sub> storage goals and targets. Need to keep duplication of capabilities with the Centers to the minimum without impeding progress at either location.
- Independent verification is important.
- Support role in storage development.
- Some overlap with existing capabilities.
- Test and evaluation only. There is no innovation which would lead to meeting goals.

### Question 2: Approach to performing the research and development

This project was rated **3.08** on its approach.

- An excellent set of equipment has been assembled.
- Appears that necessary equipment is or will be installed soon to test solid state storage systems.

- Communication with solid state researchers important to identify standard protocols.
- The project should de-emphasize the opportunity to do science since their charter is to support the program impartially.
- Competent scientists and high quality instrumentation - adequate but "nothing special."
- Approach is on target.
- Good instrumentation has been acquired but the SwRI staff needs to acquire the practical level of personal expertise that's needed for effectively exploiting these capabilities.
- A well planned out program that is structured to set the standard for measurements of reversible H<sub>2</sub> storage capacity and "net" energy. This program will be the adjudicator of all claims that new vistas in hydrogen storage/delivery have been reached.
- Quality instrumentation.
- Good combination of instrumentation.

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

This project was rated **2.75** based on accomplishments.

- Many industrial sites have recently brought similar facilities online in much less time.
- Making progress in facilities but no technical contribution made.
- Most equipment is already in place, though not all.
- Seems like progress is good toward establishing the facility.
- Wish the progress was faster.
- Hard to evaluate as major deliverables are at the end of the year.
- Good appropriate instrumentation has been acquired for getting H<sub>2</sub> sorption/desorption data. It is, however, disappointing that almost no thought has been give to getting H<sub>2</sub> adsorption heat data.
- Making good progress, but they should be. They are generously funded by DOE with a mere 20% cost share.
- Still more equipment needed to be installed.
- Good progress has been made on equipment procurement and installation.
- Progress is modest and in the right directions, although somewhat slow and not particularly impressive.

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

This project was rated **2.92** for technology transfer and collaboration.

- Meeting with Centers of Excellence. They should visit many more laboratories.
- Suggest interaction with industry in addition to academia and other government labs to transfer procedures and protocols for use development of solid start storage technologies.
- By the very nature of the project, tech transfer and collaboration have been, and will be, good.
- Efforts are being made to obtain input from all storage groups in the program.
- Highly important program.
- Collaboration with other testing organizations might be improved but adequate.
- The SwRI people have done a good job in visiting the best lab that currently does H<sub>2</sub> adsorption measurements.
- Seem to be working closely with the bigger programs funded by the Hydrogen Program. Have learned from leading experimenters with state-of-the-art facilities.
- Round-robin good.

- More interaction and collaborations would be very helpful to this effort as well as the program in general.
- Once this facility is online, this will be the single most important topic for this facility - collaborations with other laboratories.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **3.00** for proposed future work.

- Testing to begin this summer. They need to get a lot of testing under their belt to be able to identify appropriate test methods.
- Consider doing some correlation with test facilities at research providers.
- Question the effort to test a full scale system if this work is being done at UTRC too.
- Seeking to establish credibility.
- The SwRI lab operations staff may wish to once again visit and learn from the labs, especially after they have had some hands on experience with their current instrumentation.
- The plan looks good at this time. Assess where they are a year from now to make a more definitive judgment.
- Good plan.
- Well defined program of future work.

### **Strengths and weaknesses**

#### Strengths

- Facilities - excellent.
- Independence.
- Provides a much needed independent testing source for verification of solid state storage systems.
- SwRI has the expertise and personnel to provide this function to the DOE program.
- Solid state measurement of hydrogen storage.
- Facility seems to be adequate to carry out envisioned function.
- Much experience and diverse talent at site.
- Well equipped to do the work.
- Will do quality measurements.

#### Weaknesses

- Should lead in developing test protocols.
- Further development of technical expertise.
- The breadth of the effort may require that the focus be limited to capacity measurements. If the program moves to all apparatuses, and does difficult measurements (like TGA and TPD), the throughput will be slow.
- Can't be used for other forms of hydrogen storage measurement.
- Value to program is not yet established.
- New to this branch of testing.
- Achieving the 2 minute H<sub>2</sub> refueling goal requires for an H<sub>2</sub> adsorbing carrier, a very rapid transfer of adsorption heat. It's critical that there be the means to measure this heat of H<sub>2</sub> adsorption.
- From R. Page's talk it seems that the SwRI folk need to put more thought and planning on measuring heats of H<sub>2</sub> adsorption at least for carrier materials.

### **Specific recommendations and additions or deletions to the work scope**

- Must focus on getting highly dependable results every time. Fast start is also critical. Balancing these two items will be important; much attention must be paid to this.
- Once again visit and learn from other labs (including industry), especially after having had some hands on experience with their current instrumentation.
- Communicate test procedures and protocols with industry.
- Need to ensure that the lab will be capable of expanding and adapting as storage technologies advance.
- SwRI should become the testing methods standards.
- Consider expanding this to test for liquids or other type of hydrogen storage materials.
- The project should reduce its desire to do science with the very nice equipment they will have, and focus more on testing.
- This project must not fail.
- The H<sub>2</sub> storage community does indeed need a rapid measurement tool (e.g., a litmus test) that at least roughly characterizes the gravimetric and volumetric storage capacity and the reversibility properties of candidate storage materials to keep up with, e.g., combinatorial synthesis efforts throughout the DOE sponsored enterprise. Ensure that there is a rapid measurement tool.
- Integration with the center needs to be established.
- It is more important for SwRI to concentrate on materials rather than systems at this time.
- Add capability to test chemical storage.
- Add calorimetry. Add plan for measuring heats of H<sub>2</sub> adsorption.
- Define throughput goal - samples/month.

## Project # ST-P1: Next Generation Physical Hydrogen Storage

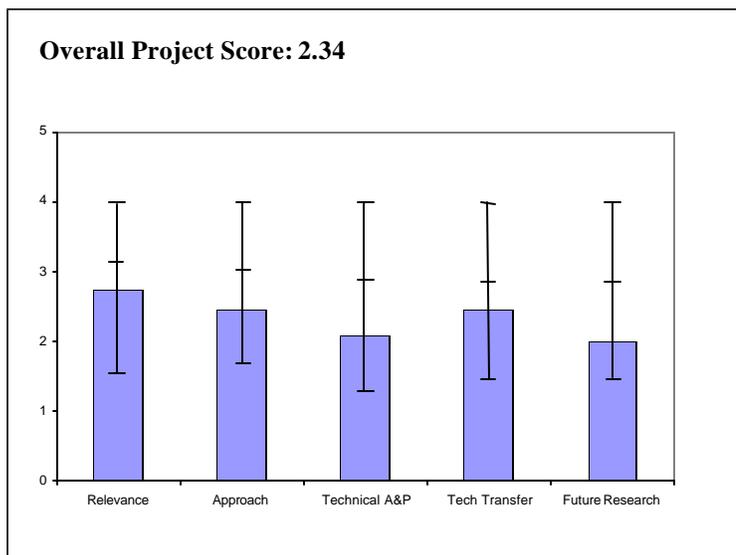
Weisberg, Andrew; Lawrence Livermore National Laboratory

### Brief Summary of Project

Lawrence Livermore National Laboratory's (LLNL) project goal is to develop conformable hydrogen tanks that will meet DOE targets. Conformable tanks have the potential to optimally utilize available space in a vehicle and thus greatly improve volumetric efficiency. In this project, LLNL is investigating pressure vessels made of replicants. **This is a new project, started in 2004.**

### Question 1: Relevance to overall DOE objectives

This project earned a score of **2.75** for its relevance to DOE objectives.



- Innovative approach to making lighter/stronger material for vessel.
- Not clear how it would apply to H<sub>2</sub> storage.
- I could not understand why statistical process control was used. The subject is to come up with the next generation hydrogen storage system.
- Information was disorganized in the slides.
- Replicants concept seems to be a good one.
- No evidence it will work, but may be worth trying.
- Work on statistics and overall system (includes synergies with vehicle) quite good. Novel ways to lighten and cheapen tanks also very on target.
- Would benefit from clearer presentation and explanation of project.
- The project is quite relevant in that it may provide the basis of a more conformable hydrogen storage system that could utilize the available space within a vehicle to much better advantage than do cylindrical tanks.
- A chance for innovative ideas.
- Relevant to conformability improvements for compressed gas storage.
- This work is of questionable relevance to DOE's objectives.

### Question 2: Approach to performing the research and development

This project was rated **2.44** on its approach.

- Novel approach.
- Too many visual aids without addressing how to resolve the technical barriers.
- Novel approach is to be commended.
- Use of statistics is good and not seen in other programs.
- Need to exhaustively evaluate all space groups is probably not the most efficient.
- Really needs to find a partner to increase the physical data portion of this work so that more grounding of the theory can be accomplished.

- Theoretical approach seems to be giving good insights into potential new storage approaches.
- Novel shapes are being considered. The use of replicant structures should be cost-effective - many similar parts that can achieve volume manufacturing economy of scale. Analysis techniques appear robust and are drawn from numerous fields. A big question is how the skin will be attached to the skeleton. The materials of construction will determine the ultimate cost of the system. Internal structure does compromise the volume density but may be partially offset by the gains in conformability.
- Lots of ideas. Not enough money to pursue many.
- Manufacturability, bonding of materials and cost may kill these innovative ideas.
- No apparent improvement in volume, weight density except for conformability.
- Creative idea, but need to focus more on the utility and applicability of this idea, rather than the theory of it.

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

This project was rated **2.07** based on accomplishments.

- By the presentation material, it wasn't clear what has been achieved beyond theoretical contemplations.
- Concepts need to be proven.
- Fairly good progress on theory, fairly little progress on confirming the theory by tests.
- Harder to gauge with theoretical work than experimental. Some advances over last year, but hard to quantify and understand.
- Budget is so small so great progress is not expected. However, it is time for some prototypes to be built and tested.
- Hasn't started.
- Needs a direct demonstration of fabricating a tank and testing it. Plastic models don't help much.
- The lack of experimental progress in this area is disturbing.

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

This project was rated **2.44** for technology transfer and collaboration.

- Appears to have wide contact.
- Very little active collaboration but potentially very important program.
- Needs to explore possibilities of small devices to verify theoretical concepts. Reduction to practice may eliminate some of the concept's advantages.
- Collaborations with universities are underway to attempt to increase academic research in the topic.
- Some universities.
- PI understands importance of collaborations but hasn't launched an effort yet.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.00** for proposed future work.

- Needs specific goals to address 2010 H<sub>2</sub> storages.
- Confusing.
- Needs clear and concise "roadmap" to project how work might actually be commercialized.
- Build and test prototypes should be the primary objective of future work.

- Too early.
- The creative new idea is there (and has been for some time); the crucial point is to try and make it work. Is there any benefit to this idea?

### **Strengths and weaknesses**

#### Strengths

- Great visual aids.
- May result in a neat concept for hydrogen storage.
- Novel approach.
- Strong theory background.
- Novel concept with significant potential for compressed storage. Well grounded in theory.
- Smart, innovative PI.
- Innovative. Very different approach to pressure tank fabrication.

#### Weaknesses

- Need to address practical (cost, manufacturing ...) issues as well.
- The poster slides were hard to follow.
- High risk.
- Budget slide unclear.
- Almost no confirmation work to date.
- Not clear this approach can be implemented in a manufactureable tank, the attachments of thousands of struts to each other and to the tank wall will be a huge challenge.
- Ideas not presented clearly.
- Collaborations appear to be with universities.
- Not focused (but this may be good if you want innovation).
- Many uncertainties in proposed tank fabrication.

### **Specific recommendations and additions or deletions to the work scope**

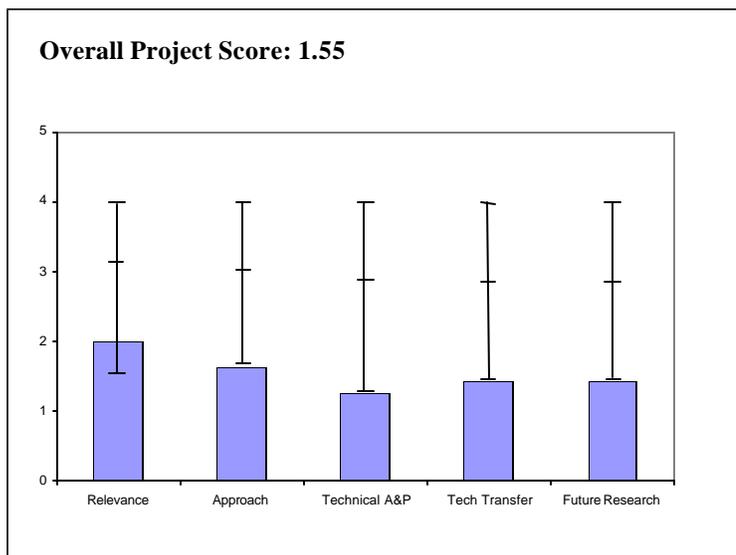
- Cost is the key in these carbon tanks, and they need to stay focused there.
- Tank manufacturers should be encouraged to interact with LLNL to develop plans and concepts for prototyping these structures.
- Would benefit from some early experimental work to confirm concepts and identify practical barriers to implementation.

## Project # ST-P3: Fuel Cell and Hydrogen Research

Stefanakos, Lee; University of South Florida

### Brief Summary of Project

This multi-faceted University of South Florida project will investigate several hydrogen production techniques: hydrogen storage in the areas of transition metal hydrides; nano-structured materials (nano-composite conducting polymers), and components designed for manufacturability; electrode improvements; water and thermal management; and a testing facility for fuel cells; and a demonstration project using solar power to generate hydrogen for storage and conversion. **This is a new Congressionally-directed project, started in 2004.**



### Question 1: Relevance to overall DOE objectives

This project earned a score of **2.00** for its relevance to DOE objectives.

- For the hydrogen storage part, it was not clear what they are doing. No answers to the questions given.
- Much of the money in this storage program goes to production and use, which does nothing to further storage.
- Traditional hydrides are highly unlikely to meet anything but the 2005 goals.
- Given the late 2004 start, that is aiming too low. Conductive polymers are dubious but worth diagnostic study – it is not clear this study will really tell if the class has a future.
- Packaging research is relevant.
- Looking at several relevant projects. Many are unrelated to storage. Production projects should be funded in that program - not storage.
- $MgFe_2H_6$  is "well-plowed" ground. What new is the team going to do?
- This project has components that by title alone are relevant to the DOE effort. However there is no coordination evident with any of the DOE projects.
- Collection of about 8 individual projects at various institutions.

### Question 2: Approach to performing the research and development

This project was rated **1.62** on its approach.

- Not clear what they are doing and how the barriers are addressed.
- Little information.
- Storage approach appears not to comprehend FreedomCAR goals.
- The material of choice  $MgFe_2H_6$  has effectively no chance of meeting the system goal of 2 kWh/kg.

- The methods to improve performance were not well understood by the presenter, and catalysts seem to be essentially a guess based on weight or benefits in the alanate system, which is functionally quite different.
- The polymer work seems only likely to work at low temperatures, but this was not in the plan as stated.
- Project needs focus. Dollars spread among lots of unconnected projects. Work appears to duplicate work going on in other DOE programs. No indication of how these projects differentiate themselves from others.
- The approach is poorly defined and lacks detail. It appears to be all things to all people.
- Management challenge.
- Excellent PI and lead at other university.
- Project way too broad.
- Storage materials approach not likely to lead to improved performance.
- Some of proposed storage materials have been thoroughly studied previously.

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

This project was rated **1.25** based on accomplishments.

- Not sure if any work was done.
- Little information.
- Program not yet started.
- New Project. No work done yet.
- No results were presented.
- Not yet started.
- New project.

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

This project was rated **1.43** for technology transfer and collaboration.

- Significant partner network planned, the reality is unknown as the work has not started.
- Appears to be little or no plan for collaboration outside this group of universities.
- Collaboration outside of the Florida schools is not evident. They appear to be operating in a vacuum.
- Not yet started.
- Only interactions are within group partners.

**Question 5: Approach to and relevance of proposed future research**

This project was rated **1.43** for proposed future work.

- The outline plans are unclear.
- Too wide.
- No focus.
- Lack of specificity.
- Planning is complete but not compelling.
- Much too much of the storage money is planned for non storage work.
- System goals unlikely to be met.
- Vague statements about future research that relates to the DOE targets and objectives.

- Not yet started.
- Examining some potentially good material candidates but low probability of success.

### **Strengths and weaknesses**

#### Strengths

- Partner network is good.
- Some experience from prior NASA H<sub>2</sub> work.

#### Weaknesses

- Lack coherency and focus.
- Too wide an objective and scope.
- Weak partnership.
- Low chance of meeting goals with systems planned as stated.
- Too many projects. Little focus. Don't know about expertise of PIs in this area. Projects unrelated to storage.
- Scope of overall project huge partners in project likely not large enough to complete even one part and certainly not all parts.
- Poorly defined program.

### **Specific recommendations and additions or deletions to the work scope**

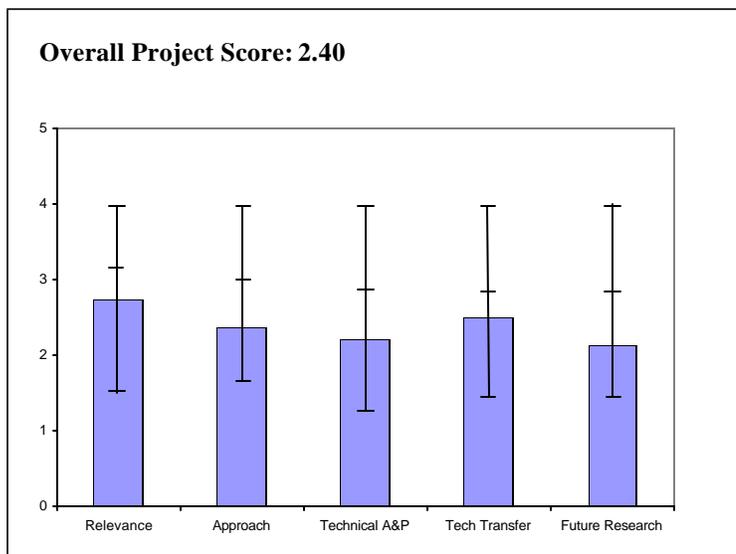
- Needs to narrow scope.
- Needs to be rethought and integrated into the overall plan with objectives more aligned with storage and more likely to succeed.
- Focus project. Eliminate the production work and collaborate with experts in storage.
- Allow them to choose balance of projects (DOE wants them to focus mainly on storage - but they have limited experience there compared to your other storage contractors.)
- Choose one strong area and focus on it. Production? Fuel cells? Demonstration? Storage: No!

## Project # ST-P4: Development of Complex Hydride Hydrogen Storage Materials and Engineering Systems

Ritter, James; University of South Carolina [Ralph White overall project PI]

### Brief Summary of Project

In the area of engineering systems research, the University of South Carolina will develop 1-D, 2-D and 3-D models for metal and complex hydride hydrogen storage systems, calibrate models using Savannah River National Laboratory's (SRNL) metal hydride hydrogen storage system, and develop a user friendly software package for metal hydride hydrogen storage system design optimization and scale-up. In the area of materials research the group will: study the effect of metal dopants, proprietary additives, and Al powder on dehydrogenation and rehydrogenation of  $\text{NaAlH}_4$ ; compare dehydrogenation kinetics of un-doped and Ti-doped  $\text{NaAlH}_4$ ,  $\text{LiAlH}_4$ , and  $\text{Mg}(\text{AlH}_4)_2$ ; and initiate Raman and other spectroscopic and molecular modeling analyses for fundamental understanding of dopant and other additives. **This is part of a new Congressionally-directed project funded in 2004.**



### Question 1: Relevance to overall DOE objectives

This project earned a score of **2.75** for its relevance to DOE objectives.

- How is this project different from others?
- Both improvement of materials and improved engineering are important goals. The work seems to trail others in the field. It will be important for this program to be interlaced with the Sandia based Center so that this work is focused on objectives not covered already (UTRC contract) or concurrently.
- Modeling work looks interesting. Not sure whether experimental work duplicates other DOE work.
- Reversible metal hydride systems do not meet the DOE performance targets. New materials are needed.
- Some good modeling and hydride characterization.
- Focus on complex hydrides is good but other work in this area has better chance of success.

### Question 2: Approach to performing the research and development

This project was rated **2.38** on its approach.

- Not sure what's new or innovative. The alternate complex metal hydrides are not very promising.
- The work outlined has been done elsewhere in part, and is planned in a much deeper and broader fashion in the Complex Hydride Center. The work is adequately designed, but is not as ambitious as

other work in the same field. Engineering effort is attacking more of the correct issues (predicting, measuring, using transfer properties) but the geometry is not very ground breaking.

- Interesting results for additive, but need more info. If patent is pending, why isn't composition given. IP should be safe if patent application has been filed. Model for actual devices is good tool. Should be made available to researchers in other groups.
- The approach lacks detail. Much appears similar to the work at Sandia National Labs.
- Some prior experience from DOD project.
- Combined theory- experiment is good. Addresses both material synthesis/performance and engineering properties.

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

This project was rated **2.21** based on accomplishments.

- Comparatively not much progress (as compared to others in the field).
- Progress good for the amount of manpower applied, the challenges have not been especially stiff to date though. Progress reported is not cutting edge any more.
- Good results on additive and model.
- Not a lot of funding. Confusion as to when the project began-slides indicate 06/06. Assume it is 06/04.
- Hasn't started yet.
- Good modeling effort on tank design.

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

This project was rated **2.50** for technology transfer and collaboration.

- Good contact with National Lab and other fields.
- Suitably connected and aligned with goals.
- Reasonably good collaborations with the notable lack of interaction with Sandia.
- Good list of collaborations. Haven't started yet.
- Good list of collaborators.
- Need to insure that the modeling and other work is integrated into existing projects in this area.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **2.14** for proposed future work.

- It is not addressing the important issues and practicality of the proposed storage system.
- Again, the work proposed is already underway elsewhere. The plans are suitable, they simply are not new.
- Would like to see results from systems design using model. Info on additive should be made public.
- Vague statements for future plans.
- Too early.
- Proposed research overlaps other efforts. Modeling of Al foam insert not generally relevant.

## **Strengths and weaknesses**

### **Strengths**

- Team seems to understand the problems. Team is doing good work
- Heat and mass transfer model. Needs to be made available to others.
- DOD experience.

### **Weaknesses**

- Work is not adding much to the knowledge base, though it does confirm the results of others, which has some value.
- Not enough information given on additive to make judgment.
- Not chosen by competitive solicitation.
- Hasn't started.
- Overlaps other complex hydride projects.

## **Specific recommendations and additions or deletions to the work scope**

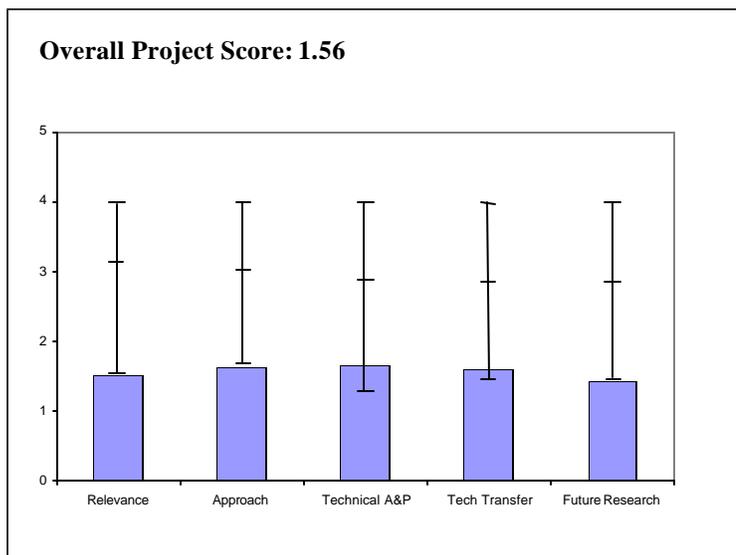
- See if some of the effort could fit in the Sandia National Lab center effort.
- Coordinate with hydride center or be part of the center.
- The heat management models are a unique and important contribution to DOE's research. This modeling work should be encouraged, and DOE should insure that these models will be publicly available to other researchers during and after the duration of this project.

## Project # ST-P5: Advanced Manufacturing Technologies for Renewable Energy Applications

Ryan, Chuck; National Center for Manufacturing Sciences

### Brief Summary of Project

Working with DOE and the private sector, the National Center for Manufacturing Sciences (NCMS) will (1) identify and develop critical manufacturing technology assessments vital to the affordable manufacturing of hydrogen-powered systems and (2) leverage technologies from other industrial sectors and work with our extensive industrial membership base to do feasibility projects on those manufacturing technologies identified as key to reducing the cost of the targeted hydrogen-powered systems. **This is a new Congressionally-directed project, started in 2004.**



### Question 1: Relevance to overall DOE objectives

This project earned a score of **1.50** for its relevance to DOE objectives.

- At present the program has essentially no goals other than to find some work that might be worthwhile to do, and then do it. As such it is impossible to say of what value the program will be to anyone.
- The relevancy with H<sub>2</sub> storage is missing.
- As a hydrogen storage program this project will not be economical in that much of the money will be spent on fuel cells and other, non-storage research. This funding should come through fuel cells, not hydrogen storage.
- This process is essentially duplicative of the DOE program and wastes effort in that duplication. It also circumvents the peer review process in funding selection.
- Little relevance to DOE programs. No storage focus.
- Assuming fuel cell developers have manufacturing issues and are willing to share them with NCMS, there may be some relevance to the DOE program. However, there is no guarantee that the developers will interact with NCMS.
- Cost is ultimate issue.
- Redundant to DOE program management.
- Huge amount of funding for a project with no connection to DOE hydrogen storage goals.

### Question 2: Approach to performing the research and development

This project was rated **1.62** on its approach.

- Not clear what is considered as barrier with objectives.

- While the technical aspects of the approach can not be appraised since the work is not laid out yet, they are stating that they will seek input on the areas needing research and then do work there, which is at least good.
- Design and feasibility are unknown, and again, no program should be funded that has so little definition beforehand.
- No prior H<sub>2</sub> expertise.
- Likely spending money to train recipient rather than generating useful information.
- The approach is similar to past efforts at NCMS and depends to a large extent on the skill of NCMS to develop projects that will attract investment from the private sector in projects attacking common problems.
- Project has not started.
- Collaborations and components/systems have not been selected.
- Proposed work has already been done.

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

This project was rated **1.67** based on accomplishments.

- No accomplishments because they have not started, but in addition at this point they have no programs defined.
- New project.
- The project is due to begin on July 1.
- Project not started.
- No work has been done.

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

This project was rated **1.60** for technology transfer and collaboration.

- Again, there is little clear direction at this time. It appears many teams will work in a coordinated plan, that aspect is good if it is fulfilled
- None yet.
- Yet to be determined.
- Project not started.
- Project is simply one of coordination; not collaboration.

### **Question 5: Approach to and relevance of proposed future research**

This project was rated **1.43** for proposed future work.

- Hard to judge what the plans are.
- Essentially no plans at present.
- Not obvious that recipient has skills, relationships, or experience needed. Don't see how this fits into storage portfolio.
- Needs to be defined.
- Proposal does some of this.

## **Strengths and weaknesses**

### **Strengths**

- Large group of players, and much industry participation, which could lead to development of a supplier base.
- Good firm - lots of manufacturing experience.
- Turned in safety 1-pager.

### **Weaknesses**

- Difficult to place this program in context of H<sub>2</sub> storage.
- No plan.
- No obvious expertise in the area of hydrogen.
- Hydrogen storage money is apparently planned to be spent only in part on hydrogen storage.
- The project seems a bit premature. The fuel cell industry is still trying to develop technology that can demonstrate the durability required for the intended applications. When they do, the technology may well look considerably different than envisioned today. Manufacturing solutions developed today at NCMS may not apply to the technology that is ultimately fielded.
- This project does not help the hydrogen storage team reach its targets.

## **Specific recommendations and additions or deletions to the work scope**

- Need to narrow the scopes on barriers or develop infrastructure.
- Use existing DOE assessment of the needs, avoid the redundancy of a second appraisal of needs.
- This program should be funded only after it has a clear set of projects that meet DOE goals if they are fruitful.
- Get OEM auto manufacturers involved.
- Delete entire project.