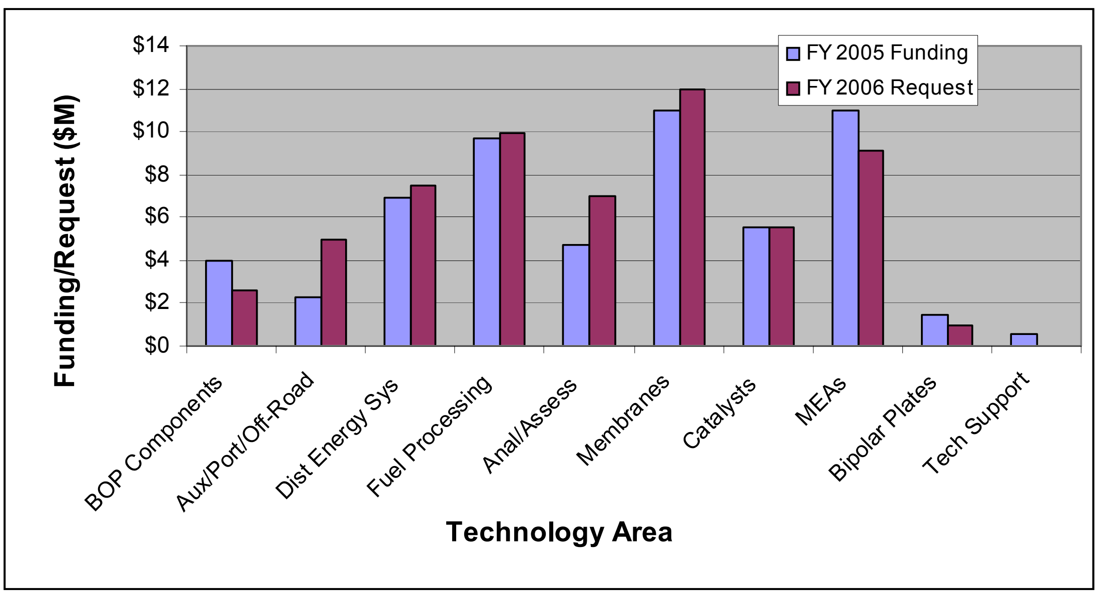
Fuel Cells
Summary of Annual Merit Review Fuel Cells Subprogram

Summary of Reviewer Comments on Fuel Cells Subprogram:

Reviewers consider fuel cell development to be a critical enabling technology for the success of the President's Hydrogen Fuel Initiative. Overall, the R&D portfolio was judged to be well managed, appropriately diverse, and focused on addressing technical barriers and meeting performance targets. Progress was considered good. As in 2004, reviewers emphasized the use of go/no-go decisions as part of the projects' work plans. Reviewers also requested that presenters show progress against the targets as they are identified in the Multi-Year Research, Development, and Demonstration Plan. Another recurring comment was that limited disclosure of technical data for some projects made evaluation a challenge.

Fuel Cell Funding by Technology:

The Fuel Cell Technology Subprogram continues to refocus in response to the National Research Council's recommendation to increase government funding on R&D dedicated to the research on breakthroughs in fuel cell materials for durability. Cost and durability of stack components i.e. membranes, catalysts, bipolar plates, membrane electrode assemblies, etc. continue to be a key focus of the subprogram.



Majority of Reviewer Comments and Recommendations:

In general, the reviewer scores for the fuel cell projects were high to average, with scores of 3.85, 2.93 and 1.91 for the highest, average and lowest scores respectively. The scores are indicative of the technical progress that has been made over the past year. Key recommendations are summarized below. DOE will act on reviewer recommendations as appropriate for the scope and coherency of the overall fuel cell research effort.

- **Membranes –** Interest continues in developing membranes that operate at higher temperatures and lower relative humidity. Improved understanding of degradation mechanisms, cyclic stresses, and durability are needed. The developers of innovative membrane concepts need to partner with industry to implement the new technologies. Additionally, membrane cost needs to be considered throughout the life of the development. Acid loss and degradation in acid-based membrane systems for high temperature applications has not been adequately addressed.

- **Catalysts –** The research in the catalyst area is well-balanced between fundamental understanding and development of new catalyst materials. Researchers report promising results for low platinum catalysts. Non-platinum catalyst research remains challenging. Researchers are encouraged to perform rotating ring disk electrode testing and compare all results against the DOE targets for electrocatalysts. Developments in non-carbon catalyst supports, i.e. ceramics and bimetallics, are encouraging and should continue.
- **MEAs -** Reviewers commented that Principal Investigators (PIs) should better integrate theory and experimentation, should collaborate more with fuel cell companies, and should publish more of their work. Reviewers also stated the importance of research protocols that incorporate cyclic test conditions and down-selection steps for manufacturability. Nanoscale pre- and post-mortem in-situ analysis of MEA components will be key to a better understanding of a fuel cell's failure mechanisms.
- **Bipolar Plates –** The Porvair carbon plate project met all of DOE's technical targets and assuming sufficient volumes the cost targets can be met. A 20-cell stack was demonstrated with performance equal or better to machined graphite. ORNL, NREL and PNNL are looking at metal plate surface modification, corrosion protection, and cladding. These projects are too early in the research stages to determine if they will show promising results. Cost-effectiveness of proposed approaches were questioned by reviewers. ORNL has good initial results with Ni-Cr and should focus on more cost-effective Fe-Cr next year.
- **Characterization, Evaluation and Analysis –** The analysis activities are providing valuable insights into fuel cell operation, especially characterizing behavior that leads to performance decay and failure. Fuel cell manufacturers need to supply more experimental data to validate the models, and the PIs need to make the results of their work publicly available. More emphasis is needed on post-mortem analysis.
- **Distributed Energy Systems –** The key benefit of the distributed energy fuel cell activity is in cell stack assembly development and testing where improvement in stack durability and low cost components can be applied to all fuel cell development efforts. DOE should focus on the development of small prototype systems. Only after these projects meet scale-up milestones on system operation, efficiency, and durability should the building of full-scale systems be considered. If performed, economic and life-cycle analysis should be coordinate very closely with Original Equipment Manufacturers.
- **Portable Power –** Reviewers stated that portable technology is not applicable to transportation applications (the key focus of the Hydrogen Program) and should be de-emphasized. Work on DMFC technology, if continued, should focus on membrane and MEA development, specifically addressing durability, performance gains, and ability to function in different methanol concentrations.
- **Auxiliary Power –** The development of auxiliary power units for heavy duty vehicles has a weak tie to the overall Hydrogen Program goals. Concern remains over the ability of the ceramics within a solid oxide fuel cell system to withstand the vibration of a heavy duty truck. The projects in this area should be better coordinated to avoid duplication.
- **Fuel Processor R&D –** The fuel processor R&D area remains in a state of transition as the on-board activities are coming to completion in FY05. With the refocus from reforming gasoline for transportation applications to reforming natural gas for stationary applications, the program needs to work with stakeholders from the stationary fuel cell community to define more specific goals and targets for the fuel processor R&D activities. Specifically, more focus should be placed on durability and cost.
- **Balance of Plant Components –** Development of compressors for fuel cell systems is essential as long as there is a need for pressurized stack operation. Cost, efficiency, and durability of compressors being developed still need to be addressed. The sensor projects with industry are concluding this year, and a limited amount of laboratory R&D in the sensor development area will continue. Projects should culminate with a relevant database of chemical and physical sensors available to fuel cell system developers. Given the Program fuel processing re-direction, the majority of remaining work should focus on developing relative humidity sensors for transportation and sensors specific to stationary system performance requirements. Thermal and water management is essential to compact, reliable, durable fuel cell power systems. Several humidification designs were selected (e.g., enthalpy wheel, membrane humidifier) and heat exchangers are being evaluated. Multiple paths are appropriate for down-selection. A better quantification of effects of system variables (such as relative humidity and operating temperature) and of the environment (such as freezing ambient temperatures, shock, and vibration) on the component design requirements would be beneficial.

Project # FC-01: Fuel Cell R&D
Lightner, Valri; U.S. Department of Energy

Brief Summary of Sub-Program

The purpose of this Fuel Cells sub-program overview was to describe sub-program objectives/targets, budgets, barriers, and approach to R&D. The presentation also highlighted technical accomplishments and collaborations, and it described subprogram future directions including the upcoming solicitation. As such, this overview set the stage and put into context the R&D and analysis projects presented in this sub-program area during the Annual Merit Review.

Degree to which the Sub-Program area was adequately covered and/or summarized

- The Team Lead covered the program adequately, including an outline of who was funded, the breakdown of funds, and key technical achievements.
- The team lead gave a good summary for the short time alloted for the presentation.
- The presentation provided a very good summary of the program, an excellent transition between current and future goals, and identification of areas of future work.
- The team leader gave a few examples of results rather than overview of whole program.
- Activities in fuel cell area were described completely and efficiently.
- Barriers and targets were clearly presented for all fuel cell applications.
- Changes were explained for focus and targets.
- Progress highlights were covered.
- The presentation covered good balance of highlights and goals.
- The team leader provided an excellent general overview of program.
- The issues were adequately tabulated but not prioritized. Some of the description had been covered in the plenary sessions and, therefore, was redundant.
- An excellent summary of main FY04 achievements was provided.
- One slide dedicated to Stationary/Distributed Energy Systems would have helped to balance presentation's focus on transportation applications.
- The sub-program area is broad and diverse and difficult to handle in a short briefing period. Thus the focus was on broader sub-program goals such as cost and schedule rather than technical details of barriers.
- The Team Lead very effectively and comprehensively covered the challenges, targets, and timing.
- It was a good summary.
- The sub-program area was covered as completely as possible in the time frame allotted.
- The presentation provided a clear idea of the scope of the sub-program.
- The team leader did a good job, with few statements susceptible to technical quibbling and no outright technical misstatements (not easy to do).

Were important problem/issue areas and challenges identified/discussed, including plans for addressing these items in the future?

- The Team Lead discussed key issues for a successful future.
- The Team Lead was unclear on future solutions to solve key issues.
- Solicitation announcements do not satisfy the future plans of the DOE funded projects.
- Multiple areas were addressed -- selected topic presented in more detail.
- Yes, critical challenges are identified and addressed through the next round of funding.
- It is not obvious that basic research is aware of necessary details of critical fuel cell problems.
- Critical challenges were described well -- status versus goals. Key recent accomplishments and progress toward goals were well summarized. A major solicitation was described, but more details around future funding would have been useful.
- Issues of cost and durability were discussed in detail, and activites in these areas were listed.
- Future go/no-go decision points were described.
- Yes; budget zero balance was addressed, with lessons learned from a year of functioning without funds.

- Issues/targets were adequately discussed.
- The presentation lacks information on plans – not enough details other than new solicitation.
- Issues/problems were identified but not really discussed enough (hard to do in such a short time). It would be helpful to provide values for priorities of issues and rationale for why. Plans for future would be helped by this information.
- The analysis of cost estimates for volume production (status versus target) is very useful.
- Important conclusions from cost estimates were drawn: highest need (opportunity) for cost reduction for transportation PEMFC to meet 2015 targets lies in MEA, stack and, ultimately, system.
- As mentioned above, the overall view from a program management perspective was the focus of the presentation. It did not get into the technical details that are the bottlenecks in achieving these program goals.
- The challenges were well identified. It was good to have 'Hydrogen Quality' included, since this aspect has been overlooked in the past and could be a significant economic barrier to commercialization. Please include 'Durability' in gap (status versus target) for transportation chart.
- The challenges and barriers were well identified.
- Challenges are well defined and focused. This sub-program clearly has benefited from a consistent and long-term effort in planning and prioritizing with input from industry and researchers. This program benefits from consensus building, and the challenges and plans reflect this consensus.
- The issues needing improvements and the approaches for addressing the problems were appropriately presented.
- Yes; future directions and administrative mechanisms towards moving in those directions were properly covered.

Does the Sub-Program area appear to be focused, managed well, and effective in addressing the Hydrogen Program R&D needs?

- Program seems to be managed very well by the Team Leader.
- Key targets have been met thus far.
- Detailed time-line was shown – briefly.
- There is adequate focus in area where multiple approaches have to be evaluated at early stages.
- Yes, critical needs are effectively addressed and managed in a focused and forward looking way.
- The Team Leader stressed the need for basic research actvities to coordinate with applied technology development.
- Yes; the only area that might need further coordination is the effect of higher purity requirement for hydrogen use in fuel cells on potential hydrogen cost.
- The team leader demonstrated a focused program but with the ability to be flexible and change as needed.
- The sub-program appears well focused, but ambitious cost targets (slide 6) require breakthrough technologies. Where are they?
- Yes, the subprogram is well managed -- addressing needs of ’05 multiyear plan.
- The area appears to be focused, but management and program effectiveness is not so strong. It is not clear from the presentation how the DOE management team works to help the program achieve the goals once they have been set and, more importantly, how the program should respond to changes in the goals that may arise from the progress made.
- The presentation showed a good refinement of technical targets. The projects focused on individual stack components are an especially good idea and are yielding significant progress in very challenging areas.
- The individual projects do address the needs of the program in various areas.
- No problems here -- the program is well-balanced and targeted.
- Many parallel project efforts cover the areas as well.
- This sub-program area is very focused and the management has been very effective as demonstrated by technology progress in the last several years. Current management appears to be building on the successful model used to date, while adding additional management assessments and formal criteria to ensure effective use of resources and project execution.
- The sub-program appears to be in the right direction. No other ideas.

- Yes it does, within the higher-level policy constraints that expend some resources in areas (SOFC's, DMFC's) that provide no real benefits to the advancement of PEMFC technology. And, given the reality of federal programs in those irrelevant areas, it is probably more efficient to administer these under the FreedomCAR fuel cell program than to set up another administrative structure to maintain subject-area purity.

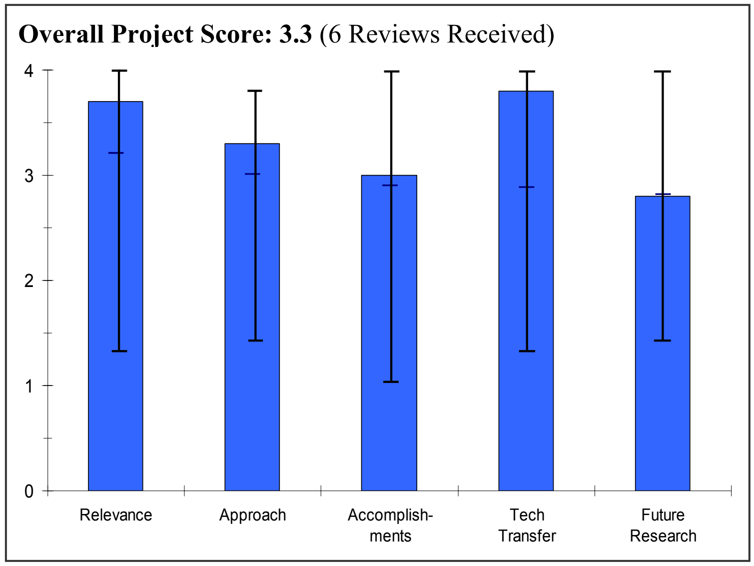
Other comments:

- The Team Leader did not explain why technical targets were changed.
- The Team Leader should have shown cost progress pre-2004.
- The Team Leader did not comment on what technical achievement would get fuel cells over the hump.
- The presentation was clearly professional.
- Focus on components is not needed – need to focus more on whole MEA system to meet specifications. Changes to any single component are unlikely to be sufficient to meet 2010 specifications.
- The presentation had well-illustrated slides with nice collage of pictures on first two slides. However, since only fuel cell vehicles are shown, one could be led to believe that this program is exclusively for transportation applications. Suggestion is to include pictures from other applications in the future
- The presentation was clear and very well delivered.
- A management-style summary of technical issues encountered in sub-program areas that highlight the tough technical challenges would help focus the tenor of the meeting into a critical problem solving mode and, thereby, might lead to the airing of new ideals and approaches to addressing the critical bottlenecks.
- The 5,000 hour durability target should be qualified as being achieved under realistic application load (power) profiles (simulated drive cycles) and not just constant power. Cost reductions achieved through reduced Pt catalyst loading must not be at the expense of durability -- Team Leader needs to verify this.
- Longer time allotment for this kind of presentation may help give more details of the sub-program tasks.
- Team Lead got the job done quickly and competently, then turned the stage over to the contractors, to whom the show belonged. What more could one ask?

Project # FC-02: Integrated Manufacturing for Advanced MEAs
DeCastro, Emory; De Nora North America

Brief Summary of Project

De Nora North America and its team are developing new ELAT structures and cathode alloys that allow an overall cell performance of greater or equal to 0.4A/cm^2 at 0.8V or 0.1A/cm^2 at 0.85V operating on hydrogen/air with precious metal loadings of 0.3mg/cm^2 and are amenable to mass manufacturing technology. Advances from this work as well as from development of a membrane which operates at 120ºC and 25% RH will be integrated into pilot manufacturing, aimed at delivering stack scale components and testing these components at the stack scale.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- Reduced Pt loading and low RH conductivity are key to objectives.
- Both high-temperature membranes and reduced catalyst loading are critical to PEM fuel cell commercialization.
- Big program. Catalyst work seems very good.
- Strong project team.
- Reduction in precious metal (PM) loading significant.
- Durability questions need to be addressed in parallel with development activities.
- Project supports President's Hydrogen Fuel Initiative.
- Project appears to be repeating information in literature.

Question 2: Approach to performing the research and development

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

- Coordination of Pt loading, gas diffusion layer (GDL) and HT membrane developments could be improved.
- De Nora has assembled a team and defined a logical program which substantially addresses DOE's technical barriers. The program is integrated with other PEM fuel cell efforts at De Nora and at its subcontractors.
- How is OCV stability at 100^{o}C, 25% RH, H_2/O_2? OCV tests are more direct than "Fenton."
- Solid focused approach.
- Good utilization of team members.
- Project addresses very difficult issues, low cost catalyst electrodes and high temperature membranes capable of operating at sub-ambient temperatures as well.
- Project scaling up platinum catalyst production.
- Project developing analytical method for evaluating wet-ability of materials.
- Ion deposition approach appears creative.

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

This project was rated **3.0** based on accomplishments.

- Ion Beam Assisted Deposition (IBAD) process may not be efficient enough for use with large scale fuel cell production.
- Good progress in developing GDLs for optimum water management.
- DeNora recognizes the importance of hydrophobicity and has identified techniques for accurate measurement.
- Platinum content has been lowered and automated bench-scale MEA fabrication processes have been developed.
- BA candidate plot very confusing.
- High temperature work is promising.
- Project scheduled to conclude in one year. No mention of integration of material into larger pilot scale manufacturing.
- Cost issues of high temperature membranes not addressed.
- Extensive discussion about influence of Teflon on performance is old technology. Well understood and patented long ago.
- Catalyst loading of $1mg/cm^2$ total was not an improvement. Not sure why Pt/Cr was picked.
- IBAD process looks very promising. Good results, but they rediscovered Pt/Co.
- HT membrane appears to be making progress but strong dependence on RH observed.
- Membrane V appears to have too large a dependence on RH to be acceptable yet claimed as successful research.

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

This project was rated **3.8** for technology transfer and collaboration.

- Team has diverse and broad capabilities.
- Exceptional degree of collaboration.
- Team members include Nuvera Fuel Cells, Dupont, Case Western Reserve University, and Northeastern University.
- Work is being disseminated through significant number of presentations/publications within the last year from most of the partners involved.
- Partners are full participants in the work.
- Good coordination and assignments of task.
- Good partnerships identified.
- Publishing and reporting data excellent.

Question 5: Approach to and relevance of proposed future research

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

- Need to focus on optimized electrodes/GDL for HT/Low RH membranes.
- The general direction is reasonable but specific steps were not sufficiently described.
- Good to see modeling proposed as structure design guide.
- Need to realistically assess if IBAD process lends itself to commercial process or is it limited by batch ability.
- A large number of potential directions have developed in this work. Don't loose your focus trying to equally address all areas.
- Program is correct on identifying durability data for catalysts.
- Good planning for BA membrane.
- No real approach detailed on AE membrane.
- Not clear why so much emphasis on development of hydrophobicity techniques. Methods exist in literature.

Strengths and weaknesses

Strengths
- Wide range of talents to attack problems. Broad scope of objectives.
- Very strong team.
- Overall excellent project.
- Extremely strong team with complimentary skills.
- Project coordination implemented very well.
- Multiple potentially successful projects (High temperature membrane, Low PM loading (IBAD)).
- Good team.

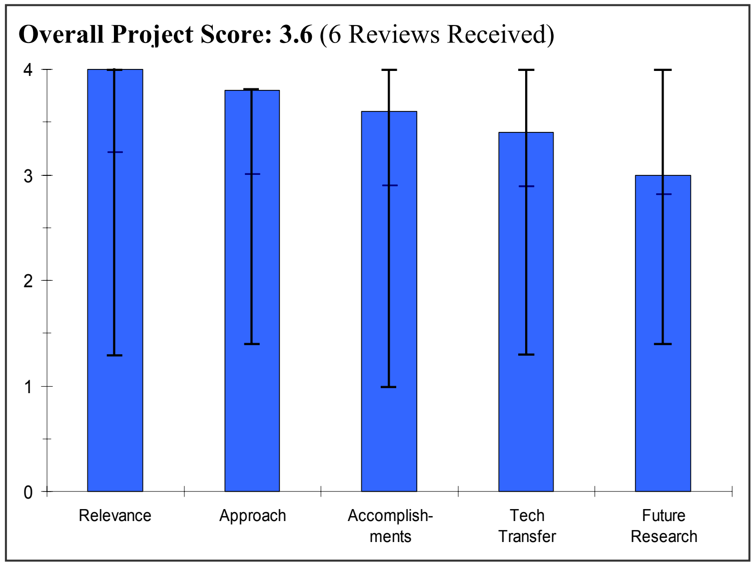
Weaknesses
- Lack of focus on optimizing system.
- The cost of the IBAD approach was not discussed.
- Hard to judge integrated progress.
- Be careful not to dilute efforts by loosing focus.
- More durability (catalyst and membrane) information would be useful.
- More catalyst stability information needed.
- Too many activities to report in such a short time. Additional reporting time needed.
- Literature appears to be ignored regarding hydrophobicity testing and evaluations.

Specific recommendations and additions or deletions to the work scope

- What are membrane choices for downselect to be made?
- Prioritize to ensure continued focus.
- Recommend serious focus on transition to continuous IBAD coating. This will be a major step toward determining commercialization potential.
- Continue with membrane durability studies.
- May want to consider accelerated membrane testing.
- Characterization of water retention properties of BA membrane is needed.
- Strong positive results for IBAD would indicate catalyst emphasis should focus on this material.
- Results on membrane V do not appear to warrant scale up.

Project # FC-03: Advanced MEAs for Enhanced Operating Conditions, Amenable to High Volume Manufacture
Debe, Mark; 3M

Brief Summary of Project



3M is developing high performance, durable, lower cost membrane electrode assemblies (MEAs) qualified to meet demanding system operating conditions of higher temperature and little to no humidification, with less precious metal catalysts and higher durability membranes than current state-of-the-art constructions. Objectives are to develop durable, lower cost MEAs for operation in the range of 85 < T < ~ 120ºC (develop next generation, thin film, ultra-thin layer catalyst electrodes [nanostructured thin film]; optimize perfluorinated sulfonic acid based ionomers modified for enhanced durability at low RH; match MEA components for enhanced performance under demanding conditions; utilize roll-good fabrication processes for lower cost) and to develop membranes for operation in the range of 120 < T < 150ºC (new proton exchange membranes that do not rely solely on standard modes of aqueous proton conduction; understanding relationships between materials, proton conductivity, T and RH; and screening materials and fabrication processes).

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- The project addresses key technological issues related to the PEM fuel cell, aiming at the development of a more efficient and temperature resistant membrane electrode assembly (MEA).
- The project also addresses PEM fuel cell cost reduction issues with the reduction of the amount of platinum in the MEA.
- This project is aligned with the goals of the HFCIT RD&D plan and addresses major cost factors (catalysts and membranes) and performance limitations.
- The progress made in this program is indicative of the strong technical team.
- High-volume production of high-performing MEA's is key to achieving fuel cell cost, durability and performance targets.
- The Relevance of the work is outstanding and if successful will lead to success.

Question 2: Approach to performing the research and development

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

- The project is well designed, and well developed.
- This project is focused on key technical barriers for improving performance and lifetime.
- Integration of catalyst and ME.
- 3M and its team thoroughly understand the technology and have formulated an approach which should be successful.
- Progress shown is impressive.
- Approach has proven to be effective.

- The approaches to Tasks 1 & 3 are outstanding. The approach to Task 2 is limited and seems to be an afterthought. Task 2 needs to be better integrated in the overall program

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

This project was rated **3.6** based on accomplishments.

- The project shows excellent results: the developed membrane withstands higher temperatures, has a larger lifetime, no loss of conductivity when compared to Nafion, and has been produced in pilot scale (1000 ft).
- Very impressive improvements in catalysts.
- Reduced the Pt loading to 0.12 mg/cm^2 total -exceeding 2015 target.
- Have improved catalyst durability.
- Appear to be very close to achieving target of 0.2gPt/kW (rated).
- Good improvements in membrane durability.
- Membrane conductivity at low RH needs improvement.
- 3M and its team have followed a well planned development plan that covers critical areas and includes parallel approaches where needed.
- Accomplishments are outstanding.
- This project is an example of a successful industry project.
- The accomplishments in Tasks 1 & 3 are outstanding. The accomplishments in Task 2 are disappointing. More needs to be done in this area. The testing at (0^0C and low humidity shows great promise but one would like to see more extensive testing at 120Ct to determine if stability issues and other unexpected durability issues arise

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

This project was rated **3.4** for technology transfer and collaboration.

- The project is being developed with a significant number of partners.
- The amount of papers published and presentations made so far are impressing.
- Good collaborations with groups looking at the fundamental science aspects.
- Could improve collaborations with end-users (fuel cell manufacturers).
- Excellent technology transfer to all partners is very evident.
- University, laboratory and company partners all seem to be contributing.
- There are excellent collaborations involved. However, some of the roles for these collaborations are not very clear- difficult to cover everything in the presentation.

Question 5: Approach to and relevance of proposed future research

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

- Future tasks are strongly relevant to the project goals.
- Membrane work for $85<T<120^{\circ}$ C should be continued before MEA testing is started. Membranes evaluated to date under low RH conditions have low conductivity.
- It appears the program appears to be on well planned path toward success.
- Project ends this fiscal year.
- Tasks 1&3 look good. Task 2 seems vague, particularly with respect to the behaviors of the membrane materials in the MEAs and how they will interact with the electrode surfaces. Some nasty surprises may be in store there.

Strengths and weaknesses

Strengths

- The project planning and designing.
- Technology transfer and collaboration.
- Technical accomplishments.

- Catalyst work has shown great improvements in reducing loading and improving durability.
- Membrane work has led to understanding of degradation mechanism and improvements in lifetime.
- Excellent approach to the whole system and demonstrates ability to go to manufacturing.

Weaknesses
- Not identified.
- Membrane performance at low RH needs improvement. From the graphs shown, 3M membrane has lower conductivity at low RH than standard PFSA. At 60%RH, 3M membrane has a lambda of ~3-3.5 and a conductivity of ~ 0.01 S/cm, the standard PFSA has a lambda of ~5 and a conductivity of ~0.02. It is only after the lambda value increases above 5 (at an RH >80%) that the 3M membrane has an advantage over standard PFSA in terms of conductivity.
- May need to incorporate additives or look at other membranes to obtain good low RH performance in this temperature range.
- The high temperature work is very exploratory and should be much further along.

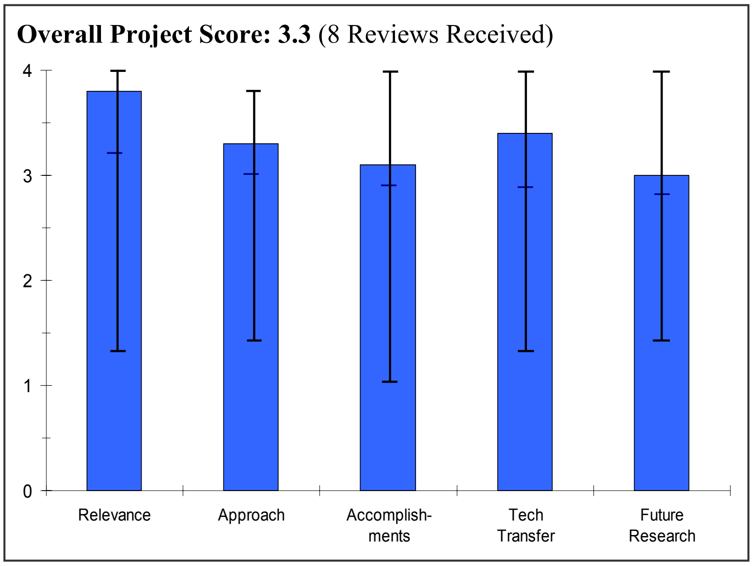
Specific recommendations and additions or deletions to the work scope

- The project could include the technique of Scanning Electrochemical Microscopy (SECM) to study the electrochemical reactions at the electrodes/electrolyte interface.
- Look for new membrane options for low RH operation in the 85-120^{0}C temperature range.
- Strong team that will make additional contributions in the future.

Project # FC-04: Development of High Temperature Membranes and Improved Cathode Catalysts for PEM Fuel Cells

Protsailo, Lesia; United Technologies Corporation

Brief Summary of Project



In the area of high temperature operation, United Technologies Corp Fuel Cells (UTCFC) is assessing and optimizing fuel cell materials that define and influence performance and durability of PEM fuel cells at operating conditions of 100-120^{0}C, 25-50% relative humidity. Scope of work includes development, evaluation and optimization for high temperature operation of such materials as membranes, catalysts, gas diffusion layers, seals, etc. Effects of temperature, relative humidity and cyclic operating conditions on performance and durability are investigated using both ex-situ and in-cell tests. Improved high temperature membranes development includes modification of Nafion-like materials with solid acids and fabrication of novel hydrocarbon ion-exchange membranes. For improved cathode catalysts, Pt alloys fabrication procedures are being developed; catalysts are fabricated and electrodes in membrane electrode assemblies are optimized. Performance and durability of these catalysts are evaluated through the use of ex-situ techniques (Rotating disc Electrode (RDE), liquid half-cell tests) and in-situ fuel cell tests.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Power density improvement and lower cost.
- The catalyst stability is addressed very well - this is critical to durability and electrode stability DOE goal.
- The project is of great interest for reducing costs and improving long-term durability. Both issues are important for commercialization.
- The increase of operation temperature and improving the oxygen reduction kinetics are important steps to increase the fuel cell efficiency.
- Catalyst activity and voltage-cycling durability work is exactly the kind of experimentation that has been needed to make a path to durable low-Pt systems plausible. High-temperature catalyst durability is a helpful side benefit. Low-humidity, high-temp work has been well-targeted but, as yet, has not been as successful.
- Shows results on hydrogen/oxygen primarily;

Question 2: Approach to performing the research and development

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

- Many partners but interactions need to be more clearly defined. Need a better plan for integrating disparate elements into future activities.
- Electrode stability studies outstanding.
- Membrane durability studies weak - the new materials are interesting, but the data showing durability is not nearly as strong as the electrode work.
- Multi-component cathode catalysts have the potential to substantially lower the amount of Pt in, and lower the cost of, the catalyst.

- Collaborating with universities to obtain fundamental understanding of high-temperature polymers should be very helpful.
- Durability study seemed very limited in scope considering complexity and importance of issues. Results on Pt dissolution are inconsistent with those reported by others. Determination of the baseline for durability of current membranes at low RH does not seem adequate.
- Difficulties of fuel cell startup due to low membrane conductivity have to be evaluated.
- For the catalysts, very nicely thought-out and executed experiments. The initial university work brought in a good diversity of approaches, and down-selects were made intelligently. The shift to a less-positive upper potential limit for the 120^{o}C cycling work, while perhaps logically inconsistent, was probably the practical thing to do.
- The Rotating Ring-disc Electrode (RRDE) peroxide results in TetrafluoroMethane Sulfonic Acid (TFMSA) needs further explanation. Peroxide usually peaks at lower potentials than shown, even in other weakly-adsorbed acids such as perchloric.
- High-temperature membrane work was also well-planned; it just hasn't panned out as well.
- Why was the membrane portion strongly de-emphasized?
- Cathode work is high quality, with solid focus on operating fundamentals, durability.
- Low RH is not the same as high acid concentration in solution. It is unclear whether any conclusions from the latter will reflect reality of the former.

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

This project was rated **3.1** based on accomplishments.

- The PtCo and PtIrCo work is clearly industry leading.
- The membrane developments are not as strong, but overall, quite reasonable.
- The Pt-Ir-Co catalyst appears to be very promising, based on the activity and stability data presented.
- Both approaches to high-temperature membranes are showing promise.
- The failure of Nafion membranes at low RH needs to be better understood because it will impact development of alternative membranes, e.g. why will polymer X have greater stability than Nafion if you don't know why Nafion fails.
- The reasons for the improved performance of PtCo/C were not discussed.
- Good Progress.
- Catalysts: The alloy systems studied here are perhaps not the most imaginative or innovative in the world, but this work gives a more solid demonstration of simultaneously-improved activity and durability than had been previously publicly reported.
- Membranes: Results have been perhaps a bit disappointing, but the demonstration of improved durability of Sulfonated Biphenyl Sulfones (BPSH) through lower oxygen permeability and therefore decreased peroxide is a nice piece of work.

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

This project was rated **3.4** for technology transfer and collaboration.

- Gives appearance of several smaller projects each making progress but limited collaboration; will catalyst and membrane work together and be compatible?
- Who will commercialize MEA they develop?
- The contractor is a commercial developer. Close collaboration with their own research center and the three universities can enable rapid implementation of advances as they occur.
- Close interaction between universities and a major developer.
- Need to establish collaboration with membrane OEMs.
- None of the partners listed has the expertise of mass manufacturing of membranes.
- Much of this work started with input from universities. The best parts of that work were taken in to the industrial part of this program. This project appears to have worked as a seed that has stimulated other industrial

interest in some of the Va. Tech membrane work. It also has helped to stimulate catalyst manufacturers to work harder on bringing Pt alloys to a fully-commercial state.
- Solid Collaboration.
- Need to involve catalyst makers, MEA makers in industry.

Question 5: Approach to and relevance of proposed future research

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

- Not clear how all results will be integrated and meaningful data obtained in next 6 months.
- The plan is reasonable relative to understanding and development of durable MEA, but I would like to see more interactions with MEA suppliers. Otherwise these promising new MEAs may not reach large scale commercial realization.
- Testing of the new catalysts in full-size cells and in a stack will be complemented by fundamental studies of catalyst stability and membrane durability.
- Continuing work in high-temperature membrane development is mostly experimental demonstrations.
- Use of surface and bulk analytical techniques may help to understand the PtCo/C catalytic activity.
- Should spend a little time addressing manufacturability and costs. This is where a membrane partner will greatly contribute.
- Future work is well planned, though since the project still has little time to go, the scope is a bit limited.
- No clear path provided to achieve improved MEAs for high T or to lower loading.
- Durability focus reasonable.

Strengths and weaknesses

Strengths
- Basic work being performed by wide array of qualified participants.
- Strong team that uses strengths of individual subcontractors/partners very effectively.
- A good blend of fundamental understanding (e.g. catalyst stability, peroxide generation, etc.) and experimental work to develop new approaches to solve durability issues.
- UTC Fuel Cells has an excellent depth and breadth of experience in fuel cell development, and in addressing issues of catalyst activity and stability.
- They have teamed up with well-respected academic groups for both the cathode catalyst and high-temperature membrane work.
- Activity and stability of the PtCo/C catalysts.
- Good progress.
- Good use of federal money to survey technical developments in two fields, drawing the best from universities, and subject them to hardheaded, relevant, industrial testing. Subsequent good industrial development to solidly establish which of those technologies are actually useful. Good response to priorities that changed a bit over time. Good communication of results. Good mid-course correction to avoid money wasted on too-large-scale demonstration of materials that weren't yet ready. Short-stack testing of the more mature alloy catalysts should give the DOE program a bit firmer base for program decisions.
- Good team.
- Important accomplishments in catalyst durability.

Weaknesses
- Specific program targets undefined other than "improved". Role of UTC not clear. Have any specific milestones been accomplished? I.e. has the membrane been selected for Phase 2? Has the optimum catalyst-membrane combination for phase 3 been identified? Greater clarity on status and metrics required.
- Who/how will successful results/developments be commercialized given that neither UTRC nor any of its partners in this program make MEAs?
- The team has strong polymer chemists, but is weaker in testing and understanding of membrane durability and the development of fundamentals that lead to membrane degradation.

- Although the Pt-Ir-Co catalyst material is referred to as an alloy, is it really so? Is alloying needed for the shown activity and stability?
- Only one catalyst composition was presented and discussed - is this the result of some optimization, or is some optimization planned in future work?
- In the high-temperature membrane work, the results presented are not encouraging for successful operation under essentially dry conditions.
- Fundamental techniques for material characterization seem to have been given a low priority.
- Absence of critical analysis of the chance of success of the new membranes.
- Lack membrane supplier/manufacturers involvement.
- Early settling for 50% RH at 120C for membrane work, since apparently corrected. Membrane results to date have been a bit disappointing. More could have been done with improvements through modified catalyst supports.
- No real path to lowered catalyst loading.
- Need to adjust for low RH; membrane approach does not tend in t hat direction.

Specific recommendations and additions or deletions to the work scope

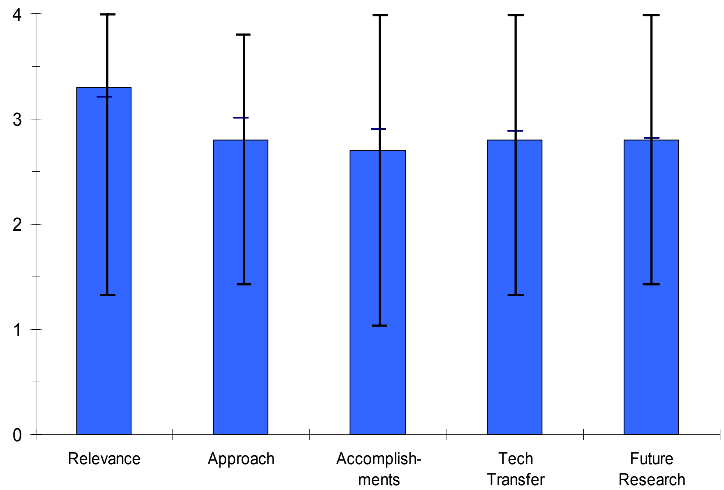
- Scope is fine but need further definition of Program Metrics. What constitutes success? All results should be incorporated in stack test and full results given including stoichiometry operating conditions, etc to make project beneficial throughout industry and not just to project participants. Fundamental understandings should be broadly disseminated.
- Develop/present plan for how developed MEA technology will be shared in the industry, or if it won't, how the team is planning to commercial them.
- Is anode CO tolerance still a goal for this project? If so, should the durability target for the high-temperature membrane be extended to 40,000 hours?
- Will they be providing the improved cathode catalyst to other developers and researchers for testing and evaluation?
- Need to establish collaborations with membrane suppliers/manufacturers.
- If there is a follow-up project, more attention should be given to corrosion-resistant supports.
- Should transfer findings to industry from universities.

Project # FC-05: Electrocatalyst Supports and Electrode Structures
Wilson, Mahlon; Los Alamos National Laboratory

Brief Summary of Project

Work on this task will build upon the ongoing collaboration with Brookhaven National Laboratory (BNL) on testing low-Pt content catalysts, but will primarily focus on investigating alternative low-cost supports. A secondary goal in developing new support materials is mitigating corrosion (i.e., carbon corrosion). Certain aspects of these new materials may enable the development of electrode structures that further improve catalyst layer performance and manufacturability. This year, LANL is also initiating an atomistic-level modeling effort to guide the development of these new supports.

Overall Project Score: 2.9 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Project is mostly aligned.
- Durability and reduction of Pt are important critical aspects of the R&D plan and are addressed in the program.
- Reducing Pt usage is certainly relevant. Project very limited in scope.
- So-called testing function, which appears to be a significant part of the effort, is weakly coupled to R&D Plan objectives.
- Improving the electrode structure and/or the catalyst properties will help improve the fuel cell efficiency.

Question 2: Approach to performing the research and development

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

- Project is heterogeneous: Testing catalyst for durability has little to do with the support development.
- Pt, Pd and Ru are all fairly expensive and therefore reduction in Pt content does not lead to cost reduction if substituted with Pd, Rh, etc. The approach using Ni, Au (example given) is more promising.
- Focus on support development is novel. Lack of extensive analytical characterization may limit understanding and progress (e.g. microstructural characterization of surface area changes, H_2O_2 generation).
- What value is added by LANL in this so-called catalyst testing? What is the approach to developing alternative supports to carbon? None described that I can see.
- Excellent approach and attitude toward technical barriers and reevaluation.
- Characterization of the conductivity of the ceramic material is crucial.

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

This project was rated **2.7** based on accomplishments.

- While 3000 hours of BNL catalyst testing is impressive, the performance of the catalyst is rather inadequate; less than 0.6V at 0.6 A. Not in agreement with high RDE activity presented by BNL.
- Good progress demonstrated in relatively short time.

- Good progress for relatively new project. Need to complete analysis of BNL materials and concentrate on CS candidates.
- Testing of BNL catalyst poorly done. Test regime (constant current) not relevant to fuel cell application. Programmatic need to define durability test protocol for PEMFC the way the HEV program has done for batteries. In the meantime testing must include variable loads, start-stop and hot standby. Pure Pd catalyst should have also been used as a control as at such low potentials there is little difference between Pd and Pt. The presentation of the new "ceramic supports" was so obscure it was useless.
- Steady state polarization measurements with the CS materials are required.
- Check for temperature effect on the polarization response and materials stability.

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

This project was rated **2.8** for technology transfer and collaboration.

- Better coordination with BNL would be beneficial for understanding the fuel cell performance.
- BNL and internal LANL collaboration demonstrated.
- Good collaboration with BNL.
- Where is the collaboration with a fuel cell or catalyst developer?
- Interact more outside national labs.
- Only national laboratories are involved in the project. This is all right because materials are still very far from practical applications.

Question 5: Approach to and relevance of proposed future research

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

- Study the compositional changes of BNL catalyst.
- High surface area ceramic support is innovative approach which should be pursued further.
- Need to quickly demonstrate potential CS approach under fuel cell conditions.
- Where is this project going?
- A realistic analysis of the chance of success with the CS is missing.

Strengths and weaknesses

Strengths
- The risk in pursuing the ceramic core support is worth it.
- Good exploratory project.
- Understanding of complex materials.
- Strong theoretical base to support the test results.
- Good scientific quality and potential of practical applications of the multi-layered metallic catalyst particles.

Weaknesses
- Not clear if the solutions developed are applicable for high volume manufacturing.
- Narrow focus on substrate for Pt. Lack of strong analytical support (characterization/mechanisms of degradation under fuel cell-like conditions).
- Too many to list. Fundamentally they don't know what to do.
- Lack of fundamental criteria for choosing the new support, which was not based on previous evaluations of stability, conductivity, surface area.

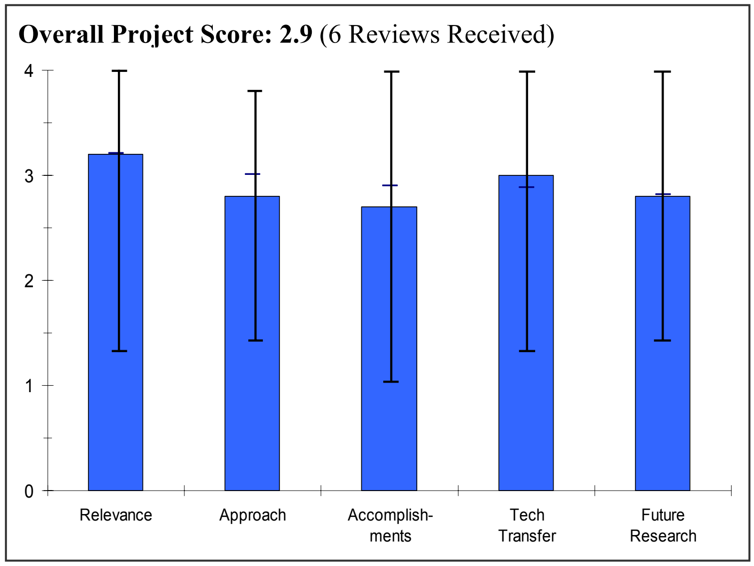
Specific recommendations and additions or deletions to the work scope

- If possible, split the project in two: durability and the support.
- Compare how ceramic supports improve durability relative to carbon supports including graphitic carbons.

Project # FC-06: Development of New Polymer Electrolytes for Operation at High Temperature and Low Relative Humidity
Zawodzinski, Tom; Case Western Reserve University

Brief Summary of Project

Case Western Reserve University is developing membranes for PEM fuel cells, targeting operation at low relative humidity and/or T>100ºC. The project includes proof-of-concept activities to find ways of achieving conduction under demanding conditions.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Address critical barriers to PEMFCs, especially mitigation of poisoning and improved performance at higher temperatures.
- With the drive to have on-board hydrogen storage, what is the driver for this project?
- High-temperature, low-humidity membranes are key to PEM fuel cell commercialization.
- If successful, would expect program to yield valuable information to the design of high temperature MEAs – certainly making the program aligned with goals and objectives.
- Change in emphasis to low relative humidity improved the relevance of this project.
- This work, particularly with its newly-claimed emphasis on low-humidity applications, seeks both to gain fundamental insights and, through a building-block approach, to develop improved membranes and ionomers for use in electrode layers. If these aims could be brought to fruition, they would substantially advance PEM fuel cell technology.

Question 2: Approach to performing the research and development

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

- Good breadth of synthetic approaches. Good balance of exploratory and more advanced (MEA, stack-level) studies. Good correlation of chemical and physical properties with expected device performance.
- The program is focused only on conduction to gain insight into mechanisms with the intent of discovering something which will enable a breakthrough or development of the optimum membrane.
- The program has adjusted to DOE interest in low humidity operation.
- The nature of the program leads to the appearance of a shotgun approach.
- With so many various partners, a standard stability or "performance" protocol should be enforced, such as a thermal stability test, conductivity, and Fenton-stability in order to gauge across all partners the relative strength of the approach.
- The "John Kerr" approach of designed structures to obtain theoretical answers is highly endorsed.
- Stated approach is good (e.g. seek fundamental understanding of proton-conduction mechanism), but evidence of this approach actually being the guiding focus is lacking.
- Somehow, major elements of this project seem to have taken forever to get started. It is not clear to this reviewer why this is the case. Are there problems with having one university serve as coordinator for the work of others? The basic tenet of trying things to gain understanding of fundamental properties, coming back to try to patch the durability later, is OK as long as forward momentum is maintained. But temporarily putting aside

durability to check out performance ideas is a bit dangerous, as the durability shortfalls of current material sets are probably more serious than the performance shortfalls and their thermal consequences.

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

This project was rated **2.7** based on accomplishments.

- Order of magnitude improvement in conductivity for network structures important. Significant number of variables to work with for each proposed electrolyte; not clear whether progress is due to empirical luck or well thought-out science.
- This project is highly fundamental in nature and appears to have no real emphasis on testing for durability.
- Progress seems slow, partly because some of the subcontractors have received funding fairly recently.
- Some promising results were reported but there is a long way to go.
- Although just starting the program, the array of approaches appears to already shed important information on approaches and theoretical limitations.
- Impressive array of compounds synthesized and tested (as expected for the funding level), but what is the end deliverable: (a) A better fundamental understanding? If so, this is not evident - yet. (b) Materials with high conductivity at low RH? If so, this is not evident - yet. (c) Materials that meet all DOE requirements? If so, this is not evident - yet. And, not likely with very little emphasis on stability.
- It looks like some of the block copolymer and structured composite work is starting to bear fruit. Some of the other work still seems a bit like futzing around with all ideas that are plausible through one line of reasoning without enough down selecting through the application of other lines of reasoning. For example, in the imidazole work is anything being done to answer questions about poisoning of ORR kinetics on Pt by imidazole?

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

This project was rated **3.0** for technology transfer and collaboration.

- Most projects in early stage, so not surprising transition not happening yet. Institutions within project could accelerate progress with better interactions.
- The program includes Foster-Miller, Mitsubishi Chemicals, two national labs, four universities, and the Naval Air Warfare Center Weapons Division.
- Ideas and results seem to be cross-pollinated between team members.
- Industrial Partners or validations were not listed.
- Obviously, the collaborations with other universities and national labs are excellent.
- However, PI claims numerous interactions with industry, but other than some samples from Mitshibushi Chemical and discussions with Dupont, it is not clear what the plan is or who are the potential industry collaborators.
- Bits and pieces of knowledge from diverse sources are being put together into a greater collection of experience that could lead to wise choices in material development. But there seem to be rough edges in even the formal collaborations within the program - why didn't a couple of contractors get in updates on their work in time for inclusion in this presentation? It's not clear that there has been a significant flow from case to industry in this project, though such contacts are working well when nominally led from industry.

Question 5: Approach to and relevance of proposed future research

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

- Lacked specific goals/milestones for each project for the coming year.
- High level of focus on low RH conductivity measurements, not enough focus on longevity/durability testing.
- Future work follows logically from the progress to date. Plausible paths forward have been identified.
- There don't seem to any off-ramps in the program to deal with dead ends.
- Additional electrochemical stability testing or simulation thereof appears to be missing.
- If fundamental understanding is the goal, then why test MEAs?

- Future focus should be establishing some hypothesis for proton conductivity in polymer electrolytes (that includes a good explanation for the critical role of hydration) and testing/validating these hypotheses.
- Plans seem good, though durability considerations shouldn't be put off for too long, as they are probably the most important considerations in the choice of technically useful membranes.

Strengths and weaknesses

Strengths

- Good focus on conduction mechanisms and understanding, without trying to solve too much (mechanical stability, long term performance) on new exploratory materials.
- Very strong teams with good experience in establishing sound motives for design, and the capability to experimentally verify strategies.
- Increased emphasis on low relative humidity and decreased emphasis on high temperature was a good change.
- Have synthesized some unique materials that offer the potential to study and understand ionic conductivity mechanisms.
- LBNL results were very good, since it addresses fundamental mechanism (although conclusion of 10-2 is not clear).
- Diffusion length scale results on BPSH also good, since it does result in some interesting hypothesis.
- Good gathering of ideas from diverse sources. Building-block approach may eventually yield important results. P.I. knows his stuff, has good technical judgment, and seems to have an inspirational enthusiasm for the work.

Weaknesses

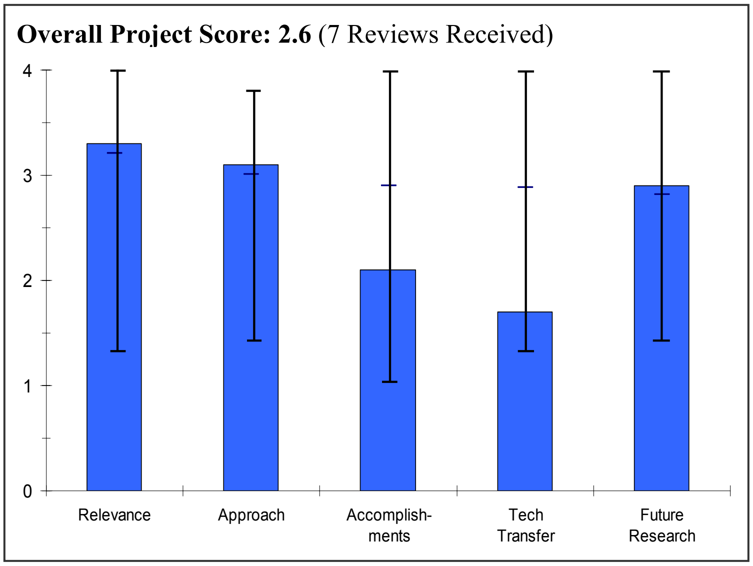
- Complimentary approaches, but seems like each team member working solo - need to hold several team meetings each year to improve interactions. What is the roadmap for this project? Not clear what/when the endpoint is. Any downselect plans to focus effort?
- Some sort of electrochemical simulation or testing strategy needs to be added.
- Needs to establish standard tests for conductivity, thermal stability, mechanical strength, and oxidative stability (such as Fenton's) that are performed by each partner on their most promising candidates/approaches in order to be able to rank across the board.
- Despite stated intent, still not enough emphasis on fundamental understanding (e.g., what is the conductivity mechanism in polymer electrolytes and why is hydration so critical?). There are certainly a lot of polymers that have been synthesized and tested here, but not much progress towards answering the fundamental question above, even though conductivity mechanism is claimed to be the main focus. If progress on fundamental understanding is being made, it is not clear from this presentation.
- It has taken a very long time for this work to gain traction. One keeps expecting the PI, who is an excellent scientist with a clear passion for the work and lots of good ideas (and who is a top-notch communicator at times) to give a review talk that will knock the socks off the audience. But it hasn't happened yet in this program. Maybe there is too much to cover, so that work that actually has significant depth appears superficial. But a bit more attention to the flow of the talk, and a better mix of data and words on the same slide (less phase separation of word slides and data charts) would probably help. Some good stories about ideas, testing of ideas, and revised hypotheses should be distilled out of what tends to look like uncoordinated activity. This is a good project, but by all rights it should be a great project. This presentation appeared to be put together without full dedication.

Specific recommendations and additions or deletions to the work scope

- Maybe fewer fronts would be more effective. Drop the least promising approaches.
- Develop a RDE method for simulating electrochemical environment on small samples of material derived from the program (such as casting a film of the polyelectrolyte over catalyst on the RDE tip, etc.).
- Establish standard tests that all partners perform on their most promising candidates/approaches.
- Reduce the diversity of materials being studied to a limited number of "model compounds" to uncover the key to ionic conductivity and the importance of hydration (e.g. more quality and less quantity).
- Focus on what the PI claims is the goal (namely, obtain fundamental understanding of proton conductivity) and less emphasis on actual conductivity values and/or plethora of systems. If this is being done, it is not obvious from the presentation, i.e. what have we learned about proton conductivity in polymer systems?

- Ignoring durability aspects is fine, as long as definite progress is being made on understanding proton conduction.
- Generate some strong data about conduction mechanisms so that the community can more readily buy into the idea of temporarily relaxing the noose of plausible durability. Might formal downselect dates help to keep things moving along and keep the lines of communication more open?

Project # FC-07: High-Temperature Polymer Electrolyte Membranes
Myers, Debbie; Argonne National Laboratory



Brief Summary of Project

Argonne National Laboratory (ANL) is working to develop a proton-conducting membrane electrolyte for operation at 120-150ºC and low humidities to meet DOE's technical targets. The project is investigating dendritic macromolecules attached to polymer backbones, cross-linked dendrimers, and inorganic-organic hybrids.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- DOE Targets for conductivity, cost, durability and temperature clearly addressed.
- Project focused on working towards DOE targets.
- Fundamental long-term work.
- Focus of project very relevant.
- Good focus on high temperature and low RH membrane improvements.
- High temperature, low relative humidity membranes are vital to the long term viability of fuel cells.
- The $200/m^2 cost target seems a little high. Presumably this is not a long term cost target.

Question 2: Approach to performing the research and development

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

- Approach good dealing with dimensional stability issue, while addressing higher temperatures and conductivity
- Need to incorporate ionomer in catalyst layer as part of study - what ionomer will be used in catalyst to evaluate MEAs?
- Once temperature stability issues are solved, need to evaluate Fenton's test at higher temperatures.
- Clear directions laid out.
- Narrow but appropriate focus on dendritic macromolecules and inorganic-organic hybrids.
- Presenter did not quantify how the approach is expected to reach a 120ºC minimum operating temperature target (e.g., what fundamental material property limitations might limit maximum temperature and how does the approach address those potential limitations, quantitatively, not qualitatively).
- Design seems to focus on high water uptake membranes that must have increased dimensional instability. It is not clear that sufficient water will be retained at low RH, high temperature conditions.

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

This project was rated **2.1** based on accomplishments.

- Goals clearly identified.
- Good progress this year, but dimensional stability issue remains problematic.
- Not much progress in FY '05.
- Still no conductivity data at target conditions (120-150ºC).
- Progress vs. spending lover than many other projects.

- It is unclear how the low temperature operation is addressed.
- Even with cross links conductivity drops off at approximately 90ºC.
- 100 hour durability test is not very long.
- Comparisons of dimensional stability were made between the cross-linked membrane and benchmark membrane at different relative humidities, so meaningful comparison was difficult.
- Cross-linked membrane showed dimensional stability transition improvement from about 80ºC to about 85ºC, which is only a 5ºC improvement, whereas a >40ºC improvement in the transition is ultimately required.
- Low conductivity even at moderate temperatures and pressures. Perhaps the plan to increase the organic component in the composite will address this issue.

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

This project was rated **1.7** for technology transfer and collaboration.

- Consider bringing MEA suppliers into the project.
- To date, no work outside of ANL, although interactions with GM/Giner planned.
- Appears to be research without interaction with manufacturers.
- Need to establish collaboration/interactions with membrane and MEA OEMs.
- Tech transfer is not really addressed.
- Collaboration with industry (e.g. GM) is good for verification.
- More collaboration with industry (e.g. materials manufacturing companies) may reduce the potential for "dead-end" research.

Question 5: Approach to and relevance of proposed future research

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

- PI has identified and is working on the primary challenges of this project including dimensional stability and conductivity.
- Based upon preliminary data, future plans look reasonable.
- Need more emphasis on MEA assembly.
- Needs a quicker timeline.
- Should address the low temperature operating capacity.
- Good overall approach to addressing issues identified to date.
- Pathway for future research was clear.
- Quantification of potential for dendronized polymer path would improve confidence, however.

Strengths and weaknesses

Strengths
- PI (project) is focused on DOE technical R112 good fundamental research
- Fundamental study.
- Debbie's presentation was clear and clearly articulated. Good use of data.

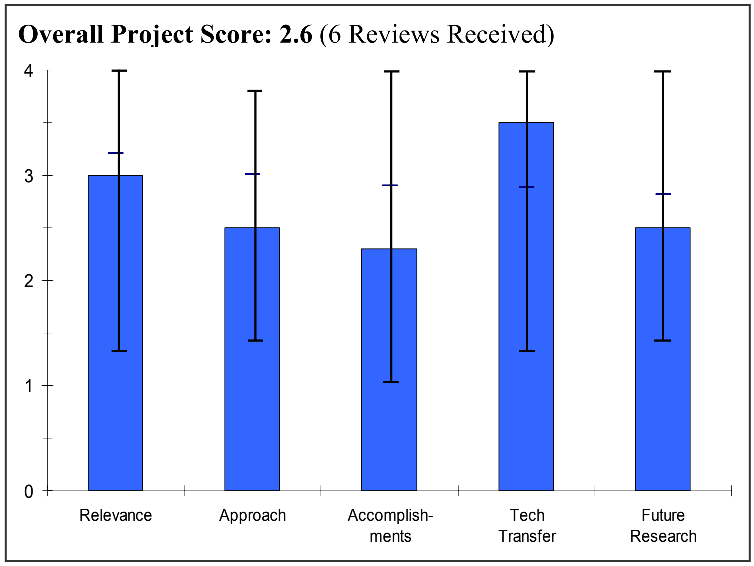
Weaknesses
- Challenges of dimensional stability still problematic.
- Slow pace.
- No MEA testing yet.
- Dimensional stability of materials.
- Low temperature stability of backbone, although plan in place to evaluate more stable materials.
- Sense of urgency is needed.
- Lack membrane supplier/manufacturers involvement.
- Need to articulate quantitatively what leads the researcher to believe the targets are ultimately achievable using the proposed approach.

Specific recommendations and additions or deletions to the work scope

- Continue testing at higher temperatures >120ºC.
- More MEA testing.
- Review of goals vs. out years with industry ties is recommended.
- Need to establish collaborations with membrane suppliers/manufacturers.
- Address the low subzero operation capacity.
- Would like to see more work done in the inorganic-organic hybrid area.

Project # FC-08: Development of Polybenzimidazole-based, High Temperature Membrane and Electrode Assemblies for Stationary and Automotive Applications
Staudt, Rhonda; Plug Power

Brief Summary of Project



This Plug Power project is identifying and demonstrating a membrane electrode assembly (MEA) based on a high temperature polybenzimidazole (PBI) membrane that can achieve the performance, durability, and cost targets of stationary fuel cell applications. Initial screening of potential PBI-based chemistries and structures has been completed, and the top 5-10 candidate materials based on chemical and physical properties have been down selected. Over the past year, Plug Power continued the rapid screening of candidate PBI materials in 50 cm^2 MEAs; performed detailed electrochemical characterization of MEAs made with selected PBI polymers; evaluated low cost acid-absorbing materials for phosphoric acid management within the system; continued design and development of bipolar plates with PBI-specific flow fields; and initiated development of a PBI membrane-based MEA with advanced electrode structures providing high catalyst utilization and performance exceeding that of Nafion-based proton exchange membranes.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This program focuses on residential applications rather than transportation.
- The change in program direction to stationary only limits the benefits.
- This project would be more relevant if it was applicable to automotive stacks in addition to stationary fuel cell stacks.
- Higher temperature fuel cells, i.e., SOFC, will provide higher efficiency and higher value heat for residential applications.
- Stationary PEMFC operating at high temperature are an important near-term PEMFC application due to their better suitability for existing fuel infrastructure and carbon-containing renewable fuels (CO-tolerance).
- CHP aspect of high-temperature PEMFC systems increases their overall efficiency.
- High-temperature PEM in general is also desirable for transportation applications to facilitate thermal management.
- Near-term application has potential to accelerate cost reduction and commercialization of all (PEM) fuel cells
- Strong team.

Question 2: Approach to performing the research and development

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

- The task structure is logical.
- RPI effort is very good.
- Unanticipated system upsets that lead to water in the stack will potentially cause catastrophic loss of acid and stack failure.

- The approach addresses the acid evaporation issue at the system level rather than at the membrane level.
- Current density is not specifically addressed; it has a direct impact on stack cost.
- One Reviewer commented that there is no apparent validation of the University of South Carolina acid flow model, and he questioned whether membrane mechanical properties are being characterized.
- To minimize fuel cell costs it would be better if stationary and automotive applications shared as many components as possible.

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

This project was rated **2.3** based on accomplishments.

- Initial power density of the MEA is low and no data was provided on decay with acid loss. The basis for the 40,000 hour life was not presented.
- Solution for membrane creep was not found.
- Membrane mechanical properties require more work.
- Mechanical stability is still a major hurdle that may require additional time to solve.
- Open circuit voltage should be improved.
- The presentation did not include a plot showing performance (cell voltage and cell resistance) as a function of time.
- There was minimum information on trade-offs between design choices.
- Plug Power should do a ballpark screening on cost earlier in the program.

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

This project was rated **3.5** for technology transfer and collaboration.

- There is good cooperation between university and industry.
- At this stage, the partners are appropriate.
- The project has a complete range of partners from fundamental science providers to systems integrators.
- The Principle Investigator has involved external resources for the specific challenges of this program.
- Fundamental understanding and optimization of membrane structure is important.

Question 5: Approach to and relevance of proposed future research

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

- With one year in the program, fundamental problems still exist: acid evaporation, low power density, and membrane mechanical properties.
- These issues should be addressed before progressing on to the construction of a full size module.
- How is acceptable acid loss rate determined?
- What can be learnt from the PAFC literature?
- Survivability: does PBI survive crystal formation of H_3PO_4 upon cooling of stack to room temperature or below?
- How is stability of nano catalyst at 160ºC measured?
- Acid loss should be measured in full-size stack.
- There is substantial effort remaining to be completed and no indication of comprehensive cost analysis.
- If the cost analysis shows the membrane to be exorbitant, we'll know this information LATE in the project.
- A new approach to solving dimensional stability is needed.
- One reviewer commented that he would like to see data on the effect of operation at low relative humidity.

Strengths and weaknesses

Strengths
- High temperature membranes have the potential to provide CO tolerance, improved heat rejection, and higher value heat for CHP applications.
- They have a good team.
- There is good integration.
- A complete range of partners are collaborating on the project.
- It is refreshing to see a speaker show targets and status for THEIR project on a slide. It makes it much easier for reviewers to assess progress.
- Focused Ion Beam analysis of electrode 3-D porosity is a good technique.
- Acid loss appears to be manageable.

Weaknesses
- The technology has fundamental issues, and this presentation did not show interactions between these. For example, low power density was attributed to oxygen concentration limitations; however, increasing air stoichiometry with higher air flow would increase acid evaporation.
- Does membrane-creep lead to catastrophic failure of the stack or gradual loss of performance?
- The commercial supplier of the membrane should be addressing the fundamental issues with the technology, while the Principle Investigator should be focusing on implementation of the technology into a stack and system.
- There is no indicator of improvement over existing PACC stack designs; cell performance is marginal. Minimal data was provided to support technical targets table.
- No endurance data was shown, except for acid loss rate over ~5800 hours. Current density is not mentioned in that graph. However, acid loss calculation was based on 0.2 A/cm^2, which leads to the assumption that endurance tests were performed at that low current density. If this is true, and if there is indeed an issue with operating at higher current density (such as PAFC experience suggests), electrode area per kW would be significantly higher than in PFSA-based PEMFC, leading to a concern for increased stack cost. How can the current density be increased to meet requirements?
- Acid management requirement results in additional system complexity and cost. These should be quantified/estimated.
- It is not clear "extraction" problem is solved.
- The choice of Albany Nanotech for electrode modification seemed to be based on geography rather than capability.
- Lack of applicability to automotive stacks will limit cost reduction ability.

Specific recommendations and additions or deletions to the work scope

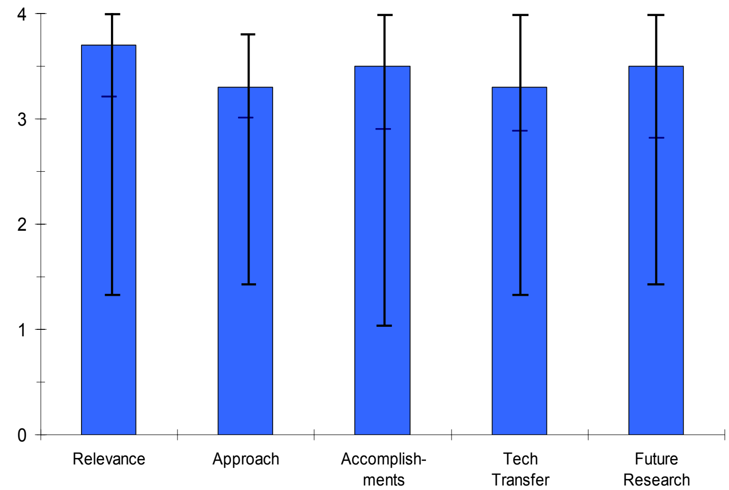
- In the balance of the project, focus should be on resolution of the fundamental issues rather than fabrication of a full size demonstration unit.
- The program needs to generate performance degradation data versus acid loss and creep of the membrane.
- Shift membrane reformulation and electrode development effort to PEMEAS, while Plug focuses on use of the membrane in a stack, i.e. flow field design and sealing.
- Include a comprehensive cost analysis.
- Include a comparison to traditional PAFC stack design.
- Suggest durability testing on (simulated) reformate.
- Sensitivity to upsets causing water condensation on membrane should be addressed.

Project # FC-09: Enabling Commercial PEM Fuel Cells with Breakthrough Lifetime Improvements
Escobedo, Gonzalo; Dupont

Brief Summary of Project

This Dupont project will utilize both experiments and modeling to develop an understanding of potential mechanisms that can lead to membrane failure, including H_2O_2 formation; radical formation; attack of polymer weak sites; material properties degradation; localized stress which promotes cracks/fissures; and crossover failure occurrences. Mitigation strategies such as peroxide prevention, peroxide decomposition, polymer stabilization, membrane reinforcement, and edge seal design and optimization are being investigated to improve membrane durability. The project will optimize each and incorporate them, in total, into fuel cell products.

Overall Project Score: 3.5 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- Well focused on cost and durability. Aware of need to address performance later on.
- Improved performance and durability is needed for fuel cell to meet lifetime requirements.
- PEM membrane durability and performance critical to EERE's program.
- Long term durability of 5000hrs. For vehicles uses and 40,000 for stationary have to be achieved if Hydrogen Fuel Cell Initiative is to be realized.

Question 2: Approach to performing the research and development

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

- Good combination of theory and experiment. Good balance between fundamental and cell level tests and their correlation.
- Focus of approach is improved membrane performance. Modeling and experimental approach yields balanced effort to maximize technical contribution of this project.
- Peroxide formation prevention does not seem possible within current scope.
- Alignment with automotive applications supports EERE program.
- Well thought approach to understanding peroxide attack of Nafion membranes and to develop new materials improvements that reduce membrane and edge seal degradation.

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

This project was rated **3.5** based on accomplishments.

- Water influence on chain motion important to understand to improve existing materials and develop new ones. Good progress in Gen A membrane performance, with additional ideas for further improvement.
- Steady progress in membrane performance and durability.
- Excellent correlation between Fenton's test fragments and fuel cell tests.
- Excellent progress in membrane stability and identification of degradation mechanism.

- Excellent progress. New insights are being gained in membrane degradation and the role that water humidification and swelling play in this process. Membranes have been made with significantly improved mechanical strength and chemical stability with new tougher polymers and reinforced membranes. Methodology for tracking degradation- fluoride emission rate show significant 7X reduction.
- It is not clear what conditions the membranes were exposed to during all tests (RH, humidity). Is the membrane being tested under low RH, high temperature conditions?

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

This project was rated **3.3** for technology transfer and collaboration.

- Complimentary team expertise and close interaction definitely accelerating this project.
- Excellent partners in this project with complimentary experience and expertise focused on membrane development.
- Dupont / UTC teaming strong collaboration, but there is a lack of publications for this size project.
- Good joint venture between a membrane supplier, Dupont, and a fuel cell maker UTC.

Question 5: Approach to and relevance of proposed future research

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

- Well defined road map and milestones. Strong iterative theory and experiment process for continued improvements.
- Testing in short stack fuel cells will quantify the improved performance of the membrane materials (and seals) developed in this project.
- Clear strategies for UTC and Dupont, unclear future collaboration with USM.
- Acceleration test and factors, and identification of chemical degradation modes likely to lead to best long-term advances.
- Critical application of degradation reduction beyond cells to short stacks. Will also explore a new edge seal design, another key durability issue.
- It is not clear that the project is working toward the tough temperature and RH targets.

Strengths and weaknesses

Strengths
- Strong team with appropriate skills to accomplish technical objective.
- Long term durability of 5000hrs. For vehicles uses and 40,000 for stationary have to be achieved if Hydrogen Fuel Cell Initiative is to be realized.
- Excellent progress. New insights are being gained in membrane degradation and the role that water humidification and swelling play in this process.
- Membranes have been made with significantly improved mechanical strength and chemical stability with new tougher polymers and reinforced membranes. Methodology for tracking degradation - fluoride emission rate- show significant 7X reduction.
- Next year work on edge seal design, another key durability issue.
- Comprehensive approach that is addressing the obvious failure modes of the MEA in a systematic way.

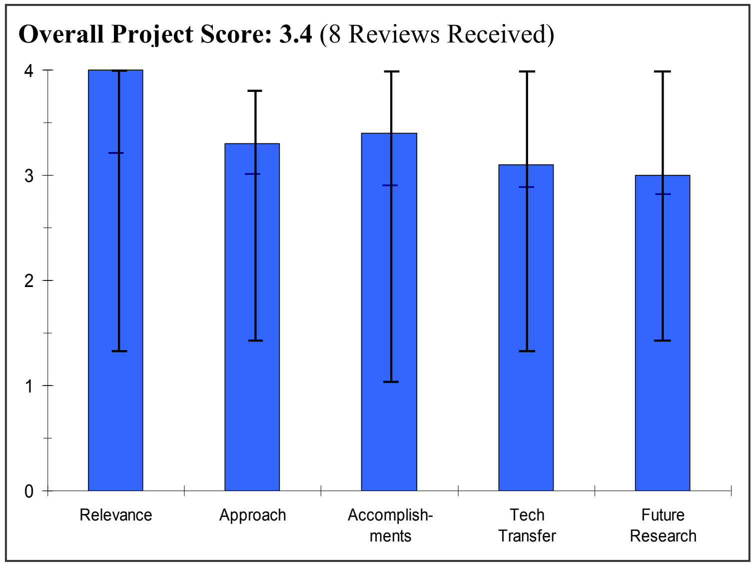
Weaknesses
- Slide 13: How is stability factor defined? Mechanical test may provide some guidance, but as described doesn't mimic cyclic stresses in fuel cell (only half a cycle).
- Unclear what role USM plays in project or what they have contributed to project. No clear USM results to date.
- Unclear from the presentation what conditions the membrane is exposed to during durability tests. To meet overall DOE program goals, temperature and RH targets are significant. Are these the conditions being used?

Specific recommendations and additions or deletions to the work scope

- Simulation to correlate accelerated testing conditions with actual test data.
- Publish degradation mechanisms.
- How does durability change from top to bottom in a stack and side to side? Are there tools like the NIST neutron imaging that can used to monitor degradation onset? What is the dependence of MEA degradation with operating conditions and is there an optimum point.

Project # FC-10: New Electrocatalysts for Fuel Cells
Ross, Phil; Lawrence Berkeley National Laboratory

Brief Summary of Project



For this Lawrence Berkeley National Laboratory (LBNL) project, new catalysts for both anodes and cathodes are being developed following a unified concept of platinum group metal (PGM)-based bimetallic nanoparticles with a "grape" structure (a PGM "skin" with base metal core). The choice of PGM and core metals for the anode and cathode will be based on computational screening of PGM core-shell nanostructures using newly developed (under BES funding) Monte Carlo simulations. LBNL will pursue new synthetic chemistry to synthesize nanoparticles with a "grape" structure, continue focus on Re as metal core with Pt and Pd as PGM, optimize AuPd as an alternative to Pt in anodes, and conduct fundamental studies of the crystallite size effect for the oxygen reduction reaction in acidic electrolytes on carbon supported Pt and Pt alloy nanoparticles.

Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- Very well aligned and supporting the goals and objectives of the DOE R&D plan.
- Strong intersection of theory and experimentation to address an enduring, fundamental challenge
- Basic work such as this is critical to provide the basis for the development of active catalysts with reduced cost and higher activity.
- The understanding of the ORR mechanism is crucial for optimizing catalyst design.
- Critical project that attempts to understand catalyst performance.
- Fuel cells only work as well as catalysts work.
- Establishes the fundamental science towards the durable activities of ORR catalysts 4x those of Pt that are needed for automotive fuel cells to become a reality.
- Addresses fundamental aspects of peroxide generation, a major source of degradation in fuel cells.
- Low-temperature work further substantiates ability of PEM stacks to operate so long as gas flow isn't blocked by ice.

Question 2: Approach to performing the research and development

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

- It is a well-established and precise approach, followed throughout the years.
- Current phenomenology and state-of-the-art tools.
- Would benefit from more field data.
- Using key questions as captions is very effective.
- Clear path on enhancement of catalyst activity for ORR. Unclear approach on mitigation of H_2O_2 formation.
- Excellent approach to developing a basic understanding of electrocatalysts and reaction mechanisms.
- The approach is clearly fundamental and in the right direction.
- Bimetallic catalysts (alloys) are well studied. Large literature in this area.
- Good combination of experiment and theory.

- PI needs to tell how to stabilize alloys for a long lifetime.
- Single crystal and other flat-surface catalysts simplify the catalytic environment just enough to allow clear experimental identification of critical parameters.
- Experience and perspective cast diverse pieces of data into a coherent whole that is relevant to operation of active fuel cells.

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

This project was rated **3.4** based on accomplishments.

- Well done on evaluation of peroxide formation as function of temperature and Pt crystallite size is new and an important contribution.
- Agreement of theory and experiment confirms understanding and hypotheses, and paves way for progress.
- More connectivity to actual stack lifetime and durability campaigns and data would help establish connection to audience and ultimate pursuits.
- Good to identify significant factors of size and shape effects. Poor explanation how to obtain H_2O_2 formation mechanism by using test data presented.
- Significant progress in characterization of peroxide formation as a function of potential and in verifying the effect of alloying on the d-band center of Pt.
- Very interesting and relevant results were reported. The analysis provides clear ideas of the important factors affecting the ORR mechanism.
- Progress seems slow. The progress made over the past three years is not obvious.
- Good basic science work. Keep it up!
- Ross continues to make real, significant contributions.
- Blending of theory and experiments on model catalysts to explain why some alloys are effective.
- Quantization of peroxide formation as function of catalyst composition and operating temperature.
- Demonstration of the importance of catalyst particle size and shape in controlling activity.

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

This project was rated **3.1** for technology transfer and collaboration.

- Excellent collaboration and support of applied development with fundamental research.
- Very strong MEA developer (3M) and integrator (GM), but would encourage involvement of more partners.
- No strong indication of Technology Transfer in this presentation, though past presentations have shown strong collaborations with GM and 3M.
- Collaborations are mentioned, but their contributions seem negligible.
- Need to establish collaboration with more MEAs and catalyst manufacturers/suppliers.
- Excellent interactions with industrial partners.
- Formal interactions with 3M have benefited both this project and the 3M advanced MEA project by establishing importance of catalyst structure and legitimizing what otherwise seemed to be magical aspects of the claims for the 3M whisker systems.
- Less formal consulting within DOE and with many elements of the fuel cell community has opened many eyes towards productive directions in solving durability issues.

Question 5: Approach to and relevance of proposed future research

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

- Non-carbon support work and PtRe catalyst evaluation. Work planned is of importance.
- Consideration of bimetallics would be strongly benefited by tighter collaboration with catalyst companies.
- A clear justification for the future research directions was not presented, but was alluded to in the answers to questions.
- Proposed future works are with the same high quality as those of the present steps.

- Good to focus on shape effect and support material.
- What is the value of conducting a remanufactured MEA? Especially when all MEAs are different in shapes, thicknesses, size, etc.?
- How is all this knowledge transfer when Ross retires?
- PI intends to finish these studies and then retire.
- Important to get these important results fully documented.
- It is not clear that Re is the best choice as a non-Pt core for a shell electrode. Pt-Re alloys showed no activity improvement upon annealing, so it's not clear that a stable Pt-only surface would have or maintain increased activity. Re is also very expensive. Wouldn't W or maybe Ta also satisfy the surface energy requirements to give a Pt surface (they would, admittedly, be harder to prepare with an initial oxide-free surface)?
- Plan of the PI to retire would deprive the fuel cell community from full-time access to an important resource. It is to be hoped that significant part-time fuel cell activities will continue.

Strengths and weaknesses

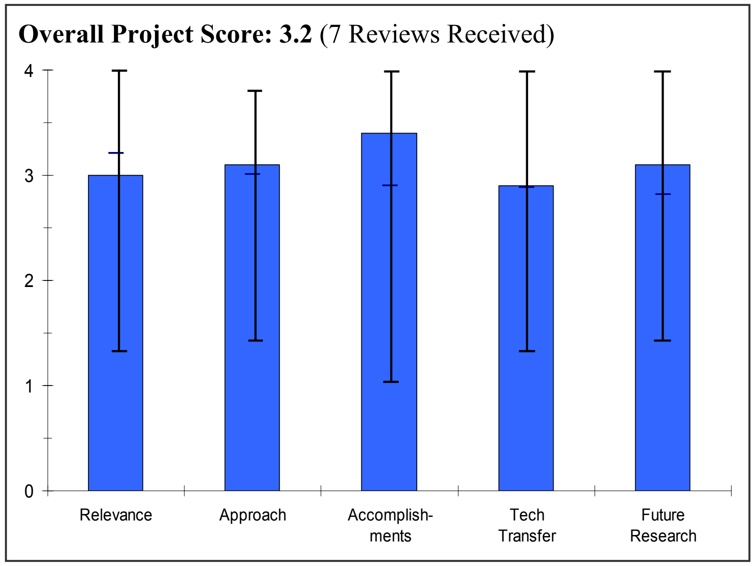
Strengths
- Use of scientific fundamentals to address key industry challenges.
- Good focus on shape effect and support material.
- Research such as this, that bridges the gap between very basic research and applied materials development efforts, is necessary. Understanding the basis for not achieving the expected activity improvements with Pt_3Co is also important. Also, the understanding of peroxide formation is necessary to understanding degradation mechanisms and to avoid operating conditions that will cause membrane degradation.
- Clear understanding of the factors affecting the ORR mechanism and electrocatalysis.
- Some very good basic science work.
- Carefully thought-out and executed experiments to demonstrate the fundamental origins of improvements in activity.
- Blending of theory and experiments on model catalysts in the context of operation of real fuel cells.
- Modeling capability on catalyst.

Weaknesses
- Lack of more familiar durability datasets.
- Lean collaboration team.
- Modeling capability on catalyst.
- Lack of rationale in the presentation for choosing the Pt-Re system and indication of how to achieve the desired nano-scale catalyst structures (e.g., rods vs. cubo-octahedra particles).
- Real fuel cell testing required.
- Need to establish collaborations with suppliers/manufacturers of MEAs and catalysts.
- It is unclear how the work of Dr. Ross will be transferred as he retires. I hope that all the investments over the past years are not lost in the transition.
- How is this work transfer to catalyst and MEA suppliers/manufacturers?
- Historically alloys have not been stable. For example cobalt dissolves from Pt/Co alloys, leaving a high surface area-residue of Pt. That Pt slowly rearranges. The result is a measurable voltage enhancement, but that is a short-lived result. There is a possibility that dissolved Co will enter the membrane and decrease conductance.
- This alloy stability issue needs to be considered. A set of experiments that describe the catalyst (size, shape, and composition) as a function of fuel cell run time remains to be done.
- Not all of the quantitative concepts are always carried forward quantitatively as far as they could be. For example, does the equation put forward to explain the effects of OH_{ads} on ORR experiments quantitatively explain the downward concavity in the Tafel slopes, using reasonable adsorption parameters?

Specific recommendations and additions or deletions to the work scope

- Characterization of additional skin-core structured catalysts with a wider range of compositions (i.e., ternaries?).
- Conduct single cell testing with some collaborators.
- Need to establish collaborations with suppliers/manufacturers of MEAs and catalysts.
- Need to figure out how the work of Dr. Ross will be transferred to colleagues and industry as he retires.
- PI's results show that peroxide is produced at the anode as well as the cathode. This explains a lot why thin membranes have such poor durability. Oxygen is appreciably soluble in fluorinated materials (like Teflon = Nafion). Membrane developers need to measure oxygen solubility in their membranes, because these results suggest that polymer decay is the result of oxygen transport, and depends, being facilitated transport, on solubility. Air bleed into the anode obviously is good way to make anode peroxide. Probably not a good idea.
- Consider another choice than Re for the cores of shell/core catalysts.

Project # FC-11: Non-Precious Metal Catalysts
Zelenay, Piotr; Los Alamos National Laboratory



Brief Summary of Project

This Los Alamos National Laboratory (LANL) project on non-precious metal catalysts for polymer electrolyte fuel cells focuses on performance stability and the mechanism of oxygen reduction reaction (ORR) on pyrolyzed N_4-chelates, including identification of the active catalyst sites. In a later phase, the project will also include an initial viability study of other non-noble ORR catalysts, such as metal oxides (NiO, Co_2O_3, $NiCoO_2$, perovskite LaSrCo oxide, Cu-Mn oxides) and, in another group, cluster-like materials derived from ruthenium selenides (Ru_xSe_y).

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- There is a certain possibility to develop the catalysts with satisfactory properties to replace Pt using this approach.
- In principle, the project addresses one key barrier, of cost, by focusing on non-precious metal catalysts. It will be important to consider all aspects of cost; particularly process costs if one of the materials sets being considered were ultimately to be scaled up for required quality control for real applications.
- Performance target is unclear. Not consistent with MEA performance target.
- Reduced cost electrocatalysts are a key enabler for automotive fuel cell commercialization
- A gap seems to exist between the potentiality of the non-precious catalyst performance and the general expectation for the fuel cell efficiency.
- Reducing catalyst costs cuts device costs.
- Essential to find lower cost catalysts.
- Focused on one critical barrier.
- Exploration of non-precious metal catalysts important research area.
- Specific DOE targets/barriers are not highlighted.

Question 2: Approach to performing the research and development

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

- The approach is based on the analysis of the results of several groups that were engaged with some success in similar studies. This was a good approach for the start.
- Their approach is based on some established material paths suggested in the literature and are credible.
- The role of the silica was not clear.
- Theoretically good to identify active site and focus on its distribution to improve performance. However, performance target is not supporting DOE MEA target.
- Methodology is adequate and will surely lead to conclusive results.
- Important synthesis of catalysts done off-site at a university, perhaps prepared without adequate documentation.
- Program requires reliable, reproducible catalyst sources.
- Parallel research in the areas of catalyst activity and stability studies is good approach.
- Approach is logical.

- Approach is very academic.
- Industrial contributions should be incorporated into the project from the beginning.

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

This project was rated **3.4** based on accomplishments.

- For identifying the active centers of pyrolyzed macrocycles, more sensitive techniques seem necessary than those used so far. Excellent activity was obtained with Co-TPP.
- The CoTPP and chalcogenide performance results are amazing given how quickly they were achieved compared to many other research groups' efforts in the area of non-precious metal catalysts, and how relatively few research dollars were required. The tests to check that there was no chance of Pt on the cathode are good. It is a concern however if the excessively high anode loadings have any effect in some unexpected way, e.g. a fraction of a per cent of Pt migration from the anode to the cathode. It would only take a few micrograms/cm^2 of Pt on the cathode to give the performance seen. It is critical then to characterize the tested MEAs for Pt migration after fuel cell testing.
- Non-PM catalyst performance is equal to, or best to date. Good progress for this one-year-old program. However, there is still a very long way to go before these, or any other, non-PM catalysts exhibit viable performance for automotive application.
- Quite significant accomplishments were achieved. A large gap between the performance of a Pt/C and of the non-precious catalysts was clearly evident.
- Fuel cell results are less exciting than expected.
- Performance goal should approach 0.7 W/cm^2! Results are far from this goal, too far.
- This catalyst system is just not useful in acid solutions; this is well documented in the literature. (The catalysts do work well in basic solutions, say KOH fuel cells, and there they show better performance and lifetime than PM catalysts.) There needs to be some real thought about this stability issue.
- Good progress.
- Significant number of milestones met or exceeded.
- Unclear if on track to meeting key DOE technical barriers/targets.
- Date for project to conclude unclear, 2005? 2006? Not apparent that targets will be achieved.

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

This project was rated **2.9** for technology transfer and collaboration.

- Excellent collaboration from the start of the project.
- Very good collaborations at this time, but it was not explained why non-precious metal catalysts would be as critical for small portable devices where the $/kW are orders of magnitude higher than for automotive.
- Collaborations with the different research centers seem not significantly implemented.
- Making catalysts is a complicated task.
- Essential to find other catalyst vendors who can provide a well-documented and reproducible set of materials.
- Large number of collaborative partners.
- Continue to strive for participation of all partners in overall project.
- Dissemination of non-proprietary results through additional publications recommended.

Question 5: Approach to and relevance of proposed future research

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

- The plan is realistic and addresses the key problems of these catalysts.
- Good objectives, particularly to carefully try to identify the ORR site. Further emphasis on understanding mechanisms rather than demonstrating the highest fuel cell performance should be considered. The PI should carefully continue to look for alternative artifacts that could somehow contaminate their Cathode with Pt or Ru. Look carefully for traces of Pt on the tested MEA cathodes. Another suggestion is to use a non-Pt hydrogen

oxidation catalyst that will still be adequate for the cathode current densities seen but which will completely eliminate any chance of Pt contamination, e.g. Pd the HOR catalyst.
- Formation of H_2O_2 should be investigated also.
- Properly isn't rushing to scale up.
- Future works seem to present the right focus and aim to a better understanding of the phenomena reported during this year.
- Reasonable focused work.
- Material cost should be discussed, if it is in the DOE target description.
- Good overlap between catalyst activity work and stability work.
- Catalyst stability vs. durability should be addressed.

Strengths and weaknesses

Strengths
- This is an excellent group of researchers that can address this difficult task.
- Strong fuel cell fundamentals team. Significant experience of U. of NM in synthesis of these materials.
- Implementation of electrochemical measurement on catalyst evaluation.
- Non-PM performance is equal to or better to date.
- The large progress on the research, and the impressive performance of the non-noble catalyst.
- The DMFC data are interesting.
- Strong team.
- Important technical issues addressed.
- Sound technical approach.

Weaknesses
- Performance is still far below that useful for automotive commercialization.
- There is still a gap between the Pt and the non-precious metal catalyst performances.
- CoPP types of catalyst are just not sufficiently stable for consideration.
- The goal is to get the fuel cell system to be low cost. The catalyst cost is part of that cost. A poor catalyst, no matter how cheap, will never result in a low cost fuel cell. Higher voltage performance needs to be demonstrated soon, or this class of catalysts needs to be eliminated from consideration.
- No discussion of project status relative to DOE targets/barriers.
- Total project timeline vs. budget is unclear.
- Significant performance improvements remain relative to commercially available Pt catalyst.

Specific recommendations and additions or deletions to the work scope

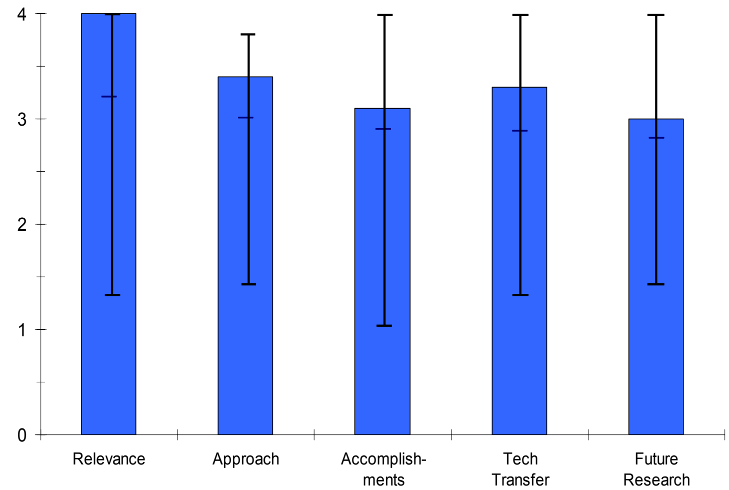
- Identity performance target which is consistent with DOE MEA performance target.
- When applicable, concentrate on performance in the 0.6-0.8 V regime. High power density at <0.6 V is not useful for automotive as efficiency will be too low.
- When performance improves, expand to investigate low RH conditions.
- Report results in the format of non-PM catalyst roadmap target: activity per volume of supported catalyst. 2010 target: >130 A/cm^3 @ $800mV_{ir\text{-}free}$.
- Work on direct-methanol is not applicable to DOE program: discontinue.
- Without a strategy for long-term stability (10,000 hours), this project needs to be reconsidered. The use of pyrolyzed porphyrins was well-studied on PEM systems earlier. Those data showed good performance, but not for long. The challenge is to achieve lifetime with these materials.
- More industry participation.
- Include timetable, milestones, go/no-go concepts into future plan thought process. Otherwise screening can continue indefinitely.
- Need to realistically assess if proposed catalysts are feasible relative to ability to commercially manufacture.
- Need to compare material costs of proposed non-precious metal catalysts to material cost target.

Project # FC-12: MEA and Stack Durability for PEM Fuel Cells
Hicks, Mike; 3M

Brief Summary of Project

During this project, 3M will determine root causes of membrane electrode assembly (MEA) failure modes and develop an MEA with enhanced durability and maintained performance that can be manufactured in a high volume process, meets market required targets for lifetime and cost, and is optimized for field-ready systems. The system demonstration will be for >2,000 hrs. The focus will be on MEA component development, MEA characterization and diagnostics, and defining a system operating window.

Overall Project Score: 3.4 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- The project is very relevant to the R&D targets.
- Strong team
- This project is extremely relevant to the DOE objectives of quantifying and improving durability and performance.
- Many aspects of fundamental durability issues critical to PEM fuel cells for all applications are covered.
- Improved membrane durability is critical to the success of the hydrogen program.
- 3M makes a good point regarding system effects on component durability. Project breakdown to component effects is indeed helpful.
- Progress towards automotive targets as well as toward stationary should be presented, even if the fuel cell developer associated with the project is in the stationary business.
- The Principle Investigator's answer (in response to question on progress toward automotive targets) that accelerated tests for stationary are just automotive conditions, is largely correct, but a bit of explicit attention to the automotive situation would improve effectiveness of the work for both applications. Page 19 comments that catalyst support degradation is not a critical barrier seems to indicate a restriction of thought processes to stationary applications – note that even with proper mitigation strategies in place, automotive applications put a lot of stress on to catalyst supports.

Question 2: Approach to performing the research and development

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

- Components behavior and ex situ studies are good as the first effort. In situ methodology should be developed.
- 3M uses a well thought approach to evaluating the various mechanisms contributing to the loss of performance.
- How about 100 degrees C/25% RH H_2/O_2 open circuit voltage (OCV) stability?
- The strategy used to integrate system level issues into test plan is excellent.
- The approach using model compounds to study membrane degradation processes is excellent.
- Membrane degradation should be quantified using additional physical property characterizations rather than just weight loss and fluoride ion release.
- 3M illustrates a good understanding of gas diffusion layer (GDL) permeability re: pore size. There is a clear distinction between chemical (peroxide effects) and mechanical failure modes, but are F^- ions the only evidence?

- Government-funded research should concentrate on optimizing MEAs and components for durability. The second topic on slide 4 "Optimize System Operating Conditions to Minimize Performance Decay" is a topic better covered in a competitive development environment. At least in the early stages of the project, it has seemed to emphasize the former. The 2000 hour system demonstration target could entail primarily the latter. This demonstration, however, may have arisen from terms of the original DOE solicitation that perhaps should be reconsidered.
- Overall approach is good -- component-by-component evaluation of individual degradation modes and means to mitigate them, primarily through materials solutions.
- Attempt to match projections from accelerated tests to actual lifetimes under other conditions is a very nicely-planned experiment -- it will be interesting to see how it comes out.
- Accelerated degradation testing approach is good.
- 3M will need to establish correlation between accelerated and actual degradation.
- Recognition of the effect of system operation on durability is good.

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

This project was rated **3.1** based on accomplishments.

- 3M has made excellent progress in all areas.
- Good improvement of the MEA stability was achieved in accelerated tests.
- 3M shows good progress in identifying reasons for loss due to GDL oxidation at high voltage operation.
- There may be too much relevance placed on "Fenton" test and "fluoride" release.
- The presentation was clear and well articulated. However, interactions of separate component improvements on overall improvement were not explained well.
- New 3M membrane materials, with stable end groups and appropriate additives, would appear to provide an important tool towards reaching durability goals.
- Result that tear strength is unchanged after up to 25% membrane weight loss is quite remarkable; it helps set targets for allowable membrane chemical degradation rates.
- From the presentation it was not immediately clear how some of the accomplishments, e.g. membrane additives, differ from the accomplishments presented last year or under earlier 3M projects.
- Identification of the class of additives used, e.g. antioxidant or Fe2+-sequestering agent, would help fuel cell developers better plan to make use of the advantages offered by advanced 3M materials.
- CO + air bleed effect increasing F^- generation is nice work, but wasn't the effect of CO + air bleed in increasing peroxide generation demonstrated in the open literature a while ago?
- Results to date are very promising.
- The presentation did not address the improved-durability membranes' effect on performance or cost.

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

This project was rated **3.3** for technology transfer and collaboration.

- There are multiple collaborators at universities and in industry.
- It is not clear how the partners, i.e., CWRO and Plug Power, participate or contribute to the program.
- Work assignments are not very clear -- what did University of Miami do?
- Interactions with Plug seem to be going and probably will become more apparent later in the project. The presentation did not really define how the collaborations with Case Western are advancing the art.
- The collaboration has improved since last year.
- One reviewer asked, "Will other companies benefit besides 3M?"

Question 5: Approach to and relevance of proposed future research

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

- It is difficult to judge with undisclosed chemistry.

- Detail on future research plans is lacking.
- Need to evaluate effect and contribution of catalysts to the failure or decrease of performance of long term test experiments.
- 3M needs to clarify which components are expected to add the most value.
- One reviewer expressed concern that too much of the future effort will be wrapped up in a system demonstration rather than in further improvements in materials and accelerated testing methodologies.

Strengths and weaknesses

Strengths
- Syntheses and production facilities are excellent.
- Development of accelerated test protocols is important and well addressed in the program.
- Integrated approach is excellent.
- The approach is very methodical: GDL permeability, chemical/mechanical degradation correlations, segmented cell to see inlet/outlet variations. Good work was performed on GDL surface chemistry effects.
- 3M presented some apparent significant steps forward in membrane durability.
- There appears to be improvement in understanding of how to make more durable electrode structures. (However, insufficient details were shared to allow a real judgment to be made).
- 3M has made a nice start on an attempt to map results of accelerated tests onto unaccelerated lifetimes.
- Recognition of the effect of system operation on durability is good.
- Accelerated degradation approach is good.

Weaknesses
- The presentation did not address the effect of catalyst degradation.
- Mechanical strength should not be the only measure of membrane performance degradation after Fenton's test. Instead of using mass loss to determine membrane degradation, use a physical property measurement, such as viscosity or conductivity. Degradation of the MW and loss of sulfonate groups may not be evident in mass change measurements. Shouldn't the performance of the MEA be the metric for the quality of a component rather than fluoride ion release and projecting that this will affect the performance? Other factors may affect performance; fluoride ion release is an indirect method.
- COOH end-groups are known to be unstable (which was recognized in the course of this project). Why not combine GDL effects and membrane improvements in the study? It is not very clear how the lifetime predictions were made.
- Page 13 addresses break-in of MEA, not startup of fuel cell (language susceptible to temporary misinterpretation).
- The presentation should have clarified what is new (versus accomplishments from last year or from previous projects.)
- Given the broad scope of the project title, one might have expected a bit more attention to catalyst durability (but attention to this topic has been given under another 3M project.)
- 3M needs to address other targets in concert with durability (e.g., cost, performance).

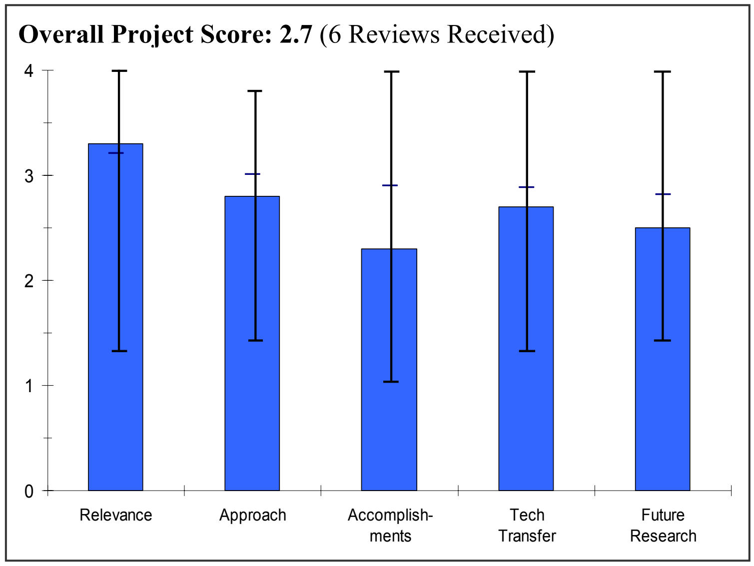
Specific recommendations and additions or deletions to the work scope

- Evaluate catalyst degradation and how it affects the overall system/MEA durability.
- Perform additional characterization of membrane physical properties and effect of aging on these properties.
- What about considering reinforced membranes? Determine the source of F^-: membrane or electrode formulation? 3M needs to clarify which component added the most value from A to C improvements.
- Extensive hot/dry accelerated testing should be supplemented by some sensitivity testing of individual phenomena, lest surprises arise from cases where hot/dry isn't the most aggressive condition. Page 6 is a case where the program already properly addressed this kind of issue.
- Enhanced collaboration on the system operation effect on membrane durability could improve understanding of decay mechanisms and solutions.

Project # FC-13: Development of Transition Metal/Chalcogen Based Cathode Catalysts for PEM Fuel Cells
Campbell, Stephen; Ballard Power Systems

Brief Summary of Project

Ballard Power Systems intends to develop a non-precious metal cathode catalyst for PEM fuel cells that will be as active and as durable as current platinum group metal (PGM) based catalysts, at a significantly reduced cost. The plan includes development of composition and structure, process development (can be scaled up), and evaluation/demonstration in fuel cells and stacks.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- There is a certain possibility to develop the catalysts with satisfactory properties to replace Pt using this approach.
- Critical to reduce or eliminate the high cost component of MEAs - platinum.
- Supports the goals of the HFCIT program for decreasing catalyst costs.
- Objective of project is highly relevant to DOE goals to develop non-platinum group catalysts.
- Non-PGM catalysts clearly on critical path to cost target of $ 40 /kW.

Question 2: Approach to performing the research and development

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

- Studies of new chalcogenides with well justified fundamentals for an expected activity for the ORR could produce interesting catalysts. This aspect should be visible in future work.
- The project has a lack of focus on durability testing prior to incorporation into stacks.
- How does the cost compare to conventional materials?
- Good to see congruence between sputter-deposits and supported catalysts, although some chalcogens can be multi-phase so there may always be the possibility of having to make sure the best sputter-made compound(s) can be made in supported form.
- Strongly endorse the approach of checking for oxygen substitution: would suggest simple “shelf life” tests open to the air to make sure stable to slow oxygen replacement.
- Incorporate some sort of “OCV” testing in durability plan.
- Thin films can provide well characterized structures with defined surface areas.
- Films expose only one crystal face, while particles will have other faces exposed also.
- The approach of using sputtered thin films for comparison and structural/compositional/activity characterization is excellent to avoid the difficulties with synthesis and characterization of high surface area catalysts.
- A combinatorial approach is probably a good one for projects pursuing non-PGM catalysts, but a 3x3 matrix can hardly be considered "combinatorial". No justification was given for such a limited "library".

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

This project was rated **2.3** based on accomplishments.

- Despite good research effort, the activity of the best catalyst for the ORR was low.
- There is a need for a new approach in selecting promising systems.
- Good analytical basis on results
- “Absolute” electrochemical performance has a ways to go.
- Fe and Cr selenides do not look promising at all, while Co selenide shows a little promise.
- Thermodynamic potential for Co selenides probably too low to be useful.
- An impressive number of catalysts have been screened and characterized. The activity of these catalysts for the ORR is very low, even when going to carbon-supported catalysts indicating that these catalysts may just intrinsically have low activity, not limited by surface area.
- For a combinatorial approach, I consider the results to be rather poor. None of the catalysts were even close to meeting the targets, not even close to the results on non-PGM catalysts reported by others at this meeting. One of the benefits to the combinatorial methods is that you can identify trends in activity without knowing the mechanism of action. But that is not possible if the library is so small. Very disappointing effort.

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

This project was rated **2.7** for technology transfer and collaboration.

- Collaboration with two Universities seems to be in place.
- Moderately good collaborations.
- Collaboration with a catalyst group or physical chemistry group specializing in surface adsorption techniques to develop a method for measuring surface area of the chalcogenides would be helpful.
- Not clear what university partners are doing to contribute to project or that a strong collaboration exists.

Question 5: Approach to and relevance of proposed future research

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

- More promising systems should be selected.
- More electrochemical testing. See recommendations below.
- Has the PI formulated a complete matrix for selecting acceptable catalysts? For example, minimum current density, stability under defined conditions, etc?
- Investigating the sulfides is the next logical step. If thermodynamics of these systems is no better then direction should be changed to find a thermodynamically beneficial system.
- A clear route to obtaining higher activities was not established, especially since it appears that the material is intrinsically inactive with a low thermodynamic potential for ORR.
- No vision.

Strengths and weaknesses

Strengths
- Good science and characterization possibilities.
- Solid analytical/structural capability: good use of an extensive portfolio of analytical instrumentation.
- Good approach of using sputter method for new lead development, and reduction to “catalyst” through supported form.
- Provides well defined systems to look at so rate information and mechanistic details easier to obtain
- Approach to looking at well-defined and characterized thin films as a screening method before attempting synthesis of high surface area materials.
- Incorporation of a Go/No-Go decision at the end of FY'05.
- Ballard has the resources and seemingly the interest in a combinatorial synthesis-rapid screening protocol.

Weaknesses

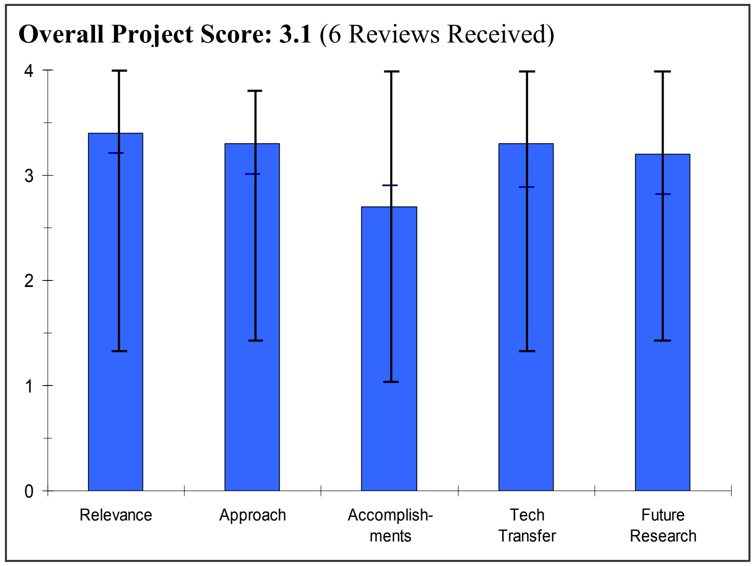
- Electrochemical testing should be expanded in the analytical direction (See RRDE below).
- Durability to "open circuit potential" or simulation thereof is missing in the approach.
- Problems with film growth and adhesion have slowed progress.
- Should normalize ORR currents to catalyst surface area (BET area perhaps?) or catalyst weight to allow comparison of activities.
- The combinatorial synthesis-rapid screening protocol was poorly executed, consequently producing neither a lead to a new catalyst nor any new information.

Specific recommendations and additions or deletions to the work scope

- Incorporate RRDE measurements to check for peroxide: if it's too late for RRDE purchase, use plots on Koutecky-Levitch and use the slope to estimate effectiveness for 4 electron reduction.
- Open circuit testing for stability, or simulation of process.
- A dual benchmark should be presented on the yearly review: fuel cell polarization or electrochemical test comparing internal progress compared to initial efforts/catalysts and the same for state of the art chalcogens/alternatives from the catalyst community.
- General comment for all "non-platinum" catalyst programs: Original PEMFC cost targets derived from sophisticated model relating loading of platinum, power output, and current density of MEA to final stack cost. There was a recommended optimum based on the lowest platinum needed to achieve a reasonable current density (do not want large stacks as part costs increase). Does the model need to be re-examined for non-platinum case and a new performance target set (should the 0.6g/kW target be converted to something like $12/kW effective catalyst cost, which would include prep costs, etc.?).
- Would be beneficial to develop a methodology to measure the surface area of these chalcogenide catalysts to allow comparisons of work on dispersed catalysts.
- Conclude project in as timely a manner as possible.

Project # FC-14: Novel Approach to Non-Precious Metal Catalysts
Atanasoski, Radoslav; 3M

Brief Summary of Project



This 3M project will develop and demonstrate non-precious metal cathode catalyst to lower cost (goal of 50% less vs. target of 0.2g Pt/peak kW) and to reduce the dependence of proton exchange membrane fuel cell catalysts on precious metals. 3M will identify opportunities for system cost reduction through breakthroughs in key areas of the fuel cell, the catalyst, and application of cost-effective processes for membrane electrode assembly (MEA) fabrication closely associated with the development of the new catalyst. Sample tasks include investigation of Fe-N-C as a model catalytic site, vacuum 1- and 2- step synthesis processes, combinatorial approach in identifying potential catalysts, nanotechnology processes, and fabrication and characterization of MEAs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Development of a viable non-precious metal catalyst with sufficient performance and durability would reduce fuel cell costs - a major DOE technical objective.
- Critical to reduce or eliminate the high cost component of MEAs - platinum.
- Reduced cost electrocatalysts are a key enabler for automotive fuel cell commercialization.
- The objectives are good and address the longer range issues that are important to the program.
- This project is extremely relevant to DOE's mission if significant activity improvements are made.
- Lower cost electrocatalysts are critical to achieving cost targets for PEM fuel cell systems.
- It is not sufficient to simply eliminate precious metals from the electrocatalysts, but also demonstrate low-cost synthesis methods (this aspect could not be assessed).

Question 2: Approach to performing the research and development

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

- A systematic, combinatorial approach incorporating synthesis of many catalyst samples in the Fe-N-C system.
- Outstanding and comprehensive approach covering all aspects of catalyst discovery: theory, synthesis/experiment, and reduction to practice.
- The use of outside expertise at the National labs and the university communities are excellent uses of available resources and may provide the needed expertise to maximize success.
- Good balanced approach with combinatorial, characterization, and modeling efforts.
- The electrocatalyst development work is augmented by modeling, high throughput screening, and state-of-the-art characterization. At the same time, the high throughput screening methodology used only provides information on initial activity (not durability) and hides information related to the effects of synthesis techniques.
- There has been no real effort to date on evaluating durability of the electrocatalysts.

- It is not clear from the information provided on the "nanotechnology" synthesis process (i.e., none) that the process is scalable to low cost production. It is insufficient to state evidence of scalability based on the "number of fuel cell samples prepared".

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

This project was rated **2.7** based on accomplishments.

- Steady improvement in catalyst activity has been achieved but performance is still only "approaching results reported elsewhere".
- Increase in actual current density with non-precious metals substantial.
- Use of 10% Pt/C unusual as benchmark: this is commonly used in phosphoric acid fuel cell, and occasionally as anode in PEMFC, but not often on cathode.
- Needs stronger durability component.
- Demonstrated considerable progress in catalyst performance in the last year.
- However, still far below viable performance for automotive systems and below modest "interim milestone #2".
- Good progress has been made on a tough technical problem.
- Significant progress has been achieved in improving initial activity levels.
- The infrastructure for catalyst development appears to be well established.
- Durability should have been assessed much earlier in this project.

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

This project was rated **3.3** for technology transfer and collaboration.

- Collaboration with universities and a national laboratory.
- Project would benefit from collaboration with fuel cell and/or commercial catalyst supplier.
- Very good use and coordination between partners.
- One of the biggest pluses for this activity is the ability of the company to address scale up and processing issues hand in hand with the latest research results.
- Excellent collaboration with university partners and clear indication of what they are contributing to project.
- Aside from the collaboration with Dalhousie University, it is not clear that other subcontractors are actively engaged in the project.
- Technology transfer is inherent to the project, since 3M is ultimately capable of commercializing the technology if technical and economic viability is proven.

Question 5: Approach to and relevance of proposed future research

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

- Good plans for completion of project - potential benefit depends on results obtained in the remainder of the project.
- Request emphasis on stability/durability beyond hydrogen peroxide test (OCV stability, cycling, resistance to acid corrosion, temperature stability).
- Scale up ("1-2 kW") should be delayed until performance is within sight of that of Pt catalysts.
- There is a well organized plan for how to address future activities and they are well prioritized.
- A clear path to the necessary activity improvements was not presented.
- Durability testing was listed as an activity for 2006 – this activity needs to occur sooner rather than later.
- A manufacturing cost analysis should be performed – to verify that cost targets are indeed achievable.

Strengths and Weaknesses

Strengths
- A thorough, systematic and committed approach.
- Comprehensive approach and good team to cover not only catalyst discovery but reduction to practice.
- National lab and university involvement. Ability to translate results to the "processing" scale.
- Excellent array of catalyst characterization techniques, collaboration with the Dahn group and BNL, and modeling effort.
- Good combination of synthesis, screening, characterization and modeling are being utilized on the project.
- Excellent technical progress is being made toward identifying active electrocatalysts.

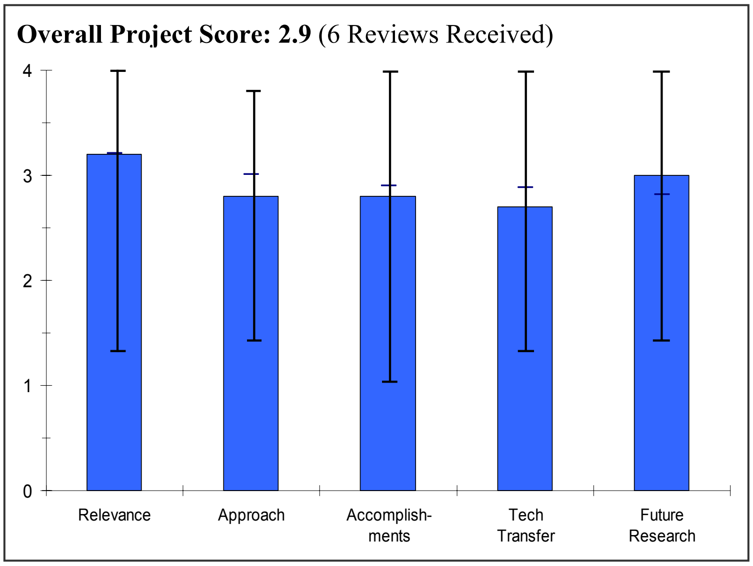
Weaknesses
- Surprising to see that with this level of sophistication in approach, more detailed electrochemical characterization (such as RRDE to look for hydrogen peroxide) or stability tests (thermal, acid, open circuit potential) seem to be missing.
- Being limited by intellectual property considerations from interacting freely and openly with the community at large.
- Catalyst activity is still low, despite improvements, relative to both the Dodelet results on similar materials as these and to platinum.
- Lack of durability testing as a key criterion during compositional screening.
- No real evidence that the synthesis process is scaleable at low cost.

Specific recommendations and additions or deletions to the work scope

- Incorporate RRDE measurements to check for peroxide.
- Open circuit testing for stability: compare to Pt benchmarks.
- Increase routine screening for thermal, chemical (acid), and electrochemical stability (OCV).
- General comment for all "non-platinum" catalyst programs: Original PEMFC cost targets derived from sophisticated model relating loading of platinum, power output, and current density of MEA to final stack cost. There was a recommended optimum based on the lowest platinum needed to achieve a reasonable current density (do not want large stacks as part costs increase). Does the model need to be re-examined for non-platinum case and a new performance target set (should the 0.6g Pt/kW target be converted to something like $12/kW effective catalyst cost, which would include prep costs, etc.?).
- When applicable, concentrate on performance in the 0.6-0.8V regime. High power density at <0.6V is not useful for automotive as efficiency will be too low.
- Some poll curves should use conditions relevant to automotive: 1-3 bar (absolute) air.
- Report results in the format of non-platinum catalyst FreedomCAR Fuel Cell Technologies roadmap target: activity per volume of supported catalyst. 2010 target: >130 A/cm3 @ 800mV ir-free.
- When performance improves, expand to investigate low RH conditions.
- I would suggest that some effort be given to evaluating the best low Pt content catalysts in the MEA configuration as a "control" and challenge point to future Fe catalyst development.
- Suggest incorporation of a Go/No-Go decision at the end of FY'05.

Project # FC-15: Novel Non-Precious Metals for PEMFC: Catalyst Selection through Molecular Modeling and Durability Studies

Popov, Branko N.; University of South Carolina



Brief Summary of Project

The University of South Carolina will synthesize novel non-precious metal electrocatalysts with similar activity and stability as platinum (Pt) for oxygen reduction reaction (ORR). They will focus on high activity for ORR, mass production methods, corrosion resistance, low cost, and improving understanding of reaction mechanism of oxygen reduction on non-precious metal catalysts. Supporting tasks include theoretical molecular modeling, electrochemical characterization, structural studies (XPS, EXAFS, XANES), identifying the correlation among the catalyst composition, heat treatment and catalytic sites for oxygen reduction, and demonstrating the potential of the novel non-precious metal electrocatalysts as a substitute for Pt currently used in membrane electrode assemblies.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- There is a certain possibility to develop the catalysts with satisfactory properties to replace Pt using this approach.
- Addresses in reality, one of the critical issues – lower cost through catalyst cost reduction. Until the performances of any such materials can be made competitive with Pt, it is too early to claim they will help with durability.
- Critical to reduce or eliminate the high cost component of MEAs – platinum.
- Non-PGM catalysts are clearly on the fuel cell electric vehicle roadmap to meeting long term $40/kW target.
- Use of new catalysts based on non-noble materials is of large importance for reducing the fuel cell cost.
- Exploration of non-precious metal catalysts important research area.

Question 2: Approach to performing the research and development

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

- The authors selected Co-based alloys and C precursors for making the non-precious metal catalysts based on promising data in the literature.
- Objectives appear to include everything on a wish list – too many to be realistically accomplished with the resources and time allotted, e.g. it is unlikely the PI will be able to accomplish low cost catalyst through mass production methods at a university - simply because his lab has access to large ovens. Also, to achieve high activity, durability and low peroxide production all at the same time will be difficult in one contract – people have been trying to do that with Pt for years. The PI is aware of the important issues but has inadequate time and financial and personnel resources to accomplish these goals. Various transition metals, including Co and Fe supported on carbons have been studied before, by commercial catalyst suppliers (e.g. Johnson Matthey). It is not clear what will be new here except the removal of the Pt. Ru is a precious metal too – is it being studied as a serious candidate?
- Good approach with outstanding emphasis on electrochemical characterization.

- Increase non-electrochemical stability tests: thermal and acid.
- Start OCV tests and compare to Pt.
- Co chelate is of low cost but the performance may affect negatively the fuel cell efficiency.
- Ru-based materials are of less importance if the aim is to reduce costs.
- Parallel research in the areas of catalyst activity and stability studies is good approach.
- Approach is logical.
- Potential poisoning of membrane should be addressed.
- Approach is very academic.

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

This project was rated **2.8** based on accomplishments.

- Making carbon active for a four electron reduction of O_2 is interesting for some specific applications.
- The activity of Co-C is very good, while the high activity of Ru is not found in the presentation.
- Effect of surface modifiers for influencing the catalyst format and activity is good, but how much of this is already practiced by commercial catalyst suppliers? Working with such a catalyst supplier, as a consultant might be useful and allow moving faster – such as an Engelhard, TKK or JM. In general, too many topics are being pursued, and most do not seem to be on a path that can meet the targets.
- Very good results on innate (RDE) electrochemical activity.
- Good relative absolute activity in single cell testing "within 100 mV of Pt.".
- Again the slides were rather "chaotic" making it difficult to sort out in what the direction the results were going. The results with Ru-based catalysts were said to be just for testing the method of catalyst preparation, but most of the results and the summary appeared to focus on the Ru-based, which are PGM and thus not consistent with the objectives. But some of the results with Co-Fe were interesting and possibly the most promising in all the non-PGM catalyst presentations.
- Performance of Co chelate is still very far from satisfactory. However, stability seems promising.
- Good quality data generated.
- Project scheduled to conclude in two years. A significant amount of research remains in the areas of durability, material costs, and cost of manufacturing the catalyst and the stack.
- Material and manufacturing costs not discussed for any of the catalyst alternatives.

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

This project was rated **2.7** for technology transfer and collaboration.

- There are several active collaborators on the project.
- The collaborations could be usefully broadened. The PI should consider working with a commercial catalyst supplier to gain non-proprietary knowledge on catalyst support modification. Also, some fundamental modeling should be able to answer if any form of Co-Fe could ever have the activity of current state-of-the-art Pt. Current Density Functional Theory (e.g. Mavrikakis at U. of Wisconsin. Madison) should be able to provide some insight.
- Include industrial partner beyond University alliances for external validation.
- So far, the work was quite exploratory and is being conducted satisfactorily with no need of help.
- Work is being disseminated through significant number of presentations/publications within the last year.
- Increase in activity by CWRU and NEU apparent. CWRU and NEU participants are encouraged to also publish contributions to the project.
- Increased industry participation/collaboration strongly encouraged.
- Industrial contributions of Fuji Film and Faraday Technologies not apparent. Neither company listed as partner/collaborator.

Question 5: Approach to and relevance of proposed future research

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

- The authors should explain the role of quinones and their stability.
- Again, the proposed work is very ambitious. The stated future work is primarily a list of goals without an indication of what will be done to achieve them.
- Endorse approach to using RRDE/hydrogen peroxide detection as part of catalyst characterization. However, add other qualifying tests such as OCV testing (or simulation), stability to acid, and thermal stability.
- There is progress to build on. But the Future Work slide was unclear about what approaches were going to be used to improve the Fe-Co catalysts.
- Plans are in the right directions.
- Good overlap between catalyst activity work and stability work.
- Ambitious plan.
- Screening/development activities are still proposed. Will need to focus on specific non-precious metals and begin extensive studies over the next year.
- Catalyst stability vs. durability should be addressed.

Strengths and weaknesses

Strengths
- Excellent synthetic skills.
- Good approach from a materials viewpoint as well as thorough examination of fundamentals behind creation of a successful catalyst.
- Some reasonably good ideas based on previous work in literature are being pursued.
- Good performance of the Ru-based alloys. Very comprehensive studies of the non-noble metal catalysts.
- Solid, experienced team.
- Important technical issues addressed.

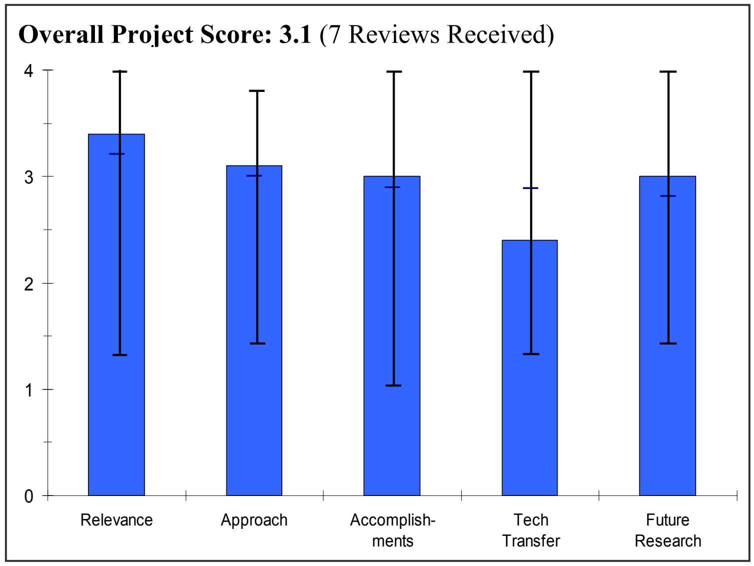
Weaknesses
- Need more efforts on non electrochemical stability tests.
- Not clear things are being done in a systematic manner.
- Low prospects for Co chelates for practical applications.
- Project would benefit from additional collaboration.
- Proposed work still very broad.

Specific recommendations and additions or deletions to the work scope

- Consider focusing on either the metal-free or the non-Pt metal catalysts, one or the other, for faster progress. A more in-depth understanding of one system would be preferable to less understanding of two.
- Increase routine screening for thermal, chemical (acid), and electrochemical stability (OCV).
- Benchmark current cost/performance based on DOE cost targets.
- General comment for all "non-platinum" catalyst programs: Original PEMFC cost targets derived from sophisticated model relating loading of platinum, power output, and current density of MEA to final stack cost. There was a recommended optimum based on the lowest platinum needed to achieve a reasonable current density (do not want large stacks as part costs increase). Does the model need to be re-examined for non-platinum case and a new performance target set (should the 0.6g Pt/kW target be converted to something like $12/kW effective catalyst cost, which would include prep costs, etc.?).
- Try to be more systematic and lay out the results in a clearer manner.
- More industry participation.
- Prioritize proposed studies.
- Include timetable, milestones, go/no-go concepts in future plans. Otherwise screening continues indefinitely.
- Need to realistically assess if proposed catalysts are feasible relative to ability to commercially manufacture.
- Need to compare material costs of proposed non-precious metal catalysts to material cost target.

Project # FC-16: Low-Platinum Catalysts for Oxygen Reduction at PEMFC Cathodes

Swider-Lyons, Karen; Naval Research Laboratory



Brief Summary of Project

The Naval Research Laboratory (NRL) is targeting the DOE goal of 0.2 g Pt/rated kW before 2010 by focusing on lowering Pt in the fuel cell cathode. NRL is using oxide-based supports for Pt and other metals to leverage: oxygen dissociation by oxides, metal-support interactions, and ionic mobility of oxide supports. Objectives for 2004-2005 include rigorously characterizing active and inactive catalysts, devising mechanism(s) to explain catalyst activity, and designing new active and stable catalysts.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Fundamental approaches to PGM reduction in stack critical to achievement of stack cost targets.
- Project addresses important PEMFC cost reduction issues through the use of low platinum catalyst.
- Project well focused on targets for coat, performance and durability.
- Low cost, durable ORR catalysts are essential for achievement of DOE fuel cell targets.
- Catalyst reduction will help reduce cost but it was not reported how big a factor catalyst reduction will benefit program.
- Important problem.
- Unclear how this project will be translated into work that catalyst companies can use.

Question 2: Approach to performing the research and development

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

- The project did not build up on the successes presented a year ago.
- Development of microkinetic model would enhance understanding of the mechanism, rather than reliance of potential scan curves and Tafel slopes.
- Kinetic model would provide prediction of rate versus temperature and concentration. This would provide validation of the underlying mechanism.
- Kinetic model parameters could then be correlated to quantum mechanical models of the various materials
- Project approach is suitable to the planned goals.
- Approach redirected per 2004 comments and recommendations.
- Approach is well reasoned and implemented, but this is a difficult technical challenge.
- Very good approach.
- Appears to be well thought out.
- Is there a conductivity problem using TaPO?
- Strong combination of methods used for characterization.
- Well-reasoned approach - a little off-center, but we need this to make progress.
- Needs to get this catalyst into fuel cells.
- Needs still more characterization to understand nature of observed effect.
- PI is particularly well-attuned to guidance from reviews.

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

This project was rated **3.0** based on accomplishments.

- Two many different experimental attempts, not necessarily all contributing to the goal of the project.
- Project has gotten insights into the mechanism by comparison of the electrochemical performance of the Ta and Fe forms of the catalyst.
- Project has shown the potential of this approach for reducing platinum loading.
- Project shows clear accomplishments concerning the electrochemical properties of the new catalysts.
- Project is making progress towards performance goals, but it's not clear on progress towards cost and durability goals.
- Good progress in understanding of mechanistics- the 'key' to success.
- Good progress on new catalyst.
- Good representations of data.
- Problem with Pt metal and Pt ions clearly identified even though a negative result. Still helps to make decisions.
- Really needs some fuel cell testing of catalysts.
- Some interesting results and observations.
- Steady progress.

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

This project was rated **2.4** for technology transfer and collaboration.

- Not clear why the project has difficulties finding partners for large scale testing. Some explanations were provided.
- The PI could interact with other universities to gain further insight into the mechanism and kinetics.
- External interactions appear to be focused on testing and evaluation.
- PI needs to strengthen / increase collaboration with other institutions.
- Project shows good number of publications, including two patent disclosures.
- Good collaborative effort, but should get better engaged with industry partners. Not as involved with GM & E-TEK this year.
- Early days yet. Collaboration will definitely help with the assessment of durability in MEAs.
- Technology transfer did not appear to be done in this last reporting period.
- Needs to provide path forward for tech transfer.

Question 5: Approach to and relevance of proposed future research

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

- Focus on Pt; avoid venturing in non-Pt yet. The support is complicated as it is.
- Future effort should include effort to quantify mechanism with kinetic and quantum mechanical models.
- The models would provide validation of the mechanism (temperature and concentration dependence) and guidance on how to enhance materials and improve sulfur tolerance.
- It is not clear if the project will be able to overcome the technical barriers in a timely manner.
- Future work focused on DOE goals.
- Profitable areas of future work have been well identified by the PI.
- Future work appears to be well-planned and has good mix of chemistry, electrochemistry, and analytical methods.
- Not sufficiently addressing how to build on her understanding and extend lessons learned.

Strengths and weaknesses

Strengths
- Good potential for catalyst development. The novelties have to be exploited further.
- Fundamental approach to reducing platinum loading.
- Project results related to the electrochemical properties of the new catalyst.
- Technology transfer: good number of publications.
- Goal focused.
- Addressing one of most important challenges – cost.
- Approach is based on sound, proven technology i.e. oxides or metals supported on oxides- greater prospects for success.
- Good approach with lots of characterization.
- Very conscientious PI.
- Essentially fundamental project with results that the community can learn from.
- Approach is not the usual one, but we need this type of creativity to make progress.

Weaknesses
- It seems that the project was slightly defocused in 2005.
- Should compare activity of catalysts on catalyst surface area basis.
- Project relationships with industry and other research institutions.
- No definition of the percent of completion.
- Project activities have drifted away from industry partners in last year.
- Progress to cost and durability goals not well addressed in presentation.
- Need to progress soon to assess durability, particularly since stability/oxidation issues already identified. Do not claim10x reduction in Pt until durability confirmed.
- Needs to provide path forward to taking this essentially fundamental approach and taking the steps necessary to see it to practicality - use in fuel cells.

Specific recommendations and additions or deletions to the work scope

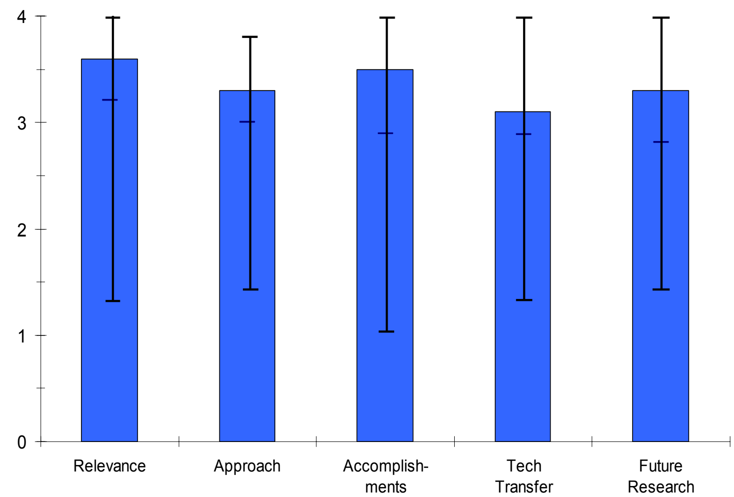
- The project should focus on the synthetic aspects of new low Pt catalysts based on the variety of substrate materials. In-depth materials characterization should be limited for the most successful catalysts and mechanistic ORR considerations on these less understood catalysts should be delayed.
- Re-connect with industry partners.
- Work with MEA suppliers.
- Future work outlined in the presentation is appropriate, and overall this project is good value for money spent. Later in the project, need to have any claimed cost reduction independently verified, i.e., what $/kW catalyst cost are we looking at?
- The program should be continued even though some difficulties are evident. The approach is very creative.

Project # FC-17: Low Pt Loading Fuel Cell Electrocatalysts
Adzic, Radoslav; Brookhaven National Laboratory

Brief Summary of Project

The purpose of this Brookhaven National Laboratory (BNL) project is to develop low platinum-loading electrocatalysts. The objectives are: to demonstrate the stability of the Pt monolayer electrocatalysts for O_2 reduction in fuel cell tests (milestone experiment), to further the understanding of the properties of Pt monolayer electrocatalysts, to improve the activity of Pt monolayer (Pt/Pd/C) electrocatalysts, to improve the syntheses of electrocatalysts with ultra-low, or no Pt content: Pt/Au/Ni and Pd_2Co, and to test them in fuel cells (milestones 2004) and to explore a novel class of electrocatalysts for O_2 reduction consisting of mixed monolayers of Pt and late transition metals.

Overall Project Score: 3.4 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Achieving lower Pt loadings with cheaper electrode materials at higher fuel cell performance is critical to cost effective fuel cells.
- Platinum remains key cost. Program is well aligned with goals and objectives.
- This effort is facilitating a fundamental understanding of low catalysts loading-essential to achieving long term goals.
- Further reductions in noble metals required are necessary to meet automotive cost targets and could be beneficial for other applications with less aggressive cost targets.
- Directly addresses catalysts Pt loading and durability improvement goals.
- Critical route to cost reduction.
- Innovative approaches to high activity with low loadings of Pt group metals are needed if fuel cells are to meet automotive cost goals. This project investigates several such approaches.
- The work helps to establish how little of an alloying element (or Pt) is needed to give the full activity of a properly-prepared alloy catalyst.

Question 2: Approach to performing the research and development

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

- A number of interesting material options are being investigated - Monolayer Pt/Pd/C, Pt/Au/Ni and Pd_2Co - that minimize materials usage or are lower cost. Electrochemical characterization of the potential of these materials is good for the scoping exercise to date.
- In the future would like to see more confirmation of results achieved so far on realistic fuel cells of the most promising candidates in terms of activity and stability to ensure that this is not just a laboratory curiosity. Since materials scale up is not a problem, this should not be that difficult.
- Very good combination of theory, experiment, and scaling to single-cell evaluation to validate findings.
- Approach is solid. Uses facilities, technique and resources appropriately.
- It is clear what theoretical considerations are used to determine the materials that are synthesized and tested.

- However, fuel-cell testing appears to have been limited to steady-state holds, to date. This is unrealistic, especially for transportation applications.
- Monolayers are a good approach to addressing Pt loading.
- Approach is sound.
- The catalyst synthesis methods are not robust (following reproducible conditions using well-characterized starting materials.) There needs to be discipline here, assuring that catalyst preparations give reproducible results. A series of experiments with the same catalyst formulation done repeatedly is needed to convince. (So many other factors I in the MEA formulation may also affect results).
- Overall approach is excellent, particularly the pursuit of monolayer catalysts first in smooth, often single-crystal, electrodes, then in supported systems.
- Biggest weakness is too much emphasis on Pd as essentially a support. One cannot assume that Pd would remain cheaper than Pt, and while a few sources of Pd are largely independent of Pt, large-scale use of Pd would mean most of the Pd would come from the same mines, all in a geographically-limited area, that produce most Pt.
- Pt/Au (Ni) work shows a possible way to avoid the use of Pd while maintaining the advantages of the Pt monolayer catalysts. Relatively little Au seems to be needed. Au is always cheaper than Pt, and Au comes from more widely-distributed sources.
- Nice blend of theory and experiment. Explanation of different tafel slopes for monolayer and mixed-monolayer catalysts vs. bulk Pt is especially thought-provoking.
- It would be good to do the RRDE work at 60ºC rather than 25ºC to make the data more directly relatable to fuel cells.

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

This project was rated **3.5** based on accomplishments.

- Achievements of 20x improvement in Pt activity in PtRe/Pd/C and in Pt/Au/Ni/C catalysts that have been validated by preliminary fuel cell test results at LANL is a solid achievement and confirms that this is a promising approach.
- Precious metal content and lifetime goal achieved.
- Verification by third party (LANL) key milestone.
- Excellent progress. Expanding and building on past. 3000 hour test lends credibility to approach.
- (3000 hr) durability test protocol needs to include stress factors, consistent current testing is not sufficient to evaluate stability.
- Project has good grasp of what needs to be adjusted and has shown excellent progress.
- Activity improvements are impressive.
- Interesting results, but other precious metals are involved (Ru, Pd Ir) which are also costly.
- Pt/Re/Pd data are exciting.
- Activity per mass of total noble metal appears excellent, particularly for the Pt/AuNi system.
- Long-term (3000 hr) stability of Pt/Pd/C seems a bit questionable- 140 mV is a lot to lose.
- Demonstration of mixed-monolayer effect suggests a design rule for alloy catalysts.
- It should be noted that achievement of 2005 target of 2.7gPt/kW is not anything that special. Normal catalyst systems are already bettering that value (status in roadmap is now below the historical 2.7 figure).

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

This project was rated **3.1** for technology transfer and collaboration.

- Good interactions established with LANL and Plug Power but would like to see fuel cell testing enhanced in the future. Are Pt monolayer materials survivable in a fuel cell environment? Can the OH-OH coverage in the Pt monolayer catalysts be done reproducibly?
- Agree with last year reviewer's comments that there should be faster tech transfer. This type of activity will establish what the real problems are with these materials. Until this is done these interesting results are laboratory curiosities.

- Plug Power interaction applauded.
- LANL role is critical to success of program.
- Addition of Battelle is noted.
- He has fully engaged appropriate "partners."
- More appropriate industry collaboration would be with companies that make catalysts, rather than fuel-cell OEMs like Plug Power and GM.
- Numerous papers and presentations have resulted from this project.
- Time to team with a quality electrocatalyst supplier and prepare sufficient quantities of several of these materials for extensive testing.
- Collaboration with LANL seems to be working very well. Industrial interactions on cathode catalysts have started. Plug has tested an anode catalyst.

Question 5: Approach to and relevance of proposed future research

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

- The small $330K budget should be increased to allow more fuel cell testing of the activity and stability of these promising electrode materials. If this is not possible, the 2006 effort on 4 candidates should be concentrated on the 2-3 most promising candidates with more fuel cell testing at companies like a Plug Power or a DuPont. 2006 work plan is not focused enough with the current budget and testing resources. Faster tech transfer is needed.
- Strongly endorse PI's suggestion of OCP and potential cycling tests.
- Suggest adding post-mortem diagnostics (ECSA, imaging, XRD, etc) to confirm that reported 21μV/hr decay rate with constant current is due to non-catalytic processes.
- PI did not indicate preparation scale, but suggest achieving 100 g level.
- Determine Pt content after test. Post-mortem analysis should be performed.
- Non-PM is most consistent with DOE MEA target.
- Plans are clear and make sense.
- Potential cycle testing is essential.
- Promising catalysts (those with best Pt utilization) need to be tested thoroughly.
- Should try to wean from dependence on large amounts of Pd sooner in timeline.
- Intention on voltage cycling testing of monolayer catalysts is just what is needed.
- Don't spend an excessive amount of time on PdCo unless % Pd can be driven very low.
- Some concerted effort on trying to scale up the UPD exchange monolayer synthesis process is needed.

Strengths and weaknesses

Strengths

- A number of interesting material options are being investigated - Monolayer Pt/Pd/C, Pt/Au/Ni and Pd_2Co- that minimize materials usage or are lower cost.
- Impressive results on all 4 candidate approaches.
- Achievement of 20x improvements in Pt specific activity in PtRe/Pd/C and in Pt/Au/Ni/C catalysts that has been validated by preliminary fuel cell test results at LANL is a solid achievement. Confirms that this is a promising approach.
- Preliminary fuel cell validation of promise of these materials.
- Strong theoretical underpinnings provide good basis for design of catalysts.
- LANL test relationship critical and an important partner.
- Solid team. Technically strong. Facilities/ resources rich.
- Good approach (e.g. not combinatorial, but driven by theoretical considerations) and good progress.
- Experimental plan has good theoretical base.
- Good insights on the origin of enhanced ORR activities. This can cure the fixed mindset that a bulk alloy phase, preferably with long-range order, is needed.

Weaknesses

- Good interactions established with LANL and Plug Power but would like to see fuel cell testing enhanced in the future. Are Pt monolayer materials survivable in a fuel cell environment? Can the OH-OH coverage in the Pt monolayer catalysts be done reproducibly?
- Although PI has achieved g/kW goal due to the ultra-low PM content, should also remember that there is a power goal as well, namely 0.8 V at 0.4 A/cm^2: encourage additional work to boost voltage at 0.4 A/cm^2.
- New and additional fuel cell testing. Not enough funding.
- Keep in mind that the relative prices for Pd and Pt change with relative demands. Always try to minimize total noble metal content, or at least the total platinum-group metal content.

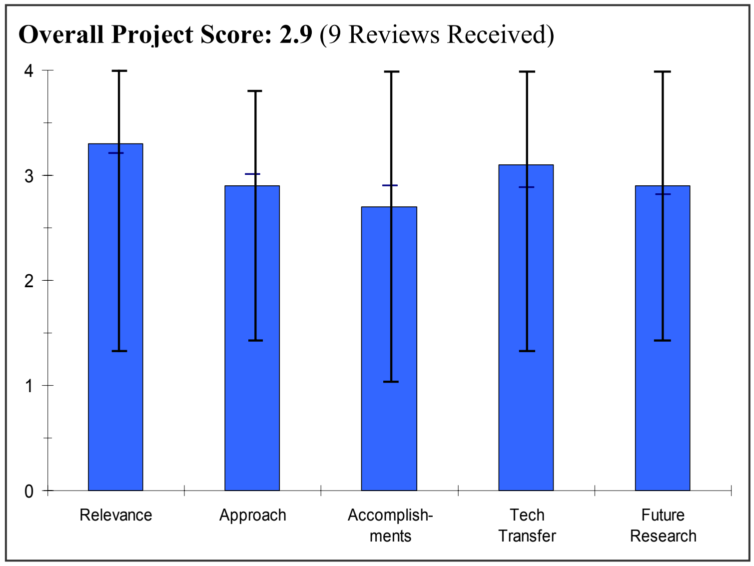
Specific recommendations and additions or deletions to the work scope

- The small $330K budget should be increased to allow more fuel cell testing of the activity and stability of these promising electrode materials. If this is not possible, the 2006 effort on 4 candidates should be concentrated on the 2-3 most promising candidates with more fuel cell testing at companies like a Plug Power or a DuPont. 2006 work plan is not focused enough with the current budget and testing resources. Faster tech transfer is needed.
- Although "catalyst manufacture" is not part of BNL mission, suggest scaling best catalyst to 100 grams to confirm PI's assertion that process is "straightforward slurry chemistry." (PI did not indicate current prep scale).
- With partner LANL, work on boosting cathode performance to DOE target at 0.8 V of 0.4 A/cm^2.
- Should continue to explore new catalysts. Expand Pd effort.
- Include repeated potential cycling tests as part of durability screening of promising candidates.
- These data are well enough along to begin a testing program documenting performance and durability. Such testing requires well-characterized catalyst preparation and careful single cell measurements.
- Cutting the platinum requirement by a factor of 4, as might be possible, is a very important task.
- Do small-scale development of the processes needed to make 50 cm^2 MEAs and, eventually, short-stack quantities of catalysts.

Project # FC-18: Development of a Low-Cost, Durable Membrane and MEA for Stationary and Mobile Fuel Cell Applications
Foure, Michel; Arkema Chemicals

<u>**Brief Summary of Project**</u>

The objective of this Arkema Chemicals project is to develop low-cost, high-durability membranes by optimizing chemistry and process, validating scale-up, developing membrane electrode assemblies (MEAs) based on these membranes, optimizing MEA for new membrane, validating MEA performance, and validating the MEA performance in single cells and in stacks.



<u>**Question 1: Relevance to overall DOE objectives**</u>

This project earned a score of **3.3** for its relevance to DOE objectives.

- Project addresses key objectives in the HFCIT R&D plan.
- This project addresses cost and durability, which present two of the most significant barriers identified by the DOE program.
- Operation at low relative humidity (25-50% RH) and elevated temperature is desirable.
- The relevance to the program is good and the focus is on achieving the program's performance goals both in cost and endurance.
- Initial choice of Kynar and polyelectrolyte-simplistic. Affordable, durable, high-performance membranes are key to achieving fuel cell cost and durability targets.
- Strong project team.
- Lower-cost, high durability membrane development is one of DOE's key objectives.
- Durability questions need to be addressed in parallel with development activities.

<u>**Question 2: Approach to performing the research and development**</u>

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

- Approach is reasonable given the fact that they are trying to understand the weakness (degradation) they found in their membrane product.
- Approach could lead to very inexpensive membranes.
- Lack of detail and data makes it difficult to assess whether the specifics will allow them to get there.
- Only durability target listed is 5,000 hours; also need to consider 40,000 hours for stationary applications.
- PVDF is partially fluorinated. Is it as stable as fully fluorinated polymers? (PTFE & PFSA polymers)
- The approach is well thought out but the presentation didn't give enough chemical structural details to be able to evaluate the actual strategies being used and the chemical viability of the approach.
- Approach has improved, with more emphasis this year on understanding degradation mechanisms to guide further development.
- Not clear that membrane is being tested under benign or extreme operating conditions. Key DOE targets require high temperature, low RH membranes.
- Approach is logical.
- Process scaled to pilot plant.
- No mention of potential membrane material or manufacturing costs or MEA costs.

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

This project was rated **2.7** based on accomplishments.

- Durability testing needs to include stress factors.
- Good idea to start- looks like it doesn't work due to polymer degradation. Presentation lacked information on plans to resolve fundamental polymer limitation.
- Membrane shows good initial performance.
- Have made progress in durability.
- No data was provided for low humidity conditions.
- No new endurance data.
- Significant improvement in life is needed; M31_2 reached End of Life (EOL, defined by DOE as 10% loss compared to Beginning of Life (BOL)) after 1,500 hours while operated in constant current mode; DOE goal is 40,000 hours and 2004 Status is 9000 hours (MY Plan pages 3-86 and 3-88, foot note c).
- Open Circuit Voltage (OCV) on H_2/O_2 is low (0.85-0.9V); could be increased by up to 0.1.
- Significant progress has been made in identifying the mechanism for performance degradation.
- Good progress in learning
- Good postmortem failure analysis on finalized samples.
- Clearly need to extend polymer durability, but team seems to have a good plan to address.
- Good quality data generated.
- Project scheduled to conclude in two years and appears on track.

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

This project was rated **3.1** for technology transfer and collaboration.

- Not really working with major players who would implement technology if successful.
- Collaboration with Oak Ridge should allow for increased understanding of degradation mechanism.
- ORNL is valuable addition to the team.
- No results presented from Georgia Tech work.
- Georgia Tech capabilities appear not to be fully utilized (water sorption measurement, mechanical properties, and spectroscopy?
- There are good coordination efforts with team members that are beneficial to the progress of the effort.
- Development not ready for tech transfer.
- Partners are helping identify/quantify degradation mechanisms to the overall benefit of the project.
- Makes good use of collaboration opportunities.
- Partners are full participants in the work.
- Good coordination and assignments of tasks.
- Dissemination of non-proprietary results through additional publications recommended.

Question 5: Approach to and relevance of proposed future research

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

- Proposed approach will help understand degradation.
- Details on the chemistry of sulfur release and plans for ameliorating it are vague.
- Should specifically address milestone 51 from MY RD&D Plan (2000 hour test with advanced membrane).
- The future work plan is well organized and thoughtful. The focus is appropriate.
- Future work plan is improved.
- Should evaluate whether project funds are best spent on process scale-up, or whether further material improvement is needed.
- Should consider higher voltage (OCV) testing to find problems caused by chemical attack.
- Reasonable focused plan.
- High temperature operation is not the primary focus of research; however, it should be looked at, briefly.

Strengths and weaknesses

Strengths
- Polymer chemistry, reasonable beginning-of-life performance.
- Good concept and good initial performance.
- Collaborations in place should allow for further advancement.
- Interesting approach to novel low cost membrane.
- Analysis of membrane degradation mechanisms.
- Lack of fluoride generation, lower gas permeability and promise of low hydroperoxyl radical formation.
- Demonstrated learning.
- Large number of ex-situ tests used to characterize membranes.
- Overall well thought-out and managed project.
- Innovative approach.
- Strong team.

Weaknesses
- Lack of collaboration with major players (Ballard, Gore, 3M, Dupont) who would implement this technology.
- Have not provided enough data to show that they can decrease sulfur losses to acceptable levels.
- Data showing increased conductivity at low RH were not provided- the ability to operate at low RH is a key goal for DOE.
- Progress appears to be slow.
- Only new cell data reported concerns membrane conditioning procedure.
- Reason for testing under pressurized conditions not apparent; recommend testing at atmospheric pressure.
- No data presented for conductivity or cell performance at low hydration conditions.
- No mechanical properties reported.
- No clear advantage over Nafion in terms of performance, durability or cost shown to date.
- No discussion of membrane morphology- is it microporous or is it a dense film? PVDF is widely used in the membrane industry.
- It is possible that testing is not being done under the extreme kinds of conditions (cycling, low RH, high T, degradation at OCV). The test conditions were not made clear in the presentation.
- Project would benefit from more external dissemination of non-proprietary results (publications).
- Membrane material and manufacturing costs not discussed or compared.
- Resulting stack material and manufacturing costs not discussed or compared.

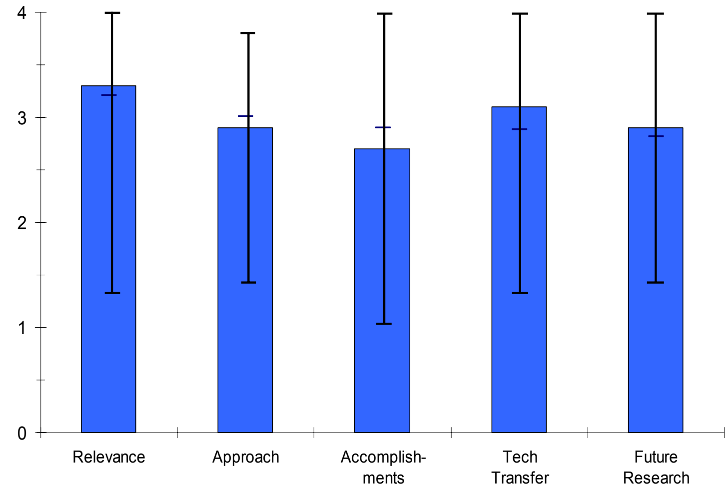
Specific recommendations and additions or deletions to the work scope

- Should utilize a durability protocol including cycling, especially when a more durable membrane is obtained.
- Test under more realistic conditions: use Hydrogen/Air (instead of Hydrogen/Oxygen) and low relative humidity (25-50%).
- Test mechanical properties of hydrated membrane (in addition to dry).
- Fluoride ion release into water effluents should also be given as fraction of total F in MEA.
- Measure and report fuel cell high frequency resistance.
- Permeability data did not include specifics on membrane area and pressure difference; hydrogen electrochemical permeability should also be given in mA/cm^2.
- List complete testing conditions for all data reported (including gas utilization, electrode loading and cell area).
- Please list units for all numerical values (including effluent concentrations.
- Has this team considered trying to crosslink the sulfonated polyelectrolyte with multivalent metals -post membrane formation. If the polyelectrolyte is acrylic based, i.e. AMPS, then suitable post formation cross linking based inclusion of other reactive acrylic monomers are indicated.
- Should consider postponing process scale-up.
- Continue with membrane durability studies.
- May want to consider accelerated membrane testing.

Project # FC-19: Development of High-Performance, Low-Pt Cathodes Containing New Catalysts and Layer Structures
Atanassova, Paolina; Cabot Superior MicroPowders

Brief Summary of Project

Overall Project Score: 2.9 (8 Reviews Received)



This is a four year project led by Cabot Corporation to develop and apply combinatorial powder synthesis platform based on spray pyrolysis for discovery of high performance low-Pt cathode electrocatalysts for PEM automotive fuel cells. This project will use the platform for electrocatalyst composition discovery and microstructure optimization under conditions that can be scaled for commercial powder production, and will deliver high-performance cathode electrocatalysts and membrane electrode assemblies (MEAs) with lower Pt content to meet the DOE target of 0.6 gPt/kW in 2005. Specific objectives include completing the development of rapid testing equipment – Dupont Fuel Cells; starting high throughput synthesis of ternary alloy compositions in a discovery mode; further optimizing MEA electrode structure; testing long-term stability of new electrocatalysts; and delivering electrocatalysts and test MEAs to stack manufacturers.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Identification of low pt catalyst is key to low cost fuel cell goal.
- Well focused and relevant project.
- Further reductions in noble metals required are necessary to meet automotive cost targets and could be beneficial for other applications with less aggressive cost targets.
- Combinatorial approach might yield new information that would allow lower Pt loading.
- Important problem.
- Cost appears to be emphasized in turn minimizing other key factors like performance and durability

Question 2: Approach to performing the research and development

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

- Searching some ternary catalysts may be a good approach towards finding the active ORR catalysts.
- It will be more difficult to optimize for durability, than identify the best catalysts out of all possibilities. Care should be taken not to reject durable candidates in the initial screen for better activity.
- Good approach in that it is a unique process/fabrication method that provides an alternative to the standard methods for making dispersed catalysts. Combinatorial capability makes a lot of sense.
- Focusing on ternaries is also a good focus point given the known literature results.
- It is not clear if the carbon support technology is also being screened, or if their approach is limited to coating on the gas diffusion layers.
- Rapid screening is a good experimental approach. Needs theoretical approach also to identity high performance catalyst.
- It is not clear whether theoretical considerations have any bearing on deciding what materials are screened. Despite speed of screening, some theory could improve efficiency of this program.

- To date fuel-cell testing appears to have been limited to steady-state conditions. Decay rates are high for steady-state conditions. Do these numbers reflect recoverable decay?
- Must include repeated potential cycling testing to assess durability.
- Interesting combinatorial-high thru-put screening approach based on carbon supported catalyst produced by spray pyrolysis. PtM1M2 library covers many systems already studied but also covers new regions.
- Is there a chance that this approach might only lead to conventional catalysts prepared by an alternative method?
- What advantages will be derived from this alternative approach.

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

This project was rated **2.9** based on accomplishments.

- A very good improvement in activity was accomplished with the ternary catalysts. Their long-term stability has to be carefully evaluated. The origin and duration of the effect of leaching should be established.
- Excellent progress in ternary selection and demonstration of performance in MEA. Good advances in pretreatment and characterization.
- Several hundred catalyst types being screened in the past year is good progress. Surprising there is not more variation in activities with composition of the multiple libraries. Two fold increases in activity for alloys is consistent with that seen with alloy made by conventional methods.
- Progress on improving performance has been steady since start of the contract, validating the approach. Performance, catalyst utilization and durability still need improvement.
- Given that over 500 samples have been made and screened the quantity of work is stupendous. Good progress.
- Must show activity normalized per mass of total noble metal, not just Pt mass. This is essential since the identity of the alloying elements is not disclosed (i.e., no way to assess ultimate material costs of compounds).
- Need to differentiate between non-recoverable and recoverable decay in "long-term" testing. Recoverable decay can be recovered by simply reducing the cathode to the anode potential for a brief time.
- Claims to have found new compositions of PtM1M2 ternary catalyst, but activity lies in band of Pt3Co now reported by several labs. Not clear there is really anything new.
- Solid progress toward stated goals but not much toward real DOE targets.

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

This project was rated **3.1** for technology transfer and collaboration.

- An effort to enhance collaboration has been made.
- Need to build relation with Dupont to understand all durability issues especially electrode/membrane interactions.
- Close working relationship with a primary membrane supplier.
- The interaction with Dupont is good and obviously beneficial.
- Although presenter said she explained it, I did not understand the role of Dupont.
- Who did the high thru-put screening?
- Substantial work on vertically integrating this effort.

Question 5: Approach to and relevance of proposed future research

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

- The tests under potential cycling conditions should be started soon.
- Need to build a battery of durability tests including H_2O_2, F^- to study the effect of the catalyst on membrane and overall catalyst life time.
- Opportunities to alter or modify the carbon support materials in the CSMP process should be a part of any catalyst durability studies. Much is already known about the carbon's limitation against oxidation at high temperature and high voltages. Data is also appearing now on the limitations of fine dispersed catalysts under

cycling conditions. Their future work indicates that they will continue to screen and characterize the catalysts, but it was not clear how their approach will minimize or improve on any of these fundamental limitations.
- Good plan for the future.
- Unclear what the compounds are or if they are cost-effective, difficult to judge future plans here.
- Cycle testing is essential.

Strengths and weaknesses

Strengths
- Production capabilities.
- Expertise in powder synthesis and characterization collaboration with fuel cell membrane supplies.
- Significant capabilities and expertise in catalyst fabrication. Close working relationship and alignment with a primary membrane supplier. Novel catalyst synthesis/deposition method.
- The ability to scale up is a strong point. The existing partnerships are fruitful.
- High thru-put synthesis of catalysts in the same form as they would be used in cells.
- Excellent technology.
- Significant effort to involve MEA makers.

Weaknesses
- Closer collaboration with GM or other fuel cell developer needed to understand durability issues.
- Not clear, but if the CSMP approach is limited to catalyst deposition on the GDL to form GDE's rather than coating on the membranes to form CCM's, then they may be fundamentally limited in reaching higher levels of catalyst utilization.
- The Edisionian approach can be both a strength and a weakness depending on what input is being used to guide the "parameter space" being screened. I would hope that the PI is keeping a close eye on the other catalytic research results.
- Approach (e.g. combinatorial) does not appear to be influenced by theoretical considerations. Progress not clear since total noble metal data are not provided. No way of knowing if progress against cost has been made.
- It doesn't appear that they have found anything new with the library used.

Specific recommendations and additions or deletions to the work scope

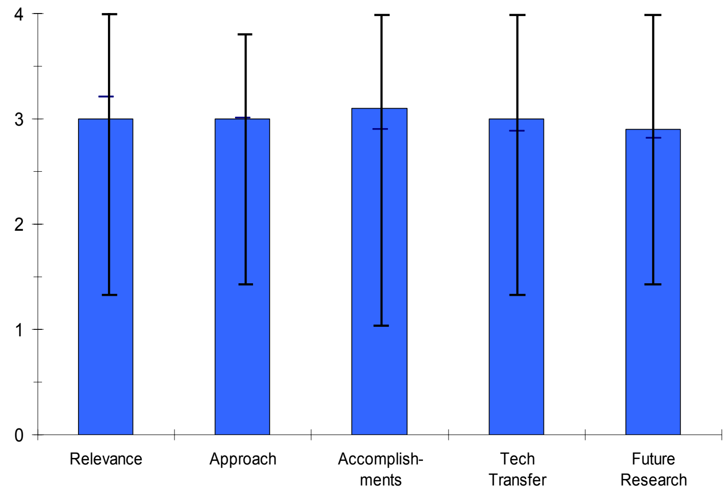
- Include repeated potential cycling tests as part of durability screening of promising candidates.
- Improvements in mass-transport regime may be due to the removal of trace foreign cations that reduce conductivity of the ionomer in the catalyst layer. Reduced ionic transport in the catalyst layer will result in similar results, although it can be diagnosed by the dependence of performance on oxygen partial pressure (e.g., see: M. Perry, J. Newman and E. Cairns, Journal of the Electrochemical Society, Vol. 145, No. 1, pp. 5-15, 1998).
- Why not repeat library with a different support, i.e. an electronically conducting oxide.
- This approach is an elegant one for synthesizing/preparing catalysts but they need an infusion of ideas to provide something worth making. Perhaps the investigators can team up with some other partners who are making interesting materials. They should also think through what particular advantages accrue to their approach in the context of improving the cathode.

Project # FC-20: Platinum Recycling Technology Development
Grot, Stephen; Ion Power, Inc.

Brief Summary of Project

This Ion Power, Inc. project will assist the DOE in demonstrating a cost effective and environmentally friendly recovery and reuse technology for Platinum Group Metal containing materials used in fuel cell systems. The initial objectives include development of lab scale processes for solubilizing catalyst coated membranes, development of a lab scale processes for catalyst and ionomer materials, development of test methods to determine vitality of the recovered materials, and partnering with the key stakeholders in this technology area.

Overall Project Score: 3.0 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This is good and necessary work that needs to be done to answer questions that will ultimately be. However, it is not so critical an aspect of cost reduction as some other things and that is the only reason for the 2.
- Development of a cost model is also necessary, because cost should be a primary driver of this project to meet DOE Target.
- Platinum Group Metals are a major cost factor in PEMFC; recycling of PGM is needed for a sustainable PEMFC industry.
- Directly addresses HFCIT Multi-Year RD&D plan milestone 55.
- Viability of high efficiency recovery of platinum has been demonstrated convincingly.
- Recycling and lifetime are important to meet fuel cell targets.

Question 2: Approach to performing the research and development

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

- Size of autoclave is good – large enough to demonstrate practicality. It is a good alternative to burning.
- Questionable whether used PFSA will ever be useful for direct recycling as PEM for fuel cells, but important to verify one way or the other. It may not be performance only but water management behavior and durability of recycled PEMS as well that will need to be evaluated before a decision can be made whether it is viable to reuse the material for a fuel cell membrane. More basic properties should be measured.
- Sound approach to ionomer recycling.
- Not clear if suggestion is to reuse carbon supported catalysts, or just recover the PGM; if it is the former, much more in-depth analysis than reported catalyst vitality test is needed.
- Autoclave reactor has provided an HF free alternative to classical combustion i.e. environmentally friendly objective has been achieved.
- Keep the focus on catalyst recovery. Do not waste time on membrane recovery.
- Need to be aware of the energy cost of recycling.
- Good idea to recycle both catalyst and membrane.
- Autoclaving will solubilize Nafion-like materials.
- Catalysts may be useful exactly as recovered, just like the polymer is useful. (Of course the supported materials may need some cleanup steps.)

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

This project was rated **3.1** based on accomplishments.

- Needs to investigate chemical structure change in recycled ionomer and durability of recycled membrane in MEA. Good demonstration of the performance of a re-manufactured fuel cell membrane.
- Beginning of Life (BOL) performance of MEA with re-manufactured membrane is close to that of MEA with new membrane; degradation rates of re-manufactured and new MEAs should be compared.
- Good recovery rate of starting materials.
- MEA recycling cost model should be refined.
- Platinum recovery excellent, but recovery and re-use of membrane material are only of marginal value.
- A good start to initially defining an approach to fuel cell recycling.
- Good progress. Need to keep in mind external factors that would affect the overall efficiency of the recycling process.
- Reuse of Nafion is an interesting result.

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

This project was rated **3.0** for technology transfer and collaboration.

- Good list of collaborators and mix of large and small companies, industry and academia.
- Need to include manufacturing in cost (financial)/recycle model.
- Added a number of good partners for obtaining End-of-Life (EOL) MEAs.
- Partnering with catalyst manufacturer would strengthen EOL catalyst characterization and re-manufacturing/recycling.
- Good broad-based partnering.
- Need to establish collaboration with stack manufacturers/suppliers. The wider the experiences the better you will become at addressing recycling.
- Right organizations are on board.

Question 5: Approach to and relevance of proposed future research

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

- The economic analysis will be important. But rather than strive to show a stack running on re-manufactured MEA's using recast membranes, (i.e. functional tests) it would make more sense to characterize the fundamental properties of the membranes to see how those have changed – e.g. EW, water uptake versus water activity, conductivity versus lambda (water molecules per sulfonate group), mechanical properties like puncture strength, durability in Fenton's reagent, ICP analysis of impurities, etc.
- Not clear if re-manufactured MEAs will contain re-manufactured membrane with new catalyst (as in this presentation), or recycled catalyst will be used.
- Focus on concluding/ optimizing the platinum metal recovery process and determining process economics.
- What is the value of conducting a remanufactured MEA? Especially when all MEAs are different in shapes, thicknesses, size, etc?

Strengths and weaknesses

Strengths
- Also recycles ionomer, which has the potential to lower MEA cost.
- Good membrane recycling process.
- Primary objective of high content platinum recovery has been achieved using environmentally friendly process
- Demonstrated feasibility of recycling of both membrane and electrode.

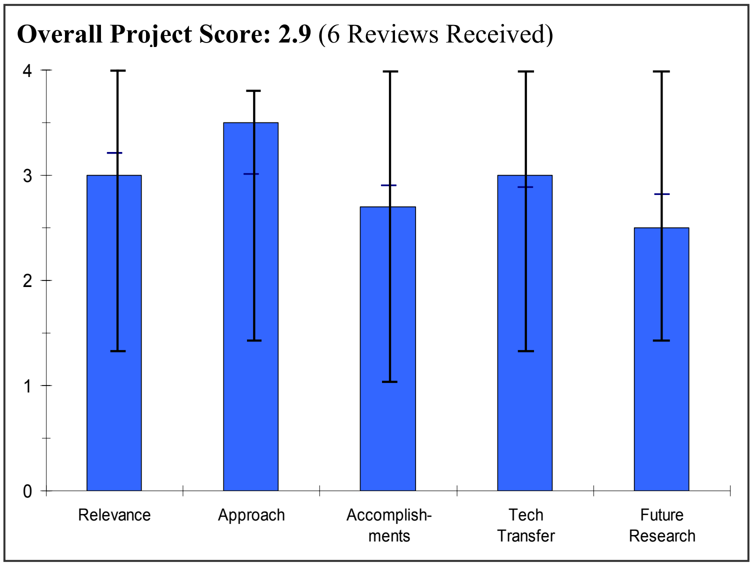
- Data is good enough to provide input information about end-of-life value and then to include those numbers in cost analyses. Most likely most of the precious metal cost can be recovered, and that could impact FCV costs. (This must be similar to nickel metal hydride batteries, again a system where at end-of-life the metals, which contribute to cost, are recycled with good return of value).

Weaknesses
- Emissions from catalyst combustion not addressed in presentation.
- Purity of Pt recovered after combustion is not known.
- Pursuit of re-usable membrane polymer is not justified. No fuel cell manufacturer would give a warranty on a fuel cell system that used reclaimed membranes.
- Recycle targets may change in future as technology develops.
- Need to establish collaboration with all MEAs supplier/manufacturers.
- Need to establish collaboration with all stack supplier/manufacturers to understand disassembly techniques. It may affect recycling.
- Need to consider some concepts for other membrane materials, in additional to Nafion. Recycling costs might impact future membrane selection.

Specific recommendations and additions or deletions to the work scope

- Suggest endurance testing MEAs with cast membranes made from virgin Nafion and re-manufactured Nafion under the same conditions to determine whether EOL performance is similarly close as at BOL
- Suggest addressing recovery of Pt alloy catalysts, such as PtRu/C.
- Suggest elemental analysis of virgin and re-manufactured Nafion to determine effectiveness of filtration in removing contaminants.
- Suggest elemental analysis of recovered PGM and estimation of cost to purity to catalyst manufacturer specifications.
- Indicate net cost savings i.e. PEM value minus recycling process costs. Proposed ageing and rebuilding of 5kw size fuel cell stack is of limited value.
- Some interaction with chemical processing and polymer producer’s actual processes should be valuable here.
- Need to establish collaboration with all MEAs supplier/manufacturers.
- Need to establish collaboration with all stack supplier/manufacturers to understand disassembly techniques. It may affect recycling.
- Need to reconsider the value of conducting a remanufacture MEA? Especially when all MEAs are different in shapes, thicknesses, size, etc.
- Might consider techniques to “clean up”, “restore”, etc. a PEM fuel cell without tearing up the device and regain original performance. This could involve removing impurities from the MEA, like, for example, chloride ion. May want to evaluate rebuilding compared with recycling

Project # FC-21: Platinum Group Metal Recycling Technology Development
Shore, Larry; Engelhard Corporation



Brief Summary of Project

This Engelhard Corp. project will examine methods to recycle all precious metal containing catalysts in a fuel cell "system." A primary objective is to develop a commercially-acceptable, environmentally friendly process for recovering and recycling Platinum (Pt) and Ruthenium (Ru) from membrane electrode assemblies (MEAs) by developing a process that does not emit pollutants (especially HF) and evaluating Ru recovery from MEAs. A process for precious metal (PM) recovery from metal monoliths, which are used in stationary reformers and potentially in hydrogen production, will be evaluated.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Very good and necessary work. However, it received a 2 rating because it only addresses one cost reduction barrier.
- Extremely good progress made to the realization of objectives.
- High platinum recoveries will be required to launch the fuel cell industry on a large scale.

Question 2: Approach to performing the research and development

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

- Could the recovered ionomer be reinserted at some point in the membrane synthesis steps? Approach seems limited to extraction of Pt/C from catalyst coated membranes. What about catalyst coated on the gas diffusion layer (GDL)?
- Good assessment that in reality the wide variation of MEA components and constructions may make any one approach limiting. Sole focus on recovering Pt for refining and ionomer recovery for some type of recycling makes sense.
- Good use of partners to address different approaches.
- Comparative evaluation of the three alternative processes very valuable and informative.
- Keep the focus on catalyst recovery. Do not waste time on membrane recovery.
- Keep the awareness of the negative side effects of recycling.
- Need to be aware of the energy cost of recycling if using microwaves.
- Multiple approaches to removing the platinum from the membrane is a good strategy.

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

This project was rated **2.7** based on accomplishments.

- Good progress in that definite conclusions have been reached on relative capabilities of several unique approaches for catalyst recovery as apposed to a pyrometallurgical approach.

- Only completion of work on delamination of electrode materials from PEM in past year seems to be a low rate of progress.
- Why is it necessary to wait to get an aged lot of MEA's from Greenlight – aren't these available already?
- The program should move along faster. Downselection of best approach and focusing of resources on one process looks like best approach.
- High pt energy (>98%) very effective demonstration.
- Good progress. Need to keep in mind external factors that would affect the overall efficiency of the recycling process.
- The key element in the project is the elimination of HF emissions, but there was no discussion about how this is being accomplished.
- Good analyses of exhaust gas identified other items of concern in the exhaust gas, but again, not clear how these will be contained.

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

This project was rated **3.0** for technology transfer and collaboration.

- Should consider MEA's in which the catalyst is coated on the GDL also before bonding to the membrane, and work with a supplier for these.
- Good group of partners.
- A broader based industry partnering would be beneficial.
- Need to establish collaboration with all MEA manufacturers. The wider the experiences the better you will become at addressing recycling.
- Could establish greater range of companies to supply materials for reclamation to determine applicability across wide range of makers.

Question 5: Approach to and relevance of proposed future research

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

- Consider working with a membrane supplier to evaluate if the ionomer recovered can be reused for making MEA's, even if it is a mix of different starting monomers.
- Also, an economic analysis of the various processes was not explicitly stated to be an objective of the project but should be seriously considered for one or more of the methods under test.
- Select preferred process for pt recovery, optimizing it and then establish process economics.
- Need to conduct recycling efforts on major MEAs from Ballard, UTC, etc.
- Not clear what the approach is going to be to eliminate HF emissions.

Strengths and weaknesses

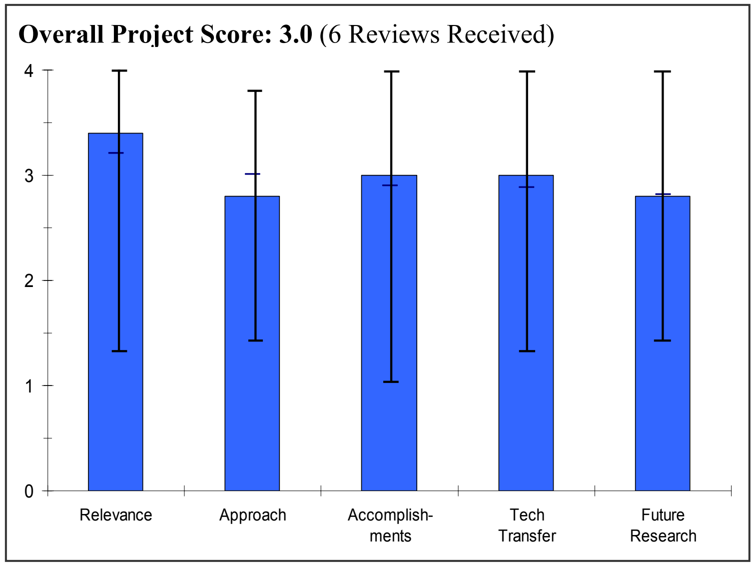
Strengths
- Key expertise to understand the viability of the PGM recovery.
- Strong partners and well laid out approach.
- Development of emission free pt recovery process is within sight.
- Good progress and is aware of the potential negative side effects.

Weaknesses
- Rate of progress appears to be slow, need to accelerate efforts.
- Recycle targets may change in future as technology develops.
- Need to establish collaboration with MEAs supplier/manufacturers.
- Need to establish collaboration with stack supplier/manufacturers to understand disassembly techniques. It may affect recycling.

Specific recommendations and additions or deletions to the work scope

- Accelerate timeline to drive selection of best approach and focusing of resources.
- Back-up option of pyrometallurgical approach should not be dismissed because of loss of polymer, which is only of marginal value.
- If not yet done, a general economic study of solvent vs. microwave v. combustion would be informative.
- Need to establish collaboration with MEAs supplier/manufacturers.
- Need to establish collaboration with stack supplier/manufacturers to understand disassembly techniques. It may affect recycling.

Project # FC-22: Scale-Up of Carbon/Carbon Bipolar Plates
Haack, David; Porvair Fuel Cell Technology Corporation



Brief Summary of Project

Porvair Fuel Cell Technology intends to develop material and manufacturing methods leading to a low-cost carbon/carbon bipolar plate. Objectives are to evaluate and demonstrate performance within a fuel cell stack; evaluate potential cost of manufacture; develop low volume production capabilities; develop incremental, near term cost reduction technologies; manufacture 10 kW fuel cell sealed plate demonstration stack; develop and implement a comprehensive quality assurance plan; and develop a comprehensive cost model for high volume production.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Bipolar plate cost and performance are important drivers for PEM fuel cell commercialization.
- Project addresses stack performance and cost goals.
- An important component of fuel cell systems with PI demonstrating knowledge of cost, weight, conductivity, and mechanical performance demands.
- Low cost bipolar plates are an industry priority but presenter did not articulate the connection of work presented to objectives. Project may be relevant but speaker was not convincing.
- Affordable, design-appropriate bipolar plates are key to achieving fuel cell cost and durability targets.

Question 2: Approach to performing the research and development

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

- Porvair has scaled up bipolar plate manufacturing and is now attempting net-shape molding.
- PI aimed at development of a low cost net shape molding process that is amenable to high volume manufacturing. Validated with a robust 10kW sealed plate stack demonstration. Cost- outs of process to high volume manufacturing to be done based on learning in 10kW demo.
- No discussion of plate features or manufacturing methods.
- The "reference" plates were not described.
- Production is happening, but what is involved?
- There was no elaboration on the production cost model, why should anyone believe the cost projections?
- The approach was simply not clear.
- Approach seems unlikely to meet cost targets (bonding two monopolar plates to get bipolar plate, minimum of two pyrolysis steps, remaining questions of permeability and dimensional control).

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

This project was rated **3.0** based on accomplishments.

- Porvair has met DOE's technical targets. Cost targets are more difficult but Porvair has estimated that they can be met at sufficient volumes.
- Difficult to access accomplishments based on data presented. Results presented show cumulative cost without breakdown by material and process.
- Impressive demo of 20 cell stack with performance equal or better to machined graphite. Good VI results and stability. Cost analysis indicates that low cost, high volume.
- Manufacturing is possible with sensitivity factors identified.
- No discussion of design and mfg process options or different scenarios ("investments" used loosely).
- Only one cost curve was presented, and without any substantive justification.
- Polarization curves were treated casually, and were assumed fixed- isn't technical progress envisioned?
- What about curves for comparison?
- No description of specific manufacturing outlays or investments required to build capacity.
- Though progress has been made since project inception, progress this year seems minimal.

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

This project was rated **3.0** for technology transfer and collaboration.

- In addition to UTC Fuel Cells, Porvair is collaborating with developers of both stationary and automotive fuel cell stacks.
- Collaborations not discussed in this presentation. Porvair products being evaluated by stack developers.
- Work done with collaboration of a major PEMFC manufacturer, UTC, for testing validation.
- UTC is a strong partner, but this project necessitates collaborating with ALL plate users/developers.
- Reliance on single partner will bias the project.
- No commentary on how bipolar plate geometry and materials might change, project should include consultation of relevant experts.
- Collaboration with UTC good, but UTC design requirements differ from most of emerging industry.
- No other partnering attempts apparent.

Question 5: Approach to and relevance of proposed future research

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

- Description of future work was vague but followed logically.
- Project complete and Porvair efforts will be guided by developer feedback and needs.
- DOE project concluded in April 2005. With robust demonstration of technology, project is moving forward at Porvair for further scale-up. Initiated product qualification with a number of fuel cell customers. All of this is a positive indicator of project success.
- Plans for the future were not clearly explained.
- The model must incorporate much wider input, explore scenarios, and be articulated clearly to be credible.
- Project is complete.

Strengths and weaknesses

Strengths
- Success of this technology will be demonstrated by its adoption by developers.
- Impressive demo of 20 cell stack with performance equal or better to machined graphite. Good VI results and stability. Cost analysis indicates that low cost, high volume manufacturing is possible with sensitivity factors identified.
- Very promising for $4/kW.

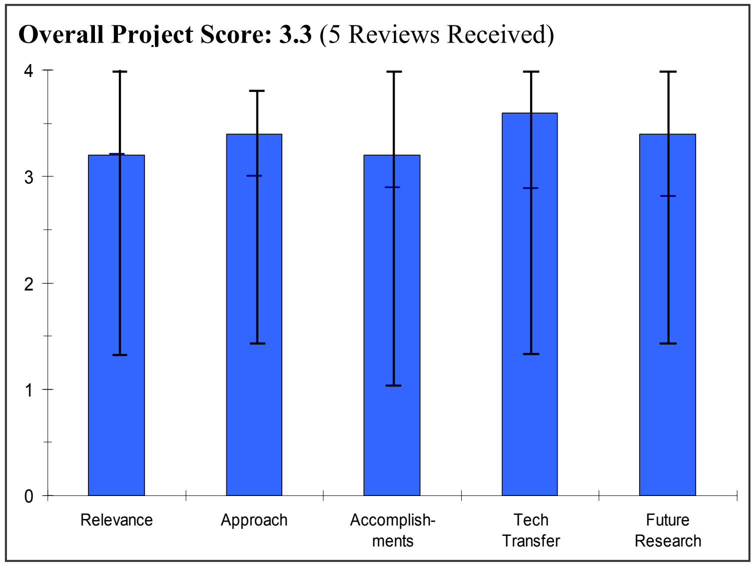
- Making predictions of future costs is important (but it MUST be credible, and the assumptions clear).
- Project Relevance to the Hydrogen Program. Project Accomplishments.

Weaknesses
- Project directed toward commercialization of this technology. Limited public knowledge developed in this project.
- Lack of explanation of project details and ambition.

Specific recommendations and additions or deletions to the work scope

- Cost model should be a publicly available deliverable.
- Time to let the market decide if this technology is desired.

Project # FC-23: Cost-Effective Surface Modification for Metallic Bipolar Plates
Brady, Mike presented by More, Karren; Oak Ridge National Laboratory



Brief Summary of Project

Oak Ridge National Laboratory (ORNL) and National Renewable Energy Laboratory (NREL) are developing a surface treatment to protect metallic bipolar plates by thermal (gas) nitridation of Cr-containing alloys to form a pin-hole free Cr-nitride surface. Results from a post-fuel-cell-test assessment of model nitrided Ni-50Cr test plates (long term, cyclic conditions) are discussed. Work to form protective nitrides on cheaper alloys, such as commercial Ni-Cr base alloys and Fe-Cr based stainless steels, will also be pursued and the amenability of this approach to stamped foil will be assessed.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Low-cost bipolar plates with long-term durability are essential to PEMFC technology.
- Achieving cost effective, durable BSPs is important.
- Many organizations are working on metallic stack technology.
- The need for metal coatings and treatments for metallic fuel cells has not been confirmed. Nuvera does not encounter lifetime issues associated with metal through 5000 hr.
- Project addresses a key component for reaching the DOE objectives for fuel cells.
- Very focused project with good integration with program goals.

Question 2: Approach to performing the research and development

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

- The approach is systematic and logical.
- Infiltration approach to growing Cr-nitride overcoats is a good one and has been used in other applications such as making SiC protective overcoats on graphite susceptors for epitaxial silicon semiconductor applications.
- Testing of anode plates at GM and FuelCell Energy as well as corrosion studies provides robust evaluation.
- Nitriding process was not adequately described, in terms of mass, energy, and time (and cost) requirements.
- Not clear what the steps of process improvement are, e.g. warping challenge claimed to be overcome, but no description of how.
- The project is sharply focused on addressing cost and performance issues associated with bipolar plates.
- The approach is methodical and well-reasoned.

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

This project was rated **3.2** based on accomplishments.

- Steady progress has been made although the pace of the work seems to be fairly slow.
- Gets good marks. Proof of concept established that nitrided Ni - 30Cr alloys with high quality corrosion resistance can be made and stamped (GenCell collaboration). Learning applied to more cost-effective SS alloys with pinhole-free nitrided layers made. Results are encouraging but corrosion resistance needs further improvement. Project responsive to reviewer comments.
- Cost effectiveness of nitriding process, and roadmap for achieving it, were not clearly explained.
- Connection of corrosion curves and associated test conditions to actual stacks requires more diligence.
- Showing good durability results on FCs containing treated materials is good, but group must ALSO show the reference case without treatment, otherwise meaningless.
- Why do alloying elements help? This was not explained.
- Have made significant progress and demonstrated corrosion resistance of Nitride film in Fe-Cr system.
- Have shown resistivity meeting DOE goal for 2010 of 0.01 ohm-cm^2.
- The productivity and accomplishments are good, especially considering the low funding level.

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

This project was rated **3.6** for technology transfer and collaboration.

- Excellent collaborations and plan to transfer the technology to industry when the project is complete. If the project meets its objective transfer should not be difficult.
- Good interactions established with major fuel cell companies -GM, FC Energy, MTI Micro for evaluation of coated BSPs with results available now not just being talked about. POC stamping done with a company GenCell that does this for a business.
- Good, but encourage more proactivity in seeking to work with other metal stack developers, e.g. Honda, Nuvera, etc.
- Collaborations in place with University, other National Labs, automakers, and fuel-cell manufacturers.
- Good interactions and collaborations.

Question 5: Approach to and relevance of proposed future research

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

- Plans to complete the project are straightforward provided a commercial alloy meeting the nitriding requirements can be identified.
- Very promising based on results so far. Focus on Fe-Cr alloys is good with major issues being addressed. Need to also address long term interactions of nitrided BSPs with MEA.
- Process development roadmap needed.
- Well thought out and methodical.

Strengths and weaknesses

Strengths
- Builds on strong ORNL materials experience and capabilities.
- First results are encouraging.
- Evidence of ability to perform nitriding and produce sample.
- Understanding of characterization methods.
- Collaborations in place for fuel cell testing and for tech-transfer to occur to customers.
- The viability of the approach is good and should be easily scaled up.

Weaknesses

- The requirement for "minor alloy additions" was not discussed in any detail -- the cost and processing impact should be quantified.
- Corrosion improvements on Fe-Cr not yet demonstrated but to be focus of 2006 work.
- Lack of process details and cost model.
- No results confirming the need for coatings/treatments (corrosion curves do not always reflect fuel cell operation reality).
- I would like to see some alternate thinking used and tried. What about taking a stainless steel stamped part and electrodepositing Cr followed by nitriding?

Specific recommendations and additions or deletions to the work scope

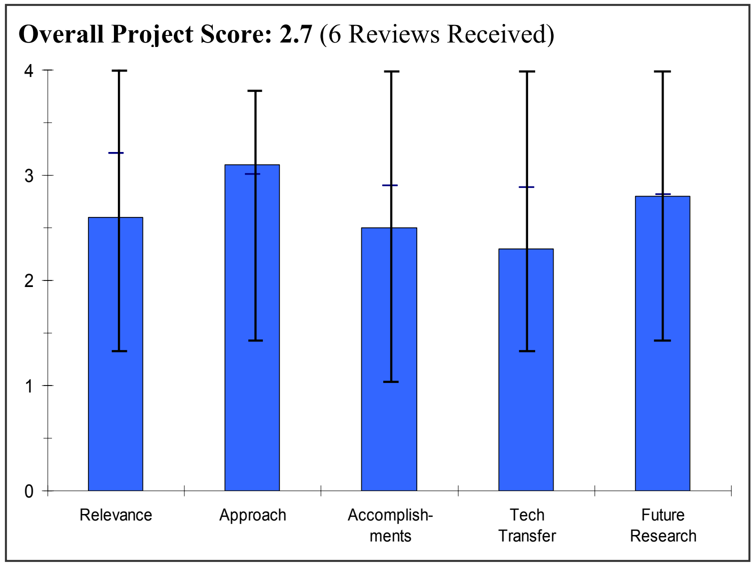
- Accelerate schedule and complete the project no later than the end of FY06.
- Continue the good work.
- Focus on cost-effective Fe -Cr alloys next year.

Project # FC-24: Water Gas Shift Catalysis

Krause, Theodore; Argonne National Laboratory

Brief Summary of Project

This Argonne National Laboratory (ANL) project will develop water-gas shift (WGS) catalysts which, when compared to Cu-Zn and Fe-Cr WGS catalysts, will be more active (higher turnover rates); less prone to deactivation due to temperature excursions; more structurally stable (able to withstand frequent cycles of vaporizing and condensing water); and more resistant to sulfur poisoning. This project is intended to improve the understanding of reaction mechanisms, catalyst deactivation, and sulfur poisoning, as well as define operating parameters (e.g., steam: carbon ratios, temperature, gas hourly space velocities, catalyst geometry) to optimize catalyst performance and lifetime.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- The re-directed focus from on-board to off-board fuel processing limits the benefits.
- Can support off-board hydrogen production if superior catalysts to those commercially available are developed.
- Refocusing the project appears to be moving this in the right direction.
- Water gas shift catalysts are most demanding catalysts in reforming process. Current generation of commercial WGS catalysts is significant technical barrier to low cost reforming of hydrocarbons.
- Improved and cost effective WGS catalysts are important to a number of hydrogen production and fuel processing systems for PEM fuel cell systems.
- Engineering solutions exist for using commercially available catalysts for stationary systems, so the project will not have wide-scale applicability without a major breakthrough.

Question 2: Approach to performing the research and development

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

- The objectives were clearly defined.
- Good they're moving away from precious metals and beginning to look at durability.
- Good use of advanced analytical techniques to identify catalytic mechanisms.
- Good to see more focus on non-precious metal catalysts.
- Appreciated seeing comparison of activity to Fe-Cr catalysts under same conditions.
- An impressive array of characterization and modeling work is being performed to gain a fundamental understanding of WGS catalysis in the PGM/ceria and base metal systems.
- Catalyst durability is being tested routinely.
- Ruthenium and rhenium do not provide a path to low cost WGS catalysts.
- If the target application is for LPG, then sulfur is a key issue that needs to be addressed. There is no reason to expect that the base metal catalyst formulations being pursued will be even moderately robust against sulfur impurities.
- The issue of pyrophoricity is not being addressed.

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

This project was rated **2.5** based on accomplishments.

- A good number of catalyst systems have been tested.
- Needs more concrete definition of objectives: performance & cost targets.
- Not clear if performance is better than commercial Cu-Zn WGS catalyst.
- Copious data presented, but difficult to tell how much was done in 2004 and how much was done in 1998-2003 period project has been active.
- Considerable insights have been gained related to WGS reaction mechanisms, both in PGM/ceria and base metal catalysts.
- The issue of methanation is being addressed, and progress has been made, but there is still a long way to go and no clear evidence that the problem can be solved with the approaches being pursued.

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

This project was rated **2.3** for technology transfer and collaboration.

- There a few program partners.
- Collaborate with commercial WGS developer?
- Have past collaborators; little indication of any present collaborators.
- Need to make stronger "official" tie to industry.
- Need to renew contacts with industrial catalyst companies. Much progress in WGS catalysts has been accomplished by industry, but not reported in literature.
- There is no evidence of collaboration and no apparent interest by catalyst companies in licensing the technology. If the primary benefit of the project is improving our fundamental understanding of WGS catalysis (as admitted by the author), wouldn’t this benefit be strengthened by providing details on catalyst formulations and synthesis methods?
- From the recent patent literature, it is apparent that most of the large catalyst companies worldwide are pursuing development of high performance WGS catalysts – is this project duplicative and/or necessary in light of larger efforts being funded without DOE support?

Question 5: Approach to and relevance of proposed future research

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

- Program requires more emphasis of fuel system integration.
- Appears to be addressing the correct issues including impurities.
- Will Ruthenium and Rhenium meet cost targets?
- Need more focus on real-world failure mechanisms such as multiple start/stop cycles (with water condensation), cyclic exposure to low levels of sulfur-breakthrough, and over-temperature exposure.
- Future work on the ruthenium-rhenium system should be abandoned unless loadings can be reduced significantly and performance (and perhaps sulfur tolerance) can be improved to the point where the cost is justified.
- The primary focus of future work needs to be on improving performance and durability and reducing methanation. Without some progress in these areas, it is questionable whether effort should be expended in other areas (e.g., kinetic modeling, effects of impurities, etc.).

Strengths and weaknesses

Strengths
- The modeling effort will be beneficial to other programs.
- Team has a strong portfolio of diagnostic capabilities.
- Very good catalyst characterization capabilities.

- Strong use of data.
- Very good use of analytical infrastructure to unravel catalytic mechanisms.
- PI showed good understanding of problems with current commercial Cu/Zn WGS catalysts.
- PI showed good understanding of fact that sulfur continues to be major problem in all hydrocarbon reforming processes, including natural gas.
- Good combination of characterization, testing and modeling are being utilized on the project.
- Fundamental understanding of WGS catalysis and reaction mechanisms being obtained.

Weaknesses

- There was minimal discussion about impurity effect. No discussion of the integration into a full system (one of the barriers addressed). Future work is limited.
- Program needs better definition of quantifiable targets.
- Is there a strong need for WGS catalysts or would a better approach be to engineer around the limitations of conventional WGS catalysts?
- Refocusing of team needs to be ensured. We need to see this technology developed.
- Testing does not mimic failure mechanisms seen in real world fuel processors and thus makes it difficult to determine if catalysts would be durable under those circumstances.
- Minimal progress toward developing WGS catalysts that will meet the needs of industry.
- Path toward achieving technical and commercial viability is not clear.

Specific recommendations and additions or deletions to the work scope

- Include Cu-Zn in performance plots as a comparison to program's progress.
- Move more rapidly away from catalysts containing expensive materials (Re, etc.).
- Continue project.
- Discuss WGS failure mechanisms seen in current generations of fuel processors with people developing those fuel processors.
- Implement tests that mimic upset conditions to determine WGS vulnerability to those upsets.
- The issue of pyrophoricity needs to be addressed.
- Better and more realistic milestones need to be defined. These milestones should be based on both cost and performance, and be defined such that progress toward commercially-viable catalysts can be assessed.
- A go/no-go decision on this project should be implemented which should be based on progress toward meeting milestones and by the level of industrial interest.

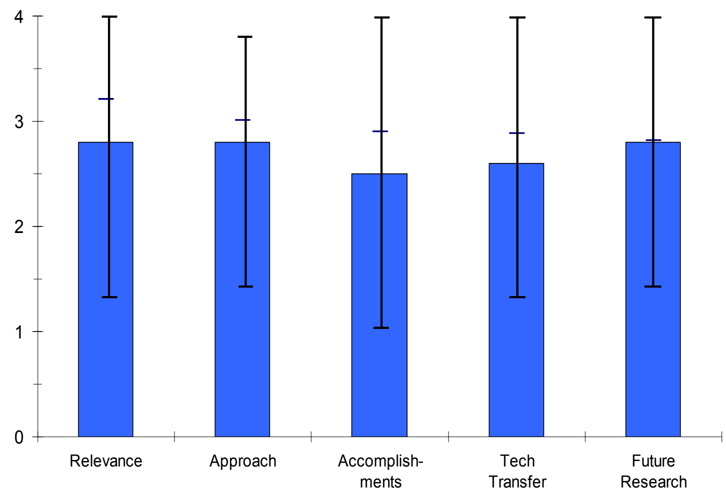
Project # FC-25: Catalysts for Autothermal Reforming

Krause, Theodore; Argonne National Laboratory

Brief Summary of Project

Argonne National Laboratory (ANL) plans to develop advanced fuel processing catalysts which will, when compared to Ni-based steam reforming catalysts, be able to process complex fuel mixtures such as gasoline; process these fuels at higher rates; be more resistant to coking and sulfur poisoning; improve our understanding of reforming reaction mechanisms, catalyst deactivation, and sulfur poisoning; and define operating parameters (e.g., air: fuel and steam: fuel ratios, temperature, gas hourly space velocities, catalyst geometry) to optimize catalyst performance and lifetime.

Overall Project Score: 2.7 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Should specify target performance in objectives.
- The change in direction to off-board reforming limits the program benefits.
- Development of new reforming catalysts is important for hydrogen production from fossil/renewable fuels.
- Current commercially available ATR catalysts are quite good and not large cost component compared to other sorbents and catalysts.
- Improved and cost effective reforming catalysts are important in distributed hydrogen production PEM fuel cell systems.
- Engineering solutions exist for using commercially available (nickel-based) catalysts for stationary systems so the project might not have wide scale applicability.
- Establishing a fundamental understanding of reaction conditions and mechanisms, and effects of fuel contaminants will be valuable to fuel processor system developers.
- Making hydrogen is essential for the hydrogen vision.

Question 2: Approach to performing the research and development

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

- Should consider other catalysts since national gas and LPG are simpler hydrocarbons than gasoline. Excellent choice of key experiments and parameters to understand catalyst behavior.
- Reactor studies and catalyst characterization will be beneficial.
- Good use of advanced analytical techniques to identify catalytic mechanisms.
- Not clear that more study on nickel or nickel/Rh catalysts is more valuable than work on less investigated non-precious metals.
- Testing approaches being pursued at Argonne appear to be very suitable for the work being conducted. The work is being augmented by extensive catalyst characterization using a variety of tools.
- Catalyst durability is being tested for each catalyst, in addition to activity and selectivity.
- The bi-metallic approaches being pursued are a logical way of reducing precious metal loadings.
- Commercially available catalysts should be tested to provide a baseline for comparing performance. Perhaps this has been done, but the data were not reported.

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

This project was rated **2.5** based on accomplishments.

- Good progress in understanding support-catalyst interactions. On right path to address carbon/coping formation in these catalyst systems.
- Did not address the barrier related to "fuel cell power system integration".
- It can take time to switch over to new fuels.
- This project has been in progress since 1995. Materials developed don't appear to be competitive with commercially available ATR catalysts.
- Although a number of tests have been completed, it was difficult to assess the relative performance of these catalysts without realistic comparisons to commercial catalysts.
- Low deactivation rates have been observed, but it is difficult to accurately claim comparative differences because of considerable scatter in the data.
- Good conversion results using ANL catalysts formulated with Rh.
- Ni-Rh catalysts are interesting, but not novel.

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

This project was rated **2.6** for technology transfer and collaboration.

- Complimentary capabilities throughout team accelerating program and avoiding effort duplication (i.e., propane experiments done where set up exists rather than re-create).
- There are limited program partners.
- Not clear if there are any present collaborators.
- Two publications submitted, work with GE, and multiple university contacts are good indicators of collaboration.
- It is not clear that the mentioned collaborations with Alabama, Puerto Rico, GM, and Minnesota were especially active ones.
- There doesn’t appear to be active interest in this project by catalyst companies or other potential commercialization partners.
- It might be useful to establish collaboration with Los Alamos, where reforming work also has been ongoing for several years.
- Need to work with catalyst companies in addition to universities.

Question 5: Approach to and relevance of proposed future research

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

- Logical follow -on based on prior results. Should define plan to address sulfur issues (bullet, but no strategy).
- Program requires more focus on integration.
- Good to see plan for more focus on sulfur sensitivity and durability testing.
- It is a good idea to evaluate Pd and Pt as co-catalysts with nickel, although at some point the focus should move toward non-precious metal catalysts (if the project goal is to deliver a non-precious metal, sulfur-tolerant ATR catalyst in FY’10).
- I agree with the Argonne’s decision to include the effects of sulfur (H_2S) impurities within their testing matrix (despite a comment to the contrary from a reviewer last year).
- If LPG and natural gas are to be the focus of this project, then future testing should be conducted with real LPG fuels (or more realistic surrogates).

Strengths and weaknesses

Strengths
- Good team with complimentary expertise. Well coordinated effort.
- The numerous publications will benefit discussion and learning.
- Very good use of analytical infrastructure to unravel catalytic mechanisms.
- Good combination of catalyst synthesis, characterization and testing are being utilized on the project.
- Pragmatic approach to catalyst development.
- Interesting experiments.
- Emphasis on understanding of bimetallic catalysts is a good approach.
- Concern on carbon deposition is essential.

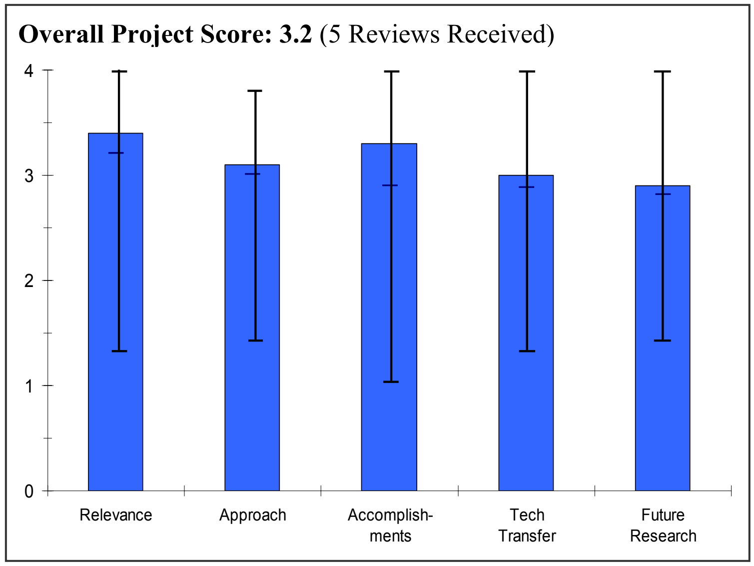
Weaknesses
- Catalyst has a high rate of deactivation.
- Did not show compelling reasons for nickel work.
- Path toward eliminating precious metals from the catalyst is not clear.
- Existing technology to remove H_2S from natural gas works well. This is true for mercaptans used for odorants as well. Probably need to do remove sulfur, so sulfur tolerance in catalysts is not critical.
- Rhodium has a long, successful history in POX. The question is, do you need to use Rh?

Specific recommendations and additions or deletions to the work scope

- DOE/industry need to address planned off-board reforming strategy, since need (or lack of need) for sulfur-tolerant reforming catalysts has significant consequences for catalyst development approaches. For off-board, a separate desulfurization step may make more sense.
- The program should focus on either optimizing catalyst for ATR or steam reforming.
- Pick up pace of catalyst formulation optimization.
- Apply advanced analytical techniques to determine mechanisms of sulfur poisoning and effects of promoters that can reduce these effects at low sulfur levels.
- Testing of real (and not simulated fuels) needs to be conducted.
- Baseline testing with commercially available catalysts needs to be performed.
- Experiments that include steam and other oxygen-containing compounds to explore soot implications would be interesting.
- Some soot could be volatile. Might want to add a "soot detector" to analyze the gas phase effluent.

Project # FC-26: Selective Catalytic Oxidation of Hydrogen Sulfide
Schwartz, Viviane; Oak Ridge National Laboratory

Brief Summary of Project



The goal of this project is to develop and optimize an oxidative process to reduce sulfur levels to the parts per billion range in a hydrogen-rich gas stream using low-cost, carbon-based catalysts to produce a low-sulfur fuel for use in fuel cells. Oak Ridge National Laboratory (ORNL) is identifying and using activated carbon catalysts that show potential for complete conversion of H_2S to elemental sulfur without formation of undesired products; investigating different activation protocols and carbon-based precursors that can lead to improved catalytic properties; characterizing the microstructures, surface properties, and impurity level of the catalysts and correlating to catalytic activity, selectivity, and durability; and performing catalytic studies of catalysts in hydrogen-rich and model reformate gas streams and investigating reaction mechanisms.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Very relevant- sulfur impurities, purification approaches with advanced materials very desirable.
- Very important and relevant project for the program.
- Sulfur removal is key technical hurdle in reforming sulfur containing feedstocks such as natural gas, LPG, biomass, and logistic fuels.
- Improved and cost effective approaches for eliminating sulfur are important for distributed hydrogen production when natural gas, propane and other hydrocarbons will be employed.
- Commercially available sulfur sorbents can be used in many distributed hydrogen generation applications but the selective oxidation technology has promise for reducing overall system size and minimizing maintenance.

Question 2: Approach to performing the research and development

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

- The approach is addressing key barriers and very well designed.
- Objectives and approach well defined. There are issues, however, with adding air/oxygen to the fuel stream.
- Strong approach to a difficult problem.
- Well designed test program with good mix of testing commercially available carbons and "lab made" carbons.
- Really need to see data on effects of many regeneration cycles on capacity and selectivity.
- The testing approaches being pursued appear to be viable for evaluating different catalyst formulations and reaction conditions.
- As the project proceeds, it will be important to conduct testing for longer durations and under conditions that more closely resemble the targeted application.

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

This project was rated **3.3** based on accomplishments.

- Very good progress and improved understanding achieved.
- Project requires further definition of how the process will be integrated into a complete system.
- Outstanding progress towards a viable system. Especially strong considering limited funding.
- Good progress for first year.
- Good presentation and explanation of raw data.
- It appears that ORNL has developed a superior carbon-based catalyst for their application based on comparisons with commercially available carbon catalysts.

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

This project was rated **3.0** for technology transfer and collaboration.

- Major companies with interest in the technology are approached.
- Limited collaborations with industry.
- Good team.
- Need to finalize collaboration with industry partner.
- Discussions have been initiated with UTC Fuel Cells, Conoco Phillips and Chevron-Texaco. These relationships should be pursued, both as a means of finding commercial outlets and for identifying the best testing conditions.
- An impressive list of publications and presentations from the past year.

Question 5: Approach to and relevance of proposed future research

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

- Focusing on improved catalyst by using supported carbons and by ranging the structure and morphology of the carbon catalysts is excellent goal.
- Very well reasoned future plan.
- Now that good materials have been identified, need to focus on process development such as regeneration of spent bed.
- For the most part, the future work plan is acceptable.
- Industry stakeholders should be consulted regarding testing conditions required to establish feasibility and generate interest in the technology.
- A manufacturing cost assessment should be conducted.

Strengths and weaknesses

Strengths

- Program includes strong investigation into limiting side reactions.
- Good viability.
- Novel approach to dealing with high levels of sulfur. No other technology appears to be as good fit for reforming.
- High level of expertise in group.
- Good understanding of technical hurdles of real world reforming.
- Systematic approach to catalyst development.
- Quality of results obtained given a relatively low budget.

Weaknesses

- No clear plan on how to remove sulfur from oxidation sites.
- Lack of technical details limits the amount of feedback that a reviewer can give. What are the precursors to be looked at? Will a pillared clay or silicate be used as a template? Exactly what will the new carbons be made from and what design variables will be employed?
- Need more focus on process development.
- Relatively low space velocity (about 3100 h^{-1}) means large beds.
- Need more application-specific testing conditions and longer term test durations.

Specific recommendations and additions or deletions to the work scope

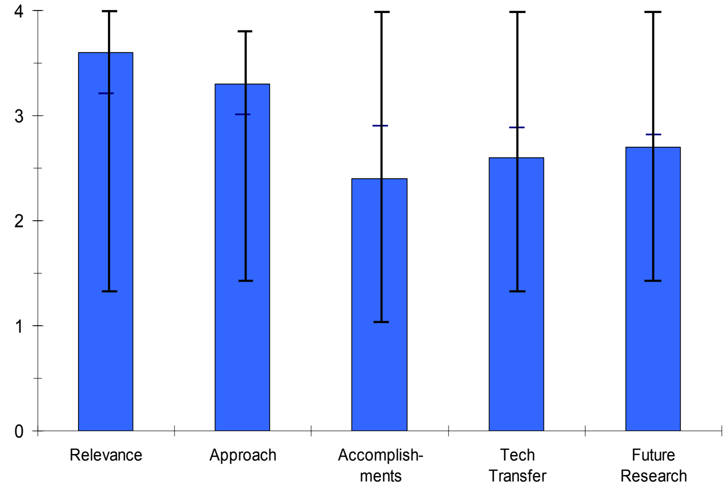
- Develop regeneration methods.
- Test effect of regeneration on carbon activity and capacity.
- Estimates of cost were not presented.
- A manufacturing cost analysis for the catalyst and for the process is needed for an assessment of viability.

Project # FC-27: Cost and Performance Enhancements for a PEM Fuel Cell Turbocompressor
Gee, Mark K.; Honeywell

Brief Summary of Project

Honeywell is developing an enhanced turbocompressor configuration for integration into a PEM fuel cell that reduces costs while increasing design flexibility. Honeywell is utilizing their expertise in automotive and aerospace turbo machinery technology, variable nozzle turbine inlet geometry, mixed flow type compressors, and contaminant/oil free, zero maintenance compliant foil air bearings to achieve this objective. The final product will have a modular design, high efficiency, and variable speed motor controller topology design

Overall Project Score: 2.9 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.6** for its relevance to DOE objectives.

- Turbocompressor and motor development for pressurized automotive application.
- Cost was identified as a major issue. There was no analysis plan presented which might lead to reduced cost.
- Building on existing turbomachinery technology a logical approach- but will it be sufficient for this very challenging application?
- While important, less so as developers move to lower pressure.
- Explicitly addresses a need identified by DOE.
- Definite need for efficient compressor for transportation systems.
- Project is important.

Question 2: Approach to performing the research and development

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

- As long as there is a need for pressurized stack operation this project is essential.
- Basic approach is correct; modularization is key.
- Hardware development and testing is a good blend towards objective.
- Work based on applying best use of existing technology.
- Have made excellent progress on their design.
- Apparently good although little useful information presented.
- It is unlikely that this configuration can ever come close to cost targets.
- Good understanding of technical barriers and customer needs.
- Approach is logical.
- Unclear what input may have been obtained from fuel cell power system/automotive OEM'.

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

This project was rated **2.4** based on accomplishments.

- This has been a slow project but with steady progress.

- The incorporation of variable nozzle geometry to flatten efficiency curve and foil bearings for durability were important steps.
- Features impact cost of course and cost is a key criterion for automotive applications.
- A ruler to indicate actual size of the hardware would have been helpful. Pictures of turbine wheels would be useful. Good mechanical design with built in flexibility for future vendors.
- Detailed design done with significant improvements to the modular design to make it easier to adapt to different applications and most cost effective to manufacture.
- Inability to reach peak power goal a little troubling, although part-load power good.
- No evidence presented that any of the components will meet targets.
- Virtually everything presented was “estimated” or “Projected”.
- Relatively little seems to have been accomplished for the time and money invested.
- Progress after 8 year project life is disappointing.
- Motor controller volume too large.
- Progress seemed slow. The 4th objective should belong to a power electronics supplier. There are quite a few of them out there. Honeywell should focus on the turbomachinery itself.
- Appears as if initial DOE targets have been met.
- Progress remains slow. Technical objectives 2 and 3 remain incomplete.
- Unclear progress made since 2004.

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

This project was rated **2.6** for technology transfer and collaboration.

- Limited collaboration shown. Honeywell is not enough. Involve the big three.
- Collaboration with Argonne and fuel cell /automotive OEM's and important to define requirements. However it is not clear how the technology will be transferred to industry.
- Although some communication with potential users, it is not clear the extent to which this design will be able to meet the needs of users.
- Some interactions claimed but little evidence of real exchange.
- No links mentioned in presentation, but in answer to question from audience presenter mentioned confidential negotiations.
- Need to establish collaboration with power electronic suppliers and initiate OEM involvement for air system testing.
- OEM contributions not detailed.
- Argonne National Labs contributions not specified.
- Dissemination of non-proprietary results through additional publications recommended.

Question 5: Approach to and relevance of proposed future research

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

- No contingencies addressed. Program completion is tight, no chance for design corrections.
- There was no discussion of plans beyond defined program objectives.
- Approach to future work seems reasonable.
- A continuation of hardware development with no mention of critical reviews, test plans, etc.
- Need to complete project as planned.
- Need to focus on the turbocompressor, provide test results for the unit.
- Proposed work will conclude project.
- Additional details would have been useful (What is needed to complete design, what remains in the fabrication and assembly areas)?

Strengths and weaknesses

Strengths
- Solid hardware design and build program. Utilizing past experience and industry expertise.
- Program incorporates good blend of hardware, testing, and analysis.
- Based on existing production technology- good chance to be as cost effective as possible
- Modular design improved and the right approach
- An organization with known strengths in turbomachinery design and development.
- High degree of mechanical engineering skills demonstrated.
- Design work.
- Strong background in the design and manufacture of turbocompressors.

Weaknesses
- Program plan needs time for second build although modular approach is beneficial in shortening turnaround time. Add feature of testing with integrated fuel cell system.
- It is evident that current test hardware won't meet DOE guidelines. There should be a path (analytics) which highlights areas that must be addressed to meet those goals.
- Input power is 2X too high and no approach identified to reach that goal.
- Potential matching issues to fuel cell applications- no data shown to demonstrate if they can satisfy the requirements of a range of fuel cell sizes and applications.
- Although this design can operate as only a compressor, existing power consumption problems are likely to be magnified if no expander section included- need data to understand this situation.
- Did not show how to improve transient response time.
- It appears that this program is getting relatively little priority within the organization. Accomplishments seem to be much less than they should be at this stage.
- Little evidence of collaboration or use of outside experts.
- No cost estimates presented.
- Need to build and test system.
- Details significantly lacking.
- Data on why this design is expected to be superior to previous designs,
- What were the results from Technical Objective 1?
- No discussion on how efficiency, cost, weight, etc. may compare with what is currently available

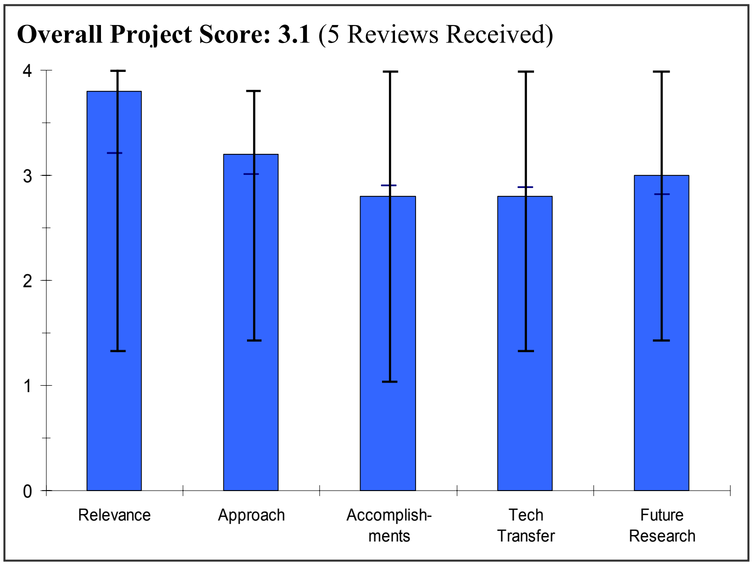
Specific recommendations and additions or deletions to the work scope

- Program scope is about right except for integrated fuel cell system test. Could be in a simulated configuration.
- Minor things only: (1) Provide data to show that this design can meet requirements for a range of fuel cells (2) Focus on solving input power issue- or show % of operating range they meet power targets (3) Discuss operation as a compressor only - show power required (4) More emphasis on improving transient response to meet targets
- Do a careful review of the project, including a critical design review, to see if there is any reasonable hope of meeting targets.
- Cost projections should be done with and without the expander.
- Need to establish collaboration with power electronic suppliers and initiate OEM involvement for air system testing.
- Initiate involvement with Ohio State University for air system testing.
- Detail fuel cell cost and performance enhancements as a result of implementation of the turbocompressor.
- Document interactions and contributions from team members.
- Publish.

Project # FC-28: Development and Testing of a Toroidal Intersecting Vane Machine (TIVM) Air Management System
Bailey, Sterling; Mechanology, LLC

Brief Summary of Project

Mechanology's overall objective in this project is to develop the innovative Toroidal Intersecting Vane Machine (TIVM) concept into working compressor/expander/motor hardware that satisfies the FreedomCAR guidelines and is easily adaptable to individual car system requirements. Mechanology measures the TIVM air management system performance with the goal of providing the global automotive fuel cell market a significantly improved air management product.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- The project addresses a vital component in the "balance of plant" for automotive fuel cells- a pump (compressor) for the air handling system.
- The relevance is demonstrated by project goals that track DOE requirements.
- Hardware being addressed would greatly benefit DOE fuel cell objectives if successful.
- Compressor/expander is a key element in successful fuel cell commercialization.
- Low cost, efficient air compressors are a key automotive fuel cell system enabler.
- This new concept, at this stage, has great prospects.

Question 2: Approach to performing the research and development

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

- The approach, while containing a high degree of technical risk, is fundamentally sound and apparently working.
- The project is designed to meet DOE objectives.
- Program has developed some impressive, but complicated hardware. Approach seems to tinker with the hardware to achieve performance enhancements.
- Innovative but high risk approach.
- Complexity implies high cost- high part content.
- This might not be the best application for this component.
- This project is a major development effort using a completely new approach- is the payoff worth it?
- Appears to be very novel concept.
- The innovative designs, particularly the 'layer cake' version, offer the potential of achieving most, if not all, of the primary goals.

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

This project was rated **2.8** based on accomplishments.

- The PI reports that all objectives are being met, or are expected to be met, with one possible exception—the input power requirement.

- Additional work is required to reduce friction (increase efficiency).
- Analysis needs to be done on the effect of shock and vibration on an operating device (maximum speed), to see if the device can withstand automotive operating conditions. (The device has a large number of surfaces, machined to tight tolerances moving past each other at high speed.)
- Safety analysis does not include companion electric motor as potential ignition source.
- Progress at low/intermediate speeds looks promising; key will be when operating at high speed. It is not clear that low production costs will be achievable when seal and tolerance impacts appear at high speed.
- This project seems not to be on track for completion on time.
- Appears to have made significant progress on an improved design but I am not sure it is enough.
- New design features simplify parts.
- Improved air handling directly improves efficiency.
- New seals needed - design being developed.
- At the beginning of developing seal materials.
- Some progress made as machine has achieved half rated speed.
- The seals are key to achieving efficiency, durable design, and a contaminant free air stream. These have yet to be demonstrated.
- Air machine efficiency has yet to be demonstrated.
- Excellent progress made in relatively short time.

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

This project was rated **2.8** for technology transfer and collaboration.

- PI spoke of a number of interactions with auto makers.
- Patent disclosures have been made on the technology.
- Journal publication not relevant for this project.
- Major interaction has been with Argonne National Laboratory. PI only.
- Although vague, indications are that sufficient collaborations with potential users and high volume manufacturers exist.
- Appears to be a single-team effort. Help needed on seal design.
- Close collaborations with end-users is imperative- particularly at the evaluation/validation stage.

Question 5: Approach to and relevance of proposed future research

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

- PI proposes appropriate activities to attempt to achieve goals and to provide contract deliverables.
- PI spoke of spin-off product that may be commercialized in CY 2006 with private funding.
- PI did not present a path beyond achieving currently defines objectives.
- The plans laid out seem appropriate but assume solutions to several large problems (like sealing) will come quickly. This is a major developmental effort on a complex new mechanical device that has many issues.
- Future direction addresses the correct issues. The question is the probability of achieving the targets.
- Proposed future work is on target. Need to see efficiency map data.

Strengths and weaknesses

Strengths
- Innovative approach to designing an air pump, with characteristics appropriate for fuel cell use.
- Program is making good progress in hardware development and testing.
- A new approach with many potential applications.
- Significant progress made but a lot more to go.
- Novel compressor/expander design.
- Capable of a range of pressures at all flows which is desirable for stack operating condition control.
- Notable strength is the combination of innovative designs and the PI's exceptional enthusiasm.

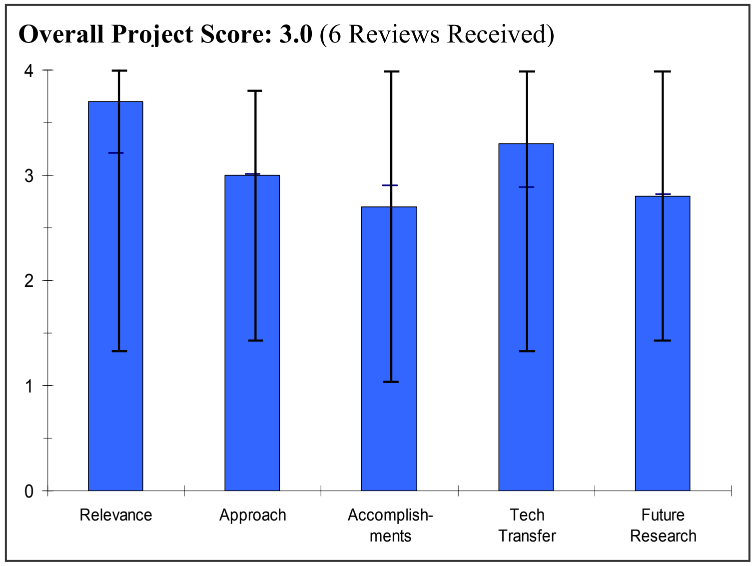
Weaknesses

- Durability of design still uncertain since it remains to be analyzed and tested.
- Ability to achieve and to maintain low power load and design efficiencies during use yet to be established.
- The major barriers to achievement will not be known until high speed testing of integrated hardware is accomplished. That is when the impacts of friction, seals, and tolerances will be fully understood in a dynamic thermal environment.
- Shock and resistance not really addressed.
- No discussion on the motor/controller needed.
- Initial data is not that encouraging - have not demonstrated ability to attain pressure.
- No data on efficiency.
- Significant sealing issues with no real idea how to solve them.
- Still have friction issues.
- Have not tested at temperature or under moist conditions.
- Low speed, positive displacement (PD) devices generally are large for a given airflow. This device is claimed to be smaller, but it is unclear how much so. It is not predicted to reach DOE target. Data is required.
- PD devices tend to be less efficient than dynamic machines. The developers need to demonstrate that this machine is capable of the high efficiencies promised.

Specific recommendations and additions or deletions to the work scope

- The operating concept is difficult to understand; the early concept vu graphs did little to help. Video would have been useful.
- I am not sure that a fuel cell application is really the right one for this device. This is a high risk development project that is making progress but has little chance of being completed on time while solving all the problems that remain. Significant progress on producing data relevant to fuel cell applications is needed. Although achieving the appropriate boost levels for fuel cell applications appears to be possible, many other fundamental issues have not been addressed such as efficiency (a critical issue), shock and vibration, operation at temperature and under moist conditions, and the very formidable sealing issues (which bear on friction- still a problem- efficiency and durability).
- If the program managers deem this high risk R&D project to have great potential, be ready to extend the time and funding to solve these serious un-resolved developmental issues. My judgment from the data presented here is that we don't really have the information to determine if this device really has enough potential to continue its funding. It will be an up-hill battle to develop this device for fuel cell applications. My own feeling is that is has other potential applications that are not as severe as fuel cells where it would likely make more sense IF the significant issues can be solved satisfactorily- and that is certainly not a given.
- Just get the data already! Good luck!
- Have independent, third party verification of claimed attractive cost estimates. More efficiency V's load data needed.

Project # FC-29: Development of a Thermal and Water Management (TWM) System for PEM Fuel Cells
Pont, Giullermo; Honeywell



Brief Summary of Project

Honeywell's project is to assist DOE in developing a humidification and cooling system for PEM fuel cells in transportation applications. Objectives of the project are to focus on cathode humidification, and fuel cell heat rejection for an 80 kW fuel cell power system; study pressurized thermal and water management (TWM) system performance; analyze steady-state automotive operating conditions for comparison of concept schematics; establish TWM system/component specification; and demonstrate the performance of a breadboard TWM system with research hardware.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- Well focused on thermal and H_2O management issues. Doesn't address integration with other system components.
- Supports complexity resolution regarding H_2O dynamics in large PEM systems.
- The project addresses technologies critical to obtaining the HFCIT goals for fuel cells.
- Essential components for successful FC commercialization.
- This type of (apparent) in-depth investigation is extremely important in pursuing optimum configurations.
- TWM is essential to compact, reliable, durable fuel cell power systems.

Question 2: Approach to performing the research and development

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

- Good survey of relevant approaches.
- Multiple paths appropriate to be able to downselect. QFD used effectively. Why 60% RH requirement when we know PEM's need higher water content?
- Approach is well thought out.
- Opportunity for integrating enthalpy wheel with heat-management should be investigated.
- Humidification to 60%RH at cathode only may be insufficient humidification for some designs-may need to humidify anode or go to higher RH to prevent dehydration and ensure adequate membrane lifetimes in other MEA designs.
- Humidification approach on target.
- Thermal options based on existing heat exchangers- will this be adequate?
- No mention of models or simulation techniques.
- Sophistication and validity of analyses are unknown.
- Choice of alternative configurations and especially materials seem too limited.
- The evaluation priority scheme is a way to choose between a number of less-than-adequate approaches.
- Weighting and priority need input from OEMs and system developers.
- Apparently no consideration to date of startup (dry?), transients or wet shutdown (freeze?)
- Approach should not lose site of importance of non-steady operation.

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

This project was rated **2.7** based on accomplishments.

- Criteria prioritization system well defined. Good basis for system down select. Critical subscale tests promising. Show target can be met for water management.
- Most of these methods have been available- not sure what is new.
- Good progress made in design studies and developing system targets.
- Systems that can meet targets have been identified.
- Most work on humidification- but the real challenge is for thermal management.
- Two humidification designs selected and developed- one appears better than the other but design and evaluation is continuing.
- Aerospace and automotive heat exchangers evaluated- AL foam, microchannel and aerospace designs selected all need significant development for mass production and cost reduction.
- Also need validation of performance of heat exchanger materials and designs.
- Apparently good. Too few details given to be sure.
- More alternatives probably should have been considered.
- Selected options have been evaluated several times over the last decade by others and judged inadequate.

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

This project was rated **3.3** for technology transfer and collaboration.

- Strong group of collaborations with critical expertise in necessary areas.
- Should enhance and broaden relationships.
- More collaboration with OEM's and automakers would help further define target.
- Improved collaboration with fuel cell community described.
- Need more collaboration with organizations that can assist heat exchanger material and design development.
- Appears that there is excellent collaboration.
- Appropriate collaborations are underway.
- No technology to transfer at this point.

Question 5: Approach to and relevance of proposed future research

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

- Need to identify targets better. Are WM and TM independent or interdependent and how is this being addressed?
- Must focus on system as a whole. Emprise reliability should be assessed.
- On track to finish water management components.
- Work focusing on designing and developing the advanced heat exchangers.
- Follow-on proposed should address many important issues.
- Planning adequate but uninspired.

Strengths and weaknesses

Strengths
- Systematic approaches with rational down select based on design and subscale experiments.
- Well organized component selection process. Lots of experience with aerospace engineering in-house.
- Project team has the capabilities needed.
- Thorough work on humidification design.
- Organization has excellent known appropriate capabilities.

Weaknesses

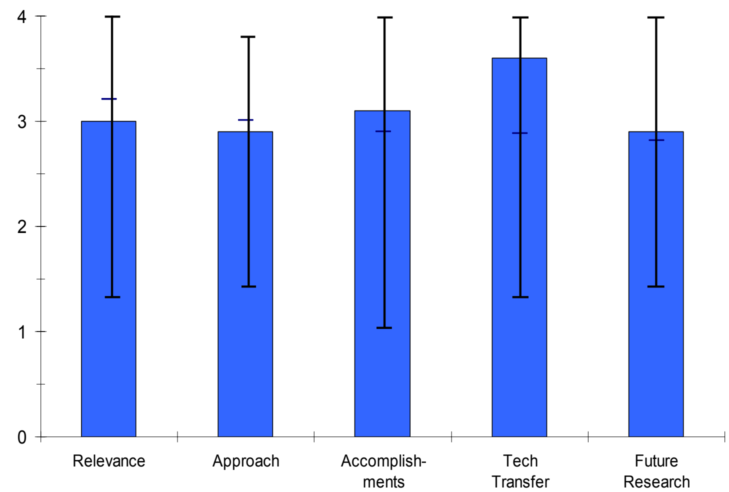
- Looks like power requirement (target) changed from 50 kW to 80 kW. Is this program adjusting spec for future work? Doesn't address if TWM system requirements are overly sensitive to specific PEM used. Is there a universal TWM solution for low T and high T PEM?
- Cathode only - why not anode? Design is based on a low RH. Complexity of controls should be considered. Selection of technology will be fuel cell system design specific. Can this work be generalized?
- Have not looked at freeze issues in humidification system.
- Have not built in vibration and shock analysis to humidification system design screening.
- Have not considered possible recycled water contamination issues.
- Not made significant progress on the heat exchanger development- this is the real challenge and the largest technological stretch.
- May not have adequately looked at all the potential heat exchanger material options.
- The work does not seem to have the depth that it should have.
- The 1 to 5 rating approach is really poor.
- Project unlikely to advance state of the art.

Specific recommendations and additions or deletions to the work scope

- Thermal management comparison to standard auto not clear. What are the targets? (No numbers on graph). Are targets significantly different for fuel cell systems than current automotive systems?
- Include freeze protection design considerations.
- A better quantification of effects of system variables (such as %RH needed or temperature of operation) on system requirements would be beneficial.
- Need to address freeze issues, shock and vibration requirement in humidification design.
- Need to analyze and address possible water contamination issues with recycled water.
- Status of heat exchanger work troubling. This key technological barrier needs more effort. May not have adequately considered all material and design options.
- Significant issues remain in cost effective manufacture and durability of aluminum foam heat exchangers - more emphasis on this seems prudent. Perhaps a new collaboration partner in this field is appropriate.
- A rating approach for attributes can be very useful, but should be better thought out than this one.

Project # FC-30: Development of Sensors for Automotive PEM-based Fuel Cells
Knight, Brian; United Technologies Corporation

Overall Project Score: 3.1 (7 Reviews Received)



Brief Summary of Project

United Technologies Corp. Fuel Cells (UTCFC) and its team are developing physical and chemical sensors for proton exchange membrane fuel cell power plants for automotive applications aimed at low cost (<$20 per sensor) at 500,000 quantity. Work in chemical sensors includes process streams before, in, and after reformer and before and in fuel cell stack; CO, H_2, O_2, H_2S, NH_3 types; and safety (hydrogen). Physical sensors focus includes temperature, pressure, relative humidity, and flow.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Sensor development critical for the development of robust systems and to meeting life targets.
- This project presumably started out as being applicable to reformate-based automotive fuel cell systems. With the change to direct hydrogen systems for automotive use, many of the chemical species sensors will be of interest only to stationary systems, for which performance requirements (e.g. response time) are quite different from those for automotive systems.
- The program aimed to produce a sensor database, which would be useful to a variety of vendors.
- The UTC team includes some of the premier developers of sensor components.
- New automotive-grade sensors contribute to successful fuel cell commercialization.
- Reformate-based sensors can contribute to off-board hydrogen production technologies.

Question 2: Approach to performing the research and development

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

- Question the need for T, P, and flow sensor development. Can business case be made for these sensors compared with existing sensors?
- Chemical sensors have immediate need.
- Approach relies on selection of multiple suppliers to develop the sensor technologies to UTC requirements.
- Only two of the team members (ATMI and NexTech) are doing any developmental work. UTCFC, UTRC, and IIT appear to primarily be doing identifying existing sensors or sensing approaches, and testing sensors developed or provided by others. Indeed, there appears to be little or no development needed for the physical sensors (T, P, RH, flow).
- ATMI deserves credit for exploring the MEMS-based platform with multi-layer sensing films.
- Program well structured to develop (as necessary), evaluate, and report on a wide range of sensors applicable to future fuel cell vehicle development.

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

This project was rated **3.1** based on accomplishments.

- Sulfur sensors need lower sensitivity.
- Sensors need additional sensitivity testing for cross-sensitivity (such as RH, S).
- Difficult to gauge progress by the data shown, however, drift in sensors appears to be an issue.
- Would be useful to have results plotted against target sensitivity levels, response curves, and drift with time.
- Approaches for addressing drift not discussed.
- The main developments have been by ATMI and NexTech.
- ATMI has shown promising results with ambient (safety) hydrogen sensor and process hydrogen sensor, including reducing some of the baseline drift rate (although that is still too high), as well as the H_2S sensor.
- NexTech's H_2S sensor data look promising, but the performance of the CO sensor is still inadequate - it seems to get "flooded" by high levels of CO so that it is difficult to correlate response to the exposure level.
- The results reported appear commensurate with program goals and resources applied. Program embodied a good technical team.
- Each sensor's operation and reliability are making progress.
- Good benchmarking work. Need to share information with other sensor development work besides just UTC.
- Keep the focus on sensitivity, reliability and durability. Cost should be the lowest priority for this task.

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

This project was rated **3.6** for technology transfer and collaboration.

- Good integrated team.
- Large team assembled.
- Would be helpful to see allocation of resources against partners and sensor area.
- The project team includes academic, research, and commercial (end-user) organizations, so that advances can be readily validated and incorporated into prototype and commercial systems.
- Project had excellent collaboration with relevant participants with necessary resources to accomplish goals.
- Close technical coordination is evident and the progress shows this.
- A strong team of developers and testing capability.
- Good collaboration but lack OEM involvement.

Question 5: Approach to and relevance of proposed future research

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

- H_2S cross-sensitivity with CO sensors critical.
- RH cross-sensitivity critical for all sensors.
- PI did not present clear picture of how subcontractors will address gaps in performance. Whether problems are fundamental in nature or engineering/design issues.
- As the project objectives have shifted from automotive to stationary systems, with the concomitant change in performance requirements, future activities should change accordingly. On the other hand, since this project is close to completion, the identified future activities appear to be the logical ones to bring the project to an orderly conclusion.
- Although continued development and evaluation of various sensors should be continued, this effort should conclude with a meaningful technical report available to all fuel cell and automotive OEM's. Publication of this report was not discussed.
- The future work appears to address the open issues identified.
- Given Hydrogen Program re-direction, majority of remaining work should be on RH sensor.

Strengths and weaknesses

Strengths
- The project is being directed and coordinated by an end-user, a fuel cell company, which helps ensure that the work is relevant and practical considerations are not ignored.
- ATMI's MEMS-based approach has the potential to yield, compact, low-cost, and sensitive sensors.
- This activity investigated a wide variety of potential fuel cell sensors in relevant operational conditions.
- The UTC team is unique. Each sensor will be tested in a 'real' UTC fuel cell and stack test stand using established test protocols.
- A strong multi-disciplinary team.
- Appears to have shown very good progress in most sensor designs with commercialization achieved in a few.
- Good results.

Weaknesses
- Work seems primarily only anode sensors; need cathode chemical sensors also.
- The long-term baseline drift in the H_2S sensor makes it likely only for an application where it can be recalibrated (lab).
- It would be useful to understand how sensor requirements derive from system requirements and how success against these requirements would impact system performance and life.
- Data presentation could be improved to show performance relative to requirements, time scales relevant to response times.
- The sensors are primarily for fuel processor-based systems. Since these will be stationary systems, the performance requirements are different from those needed for automotive systems and these different requirements should be incorporated into program, such as longer lifetimes and, perhaps, somewhat higher costs.
- PI did not discuss progress against established cost targets.
- Need to share data to other sensor developers. Sensor is not UTC core business.

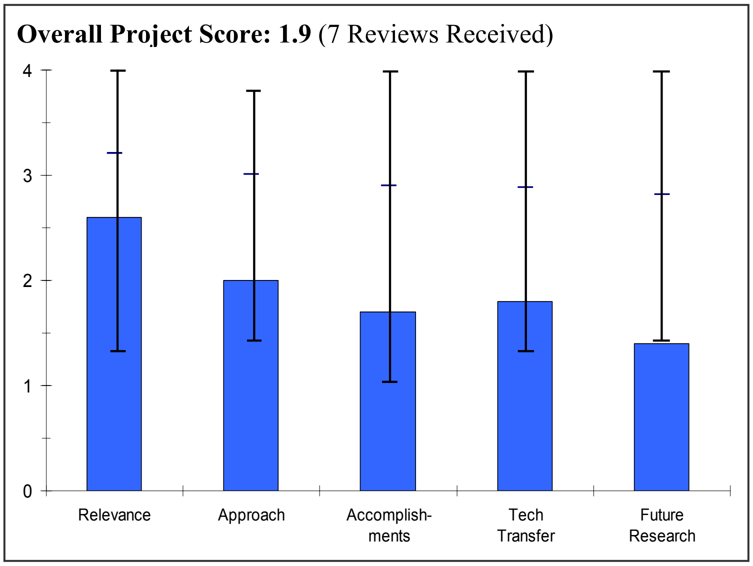
Specific recommendations and additions or deletions to the work scope

- Stack sensor tested 2.5-40% hydrogen - should be evaluated to 100% hydrogen.
- H_2S sensitivity needs to be measured for all sensors.
- RH sensitivities need to be measured for all sensors.
- 10 ppb H_2S detection limit should be lowered. 10 ppb levels of H_2S still are an anode poison.
- This project is coming to a close.
- Program should culminate with a relevant database available to all participants in the DOE program.
- Given Hydrogen Program re-direction, majority of remaining work should be on RH sensor.
- Need to work with a few OEM on the development and validation.

Project # FC-31: 2005 DOE Hydrogen Program Sensor Development
Gehman, Richard; Honeywell

Brief Summary of Project

This Honeywell project is leading to the creation of physical sensors suitable for monitoring and controlling a proton exchange membrane fuel cell system. Key tasks include defining sensor requirements, developing sensors, building and testing prototype sensors, and field testing.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- RH sensors are needed for anode, cathode, and fuel processors.
- Achieving DOE objectives will require well-controlled fuel cell operation, which in turn requires accurate and stable sensors of operating parameters and conditions.
- Although program was well planned, disappointing results will result in program termination.
- This program in impossible to rate. Honeywell's effort may have been good but the results were not acceptable to the DOE program.
- Automotive-grade RH sensors would contribute to successful fuel cell commercialization.
- There was less need for development of other physical sensors (T, P, air flow), which is reflected in the mid-program decisions to de-emphasize their development.
- Inexpensive, fast, robust sensors will be essential to deployment of reliable fuel cell power systems.

Question 2: Approach to performing the research and development

This project was rated **2.0** on its approach.

- Approach did not work (in hindsight).
- The project is not only "unlikely" to meet objectives; the project has not met objectives one by one until it has reached the point where the PI is voluntarily terminating the project early.
- The PI was conscientious to address technical barriers in the DOE technical plan and industry needs.
- The PI was conscientious to work with potential customers to ensure that research and development met their current needs.
- The approach is "poor" overall because the PI is simply giving up.
- Approach was sound, goals well defined; results were fairly and honestly reported.
- Approach of trying to specify, adapt and package existing technology has proven to not achieve goals.
- The discussion on hydrogen sensing does not sound right. There is a real need for wet hydrogen flow metering.

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

This project was rated **1.7** based on accomplishments.

- Design for RH sensor did not show stability. Possible that pre-conditioning might make it more stable.
- The project is terminating early for failure to meet its overall technical objectives.
- When asked what useful information was generated by this project despite its early termination, PI rattled off a list of achievements that could play roles in future sensor development.

- Examples of valuable lessons learned include methods for dealing with condensation in relative humidity sensors, methods for decreasing response time, and methods for measuring airflow.
- Results will lead to no meaningful enhancements to DOE program.
- Cannot fault investigators for re-direction to hydrogen, which made the majority of the physical sensors obsolete.
- Some progress on the RH sensor, which still has application to DOE automotive goals.
- Project apparently terminated and company investment stopped.
- Little to show for effort.
- Lack comparison against sensors on the market. How do these benchmark against the existing sensors?

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

This project was rated **1.8** for technology transfer and collaboration.

- The PI described much effort to coordinate work with potential automotive customers in order to meet their needs.
- This project suffered from Day 1 with "not invented here" syndrome- project was scoped to apply applicant's existing product line to fuel cell application, rather than to use the applicant's knowledge, skills, and experience to seek out and apply best available technology from whatever source.
- Contractor had all necessary in house resources to accomplish this effort.
- No indication of useful collaborations.
- No technology to transfer.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.4** for proposed future work.

- Project being terminated.
- The PI should be commended for his honesty to halt a line of research when it became clear that this line would not meet program objectives.
- When asked about thoughts for directions of future research on relative humidity sensors, PI rattled off a list of ideas, such as designing a sensor based on thermal conductivity.
- It is incongruous that the project voluntarily terminated early rather than beginning to address alternate means identified that could have advanced program and project goals.
- More or less N/A as program is scheduled for early termination.
- Unclear as to why RH sensor was judged not promising for further development.

Strengths and weaknesses

Strengths
- Commendable honesty
- Developed transferable knowledge about techniques in design of sensors for automotive fuel cell applications.
- Good approach and objectives, contractor developed and tested sensors in accordance with plan.
- The team has strong capabilities in physical sensor development.

Weaknesses
- Business case does not work for most sensors proposed.
- Narrow scoping/outlook of project to adaptation of applicant's current products led to questionable decision to terminate project early.
- Contractor discovered three of the four sensors were irrelevant to DOE Program; fourth sensor could not satisfy program objectives.
- Unfortunately Honeywell decided to terminate its effort for business reasons and no sensor products that could be "shoe-horned" into the DOE program.

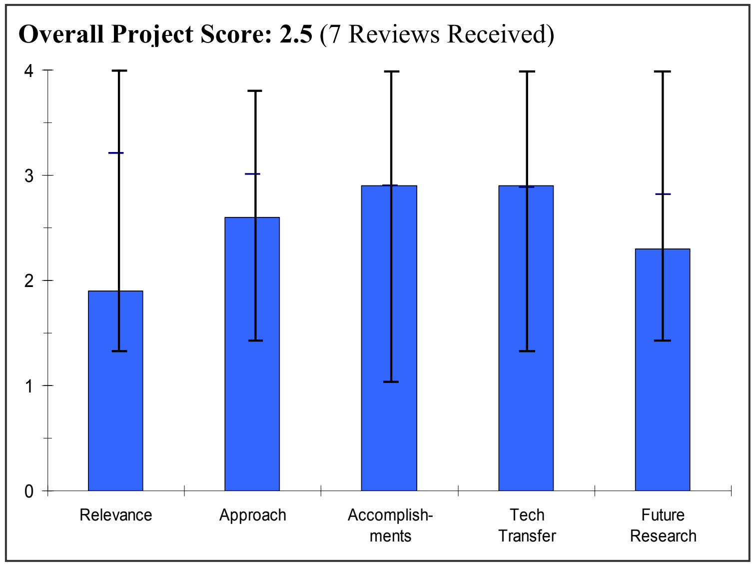
Specific recommendations and additions or deletions to the work scope

- Since approach is not working, and apparently will not work, project should be terminated.
- Possible that pre-conditioning of sensor/and sensor material might limit sensor baseline drift.
- The RH sensor performance should be compared to that in the Honeywell program, and against other concurrently developed commercial and lab-grade devices. Perhaps there is still an application here?
- Ensure that learning's, especially on RH sensor, are documented and made available to continuing programs.
- Decision to terminate project seems correct.

Project # FC-32: DMFC Prototype Demonstration for Consumer Electronics Applications
Sievers, Bob; MTI MicroFuel Cells

Brief Summary of Project

This program, led by MTI and including a number of industrial partners, will deliver a demonstration and real-world validation of a complete, integrated portable direct methanol fuel cell (DMFC) system for consumer electronics. High volume manufacturing techniques will be developed to reduce cost to levels appropriate for consumer electronics market entry. The fuel refill infrastructure and product design will be driven by the evolving regulatory framework, such that unrestricted and ubiquitous use of methanol powered devices can be achieved.



Question 1: Relevance to overall DOE objectives

This project earned a score of **1.9** for its relevance to DOE objectives.

- DMFC (direct methanol fuel cells) fit fairly well in the hydrogen fuel initiative program.
- The fuel is methanol and not hydrogen.
- Consumers will be exposed to fuel cells pre cars and stationary applications which is beneficial.
- I like the application, but the sponsor and contractor need a much more effective way of articulating the synergy of this program - it looks like corporate welfare.
- Not clear at all what the $6.2 Million is paying for.
- Not clear which DMFC technology could translate to other hydrogen fuel cell initiatives.
- Critical for early adoption of fuel cells, but it's not clear if timing fits 2010 goals.
- The relevance of the project lies in the advancement of fuel cell technology generally. The project has less relevance to the DOE mission to reduce energy consumption and reduce the Nation's dependence on foreign oil. A successful demonstration will provide an opportunity to advance manufacturing process technology for fuel cell technology.
- The connection between small-scale DMFC's for consumer electronics and the future hydrogen economy (e.g., stationary and/or transportation) is not clear to the reviewer and was not well articulated.
- Synergies between DMFC's and stationary/transportation applications seem to be weak. Even if similar membranes are used, DMFC work does not appear to be advancing membrane technology, and the membrane volume even at high production levels for consumer electronic applications is miniscule compared with the volumes required for transportation (see recommendations below for calculations). Thus, little stands to be gained from economies of scale/learning curve standpoint.
- The DMFC technology is much nearer the market requirements for performance, cost, size, and weight than for other fuel cell applications. Thus, it seems to the reviewer that DOE funding of such applications is not required and will be undertaken by industry.
- While it is important to get some fuel cells out there, it is unclear how this helps toward any goals; technology is totally different from what will be used for any of the main applications.

Question 2: Approach to performing the research and development

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

- The approach of balance of plant and manufacturing is well thought out.
- MEA (membrane electrode assembly) optimization work is very important.
- MEA work was not covered sufficiently.
- The project has a lack of focus on durability. The project has no goal to improve efficiency, which may lead to some of the projected power goals.
- Reducing cost via system simplification is good, but the cost versus volume was not adequately described.
- Design looks mature already, and presentation gave impression that it was - no significant technical challenges were outlined or explained.
- What is the manufacturing process? No flowcharts explained, nor itemization of process challenges.
- What explicit steps will be taken to reduce cost?
- Approach seems overly simple.
- Plans to reduce MEA cost not addressed.
- Approach is focused on developing low cost manufacturing processes. This approach may not meet all of the DOE targets without hybridizing the system.
- The approach is well thought out. In order to replace batteries in consumer electronics, a low cost manufacturing process is a necessity.
- Simplified system approach is excellent.

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

This project was rated **2.9** based on accomplishments.

- DMFC prototypes have been built by PI's company.
- Codes and Standards have been examined and modified.
- Need much more data, performance of cells AND details about mfg, e.g. QA and SPC.
- Codes & standards work gives a good impression, and the vision of the product is clear.
- This is difficult to judge because so little was said about technical challenges and what was being done to address them.
- Status against objectives was well-presented, but not well-explained.
- Reasonable progress given start date.
- Significant progress is evident in less than a year. Codes and standards development is important. However, a significant development effort is still required to achieve the project technical targets. Also a low cost manufacturing process still remains to be defined.
- Results to date very promising
- Annoying lack of detail on anything technical, other than the blandest platitudes. We need to know what the detailed needs are to address key hurdles.
- Just getting started.

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

This project was rated **2.9** for technology transfer and collaboration.

- PI works with many partners.
- The PI needs to discuss partners' roles and future work with greater detail.
- Very company and application-specific program.
- The synergies and connectivity to hydrogen systems need to be much better explained.
- Good collection of partners with vested interest in technology.
- Unclear if partners are contributing.

- Collaboration with other organizations is evident. Duracell provides an entry into the very large consumer battery market.
- High degree of collaboration.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- PI presented good road map for more changes in codes and standards.
- Good work plan for manufacturing improvements.
- PI needs to present future work plans of partners - none was discussed.
- While the objectives and performance metrics are clear, the pathway and work required are not.
- Need to define timelines and roadmap.
- MTI indicated that the specific fuel cell system design has not been finalized. Issues remain with regard to membranes, air and thermal management and fuel concentration. Technical targets have not been met requiring further development. Most of this development will apparently be done by their suppliers. Little or no details were provided. Emphasis in the presentation was on manufacturing studies.
- Good plan for company goals but little insight into how these align with needs of the fuel cell community.

Strengths and weaknesses

Strengths
- MTI is one of the sole American companies that manufacture DMFC systems.
- MTI has experience in this field.
- Product development and commercialization focus, especially with regard to codes & standards, is excellent.
- Collection of collaborators.
- Work to establish standards.
- Passive fuel delivery approach.
- Good teaming is evident with their manufacturing partners. Good cost share. Leading the way on codes and standards.
- Solid plan.
- Excellent work toward commercializing product.

Weaknesses
- PI needs to further discuss roles of partners - MEA is key design element and nothing was mentioned.
- PI needs a realistic view on true energy density and power density taking into account the whole package (including methanol) to be truly comparable to Li-ion batteries.
- Lack of explanation of synergy to DOE goals.
- No focus on MEA cost.
- Lack of timeline/roadmap.
- Lack of overall technical detail – talk was more of a marketing talk than a technical talk.
- Still a long way to achieve the technical targets. Not all of the objectives may be met.
- Why is DOE funding this? In a time of limited resources, DOE needs to maintain focus.
- What will the field learn? This project is simply pitched as a black box- shovel money in, we'll try to make a product but little insight provided into who, what, when, where, how or why.

Specific recommendations and additions or deletions to the work scope

- The presentations are supposed to be non-proprietary. It is better not to show poor resolution images than put them in the presentation and say they are proprietary.
- Roadmaps should detail how the required technical advancements are going to be achieved.
- It is not clear to the reviewer why the DOE should be funding this technology development.

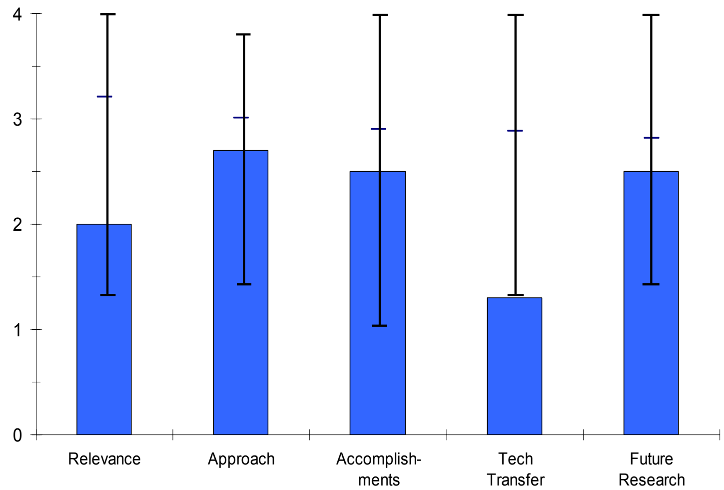
- As a further specific example for the low degree of potential benefit from economies of scale to be achieved from cell phone applications, it should be noted that even at a high sales volume of 1,000,000/year, the annual total production, on a power basis (which will be roughly proportional to total membrane area) would be around 450 kW (assuming a 450 mW device, per the 2007 projections in the presentation). This is roughly equivalent to a whopping 3 stationary power plants (150 kW per plant), or roughly 6 vehicle stacks (at ~75 kW/stack). Thus, the small-scale consumer electronics market would do next to nothing to develop the large-scale manufacturing processes and supplier base required for stationary and/or transportation markets for membranes, which is the ultimate goal and will be required to have any impact on energy consumption, petroleum imports, etc. Similarly, there is no overlap on balance of plant, and little, if any, overlap of manufacturing or assembly processes for the stacks themselves.
- Minimize the length and dollar commitment for this project. The company should be compelled to provide an intellectual contribution to the field. Collaborators should be named so that the key contributors at state of the art are identified.

Project # FC-33: DMFC Power Supply for All-Day True-Wireless Mobile Computing
Wells, Brian; Polyfuel, Inc.

Brief Summary of Project

PolyFuel, Inc. is developing a novel direct methanol fuel cell (DMFC) system for use as an all-day power source on a laptop computer. The general approach is to simplify the system as much as possible, particularly by reducing the overall size and complexity of the balance of plant. The heart of the project is the development of a next generation hydrocarbon membrane that will be engineered to best meet the requirements of the complete DMFC system. Work will be done to miniaturize and integrate the remaining balance of plant components into a package small enough to fit into a laptop computer.

Overall Project Score: 2.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.0** for its relevance to DOE objectives.

- DMFC (direct methanol fuel cells) fit fairly well in the hydrogen fuel initiative program.
- The fuel is methanol and not hydrogen.
- Consumers will be exposed to fuel cells pre cars and stationary applications, which is beneficial.
- Not clear which DMFC technology could translate to other hydrogen fuel cell initiatives.
- Critical for early adoption of fuel cells, but it's not clear if timing fits 2010 goals.
- Project is aligned with DOE goals for portable power fuel cells.
- It is difficult to make a case that this project supports the President's initiative, although there are some commonalities in membrane and catalyst areas. That said, successful commercialization of this technology would attract investment in fuel cell technology, which will have considerable benefit to the Initiative.
- The connection between small-scale DMFC's for consumer electronics and the future hydrogen economy (e.g., stationary and/or transportation) is not clear to the reviewer and was not well articulated.
- Synergies between DMFC's and stationary/transportation applications seem to be weak. Even if similar membranes are used, DMFC work does not appear to be advancing membrane technology, and the membrane volume even at high production levels for consumer electronic applications is miniscule compared with the volumes required for transportation (see calculations below). Thus, little stands to be gained from an economy of scale/learning curve standpoint.
- The DMFC technology is much nearer the market requirements for performance, cost, size, and weight than for other fuel cell applications. Thus, it seems to the reviewer that DOE funding of such applications is not required and will be undertaken by industry.
- While it is important to get some fuel cells out there, it is unclear how this helps toward any goals; technology is totally different from what will be used for any of the main applications.

Question 2: Approach to performing the research and development

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

- The PI and Polyfuel have recently developed a DMFC membrane and MEA (membrane electrode assembly) that demonstrates some strength -- lower MeOH cross-over.
- The PI has no experience building a DMFC system.

- The PI has no experience with electronics.
- System design – membrane interaction approach is well thought out.
- Approach to co-optimize the system strategy and membrane should lead to best overall solution.
- The approach being pursued appears to be a rational plan for building a DMFC system around the PolyFuel membrane platform.
- It is unclear that PolyFuel, as a membrane company, has all of the required competencies to carry out the work required to design and build a system that will be competitive in the market.
- The approach does not come close to the goal suggested by the title.
- The central technology here is a new membrane but the balance is weak, even if one accepts the implicit premise that the membrane can solve all problems.
- Desperate need for partnering.

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

This project was rated **2.5** based on accomplishments.

- Membrane work towards 1M MeOH feeds have been demonstrated - no work with neat MeOH.
- No work on real systems - only models.
- Reasonable progress with new membranes given start date.
- Have defined requirements for the power supply.
- Have developed preferred architecture and identified key membrane properties.
- Based on the way data were presented, it is difficult to assess the technical merits of PolyFuel's membrane platform. Compared to conventional perfluorinated membranes, the PolyFuel membranes have lower methanol crossover and electro-osmotic drag coefficients. However, no quantitative (or even comparative) data on conductivity or fuel cell performance was presented.
- Just getting started.

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

This project was rated **1.3** for technology transfer and collaboration.

- The PI needs to develop strong collaborations with electronic companies.
- No collaborations are in place.
- No collaboration with outside organization.
- At this time, project appears to be self-contained at Polyfuel.
- There is no evidence of external collaborations, except for interactions with vendors for specific components.
- PolyFuel appears to be going at this alone, which is extremely risky.
- PolyFuel should consider adding a team member with expertise in balance of plant and system design to augment the skills they are growing in-house.
- There is a desperate need for partnering to realize the goals of the project. The presenter did not provide a single detail about how they planned to get to a system via strategic partnering and the resources to get a system are not apparent.

Question 5: Approach to and relevance of proposed future research

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

- The PI should focus on their strengths - membrane and MEA development for DMFC applications.
- The PI does not appear to have the personnel or expertise to complete the proposed work on systems.
- Well defined- model work key.
- A lot of 'system' work for a 'membrane' company.
- The future work plan is sketchy, but includes the activities required to proceed with development of a DMFC system.
- Elements of a plan are there but the teaming required for rapid and efficient progress to end-goal is lacking.

Strengths and weaknesses

Strengths
- The PI has shown promising data on new membranes and MEAs for DMFC applications.
- Membrane technology.
- Passive fuel delivery approach.
- Membrane technology and expertise are the main strength.
- The PolyFuel membrane appears to be a good platform for a DMFC system.
- Solid plan.

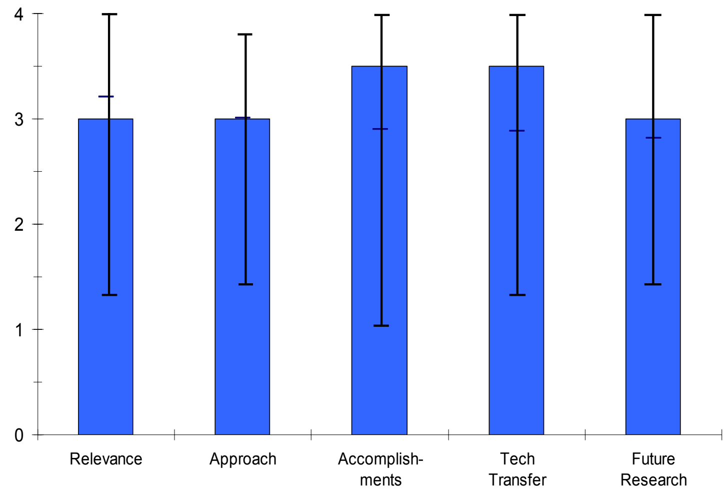
Weaknesses
- The PI has demonstrated no competency in system design or assembly of DMFC applications.
- No collaborations.
- Could probably benefit from collaboration with systems people or fuel cell developers.
- Lack of collaborators with a stake in the outcome of the project. If vendors are being requested to deliver specialized components, then it might make sense for them to be formal collaborators on the project.
- PolyFuel is going it alone on this project, attempting to develop a "holy grail" product in a market space where other companies (in Germany, Japan and Korea) are many years ahead in system development and have the manufacturing, marketing and distribution infrastructures already in place. If PolyFuel remains intent on going it alone, their chances for commercial success will be nil, regardless of whether they achieve technical success.
- Why is DOE funding this? It seems to be a project driven by a 'squeaky wheel' asking for funding rather than a contributor to the critical needs of the main points of emphasis for DOE.
- This group has long hill to climb to get to a compelling commercialization scenario.

Specific recommendations and additions or deletions to the work scope

- The PI should focus all of their attention on membrane and MEA development. Many questions in that arena still need to be addressed: 1. Durability, 2. Performance gains, 3. Ability to function in different MeOH concentrations.
- The PI should abandon DMFC system design unless a strong collaboration with an electronic company is formed.
- Would recommend they interact with some systems developers/modelers.
- This project requires real milestones with quantitative metrics, so that progress can be more accurately assessed.
- It is not clear to the reviewer why the DOE should be funding this technology development.
- As a further specific example for the low degree of potential benefit from economies of scale to be achieved from laptop applications, it should be noted that even at a high sales volume of 1,000,000/year, the annual total production, on a power basis (which will be roughly proportional to total membrane area) would be around 15000 kW (assuming a 15 W device, per presenters comments). This is roughly equivalent to a 100 stationary power plants (150 kW per plant), or roughly 200 vehicle stacks (at ~75 kW/stack), or about 0.001% of the vehicle market. Thus, the small-scale consumer electronics market would do little to develop the large-scale manufacturing processes and supplier base required for stationary and/or transportation markets for membranes, which is the ultimate goal and will be required to have any impact on energy consumption, petroleum imports, etc. The case for economies of scale/development of supplier base, is somewhat stronger for laptops than for cell phones (due to the larger power requirement for laptops), although still weak. Similarly, there is no overlap on balance of plant, and little, if any, overlap of manufacturing or assembly processes for the stacks themselves.
- Minimize the length and dollar commitment for this project.
- Put maximum effort into building a vertically integrated team.

Project # FC-34: Direct Methanol Fuel Cells
Zelenay, Piotr; Los Alamos National Laboratory

Overall Project Score: 3.2 (4 Reviews Received)



Brief Summary of Project

This Los Alamos National Laboratory (LANL) project on direct methanol fuel cells (DMFCs) currently targets development of novel catalysts for DMFC cathode with smaller particle size, higher activity, better methanol tolerance and lower cost (focus on both Pt-based and non-precious materials); improving DMFC stability by minimizing or eliminating major factors impacting long-term performance of the DMFC (catalyst and ionomer loss, ruthenium crossover, etc.); and design of membrane-electrode assemblies, both Nafion and alternative-membrane based, with superior performance and improved long-term stability.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Key objective for the President’s military readiness “electronic soldier."
- May support President's Initiative, but alignment to DOE goals not clear for electrocatalysis and membranes & MEAs.
- Identify goals/barriers to be overcome more explicit for electrocatalysis and membranes.
- DMFC may provide early commercialization opportunities which could mitigate industry disenchantment with distant hydrogen fuel cell.
- Clearly focused on R&D goals. Project is critical to realization of portable power targets.

Question 2: Approach to performing the research and development

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

- Excellent overall approach on each critical area of DMFC performance/durability.
- Approach for each area included in this project is not consistently clear.
- Lower-crossover membrane and durability are primary barriers and need to be addressed.
- Project utilizes expertise from a number other sources. In past year, project has added emphasis on industrial collaborations to capitalize on impressive lab achievements.

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

This project was rated **3.5** based on accomplishments.

- Catalyst improvement.
- Membrane evaluation/analysis/advancement.
- Achieved high specific power in stack assemblies, although stack work outside scope of this program.
- Close to achieving 2006 goals. Durability improvement remarkable: Only 15% loss in 3000 hrs.
- Progress to milestones very good, but direct linkage to DOE goals not clear for electrocatalysis and membranes/MEA research. Very good progress for stack performance for portable applications.

- Significant technical progress achieved on multiple fronts. A number of milestones achieved or surpassed ahead of schedule.

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

This project was rated **3.5** for technology transfer and collaboration.

- Significant improvement over 2004 – especially the addition of Mesoscopic Devices.
- While industrial collaborative efforts are being negotiated, not enough information is provided to determine if they will be enough.
- Excellent transfer through publication, sufficient breadth of collaboration partners, and licensing effort.
- Excellent collaboration on all fronts especially with academia. Probably could benefit from further industrial collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Agree on PI's suggestion of work on electrode structure and elucidation of best structure.
- Future objectives are appropriate to advance technology.
- Assuming advances in lower-crossover membranes, then methanol-tolerant cathode would become lower in priority.
- Strong work plan for future to address remaining DMFC barriers. Didn't discuss much on contingencies or alternate paths. Probably not much of an issue given the projects broad approach.

Strengths and weaknesses

Strengths
- Probably the highest "value" of return on research investment of any of the DOE supported programs: outstanding combination of fundamentals, and practical solutions to overcome limitations of the technology.
- Project focused on achieving milestones.
- Specific milestones identified.
- Excellent project. Well designed and managed with impressive technical achievement.

Weaknesses
- Level of funding commitment not on par with the needs of the community.
- Missing a strong modeling component to guide proposed electrode design.
- Linkage to DOE goals for electrocatalysis and membrane activities not clear.
- No significant weakness.

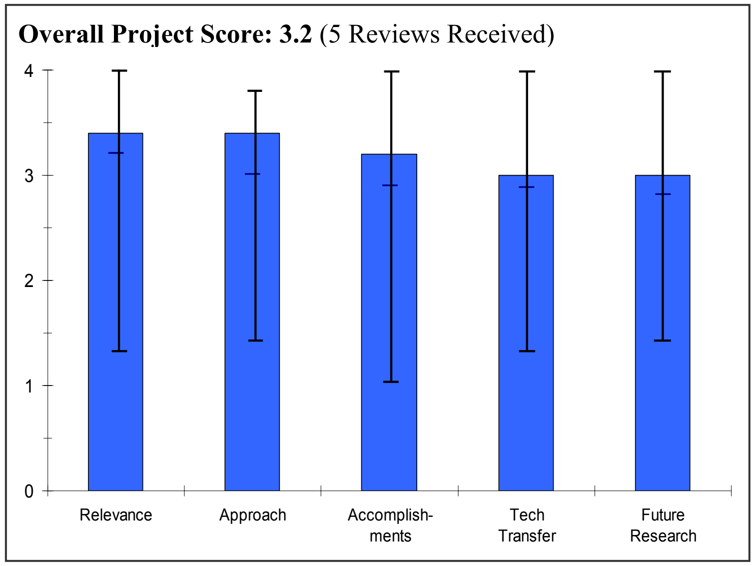
Specific recommendations and additions or deletions to the work scope

- Add off/on cycling.
- On the strategy for using concentrated MeOH: why stop at 5M? Pure MeOH should be the final goal to maximize storage energy density.
- Form alliance with modeling group to solve issue of electrode design: Professor Newman?
- Provide funding breakdown for each topic area.
- Clarify approach for each topic area and goals for those areas not already containing them.
- Consider further simplifications in water management.

Project # FC-35: Fuel Cell Systems Analysis
Ahluwalia, Rajesh; Argonne National Laboratory

Brief Summary of Project

For this project, Argonne National Laboratory (ANL) will develop a validated system model and use it to assess design-point, part-load and dynamic performance of automotive fuel cell systems. This effort is aimed at supporting DOE in setting R&D goals and research directions and establishing metrics for gauging progress of R&D activities. Objectives are to develop, document, and make available versatile system design and analysis tools, and to apply the models to issues of current interest.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- Validated system model.
- Gives important feedback of which areas to focus on and which combinations of stack operating conditions and BOP give the best system efficiency and flexibility.
- Effective simulation essential for (1) establishing proper targets (2) minimizing experimental hardware (3) minimizing test requirements.
- Did not compare adequately his anti-freezing strategy with other alternatives.

Question 2: Approach to performing the research and development

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

- Writing code, obtaining results and validating. Work on humidifier models excellent and long overdue.
- Covered both automotive and stationary needs.
- Validation of models with laboratory data is very important to continuously improve models.
- Appears good but little verification by comparison with actual data; virtually no explanation of models or simulation; without verification, results of complex processes are highly suspect.
- The objectives of this project should be updated since the DOE program has included the Technology Validation.
- This program should focus on understanding the data from the Technology Validation program on the fuel engine part of the vehicle and then develop generic models to evaluate it.
- Support DOE on goal setting and Metrics.

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

This project was rated **3.2** based on accomplishments.

- Experimental validation of humidifier models outstanding. Stack freezing/startup models broadened understanding. Hybrid/ fuel cell models comparison helpful.
- Very interesting data on humidification options and start-up from subfreezing.

- Utilization of the simulation has been rather limited. No convincing verification that results are valid (they probably are but no evidence is presented). This can be an extremely important tool but needs verification and much more utilization.
- Lack collaboration with NREL and other universities that are developing models of fuel cell automotive systems.

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

This project was rated **3.0** for technology transfer and collaboration.

- Excellent but could add OEM stack/system developers. Many excellent presentations.
- Validation of models with data from fuel cell developers is important.
- Primary collaboration seems to be with Honeywell. More collaboration with other developers is needed.
- The interface with NREL seemed weak. This relationship should be strengthened to help the program establish validity with model results.
- Responsive to last year's reviewer comments.

Question 5: Approach to and relevance of proposed future research

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

- Inclusion of CHP applications for future was a plus.
- Good plan; study of CHP applications will be helpful.
- Please continue to investigate good solutions for high-temperature membrane systems for atmospheric, low RH, 120°C membranes.
- Important issues are planned for investigation, but more verification and more collaborative investigations are needed.
- Weak future work proposal, the future work first priority item should be heavily focused on utilizing data from the Technology Validation program to bring validity to the vehicle-based model.

Strengths and weaknesses

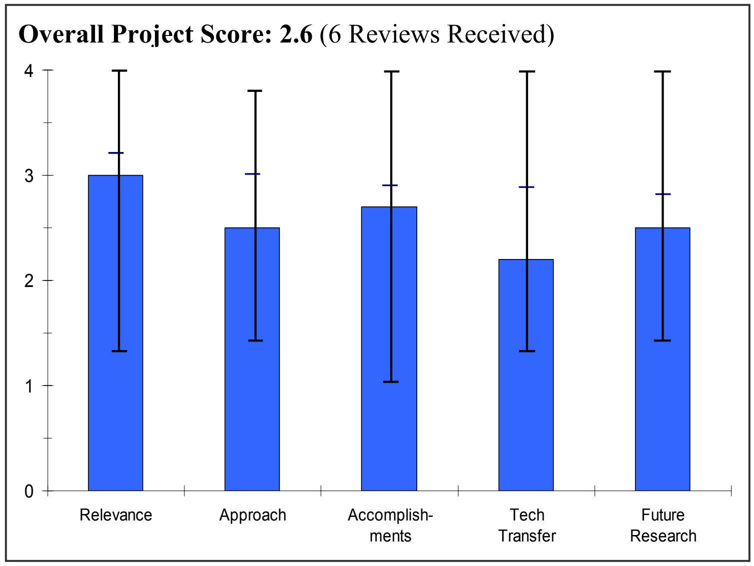
Strengths
- Solid approach with excellent validated results for many models.
- Good breadth of systems and applications studied.
- Good capabilities in developing models and simulation techniques have been previously demonstrated.
- Has a pretty good model to build on.
- The energy measurements give us a realistic overview of the FreedomCAR goal setting.
- Insulated stack with electrical re-heating appears to be an efficient strategy to prevent freezing. Start up only once every 30 hrs at -20°C.

Weaknesses
- Some models cannot be verified now because no complete system data are available.
- Have made little effort to establish credibility of simulation capabilities through verification by data comparisons.
- Have made little effort to explain models and simulation techniques (and corresponding limitations, which all models and simulations have).
- Model lacks real world vehicle data to validate system simulation results.
- Is there any collaboration with NREL on this particular topic or is this just overlapping work?
- Continue work on freeze-start of fuel cell systems.
- Continue collaboration with Honeywell on thermal and water management system.

Specific recommendations and additions or deletions to the work scope

- Incorporate stationary systems in models as another means of code validation for a real system.
- Suggest doing more modeling for 120°C operation to identify favorable stack hardware and operating conditions as well as critical BOP elements for both stationary and automotive applications. Suggest that model is based on best existing High Temperature Membrane (HTM) from current DOE contractors, as well as 2010 DOE HTM targets. Present results at HTMWG meeting for discussion and further input.
- Prepare and make available documentation to inform others of systems analysis capabilities (and limitations).
- Expand collaborations.
- Verify models with data where possible and note limitations where verification is not possible.
- I would like to see more collaboration between ANL and NREL on this topic.
- Continue work on freeze-start of fuel cell systems.
- Continue collaboration with Honeywell on thermal and water management system.
- The Technology Validation program provides critical real world data to ANL model yet I do not see a high priority to use such data to improve and build credibility with their model. This should be this program's first priority for this year and the coming years.
- Energy storage should continue to be an important part of systems analysis.

Project # FC-36: Bipolar Plate-Supported Solid Oxide Fuel Cell "Tuffcell"
Carter, J. David; Argonne National Laboratory



Brief Summary of Project

Argonne National Laboratory (ANL) is developing an improved solid oxide fuel cell (SOFC) for auxiliary power units and other portable applications that address SOFC startup time, durability to temperature cycling, vibration and shock resistance, and materials and manufacturing costs.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This project seeks to develop low-cost, SOFC bipolar plates with sufficient durability to meet DOE cost and performance goals.
- Approach could solve some long standing issues associated with SOFC's.
- The project fits directly in the hydrogen program. It is a revolutionary but ambitious proposal because of the technical barriers inherent between differences in the metal and ceramics properties.
- APU development seems to have a loose relationship to the Hydrogen Program. APUs will assist SOFC commercialization primarily in stationary applications but will also assist application to hydrogen production via electrolysis. Reduction of dependence on foreign oil does not appear to be a significant driver for this work.
- In particular, the advantage in cost of the presented design versus anode design was based on erroneous assumptions with respect to current SOFC anode supported technology. Typical anode supported designs are 0.3 - 0.5 μm and may also be fired in one step. The current < 0.5 μm anodes invalidate the cost comparison and the number of firing steps on a 24/7 basis using high quality industrial equipment is not significant even with multiple firing steps.
- Project supports goals and objectives of RD&D plans.

Question 2: Approach to performing the research and development

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

- The approach supports development of metallic bipolar plates to improve durability while minimizing the requirement for high-cost ceramics.
- The project started in October 2001. Technical barriers are expected because the PI must master the processing techniques of different materials to get adhesion between metal and ceramics, sintering, etc., at a relatively low temperature.
- The essential difficulty with a monolithic single fired stack is the yield. It is impossible to QA single cells after firing prior to forming the stack with this design. Given the series connection one poor or bad cell invalidates the entire stack. The yields of ceramic parts are a difficult problem and single fired stacks essentially make the issue untenable.
- Performance in a single fired stack (particularly large stack) is usually substandard due to the incompatibility of the multiple component firing schedules, i.e., different materials require different temperatures. In this case the multiple materials are exposed to identical conditions which do not optimize performance.
- The metallic support plate is ductile, therefore it must be thicker than a non-ductile ceramic to ensure twisting or out-of-plane deflections do not occur. The cost-benefit analysis of the much thicker metal versus ceramic is not addressed.

- Project has solid conventional approach focusing on technical barriers. Well received, strong emphasis on cost analysis up front. Project should follow through on future emphasis of improving single cell performance.
- Metal seal design is interesting. Approach does represent a cost saving over thick 1 mm anode-supported designs.

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

This project was rated **2.7** based on accomplishments.

- Good progress with cost analysis completed, 2- and 3-cell stack completed and tested and single cell tests on cycleability and start-up initiated.
- Improved seal design - now need to improve cell/stack performance.
- Stack test results are not satisfactory. PI notes progress in spite of difficulties and small amount of funding. PI must interact with SOFC developers.
- Given the fundamental limitations of the approach, reasonably good progress. Fifty percent complete. The performance results to date have been relatively poor. Low power density will add to cost of the stack as a first order effect. The demonstrated start time of approximately 80 minutes is not a significant improvement with respect to other SOFC designs that have been designed for high cycle applications. A < 30 minute start would be a significant improvement but this has not been demonstrated. Although a significant improvement, a < 30 minute start would not meet automotive requirements of < 1 minute.
- Solid technical accomplishments especially innovative cell design and self-sealing concept. Further progress needed on improving power density and overall single cell performance.
- 0.14-0.25 mW/cm^2 is too low. Needs more careful stack development.

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

This project was rated **2.2** for technology transfer and collaboration.

- Collaborators and interested parties participating in project. "Hope" to transfer technology.
- There is no indication about technology transfer yet. Must interact with SOFC developers.
- Korea Advanced Institute. Motorola. Idaho National Laboratory. These organizations have expressed an interest but are not actually collaborators.
- Project would benefit from increased collaborations especially at the basic research level to improve cell performance. Mentions collaboration with Korea Advanced Institute of Science and Technology but is not clear on the extent of collaboration.
- No evidence presented.

Question 5: Approach to and relevance of proposed future research

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

- A more detailed plan for future work is needed.
- Focus on improving cell/stack performance including long-term performance. Find ways to look at performance of individual components and interfaces (i.e., find sources of poor performance and focus on improving these).
- Technology transfer, scale-up and system integration, and stack demonstration are planned for the future.
- Based on the presentation, future effort appears focused on demonstrating the technology. This is very premature given the level of maturity presented. More development and proof-of-concept on a small scale is required.
- Solid conventional future work plan. Work plan would benefit from more details and explanation of potential paths to improving single cell performance with built-in contingencies.

Strengths and weaknesses

Strengths
- Good analysis and testing program underway.
- Novel stack design which could overcome some SOFC stack hurdles.
- Progress in spite of the difficulties of processing different materials to get adhesion between metal and ceramics, sintering, etc, at relatively low temperature, and small amount of funding.
- Solid project which has identified and shown feasibility of innovative SOFC cell design which can lower costs as well as self sealing concept.
- Metal supported SOFC may be important for automotive and APU applications. Approach does eliminate sintering steps.

Weaknesses
- Start-up time 10°C/minute is still slow.
- Poor cell/stack performance.
- Interactions with SOFC developers.
- Not clear if cell performance will be completely acceptable.

Specific recommendations and additions or deletions to the work scope

- Need experimental data showing improved stack performance.
- As previously outlined in this review, monolithic firing of complete stacks has fundamental issues, among the more important, meeting cost-effective yield requirements. The basic concept of co-firing a powder metal mixture and the ceramic on a cell level does have some interest. There are many technical uncertainties that will require development. Continued work should focus on optimization at the cell level.
- Monolith designs (co-fired) are difficult development efforts.
- Project would benefit from expanded collaborations especially at the basic research level to improve cell performance.

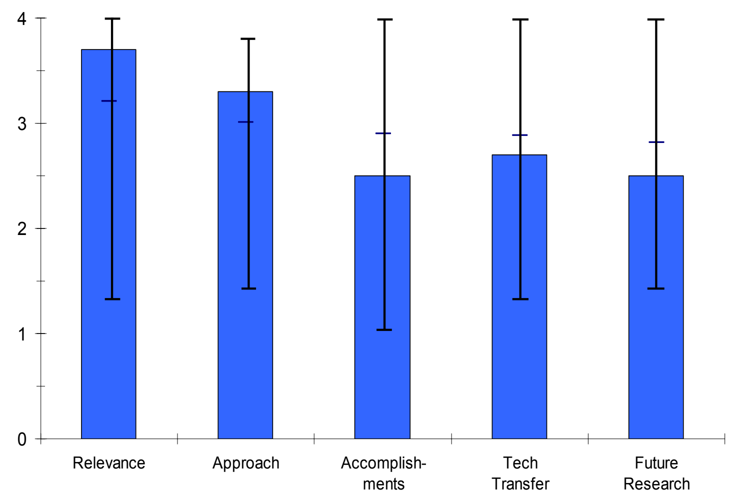
Project # FC-37: Effect of Fuel and Air Impurities on Fuel Cell Performance

Garzon, Fernando; Los Alamos National Laboratory

Brief Summary of Project

This task is focused on evaluating and alleviating the effects of impurities in the fuel and oxidant streams. The effects of fuel impurities such as hydrogen sulfide and air impurities such as sulfur dioxide and nitrogen oxides are specifically targeted in this study. The investigation includes the elucidation of potential mechanisms of action on fuel cell components. One important goal during this year is to develop a method for cleaning sulfur-poisoned catalyst layers.

Overall Project Score: 2.9 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- This program is important and will identify PEMFC operation in gas environments that are real (not clean and ideal).
- A better, and more widespread, understanding of these effects will certainly be beneficial.
- Diagnostic methods and instrumentation are critical to the development of Fuel Cell basic science development.
- Temperature and Humidity measurement is useful; it is questionable if a temperature sensor alone is useful.
- Diagnostic instrumentation and methods must be as nonobtrusive to the existing fuel cell as possible in this analytical development.

Question 2: Approach to performing the research and development

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

- The PI should consider putting more emphasis on understanding mechanisms and sources of impurities and less on recording functional responses (studying effects?) to various impurity challenges. Many sulfur-bearing impurities can originate in the membrane electrode assembly itself as decay products which can then poison the catalysts. These cannot be filtered out as easily as externally generated impurities.
- LANL has assembled a good team. ORNL provides critical post-test analysis.
- Evaluating both the effect of impurities and the possibility of recovery is good.
- Including both dry and wet conditions is good too.
- Focus on CO/NH_3/ H_2S /HCN reformer products should be rethought relative to the list of contaminants of concern found in the Hydrogen Fuel Quality specifications and in the JARI list of species. Not convinced that diesel fumes are that big a problem.
- I recommend the elimination of this aspect on the approach because this should be conducted by the fuel cell manufacturers.
- Find ways to mitigate negative effects of impurities.
- They should know their system best and how to overcome the discovered issues by this research.

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

This project was rated **2.5** based on accomplishments.

- Although a fairly new program, it was not clear how much of the results shown were really new or just revisiting prior work done at LANL (e.g. pulse method for cleaning catalysts, and impurity studies from several years ago).
- It appears the progress is slow and not consisted with the funding level.
- Showing that sulfur contamination is reversible is excellent, however an even simpler recovery process is possible, which is simply to expose the stack to air on both electrodes. No external power supply is required and no worries about individual cell voltages in the stack with no unusually high potentials on some electrodes.
- Would have been nice to show some foreign cation results as well (e.g., exposure to NaCl).
- The work on SO_2 should correspond to ambient concentrations of SO_2; most locations are not 1.5 ppm.
- Single Cell Test Protocol vital to success of fuel cell program.
- The progress on this research could be vastly expedited if the researcher spends more time on discovering impurities on fuel cells instead of looking for solutions to these issues that are not applicable in a system perspective or could potentially create more problems for the fuel cells.
- A proposal of 1.2 V per cell at a starting point of 0.4 A/cm^2 is just not realistic. Most fuel cell stacks have over 350 cells and the cells are at least 500 cm^2.

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

This project was rated **2.7** for technology transfer and collaboration.

- It would be beneficial for this project to become more closely aligned with one or two fuel cell companies who have been acquiring information on failure modes analysis to learn what kinds of impurities they have experience with.
- Better interaction should be possible at these funding levels.
- Not totally clear how these results will be disseminated to the industry. Will a recommended fuel specification be the end result?
- Strengthen ties to the FreedomCAR Codes & Standards Technical Team (CSTT). Review the project work at one of the CSTT meetings. Also review this work with the FreedomCAR Hydrogen Storage Tech Team for their input
- UTCFC and Ballard should be involved in this process. Are they?
- Is there a joint effort with the work at the University of Hawaii?
- Where is the collaboration with the Codes and Standards and Hydrogen Storage Tech Team on the impurities effects?

Question 5: Approach to and relevance of proposed future research

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

- The current plan is directed at studying the degree to which external impurities affect current catalysts and other MEA components. But if filtration media can be used to minimize these effects, that should be considered as a mitigating strategy also, rather than changes to the MEA components to try and make them more robust in that respect. The internally generated impurities, from membrane decomposition to bipolar plate corrosion are more difficult to deal with and identify, but potentially as important. Their future work should include a plan to address these types of impurities.
- Based on the results thus far, the future work should emphasize the most critical life factors.
- Future plans are not clear.
- I am not convinced that diesel fumes are that big a problem. Do a rough assessment of how real this potential is before devoting a large fraction of resources to it. Sulfur poisoning model development and mitigation good.
- Solid proposal, collaboration with industrial partners and the eFreedomCAR Tech Teams is highly encouraged.

<u>Strengths and weaknesses</u>

Strengths
- Great capabilities and expertise.
- Excellent choice for a DOE-sponsored research. These effects are of industry-wide interest. Conducting tests under both dry and wet conditions ensures that different types of fuel-cell systems are covered.
- Engagement of US Fuel Cell Council is extremely important to the success of this project.
- Developing a good knowledge database on impurities and their effects on fuel cells; this is very important to the overall fuel cell program.
- Future work proposal is solid, but could lack the collaboration with the industrial partners.
- A very important problem.

Weaknesses
- Complete deliverables of this program are not clear. What is the complete list of impurities that are planned? (Changes in plans are understood to occur and are OK, but it is difficult to provide feedback on the plan when the plan is not clearly stated).
- Will fuel and air specifications be a deliverable? Will prevention methods (e.g., filters), as well as recovery methods, be included?
- I am not convinced that impurities from reformers are the highest priority species to be evaluating.
- Many sulfur species (not H_2S) are present in natural gas.

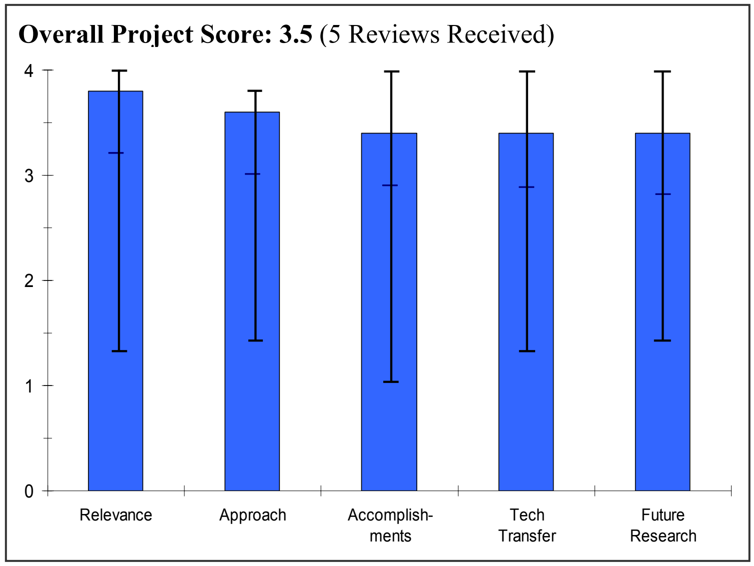
<u>Specific recommendations and additions or deletions to the work scope</u>

- Broaden the scope to include internally generated impurities that can and will result under the stressful automotive operating conditions with recycled water. Understanding the magnitude of those impurity generation levels and dependence on operating conditions will provide a sound basis for determining where improvements in robustness need to be made.
- Focus on gaseous contaminants. Delay studying the effects of salt.
- Attempt recovery from sulfur poisoning by simply exposing both electrodes to air while the cell is still hot. (Although recovery may not always be complete, sufficient recovery may be obtained).
- For each impurity, an analysis should be made using a transportation lifetime target (e.g., 5,000 to 8000 hours) of the maximum exposure of that impurity that can be tolerated without significant performance losses (e.g., > 5%). This could result in some very useful deliverables, for example: 1) Is NaCl in the air in coastal areas an issue for automotive applications, e.g., is mitigation methods (e.g., filters) required or not? 2) Recommended hydrogen purity specifications for transportation applications.
- Consider including the evaluation of the effectiveness of mitigation methods (e.g., filters) for each class of impurity.
- Develop complete list of contaminants, prioritize them, develop a test protocol to evaluate them, carry out the actual testing, and develop a mitigation strategy. Should at least include also formaldehyde, formic acid, and non-methane hydrocarbons (NMHC). This project work should be included in an overall Hydrogen Fuel Quality Specification project tracked under the Codes & Standards Tech Team.
- I would like to eliminate these technical objectives for this program (1) Find materials and devices able to mitigate negative effects of impurities, (2) Develop test procedures for reactivating electrodes poisoned with sulfur compounds. I believe that this is not the best use of National Lab's resources. I would like to see a focus on finding problems that we are not aware of and the mechanism of these problems. Leave the finding of applicable solutions to the fuel cell manufacturers. Please focus on identifying real world impurities which have an adverse effect on fuel cells with industrial and other National Lab partners.
- Need to extend contaminates to PM10 particulates and trace metals.

Project # FC-38: Neutron Imaging Study of the Water Transport Mechanism in a Working Fuel Cell
Arif, Muhammad; National Institute of Standards and Technology

Brief Summary of Project

This National Institute of Standards and Technology (NIST) project is intended to develop an effective neutron-imaging-based, non-destructive diagnostics tool to characterize water transport in PEM fuel cells. Objectives include: providing research and testing infrastructure to enable the fuel cell industry to test commercial grade fuel cell flow field designs; training industry to enable them to use the imaging facility independently; and transferring data interpretation and analysis algorithms/techniques to industry to enable them to use research data more effectively and independently.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- New device will be much more conducive to real fuel cell experiments than current device. Potential for 3D imaging is exciting. Imaging has to be combined with other analytical techniques to characterize water management.
- Understanding basic mechanisms of fuel cell operation.
- Very useful diagnostic and tool enabling further development MEAs and stacks.
- Thermal and water management are key issues for the successful commercialization of fuel cells.
- Diagnostic methods and instrumentation are critical to the development of Fuel Cell basic science development.

Question 2: Approach to performing the research and development

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

- A technique-specific project has to make sure it is applied to right questions and gives answers that are useful. Water transport is a key issue but technique has to be combined with other studies/models to be really useful.
- Fundamental approach to understanding movement of water in plates and membrane.
- Potential for understanding hydrogen storage.
- Unique capability.
- The approach is uniquely qualified for probing MEA/Stack dynamics in real-time.
- Test commercial grade fuel cell for water management.
- Provides infrastructure and facilities for testing fuel cell.
- Evaluate impacts of research.
- Transfer technology.
- Continuous improvement of capabilities of neutron imaging- a very dynamic field- very sensitive to water formation- can look through 10 - 50 microns of water.
- Added real-time imaging in 2004- 30 frames/sec.
- Building a new, larger system in '06.
- Proprietary work now 70%, want to move to 50%.

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

This project was rated **3.4** based on accomplishments.

- Real time imaging is crucial to studying transient effects. Need to better pin down location of water through the thickness of the cell (membrane/GDL/flowfield).
- Taking independent initiative to improve capability and resolution for larger stack samples.
- New 3D imaging capability is very promising.
- Measured water content as a function of the polarization curve.
- Are developing new approach to disconsolation and reconstruction to do 3D imaging of the fuel cell system.
- Direct measurement of GDL water uptake.
- Are publishing 4 papers this year.
- Are increasing their work with modeling.
- By end of year, increase resolution to 25 microns; in two years, down to 10 microns.
- Faster progress will only help the industry. Please allocate more resources to make this enabling technology available to OEMs and suppliers faster.

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

This project was rated **3.4** for technology transfer and collaboration.

- For this to be really successful, the technique developers will have to do much more than just provide training on operation. They will have to go at least part way in understanding the real questions.
- Extensive list of participants and individual projects.
- Publishing reviewed papers.
- A lot of work with outside institutions.
- Good collaborations; training efforts particularly good.
- Should promote more publications from users.

Question 5: Approach to and relevance of proposed future research

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

- The potential for 3D imaging is very important to utility of the technique for water transport issues.
- Potential for 10 micrometer capability is substantial improvement.
- Real time imaging in 3D should dramatically increase understanding.
- Setting up new imaging unit is critical to success.
- Will complete new facility in Fall 2005.
- Have great plans to expand the usability of this tool.
- Will be able look at laminar flow in all levels of fuel cell stack at once.
- Three dimensional analysis is critical for stack analysis.
- Two dimensional measurement of a single cell is limited. Please make 3-D available ASAP.
- Is it possible to provide a vertical beam for horizontal analysis of fuel cell in operation?

Strengths and weaknesses

Strengths

- Unique experimental capabilities.
- Unique test facility and imaging know-how.
- Took initiative to bring technology to fuel cells.
- Teaching others to use the technique/equipment.
- Unique facility that produces valuable data unavailable anywhere else.
- Continuous improvement increases the resolution and analytic capabilities.

- Good collaboration with industry, national labs and universities.
- Great analysis tool for the industry.

Weaknesses

- Fuel cell researchers may not understand how best to utilize capabilities. NIST analytical experts may not understand what the real questions are.
- Need more exposure.
- Limited publications.
- Need to continue to make results of their work publicly available - need to make steady progress towards 50% public research goal.
- Should provide resources to supply hydrogen and oxygen if possible to small stack analysis.

Specific recommendations and additions or deletions to the work scope

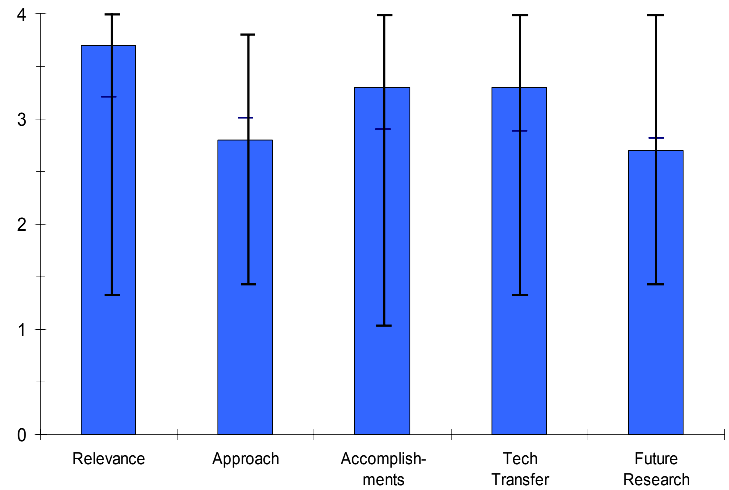
- Additional financial support of planned improvements.
- I would like to see more National Labs and Universities involved in this diagnostic process.
- Please commit enough resources to implement the full capability of this diagnostic instrument for the industry and others to use.

Project # FC-39: Microstructural Characterization of PEM Fuel Cell MEAs
More, Karren; Oak Ridge National Laboratory

Brief Summary of Project

Oak Ridge National Laboratory (ORNL) is focused on gaining a better understanding of membrane electrode assembly (MEA) degradation mechanisms, including structural and compositional changes as a function of MEA processing, correlation of microstructure with performance, and morphological changes occurring during MEA aging/use. In addition, ORNL will collaborate with PEM fuel cell developers and manufacturers to evaluate MEAs using advanced microstructural characterization techniques and provide feedback for MEA optimization.

Overall Project Score: 3.2 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- Development of methodologies and fundamental work to enable evaluation of factors affecting the durability is highly relevant.
- Providing fundamental diagnostic tool to evaluate MEA durability.
- This work requires extraordinary analytical techniques. Results are critical for PEMFC success.
- Characterization of the structural and morphological changes of the MEAs components caused by aging is important to find ways of improving fuel cell durability.
- It is important to understand degradation in PEM systems, and this project makes sense on that pathway.
- Very relevant to durability.
- Impact could be much greater if scope were wider.
- Need to work on industry-standard materials.

Question 2: Approach to performing the research and development

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

- Good progress in technique development, approach is well-thought out and expanding on the ways to evaluate important factors such as uniformity of 3-phase structure.
- Solid techniques developed.
- Need more effort to determine decay mechanisms.
- Proper analysis and interpretation of post-test results are the primary focus.
- The approach is adequate to accomplish the proposed tasks.
- Emphasis is more based on analytical methods than on solving fuel cell problems. This is OK for *first year*.
- The approach is a solid research angle.
- The lack of statistics is most distressing.
- Need to work on industry-standard materials; LANL materials may provide different results than those of 3M, DuPont, and Gore.

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

This project was rated **3.3** based on accomplishments.

- Excellent progress in comparing Pt crystallizable size for newly prepared MEAs and after cycling test conditions.
- Very good images.
- The ORNL staff has shown their expertise in this area. There are very few labs in the world that compare.
- Very significant results were obtained, showing the important changes of the MEA structure after cycling and log term fuel cell operation.
- The tools are good. There needs to be a more focused set of problems. For example, the study of the influence of one variable on the structure effects.
- Within the bounds of concerns about relevance, this work provides some truly interesting clues into the nature of catalyst layers.
- Too limited in number of results. Did they spend all of the resources for an entire year studying a single MEA, as implied by the talk?

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

This project was rated **3.3** for technology transfer and collaboration.

- Broad interactions with other national labs and industry.
- Working with several fuel cell companies.
- ORNL and the developers work in close co-operation.
- Need to team with those who are working with commercial MEA designs. LANL materials may not be relevant.
- It seems as if some partnerships are forming with industry.
- Needs a method to communicate results more than once per year if National Lab is to fulfill their support mission.

Question 5: Approach to and relevance of proposed future research

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

- Good plan for non - and proprietary research. Continue good work. Add statistical data and imaging analysis software.
- No info provided except working with industry.
- Need to establish experiments to elucidate decay mechanism.
- ORNL has an excellent plan for FY 2005 and FY 2006.
- There are no specific goals regarding the search of other causes of fuel cell degradation.
- Planning is not well-communicated, other than continue on the path.
- How do the workers intend to get beyond observations to provide broader insight?

Strengths and weaknesses

Strengths
- Collaboration with industry.
- ORNL staff is very skilled in the microstructural analysis.
- The sample preparation technique and the catalyst mapping.
- Intriguing results make this effort worthwhile.

Weaknesses

- Few publications on results.
- Not all mechanisms of fuel cell operation are considered.
- The electrodes shown were very heterogeneous. Most likely they are not reproducible. It is hard to make much sense out of looking at a small section of a heterogeneous sample. There needs to be some statistical analyses techniques that can address this.
- A combination of “Oswald” ripening and particle migration is a well known problem with PEM fuel cells.
- Critical need to extend the work to include MEAs made by industry standard approaches. Also, the researchers need to provide context for their work to go beyond the purely descriptive approach.
- More than a few samples per year should be studied, even with somewhat limited resources.
- Not much bang for the buck.

Specific recommendations and additions or deletions to the work scope

- Incorporate image analysis software for collecting statistical data on Pt particle sizes and distributions before and after aging.
- Use of other analytical techniques may allow characterization of chemical changes in the membrane.
- There needs to be more care in experimental planning. It is very important to study both new samples and then samples which have been VERY CAREFULLY tested.
- This work provides insight through pictures but there simply needs to be much more of it and an effective method provided for rapid communication to the field. National lab work is often good but poorly disseminated, and not in a timely way. The labs should be putting results out in near real time, regardless of their claims of IP. The results should be available to those who make the technology real. The lab insistence on keeping key results quiet slows progress of the entire field.

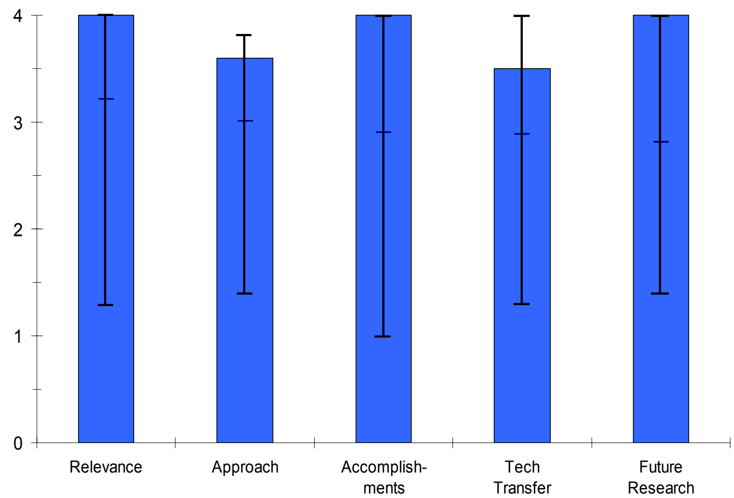
Project # FC-40: PEM Fuel Cell Durability

Borup, Rodney; Los Alamos National Laboratory

Brief Summary of Project

In this project, Los Alamos National Laboratory (LANL) will identify and quantify factors that limit PEM fuel cell durability by measuring property changes in fuel cell components during long-term testing (membrane-electrode durability, electrocatalyst activity and stability, gas diffusion media hydrophobicity, bipolar plate materials, and corrosion products) and developing and applying methods for accelerated and off-line testing.

Overall Project Score: 3.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **4.0** for its relevance to DOE objectives.

- PEM durability fundamentals, understanding and improvement.
- Determining decay mechanisms.
- Very relevant work to understanding the issues related to automotive duty cycle operation.
- Durability, cost and performance are the main barriers for PEMFCs.
- Understanding of degradation mechanisms is key to improving durability.
- New tools for understanding degradation are right on target.

Question 2: Approach to performing the research and development

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

- Basic no-nonsense experimental approach combining results from different tests as a means to achieve understanding of impact on durability.
- Good view of the big picture.
- The approach is common with that being used by many other groups in the industry who are working on this problem. I hope that the approach used at the National Lab will emphasize mechanistic understanding and not just materials screening. Authors are also working on many different aspects of MEA durability; membrane degradation, catalyst durability (C corrosion and Pt dissolution) and GDL durability. Do they have resources to tackle all of these issues? I would rather they focus in one area and gain a deeper understanding.
- Very comprehensive *in* and *ex situ* testing.
- This work is a great beginning.
- LANL's small team needs to focus on getting understanding of specific parameters. Hopefully, next year some definite, well-planned and documented tests will be presented.

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

This project was rated **4.0** based on accomplishments.

- Many learnings identified for real world operating conditions: effect on catalyst growth, carbon corrosion, thinning etc.
- Hydrophobicity impact especially important for automotive and stationary systems.
- Investigated many parameters important to durability.

- A lot of good early results were shared. The PIs have been able to rapidly develop accelerated test methods for a number of key degradation mechanisms in duty cycle operation. Can they now use these tests to develop an understanding of the degradation mechanism and predict materials solutions and lifetimes?
- Important conclusions drawn from well-documented data.
- These results point out the complexity of fuel cell operation.
- There needs to be effort spent on non-uniform temperature during fuel cell operation.
- There also needs to be understanding of the uniformity of current density during fuel cell testing. Fuji Electric showed good data that PEM fuel cell operation resulted in "hot spots" which were transitory. LANL needs to understand the effects of this non-uniform current density. Measure temperature in the plane of the MEA.

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

This project was rated **3.5** for technology transfer and collaboration.

- Good presentation list. Could have more interactions with OEMs.
- Interacting with many institutions.
- Numerous collaborations were reported. Can LANL approach the automotive OEMs to get better insight into the expected fuel cell duty cycle operational conditions? How will these conditions influence what they have already learned?
- Synergistic team.
- Need to work with suppliers who have SOA MEA products, and not really spend much time on poorly-documented samples.

Question 5: Approach to and relevance of proposed future research

This project was rated **4.0** for proposed future work.

- Needs more work on GDL and hydrophobicity.
- Experiments are planned to evaluate the key durability variables.
- Focus on the mechanistic understanding.
- Good plan.

Strengths and weaknesses

Strengths
- Solid laboratory approach with excellent analysis of results.
- High confidence in applicability and validity of results.
- Team expertise.
- Evaluating key parameters in systematic way.
- Solid understanding of fundamentals.
- Strong team.
- Excellent beginning to identify degradation mechanisms. Historically all experiments like this (how fuel cells change with time) show extensive catalyst migration even though the devices continue to operate, more or less, well. Good tools are in place.

Weaknesses
- Expansion to stationary applications and requirements (40,000 hours) would be a plus.
- Additional interaction with stack and system OEM's a benefit.
- Have yet to address ways to improve durability.
- This is a classical "many-variable" problem. LANL needs to focus on specific problems and work with very well-characterized electrodes. Perhaps they need to build (or buy) MEAs that have very well-characterized properties and study them as model systems. Many variables, all connected. Need to get through this with very carefully planned experiments.

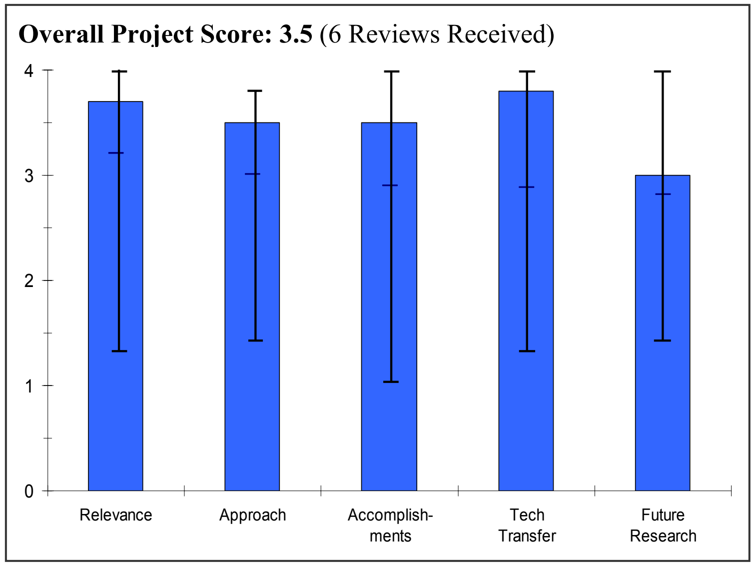
Specific recommendations and additions or deletions to the work scope

- Continue as is or expand if possible.
- Proposed future work looks good.
- Study temperature effect of electrode carbon corrosion at low relative humidity (especially at 120°C).
- Investigate Pt sintering as a function of Pt loading on carbon support.
- The DOE should seriously consider establishing a "fuel cell diagnostic center" built on the foundation presented here. This would be a user facility where developers would come (with their products) and learn both what they have made and how their products can be improved. This would be parallel to a similar center recently announced by the Japanese government to serve their fuel cell industry. This center should take a major responsibility in scripting test procedures.

Project # FC-41: Sub-Freezing Fuel Cell Effects
Bryan Pivovar; Los Alamos National Laboratory

Brief Summary of Project

This Los Alamos National Laboratory (LANL) project investigates start-up and survivability at sub-freezing temperatures of fuel cells, issues known to be a concern in fuel cells. This effort consists of hosting a DOE-sponsored workshop to address research needs for the freeze issues and experimental investigation of freeze effects on fuel cells and components. The result of this work will lead to a better understanding of the limitation of fuel cell systems due to sub-freezing temperatures.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.7** for its relevance to DOE objectives.

- Project initiated to address specific DOE need.
- This project is a great example of how the National Labs can support the industry.
- Critical to understand and quantify.
- A better, and more widespread, understanding of these effects would certainly be beneficial.
- The project is relevant to the DOE hydrogen and fuel cell program as it seeks to understand the fundamentals and quantifying effects of freezing on fuel cell durability and operability.
- Project generates useful fundamental knowledge and will lead to improved public-record perspective on freeze issues.

Question 2: Approach to performing the research and development

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

- Basic approaches are still under development but the work already done is very sound.
- Organization of workshop is applauded.
- Ambition to assess much wider range of materials is appropriate and necessary.
- Expert personnel and state-of-the-art facilities are well-aligned to the topic.
- Should add structural issues: impact of frozen water on membrane.
- Starting with a workshop to survey the community about the issue was an excellent idea and resulted in a good starting point.
- Good identification of the barriers/challenges.
- The approach utilized the workshop format to discuss the research needs for developing a fundamental understanding of the effects of freezing on fuel cell component performance and defining precompetitive research efforts.
- Approach will be clearer once the R&D roadmap is developed.
- The workshop appears to have functioned well.
- Survey of previous public-domain freeze work is thoughtful and appears to have been thorough.

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

This project was rated **3.5** based on accomplishments.

- Combining workshop findings with internal work has made an excellent start. Need to do more thermal cycles (>100) to look for high cycle failure modes.
- It's early in the program; need to work with industry to ensure test matrix reflects the full spectrum of materials being used by developers.
- Standard test protocols for components would be very helpful.
- Initial characterization work should provide additional insight into path forward.
- Good start to working on fundamental understanding, which should continue to be the focus of this program.
- Organizing and managing the workshop was the most significant accomplishment. LANL quantified membrane electrode interfacial resistance over time. LANL did not see performance loss until temperatures decreased to -80°C whereas others have reported performance loss at temperatures below about -30°C.
- Some good initial fundamental studies of water in membranes as function of subfreezing temperature.
- Initial studies of level of sensitivity of single cells to freeze-thaw conditions help to put limited data from literature into context.
- Very good start in the field helps to establish the appropriate size of the further commitment of resources.

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

This project was rated **3.8** for technology transfer and collaboration.

- Have incorporated knowledge of large number of investigators.
- Workshop demonstrates commitment to working with widest range of stakeholders, must maintain this.
- Too early.
- Workshop was an excellent means of initiating collaborations, as well some transfer of current knowledge/beliefs on this subject.
- Collaboration with other organizations in the workshop was good.
- Workshop seems to have drawn productively a bit of data out of the woodwork and brought together diverse opinions for consideration.

Question 5: Approach to and relevance of proposed future research

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

- One additional study that should be added is changes in low temp behavior of aged fuel cells.
- Creating an economical test matrix representative of materials and components spectrum and operating protocols is a tall order, but LANL is a top candidate for the job.
- Impact of GCL needs to be included. Neutron imaging would be extremely valuable.
- Good and clear presentation of future plans.
- Future plans were a little vague. They may become more focused when the R&D roadmap is developed.
- Future plans not given in great detail, as perhaps is appropriate with initial results not showing major evidence of problems as serious as are clearly present in other durability areas.

Strengths and weaknesses

Strengths

- Ability to draw on both internal and external knowledge.
- Technical team expertise and leadership.
- Consensus of issues through workshop.
- Ambitions to elucidate technical issues associated with freezing.
- LANL has the resources.
- Excellent choice for a DOE-sponsored research. These effects are of industry-wide interest.

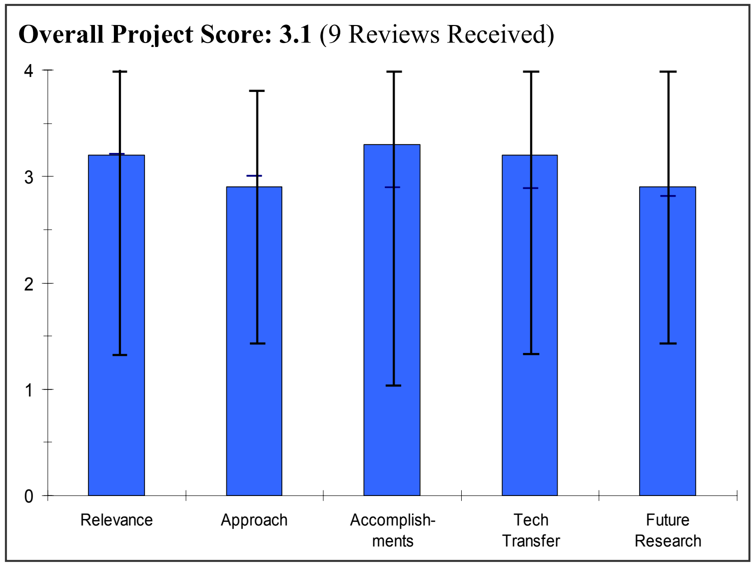
- This presentation put freeze issues into a well-considered perspective. Carefully-considered project selection in concert with that perspective would help to assure a positive return on the investment of appropriately-scaled federal resources.

Weaknesses
- None yet.
- None identified.
- Implicit assumption that a major federal investment in freeze research and development activities would necessarily be highly productive.
- Take care that one doesn't *a priori* assume that a phase change of water in a membrane will necessarily cause problems (other than the clear additional heat needed to melt the phase).
- Not all automotive OEMs actively contributed to the workshop. Further attempts at persuasion might have been helpful.

Specific recommendations and additions or deletions to the work scope

- Add neutron imaging.
- Continue to engage industry, especially those working on automotive applications, because this is a rapidly evolving area and perceived issues may change, as well as willingness to disclose concerns/results.
- Tough to stay away from engineering design considerations that are considered proprietary. Program needs to be integrated with the outcome from the workshop and remain focused on fundamentals.
- Be prepared to shift activity to other areas if fundamental studies here and in other projects, plus developer experience, suggest that freeze issues do not pose as pressing challenges as other durability or cost issues.
- Make sure that the project does not devolve into running the same tests on an infinite series of membrane materials and diffusion media. But be flexible and prepared to jump in if significant freeze issues do irrefutably arise.
- Continue to keep in mind the boundaries between pre-competitive research and competitive development. Put the federal dollars in the areas where they can best advance the technology.

Project # FC-42: Fiber Optic Temperature Sensors for PEM Fuel Cells
McIntyre, Tim; Oak Ridge National Laboratory



Brief Summary of Project

The objectives of this Oak Ridge National Laboratory (ORNL) project include the development of small and rugged fiber optic temperature and humidity sensors for monitoring critical fuel cell operational parameters, development of a measurement platform that enables a multi-point measurement capability for mapping conditions throughout the fuel cell, and utilization of these diagnostic tools, in concert with fuel cell developers, to validate design models and operational strategies to maximize performance. Furthermore, coupling of these measurements to miniature, non-optical methods for analyzing gas composition within the membrane region will provide the fuel cell community with a performance analysis test bed to support future design and development efforts.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Sensors are a critical need in order for fuel cells to meet DOE performance targets.
- Project is relevant to the DOE Hydrogen Program once its objective is to provide measurement and diagnostic tools to fuel cell developers for system performance optimization and model validation.
- This effort has high potential for significant understanding of fuel cell operation inside stack/cells.
- Real-time intra-fuel cell transient temp diagnostics.
- Diagnostic sensor development can be used for insight into internal fuel cell processes which can address performance and durability.
- Diagnostic methods and instrumentation are critical to the development of Fuel Cell basic science development.
- Temperature and humidity sensors are useful; it is questionable if a temperature sensor alone is useful.
- Diagnostic instrumentation and methods must be as nonobtrusive to the existing fuel cell as possible in this analytical development.
- Sensors are not critical to the realization of President's Hydrogen Fuel Initiative, but will provide useful information during the development and use of fuel cells.
- The program develops analytical tools to better understand the characteristics of fuel cells.

Question 2: Approach to performing the research and development

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

- Five-sensor platform developed and in testing in-house and at industrial partner with intra-cell temperature, pressure and species concentration measurements.
- Project embodies good mix of sensor development, experiments, and analysis.
- Developed luminescent sensor.
- Develop microcapillary for species measurements.
- Investigator's emphasis should be more on the diagnostic development than on application to the in-house developed stack.
- Requires more validation that readings are accurate (especially RH).

- Need to consider how this method can be applied to thin bipolar-plate-type fuel cells. Carbon-based plates are not for automotive applications for much longer.
- Project objective not entirely in agreement with outlined technical barriers and targets. Project objective is directed toward fuel cell developers, while the target includes not only lab development but operating fuel cells in automotive applications.
- If lab sensor is the target, then approach is logical.
- Very creative approach.
- The temperature-measuring device will open a greater understanding of fuel cells.
- Not completely sure chemical probe will give real-time data.

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

This project was rated **3.3** based on accomplishments.

- Excellent progress with integrated sensor platform under test at two locations. Meaningful data being collected and analyzed.
- Results to date provide unique insights into the dynamics of an operating fuel cell.
- Demonstrated free space measurement.
- See transient thermal operations.
- Can ID water activity and some species measurements.
- Temperature dynamics quite slow - 1 to 10 minutes.
- Interior temperatures can be higher or lower than manifold temperature.
- Spatial distribution valuable - but not enlightening.
- Some localized humidity during cooling - good correlation between RH and cooling.
- Could identify local limiting processes.
- Sensors appear to be sufficiently developed to be useful tools.
- Developing a good understanding of measurement results and confidence. This is good.
- Maintain focus on further developing the tool and refraining from investing too much time on testing for characterization of fuel cells under various conditions.
- Good progress.
- Response time for sensors is good.
- Clear demonstration of temperature and species determination through use of the sensors.
- No comparison with currently available technology.
- Project scheduled to conclude in 2005. Not apparent that targets will be achieved.
- The results are outstanding and very creative science.

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

This project was rated **3.2** for technology transfer and collaboration.

- Excellent collection of partners and potential users including major auto manufacturers.
- This project has an excellent fuel cell OEM onboard. Efforts to broaden OEM base will help to spread knowledge and ensure that results are not fuel cell design specific.
- Improved number of research partners - it could always be better, but attracting GM is important link to transportation applications.
- Work with outside experts to develop test plans and analysis of results. Suggest LBNL and LANL programs, amongst others, which are focused on durability and performance and that have complementary spatially distributed diagnostics.
- Large number of collaborative partners.
- Partner contributions unclear.
- Work has been disseminated through presentations/publications within the last year.
- Results are well-published.

Question 5: Approach to and relevance of proposed future research

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

- Good plans for future work.
- The PI identified an excellent set of future development ideas. These need to be better focused in the next year to prioritize future experiments.
- Move to measuring anode and cathode separately.
- Could identify rate-limiting processes, e.g. blockage of active sites.
- May have insight into next level performance- but did not clearly identify what is done next nor how they were to interact with their new partner - GM.
- Concentrate more on diagnostics: including reduced size, perhaps RH transient response (time constant was not stated).
- Investigate potential intrusive impact of the diagnostics.
- This topic should be eliminated from your future work objectives.
- More controlled operation variations (drive cycle) to identify origins of local efficiency limitations.
- Reasonably focused work.
- Although cost may not be a significant barrier, it is in the target description and should be stated.
- Discuss methods to demonstrate accuracy of measurements.
- Agree with approach and look forward to next year's results.
- Application to stacks is great idea.

Strengths and weaknesses

Strengths

- Strong and supportive team.
- Relevance and approach.
- The project has provided unique and valuable insights into the interior dynamics of an operating fuel cell stack. Project facilitates analysis of the chemistry and physics of the electrochemical reactions within an operating fuel cell.
- Has the potential for showing how to overcome issues with MEAs and show the path for improving fundamental fuel cell efficiency.
- A nice diagnostic tool has been developed.
- Developing good analytical instrumentation which will be useful for fuel cell development.
- Learning about transient, localized conditions during stack operation is necessary to optimize fluid-dynamic designs. This is a good first step.
- Strong team.
- Technology very appropriate for application.

Weaknesses

- Not identified.
- It is not clear that the location of sensors within the flow field impacts the results.
- Not clear what the next set of experiments are or will show.
- No information on new directions or how they will improve or expand their fuel cell measurements and capabilities.
- Long species sampling times limit their usefulness but can still show trends.
- Did not state the improvement of this approach over conventional approaches used to measure temperature or diagnose these issues.
- This diagnostic method is not easy to implement into fuel cell system. A little more time should be spent on understanding how these sensors can be implemented in a less intrusive manner.
- Probe is sufficiently large to perturb flow and give data that may not be representative.

- This activity needs a CFD modeling project that operates cooperatively. Most likely, the calculations will be essential as flow field design tools. The thermal physics here are pretty simple, although the fact that steam can be "supersaturated" in the gas phase (maybe a fog) will complicate the issue. Los Alamos developed a series of "down the channel" models that could be useful to replicate these experiments.
- No discussion of sensor accuracy.
- May not meet targets.

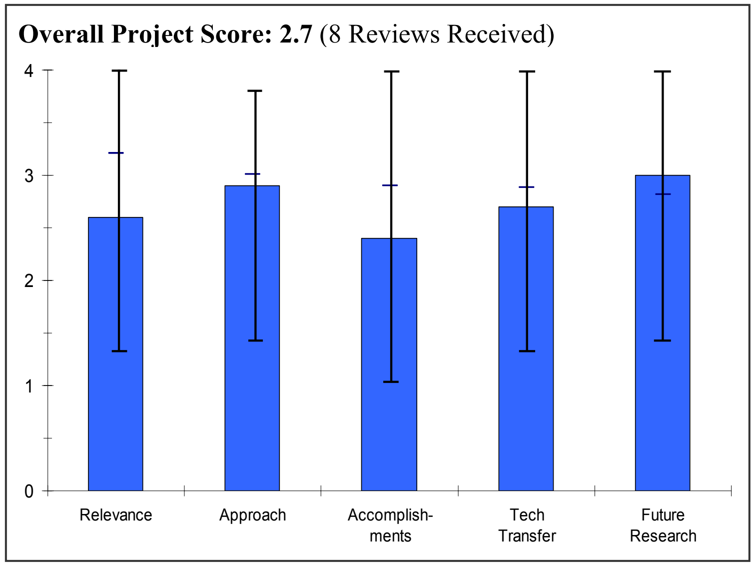
Specific recommendations and additions or deletions to the work scope

- Increase sensor sensitivity and data collection rate to resolve detailed fuel cell stack kinetics.
- Project should be expanded to place sensors in more locations within individual cells to ensure flow field geometry is not impacting results. Fuel cell stacks from additional fuel cell OEMs should be incorporated into the program.
- Need to correlate these measurements with known issues from fuel cell development studies.
- Show how this data has led to changes in fuel cell design to overcome issues identified with this technique.
- Too much emphasis placed on its application to a 2-cell in-house stack. Work with other DOE programs to apply to problems of interest. Again, suggest LANL, LBNL.
- More validation needed, especially RH sensor.
- Investigate potential intrusiveness of the present diagnostics. Are the probes small enough to not have significant impact on cell operation?
- I recommend that this project should not be a part of your program objectives. This work should be left to the fuel cell supplier to deal with. This work should be eliminated from your future research plans.
- Need more controlled operation variations (drive cycle) to identify origins of local efficiency limitations.
- There needs to be integration with this work and the studies done at NIST (neutron scattering).
- Prof. Trung Nguyen/KU knows about modeling this system and he would be a useful collaborator.
- Prioritize proposed work. May not be able to accomplish all during remaining project time.
- Continue interactions with automotive fuel cell companies.
- Continue activity.

Project # FC-43: Research and Development for Off-road Fuel Cell Applications
Simpkins, Erik; IdaTech

Brief Summary of Project

This IdaTech project, supported by Donaldson, Toro, and UC Davis, will characterize the effects of off road conditions (air quality, shock, and vibration) on fuel cell and fuel processor systems. Analysis of off road (greens keeping and similar environments) air quality and operating conditions will be accomplished, and load profiles and mitigation strategies will be designed and tested for impact on the power generation system.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- This project addresses issues with field conditions that a fuel cell system will have to operate under.
- Project is narrowly focused on and off road application with limited relevance to DOE automotive fuel cell objectives.
- Project does address the need to validate the potential for application of fuel cell to off-road vehicles, albeit to a limited size, niche market.
- Off-road vehicles may have an easier market entry on some points (power requirements, price point), but fueling infrastructure may well limit.
- Target application will not have a significant impact on reducing oil imports.
- Off-road vehicles do have a significant effect on air emissions, and this is relevant to the program.
- Operating conditions are more demanding than on-road light duty vehicles, and this presents a useful challenge to system designers and will advance the technology.
- There is some connection with the hydrogen vision; off-road applications importantly also speak to clean air targets.
- The program address vehicles which are not necessarily transportation structured.
- The program does offer opportunities to make improvements to fuel cell systems that could translate to transportation applications.

Question 2: Approach to performing the research and development

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

- Logical approach to first understand application requirements for both power and environment.
- Would consider preparation of system and performance models in parallel with collection of the data in the early tasks.
- The plan to generate operational data from and off road fuel cell vehicle is sound. One concern is that the application may not be representative of a variety of off road applications and vehicles.
- Sound systems engineering principles have been applied to fully understand the requirements of the application i.e. power and torque profiles.
- Solid systems approach of gaining relevant data on application load profiles, power requirements and impulse and vibration.
- Fuel cell and power system design not being undertaken this year – DOE should follow closely in out years.

- The project seeks to deploy a hydrogen-fuel cell to power a utility electric vehicle. This application is a battery substitution.
- The approach offers good opportunity to identify methods of cleaning the air into the air side of the fuel cell.
- The approach may provide important information regarding vibration protection for fuel cells.
- The approach has activities that are not generally beneficial to transportation fuel cells for the hydrogen economy.

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

This project was rated **2.4** based on accomplishments.

- Project appears to be moving along as planned.
- Conclusion to use fuel cell hybrid is reasonable.
- Results on air quality for the applications considered not presented.
- More specific data (from existing information) should be identified to design single cell testings.
- This project is in the early stages of development. Results to date appear commensurate with resources expended. The progress will accelerate once vehicle development and testing are further along.
- Program has only been funded a few months, so progress is limited to data collection on operating envelope and requirements.
- The accomplishments to date are modest. The tasks are certainly necessary, but not very challenging.
- The program is in a start up phase having only been signed in the last 5 months. For that short period, the program has collected basic data for analysis.

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

This project was rated **2.7** for technology transfer and collaboration.

- Appropriate team selected to address modeling, air filtration, and development of application requirements, fuel cell development, and demonstration of system in the application.
- Does team have the appropriate expertise to effectively develop controls for the hybrid system controls and power electronics?
- A good team has been established to complete identified program goals.
- A modest team of partners but adequate for niche applications.
- Partnership with Toro is good.
- Donaldson is involved in cathode-air filtering in this and other projects.
- UC Davis involvement will help ensure learning from a specific platform is transferable.
- Collaboration is between an OEM (Toro) and a fuel cell vendor.
- The basics for collaborations with other industries and with a university are established. It is too early in the program to expect substantial technology transfers to have occurred.

Question 5: Approach to and relevance of proposed future research

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

- Bulk of the project lies ahead in terms of hardware development and field demonstration in various environments (7% of budget expended).
- The identified program objectives and tasks are responsive to the goals of this program. Future work beyond the designed program must omit future results.
- Next year should bring definition of major system specifications.
- Planning seems appropriate for project scope.
- The follow on work involves installation of a fuel cell system and a fuel system into the commercial platform. Those tasks seem straightforward, perhaps more “development” than ‘research”.
- Well organized and scheduled future work.

Strengths and weaknesses

Strengths
- This program will gather detailed information of fuel cell operation in an actual vehicle.
- These team members have a good understanding of the off-road application market and vehicle hybrid power train architecture.
- Working with a proven electric (battery) platform is an excellent starting point and Toro has a good technical record in this area. The team seems on track and doing the correct things.

Weaknesses
- Presentation did not clearly describe the system configuration intended for use in the demonstration.
- This program addresses a very specific application and vehicle. H is not apparent that data developed will be widely applicable.
- None evident yet.
- Weakness is in project relevance.
- The vibration study probably is mainly about shock performance of the fuel cell system. Those results will be very system dependent and therefore that system needs to be well characterized (mass of components, mounting details, mechanical strength, etc.) before the vibration study makes sense. (Obviously Toro designed the battery hardware with full understanding of the shock requirements for the vehicle.). This is also true about the “load profile”. Seems like those tasks would be required to have already been done (by Toro), and it is not necessary to repeat them, seems to this reviewer.

Specific recommendations and additions or deletions to the work scope

- Need to ensure that environments monitored during field tests and results correlated with performance of the filters and performance of the stack versus time.
- No changes needed yet. Employ computer modeling to understand trade-off between fuel cell and battery sizing.
- Project and DOE should strive to make learning as broadly applicable as possible, since target application is weak.
- Program should emphasize air purity and clean up information that can be transferred to fuel cell transportation applications.

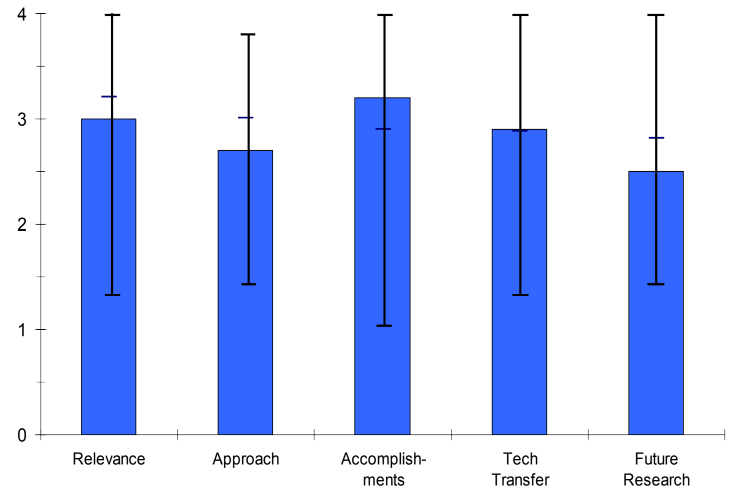
Project # FC-44: 50 kW Absorption Enhanced Natural Gas Reformer

Stevens, Jim; ChevronTexaco Technology Ventures

Brief Summary of Project

In this project, ChevronTexaco Technology Ventures will assist the DOE in developing hydrogen based distributed generation technology with significant cost advantages in reduced reformer and PEMFC system operating costs. These cost advantages will be achieved through improved fuel efficiency, reduced capital costs, reduced system complexity, and reduced reformer + fuel cell system costs. The first year focused on catalyst and sorbent testing with small scale reactors. The second year will be devoted to scaling up the reactors. The final product will be a natural gas reformer sized to provide hydrogen to a 50kW PEM fuel cell.

Overall Project Score: 2.9 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- This project has the objective of producing hydrogen from natural gas in a simple, efficient manner. Projected cost savings and efficiency improvements are quite significant.
- Hydrogen production from natural gas is a key technology for distributed fuel cell power generation.
- The technology being developed has potential to greatly simplify natural gas fuel processors and reduce cost of such systems.
- Cost of hydrogen will be tied directly to cost of natural gas.
- The idea is very innovative, but the benefits for hydrogen production (while claimed) were not well-articulated.
- Purported benefits were not quantified in the presentation.

Question 2: Approach to performing the research and development

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

- The system design is unclear and it appears they are still exploring basic concepts such as the clean-up steps. Before scale-up is finished, it seems that the system design should be finished, as these systems invariably have basic thermal interactions.
- To have complete hydrogen cost, efficiency/thermal integration needs to be understood.
- Dynamic process modeling is important for predicting benefits, but no evidence of such analysis was shown.
- Modeling and control approaches were not explained.
- Focus on absorbent longevity is appropriate and should be precursor to go/no-go decision.
- By simultaneous removal of CO_2 (and sulfur) during the reforming process, the equilibrium is shifted to producing more hydrogen, even at the higher reforming temperatures than would be required for a separate water-gas shift reactor to reach the same low level of CO.
- This approach, however, will not produce even 98% pure hydrogen (initial target), much lower than the 99.99% hydrogen (revised ?) target.
- An excellent approach is being pursued on this project, with catalyst materials being qualified in relatively large reactors (1-kW) so that scale-up to 50-kW reactor size is less risky.
- There is a focus on long-term and cycling tests so that catalyst durability is being addressed earlier rather than later in the project.

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

This project was rated **3.2** based on accomplishments.

- There is good improvement on physical stability of reforming extradites; however, overall durability still appears insufficient.
- The outcome of the cost-study (which was reportedly done) should have been presented in order to understand the viability of the project.
- Absence of modeling is a weakness.
- It would be helpful to show CO_2 absorption data to compel the concept.
- Show more process details, e.g. heat and mass balances and their temporal character in cycling.
- While adding inerts to sorbent is important for stabilization, inerts also add thermal mass and cause efficiency loss. There was no description of impacts.
- No work on methanation step was described.
- Modeling effort is very beneficial. More information on cost breakdown should have been provided.
- The primary accomplishment has been to test for and demonstrate cycle life for the sorbent. It has also been shown that the "kinetic" capacity is significantly lower than the "thermodynamic" capacity, perhaps by a factor of almost 2.
- The other accomplishment has been to build the two sub-scale (1 kW) reactors to demonstrate the concept.
- They have also completed a "rough" cost study, but the results were not presented or discussed.
- An impressive amount of data has been collected during the past year of the project, and advances in sorbent durability were demonstrated.
- Much of the inherent risk associated with sorbent durability (e.g., related to sintering and/or attrition resistance) appears to have been mitigated by the results obtained.

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

This project was rated **2.9** for technology transfer and collaboration.

- The partners in this project all appear to be fully engaged in the project.
- Partner seems appropriate for absorbent.
- One reviewer commented that it is unclear how any technology is being transferred (2 presentations), but looks like limited interaction with Cabot and Engelhard as their materials are incorporated in the system.
- This point was not discussed explicitly, but it was indicated that Cabot Superior Micro Powders is marketing the developed materials. It is not clear if the prime contractor, ChevronTexaco, intends to make this technology, once proven, available to others interested in this approach to hydrogen production.
- Chevron is certainly qualified to scale-up and commercialize the technology when this project is completed.

Question 5: Approach to and relevance of proposed future research

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

One reviewer commented that the future plans are fine, but other reviewers noted the following:
- Closer interaction with Engelhard and more reforming catalyst development is still warranted.
- Considering the durability doesn't appear to be sufficient for a production/commercialized system, scale-up to 50kW is probably not the best use of their time.
- It is not clear if the sorbent cycling stability issue has been solved.
- Modeling-based process and control optimization needs attention.
- The "future plans" require more detail of tasks and milestones as well as expected lessons learned.
- The future plans were given in generic terms. Overall, the activities listed are OK, but specific activities were not identified.
- It was hinted at during the talk that some further development of the commercial reforming/shift catalyst may be needed, but no future activity related to that was indicated.

Strengths and weaknesses

Strengths
- Innovative concept shows well insofar as exploratory R&D.
- Program benefits from strong industry experience.
- The project team includes commercial partners who can identify the likely commercialization issues and address them throughout the course of the project.
- Strong team, with good approach and plan, has excellent results to date.

Weaknesses
- After ~ 45 cycles, it appears the AER has about 3.5% CH_4, 2% CO + 1 % CO_2 in the effluent. Methanation of the CO and CO_2 will consume another (CO + 3 H_2 --> CH_4 + H_2O) 6% hydrogen and (CO_2 + 4 H_2 --> CH_4 + 2 H_2O) 4 % hydrogen – to sum to 13.5% CH_4 slip out of the fuel processor system. This leaves a tremendously high amount of CH_4 unconverted in the system. No information was provided to predict the overall efficiency of the system and to determine whether the unconverted CH_4 heat value can be used.
- The decrease in activity of AER and CO_2 sorbents do not appear to have sufficient activity. They seem to have high decreases in activity or capacity during < 100 cycles.
- Relevance for fuel cell systems was not quantified or adequately described.
- Maybe this project would be better suited for joint funding with Industrial Applications sector of federal funding?
- Detailed cost analysis is required. The presentation should include results of 1 kW reactor testing.
- The 1-kW reactors are small enough that they cannot really operate in adiabatic mode. Extrapolating the data obtained from these reactors to the 50-kW reactor may not be as straight-forward as the team proposes.
- It is not likely that 98% hydrogen can be obtained from this process directly. Further purification to 99.99% (or whatever the purity target may come to be defined as) will require additional steps and processes.

Specific recommendations and additions or deletions to the work scope

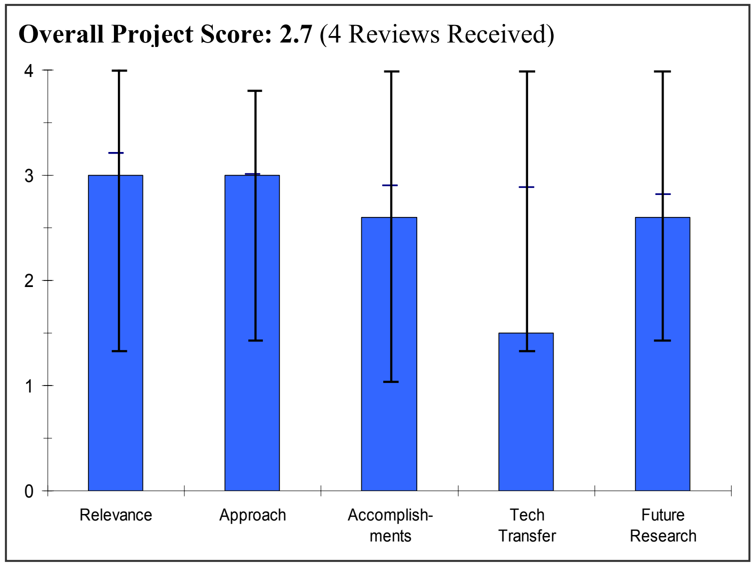
- No data was shown on sulfur tolerance of the AER materials. The reviewer assumed the natural gas odorants are removed before the AER step but did not see an overall system design.
- Project should continue to improve materials, increase materials durability, and perform a good cost study. More long-term improvement will be accomplished by that than the planned scale-up to 50 kW.
- As stated in presentation, improving the purification of the hydrogen stream well above 98% is needed.
- Cycle times for reforming and regeneration need to be explicitly defined and discussed. The influence of cycle time on reactor design (and size) and system operation does not appear to be given the significance it deserves.
- Sulfur issues need to be addressed explicitly.

Project # FC-45: Cost-effective High-efficiency Advanced Reforming Module

Holmes, Tom; Nuvera

Brief Summary of Project

Nuvera will develop an advanced, flexible fuel processing module for use in stationary applications. A new generation of low pressure steam reformer will be produced to address the widest range of specifications with the lowest risk. The reformer will be designed for high efficiency and long life, low capital cost in accord with DFMA principles, and will respect emissions standards and afford scalability. The intent is to develop a low-cost fuel processor module (FPM) that is scaleable to produce from 500 to 2,000 scfh of hydrogen. A 1,000 scfh FPM will be built and evaluated at Argonne National Laboratory. Lifetime assessment will be achieved through accelerated aging. Cost structure will be confirmed via third party audit.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Cost of hydrogen will be tied directly to cost of natural gas.
- The project is very relevant to DOE goals.
- Hydrogen production from natural gas and LPG is a key technology for distributed fuel cell power generation.
- Based on information presented, it was difficult to assess whether the technology being developed will be sufficiently cost-effective for distributed generation applications.

Question 2: Approach to performing the research and development

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

- Nuvera is doing a commendable job of preliminary design/thermal integration before trying to scale-up system–including cost trade-offs between capital and durability.
- Concept development is not directed towards DOE targets. There is no indication of lessons learned from first program.
- The approach is balanced with modeling efforts to aid in the design of fuel processor and in cost investigation.
- A formal decision making methodology was applied to the determination of the design approach being pursued.
- Nuvera used CFD modeling effectively to solve an issue related to combustion uniformity.

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

This project was rated **2.6** based on accomplishments.

- Nuvera built two systems with a down-select which is a significant accomplishment. However with limited (or no) data, it is difficult to understand the progress.
- There was no discussion of expected hydrogen purity.
- There was no detailed cost analysis.
- It was difficult to evaluate the accomplishments due to the proprietary nature of the work.

- It is difficult to assess the technical accomplishments during the past year, due to the lack of performance and cost data presented with respect to the new design.

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

This project was rated **1.5** for technology transfer and collaboration.

- Presentation did not indicate any development partners.
- No outside interaction exists. Even though this project is proprietary, collaborations with universities or national labs on fundamentals could aid the development effort and allow advancement of the state of knowledge in this area.
- There is no evidence of external collaboration on this project, although Nuvera appears to be fully capable of conducting this project.
- No publications, presentations, or interactions are noticeable.
- Nuvera is certainly qualified to establish the relationships required to scale-up and commercialize the technology when this project is successfully completed.

Question 5: Approach to and relevance of proposed future research

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

- Evaluation by ANL will be final proof of concept.
- Nuvera should have provided details of prototype test results versus DOE targets.
- Future plans are clear and follow the past progress.
- Detailed information is absent in view of the proprietary effort of this work.
- The plan appears adequate for meeting the project goals, although the level of detail presented relative to their current status makes it difficult to assess.

Strengths and weaknesses

Strengths
- Preliminary design and evaluation is a strength.
- Nuvera has excellent, integrated work.
- Nuvera has a strong fuel processing technology platform on which to build a fuel processing system.

Weaknesses
- Nuvera based down-selection of technology on overall score, however the system selected had the lower durability score. Durability should be improved – it is unclear if a concept exists to meet durability requirements.
- No presentations or publications were indicated.
- Emissions require further definition (not just ppm), for example (g of CO)/kg hydrogen produced should be included.
- Decision matrices include numerous criteria that do not relate to the ultimate DOE goals, such as schedule and patents.
- Nuvera lacks collaboration with other partners that may give some perspective on "out of the box" thinking.
- Although the efficiency of the processor is adequate with respect to cost, burner emissions are little bit on the high side. More focus should be considered to reduce emissions, i.e., are there other technologies that could be investigated, for instance catalytic combustion?
- Not enough information was provided to determine whether any weakness exist on this project.

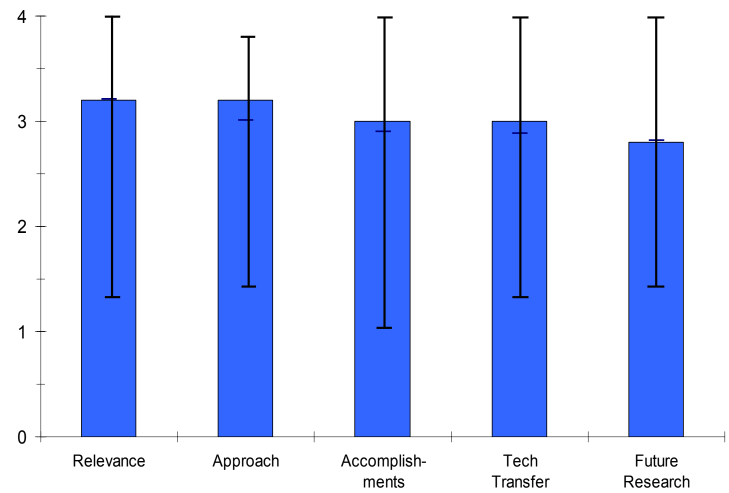
Specific recommendations and additions or deletions to the work scope

- Nuvera should participate more in publishing and presenting their work.
- Perhaps the presentation of data from system cost and efficiency models (which are assumed to exist) will provide a compromise with respect to intellectual property issues and allow for the possibility of a fair and accurate peer review.

Project # FC-46: 150 kW PEM Fuel Cell Power Plant Verification

Clark, Tom; United Technologies Corporation

Overall Project Score: 3.0 (6 Reviews Received)



<u>**Brief Summary of Project**</u>

The United Technologies Corporation (UTC) Fuel Cells and UTC Power's Stationary Power Plant project will resolve critical cell component, cell stack, and power plant reliability issues. Testing will be conducted in 20-cell stacks and 150 kW power plants.

<u>**Question 1: Relevance to overall DOE objectives**</u>

This project earned a score of **3.2** for its relevance to DOE objectives.

- This stationary program fills a need in overall hydrogen fuel cell transition for large-scale distributed power.
- Development of an industrial, stationary reformer-based power plant has limited relevance to DOE Fuel Cell automotive program. The best overlap between stationary and automotive is in the areas of endurance and cost reduction.
- This project is clearly important for the viability of stationary systems. Why were PEMs and low-grade heat chosen versus SOFCs? Why is the project only for 150 kW?
- This project is extremely pertinent to DOE fuel cell commercialization objectives.
- A stationary fuel cell system will be desirable when hydrogen replaces fossil fuels.
- This reviewer's impression of PEM fuel cell status is that there is still significant fuel cell development work required to achieve the stationary fuel cell system requirements. Integration of a 150 kW system is an expensive integration project for an immature technology.

<u>**Question 2: Approach to performing the research and development**</u>

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

- Overall plan is good but UTC needs to outline more detail and "b" & "c" level plans to address such issues as stack life. The current plan is not explicit enough.
- This program is a very heavily hardware-oriented. The upfront analysis of low cost components (e.g., seals) and membrane durability is valuable to all fuel cell applications.
- There is good balance between component issues versus systems challenges and excellent inclusion of market challenges into the overall program.
- This project is well-targeted to overcoming two crucial barriers to commercialization, namely cost and durability, in a timely manner.
- Work is focused on durability of 20 cell stacks, verification of design and reliability, waste heat utilization, seal material stability, and fuel processing.
- Seals with chemical and mechanical stability are needed and will be developed.

<u>**Question 3: Technical accomplishments and progress toward project and DOE goals**</u>

This project was rated **3.0** based on accomplishments.

- "Ganging" stacks, thermal management, and balance of plant issues are being addressed. UTC will have to down-select designs soon. Initial stack progress is positive but limited.

- Program appears on track for success. It is still early in the program; initial membrane durability and low-cost seal analysis have provided significant insights.
- Membrane endurance is impressive. Seal issues are well-defined. Durability test procedures were not well-explained, well-based, nor grounded on past experiences.
- Non-silicone materials for seals were tested.
- Accelerated testing shows great than 20,000 hour lifetime – demonstrated for 11,500 on 20 cell stack with unreinforced membrane.
- Excellent progress has been made at this early stage of the project – particularly on seal materials.
- Work focused on membrane lifetimes through accelerated testing, use of non-silicone materials, 40,000 hour seals, implementation of a steam reformation system, and 150 kW system design.
- The accelerated testing appears primarily addressed toward mechanical lifetime issues. Although an element of the chemical and environmental effects is inherently included in mechanical issues, the chemical and environmental effects did not appear to be addressed in detail. It is unclear that the accelerated test will provide "true" results with this approach.
- The use of a steam reformation system is generally more efficient than other fuel processing methods and will in general provide a more pure fuel. The downside is that a steam reformation system is far more complex and much larger in size than say, for example, a partial oxidation approach. It must be determined whether a steam reformation system can provide (under the best assumptions) a system that will meet the stated $1,800/kW system price before expending substantial effort. If not, alternatives must be found.

<u>Question 4: Technology transfer/collaborations with industry, universities and other laboratories</u>

This project was rated **3.0** for technology transfer and collaboration.

- UTRC is the only partner currently. EPRI is planned to participate.
- UTC has incorporated key players in the stationary power market-place. It is not clear that automotive OEM participation would be meaningful.
- Comprehensive choice of stakeholders and other players in the value-chain. However, UTC needs to identify the component partners better.
- At this stage, UTC has limited its partnering to user utilities, but it is understandable with 50% cost share.
- UTC has a good list of potential demonstration partners, but they are not yet engaged. Given that the project has many detailed technical issues to resolve, more collaboration with R&D organizations may be suggested.

<u>Question 5: Approach to and relevance of proposed future research</u>

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

- Focus additional efforts on balance of plant. Run tests using reformate. Perform cycling and transient tests. Perhaps an accelerated test correlation study should be performed.
- This program is a highly integrated; defined future tasks must be completed to achieve overall program objectives.
- The project is somewhat pedestrian – need to seriously compare SOFCs and PEMs in stationary applications.
- The next 4 years are on target to be very profitable in terms of R&D success – especially with regard to durability targets (40,000 hours).
- Test the 150kW unit with steam reformation and pressure swing absorption.
- Continue accelerated testing on membranes.
- Advanced seals
- The future work scope is reasonable, but must be tempered with the reviewer's view that significant detailed technical issues, that are not included in the project scope, require resolution.
- The post 2005 work plan is structured adequately to meet overall program activities.

Strengths and weaknesses

Strengths
- Organization has extensive "system" experience. UTC can adapt PEM automotive experiences to this effort.
- The key benefits of this project to DOE rests mainly in cell stack assembly (CSA) development and testing. Improvement in stack durability and low cost components are broadly applicable to all fuel cell development efforts.
- The project has very solid incorporation of past experiences. The broad (large), well-funded, comprehensive program balances component-scale evaluations up to systems integration challenges. The fuel processing systems and thermal integration challenges are well-analyzed.
- The PI's extensive experience and knowledge in stationary fuel cell systems is a great asset. PC25 experience with fuel processing is also of high value.
- Improved compression set predicts 43,000 hours of lifetime.

Weaknesses
- Long life stationary requirements are not the same as auto PEM requirements – these requirements might require a totally different mindset. What is the impact of "ganging" stacks? UTC should focus more on system intersection issues.
- It is not clear that efforts on catalytic systems reforming, pressure swing absorption, and thermal integration have any applicability to automotive applications. The stationary power plant market assessment does not support any of the DOE program objectives.
- Need to be more open (revealing) about component improvements (which could be of benefit to other small players). Market assessments left out U.S. Department of Defense (DOD) and Japan. Why only natural gas as a fuel?
- Efficiency will always be low on natural gas – 30-37% using lower heating value. Must use hydrogen.
- Accelerated testing protocols are always suspect.

Specific recommendations and additions or deletions to the work scope

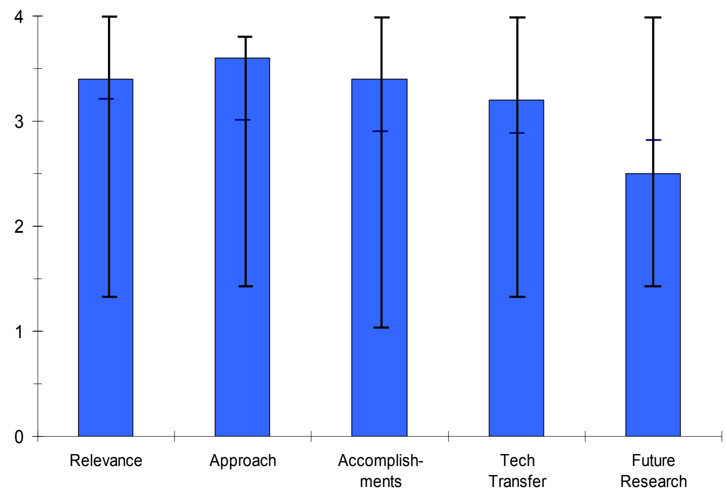
- Tests should be performed with reformate. Cost analysis should include cost of compression of fuel.
- Cold weather testing should be performed.
- Desulfurization sub-system.
- Add U.S. DOD and Japan market analysis. Need complimentary study to compare and contrast PEMFC and SOFC systems.
- Focus on smaller proof of concept of sub-systems and integrated sub-system tests before expending substantial resources on a 150kW demonstration.
- New Reformer (non-POx) may be improvement but CSR/PSA will still not produce >74% efficiency.
- One reviewer noted his belief that no changes are necessary.

Project # FC-47: Back-up/Peak-Shaving Fuel Cells
Vogel, John; Plug Power

Brief Summary of Project

The objective of the Plug Power project is to advance the state of the art of fuel cell technology with the development of a new generation of commercially viable, stationary, back-up/peak-shaving fuel cell systems. Plug Power will develop, build and test three identical fuel cell back-up systems and field test them at three sites, including an industry host site to identify technical barriers. Other objectives are to develop a cost-reduced polymer electrolyte membrane fuel cell stack tailored for hydrogen fuel; to develop a modular, scalable power conditioning system tailored to market requirements; to design scaled-down, cost-reduced balance of plant; and to certify design to Network Equipment Building Standards and Underwriters Laboratories.

Overall Project Score: 3.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.4** for its relevance to DOE objectives.

- This project addresses the need for APU systems and UPS systems.
- These applications are key in launching the hydrogen economy – excellent application.
- This project is very relevant to determining whether fuel cell based UPS/peaking shaving power sources are technically and commercially viable.
- Back up power systems do not appear directly significant to U.S. national goals but will provide experience.

Question 2: Approach to performing the research and development

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

- The approach involves demonstrating many of the key issues in fuel cell performance and cost goals.
- Good combination of development, testing, validation.
- The broad, system-wide approach is effectively addressing all major cost/reliability challenges to commercialization.
- The back-up system will use H_2 (no reformation). Use of propane with a small hydrogen start reserve would seem more practical for the customer.
- Use of stored hydrogen will impact operational cost but will enable a less expensive price. The developer stated that stack cost is most important.
- The PEM catalyst dissolution issue at open circuit or near open circuit is well known and may impact the practicality of this concept.

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

This project was rated **3.4** based on accomplishments.

- Plug Power is making steady progress toward meeting the project's goal and objectives.

- Plug Power is building on a good foundation of prior work. Plug Power has developed a complete platform design based on requirements for sub-systems and complete system.
- This excellent approach to testing and selecting components should work successfully and economically in the niche power backup applications.
- The flexible platform design has 50% cost reduction over prototype design while maintaining high efficiency.
- Numerous successes have been somewhat tempered by a number of No-Go disappointments, but it was important to investigate them.
- Power Conditioning System efficiency appears excellent, roughly 95 - 97%.
- Plug Power determined what technology will be fully incorporated into the system.
- Membrane is degradation severe. Target is 1500 hours over 10 years. Again, given the well known catalyst dissolution issue above roughly 0.9 volts and other issues of system life without respect to operation, this long of a lifetime may be an issue.

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

This project was rated **3.2** for technology transfer and collaboration.

- Partners are generally users who will provide on-site operational performance and durability data from a user perspective.
- Excellent combination of interaction with suppliers, potential users, and validation partners.
- User community is well represented.
- Argonne National Laboratory; Bell Labs, etc. are potential demonstration partners, not collaborators.
- Because this project involves commercialization, the necessary customers have been involved.

Question 5: Approach to and relevance of proposed future research

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

- Plans are in place to complete the project in a timely fashion and on-schedule.
- A lot remains to be done in the remaining project time.
- No future "research" is planned. Future plans include engineering and packaging systems for the demonstration.

Strengths and weaknesses

Strengths
- Strong capability, based Plug Power experience and fuel cell expertise; this project will result in a commercial product.
- Excellent approach to develop a commercially-viable fuel cell back-up power unit that has a high probability of success.
- The presentation described a clear, thoughtful road of product development and a plan for beta testing at customer sites.

Weaknesses
- None apparent – great work!
- In retrospect, it is possible that the overall scope of the project was too ambitious.

Specific recommendations and additions or deletions to the work scope

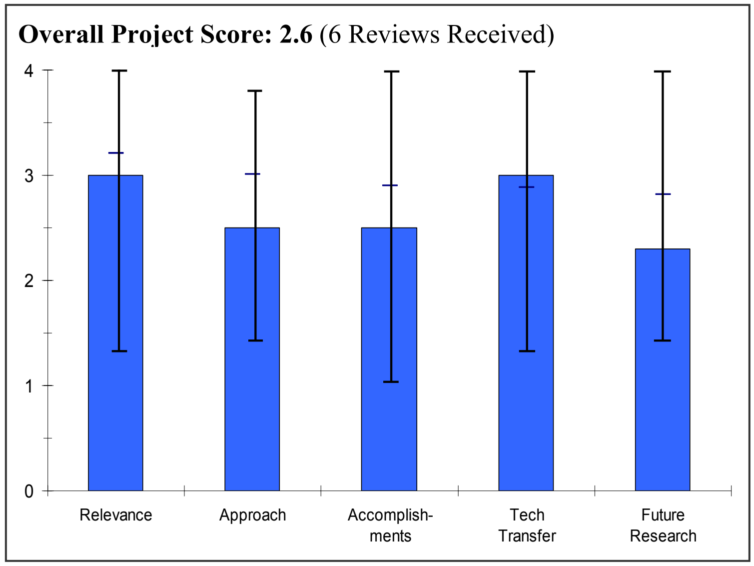
- None – the project appears to be very well conceived and operated – it should be a great success for Plug Power and for the DOE program!
- Review feasibility/merit of pursuing all of the remaining (yet to be completed) tasks.
- The project needs to ensure that target specifications can be met even under favorable technology assumptions. If not, then the project should focus on resolving the technology issues for this application.

Project # FC-48: Economic Analysis of Stationary PEM Fuel Cell Systems

Stone, Harry J.; Battelle Memorial Institute

Brief Summary of Project

Battelle Memorial Institute and its team will develop an understanding of the economic, technological, and market forces that are necessary through 2015 for commercialization of stationary polymer electrolyte membrane fuel cell (PEMFC) systems. The objectives are to evaluate potential stationary PEMFC applications; identify critical success factors required for commercialization; develop a technical targets table for each application (cost, reliability, size, response, emissions, electric load versus time, etc.); evaluate potential impacts of technological breakthroughs on cost and quality; and educate stakeholders and raise awareness of national programs.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Project assesses viability of PEMFC technology to meet cost targets for selected stationary markets.
- Stationary markets are not the primary focus of the Hydrogen fuel initiative.
- Commercial products are already available. Market dynamics are already in play.
- Analysis and comparison to competing technologies is important to be able to focus the efforts on the most critical barriers.
- Excellent, since cost and durability are the major barriers to fuel-cell commercialization, a better understanding of economic factors is critical.
- Program is on stationary fuel cells and contributes to transportation fuel cells in an indirect fashion.

Question 2: Approach to performing the research and development

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

- This project approach is being conducted as if no prior work has been performed. Why did Battelle focus on propane rather than natural gas?
- Approach is flawed since there is no explanation or review of conclusions with stakeholders and opportunity for feedback. Industry is not involved in decisions on selected applications. Much greater interfacing is required with stakeholders.
- Battelle team's limited knowledge of fuel cell industry and markets is apparent.
- Overall, the approach is good. Battelle has tried to consult with technical experts to verify their assumptions, but Battelle needs to broaden collaboration with fuel-cell OEMs to ensure more realistic input to the models.
- Approach is industry based and brings in the data from multiple industry sources.
- Program has ability to integrate design concepts and marketing approaches from several industries.

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

This project was rated **2.5** based on accomplishments.

- Deliverables fit the project's objectives to date.
- Product specs are already available – why set targets now? Cost analyses is not supported. Costs do not take into account recycling or manufacturing costs.
- Choice of applications and economics are critical to the success of the analysis. A 50kW stationary propane application and targets are not consistent. Study depends on matching of market requirements, and application-data is not shown.
- Limited market data for size selection; customers are not involved for all applications.
- Selections were not linked to market input but rather to an averaging of inputs from potential suppliers who had not been in markets.
- Time spans are off – 5kW backup system is now.
- Status Table and sensitivity analyses are useful.
- The project has shown good progress in collecting data.
- The assumptions for many of the data points need to reviewed among the several industry contacts.
- Large-scale jumps from 1000 to 10,000 to 100,000 in manufacturing capability may not reflect the true nature of the growth of the industry.
- Lifetime of fuel cells seems excessive even in the back-up power mode.
- Power assumption for fuel cell is too low.
- True nature of fuel cell stack cost and balance of plant for air and thermal management are lumped together. Subsequent breakout of cost yields a better understanding.
- Some of data needs discussion of assumptions which was not given.

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

This project was rated **3.0** for technology transfer and collaboration.

- At this stage the backup power units are commercial. The most relevant data would be proprietary. Who is serving whom?
- No interactive feedback of conclusions with stakeholders; decisions are arbitrary.
- Cost analysis is not realistic and industry based. Fuel cell knowledge base needs improvement.
- System designs are several years old and need updating for their 15 year projection.
- 15 year projection is ridiculous – use 5 or 10 years instead.
- Large number of partners are providing stakeholder input.
- Good, but need to expand interaction with OEMs (see Recommendations below).
- The program appears to have full support from industry.
- Battelle noted several publications and conference proceedings

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Based on results to date, Battelle should start fresh and work more closely with OEMs who have fuel cell products. Economic and life cycle analysis must be based on realistic applications and costing. We have had 200kW systems in the field for years. Let's not plow that field again. Much data is available already.
- Good, however, iterations should include closely verifying that key parameters are correct. One example is that UTC Fuel Cells has demonstrated PEM fuel-cell power densities of ~ 1 W/cm^2 at acceptable efficiencies and standard catalyst loadings. Using this value could really influence the conclusions here.
- Proposed research is consistent with program plans.
- Assumptions should be reviewed and detailed for readers.

<u>Strengths and weaknesses</u>

<u>Strengths</u>
- Limited.
- Battelle is good at organizing workshops with industry representatives.
- Battelle has had many interactions with stakeholders.
- The program is well-organized and structured.

<u>Weaknesses</u>
- This analysis is not treating the state of the art as "real". The overall cost modeling activities show no apparent depth. Each "system" will be proprietary – data input will be tenuous.
- Some key assumptions (e.g., power density ranges) are not realistic (too low). The assumptions that are shown to be critical through the sensitivity analysis must be confirmed with industry leaders. Include both state-of-the-art values, as well as projections provided by pragmatic experts.
- Program is dependent upon inputs from industry and does not appear to have means of cross-checking the validity of the industry inputs.

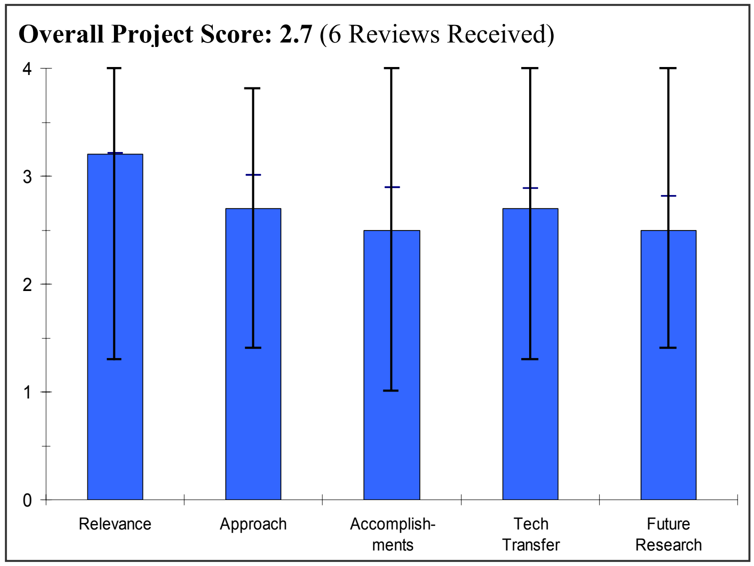
<u>Specific recommendations and additions or deletions to the work scope</u>

- Terminate this program. Let the Relion's, Ballard's, IdaTech's and Plug Power's of the world proceed with commercialization and market activities.
- Perform a complete review of this project with tighter definition of the assumptions, approach and deliverables developed.
- Expand interactions with fuel-cell system OEMs, including industry leaders (e.g., UTC Fuel Cells) to ensure that assumed values are correct.
- Develop means of cross checking industry inputs.
- Input more recent performance data into program; data is available in open literature.
- Evaluate the cost of warrantees, installation, and set up on the overall cost of a powerplant.

Project # FC-49: Advanced Buildings PEM Fuel Cell System
Taylor, Kyle; IdaTech

Brief Summary of Project

The objective of the IdaTech project is to demonstrate high electrical and overall efficiency, reduced energy consumption, and reduced emissions for hotel and follow-on applications. IdaTech will overcome technical and cost barriers through the engineering, design and construction of an integrated system with advanced fuel cell, fuel processor, and balance of plant subsystems. The 50 kW PEM fuel cell system design will be validated through field testing at three separate properties to be co-selected by Marriott International, Sempra Utilities and Puget Sound Energy. IdaTech will use the information provided from this demonstration to target early market entry opportunities.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Building demonstration program is very important.
- Stationary applications are an important part of launching the hydrogen economy.
- All objectives of this project are supportive of DOE's goals.
- A stationary fuel cell system will be desirable when hydrogen replaces fossil fuels.
- Project is focused on integrating and increasing the commercial viability of a stationary fuel cell power system.
- The lack of clearly identified technical performance objectives hinders the ability to ascertain the project metrics.
- One reviewer remarked that the development of an industrial, stationary reformer-based power plant has limited relevance to DOE Fuel Cell automotive Program; the best overlap is in the areas of endurance and low-cost.

Question 2: Approach to performing the research and development

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

- Approach is good but execution is weak. There is too much to perform in the remaining 18 months.
- Demonstration of alpha and beta stationary power systems is laudable. It is unclear what technical benefits accrue to the DOE Program.
- IdaTech aims to achieve the goals in the technology roadmap.
- The project is basically a field demonstration program – building 3 prototype integrated systems of current technologies for field test.
- Fuel cell module is scale up of 5 kW existing IdaTech module.
- Classic systems engineering/integration approach is being applied to the major subsystems, i.e. steam methane reforming, pressure swing absorption, balance of plant (BOP), and membrane electrode assemblies (MEAs). However, project resources might not be adequate to fulfill objective.
- The project focuses on membrane development – IdaTech will use off-the-shelf BOP and steam methane reforming and pressure swing absorption systems.
- IdaTech will use existing MEA.

- IdaTech is attempting to overcome technical and cost barriers via system engineering versus technology improvement.
- IdaTech will demonstrate high electrical and total efficiency.
- No estimate was given of the best system performance based on even favorable assumptions. It was difficult to assess the validity of this approach or whether more technology development is required.
- The BOP seems complicated. Cost targets were not provided but a complicated BOP would impact them.
- Approach does not clearly lay out the specific technical barriers in each of the development areas (fuel treatment, fuel processing, stack, BOP).
- The approach does not clearly lay out the technical performance criteria for each of the four phases.
- Milestones lack technical specificity and dates.
- Approach does appear to leverage existing industrial equipment in some areas.

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

This project was rated **2.5** based on accomplishments.

- Component validation is good. Scale up will be challenging. Reviewer cannot assess status of stack progress.
- Although the project was stated to be 49% complete, there were very few pictures of actual hardware. Goals stressed stack material, manufacturing costs and durability; there was little mention of any progress here.
- IdaTech operated half-scale fuel processing system and validated subsystem operation.
- IdaTech is scaling up reactor design for "optimized" performance.
- IdaTech developed packed-bed model for the reformer design.
- CFD was used to help design system flows and heat transfer.
- IdaTech has completed full-scale reactor design and begun fabrication.
- Fuel cell module is under design – a scale-up of an existing product.
- IdaTech identified off-the-shelf inverter to step up voltage.
- IdaTech has designed the complete system – on schedule to be operational this summer.
- At the 50% completed stage, this project will have to have a very industrious 18 months in order to achieve its objective of field evaluation of the Beta systems.
- Most work was focused on validating sub-systems. This approach is good; it provides flexibility in configuring different approaches and avoids the expense and inflexibility of testing large complete systems.
- Technical accomplishments appear to be progressing on a number of fronts, but are very generic. It is difficult to fully assess their significance due to the lack specific technical data. Accomplishments are mostly on the integration side – not specific improvements in component performance.

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

This project was rated **2.7** for technology transfer and collaboration.

- The extent of the collaborations is not clear (too early?)
- Contractor has a good support team for stationary power beta demonstration project.
- There is good participation from field test sites.
- More collaboration from key component suppliers would be helpful.
- Reviewer is not convinced that the collaboration and expertise of the partnership is broad-based enough to deliver the goods in a timely manner.
- There is a good list of potential demonstration partners, but it is not clear to what level each is engaged.
- Project ostensibly seems to have a number of partners, but they appear to be heavily weighted toward latter field evaluation. Role of partners is not clearly delineated.
- Additional collaborations would help improve component performance. For example, a firm specializing in stacks would be beneficial to IdaTech in their efforts to optimize the MEA.

Question 5: Approach to and relevance of proposed future research

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

- Plans to complete beta testing appear realistic.
- Basic modules will be constructed.
- Assembly documentation for alpha is underway.
- Long term durability testing for alpha will be later this year.
- Future work appears to be real stretch to achieve in the time remaining.
- Future work is not research, but the sub system validation approach is good. The design process appears disciplined.
- Contingencies and off-ramps are not well developed.
- Potential show-stoppers and the methodologies and partners to help overcome them are not well addressed.

Strengths and weaknesses

Strengths
- There is a coordinated effort on subcomponents.
- Contractor understands stationary power requirements very well. Contractor has laid out a realistic plan to achieve objectives.
- The development is straightforward using off-the-shelf parts.
- The project will give a good benchmark of existing fuel cell technology.
- IdaTech appears to have strong overall system integration capabilities.
- Significant effort has been placed on identifying partners for the demonstration and evaluation phases of the project.

Weaknesses
- There is little time to address critical issues if they arise.
- There was very little progress in the system development. Presentation did not show much actual hardware. It is unclear if the contractor truly appreciates the amount of effort required to scale the technology from 5kW to 50kW.
- The project does not advance the state-of-the-art.
- The project seems like an effort to jump-start a market for stationary fuel cells and a specific company rather than R&D (corporate welfare?).
- There is considerable duplication in reformer design – it is not likely to be significantly better than other design efforts supported by this program.
- There is no data yet on full 50 kW stack – very vague on configuration and no real indication of efficiencies or durability – which is a little disturbing for a program that is 50% complete.
- IdaTech did not discuss plans for data collection and analysis for either the Alpha system or the Beta field test systems.
- Project scope appears to be overly ambitious.
- Project is very generic and badly needs specific technical performance targets for each of the four project areas and clearly delineated approaches to achieving them.
- Project would benefit from additional collaborations to improve specific component performance.

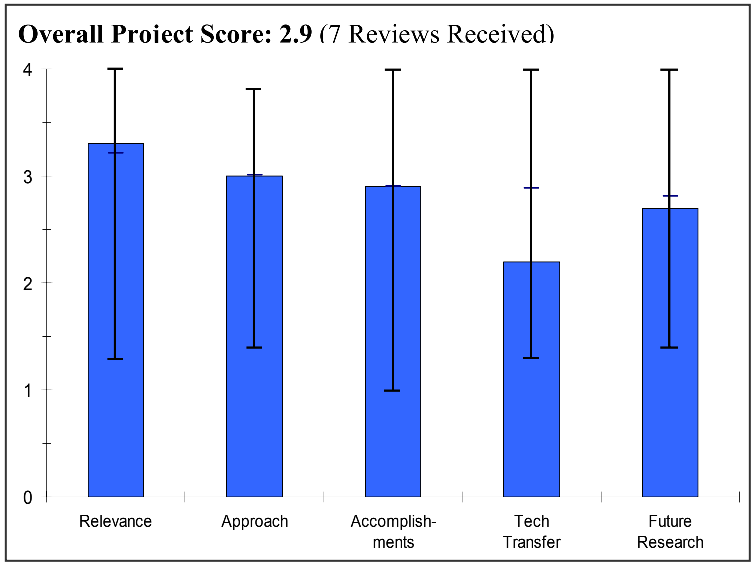
Specific recommendations and additions or deletions to the work scope

- Compare the efficiency of IdaTech's reformer systems compared to others developed by the DOE program.
- Compare the efficiency and durability of the IdaTech fuel cell stack with others developed by the DOE program.
- Closely monitor Alpha system operation, efficiency, and durability with possible modifications to program based on those results – if they don't meet DOE targets or suffer problems or delays, consider reducing the scope or canceling this project.
- Review and prioritize remaining activities and maybe rationalize appropriately.

- More clearly determine system targets relative to current technology available. Determine path forward based on this analysis.
- Clearly-defined success and failure metrics should be identified.
- IdaTech may benefit by focusing even more on being the technology integrator, as opposed to the technology developer

Project # FC-50: Investigating Failure in Polymer-Electrolyte Fuel Cells

Newman, John; Lawrence Berkeley National Laboratory



Brief Summary of Project

This project focuses on examining various types of fuel-cell failure, including both modeling and supporting experimental studies of fuel cell water and thermal management under various conditions including subzero (i.e., freeze) operation. The advanced mathematical models describe the relevant phenomena that occur in the fuel cell under the various conditions of interest. By understanding and describing these phenomena through both modeling and experimentation, failure points, such as water depletion due to membrane stress effects, and conditions that lead to failure can be identified and minimized through subsequent optimization of material properties, operating conditions, and possibly start-up and shut-down scenarios.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- Key next step to fuel cell commercialization: operation/start-up at sub-zero temperatures.
- Extremely relevant.
- Durability is key issue facing PEM fuel cells.
- It is unclear which failure mode will be focused on in this project. This size of project cannot cover numbers of failure modes.
- MEA durability is one of the primary technical barriers to fuel cell commercialization. A fundamental understanding of the failure mechanisms is critical.
- Good theoretical approach - badly needed.

Question 2: Approach to performing the research and development

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

- Strong experimental and modeling effort with appreciated goal of using results for optimization of materials.
- Although water ‘content’ certainly number one concern for freezing conditions, should other aspects of the materials be examined (will PTFE and/or PFSA membranes have phase issues?)
- Approach is good but quite ambiguous. Modeling combined with experiment will be valuable in addressing issues.
- Combining model with experimentation should provide valuable insight.
- It is good to take both model-based approach and experimental approach but it is unclear how to obtain material properties to characterize and optimize MEAs.
- A fundamental cell model and good property data, as proposed here, are vital to understanding of fuel cell performance failure.
- Not clear if a dynamic model is planned. As failures due to transient operation were mentioned, the approach would certainly benefit from such a dynamic model.
- The incorporation of accurate two-phase flow is key, and certainly non-trivial. Could be a significant contribution.

- Not clear what validation is planned.
- Needs to incorporate the "three states of water" ideas.

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

This project was rated **2.9** based on accomplishments.

- Good progress on very difficult two-phase modeling ventures.
- Key issues: initiated modeling and testing of critical parameters. Good start at delineating key design issues.
- Model looks promising.
- Results consistent with start date.
- Program just recently commenced, thus score is very tentative.
- New project.

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

This project was rated **2.2** for technology transfer and collaboration.

- While UTC is one of the fuel cell system leaders, not sure they are best collaborator.
- Too early.
- Limited interactions.
- Focused on only UTC system.
- It is recommended that the PI consider leveraging other project outcomes to identify failure mode mechanisms. Material property information should be incorporated by material suppliers.
- Unclear as to the roles of LANL and UTCFC, other than the affiliations of two other LBNL contributors.
- A strong validation effort for freeze, spatially resolved temperatures, currents, RHs, failure mechanisms and sites, etc. is vital. LANL, UTC, and others can play a valuable role.
- New project.
- The UTC and LANL collaborations will be crucial, and must be maintained to bring experimental verification and applications for model results.

Question 5: Approach to and relevance of proposed future research

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

- Some of the membrane-related issues cited by the PI may be best addressed with a partner.
- Needs more specifics.
- Include other experimental data to develop his model.
- The opportunities and issues with low RH operation are most important to address. What surface energy(ies) is needed for multi-temperature, multi-RH-capable fuel cell MEAs?
- Include other experimental data to develop the models.

Strengths and weaknesses

Strengths
- Premier modeling group with a long and consistent track record of success in grappling with complex systems and producing systematic understanding with “reality-checked” models.
- Very qualified group.
- Combining modeling with experimentation.
- The principal investigator's 30 years of fuel cell experience.

<u>Weaknesses</u>

- While it is agreed that water is the main culprit in sub-freezing conditions, are there other materials problems that need to be considered, such as freeze-related destruction issues ("frost heave") that could be anticipated with a model?
- Is the group trying to accomplish too much?
- Only one system – UTC. Will results translate to other system designs?
- Light on details. Provide more specifics on plans and collaborations.

<u>Specific recommendations and additions or deletions to the work scope</u>

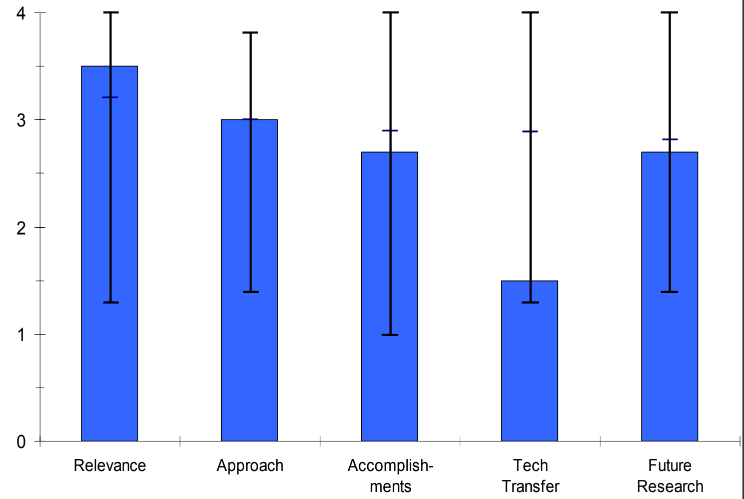
- While UTC is widely acknowledged as a fuel cell technology leader are they the most appropriate for a freeze related program? The UTC "water-filled" bipolar plate and matched GDL design are quite different from the majority of the industry and may not be the best system to mate with these modeling efforts.
- As the PI discussed, some membrane-related issues in sub-zero operation suggest forming collaboration with a membrane leader.
- This project should focus on model-base approach.
- Consider developing a dynamic model if this isn't already so. May need to be a simplified model for computational reasons.
- Consider exploring mal-distribution of temperature and flows (air, H_2, coolant, due to channel and cell tolerances). These may be strong contributors to failure that will not be explained by a perfect single-channel model.
- Land effects should be included.
- Co-, counter- and cross-flow should all be modeled.

Project # FCP-01: Montana PEM Membrane Degradation Study
Spangler, Lee; Montana State University

Brief Summary of Project

Montana State University's overall objective is to determine membrane degradation mechanisms and how to prevent or mitigate them. Specific goals are to determine changes in membrane material properties as degradation occurs, determine if any electrical properties can act as a signature of developing degradation, and investigate the potential of advanced control systems to prevent degradation problems.

Overall Project Score: 2.8 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Understanding and validating degradation mechanisms is key to the success of the PEMFCs and the Hydrogen Fuel Initiative.
- Needed fundamental work on decay mechanisms.
- The project goes deep into the membrane degradation evaluation which is one of PEM technology obstacles.
- Novel approach- but hard to interpret.
- Durability increases are central to the Initiative.
- Addresses membrane durability and degradation goals and objectives.

Question 2: Approach to performing the research and development

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

- The PI utilized Nafion 117 for his NMR experiments - Nafion 112 would more applicable for PEM applications.
- A comparison of extruded versus solvent cast Nafion would also be of interest - this might provide further information on the difference in manufacturing techniques.
- The PIs approach of voltage signatures for MEA degradation is incredibly innovative.
- How all data will be analyzed to find 'signatures' for decay mechanisms is unclear.
- Excellent use of MRI.
- The approach from three different analysis methods is interesting and with experts it should bring out some good results.
- The monitoring and NMR are useful techniques.
- Approach to getting individual membrane data is a good approach. Would like to see better tie of data to potential failure mechanisms.

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

This project was rated **2.7** based on accomplishments.

- No voltage signatures to predict MEA degradation have been found to date.
- No correlation of NMR studies to fuel cell experiments and MEA degradation have been done to date.
- Very interesting results with imaging of new membranes.
- No data on tested/aged membranes.

- So far there are no final results but the preliminary results seem interesting.
- Four membranes? 117? Publications light for a university.
- The stack used, nature of the membranes used, and protocol is not alluded to in the poster.
- Publications light for a university.
- Poster information does not include the kind of stack used, the nature of the membranes, or any summary of the research protocol.
- The T2 heterogeneity of the membrane observed is a provocative result. Needs confirmation with extensive controls described.
- Lots of data but limited analysis so far.

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

This project was rated **1.5** for technology transfer and collaboration.

- This project should work with Dupont. Currently there is no collaboration/interaction.
- No collaborations with outside researchers. Collaboration with MEA supplier would be beneficial.
- There is no internal organizational collaboration. At least there should be membranes and MEA manufacturers involved in the project.
- Don't see much.
- The program would benefit from collaboration with other institutions that might combine these results with further chemical and mechanical insights into the failure modes.
- Should partner with a membrane/MEA manufacturer and/or user.

Question 5: Approach to and relevance of proposed future research

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

- The proposed work could potentially be excellent if enough progress is made.
- It is not clear if the PI has the personnel and time to complete the proposed research project.
- Image plans very good.
- Unclear how data mining data for signatures will work.
- The preliminary results show some general failure systematics. So those areas should be studied deeper.
- If time and/or budget allow, consider AC impedance and hydrogen/nitrogen crossover current as additional electrical properties which may give insight into degradation.
- PI should try to make connections between the data collected and the potential failure modes.

Strengths and weaknesses

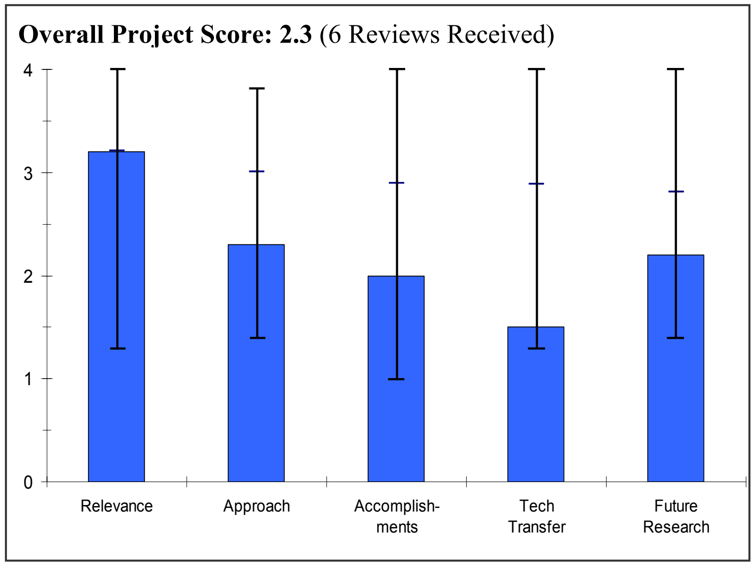
Strengths
- The concept of finding voltage signatures on MEA degradation is one which could be incredibly interesting.
- NMR analysis of various sections of the MEAs could bring more insight on membrane degradation in fuel cell operation.
- Well defined methods for imaging.
- Good theoretical know-how in three different but supplementary areas. Enthusiastic researchers.

Weaknesses
- No voltage signatures have been found yet - progress has been slow.
- Pre-treatment of membrane for NMR studies could alter membrane morphology/chemistry.
- No involvements with outside researchers- need to work with MEA manufacturer.
- Lack of industrial collaboration.

Specific recommendations and additions or deletions to the work scope

- Excellent concept - slow progress to date. Interaction with Dupont could help program.
- Integrate membranes or MEA manufacturers into this project.
- Including potential users as partners might help in determining how to use data to identify failure modes.

Project # FCP-02: Development of Higher Temperature Membrane and Electrode Assembly for Proton Exchange Membrane Fuel Cell Device
DeCarmine, Tony; Oxford Performance Materials, Inc.



Brief Summary of Project

Oxford Performance Materials, Inc. will develop novel sulfonated polyether ketone blends for use as fuel cell membranes. The focus will be to fabricate stable MEAs for operation at 120 C and low relative humidity from SPEKK-based membranes and catalyst layers. The targeted performance objectives are to surpass the electrochemical and mechanical durability performance of Nafion at similar operating conditions.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- The PIs goal of developing a high temperature/low relative humidity membrane is a viable part of the hydrogen fuel initiative.
- Higher temperature/low RH membranes and MEAs (membrane electrode assemblies) have the potential to reduce the complexity and cost of FC systems.
- The project goal of improving PEM-FC membranes for higher temperature operation is important for fuel cell program success.
- It is not clear how (nor was the PI able to explain how) the candidate materials being studied would be superior to membrane materials other than Nation that have already been developed for high-temperature operation (e.g., Celanese Celtec PBI, 3M, etc.).
- The high temperature membranes are one key area of transportation PEM systems.
- The project does a good job of addressing a critical area of need for the program.
- The relevance of the project is good and it addresses important issues.

Question 2: Approach to performing the research and development

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

- Initial concept was innovative.
- PEEKs (poly ether ester ketone membranes) are very chemically and mechanically robust materials.
- PIs modest data shows the approach was lacking - no clear viable path to successfully produce viable fuel cell membranes using PEEK as the base material.
- The "Technical Approach," as presented, is merely high-level generality (e.g., "Replace Nafion with Novel Polymer"), and "Improve Properties").
- Lacks a systematic research plan.
- Membrane research is very much trial and error. When this is done by experts it may lead to good results.
- Methodical approach to the problem addressed.
- The approach is confusing and difficult to review. The presentation is not clear on exactly what the project is trying to show

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

This project was rated **2.0** based on accomplishments.

- Minimal progress.
- Modest accomplishments.
- PI is still speculative with no clear pathway for membrane success.
- Cross linking of membrane may help.
- Membrane has many technical issues - dissolves in water.
- The presentation included few quantitative results.
- The presentation included little description of experimental method or design (even though construction of "test station" was major accomplishment of period).
- Considering the low level of funding and project duration outstanding progress.
- The data presented this late in the project is not very comprehensive.
- Very little characterization of the materials was presented and no fuel cell operating data was available.
- Only a few conductivity points are shown at 25C. No data about higher temperatures is given and the other results indicate numerous problems to be overcome in future. For example, what happens to the blends at higher temperatures? How do the blends behave in the MEAs in proximity to the electrode? The presentation talks about MEAs but the presentation does not give confidence that the problems of the interface re understood.

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

This project was rated **1.5** for technology transfer and collaboration.

- Collaboration with UConn - papers published.
- PI needs input from stack developer in order to obtain key membrane requirements.
- The collaboration with UConn- so important in previous reviews of this project- is gone and no longer in evidence.
- UConn was the source of supply of SPEKK material, now no longer available.
- The publication record is weak- two of three cited publications are "Abstracts" (not peer reviewed).
- There is no or very little collaboration.
- No specifics on this presented but there are indications from the technical presentations at meetings that this is being disseminated.
- There does not seem to be any collaboration with other organizations.
- There does not seem to be any active collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Membrane evaluation straight into a MEA and fuel cell experiments is pre-mature.
- PI should acquire knowledge to run appropriate ex-situ tests before MEA fabrication.
- Future research plans do not articulate goals for new materials that would be technically superior in some respect to the recent membrane material products made by others.
- PI has many ostensibly good ideas he intends to try.
- PI's optimism would be more credible were there to be more reported data from the project.
- Too difficult to say because the proposed membrane is not developed yet.
- Logical future plan. Direct discussions with the presenter revealed a good understanding of the options for good progress to be made.
- Project is nearing completion date and chances of success by that time seem low.
- The stabilizing strategies may help but there seems to be a lot of issues to resolve about the materials. Since the presentation does not explain the phase separation properties it is hard to know how this will pan out.
- The plans for MEA fabrication need to be spelled out more clearly e.g. just exactly what properties are sought.

Strengths and weaknesses

Strengths
- Base membrane, PEEK has good potential.
- PI is enthusiastic about what he might accomplish.
- Good practical and experimental know-how. Good knowledge about the manufacturing and about the markets.
- Unique high temperature capable polymer systems with interesting materials characteristics.

Weaknesses
- Membrane potential has not been achieved - none of the pathways have resulted in viable membrane.
- Membrane materials have low conductivity and dissolve in water.
- PI lacks expertise to perform ex-situ analysis.
- Lack of well-defined goals with respect to current state-of-the-art.
- Project has appearance of "starting over" with lack of continuity from first year.
- Lack of quantitative results from previous work.
- Industry may have other interests than doing experiments.
- As noted by the presenter- the engineering challenges surrounding the condensation of superheated steam in the hydrophilic polymer matrix could prove problematic in general for all the higher temperature membrane systems under consideration by the Hydrogen Program.
- This work is very preliminary and I believe is trying to demonstrate the proton conduction mechanisms enhanced by the blends and phase separation. Since some of these materials are unlikely to be stable in a fuel cell some of the experiments seem to try to scale up inappropriately at this time. There seems to be much confusion about the goals of this project.

Specific recommendations and additions or deletions to the work scope

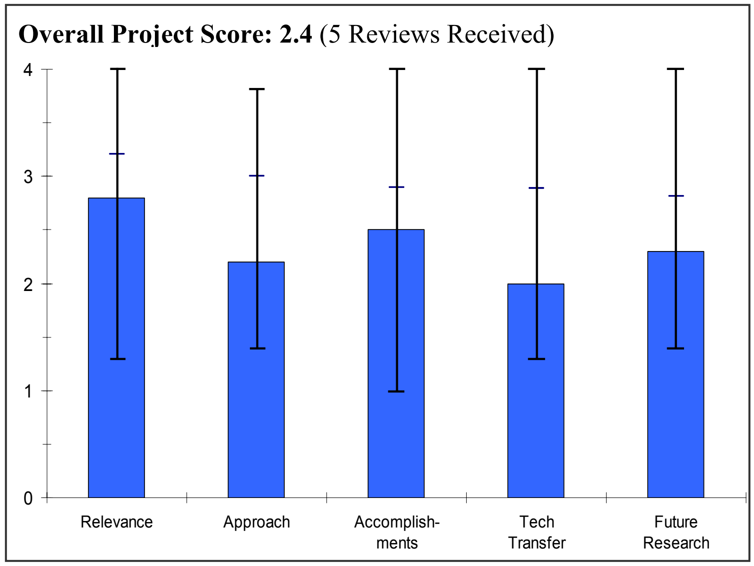
- Not to do MEA fabrication or fuel cell experiments until quality ex-situ data is obtained.
- While PEEK has initial promise, PEEK does not seem to be or become a viable building polymer for fuel cell membranes.
- Link this project with university teams to do the experimental work.
- As discussed with the presenter various cross linking options should be evaluated with the hydrophilic component. I recommend post membrane formation treatments.

Project # FCP-03: Advanced Fuel Cell Membranes Based on Heteropolyacids

Turner, John; National Renewable Energy Laboratory

Brief Summary of Project

The goal of the project at the National Renewable Energy Laboratory (NREL) in partnership with the Colorado School of Mines is to acquire an improved fundamental understanding of a class of inorganic proton conductors (heteropoly acids and their salts) that exhibit high proton conductivity at low humidities (below 25% RH) and at elevated temperatures (well above 100°C) and to apply that understanding to fuel cell membrane technology.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- The PI understands the need for high temperature/low relative humidity membranes for PEMFC applications.
- High temperature and low RH will help minimize the complexity of fuel cell systems and cost.
- Concept may or may not be low cost depending on how the acid is "membranized" and/or immobilized.
- Important problems.
- Relevance to 'solution' of problem unclear.
- The project directly addresses how to operate fuel cells at temperatures that are difficult to use water as a solvent. In addition to addressing heat management problems, significant system simplifications would result that will lead to lower costs. This one interesting approach to retaining "water "in the membrane that allows retention of known polymer materials. This project will become outstanding when and if the performance in the MEA is addressed. This aspect is lacking and plans to study the issues in the interface need to be formulated.

Question 2: Approach to performing the research and development

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

- Nafion doped in heteropoly acids (HPAs) has been shown to be feasible in 1997 by Malahotra and Datta - the PI is in need of new insight.
- The PI is in need of a realistic perspective on membrane conductivity needs for viable membranes. "Fixing" HPAs in silica/polymer matrix to be the sole proton conduction mechanism will be insufficient to obtain any practical conductivity values.
- Fundamental approach to understanding which heteropolyacids to try is sound.
- Need durability studies in actual operating fuel cell conditions.
- Concept is interesting, but it will be very challenging to stabilize the resulting compounds.
- Heteropolyacid (HPA) approach is sound as a demonstration but water solubility must be addressed—ignored here.
- HPA work is old.
- New work on polymer seems to be less than impressive. How will these polymers help the cause?
- Particularly like the scientific approach to the proton carrier's structures rather than a sand in the membrane approach. This approach should lead to understanding and not just whether it works or not. Real scientific conclusions should result.

- The one weakness is the lack of an approach to the MEA. How will the material behave in close proximity to electrode surfaces and how will adhesion be maintained?

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

This project was rated **2.5** based on accomplishments.

- Proof of concept demonstrated by PI has been shown eight years ago by another lab.
- PI needs to present conductivity values for membranes with "fixed" HPAs in silica/polymer matrix.
- Reasonable results given that this is a new program.
- One of the few new, alternative ideas for membranes in the whole DOE program.
- Unable to determine accomplishments - too early in project.
- This year's technical accomplishments toward DOE goals appear to be minimal.
- No new results on HPAs to make them more compelling.
- Making polymers but without apparent rhyme or reason.
- Excellent progress in membrane performance has been made. The MEA should be next. Gas permeabilities should be estimated.

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

This project was rated **2.0** for technology transfer and collaboration.

- Good collaboration between university and lab.
- The PI did not discuss or present collaboration with 3M - unclear what the collaboration is for or when it will begin.
- None shown with major players - 3M listed but what is their role, if any, in the program?
- Too early to tell.
- Needs to expand collaboration since they rely on University partner for HPA expertise, the most promising part of project.
- Needs influx of good polymer chemistry ideas.
- The collaboration with CSM is strong although the presentation does not make the respective roles of NREL and CSM clear. The collaboration with 3M does not seem obvious apart from reporting data to 3M for them to use in their presentation. One would think 3M would have input to clarify what is meant by mechanically strong, film-forming properties and most of all what is likely to be required in the MEA structure.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- The PI presented future research in regards to HPAs/silica polymer matrices. If composite membrane does not work, no other ideas were presented.
- If you believe the approach is worth doing, their future work is a reasonable path.
- Initial proposed ideas seem OK.
- No clear path provided to achieve lower solubility, improved materials that can meet DOE goals.
- Need to address the different needs of the MEA. This could be a show stopper and 3M should provide guidance.

Strengths and weaknesses

Strengths
- Solid basic understanding of polyacids.
- University collaborator.
- Large collection of HPA chemistry to choose from.
- Good scientific approach.

Weaknesses

- PIs focus on HPAs is unoriginal and lacks real technical feasibility for PEMFC applications.
- It is well known that acids readily wash out of polymer films during fuel cell operation - though this team recognizes that, I remain unconvinced based upon their approach that they will succeed in solving this problem
- Limited fuel cell testing and/or capabilities, especially for "real" conditions, i.e., thermal and RH cycling, long-term durability tests, detailed diagnostic testing capability, etc. This area is where a strong industrial partner could really move the program forward quickly and effectively.
- Polymer work is second-rate.
- This work seems to be the vanity project for the PI—the focus on what is really needed by DOE is lacking.
- Polymer part is weak – need to involve 3M more actively.

Specific recommendations and additions or deletions to the work scope

- Collaboration with 3M would help this project tremendously in regards to practicality and research direction - the PIs should start/increase collaboration.
- A high risk program with a low level of funding. The potential rewards of success, though, are not that high, I would certainly not increase funding level for this off-beat concept as I saw little that indicated they would be able to make a reasonable membrane, much less one where the acid was immobilized.
- If it is continued, real-world testing with thermal and RH cycling as well as long-term durability fuel cell tests should be included.
- Real collaboration with a major player (3M, DuPont, Gore, Ballard, UTC, etc.) must be included to allow this program to have any chance of success. If they can sell the program to a real commercial venture (who is willing to invest some of their time and/or money), then maybe I am wrong, and it should continue to receive funding.
- Increase role of university. Provide support for that effort. More teaming or idea exchange with polymer chemists. Integrate with High Temperature program.
- MEA behavior/design needs to be added.

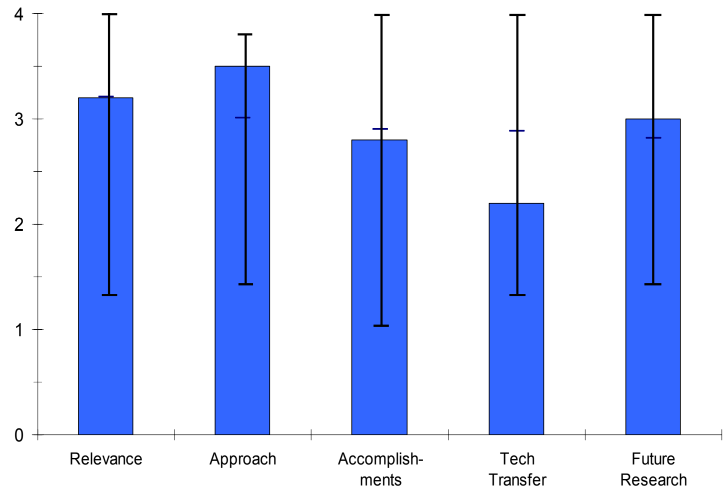
Project # FCP-04: Non-Nafion Membrane Electrode Assemblies

Pivovar, Bryan; Los Alamos National Laboratory

Brief Summary of Project

This Los Alamos National Laboratory (LANL) project investigates the use of alternative (non-perfluorosulfonic acid) ionomers in fuel cells. Alternative ionomers offer significant potential advantages in terms of cost, durability and performance, due to easier processing, lower cost of base materials, higher glass transition temperatures, and a wider range of tunable characteristics. The performance and degradation mechanisms of these systems will be investigated in terms of membrane-electrode compatibility and catastrophic failure in terms of pin-hole formation to understand better and extend the performance and limitations of such systems.

Overall Project Score: 3.0 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Developing and understanding non-Nafion MEAs (membrane electrode assemblies) are key to the success of the Hydrogen Fuel Initiative.
- Non-Nafion membranes reduce the cost significantly.
- Low cost membranes are critical-effort represents good start on a new system.
- BPSH has been around a long time now. If it were really that great, it would have been adopted by some commercial MEA supplier.
- Focus on interfacial effects really only relevant to BPSH, and don't translate well to other polymers and/or other industry standard materials and their issue.
- Relevance depends on the price of Nafion (and similar materials) when manufactured in large quantities.

Question 2: Approach to performing the research and development

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

- Technical barriers have been addressed and overcome.
- Very good approach to focus on one class of membrane materials and study its issues and potentials in some depth. Not all the programs do this. The information being learned, e.g. on electrode interface formation and durability, should apply to any other MEA systems as well.
- All aspects are being covered apparently: polymer, electrodes, and testing. Well organized effort.
- Approach is solid and well-thought out if you buy into the value of what they are doing.
- Technically, this approach seems very good and based on sound science.
- Trying alternative chemistries is the only way to find a Nafion alternative.

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

This project was rated **2.8** based on accomplishments.

- Excellent data has been obtained in MeOH.
- Excellent model has been postulated which is applicable to other membrane systems.
- Very good work. This project has focused on a real and difficult problem, the interface of electrodes and membranes, and provided critical new insight.
- Good progress in membranes and electrode fundamentals. Much more covered than reported (per in-depth discussion with presenter.)
- Good work on polymer of peripheral interest to DOE goals.
- Thus far, results look promising for a lower-cost (albeit not necessarily as good) alternative.
- Identified probably failure mechanisms.
- Alternative polymer backbone is unstable.
- Additive doesn't improve stability.
- No clear pathway to improving stability of backbone or additive.

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

This project was rated **2.2** for technology transfer and collaboration.

- Good interaction with universities.
- Stack developer involved may help this project further.
- The challenges of making a good performing MEA include the catalyst electrode structure and gas diffusion layer technologies as well. Just comparing to Nafion MEAs as a benchmark will not necessarily allow the new MEA to be optimized. The program would benefit from collaborations with institutions having extra expertise in these areas.
- Time to expand partnerships.
- I didn't see any.
- Poster shows little collaboration, but verbally, they say that considerable is occurring.
- No technology to transfer yet.
- Good collaboration with universities.

Question 5: Approach to and relevance of proposed future research

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

- To apply same methods to PEMFC will be crucial.
- The poster and presenter did not comment on the reversible decay processes apparent in slides 8 and 12. This is a key indicator that something other than interface delamination is occurring. This type of issue is seen in Nafion-based MEAs due to reversible impurity adsorption. The PI should consider any possible sources (from the new membrane?) that could cause the catalyst to be temporarily poisoned. If not, what is the source of this significant current density loss?
- Would like to see more H_s-air over methanol.
- Again if you believe in the work, the proposed future work is ok.
- Continuation as proposed seems to be justified.
- Need to demonstrate significant progress on stabilization.

Strengths and weaknesses

Strengths

- The program has shown excellent progress on a key concept which needs to be dealt with for Non-Nafion membranes ever to have a chance against Nafion.
- Strong fuel cell resource team experience and understanding. Collaboration with world class membrane materials source.
- Group plan for R&D is good- all aspects are being covered-solid plan.
- Polymer chemistry at VT - Jim McGrath is top notch.
- Facilities and capabilities at LANL.
- Approach should yield lower-cost materials if they give acceptable performance.
- Good understanding of failure mechanisms of alternative membrane.
- Should provide guidance in selecting still other membrane chemistries.

Weaknesses

- Group should not consider the electrode as a fixed entity - adapt it to the membrane.
- Interactions with real players in industry who could/would implement results in real products.
- Knowledge of real world issues in production and operation of fuel cell stacks in actual field conditions (this is especially important for new materials development and introduction).
- Cost is the primary motivation but little mention is given to cost projections for alternative materials. It seems that this should be considered from the beginning for any candidate material.
- No clear pathway to meet stabilization requirements and durability targets.
- Need to move beyond argument of "non-optimized" membrane as reason for instability.

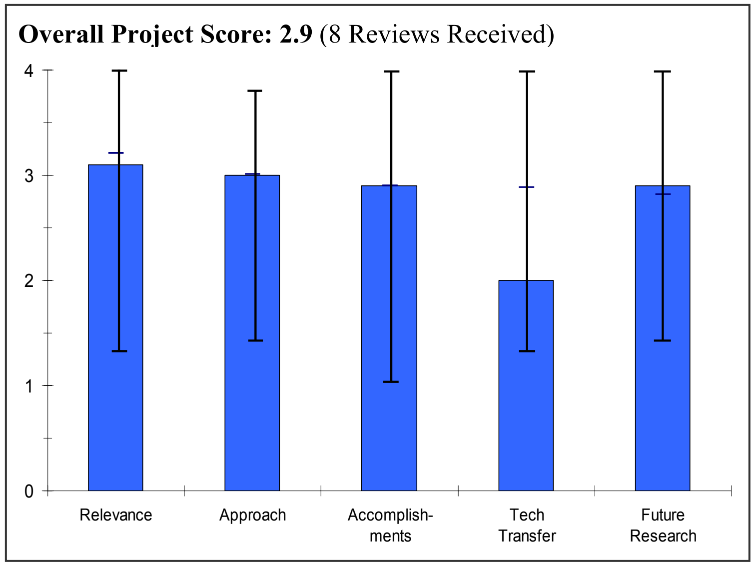
Specific recommendations and additions or deletions to the work scope

- Broaden the scope to involve stack developers.
- Publications in order to inform fuel cell community of work.
- Eventually the group needs to look at costs and manufacturability.
- This polymer has been around long enough and there has been enough work on it that it is time for the marketplace to decide whether it is viable. If it is commercially viable, let a commercial vendor license the rights to it, and bring out a product. I cannot recommend funding beyond the current cycle, not because the work is bad, but just because it's time to commercialize the material, and DOE/LANL are not the ones to do it
- Give more attention to cost projections for both Nafion-like materials as well as candidate alternatives, both produced in large volume.
- Optimize the membrane.
- Stop project or switch to other chemistries if significant progress toward stabilization isn't made within the next 6-12 months.

Project # FCP-05: Hydrocarbon Membrane
Cornelius, Christopher; Sandia National Laboratories

Brief Summary of Project

This Sandia National Laboratories (SNL) polymer electrolyte membrane (PEM) and catalyst coated membrane (CCM) development effort is an alternative approach to address the physical property limitations of perfluorinated PEM materials such as Nafion. Several alternative polymer electrolyte materials have demonstrated better fuel cell performance characteristics than Nafion. Polyphenylenes have the potential of being used in a PEM because of their excellent thermal and chemical stability and ability to form mechanically robust films. The chemistry afforded by the parent polyphenylene represents a system that has tunable chemical structure and properties.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Project has done an excellent job of identifying targets aligned to the HFCIT Multi-year Plan and the barriers it is trying to overcome.
- Good progress; many questions.
- High temperature, low relative humidity membranes critical to advancing DOE fuel cell goals.
- Key technology that must be enhanced.
- Important problems: high T, low RH.
- MEA-driven work presented—good.
- New ideas that are a little off-center.

Question 2: Approach to performing the research and development

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

- The new membrane materials (SDAPP) certainly have promising characteristics for higher temperature operation. The observed results however are often as much a result of MEA interface details as the bulk component properties. The PI should consider focusing on improving one or two primary membrane properties for one version of the polymer, such as improved sulfonation and understanding water uptake per sulfonate group to try and improve the poor sensitivity to low RH. Then they should also study the electrode/membrane interface to understand how its deficiencies may be affecting the results and conclusions being drawn about the different SDAPP bulk properties.
- Project appears well organized and focused.
- Presentation of task and milestone schedule is appreciated.
- Very systematic studies; quite productive.
- Appears to be little challenge to the team, they know the results before executing the work.
- Testing is conducted at high temperature and low RH, unlike some other projects.
- Approach is lacking in compelling motivation as to why this even works.
- Catalyst layer work seems good but totally empirical.
- Idea seems flawed on the surface: weaker acid provides better catalyst layer? What is the improvement?

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

This project was rated **2.9** based on accomplishments.

- Not applicable, program is too new, based on dates provided, yet a significant amount of data is presented and shows promise for this membrane approach. It was not clear what was achieved under the period of the contract.
- Project just started in March '05 and is scheduled to last approximately 6 months.
- Project appears to be on track to task and milestone schedule (30% complete).
- Could use bit more "hard" data.
- For the short project period and low funding level, have shown significant progress and a possible pathway to high T, low RH membranes.
- Good to see attention being devoted to MEA fabrication.
- Need to measure conductivity of new membranes as a function of temperature and RH so that membrane properties may be considered separately from MEA advances. Endorse subtask 1.1.2.
- Moderately successful results, but good given the amount of time project has been underway.
- This year's technical accomplishments toward DOE goals appear to be minimal.
- Lags the field in important way.
- Making polymers but without apparent rationale.

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

This project was rated **2.0** for technology transfer and collaboration.

- Working with an institution having electrode and MEA integration expertise would be beneficial. The goal should be to see what the best performance and durability can be with the new membrane system. This cannot be achieved by just varying the membrane component of the MEA.
- No partners involved in research, only SNL performing work.
- With short duration of project, significant collaboration may not be possible.
- Need to develop industrial contacts.
- Consider collaboration with other institution to do MEA durability tests.
- Too short a timeline to comment.
- Needs to expand collaboration to get more information on the 'why' of the effort.

Question 5: Approach to and relevance of proposed future research

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

- The proposed work appears to be too broad for the resources available. Extending to HPA additives before the basic polymer properties are optimized or understood should be reconsidered. The PI should consider taking the best polymer structure candidate to date and first trying to optimize the electrode structure and interfaces with it to try and achieve the best performance possible (and not assume that Nafion is the best – it is not) and study the issues that arise with the new membrane "system".
- Future work may lead to improved membrane performance, therefore increasing stack life.
- Need to have more aggressive challenges.
- The enhanced acidity and polymer structure/property approaches may be promising.
- Direction of future work seems logical.
- No clear path provided to achieve further gains, a result of lack of emphasis on the underlying rationale.

Strengths and weaknesses

Strengths
- Current project is a quick hit (6 months) keeping the scope manageable.
- Team is enthusiastic about work.
- Good three-way fundamental efforts.

- Good facilities for MEA characterization.
- Right subject matter.
- Good synthesis platform.
- All elements of team for testing are in place.
- New source of good ideas.

Weaknesses
- No collaboration with other organizations.
- Too small, too short of project to change the game.
- Needs more basic insight to emerge.

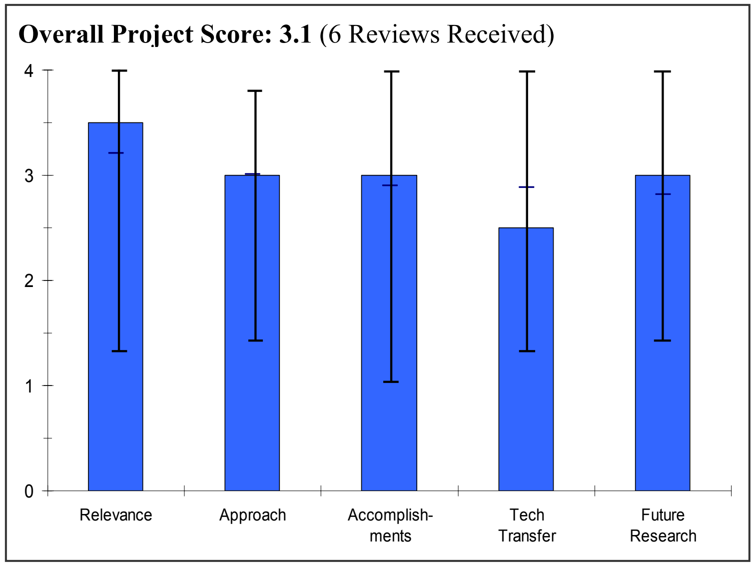
Specific recommendations and additions or deletions to the work scope

- Consider collaborating with others in future proposed work, particularly membrane producers.
- Either invest more or not continue program.
- Perversely, I would suggest that funding increase for this effort. The team is not bound by totally traditional approaches and could provide some routes to fresh ideas. Teaming should increase to obtain more detailed fundamental understanding.

Project # FCP-08: Cathode Electrocatalysis: Platinum Stability and Non-Platinum Catalysts
Myers, Debbie; Argonne National Laboratory

Brief Summary of Project

This Argonne National Laboratory (ANL) project will develop a non-platinum cathode electrocatalyst for polymer electrolyte fuel cells. The goals are to lower the cost and enhance the durability of the catalyst while maintaining and/or improving the performance as compared to the currently-used platinum-based catalysts. ANL is also elucidating the rates and mechanisms of the degradation of PEMFC platinum electrode performance by characterizing the effects of electrolyte, potential (holding and cycling), time, and temperature.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Good basic research program to understand existing catalyst and explore new catalysts.
- Well-aligned.
- Non-platinum catalyst an important field of research. Evaluation of stability/dissolution of Pt at cycling conditions very relevant to the R&D goals.
- Completely relevant.
- Performance degradation of Pt and development of more active catalysts are important tasks for improving the fuel cell durability and efficiency.
- Project is modest but addresses critical point driving DOE decision making for next-generation membrane.
- Seeks to evaluate GM claim that was based on substantial extrapolation of almost no data.

Question 2: Approach to performing the research and development

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

- Pt stability experiments done very carefully with strong combination of *in situ* and *ex situ* techniques. Balanced approach to non-Pt catalysts with solid criteria for contamination or abandonment of approaches.
- The project has two unrelated topics.
- Work on validation/evaluation of Pt solubility is great addition to the work plan. The extension to supported catalyst is very important.
- It is reasonable to try a diverse screening of many non-Pt potential catalysts.
- Standard procedures are proposed for the research and developments.
- Approach is a good start.
- Probably needs to find ways to do this with more realistic conditions next.

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

This project was rated **3.0** based on accomplishments.

- Elucidating Pt surface effects on dissolution behavior. Good comparison/correlation with experiments/theory of others-important results. Good initial results for Au-based systems. Solid decision to abandon carbide/nitride approach.
- Pt dissolution work is good.
- Good progress. Benchmark studies in the area of Pt and alloy catalysts stability is very important topic.
- Pt electrode degradation mechanism elucidation is valuable.
- Screening large number of non-Pt catalysis based on Au is no guarantee that one of the proposed catalyst formulations will work.
- Extrapolations of the present corrosion results for real systems are required.
- Too poor activity of the non-platinum catalysts, compared to that of Pt under the same conditions.
- Very important results coming from a modest effort.
- Key findings pointing to potential red herring from GM analysis.

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

This project was rated **2.5** for technology transfer and collaboration.

- Says "will collaborate with LANL"- not clear if it's happening.
- Good start. Could be expanded.
- Response to last year comment about partnering with someone with more MEA experience is inadequate.
- See no evidence that the claimed LANL collaboration is underway.
- The studies are of fundamental nature and no collaborations were required.
- Needs to expand collaboration somewhat but they are just getting started.

Question 5: Approach to and relevance of proposed future research

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

- Solid plan based on prior results. Curious to see initial results for metal centers on polymer backbone.
- It seems the project will be better focused.
- Testing in MEA is critical before going too far and synthesizing multiple samples. It is not clear what the focus of the work on bimetallic Au-Me/C or /TiO_2 is? Are alloys formed and targeted, why such low loadings?
- Success is not guaranteed.
- The reason for the choice of the new catalysts was not evident.
- Definite appreciation by PI for next directions.
- This could still be much more.

Strengths and weaknesses

Strengths
- Well-defined milestones and good progress.
- Benchmark studies such as the Pt dissolution are a good addition to the work plan.
- Payoff is high if ANL identifies non-Pt catalyst.
- Reasonable results for the Au-based catalysts, compared to other non-platinum catalysts.
- Very important problem.
- It's great that these workers will be generating key data.
- Competent group to assess key point.

Weaknesses

- Split effort on two projects (Pt stability and non-Pt catalyst) could hamper progress on non-Pt catalysts efforts by limiting range of experiments. Need to pull in other characterization techniques to better understand behavior of bimetallic systems (microscopy, etc.).
- The work on bimetallic (Au-based), transition metal carbides and nitrides, and metal centers attached to electron-conducting polymer backbone is too broad area for such relatively small project.
- Shotgun approach doesn't guarantee success.
- Trial and error method seems to be used for catalyst choice.
- Needs to expand approach to include more realism.

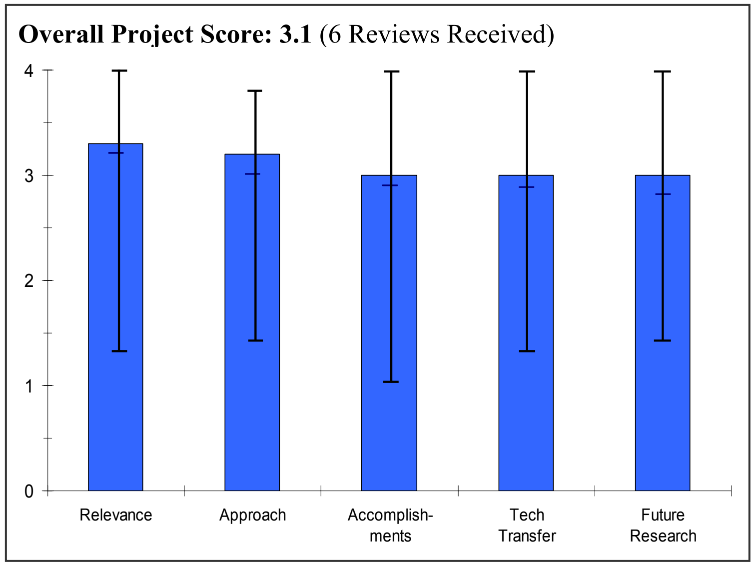
Specific recommendations and additions or deletions to the work scope

- Before you pick one or two of the above areas (plus the benchmark studies) and expand the research in 2, instead of all 4 types of material.
- Add LANL as collaboration partner and show connections at next review.
- This activity really demands additional support. The catalyst durability aspects truly are among the most important problems and this group shows immediate results in debunking!
- Project should expand to include drier conditions and polymer electrolyte interface.

Project # FCP-11: Modeling and Control of an SOFC APU

Khaleel, Mo; Pacific Northwest National Laboratory

Brief Summary of Project



This Pacific Northwest National Laboratory (PNNL) project provides SOFC-based APU development with control algorithms to optimize fuel efficiency and operating life, and models and experiments for stack response and structural failure under dynamic loading. Controls work includes developing dynamic system models, determining typical APU usage patterns, collecting electrical usage data from a working truck, and designing control algorithms to optimize fuel efficiency and stack operating life. Shock and vibration tasks are identifying failure modes under characteristic dynamic loading, determining material behavior under dynamic loading, determining guidelines for durable SOFC/APU systems, measuring truck excitations, experimentally validating the models, and defining requirements for APU isolation.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.3** for its relevance to DOE objectives.

- APUs represent an early application of fuel cell technology where fuel cells have important customer value-added features. SOFC technology is attractive because of its high stack efficiency and because of its direct operation potential without the need for an external reformer. Such applications are important for the development of a fuel cell manufacturing industry in the foreseeable future and will lay the ground for the eventual transition to a hydrogen economy.
- System control and seal durability for SOFC are important issues for successful fuel cell APU commercialization
- Addresses DOE desire to develop fuel cell based APU's for heavy duty trucks.
- Areas for heavy trucks are not directly part of Hydrogen Program (RD&D plan objective). There is a more appropriate DOE program those objectives of the project are suitable.
- The work is closely coupled and complimentary to the DOE FE SECA work and critical to the development of SOFC APU systems and application of SOFC to shock and vibration environments.

Question 2: Approach to performing the research and development

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

- Two important issues with SOFC APUs are being addressed. Project aims at using a modeling approach to: develop new algorithms that will optimize fuel efficiency and system operating life tuned to typical APU usage loads and forecast the impacts that shock and vibration could have to damage real life SOFCs.
- Models will be validated with real data from working trucks.
- The simplistic, invalidated model for SOFC is only a first approximation of real devices.
- Lack of cooperation from partners has significantly hurt the progress of the project.
- The work on the SOFC cell seals is basic and not correlated to real world data - they really don't know the shock and vibration profile to design to 2.5 years into the project.
- The project is appropriately designed and feasible.

- In this period the PI considered the key parameters to include whole system in the model (weakness pointed out in the last evaluation).
- The shock and vibration work needs to be heavily focused on coupling input frequency spectrum with natural frequency and damping characteristics of the SOFC. More focus on this area is suggested. The approach appears to be more heavily weighted to a deterministic stress analysis approach versus an input frequency/resonance coupling approach. The seal failure work is an example. With the exception of these comments the approach is good and clearly there has been some amount of frequency/resonance work, it requires more emphasis.
- This team is the best possible team to work on this effort. PNNL has developed models under SEIA which greatly leverages DOE's EERE investment.

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

This project was rated **3.0** based on accomplishments.

- Excellent progress made towards developing working models for: Systems and Control Analysis of stack and APU load including electrochemistry and thermal transient response to an APU electric load.
- Shock and vibrational impacts on glass ceramic seals, crack growth at seal/metal interfaces. Models also include thermal stress mismatch at electrolyte/ electrode interface.
- From responses to last year reviewers' comment, it's clear that this team has developed a very detailed and robust model.
- The project is far behind and not likely to be able to meet its deadlines.
- Major elements of the control system side of the project are missing (the power electronics models) and the simplistic lumped capacitance SOFC model constructed is invalidated with no apparent way to do so.
- The approach for developing a control strategy for the APU system is simplistic, incomplete, and flawed. Transient operation is not considered, and the project is far away from developing a system that can improve vehicle efficiency when the truck is moving through component electrification.
- Basic data need to successfully complete the SOFC seal part of the project is not in hand and work so far has been to apply standard statistical analysis approaches that have limited usefulness to SOFC developers.
- The weaknesses pointed out previously are progressively being solved. No information about the cost.
- The operational performance of an SOFC has received considerable attention in this work, i.e., electrochemical performance, lumped thermal model along with some amount of stress based failure, e.g. seal failure. This requires parameterization in a dynamic model; the dynamic model methodology requires more emphasis to ensure that generated data fits the analysis scheme. The progress has been good and the work excellent; the focus may have overemphasized ancillary issues.
- Developed lumped parameter dynamic model for SOFC for use in control and vibrational analysis.

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

This project was rated **3.0** for technology transfer and collaboration.

- Effective interactions with Delphi, a leader in SOFCs and auto component developments and PACCAR, a leading truck maker. Interactions with GE could lead to important applications to aerospace SOFC APUs now under development.
- Five publications/presentations.
- Contact and participation with collaborators very poor, and it has significantly hurt the project.
- Needed data for both aspects of the project has not been forthcoming and the relevance and usefulness of the project has been severely compromised.
- There are interactions with university and industry but the technology transfer is not clear in the presentation. No interaction with other PNNL project group was detected.
- PACCAR, University of Illinois/Chicago, Delphi, GE.
- Excellent Collaboration.
- Coordinating with SECA, PACCAR, Delphi, and University of Illinois.

Question 5: Approach to and relevance of proposed future research

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

- Focus is good on making models more robust and on validation on real truck data.
- The team knows what it needs to do but can not practically accomplish it in the time left for this project.
- There is no indication that the fundamental problems with this project can be or will be addressed.
- If the project run smoothly, the planning is adequate. No optional path is presented.
- More emphasis is required to develop the analytical framework in a global sense rather than detailed failure modes such as seals or essentially existing performance electrochemical and lumped heat transfer models. The methodology for an input frequency/resonance/damping model must be developed first so that collected data is in the correct form for application in the model. Detailed non-dynamic stress based failure modes is a focus of the SECA program. It is unlikely stress based failure will be predicted dynamically, the dynamic analysis will require a frequency analysis. Dynamic load factors require development for application to steady state stress analysis when done.
- Future work plan will complete the tying together of SOFC and BOP dynamic models. Vibrational analysis on entire APU then will be completed including stress determination in the stacks/cells.
- Would be good to experimentally validate the models or at least parts of the models.

Strengths and weaknesses

Strengths

- Addressing important issues associated with an early application of FC technology with modeling. Truck APU Fuel cells have important customer value-added features.
- Excellent progress made towards developing detailed robust working models for: systems and Control Analysis of stack and APU load including electrochemistry and thermal transient response to an APU electric load and shock and vibrational impacts.
- Models to be validated on working truck data with major auto supplier and truck maker.
- From responses to last year reviewers' comment, it's clear that this team has developed a very detailed and robust model.
- Understanding of problems associated with SOFC is good.
- Excellent collaboration team.
- Excellent relevance of the topic area.
- Building of MARC stress model for task 2 involving vibration. Coordinating with University of Illinois, SECA Dynamic modeling efforts for BOP. Obtaining vibrational input information from PACCAR.

Weaknesses

- The control system modeling is too simplistic, not based in reality, and will likely not be useful to a truck manufacturer.
- The SOFC seal element is not developing any new insights, is not based on the real needs of a truck application (no real data yet), and is not likely to be of much value to a SOFC manufacturer.
- This project is superfluous given the two other SOFC APU projects with truck manufacturers building and demonstrating working applications.
- The relation with Hydrogen Program.
- More focus required on the development of the general analytical framework.
- More focus on a dynamic frequency based model coupling input frequency, resonance and damping as well as determine dynamic load factors.

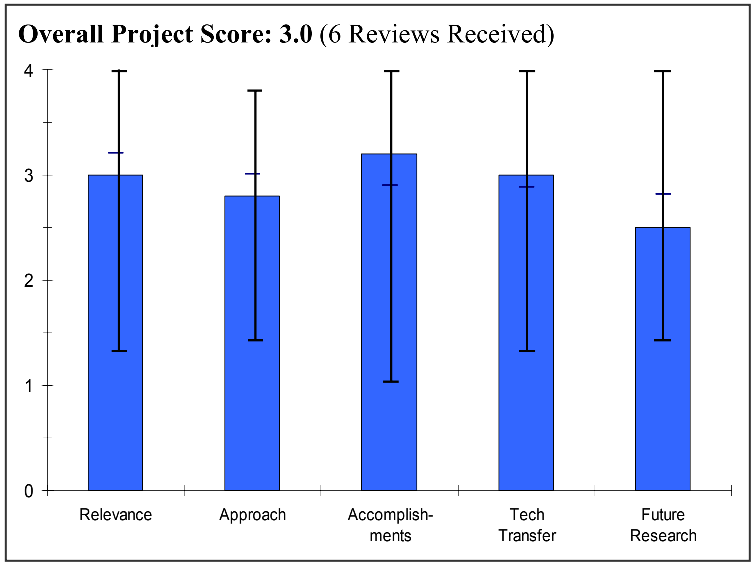
Specific recommendations and additions or deletions to the work scope

- Recommend termination of this project.
- Duplicative with projects FCP 30 and 31 with truck manufacturers.
- No unique or valuable insights likely from this work.
- Specify the handling of the exhausted gas.
- Specification of SOFC type (reforming diesel, direct diesel oxidation, etc) and consider it in the system model.
- Safety code, standards, etc.
- There are two SOFC APU projects for heavy duty truck applications which were recently awarded by the HFCIT Program. PNNL should attempt to collaborate with these projects.

Project # FCP-12: Corrosion Protection of Metallic Bipolar Plates for Fuel Cells
Turner, John; National Renewable Energy Laboratory

Brief Summary of Project

This project at the National Renewable Energy Laboratory (NREL) is concerned with the identification and characterization of metal alloys and coating for application to PEMFC bipolar plates. This work includes determining corrosion rates in simulated anode and cathode environments, measurement of interfacial contact resistance, and analytical determination of the passive film composition. NREL will correlate these results with the composition of the base metal alloy and the coating (if any) looking to identify more stable alloys and coatings to provide low interfacial contact resistance. A major portion of this work is in collaboration with Oak Ridge National Laboratory.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Low-cost bipolar plates with required durability are needed for PEM fuel cells to meet DOE cost and performance targets.
- Addresses barriers of stack cost and durability.
- Metal plates with rugged surface treatments may offer manufacturability advantages over carbon plates.
- Claim cost reduction, but will metallic plates really be cheaper than carbon?
- Based on raw material costs, not an obvious benefit to metal, especially if need special alloys or surface treatments that add cost.
- Manufacturing processes (e.g., stamping) do already exist, but molding of carbon composites is also available.
- Has anyone done cradle-to-grave comparison?
- Size is a benefit, due to superior metals strength.
- Narrow focus on one aspect of reduced stack costs.
- Research is focused towards meeting performance requirements of HFCIT MYPP. Metallic bipolar plates offer a number of inherent advantages.
- Project is well directed towards development of low-resistance, low-corrosion coatings on affordable materials.

Question 2: Approach to performing the research and development

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

- A logical, systematic approach has been developed to identify metal alloys with and without corrosion protection coating that have required durability at acceptable costs.
- This line of research necessitates more perspiration than inspiration (to use Edison's distinction), and the presenters have been diligent in acquiring needed data.
- Measuring corrosion currents and ICR is good.
- Measuring ion concentration in PEMFC (slide 17) is good.
- Do not understand why coating process used by PV industry is considered to be "low cost" although alloys tested (e.g. SnO_2) are low cost.
- Why not show Sn concentration in slide 17?

- Why not compare ionic conductivity of membrane before and after exposure in slide 17?
- Why not include carbon-plate baseline in slide 17?
- Why is PEMFC exposure time just 7.5 hr?
- Why not conduct potential-cycle testing?
- Well thought out approach to addressing corrosion protection.
- Phosphoric acid environment is a reasonable guess of yet unknown high temperature PEM fuel cell environment.
- Project is focusing on resistance and corrosion barriers to metallic bipolar plates. Project would benefit from a more structured approach as to which alloys will be targeted and why. Project may be a little too broad looking at nitride alloys, coatings, and high temperature operation given modest funding levels.
- Generally good approach towards the problem. Has adequate care been taken to ensure that the ferritic stainless alloys proposed here are sufficiently ductile to be stamped into bipolar plates with all of the necessary channels? It's well and good to develop a coating with good corrosion properties, but if the material can't be economically formed, the good corrosion results would be for naught. The old DOE target table entry of 16 $\mu A/cm^2$ corrosion current should be thoroughly expunged - at least temporarily, it should be replaced with 1 $\mu A/cm^2$. Have the economics of the thermal nitriding processes been adequately looked into?

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

This project was rated **3.2** based on accomplishments.

- Good progress has been made on this project including performance and corrosion testing. The project is moving on to identify plate materials and coating suitable for higher PEM fuel cell operating temperature which may place new durability requirements on bipolar plates.
- Data shows trends towards DOE goals; too early to tell when diminishing returns will be reached.
- Good results to date, especially for funding received to date.
- Relatively new start (2004) but has quantified performance of a number of alloys. Research progress is measured against performance indicators. Project lacking in projected cost data for various alloys and coatings. Project should conduct cycling tests to be more representative of automotive cycles and extend duration of corrosion tests.
- Work with more affordable substrates seems encouraging, but the stampability of the AISI446 should be double-checked. The corrosion behavior after nitriding, while improved, still doesn't seem stellar. It looks like some effort has gone into characterizing the true nature of the protective films (oxy-nitride) but more thorough work along these lines, correlated with longer-term corrosion data, would give more confidence in the level of durability that could be expected from these materials.

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

This project was rated **3.0** for technology transfer and collaboration.

- Two National Laboratories are collaborating on this project and an industrial partner is involved in the fuel cell testing of prototype bipolar plates.
- An industrial partner interested in commercializing the developed bipolar plates is needed.
- Collaboration between NREL and ORNL provides some advantages.
- Project has significant publication record in journals, conferences, and a patent disclosure.
- Good; already working with both stationary and transportation fuel cell OEMs (e.g., Plug Power and GM, respectively).
- Numerous papers and presentations.
- Relatively little collaboration only ORNL and Plug Power. Not clear as to exactly what NREL's role is with regards to evaluating nitrided alloys - ORNL has been doing this for several years.
- Good collaboration between two National Labs. Looks like a productive interaction with Plug Power may give some results useful for stationary, but with the danger of diverting efforts away from materials useful for automotive applications.

Question 5: Approach to and relevance of proposed future research

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

- Plans for completing this initial phase of the project appear to be adequate and reasonable.
- This project is intended to investigate one plate strategy, so contingencies and off-ramps are not much an issue.
- Many good ideas remain to be pursued, and the presenters intend to pursue them.
- Not especially clear, especially for a program that is at this level of completion.
- Recommend conducting testing with repeated potential cycling, since this is realistic for transportation applications, and this could be a real issue for these materials (i.e., analogous to Pt dissolution with potential cycling).
- Proposed timeline and long term focus for future not laid out. Clear decision points not established.
- Excessive emphasis on high temperature. Better focuses would be on durability under normal operating conditions of a material that can be readily formed.

Strengths and weaknesses

Strengths
- The national laboratory collaboration in this project is impressive utilizing the strengths of both labs.
- Testing a variety of materials with different treatment approaches and compositions.
- Focusing on direct need for low cost, high performance bipolar plates. Has quantified performance (resistance and corrosion tolerance) of a number of alloys is relatively short time.
- Some successes in corrosion protection, through nitriding, of iron-based alloys.

Weaknesses
- Could more clearly explain justification for these materials, and the processes required, and why they are projected to be low cost?
- Could do more analysis and testing of materials tested.
- Could provide theoretical justification(s) for the materials and coatings that are being tested (e.g. why is it though that they will be more stable under the relevant conditions?).
- Could benefit from a more structured approach for determining potential future alloy candidates. Project is not very clear on future cost projections. Project may be too broad (nitriding, coatings, high temperature operation) at this point.
- Not all the corrosion rates are all that low. Excessive emphasis on high temperatures and on an electrolyte that throws away all but one-third of the activity of Pt for ORR.
- Make sure materials are processable before investing a huge amount of time on corrosion protection.

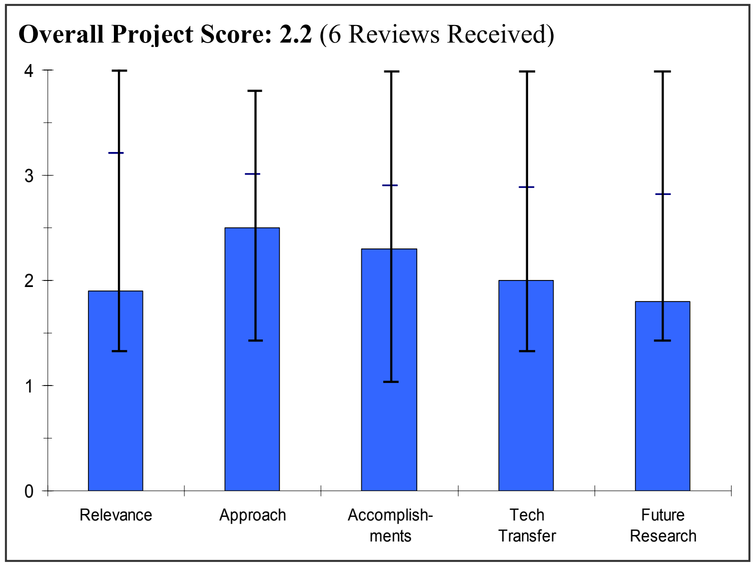
Specific recommendations and additions or deletions to the work scope

- Identify and involve a commercialization partner.
- Include repeated potential-cycling testing to simulate real-world conditions!
- DOE should consider undertaking a cradle-to-grave comparison of metal and carbon bipolar plates to obtain a better understanding of the potential benefits (e.g. which takes less energy to produce or recycle?). The auto companies certainly have a bias towards metal plates, but other than the strength/size benefit is this bias really justified or is it primarily due to the auto industry's familiarity with metal and metal processes?
- More clearly define role of ORNL and NREL with regards to nitriding. May be beneficial for NREL to focus on coatings. High temperature operating regime not clearly defined yet therefore may be best to delay this area of work. Additionally, setting operating regime at 150-170°C seems high - 120-140°C may be more appropriate.
- Avoid excessive emphasis on the polyphosphoric acid >150°C environment. Wasn't enough materials development work done on that back in the PAFC days? This topic is of possible interest only for stationary. Can one innovate to cut down the costs of nitriding?

Project # FCP-14: Fuel Processors for PEM Fuel Cells
Thompson, Levi; University of Michigan

Brief Summary of Project

The overall goal of the University of Michigan project is to discover and develop new material and reactor concepts that will enable the production of low-cost, high-efficiency systems to convert hydrocarbons like gasoline into hydrogen. These new materials will include high capacity sulfur adsorbents for liquid fuels, and high activity and durable autothermal reforming, water gas shift and preferential oxidation catalysts. Thermally integrated microsystem-based fuel processors incorporating these materials will be demonstrated and evaluated against key DOE performance and cost targets.



Question 1: Relevance to overall DOE objectives

This project earned a score of **1.9** for its relevance to DOE objectives.

- On-board fuel reforming had no-go decision. This project does not look like a cost-effective method for stationary-distributed hydrogen generation.
- This project lays out DOE technical targets for fuel processors, but as was evident from last year's review comments, it still is not clear what technical targets are being addressed in this project. The message has to be better communicated in both the written slides and presentation.
- The decision to eliminate on-board reforming limited the relevance of this program.
- The relevance of the project to the DOE program objectives decreased when a no-go decision on on-board fuel processing was made. However, some of this technology may well migrate to stationary fuel cell applications.
- Gasoline reforming is no longer a priority.
- The project does not have path forward for meeting current hydrogen fuel purity specifications.
- Reformer design is more appropriate for on-board reforming.

Question 2: Approach to performing the research and development

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

- The low-cost material approach to sulfur adsorbents for liquid fuels, and catalysts is excellent.
- Plate reactor systems do not appear to be able to come close to fuel processor start-up time and energy targets.
- Again details are missing as to the particular approach being used to develop high performance, low cost materials. Was this just going to be a vendor engineering project to selecting catalysts and then just test? Or was there a specific technical approach used as to what materials were focused on? What methodology was used to select/ rationalize cost effectiveness of catalyst candidates? A poster effort was described as development of catalyst materials, but approach to doing this is ambiguous. Was this just the usual suspects?
- One of the objectives is to develop low-cost materials that are also high performance. The approach to investigate individual components of the reformer rather than an integrated system is sound. That said, at some point an integrated design must be developed because the assembled parts do not appear to be well integrated with each other.
- The approach was reasonable at the time the project was initiated.
- Smaller, integrated units are also desirable for stationary systems.
- Catalyst and sulfur sorbent work is relevant to DOE goals.

- Difficult to compare autothermal reformer and water gas shift catalyst performance to current industry standards due to novel reactor design.
- Selective oxidation of CO cannot meet current DOE CO specifications.

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

This project was rated **2.3** based on accomplishments.

- O/C ratio of one is too high for high efficiency system.
- Adding precious metal (Au and/or Pt for water gas shift) is not a low cost approach.
- The PrOx catalytic wash coat seems to be far away from what is required and where other systems are at.
- Autothermal reformer non-noble metal catalysts look to have promise -- should do more development, testing and evaluate durability.
- A quantitative improvement in catalyst performance is not clear from the data presented. The activity of the autothermal reformer, water gas shift, PrOx is claimed to be enhanced, but by how much and against what standard? How is the 78-80% energy efficiency goal of fuel processors to be impacted by this work? The fuel to which this work is applicable is not clear. Since a microvaporizer was designed and demonstrated, this implies a liquid fuel. Why?
- Micro-channel reactors were described as being off-the-shelf??
- The program has achieved reasonable improvements but falls short of main targets (SU energy, SU time, etc).
- The program is ending. The University of Michigan autothermal reformer catalyst performs as well as the Sud-Chemie catalyst, at least over the first 10 hours of operation. University of Michigan is still having difficulty in bringing the CO concentration down to 10 ppm at the PrOx outlet.
- Predictive model was developed.
- There is no evidence of significant cost, performance or durability advantage.
- Amount of work performed has been good, but is in the wrong direction.

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

This project was rated **2.0** for technology transfer and collaboration.

- Interactions and tech transfer are limited.
- Publication record is good.
- Sud Chemie and Osram Sylvania are mentioned, but this interaction does not appear robust from the data presented.
- Program could benefit from collaborating with a fuel cell developer.
- Collaboration with other organizations appears limited. University of Michigan should have more interactions with the OEMs given the proximity to Detroit. There has been some interest by catalyst developers in select results.
- There is little apparent interaction except with catalyst suppliers.
- University of Michigan needs to get industrial and DOE feedback to prevent continuing down wrong path.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.8** for proposed future work.

- Plans are to build a 1 kW system in FY06, but will not lead to results that help anyone.
- The project should concentrate on fundamentals.
- The goals are good, but deficiencies up to this point in communicating results clearly need remedying. Clear, quantitative improvements need to be demonstrated.
- For stationary systems, microreactors have limited benefits.
- The project is finishing this calendar year. A 1-kW demonstration is planned.
- Not applicable, since project will apparently not continue beyond 2005.

- University of Michigan must incorporate path to reaching DOE hydrogen quality specifications.
- University of Michigan must compare catalyst formulations to currently available commercial catalysts.

Strengths and weaknesses

Strengths
- Catalyst development, specifically non-precious metal catalysts and testing, appears to be the strength of this program.
- Enhanced performance of autothermal reformer, water gas shift, and PrOx catalysts is claimed.
- Microsystem fuel processor design is interesting.
- Initial approach.
- There is a large group of collaborators with expertise in multiple disciplines.

Weaknesses
- Catalyst durability is not established.
- System efficiency is ~ 68% and 51%. This efficiency is too low for nearly all applications.
- It is still not clear what technical targets are being addressed in this project. Criticism on goal ambiguity of last year is still evident.
- Particular approach being used to develop high performance, low cost materials is not clear.
- Quantitative improvements in catalyst performance are not clear from the data presented.
- How is the 78-80% energy efficiency goal of fuel processors to be impacted by this work (needs better rationalization)?
- Message and impact has to be more effectively communicated in both the written slides and the presentation
- The overall approach has not demonstrated substantial improvement in the technology.
- It is difficult to see this reformer meeting the DOE weight targets. The poster did not address costs even though that is stated to be one of the project objectives.
- There is too little concern with durability and cost.
- University of Michigan has not adapted to change in emphasis away from on-board reforming to high purity hydrogen.
- University of Michigan needs industrial input.

Specific recommendations and additions or deletions to the work scope

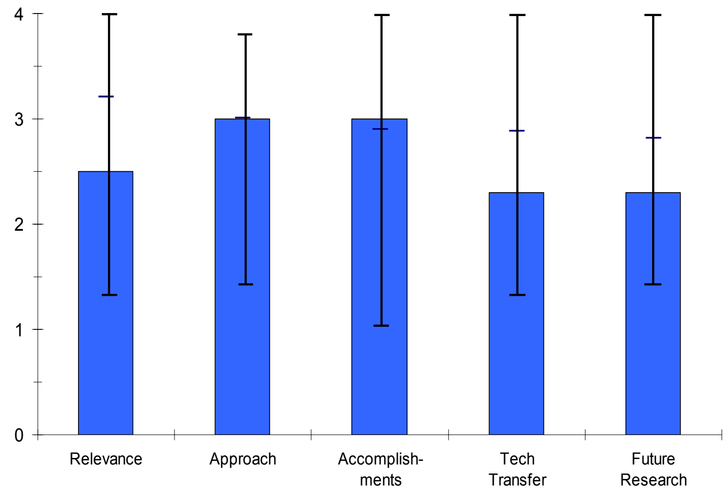
- I think this project would make better progress in concentrating on high performance autothermal reformer catalysts and examining catalyst durability, rather than building a 1 kW system.
- An integrated system design should be started as soon as possible. It doesn't appear that this will be done until the last quarter of the project when the 1 kW prototype is made.
- Focus on catalyst development and compare new and commercial catalysts for activity, specificity, durability, and likely failure modes.
- Drop PrOx from reactor design and consider how to do energy integration with purification methods such as hydrogen selective membranes or pressure swing absorption.

Project # FCP-15: Plate-Based Fuel Processing System
Yee, David; Catalytica Energy Systems, Inc.

Brief Summary of Project

Catalytica Energy Systems, Inc. is conducting this project to develop new catalytic reactor designs and reactor technology for processing hydrocarbon fuels to PEM quality hydrogen through developing advanced catalyst materials compatible with these reactor systems; designing and fabricating prototype steam reforming units in the 2–5 kW(e) scale; demonstrating steady-state and transient performance; and evaluating rapid start-up performance.

Overall Project Score: 2.7 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- On-board fuel reforming has no-go decision. This project does not look like cost-effective method for stationary-distributed hydrogen generation.
- The decision to eliminate on-board reforming limits the relevance of the program.
- The reforming is very important component of fuel cell.
- More relevant for transportation systems but also possibly for stationary.

Question 2: Approach to performing the research and development

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

- Plate reactor systems do not appear to be able to come close to fuel processor start-up time and energy targets.
- The objectives and approach are well defined.
- The doer really knows the problems, methodology and systematically approaches the goal.
- Such novel approaches needed.
- Seems reasonably well thought-out and executed.
- Potential scale-up and cost issues not considered.

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

This project was rated **3.0** based on accomplishments.

- Good reduction in T-operation and S/R ratio.
- Will not meet targets for on-board reforming.
- By utilizing existing HCAT exchanger plates, the su times and energy was substantially above targets (su-start up).
- The reformer catalysts show 10-times improvements. Cost is going down several times.
- Progressive is impressive.
- Proven advantages in CO reduction.
- Potential (not proven) advantages in start time.

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

This project was rated **2.3** for technology transfer and collaboration.

- The doer is practically doing everything by itself. The results are not shared or disseminated.
- Little evidence of significant collaboration or technology transfer.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Program closing out.
- This is the end of the project due to the no go decision even though there might be good potential for products.
- Little detail for future plans.
- Need to be more specific than "other possible applications"

Strengths and weaknesses

Strengths
- Strong theoretical and practical know-how. The doer is doing already business and R&D with catalysts and reformers.
- Novel approach.
- Apparent good understanding of catalyst issues.
- Potential to make steam reforming more practical, especially on a small scale.

Weaknesses
- Improved catalyst durability (~ 40 hrs) a long way from what is required.
- Oxidative regeneration of WGS catalyst will be difficult for a commercial application.
- Program does not include methods to eliminating sulfur compounds or other impurities. Program does not include a detailed cost analysis.
- Relatively small company doing everything by itself.
- Primarily directed towards transportation.
- Need more consideration of durability and cost issues.

Specific recommendations and additions or deletions to the work scope

- If applications for ONR and TARDEC exist, they should fund program, not EERE. Does not appear to fit into EERE program.
- This project could have good potential for a good and relatively small and cheap reformer.
- Concentrate more effort to a similarly novel approach to PROX.

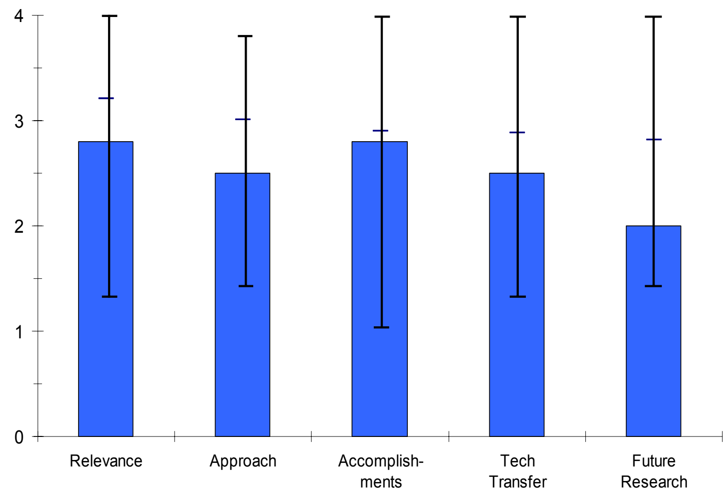
Project # FCP-16: Application of Advanced CAE Methods for Quality and Durability of Fuel Cell Components

Kelly, Ken; National Renewable Energy Laboratory

Brief Summary of Project

This project will work directly with fuel cell industry leaders to accelerate implementation of fuel cell technologies by integrating the latest computer-aided engineering (CAE) methods with advanced design techniques to overcome key manufacturing and technical barriers to improve quality, reliability, and durability of fuel cell components. Over the past two years, the National Renewable Engineering Laboratory (NREL) has worked with fuel cell industry representatives to successfully apply advanced CAE techniques to address specific fuel cell design hurdles. The application of these techniques has led to a number of design innovations and helped to improve product development time by reducing the number of component prototypes.

Overall Project Score: 2.6 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Excellent. Computer aided engineering (CAE) modeling project targets is improving durability, life-time costs of PEM fuel cells. Such analysis is now used in other industries to shorten product development lifetimes.
- This analytical work should be helpful to developers for cell and stack improvements.
- This work is supposed to improve cost and durability, but no evidence of significant impact on these barriers was presented.
- How does demonstrating that CAE is useful in designing components, which is already known, help us meet the DOE targets?
- Manufacturing variability will be an important issue when fuel cell designs are fixed.
- This project appears to be application of modern, but not to-the-purpose-developed, tools to the solution of specific developmental problems. It is not clear how this falls within the mission of government-sponsored-work as pre-competitive advanced research.
- It seems questionable whether the determination of proper bolt patterns for a specific developer's product is a proper use of scarce and expensive national lab resources. If p. 12's "innovative design concepts" could be more completely explained, perhaps the research content would be clearer.

Question 2: Approach to performing the research and development

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

- The approach is good for shrinking design times and achieving optimal architectures. NREWL uses CAE to develop robust designs of fuel cell components that take into account variations in material properties, loads and manufacturing processes. The reviewer is not sure that the dynamic variations of physical or chemical properties inside an operating stack can be taken into account for this optimization.
- Many of the important cell and stack parameters seal design, flow field design and the compressive forces needed for good performance and endurance are being addressed.

- This work is not generically applicable (i.e. it is not fundamental) and therefore does not address the barriers in a universally applicable way.
- Results appear to be limited to the optimization of specific designs and are not useful to others.
- At best, this project just demonstrates that CAE is a useful tool for designing complex products, which is widely known.
- CAE is approach vital to reduce costs.
- The project consists of many unrelated modeling efforts.
- Coupling manufacturing variability data with PEM is a good approach to characterize performance impact.
- P. 5, "Approach" lists the tools that were "utilized" (not developed) in this work. If new tools with unique functionality were actually being developed, then one could better argue that this work lies within the mission of the national labs.

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

This project was rated **2.8** based on accomplishments.

- With a relatively small amount of money, this project has already gotten results on best sealing designs and designs that minimize crack formation of BSPs during manufacturing. NREL is on the cutting edge with results on Plug Power high temperature stack.
- It appears the results obtained in stress analysis have helped a developer improve PEMFC performance.
- Progress, as expected, is limited to the optimization of specific designs with no real and new generic learning that can contribute to the optimization of other designs.
- NREL provided gasket failure and sealing recommendations used by Plug Power.
- The presented work is more data collection intensive than technology intensive. The techniques required for the analysis are well established. Progress is good with respect to analysis development and collection of a minimum level of data.
- The work appears to show a high level of technical competence.
- The customers for the individual subprojects were likely very pleased to have their detailed design work done for them.
- The use of modern design tools was taught to some of a customer's engineers.
- The work advances details of particular pieces of hardware and improves the design skills of developers who have sought such aid. It is not clear that it makes significant progress towards general advancement of the fuel cell field.

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

This project was rated **2.5** for technology transfer and collaboration.

- NREL has very effective interaction with Plug Power on next generation, high-temperature system. NREL conducted 2-day workshops on CAE Methods to get word out. Dispersion of this knowledge will help lower lifetime costs of fuel cell systems as manufacturing emerges for this industry.
- More developers other than Plug Power need to be involved.
- Interaction is limited to Plug Power, to date.
- There is no evidence that significant interaction with others is planned.
- There is plenty of National Lab/University collaboration.
- NREL is working, so far, with only Plug Power.
- Get more developer contacts to ensure that the CAE model gets used.
- Hydrogen storage safety plans should be reviewed with H_2 storage tech team and with Codes and Standards Tech Team.
- Work appears focused on a single company. Collaboration should be expanded.
- Full coordination exists, but the lab is doing industrial firefighting and training in advanced firefighting.
- Technology transfer from federal programs generally should be at the verified-concept level, not at the perfected-detail-of-design level.

Question 5: Approach to and relevance of proposed future research

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

- This relatively small project should maintain a focus on fuel cell issues and not scatter its resources working on hydrogen storage at this point.
- This work should concentrate on cell and stack designs to improve performance and life.
- Plan is to just continue aiding Plug Power with optimization of specific components.
- NREL provided no evidence that generically-applicable learning will result from this work.
- Project is too unfocused for amount of funding it receives.
- Pick 1-3 areas for CAE, and do those.
- NREL is trying to cover all areas with only $200K funding.
- The effort should focus more on establishing functional/parametric relationships between assembly/manufacturing & performance. The technique presented will be useful only if very precise relationships are developed between the assembly/manufacturing and its impact on the critical fuel cell performance and reliability issues. The future work appears focused on continued analysis methodology development and creation of assembly/manufacturing distributions versus focus on the performance and reliability relationships.
- Future plans look like more of the same. The apparently considerable skills of the project's participants should be brought into play at a higher level of abstraction – to identify broad design concepts with general utility or to develop (not just apply) innovative design tools with unique general functionality.

Strengths and weaknesses

Strengths

- This presentation shows excellent development of computer aided engineering (CAE) modeling to improve durability and life-time costs of PEM fuel cells. Such analysis is now used in other industries to shorten product development lifetimes.
- The approach is good for shrinking design times and achieving optimal architectures. NREL uses CAE to develop robust designs of fuel cell components that takes into account variations in material properties, loads and manufacturing processes.
- Project has already gotten results on best sealing designs and of designs that minimize crack formation of BSPs during manufacturing.
- The project is on cutting edge with results on Plug Power high temperature stack.
- Methodology is being dispersed through in-depth training courses.
- NREL completed a decent amount of design work, for Plug Power's benefit, with funding level available.
- CAE modeling is valuable.
- Relationship with Plug Power is vital to ensure that the model and its results get implemented.
- The analysis methodology is being developed.
- The project participants appear to be very skilled at bringing computer design tools to bear on specific design problems.

Weaknesses

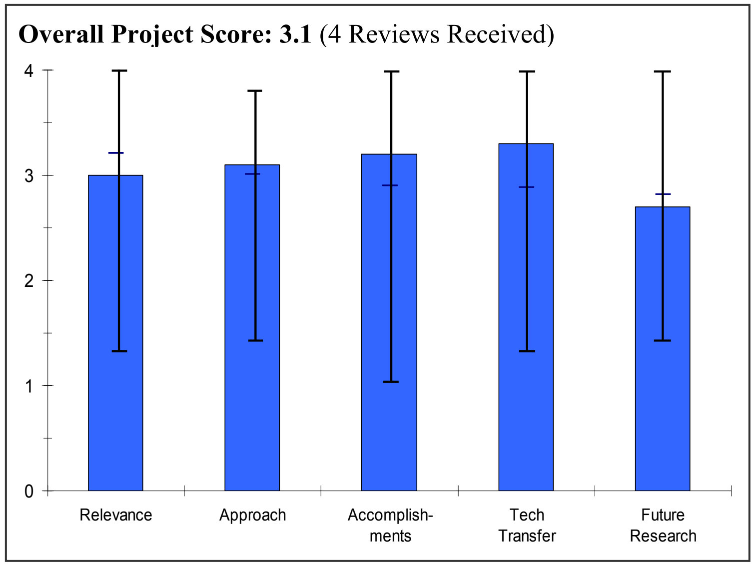
- The small project should maintain a focus on fuel cell issues and not scatter its resources working on hydrogen storage at this point.
- Can dynamic variations of physical or chemical properties be taken into account for this optimization that result from operation inside a stack?
- This appears to be an excellent example of "corporate welfare," since only one company (Plug Power) is benefiting from this government-funded program.
- Unfocused. NREL needs to pick 2-3 areas for deeper analysis.
- Collaboration with industry is not sufficiently expansive.

- Work does not focus on the most important issue, functional/parametric relationships between assembly/manufacturing and performance/reliability, it is focused on providing an analysis methodology which doesn't represent a significant advance in technology (i.e., the analysis methodology is available without a significant effort to fit it to the application).
- The project has not been set up to broaden the conceptual basis through which the national labs can best contribute to the technical and economic advancement of the nation.

Specific recommendations and additions or deletions to the work scope

- The small budget for this project should be increased based on good results obtained so far.
- Focus on cell and stack designs and configuration. Involve more developers.
- In lieu of continuing the "subsidized-design work" as planned, ask the PI to produce a report that explains what generically-applicable learning's have been generated to date. If generic learning has resulted, then these should be put in a format that can be utilized by the industry (e.g., a paper, presentation, or design manual). This documentation is the best hope of generating something that is universally useful from this work. Before continuing additional design work, an outline of the generic learning that might result from this work should be provided. If there are no generic learning offered, then stop this work.
- NREL should participate in upcoming fuel cell manufacturing workshop and use that as a guide to focus and reduce the number of different modeling tasks planned for FY06.
- Expand industry collaboration and focus on establishing the relationship between assembly/manufacturing and performance/reliability.
- A national lab is not a parts design shop. Pursue a concept rather than a part. At a minimum, distill design rules with general applicability out of work to improve particular pieces of hardware.

Project # FCP-17: Residential Fuel Cell Demonstration by the Delaware County Electric Cooperative, Inc.
Schneider, Mark; Delaware County Electric Cooperative



Brief Summary of Project

Delaware County Electric Cooperative, Inc. will validate objectives of propane-fueled hydrogen fuel cells for edge-of-grid residences via a field trial demonstration to understand the technical and economic viability of fuel cell alternatives to new line construction. Specifically, they will measure and report technical performance, provide raw cost data and economic viability analysis, document maintenance and operations concept enhancements specific to residential fuel cells, share safety related vulnerability analysis and lessons learned, and promote education of state and local consumers.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- The project strongly supports need for real world experience.
- This is a demonstration project using available commercial products; there is little if any technology development component of the project.
- Project provides some visibility on current state of stationary fuel-cell power sources, which is that they basically work, are not yet cost effective, and heavy loads like hot tubs require some compromise.
- Project neither helps nor hinders the President's objective of achieving efficient, cost-effective, "no excuses" hydrogen energy options.
- This is a very concrete systems project with clear targets.
- The project is of relevance to the stationary fuel cell initiative (but limited to single home). There is strong interplay of load-sensing and controls.
- The technical program is well-designed and if executed well, it will show the pros and cons of fuel cell systems for home use.

Question 2: Approach to performing the research and development

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

- Data gathering allowed for sensible design and load control strategy. May need to have more direct involvement by Plug Power and other suppliers for support --maintenance and operation optimization.
- This project is not about "technical barriers."
- Within its scope, gathering data about home electricity consumption and electrical power quality, the project is well designed.
- Project generates voluminous data, which is only of marginal value or relevance to other R&D.
- Project could improve by articulation of how data generated would influence future course of hydrogen technology: be specific with respect to decisions that must be made and how experiment is designed to develop the necessary data to support those decisions. ("Decisions" may be also taken to mean engineering design practices.)
- There are a lot of companies around the project sharing information and developing the system jointly.

- Very solid and methodical (including the "precaution" to remain grid-connected). Data gathering on load variations and possible load management is indeed appropriate.
- Excellent state of collaborators are participating, which will show the potential (or lack thereof) of future commercialization.
- The program is yet to start, but the depth and breadth of program are impressive.

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

This project was rated **3.2** based on accomplishments.

- Installation of complete data gathering hardware is a significant accomplishment, as is finalization of design.
- Project is making excellent progress in its own objectives of documenting residential power usage through extensive measurement.
- Project does not advance DOE technical objectives (e.g., efficiency, cost reduction, reliability improvement) beyond its value for “show and tell.”
- It is difficult to say after 6 months design phase.
- Program has yet to begin but as a system demonstration the plans are well laid out.
- The program just started, but the plan to run off fuel cell and storage (with minimal grid connection) is bold.
- There is a good plan for thermal integration.
- There is a solid plan for data monitoring (including PQ event monitoring.)

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

This project was rated **3.3** for technology transfer and collaboration.

- Get strong commitments from hardware suppliers to work together to resolve any issues.
- PI makes a good effort to establish relationships with interested technical organizations, technology suppliers, and the local university.
- I would hope to see a publication record as the project moves forward.
- There are good number of companies and other organizations around the project.
- The stat of collaboration is excellent, including a rural electric cooperative.
- The project has strong public relations and outreach plans.
- There is a good understanding of the stakeholders involved.

Question 5: Approach to and relevance of proposed future research

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

- In addition to tracking performance, final report should include a cost benefit analysis of component size tradeoffs.
- This project is a closed-end, with little purpose for continuing beyond scheduled completion. (Little additional new information would be learned).
- It is difficult to say at this early stage.
- The technical program is sound, but plans for the economic analysis are unclear.
- The length of planned research may not sufficiently explore fuel cell reliability and durability.

Strengths and weaknesses

Strengths
- This is how early implementation is likely to really occur -- at small scale by relatively small providers - so it is a really good test of how this will work.
- The project provides a tangible example of fuel-cell technology in use.
- The project is very quantitative; the test house is well instrumented.
- The project has public-relations values.

- The project group is good, and the project has a practical and concrete approach.
- The project is well-planned with a strong set of stakeholders.
- The data gathering and monitoring plans are excellent.
- The choice of energy storage, fuel cell, and thermal integration is good.

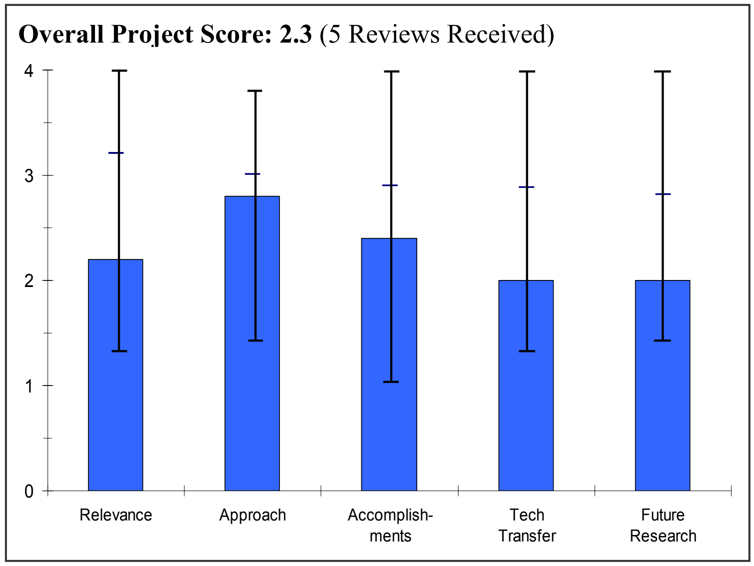
Weaknesses

- Company may be in over its head if anything serious goes wrong. Get strong commitments from hardware suppliers to work together to resolve any issues.
- The project generates little useful technical data to advance fuel-cell technology.
- The sample size of the study is 1 (house), limiting the value of conclusions that might be drawn from the study.
- Were the technology being demonstrated commercially viable, this sort of project would normally be sponsored by private sector.
- The energy storage specifics need to be identified.
- Scientifically this project is not very relevant.
- Data transparency to the outside world is needed.

Specific recommendations and additions or deletions to the work scope

- The project might play better as a "technology validation" project than as a "fuel cell" project. However, it would need to align to Technology Validation program goals.
- See how this system works and publicize the results.
- Ensure performance data (good or bad) is widely available to share fuel cell data broadly.
- A parallel effort with another fuel cell supplier may help develop further competitive data.

Project # FCP-18: Smart Fuel Cell Operated Residential Micro Micro-Grid Community Grid Community
Alam, Mohammad; University of South Alabama



Brief Summary of Project

The University of South Alabama project will build on the work of year one by expanding the smart control algorithm developed to a micro-grid of ten houses and will perform a cost analysis, evaluate alternate energy sources, study system reliability, develop the energy management algorithm, and perform micro-grid software and hardware simulations.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.2** for its relevance to DOE objectives.

- This demonstration project uses available commercial products. There is little technology development component of the project.
- Project provides some visibility on current state of stationary fuel-cell power sources -- that they are temperamental, are not yet cost effective, and cannot supply peak loads without compromise.
- Efficient, cost-effective, "no excuses" fuel-cell energy options are neither helped nor hindered by this project.
- Paper study of mini and micro grid meets DOE needs but lack of actual hardware demonstration will limit utility.
- This systems study is done theoretically. I think it will be outdated in few years.
- The program really expands upon the DOD/DOE demos with much more comprehensive data. Incorporation of energy management schemes is very good (also thermal integration).
- Project does not appear to be consistent with the DOE's Fuel Cell RD&D Plan (e.g., no milestones related to project, no tasks addressing this project).
- Value proposition of this micro-grid approach is very unclear and not articulated in the poster session.
- The "Education Value" of fabricated system architectures with an unclear business case that does not have appropriate industry collaboration (e.g., no housing developers) or support is highly questionable.

Question 2: Approach to performing the research and development

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

- Developing understanding of load leveling in micro/minigrids is useful. Need to compare cost/benefits of micro/mini to single house system.
- The project is a combination of theory and experiment.
- The experiment demonstrates a 5 kW residential fuel cell electric power generator – there is little new there.
- The theory models neighborhood minigrids using hypothetical layouts and reliability and load models.
- The theoretical approach is generally well thought out, leading to an algorithm the author calls "smart control."
- This project occurs too soon on the technology development curve: it uses well-known engineering methods that break no barriers, and it would have more impact when commercial products have advanced to the point where actual value could be added by demonstrating engineering technique.
- Again, systems study should be done by practitioners, not based on theories.
- Systemic and comprehensive project moves up from single home to 10 residences -- good modeling.
- Cost estimation source and validity are not clear.
- Fuel cell stack/power plant costs do not appear in any cost analyses.

- Value proposition and business case for a micro-grid has not been articulated. The reviewer expects the business case will not be strong, even at low future projected cost for fuel cells (due to high degree of cooperation/collaboration among housing developers/homeowners/housing associations, fuel cell system providers, etc.).

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

This project was rated **2.4** based on accomplishments.

- Analysis of central hot water approach is good but what conclusions can be drawn as to cost versus benefit? Where will the reliability data for individual failure modes come from?
- The project makes good progress on its own -- self-defined goals (theoretical modeling).
- The project does not provide much progress toward DOE fuel cell development goals (efficiency, cost, reliability, etc.).
- There are no metrics on the improvement in “understanding and comfort” that the project aims to correct.
- The accomplishment seems to be software modules in a software platform and optimization algorithms. I think this is quite basic.
- Solid from a system integration viewpoint, but system reliability may be compromised by reliability of fuel cell stacks (and system). Need more data on weakness of fuel cell systems.
- Economic analysis claimed to be "complete," but results presented are quite simplistic (e.g. only total estimated cost presented, without sources provided, for thermal system only -- no payback time, comparison of alternatives, business case, etc.).
- Little or no analysis was shown for required capacity.
- Reliability analysis results were not shown for a grid-connected system, which should have much higher reliability.

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

This project was rated **2.0** for technology transfer and collaboration.

- Coordination seems limited to software development.
- PI characterizes most interactions as merely having “made contact.”
- PI has modest publication record from project.
- There is only one software company involved. Lack of equipment providers, utilities, constructors is a weakness.
- The project needs collaboration with local utility. No clarity on how different component suppliers are helping (or not) with the program.
- No collaboration with entities ultimately responsible for purchasing/installing such a micro-grid (e.g. housing developers, homeowners, homeowner's associations), which leads to a limited understanding of the market needs and business case.

Question 5: Approach to and relevance of proposed future research

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

- Not sure how load control at the appliance level will be managed to avoid consumer upset.
- This course of research is not timely or well-connected to the efforts to remove technical barriers, lower costs, and improve reliability.
- The model can be improved infinitely without any practical goals.
- The project has a strong academic approach to systems integration and interactions. Presentation does not show clear pathway to co-opting key commercial interests.
- Future work is vague, general, and not well articulated.

Strengths and weaknesses

Strengths
- University of South Alabama has an understanding of systems interactions.
- This educational effort may involve students in a fuel cell application engineering activity.
- The researchers are very enthusiastic.
- The project has serious systems integration approaches to understand component interactions and overall system reliability.
- The project is academically appropriate moving stepwise from a single home to multiple residences.

Weaknesses
- There is a lack of plans for hardware validation of models.
- The project does not provide high-value information of any urgency.
- There is a heavy reliance upon hypothetical performance data and models.
- The project lacks systems engineers, equipment providers, customers, or utilities.
- There are no clear collaborations with other key stakeholders (e.g. local utility or t&d concern). University of South Alabama needs to highlight more clearly the weak and the strong components (with the current available data) to enable focus on key hurdles to system commercialization.

Specific recommendations and additions or deletions to the work scope

- Focus on approaches to optimize load leveling in microgrid and compare microgrid approach to single residence.
- Broaden the project group by equipment manufacturers, systems providers, utilities, constructors, etc.
- Get other stakeholders involved. Expand the program with other collaborations (up and down the supply chain). The program has strong demonstration potential, but must extend beyond the university.
- Value of this project to the overall goals of the DOE Hydrogen Program is not at all clear to the reviewer. It seems to be a very large dollar value effort ($3.5M) with little potential return on the investment.

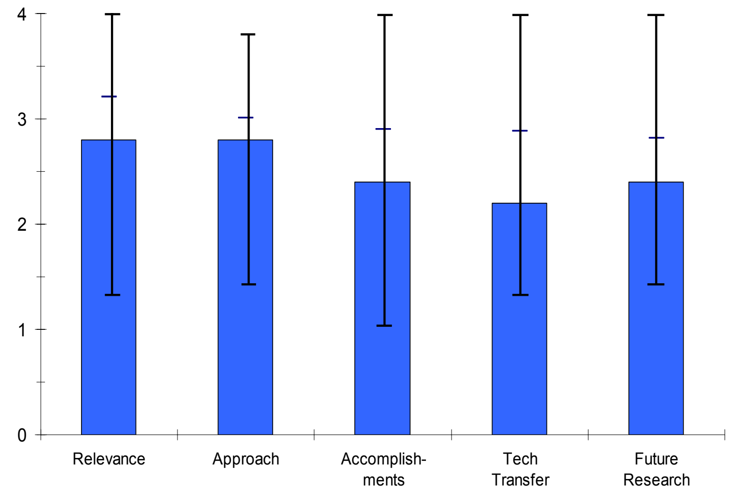
Project # FCP-19: Fuel Cells Vehicle Systems Analysis (Fuel Cell Freeze Investigation)

Pesaran, Ahmad; National Renewable Energy Laboratory

Brief Summary of Project

For this project, the National Renewable Energy Laboratory (NREL) will provide DOE and industry with technical solutions and modeling tools that accelerate the introduction of robust fuel cell technologies, quantify benefits and impacts of Hydrogen Program development efforts at the vehicle level (both current status and future goal evaluation), and highlight potential system level solutions to technical barriers.

Overall Project Score: 2.5 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Patent search and literature survey relating to fuel cell performance under freeze/thaw conditions.
- Fuel Cell Freezing Alternatives Investigation.
- This is a literature review program only. Literature reviews need to be done by developers who are trying to solve problems, not by third-party non-experimentalists.
- Project is very relevant to HFI and the DOE MYPP. Overcoming freeze/thaw barriers in an energy efficient and cost effective manner is essential to consumer acceptance of fuel cells.

Question 2: Approach to performing the research and development

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

- A systematic approach to quantifying the present state-of-the-art in fuel cell freeze/thaw operation.
- Started with extant patent literature to determine knowledge base and progressed to high level analysis.
- This program does not help the industry deal with freeze-thaw - it simply enumerates what has been done before.
- Excellent approach starting with comprehensive patent/literature search, narrowing of options, systems analysis for evaluation. Effectively leverages utilization of existing systems tools. PI effectively leveraging similar efforts with batteries and applying knowledge to fuel cells. Focusing on key issue of power at startup in 30 seconds. Provides comprehensive listing of issues facing freeze and rapid startup.

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

This project was rated **2.4** based on accomplishments.

- Significant progress toward accomplishing the objectives of the project.
- Duplicates ANL work in several cases.
- Analysis at high level only.
- No successful path developed.
- Reviewed older approaches.
- They have done what they said they would do - it just has little value.
- Relatively new start (FY04). Accomplished patent categorization. Pros and cons of various options identified. Estimated energy and power needs to thermal insulation method and for thawing.

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

This project was rated **2.2** for technology transfer and collaboration.

- Participated in workshop with industry, academic and laboratory organizations.
- Identified several organizations who seek more in-depth participation in the project and its findings.
- Need to add involvement of OEM participants.
- Need to draw conclusions and disseminate with industry comments.
- Didn't see any real collaboration - just listing of a few names.
- Needs more partners ranging from fuel cell system OEM to automotive OEMs.
- Utilizing University of South Carolina model for freeze/thaw minimizing new model development. Working with General Motors with regards to patent search analysis but additional emphasis should be placed on coordination/leveraging with other entities.

Question 5: Approach to and relevance of proposed future research

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

- Good plans for continuation of project to completion.
- In particular, recommendations of combining the identified methods to address operation under freezing conditions will be very useful.
- Must identify new approaches if existing approaches not successful.
- Test out brainstorm ideas.
- No pathways to leverage the data from the technology validation program.
- Future approach is clear and well developed.

Strengths and weaknesses

Strengths
- Appropriate approach to achieve desired results - analytical capabilities of NREL.
- Approach is objective.
- The team has appropriate tools to complete the objectives.
- Good patents analysis. Keep in mind that these are existing systems, not future system.
- Well defined, logical approach leveraging existing patent literature. Accomplishments reasonable to date.

Weaknesses
- Need to include system developers as sources for potential solutions and evaluate their ideas analytically.
- There is no research and development, only literature searching and mapping. This has very little value in solving the real problems in fuel cells unless is performed by those who are DEVELOPING solutions. This team is, apparently, not doing so, nor is it really closely connected with anyone who is.
- Need to collaborate with more systems OEMs and Automotive OEMs.
- Need to come up with potential issues that have not been patented.
- Project could benefit from expansion of partnership/ collaboration.

Specific recommendations and additions or deletions to the work scope

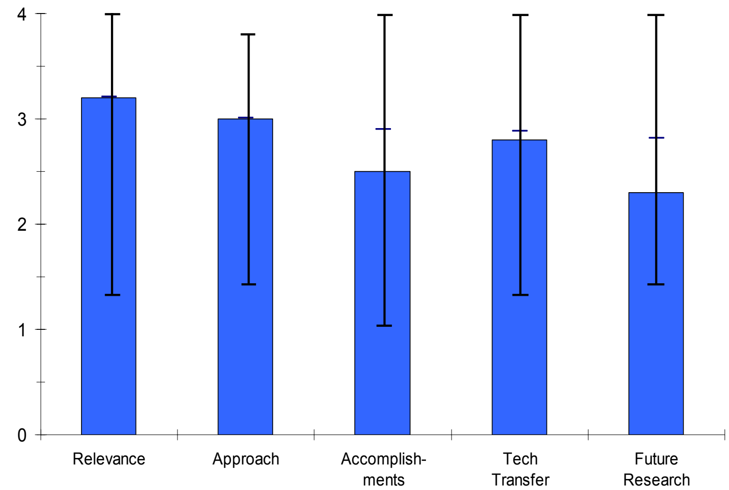
- Project scope needs input from system developers. Include stationary applications as well.
- End program. It is of little value and it costs a lot for what they are producing. I have paid commercial vendors to do similar work for 1/10th the cost and they can produce it in a few weeks to boot.
- If there is some political reason to continue funding, then REQUIRE the team to connect in REAL ways with a group or groups who are working on solutions to the problems being addressed.
- Need to leverage technology validation program data.
- Need to expand partnerships to build credibility to model.

Project # FCP-20: Graphite-based Components for Thermal Management in Fuel Cell Systems
Lara-Curzio, Edgar; Oak Ridge National Laboratory

Brief Summary of Project

Oak Ridge National Laboratory (ORNL) will develop compact, low-weight, effective thermal management components for fuel cell power systems using carbon-based materials. The objectives are to determine the feasibility of weaving high-stiffness graphite fibers into complex 3-D architectures; to assess the effect of fiber architecture on heat transfer, permeability and mechanical strength of woven fiber structures; to design, in collaboration with industrial partners, thermal management system components based on 3-D woven graphite fiber preforms.

Overall Project Score: 2.7 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.2** for its relevance to DOE objectives.

- Radiator sizing is an important issue.
- Graphite is an intriguing approach, but there is still some work to do to establish superiority over other metal-based compact HX technologies.
- Carbon-based heat exchangers.
- Thermal management is a significant factor in fuel cell automotive application.
- This technology, if successful, will yield an incremental improvement. But it is not a game-changer and doesn't have near as much impact as increasing stack temperature.
- Developing more effective heat exchangers is certainly important for FCVs.
- Effective heat transfer is a huge issue in fuel cell transportation systems.
- Novel approaches like this are badly needed.
- Very relevant to thermal management needs of fuel cell vehicles. In future, has potential to address system-wide (not just components) thermal management needs.

Question 2: Approach to performing the research and development

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

- Progression of project is rational, and prototyping supports vision very well.
- Articulation of design rules for these types of systems is important.
- Pressure drop needs to be considered more carefully.
- Many obstacles - need practical input from commercial radiator supplier and auto industry.
- Cut and try approach.
- Need more data.
- The team appears to be taking a competent, systematic engineering approach.
- While not stated, I assume the key will be the carbon fiber material cost. More needs to be said on this in the presentation. And if so, emphasis on material cost will likely need greater emphasis.
- This appears to be potentially promising route to developing an efficient and lightweight heat exchanger.
- I would like to see some analysis of cost, up-front, so that it is at least understood not to be a potential show-stopper.
- Approach is good but there is little evidence of effective modeling and simulation.

- There is too little consideration given to cost issues.
- Targeting sweet spot of optimized structure performance/cost. Excellent approach building off past ORNL experience with graphite fibers and leveraging expertise outside ORNL. Clearly addressing potential barriers of cost via 3D graphite structures utilizing graphite fibers with different properties in different directions. Working to address potential barriers of fouling via modeling.

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

This project was rated **2.5** based on accomplishments.

- Feasibility is established, pressure drop/effectiveness now needs to be more carefully assessed.
- Low cost manufacturing can be envisioned.
- Potential benefits not well defined.
- Many basic issues like durability and plugging not addressed.
- Stagnation effects on flat fibers may lead to plugging.
- More analysis required of ideal case to determine direction.
- Some progress shown in developing basic diagnostics and modeling.
- Presentation doesn't indicate progress in system performance or cost.
- Some good progress, but some thermal performance results would be a great addition, even if they are preliminary.
- Certainly good results for the funds spent.
- Very little information given on progress.
- 3-Tex has developed methods for weaving graphite fibers around tubular structures. Methods conducted for evaluating permeability and pressure drop of woven graphite fabric structures. Project will need a harder look at determining potential costs of these methods. Costs of fibers etc. have been ascertained but information has not assimilated for entire thermal management system of a component.

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

This project was rated **2.8** for technology transfer and collaboration.

- Would like to see collaboration with a HX manufacturer more than a fuel cell company.
- This project has a high value of return for the investment, and seems a very good licensing opportunity if successful.
- Some interactions shown-more needed.
- Should engage an automotive radiator supplier to leverage their design and manufacture capability and understanding of automotive thermal fundamentals.
- Commendable that the requirements were developed with the assistance of a fuel cell OEM (Ballard).
- If lab results are good, than more industry collaboration will be warranted, as planned.
- Little evidence of collaboration.
- Focusing on thermal management system for radiator using Ballard input is excellent. Leveraging of DOE/DER resources for modeling at University of Western Ontario is good. Relying on established textile manufacturer is good.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.3** for proposed future work.

- Need more definition and timescale with go/no-go points.
- Needs more definition.
- Correctly points to demonstrating a prototype to indicate possible advantages to the technology.
- Hard to judge at this point because it really depends on how promising are the initial thermal results.
- No evidence of current or future efforts to identify problem areas.
- No evidence that effective modeling or an analytical approach will be pursued.

- Future approach is logical and idea of extending to system-wide thermal management strategy spring boarding off inherent advantages of graphite woven structures is good. Project may need additional insights into optional paths should cost targets prove unattainable.

Strengths and weaknesses

Strengths
- Combination of theory, experiment, and prototype, all for very reasonable budget.
- Novel idea.
- Unique approach to an issue that needs to be addressed.
- Novel approach with real possibilities for success.
- Well defined logistical approach utilizing input from fuel cell supplier (Ballard) for radiator performance targets and leveraging of modeling (University of Western Ontario) and textile manufacturing expertise (3-Tex).

Weaknesses
- Lack of practical design rules (probably forthcoming as full scale prototype develops).
- Lack of HX company visibility as validator.
- Moving too slowly without demonstrated plan/schedule.
- Team would benefit from collaboration with company with design and manufacturing expertise.
- No thermal results yet.
- Not much seems to have been accomplished.
- No apparent plans for modeling/simulation.
- Cost is still an unknown.

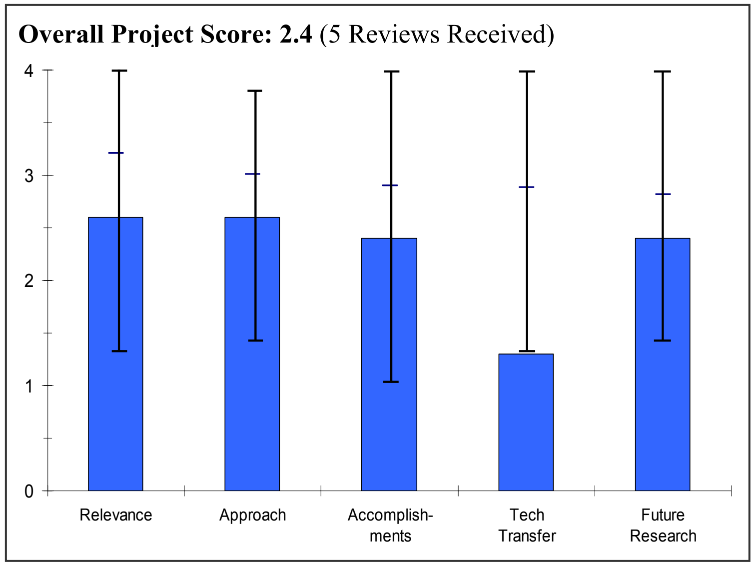
Specific recommendations and additions or deletions to the work scope

- Engage an automotive radiator supplier.
- Establish air side pressure drop and fan power targets (should receive from Ballard or auto radiator supplier).
- List anticipated magnitude of benefits (weight, volume reduction).
- Will galvanic corrosion between carbon fibers and Al tubes be an issue?
- Establish realistic milestones and schedules and incorporate modeling as feasible.
- More emphasis should be placed on ascertaining cost projections for various combinations of graphite fibers and weaving and bonding (epoxy) methodologies.
- In future, emphasize fuel cell system wide integrated thermal management strategy in concert with an OEM.

Project # FCP-25: Fuel Processor R&D
Ahmed, Shabbir; Argonne National Laboratory

Brief Summary of Project

ANL will study reforming of liquefied petroleum gas (LPG) for distributed fuel cell power by establishing the kinetics of propane reforming, both steam reforming and autothermal reforming, and addressing LPG reforming challenges, especially the effect of propylene on reforming and sulfur in LPG.



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.6** for its relevance to DOE objectives.

- Intersection of work and DOE goals not clear.
- Modeling work for LPG is not original or substantive, and seems like rehashing of prior work, and who is "customer" for the kinetics studies?
- Optimal thermal trajectory analysis for WGS is already known.
- Derivation of separation membrane specifications in WGS frame is more original, and helps push boundary of reactor design innovation.
- Presentation mentions barriers, but does not provide specific goals it is addressing.
- Fundamental kinetic test work is useful in many ways.
- Project #1. Efficient approaches to use LPG as a fuel are needed for implementing distributed generation in rural areas.
- Project #2. Improved and cost effective approaches for reducing the size and weight of WGS reactors are important for distributed hydrogen production when natural gas, propane and other hydrocarbons will be employed.
- Project #2. The single-stage WGS membrane/reactor concept is an intriguing one, but it's not clear that the approach can be cost-effective unless it eliminates all downstream CO cleanup steps.

Question 2: Approach to performing the research and development

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

- Modeling is, as usual, thoughtful and comprehensive.
- Design and prototyping of WGS reactor will be of technical interest, innovating combinations of technologies in single reactor embodiments has general value.
- Who is "customer" for single-stage WGS?
- Approach addresses kinetics and water gas shift objectives, but less on the reforming challenges.
- Experimental set-ups and test data are quite good.
- Would like to see kinetic data for components comprising >99% of LPG.
- Temperature-gradient WGS work similar to this project has been reported in literature several times. Not clear what this work adds to base knowledge.
- Project #1. The approach to reactor design and testing is sound.
- Project #2. A rational modeling and design approach is being pursued for the membrane reactor. The primary question is availability of WGS catalysts that are sufficiently active for the full advantage of the approach to be obtained.

- How is the efficiency of this whole process compared to ammonia? There is a vast use of ammonia in the rural regions.
- Good transition of work from CNG/Gasoline reformation knowledge base.

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

This project was rated **2.4** based on accomplishments.

- A little heavy on the analysis side.
- Project shows progress, but without specific goals or targets identified it is difficult to determine how much.
- Project has just started. Experimental apparatus is in place and working.
- Both of these projects are relatively young, so it might be a bit early for an assessment of technical progress. Based on results presented, it appears that both projects are on track.

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

This project was rated **1.3** for technology transfer and collaboration.

- Not clear who the "customers" are.
- This work would be helpful to establish DOE goals and specifications, but the intent of the prototyping efforts is not clear - commercial entities are doing this work.
- Project work appears to be performed by ANL exclusively.
- Did not see evidence of collaboration. Would be worthwhile to see tests of commercial catalysts in these test reactors and conditions to see if kinetics are same for materials with different precious metal loading and promoters.
- There is little evidence of outside collaborations on either project.
- Project #1. It would be useful to obtain input from stakeholders to provide directional guidance as this project proceeds.
- Project #2. There is a significant amount of membrane development work at other DOE laboratories (e.g. NETL). Collaborations with these other labs should be pursued.
- This project is new so this low score is probably not a fair rating.

Question 5: Approach to and relevance of proposed future research

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

- Future work should be better focused on more specific goals/targets.
- Good kinetic data on C_{2+} reforming is lacking as well as effect of sulfur on activity at low temperature.
- Project #1. The future plan is reasonable, with testing of "surrogate" fuels to assess effects of impurities on the reforming process. Testing on commercially available LPG fuels should be included in the matrix.
- Project #2. The technical aspects of the plan are sound. A manufacturing cost analysis of the membrane/reactor approach should be conducted, so that the economic viability can be confirmed.
- Should be better leveraged from previous work. Some of the proposed work seemed repeated from the previous years of work.

Strengths and weaknesses

Strengths
- Team capabilities in analysis.
- Lab capabilities.
- Well designed test equipment.
- Highly skilled personnel and excellent lab infrastructure.
- Both projects leverage a large experience base at Argonne.
- LPG conversion is a good leverage of previous gasoline reforming and CNG reforming work.

Weaknesses

- Lack of utility of results.
- Lack of customer and/or driver.
- Not enough details with respect to goals and targets for this research.
- Not sure LPG will have major role in stationary fuel cell power systems.
- Lack of collaborations and stakeholder input.

Specific recommendations and additions or deletions to the work scope

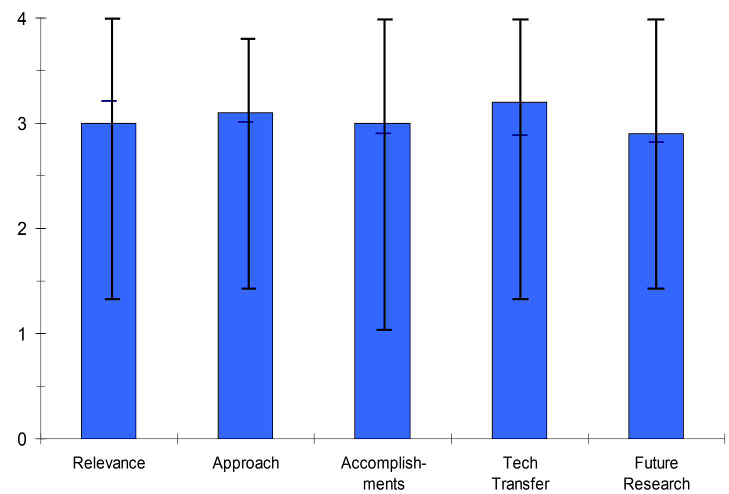
- Establish more detailed goals, aligning to DOE goals and structure research accordingly.
- Consider using same test equipment for obtaining kinetic data for reforming components of likely liquid fuels that will be used in stationary fuel cell power systems such as diesel or JP8.
- Need to compare such reformation process against ammonia which is vastly used in the rural area on an overall energy efficiency process.

Project # FCP-26: Fore Court Fuel Processing
Whyatt, Greg; Pacific Northwest National Laboratory

Brief Summary of Project

The objective of this Pacific Northwest National Laboratory (PNNL) project is to adapt the micro-channel steam reforming technology, previously being developed for on-board reforming, to station-based hydrogen production from natural gas. The micro-channel approach enables high rates of heat and mass transport resulting in substantial process intensification with improved efficiency. The current year's project will integrate a micro-channel steam reforming system with an adiabatic WGS reactor and Cu-Pd membrane separator to provide a demonstration of a highly efficient, small scale (5 kg hydrogen/day), hydrogen generation system.

Overall Project Score: 3.0 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Efficient hydrogen production for distributed power generation from natural gas as transitional approach is relevant.
- Distributed hydrogen is quite relevant and this project fits the plan elements for a hydrogen economy.
- The decision to stop on-board fuel processing limits the relevance of the program.
- Good potential for cost reduction and technology transfer to other projects.
- This project provides a path for cost effective and efficient natural gas fuel processors for distributed generation applications.
- The PNNL team has identified that their micro-channel technology provides significant technical advantages for steam reforming and is developing a fuel processor based on that platform.

Question 2: Approach to performing the research and development

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

- Builds on the micro-channel technology developed for on-board reforming. The application of this technology for compact small size application is relevant. The application for large scale does not seem as good a fit.
- Solid approach. All aspects covered- economics, modeling, prediction, hardware development, and testing.
- The use of microreactors has questionable benefits for stationary applications.
- Has adapted well to change in emphasis away from on-board reforming.
- Integration with PSA and membrane technology to meet DOE H_s quality specs is attractive.
- A rational approach to reactor and system design, modeling, testing and demonstration is proposed.

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

This project was rated **3.0** based on accomplishments.

- Modeling and flowsheets amplified, parallel evaluation of membrane and PSA as alternative approaches is good.
- Project has progressed well considering its length. Multiple paths developed and to be evaluated.

- The modeling effort was comprehensive and beneficial.
- Significant progress has been demonstrated.
- Good to see that it worked when reforming liquid fuels and integrated with PEMFC and Pd membrane.
- Demonstrated reduction in air pressure drop is important for reducing blower parasitic power.
- The design work conducted to date appears to be solid.
- Although it was confirmed that fabrication cost (and not materials cost) is the key to achieving cost targets, there was no discussion how the fabrication costs can (or will) be addressed.

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

This project was rated **3.2** for technology transfer and collaboration.

- Large number of interactions; need to focus where advantages of this technology are most applicable.
- Initial interactions constructive. Still early.
- Collaboration with a fuel cell system integrator would be beneficial.
- Several industrial collaborations were shown.
- PNNL has been extremely successful in transferring this technology to a range of fuel processing applications, and working to have the technology commercialized.
- Collaborations with a membrane manufacturer could provide significant benefit to the project.

Question 5: Approach to and relevance of proposed future research

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

- Adequately planned for the amount of funding. Reevaluate if 15,000 kg hydrogen/day is the most relevant application vs. focusing on small systems 5 kg hydrogen/day.
- Plan forward is in place. Continue to evaluate short term tests on small scale prior to 1500 kg/day.
- Program should include detailed cost analysis for scale up to 1500 kg/day.
- Planned scale-up to larger sizes good step.
- Technical aspects are sound. Cost analysis should be extended from raw materials to manufacturing costs.
- Depending on the results of the manufacturing costs, it may be necessary to direct some effort toward reducing fabrication costs.
- The cost of the membrane separator should be addressed.

Strengths and weaknesses

Strengths
- Working off of prior experience.
- Beneficial use of modeling effort.
- Good potential for reducing parasitic power and heat loss to environment.
- Working to integrate with PSA and membrane purification systems.
- Solid micro-channel technology platform around which to design and build a fuel processing system.
- Aggressive efforts aimed at transfer of the core micro-channel technology to other fuel processing applications.

Weaknesses
- Needs more detailed cost study for building larger scale reformer.
- Needs long term durability testing that include multiple start-up and shut-down cycles.
- The manufacturing cost may be prohibitive for large-scale distributed generation applications.

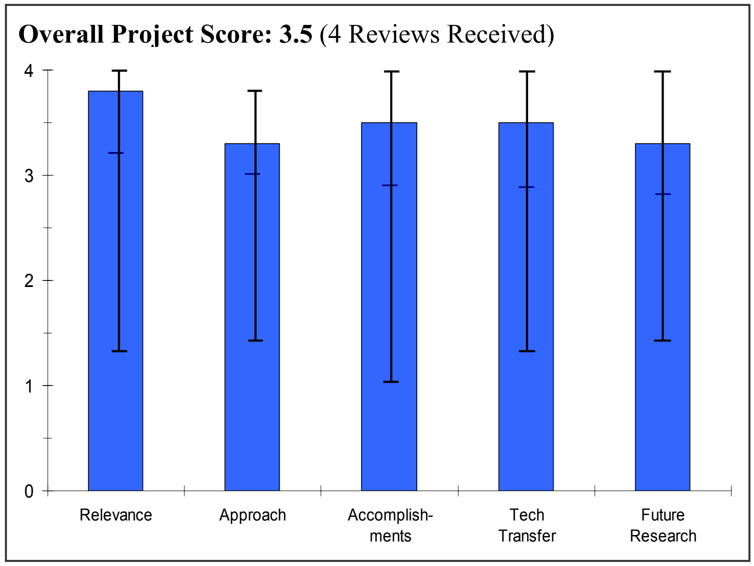
Specific recommendations and additions or deletions to the work scope

- Need to work with industrial partner to get better cost estimates for building multiple larger units.
- The cost analysis should be extended to include fabrication costs and the membrane.

Project # FCP-27: Component Benchmarking
Rockward, Tommy; Los Alamos National Laboratory

Brief Summary of Project

Los Alamos National Laboratory (LANL), in close collaboration with members of the USFCC from industry, universities and other government entities, has helped advance a collective effort to provide the polymer electrolyte membrane fuel cell industry with a standard test protocol defining a consistent, repeatable method for conducting a single cell test and generating a polarization curve. These efforts are intended to provide a comparison benchmark.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.8** for its relevance to DOE objectives.

- Common protocols for material evaluations are critical to progress.
- Standardization is a key milestone to the realization of any widely-adapted technology. Although this is an often over-looked activity, historically shown as essential to adaptation.
- This program provides the mechanism and procedures to establish the test protocols for comparisons of PEMFC testing.
- Component benchmarking is not specifically stated as a DOE objective in the RD&D plan. However, it is a necessary task and is relevant to the DOE objective of fuel cell development.

Question 2: Approach to performing the research and development

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

- Break-in may depend on specific materials - protocol should suggest when break-in is complete.
- DOE/LANL began the activity, and through member companies of USFCC effectively used the consensus approach with industry concerns to establish a procedure.
- The incorporation of statistical analysis is applauded.
- LANL has assembled a good team to help develop the SOP for comparative testing.
- The approach is good, but should be broadened, if possible with the limited funding, to include multiple MEA types and operating conditions. The conditioning/break-in procedure may be MEA-specific.
- Calibration of the instrumentation, or a standard calibration procedure, should have been established and conducted first prior to the round-robin testing.

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

This project was rated **3.5** based on accomplishments.

- Excellent progress in establishing protocol for initial performance and variability. Need to include cell assembly variability in protocol or establishing standard assembly protocols.
- The completion of a first phase of round-robin testing is a significant accomplishment.
- Adequate progress toward establishing the protocol is apparent.

- Good progress, but the MEA test results could have been more meaningful if a calibration procedure were established first.

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

This project was rated **3.5** for technology transfer and collaboration.

- May wish to involve ASTM or some other standard testing organization for assistance on establishing protocols.
- The ability to manage often conflicting interests of USFCC member companies and still obtain a standard test method with round-robin testing is a significant testimony to outstanding Tech transfer/collaborations.
- Having the industry accept SOP for testing is not easy.
- Outstanding collaborative effort, as is necessary for this type of project.

Question 5: Approach to and relevance of proposed future research

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

- Extension to durability issues is key to supporting long-term DOE test needs.
- Future plans, after refinement of the standard plan, should now move to standard durability test methods.
- More details of how LANL will implement/recommend the SOP need to be stated.
- Proposed future research was short on details, but the overall plan looks good.

Strengths and weaknesses

Strengths
- Attention to detail that is necessary for this kind of work.
- LANL widely regarded as the leader of single cell testing and diagnostics of failure modes when non-reproducible results are obtained.
- Strong collaborative effort.

Weaknesses
- Need to bring in more analytical resources to support durability testing.

Specific recommendations and additions or deletions to the work scope

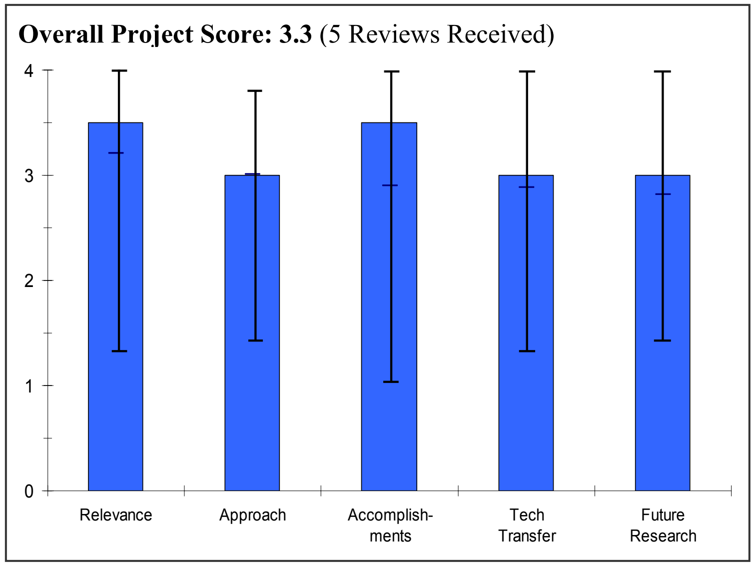
- Through USFCC become the advocate of standard method for durability test protocols.
- May be beyond the scope of this work, but should use position as leader to promote the widespread adoption of this standard (for example, report results based on USFCC standard method).
- Inclusion of other MEA types.

Project # FCP-28: Fundamental Science for Performance, Cost and Durability

Pivovar, Bryan; Los Alamos National Laboratory

Brief Summary of Project

Los Alamos National Laboratory (LANL) is developing a fundamental understanding and technical underpinnings of technologies for reduced cost and improved performance and durability. This effort focuses on modeling from the molecular level for increased understanding of proton conduction to the phenomenological level to interpret membrane-electrode interfacial breakdown and interpretation of reference electrodes.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- This project addresses some key needs for the program.
- Areas selected important for achieving long-term goals and ripe for investigation.
- Reference electrode work will be crucial to understanding severity of local durability stresses during non-steady-state operation.
- Delamination model is of potential interest, but it appears that only clear experimental case of in-cell delamination is irrelevant -80°C to 80°C case (plus possible mild case in UTC freeze workshop presentation).
- Humidity effects on ORR kinetics are probably worth studying, but there may be a more straightforward way to make more relevant measurements.
- Higher pH electrolytes are a hard sell and are probably irrelevant to automotive applications. In an alkaline H_2/O_2 fuel cell, water is produced at the anode, where there is a much lower gas flow rate (and little inert-gas component) to help sweep water out of the cell to prevent flooding. Imidazole is well known to poison Pt electrodes. So one would need a new imidazole-resistant cathode catalyst and a new anode structure that is more efficient at clearing water than any present cathode structure. Seems like a long shot.
- Program addresses developing of measurement methods, analytical models, and understanding of reaction kinetics to fuel cell processes.
- A direct correlation with a successful development of a reference electrode or some of the models with successful realization of the Presidents Hydrogen Fuel Initiative may not be possible

Question 2: Approach to performing the research and development

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

- Outstanding approach to each of the technical issues addressed.
- For reference electrode, outstanding.
- For delamination, good modeling, on the assumption that delamination does occur to a significant extent with competently-constructed MEAs in good cells. Make sure that simpler interpretations of resistive effects, such as uniform increases in electrode ionic or electronic resistance, are not overlooked.
- On humidity effects on ORR kinetics, wouldn't it be simpler and more directly relevant to just run in a 50 cm^2 at very high stoichiometries (differential conditions) so that the RH is essentially constant throughout the cell?
- The approach to the reference electrode is from the backside of the electrode and requires membrane be incorporated in the GDL. This could change the reaction properties of the system.

- The delamination model would be valid for single cells but not valid for fuel cell stacks. The model does not identify where (in the x-y plane) the delamination occurs, for example at air inlet or air exit. The value of the model may be limited.
- Effects of hydration on ORR are important for systems operating on dry air.
- It is well known that changing the pH changes the oxygen reduction kinetics to the point that at high pH, carbon is a good oxygen reduction catalyst when combined with a transition metal (peroxide decomposer). Studies in buffered solutions, pH 8 (sodium carbonate cell) are well established. It is not clear what new information is being sought by this program.

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

This project was rated **3.5** based on accomplishments.

- Exceptional progress for such a new project.
- Reference electrode modeling and experimental work is very nice and should have a rapid impact on the understanding of some severe durability issues.
- Delamination modeling seems to give important insights as to the potential impact of delamination, whether or not it is a significant factor in real fuel cells.
- High-pH imidazole is an interesting concept that could be tried if everything else fails.
- Good progress is being made on the approaches under study. The value of the progress against the President's Hydrogen Initiative is not obvious.

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

This project was rated **3.0** for technology transfer and collaboration.

- Outstanding interactions with a variety of players in the areas addressed.
- Results likely to be published and made accessible to fuel cell researchers lessen need for close collaboration with multiple partners. Collaboration with VT is appropriate.
- Reference electrode work has already been transferred in a way that should provide useful fruit in a short time.
- Other work generally in too early a stage to expect a lot of transfer yet.
- No collaboration with industry identified, no collaboration with other National Laboratories, and only one collaboration with a university.
- Information is published.
- Information should be published in refereed journals.

Question 5: Approach to and relevance of proposed future research

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

- Outstanding and thoughtful delineation of the future plans to address very important problems.
- Future work on membrane-electrode interface would be stronger if it could be expanded to more membranes than Nafion and BPSH.
- Prospect of improved Pt ORR with higher-pH electrolytes or non-precious metal ORR catalysts is not uncertain. Could Ni really work with imidazole-based electrolytes? If known, cite literature. If not, consider as an area of future work or expanded scope.
- Nice ideas and some good exploratory work. Make sure exploratory effort hasn't already produced 90% of the potential useful insights before committing significant continued resources.
- Rating of 3 is for humidity oxygen reduction work. Other work would be rated 2.

Strengths and weaknesses

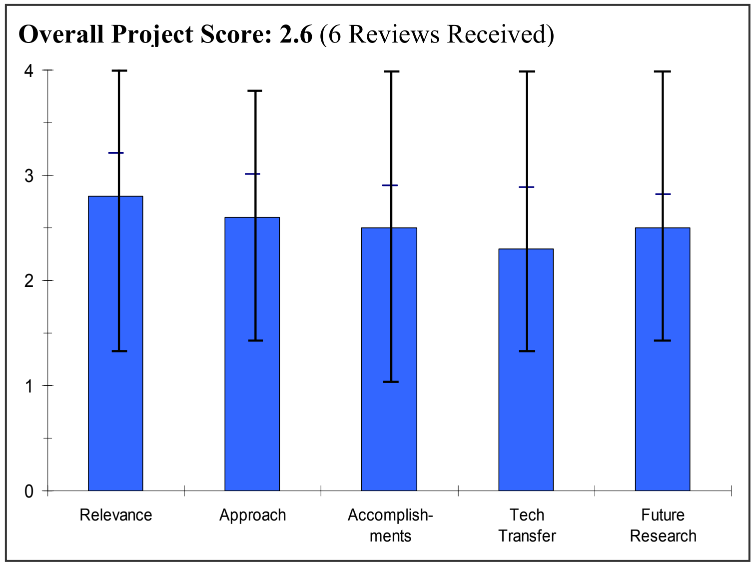
Strengths
- Exceptional team and execution.
- Nice combinations of experiments and models, some of which have produced immediate fruit that are enablers of other important work, and others of which are pretty fruity.
- Strong talented research group.

Weaknesses
- Strongly consider changing the title, which, with excessive honesty, seems to imply a $600K general technical sandbox in which to play (which might be effective but might prove hard to defend through the budgeting process in the long run). High-pH approach faces clear technical challenges as well as the tremendous momentum (backed by demonstrated technical promise in important applications) behind acid PEM development.

Specific recommendations and additions or deletions to the work scope

- Suggest dropping plans for imidazole work - the general area already has too much research effort devoted to it, and the alkaline twist proposed here has inherent disadvantages. Reference work is very good, but does it make sense to continue reference development? Strong case for relevance of delamination to practical fuel cell operation would have to be made to justify an extensive program.
- This is wonderful exploratory work with some creative concepts, but sometimes it's best to wait a little to see if the practical world catches up to areas of exploratory interest, rendering them compellingly important, before bringing out the heavy guns (and dollars).
- Program should identify fundamental needs of industry and develop understanding that would accelerate industry meeting goals of President's Hydrogen Initiative.
- Eliminate delamination effort.
- Justify pH work. Review literature on low temperature carbonate fuel cells.
- Should this program be in Basic Sciences?

Project # FCP-29: Fuel Cell Testing
Bloom, Ira; Argonne National Laboratory



Brief Summary of Project

Argonne National Laboratory is conducting independent evaluations of polymer electrolyte fuel cells and stack systems developed under the DOE Fuel Cells for Transportation Program. The present facility can test stacks and systems rated for 100 W to 50 kW. The results from this project will provide DOE with an independent assessment of DOE contract deliverables, determine the status of the state-of-the-art fuel cell technology, and provide data for model development and validation.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.8** for its relevance to DOE objectives.

- Testing facility is needed.
- Independent stack/system validation is important.
- Good opportunity for input for the development of standards and standardized test procedures.
- Vital to measure the current status of fuel cell systems against the targets.
- Not sure how necessary "independent" ANL testing is to advancing to advancing fuel cell state of the art.
- Independent assessment of SOA fuel cell technology is critical.

Question 2: Approach to performing the research and development

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

- Comprehensive test procedures.
- Testing protocol is thorough.
- Testing approach is reasonable and in line with standards if they exist.
- Project identifies procedures and protocols for testing. Indications are that there are no show stoppers with regards to testing- only dated equipment limitations. No mention of coordination with other testing facilities such as LANL or SwRI.

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

This project was rated **2.5** based on accomplishments.

- Hard to judge accomplishments due to lack of test data (at least include an overview table listing what kind of stacks were tested).
- Impossible to judge, since all the data generated go to DOE HQ under a confidentiality agreement.
- The lab is functional.
- Little said about what has been actually tested to date and the results of the testing.
- Safety systems well thought out and implemented. No specific barriers to really overcome except possibly lack of comprehensive universally recognized testing procedures. May be beneficial for project to be more involved with organization (i.e. ASME, SAE) developing test procedures. All funding for fuel cell testing and most (90%) for battery testing comes from DOE and USABC would be beneficial if private sector utilized facility.

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

This project was rated **2.3** for technology transfer and collaboration.

- Would appreciate if more of the test data could be published.
- Impossible to judge, since all the data generated go to DOE HQ under a confidentiality agreement.
- Need to provide a process for people to use this lab.
- Tech transfer not discussed.
- Project seems to operating somewhat in a vacuum. Not much coordination or information flow with other testing facilities or private sector. Does seem to be coordinating somewhat with FreedomCAR testing and modeling activities although extent is unclear.

Question 5: Approach to and relevance of proposed future research

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

- Stack durability testing for automotive and stationary applications would be very helpful.
- Can't comment. PI can't talk about his work.
- Where is the return on investment and who are the future users of this resource and investment?
- Project seems to be reflexive of immediate DOE testing needs but not particularly proactive in identifying future needs and methods of interacting/ leveraging resources outside ANL. For example, freeze/thaw analysis at NREL are producing models and estimates of energy requirements to warm up fuel cell from cold stock. There is likely some synergy here to be exploited.

Strengths and weaknesses

Strengths
- Independent performance verification.
- Independent testing/validation of developer claims vital to program.
- Written protocol for testing.
- Good lab for testing.
- Clearly there is a need to benchmark technologies and gauge technical progress of fuel cell program. Project also help fills need of providing testing data to validate models of OHFCIT and OFCVT.

Weaknesses
- Most test results remain proprietary.
- Must depend on DOE HQ to judge the quality of this project.
- Impossible for reviewers to rate this project.
- Who is using it?
- Does it make sense to put more money into it without collaboration or future users?
- Project seems to operate almost autonomously without strong communication and coordination with other testing entities, private sector, mom??? organizations establishing's test procedure. Project needs to be more proactive in establishing future wishes/needs.

Specific recommendations and additions or deletions to the work scope

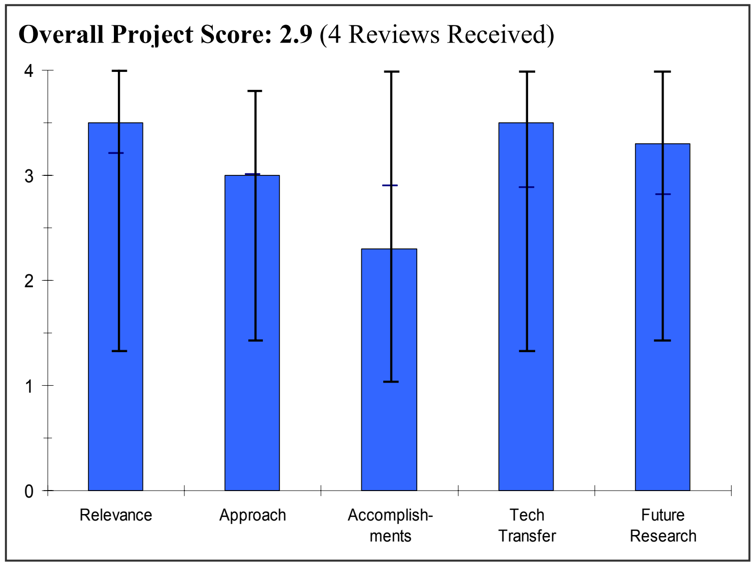
- The testing facility seems ready before the fuel cell stacks are ready for testing. Needs thinking how to utilize it in the meantime.
- Look into possibility of working with USFCC Materials and Components Working Group to establish stack testing protocols.
- Work with DOE Stack Components contractors with most promising results (in final stages of their contract) to endurance-test stack materials in standardized hardware; include results in future review presentations.
- ANL and DOE HQ work together to figure out how to show averaged results that don't identify developers.

- Need to address the topic of future users of this facility, how does it support the testing objectives and the standards/guidelines it supports.
- Would like to see comparison of ANL test results with fuel cell manufacture's test results.
- Coordinate/collaborate more with other organizations to leverage resources/activities. Seek private sector interest, needs, funding support.

Project # FCP-30: Solid Oxide Fuel Cell Development for Auxiliary Power in Heavy Duty Vehicle Applications
Simopoulos, George; Delphi

Brief Summary of Project

This DOE program will further advance the state of the art of solid oxide fuel cells (SOFCs) in the commercial trucking industry as auxiliary power units (APUs). The program's objective is to define system requirements and design, build, and test a SOFC-based APU system capable of running on low sulfur diesel fuel in commercial heavy-duty truck applications. Fuel cell APU technology allows for the reduction of primary engine idling emissions and an increase of overall vehicle fuel economy. Delphi has teamed with original equipment manufacturers, PACCAR Incorporated and Volvo Trucks North America, to define the system level requirements, and enlisted Electricore to provide administrative assistance.



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- The project is entirely relevant to DOE's goals and objectives.
- Relevant to the DOE objectives for auxiliary power.
- The area of heavy trucks is not directly part of hydrogen fuel initiative and there is a more appropriate DOE program for the project. The construction of SOFC adequate to APU, and APU are proposed.
- APU development seems to have a loose relationship to the Hydrogen Program. APUs will assist SOFC commercialization primarily in stationary applications but will also assist application to hydrogen production via electrolysis. Reduction of dependence on foreign oil does not appear to be a significant driver for this work.

Question 2: Approach to performing the research and development

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

- The project would be better if the APU were tested in an over-the road truck instead of in a simulation.
- Major tasks are listed but key milestones and dates are not described or scheduled.
- Necessary development of the reformer/stack package isn't addressed. Have the APU package and its components already been developed and qualified?
- The scope of the project is limited for the funding level considering the project is heavily leveraged by Delphi's SECA-funded activities.
- Considering the experiences of the main company and collaboration with two industries, the project is feasible. But the inclusion of SOFC developer is strongly recommended.
- The work is focused on system assembly, not directly R&D, more engineering oriented. Within this scope the approach is good.

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

This project was rated **2.3** based on accomplishments.

- Program accomplishments are not adequately described or compared to the original schedule.
- The project is relatively new, but it appears that the only accomplishment in the past 8 months has been a planning meeting. Considering Delphi has been working in this area for a number of years under SECA funding, the pace of the project should be rapid.
- The project is in the beginning. Must inform about others efforts made in the different laboratories. The project will benefit hydrogen program because of SOFC development being important the inclusion of SOFC developer or interaction with specialized laboratory. No information about the cost.
- Very little progress. Progress has consisted of signing the agreement, establishing collaboration and submitting a report. No technical accomplishments identified.

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

This project was rated **3.5** for technology transfer and collaboration.

- The team contains the necessary elements including a heavy truck manufacturer to provide real-world perspective and requirements.
- It is not clear who will provide the reformer/SOFC stack package or its individual components.
- Excellent collaborative effort.
- There are the truck companies as collaborators. But there is no indication about the technology transfer, yet. Must inform about others efforts made in different laboratories.
- PACCAR.
- Volvo.
- Excellent collaboration, key organizations that provide all the required technology for the project.

Question 5: Approach to and relevance of proposed future research

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

- Future work addresses the project objectives and logically follows progress to date.
- Limited in scope.
- The parameters to be considered is not enough nor clear for the PI. Planning is very simple.
- The list of future work is complete to provide an APU system for a truck. A thorough engineering development effort that will produce a product.

Strengths and weaknesses

Strengths
- Strong background in SOFC and reformer development for APU applications.
- Integration possibilities with other research.
- Excellent collaboration.
- A thorough engineering effort is planned.

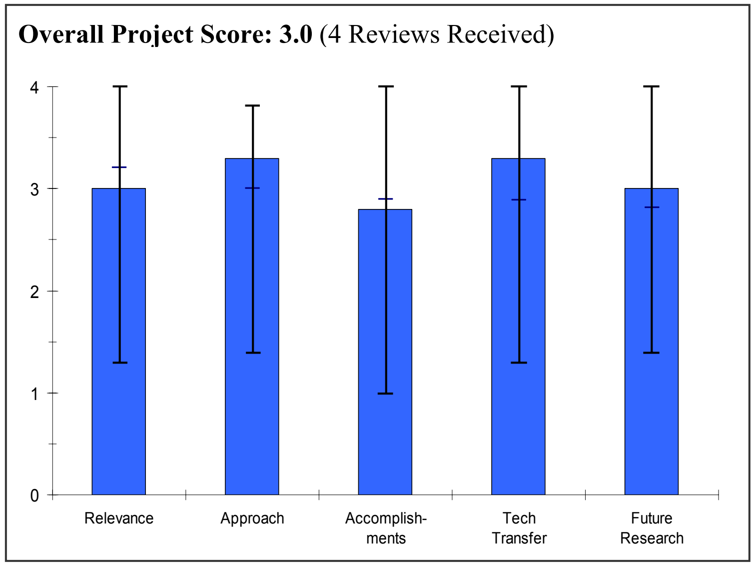
Weaknesses
- Limited scope given previous experience in the area.
- The parameters to be considered for APU development are not clear for the PI, yet.
- Very little progress.
- The product will not be tested on a truck.

Specific recommendations and additions or deletions to the work scope

- Understanding of problems associated with SOFC.
- Interaction with SOFC developer is recommended.
- Must include safety codes in the project.

Project # FCP-31: Diesel Fueled SOFC for Class 7/Class 8 On-Highway Truck Auxiliary Power
Norrick, Dan; Cummins Power Generation

Brief Summary of Project



Cummins Power Generation, with team members SOFCo-EFS Holdings LLC and International Truck and Engine, will produce and demonstrate a prototype solid oxide fuel cell (SOFC) based auxiliary power unit (APU) for application to Class 7/8 on-highway trucks. SOFC APUs are potentially clean and efficient energy sources for sleeper cab cooling, cab and engine heating, and electrical power to enable main engine shut-down during driver rest periods or other extended waits where truck main engines have traditionally been idled. Project challenges include accurately characterizing energy requirements, development of an application specific diesel fuel reformer, ruggedized system integration, and the combination of product cost and efficiency to meet stringent industry payback expectations.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.0** for its relevance to DOE objectives.

- Relevant to the DOE objectives for auxiliary power.
- The use of fuel cells to meet the auxiliary power needs for trucks is relevant to the DOE objectives. Successful development of APUs can reduce diesel fuel consumption and emissions.
- The PI addressed the problems and the importance of his project. The construction of SOFC (stack with 140 cells) in collaboration with a company and APU for heavy truck is proposed. The area of heavy trucks is not directly part of hydrogen fuel initiative (RD&D plan objective) and there is a more appropriate DOE program for the project.
- APU development seems to have a loose relationship to the Hydrogen Program. APUs will assist SOFC commercialization primarily in stationary applications but will also assist application to hydrogen production via electrolysis. Reduction of dependence on foreign oil does not appear to be a significant driver for this work.

Question 2: Approach to performing the research and development

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

- An understanding of the stack survivability from vibrations is important.
- Fuel reforming using recycled anode water is a good approach.
- The approach is sound. System requirements and load profiles were developed first before hardware specifications. Development issues related to the fuel cell and reformer systems have been identified.
- The project started in 9/01/2004. Several technical barriers are addressed. The PI presented alternative pathway in some cases but in others, he needs collaborations with SOFC specialized group to solve. The project is feasible.
- The project is broken into key technical issues. Combined fuel cell/battery design is an excellent and possibly essential approach. The substantial focus on Shock and Vibration issues is outstanding. This is a key issue for use of SOFCs in mobile applications.

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

This project was rated **2.8** based on accomplishments.

- Little progress.
- Much of the background has been established in the SECA-funded program, therefore progress should be rapid.
- The project started in September 2004 and is only about 15% complete. Analysis of thermal and electrical load profiles is underway. Micro reactor studies are also underway to support reforming catalyst development.
- The project is adequately led. The PI must inform about others efforts made in the different laboratories. The cost goal is considered as a parameter.
- Good start on the specific key issues. One issue is complete (water management). Most technical issues are still open but good progress. An excellent vibration input spectra was displayed, an essential first step in mitigating any S&V issues.

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

This project was rated **3.3** for technology transfer and collaboration.

- Excellent interactions with industrial partners.
- Collaboration between Cummins, International, and SOFCo is evident.
- There are the truck companies as collaborators. But there is no indication about the technology transfer, yet. Must inform about others efforts made in different laboratories.
- International Truck and Engine.
- SOFCo - fuel processing.
- Excellent collaboration, key organizations that provide all the required expertise for the project.

Question 5: Approach to and relevance of proposed future research

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

- Vibration studies are important to determining survivability.
- Good work plan for future research.
- Future work plans were presented. Both experimental and analysis tasks were described.
- The variables to be considered in the APU development must be detailed to the planning.
- Requires more detail. Insufficient detail regarding truck system integration. Integration of the final product in a truck is important. Task list for key technology work is good.

Strengths and weaknesses

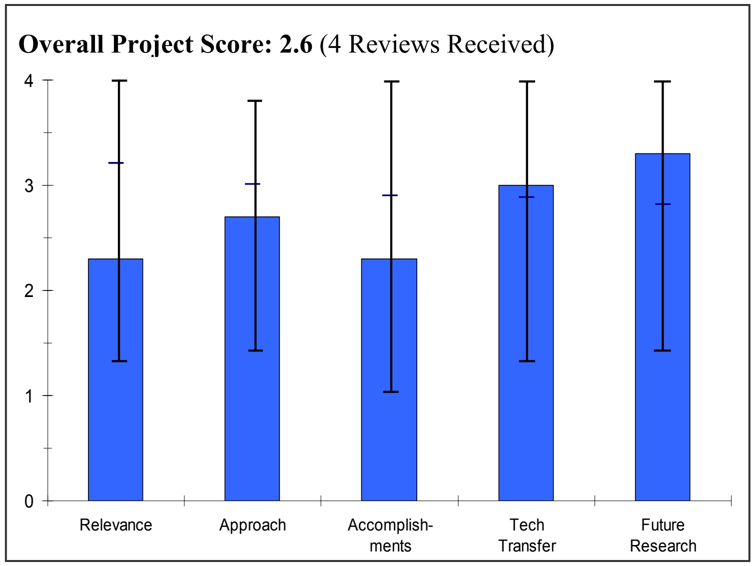
Strengths
- Good understanding of BOP, power conditioning, heat and water management.
- Good understanding of operating conditions.
- There is evidence that the team is well integrated with each organization assigned specific analysis or experimental tasks. The team should be able to demonstrate a prototype system in real over-the-road trucks. The project appears off to a good start. Poster presentation was well organized and informative.
- Good understanding of problems associated with SOFC.
- Excellent collaboration.
- Excellent focus on key technology that requires development.
- Integration in a truck will be a significant achievement.

Weaknesses
- Can an all ceramic SOFC stack survive APU conditions?
- Vibration characteristics could cause problems with solid state fuel cells. But the team is investigating this issue.
- Requires more detail with respect to system engineering and integration.

Specific recommendations and additions or deletions to the work scope

- Must include safety codes in the project.
- The project should utilize the shock and vibration analysis methodology under development at PNNL. Both projects would benefit.

Project # FCP-32: Fuel Cell APU
Brodrick, Christie-Joy; University of California, Davis



Brief Summary of Project

The Institute of Transportation Studies at the University of California at Davis (ITS-Davis) integrated and demonstrated a proton exchange membrane fuel cell system (~5 kW) in a transport refrigeration unit (TRU). ITS-Davis conducted a trade-off study of the fuel cell TRU system including quantifying cost, noise, and emissions.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.3** for its relevance to DOE objectives.

- Fuel cell-based power units for operating trailer refrigeration units can decrease fuel consumption along with emissions.
- The area of refrigeration by fuel cell is not directly part of hydrogen program.

Question 2: Approach to performing the research and development

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

- The approach has been to first quantify the magnitude of emissions as a function of power rating of conventional units, ambient temperatures, and determine the operating power profiles of such units.
- After that, the main activity has been to configure a fuel cell/ battery / diesel engine unit to verify operational characteristics and emissions.
- The project started in 03/01/2004. Several technical barriers are addressed such as the problem to purchase a SOFC. The PI substituted with PEMFC provisorily. The project is feasible but additional time is required because of the substitution of PEMFC by SOFC, and the problems associated to SOFC are different from those of PEMFC. The PI belongs to a University. It has difficulty to get collaboration.
- The hybrid SOFC/Battery approach is a good approach for transient loads. It is also a good cost approach but can impact the size for a size limited application. The power electronics approach was not appropriate to a 3-phase system. A motor generator type approach would appear better, the PI recognized this.

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

This project was rated **2.3** based on accomplishments.

- They have been unable to obtain a solid oxide fuel cell (SOFC) as initially planned. They have, however, redesigned the project to use two commercially-available polymer electrolyte fuel cell (PEFC) stacks. This has entailed significant changes from what was originally considered. For example, rather than reforming diesel to power an SOFC, they will use separate on-board hydrogen to power the PEFC.
- They have successfully developed a power inverter for this application.
- Must inform about others efforts made in the different laboratories, mainly about the problems associated with SOFC. It is difficult to evaluate the progress toward goal because of the problem to purchase a SOFC. The contribution of this project on the hydrogen program is not clear. No information about the cost.

- The prime is not an SOFC developer and has been unable to obtain an SOFC. This does not invalidate the project but is undesirable. The substantial power electronics work is excellent but is unfortunately not a desirable approach to a 3 phase system required for the application. Development of an integrated fuel cell/battery system is excellent work. Significant work has occurred; unfortunately it does not appear that the project has achieved progress toward goals.

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

This project was rated **3.0** for technology transfer and collaboration.

- They are working closely with a commercial vendor of truck refrigeration units as well as a trucking company that is a potential commercial customer of such units. Being unable to obtain the desired SOFC in time, they have obtained two stacks from a commercial PEFC developer. All of these entities (and their relevant industries) stand to gain from successful development and commercialization of this technology.
- Actually, the developed system is tested on board but PEMFC is used. There is no indication about the technology transfer, yet. Must inform about others efforts, using SOFC, made in different laboratories.
- Carrier Chevron.
- In principle an excellent collaboration that provides the right expertise. It is not evident from the work that the collaborators are significantly involved.

Question 5: Approach to and relevance of proposed future research

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

- For the rest of this project, they will complete system assembly and testing, first on the bench, and subsequently, on a truck.
- Even though this falls short of operating the fuel cell APU on diesel fuel, the project will still generate useful data on configuring a hybrid fuel cell / battery / internal combustion engine power plant for trailer refrigeration.
- Despite some false starts the work plan does recognize what must be done. If completed the work will result in an installed and integrated system.

Strengths and weaknesses

Strengths
- The project plan was well laid out. They were able to reconfigure the project to still obtain useful information on a fuel cell-based APU, even though an SOFC has not become available. The type of fuel cell used is, of course, a very important consideration; there is still a wealth of useful information that can be obtained from the balance-of-plant as well as on power conditioning, duty cycles, etc.
- Since the development by using PEMFC was done, the comparative study with the system using SOFC is interesting.
- In principle an excellent collaboration.
- Good overall work plan.
- Integration in a truck will be a significant achievement.

Weaknesses
- This project needed to realize early on that generating fuel cell quality fuel gas from the truck's on-board fuel (diesel fuel) would be a key consideration that has to be addressed. Running a fuel cell APU on hydrogen has been demonstrated earlier (by the same organization). Even with an SOFC, the need for a diesel fuel processor would not have been obviated.
- It would have been helpful if they had identified a potential partner to address that issue.
- Interactions.
- The lack of an SOFC and development of power electronics not suitable for a three phase system has hurt progress.
- Shock and vibration issues are not considered.

Specific recommendations and additions or deletions to the work scope

- This project has been of value in identifying non-fuel cell issues, such as the power conditioning needed, the hybrid architecture options that may be considered, and the need to work closely with commercial vendors that can bring a wealth of relevant practical information.
- The key issues that remain are how to get from diesel to "clean" reformate, and whether the low-sulfur diesel is low enough in sulfur for such systems to be practical. Follow-on activity, either with this organization or with other researchers, should address these two issues.
- Include comparative study between PEMFC and SOFC.
- Must include safety codes in the project.
- The project should utilize the shock and vibration analysis methodology under development at PNNL.