Fuel Cells

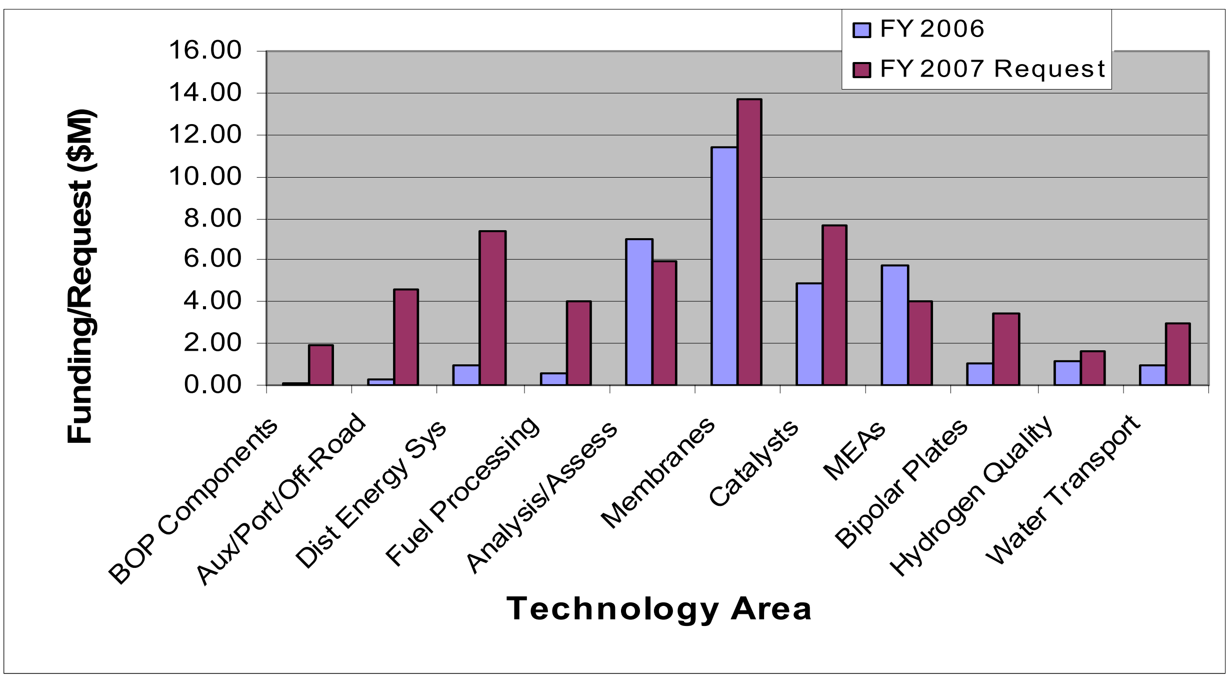
Summary of Annual Merit Review Fuel Cells Subprogram

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. Some reviewers thought that the funding for some industrial projects was too large. Others thought that some projects received insufficient funding to enable significant technical advancement. The current focus on partnering (industry, National Labs, etc.) was applauded and reviewers suggested that some projects might benefit from consolidation with other closely related programs – to establish a "critical mass" and better utilize resources and funding as many of the National Lab projects are already doing. Some reviewers thought that balance of plant was not covered adequately while others thought that components crucial to the ultimate success of PEMFCs for transportation, such as cathode catalysts, catalyst supports, membranes, and MEAs should be developed first, before significant effort and resources are invested in other areas.

Fuel Cell Funding by Technology:

The Fuel Cell Technology Subprogram continues to concentrate on the critical path technology of stack components (membranes, catalysts, bipolar plates, membrane electrode assemblies, etc.). Cost and durability of stack components 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 3.7, 3.0 and 2.0 for the highest, average and lowest scores respectively. The scores reflect 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.

MEAs: The scores for the MEA projects ranged from good to very good. The projects are completed with significant accomplishments. New awards from the recent solicitation/laboratory call will begin in the fall of 2006. E-Tek has shown good performance with fine gradient GDLs and progress with IBAD electrodes. 3M's ternary PtCoMn catalysts show high performance and ability; mass activities based on their NSTF catalysts and correlations with the whisker morphologies are impressive. The stability of UTC's PtIrCo catalyst is promising. LANL's LaB_6 support has a long way to go to reach practicality.

Catalysts: Scores for catalyst projects ranged from fair to very good; as a number of these projects conclude, new projects will be awarded in early FY07. Dr. Ross, a PI from LBNL, is retiring; he and his impressive work will be missed. Hopefully, he will continue to work in the field, perhaps as a consultant. Ways to produce Pt-alloy particles with control of exposed crystal faces in high-surface-area catalysts should be explored. BNL's work on gold overlayers shows promise to suppress potential cycling damage and should be continued. The reviewers thought great progress was made in development of non-precious metal catalysts. Many PIs have improved the activity of these catalysts, but need to continue to address the durability issues. Ballard should consider adding a small amount of a precious metal and looking for strong metal-support interactions.

Membranes: The membrane projects were ranked good on average with the exception of the 3M project which ranked among the highest. Projects were completed and new awards from the recent solicitation began. Overall cost implications and suitability of new materials and fabrication technologies for use mass production received consistent expressions of concern. Reviewers recommended that ANL, NREL, and SNL set some specific targets for critical properties that need to be demonstrated and a timeline to meet them. Plug Power should assess the long-term performance stability of PEMEAS membranes under start-up and shut-down cycles. LANL's examination of non-Nafion MEAs could be extended to other new membrane materials being funded by the DOE. The reviewers suggested that Arkema concentrate on conductivity testing and durability testing.

Bipolar Plates: These projects were consistently ranked as good. All projects were completed as new awards from the recent solicitation/laboratory call will begin in the fall of 2006. For carbon/carbon bipolar plates, most DOE targets have been met. For metal plates, techniques that minimize the effect of thermal stresses and mismatch in thermal expansion coefficients between outer layers and the bulk of the plate need to be developed. For both carbon/carbon and metal plates, the cost of materials and manufacturing at high volume production needs to be evaluated.

Recycling: The reviewers thought that the recycling projects were good. More cost analysis as well as further details on the recycling of recovered Nafion need to be presented.

Stationary: Funding limitations curtailed efforts in this area in FY06. The Plug Power backup fuel cell system is completed and is being tested at ANL and at two other sites. Two other stationary projects were ranked fair to good. Reviewers noted that analysis and reporting of the data taken is critical and suggested that similar projects be integrated.

Analysis and Characterization: These projects were ranked good to very good and they strongly support the fuel cell program. The modelers were encouraged to validate their models with real world data; as suggested in 2005. Fuel cell manufacturers need to supply more experimental data to the modelers. For characterization efforts, the PIs might consider causes of degradation and mitigation strategies. Additional stack testing, particularly with respect to freeze/thaw behavior, is recommended.

Portable Power, Auxiliary Power, BOP, Fuel Processing: Funding for these activities was suspended in January 2006 so these projects were not reviewed.

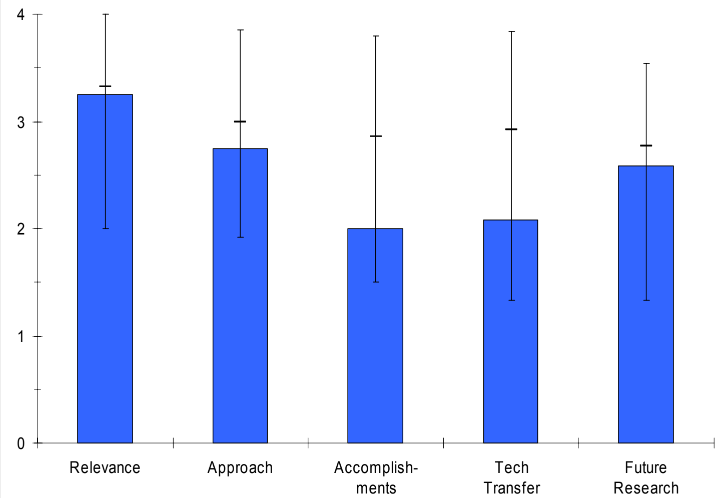
Project # FC-01: High-Temperature Polymer Electrolyte Membranes

Debbie Myers; ANL

Brief Summary of Project

Argonne National Laboratory is developing a proton-conducting membrane electrolyte for operation at 120-150ºC and low humidities to meet DOE's technical targets. ANL is investigating dendritic macromolecules attached to polymer backbones, which have a high density of proton-conducting groups to facilitate proton transfer with reduced need for water mediation.

Overall Project Score: 2.5 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project's objectives are stated to be as broad as possible towards overcoming the barriers for fuel cell components. The project's staffing and resources are not nearly adequate, however, to take such a new fundamental approach for high temperature ionomers, and make it "real", that is make it impact the President's initiative in the 2010-2015 time frame.
- The overall project is aligned with DOE objectives. The tasks are structured appropriately to support DOE objectives.
- High temperature, low relative humidity membranes are critical components for PEM fuel cells. The barriers and an understanding of the challenges were well presented.
- The program of the DOE to support high temperature/low RH membrane materials aligns well with the President's hydrogen vision and RD&D plan objectives.
- Project represents long term DOE goals.
- Clear understanding of high temperature operation requirement is necessary for automotive and stationary. For automotive, maximum temperature is 120°C and dry for short time but not continuous, most of time operating temperature is around 60°C. Good to look at required "range" of temperature, including requirements of subzero temperatures.

Question 2: Approach to performing the research and development

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

- This approach appears to offer many opportunities for molecular design. But there is too much synthesis, polymerization, dispersion, film forming, process development, membrane property characterization, as well as eventually durability and fuel cell testing to be done before it would ever be known whether this approach has the potential to meet the requirements.
- Increasing the sulfonic acid concentration using dendrons is a good approach to enhance membrane conductivity. Desulfonation of the sulfonated aromatics > 80°C will limit high temperature application of the membrane. Higher concentration of sulfonic acids will accelerate the desulfonation reaction. Dendrons are expected to be unstable under peroxide degradation conditions.
- The use of dendrons to provide high surface area for proton transfer and are tethered to a polymer backbone is a nice concept. Oxidative stability of the dendrons attached to the water insoluble polymer background is of concern. A key question is: how will this dendronized polymer interact with the catalyst in a cathode?
- The potential of dendromers suggested seems high, however the approach thus far has been very poor. Poor selection of backbone properties (low T_g) initially used could/should have been avoided. Leaving groups of

backbone being "Cl" could be a potential problem for durability – "Cl" is a known accelerator of degradation. ~Ph-SO_3H has been demonstrated in the literature several times to desulfonate.
- Ability to tailor membrane properties is a good approach. Addition of modeling at Caltech is a good addition. Doesn't appear to a systematic plan on how to select compounds to tailor membrane properties.
- Good to leverage molecular level modeling to estimate basic performance. Leveraging this modeling work, implementation of the parametric design approach could work to characterize the polymer materials.

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

This project was rated **2.0** based on accomplishments.

- This project is just not staffed adequately to have an impact on this problem in a reasonable time. There may be too many of these small discovery membrane efforts that will not be able to answer the hard questions in a reasonable time because it takes years and years of extensive resources to bring any new approach to the state ready for real-life evaluations.
- Fenton's test should be conducted on dendrons and the backbone material to which the dendrons are connected. The membrane should be subjected to hydrolysis conditions for prolonged period to evaluate the IEC stability of the membrane under high temperature aqueous conditions. The water content measurement method under 0% RH should be revised.
- The goal was a low RH, high temperature membrane yet conductivity measurements at these conditions have not been done for a project that started October 2001? An improved polymer backbone was produced, but now films can not be fabricated? Hence no membrane! No gas permeability measurements have been made. Improvements in water loss of the polymer at 150°C and 0% RH are noted. Membrane fabrication progress is too slow.
- Preliminary results of conductivity look promising. However, no membrane characterization has taken place yet – tensile strength, swelling, gas permeation. Characterization of material thus far has been very limited. Too much focus on modeling studies of Caltech – could be useful to guide research but not to make conclusions on the material which the PI did repeatedly. Membrane progress has been very limited – potentially due to lack of researchers.
- Progress appears to be slow. With no end date, estimated end date or even a Go/No-Go decision point, it is difficult to evaluate progress. Project doesn't seem sufficiently staffed based upon objectives.
- Implementation of molecular level modeling is a good accomplishment. However, in the area of polymer synthesis, significant outcomes haven't been seen. Identifying material design parameters and their sensitivity could work.

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

This project was rated **2.1** for technology transfer and collaboration.

- Only one University interaction and this came late in the project.
- More collaborative work should be done. Possibility of industrial collaboration should be explored.
- There is a nice use of modeling by Caltech to provide insight and guidance into the experiments that need to be performed. Perhaps this effort will guide the fabrication of a material that facilitates the "hopping mechanism."
- Only with Caltech at this point, with too much focus on modeling. Modeling work should only guide research, not lead to conclusions. Need to discuss with industry (polymer company) to overcome some of the serious short comings of this project.
- No industry collaborations.
- Good collaboration for molecular level modeling with Caltech.

Question 5: Approach to and relevance of proposed future research

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

- The modeling work should be validated experimentally, before it is relied on to guide the next materials to be synthesized. The material approaches seem to be broadening, rather than focusing, after 5.5 years.

- Fenton's stabilities of the spacers and dendrons should be evaluated.
- Several additional dendronized polymers with fluorinated backbones to improved stability as well as use of "spacers" to enhance stability appear to be promising, but doubts are raised as to if these will actually be fabricated into membranes and then tested are high temperature, low RH conditions.
- Need to evaluate the chemical/mechanical robustness of backbone and dendrimer very closely. At this point, serious drawbacks exist in both cases. Cost analysis should occur as dendrimers can be very expense. It is crucial that more data is generated on well designed membrane materials.
- Future plans seem rather vague, would appreciate more specifics so that progress can be judged.
- Material (polymer) characterization needs more detailed plan.

Strengths and weaknesses

Strengths
- Potentially rich technology area.
- Good approach of enhancing membrane IEC by increasing sulfonic acid concentration. Good synthetic strategy for synthesizing various dendron molecules.
- Concepts and modeling effort guiding experimental work is sound.
- Potential of dendrimers seems to be high. Possibility of using the intrinsic properties of dendrimers on the macro-morphology of the membrane could be very dramatic.
- Take modeling approach.

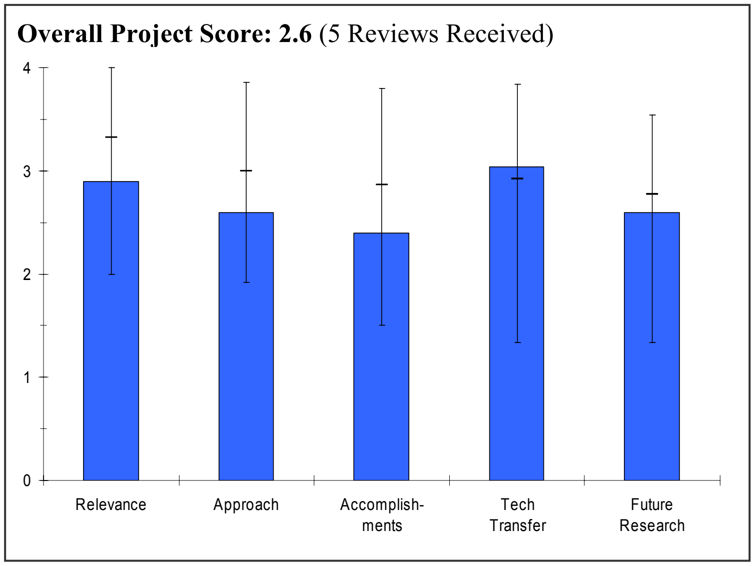
Weaknesses
- Not enough resources to effectively develop this technology in a meaningful time.
- In presence of water, the sulfonated aromatics undergo IEC loss under high temperature (>80°C). Therefore it's very likely that the dendrons will undergo IEC loss over time. The backbone and spacer structures may be unstable to Fenton's condition. Polyaromatic ionomers lose water content very rapidly ~ 80°C. Beyond this temperature, the membrane resistivity in increased significantly. Therefore, these ionomers may not be a good choice for high temperature membrane.
- Approach thus far has been poor. Poor initial evaluation of backbone/dendrimers used resulted in lost time and money in development. Some future approaches have significant short comings which can/should be avoided.
- Project is understaffed and as a result, the progress is very slow.
- Membrane fabrication is way too slow.

Specific recommendations and additions or deletions to the work scope

- The peroxide stability of dendrons, backbone material and the final membrane should be evaluated.
- Conductivity measurements must be taken under appropriate conditions i.e., low RH, >100°C. Lack of these measurements over 4 years should be cause for concern.
- It is of the highest importance that the PI have significant conversations with polymer chemists in the fuel cell industry. In the field of fuel cells, "C1" is a known contaminant and needs to be avoided if possible – not an acceptable leaving group on the backbone. Benzimidazole can adsorb onto Pt thereby killing the activity of the electrode. If the movement is high, electrode activity will be lost. Ph-SO_3H is known to be chemically weak and needs to be avoided. –COOH has been shown by DuPont to be "weak" spot of backbones and should be avoided.
- Set some very specific targets for three of the most critical properties that need to be demonstrated and a timeline to meet them. Then determine the resources needed to answer those questions in that time, and if it cannot be done, consider a Go/No-Go decision instead. Getting more feedback earlier, finding more partners to help guide the work.

Project # FC-02: Development of Polybenzimidazole-based High Temperature Membrane and Electrode Assemblies for Stationary Applications
Rhonda Staudt; 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. Plug Power is conducting rapid screening of candidate PBI materials in 50 cm^2 MEAs; performing detailed electrochemical characterization of MEAs made with selected PBI polymers; evaluating low cost acid-absorbing materials for phosphoric acid management within the system; continuing design and development of bipolar plates with PBI-specific flow fields; and continuing development of a PBI membrane-based MEA with advanced electrode structures providing high catalyst utilization.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- The project supports the President's hydrogen vision and the RD&D plan objectives in terms of lifetime durability for stationary applications. Performance goals discussed as being unlikely, and cost goals have not been addressed in the presentation.
- This PBI MEA strategy is exclusive to the distributed power application (has limited or no relevance to the transportation application), however for Distributed Energy Generation applications, this project is well suited.
- A very relevant program for development of stationary energy conversion devices that use hydrogen fuel. The project is providing valuable data on the durability of these devices and addressing several of the barriers to implementation of the technology.
- Project aligns well to with respect to durability, but could use more focus on the cost barrier as well.
- This project has very good alignment with the objectives of DOE. Evaluation of the PBI-Phosphoric acid based membrane is very critical to the success of the high temperature membrane objectives.

Question 2: Approach to performing the research and development

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

- Issues of stress relaxation and phosphoric acid evaporation have been identified as limiting factors. Improvements have been made by incorporation of inorganic particles. The current level of improvement has not met long term goals; approaches to make further improvements were not presented or defended.
- Cost and Durability are key metrics which have not been addressed at this point in the project.
- The approach appears to be one of development testing, including materials testing under conditions that are relevant to durability. The mechanical testing is good as far as it goes, but the presentation provides little connection between material identity and behavior.

- Previous removal of transportation goals has improved the focus of this project.
- The approach is very well thought-out and well designed. Use of acid absorbing material as an acid trap is a good way of dealing with washed acid.

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

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

- Demonstrated minor improvements in stress relaxation. Significantly decreased phosphoric acid evaporation rates. Significant evaporation of phosphoric acid still exists. Level of detail given on seals gives no opportunity to address novelty or usefulness of this approach. Little insight has been given into why inorganic fillers serve to improve mechanical properties or evaporation rates.
- Durability may be misrepresented. Temperature effect of durability goes from 6.3 to between 12 and 19 microv/hr to 130 microv/cycle as temperature goes up from 120°C to 160°C to 180°C.
- The project appears to have had significant budget cuts that have significantly delayed progress. Several important deliverables are not spelled out or are delayed. The causes of these delays appear to be outside the control of Plug Power; however, I am a little concerned whether Plug will reach its goals..
- Not clear that current progress on durability will lead to achieving goal of 40,000 hours by 2010. Reducing or eliminating seals can be important to enabling high volume manufacturing of MEAs and fuel cells.
- The long term durability study should be done under dynamic cycling, not under steady state condition. Acid washings should be measured after start-up and shut-down cycles. Effect of acid washings on bipolar plate should be evaluated. The change in membrane IEC over time should be evaluated. The progress of the project seems to be slower than expected to achieve the overall objectives.

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

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

- Project has strong, well rounded team. Publications and presentations are low for funding level (1 presentation occurs twice).
- Good collaboration with other institutions. Did not see collaborations with other projects.
- There are excellent contributors to the program. However, there appears to be some blockage in the provision of important results such as cost estimates. This is threatening to undermine the credibility of the project.
- Close collaboration is apparent between PI and subcontractors and is important to the success of this project. Project may also benefit from collaborating with a National Lab such as Argonne that is also working on polybenzimidazole.
- This project has wide range of technical collaborations within academia and industry, which satisfies DOE objective.

Question 5: Approach to and relevance of proposed future research

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

- The proposed plan does not inherently lead to commercializable products.
- The project is nearing completion according to the schedule. The future activities are somewhat vague and represent final commercialization steps. The details of these appear to be withheld for commercialization reasons. It is therefore questionable as to what level of DOE support is appropriate in the future.
- Future work to further sealing work is important. It is not clear that future work goes any further in addressing cost barrier.
- Significant amount of work is still incomplete after 2.5 years of the project. Is it is possible to complete the stack work under given time? Significant acceleration of the work is needed for the completion of the project.

Strengths and weaknesses

Strengths

- Appropriate technology paths are explored.
- Plug Power presented very good durability studies that demonstrate technology viability and appropriate attention to show-stopping barriers.
- Seal elimination work. Strong collaboration with Rensselear Polytechnic.
- The stability of IEC of PEMEAS membrane at high temperature may allow this project to achieve 2010 DOE goals. The membrane performance remains relatively unchanged under steady state fuel cell conditions. PEMEAS and RPI team brings in years of experience in PBI-Phosphoric acid membrane system.

Weaknesses

- Cost studies should have been done earlier. More explanations for cost and performance feasibility, along with a stronger fundamental explanation for the results presented and how to apply them. Overall the approach presented shows little insight. A discussion of how the inorganic particles interact or how they could be further improved was never discussed.
- Impossible to relate the reported results to basic materials used. Project seems to have reached a point where the activities are now in the final commercialization.
- Project may have lost some momentum due to funding cuts.
- No information on acid washings under start-up and shut-down conditions. Use of acid trap will lead to parasitic loss when used on-board in an automobile system. The durability of bipolar plates under start-up and shut-down cycle is not addressed.

Specific recommendations and additions or deletions to the work scope

- This project appears to be a development program and hence a larger proportion of cost sharing appears to be more appropriate.
- The long-term performance stability of PEMEAS membrane under start-up and shut-down cycles should be assessed. To meet the automobile requirements, the acid washings should be quantified under dynamic temperature cycling.

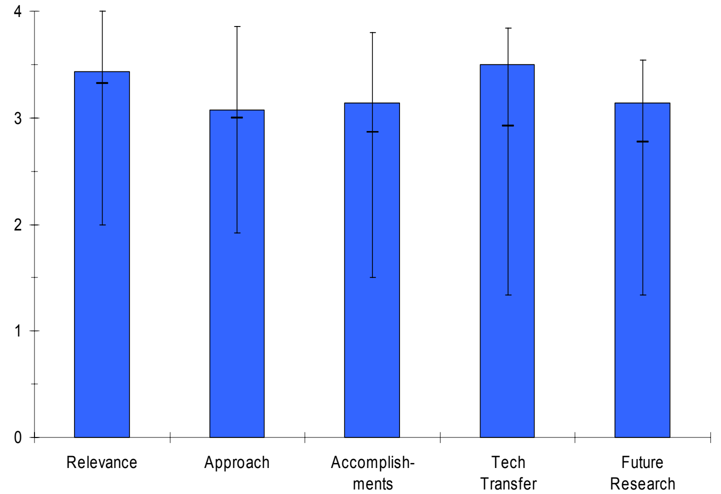
Project # FC-03: Non-Nafion Membrane Electrode Assemblies

Yu Seung Kim; LANL

Brief Summary of Project

The objective of this project is to develop low-cost, high performance and durable alternative membrane electrode assemblies (MEAs). This is being done by exploring MEA operating window dependence on architecture, performing evaluation of non-Nafion binder under H_2/air conditions, and evaluating H_2/air long-term (2000 h) performance of non-Nafion membranes under cycling conditions.

Overall Project Score: 3.2 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- There is good alignment of the project with DOE objectives. There are a good number of collaborators involved in this project.
- This project aligns well with the President's Hydrogen objectives. Understanding and optimizing HC membranes and MEAs are crucial for the success of fuel cells to meet the cost targets.
- Long-term FC performance for 4 membranes was reported for 4 MEAs in DMFCs, but this does not support the Hydrogen Initiative as H_2 has different durability issues than DMFC.
- Addressing automotive needs for fuel cells.
- Any new membrane materials efforts should include work to modify the electrodes as well. This is one of the few projects that address both new membranes and new electrodes which are key to achieving the current fuel cell targets.
- Project is focused on one of the most critical areas. Stable and lower cost electrodes will be required for long term success. LANL is perfectly suited to address this topic.

Question 2: Approach to performing the research and development

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

- The approach of working both water uptake is good. All four tasks have been focused correctly to accomplish the objectives. It is right to focus on the interaction between the membrane electrode catalyst and electrode ionomer.
- Approach is generally very good. Well thought out experiments – unclear why DMFC work is being carried out and does it always apply to hydrogen systems?
- No evidence is presented that such membranes can achieve conductivity targets using 1.5 kPa inlet water vapor pressure.
- Very good approach looking at individual components and the big picture.
- Attempt to isolate performance limitations seemed to fall short. More work is needed for a more thorough understanding of interfacial resistance, durability, and transport effects.
- Good to take parametric design approach and look at sensitivity with water-uptake. Membrane performance should depend on relative humidity. A clearer path needs to be identified to meet the target of alternative membrane development.
- The approach is OK, the catalyst layer (electrode) development appears to be OK, but the analyses and initial observations and conclusions need further refinement. Expectation is that this group will look at the fundamentals of such systems, and then design the appropriate experimental path.
- It is not clear that the actual mechanisms are being elucidated.

- This reviewer feels there are too many mechanisms in play which can lead to erroneous interpretations. 700 hours is not long enough yet under certain circumstances, a dry membrane can fail in hours.
- It is still a sulfonic acid system – water dynamics and transport phenomena are still water-based, regardless of the membrane chemistry.
- Sawtoothing is common – researchers should be aware of such behavior and what creates the response.
- Lower water content membranes at the same equivalent weight was a major difference between Dow and DuPont membranes.
- Can't exclude other phenomena such as catalyst/ionomer degradation, poisoning, etc.

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

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

- The performance has been achieved by incorporating fluorinated bis-phenol, but it cannot be implemented practically, since we don't know about the long term effect of the bis-phenol in fuel cell operational condition. Non-Nafion ionomers should be examined with hydrocarbon membranes to evaluate the true compatibility of the electrode layer with the surface of the hydrocarbon membrane.
- Accomplishments have been very good. Good progress has been made in order to understand the needs for HC MEAs. Durability experiments have shown insight into possible mechanisms. Interfacial resistance measurements between membrane and electrode layer are key technology found.
- Achievement of improved interface of PFSA electrode to non-Nafion membrane is solid accomplishment.
- The team appears to have met all current milestones.
- Well coordinated and strong effort. The approach can be used as a model for the implementation of other non-Nafion materials.
- Identifying proper non-Nafion ionomer which provides less sensitivity and taking a good systematic approach.
- Project needs some refinement in the testing methods. A better screening experiment is needed to determine the key variables rather than making assumptions.
- Further refinement is necessary before any scale-up activity is planned. It is too early.
- Peroxide should still be a factor under these conditions – need to better understand the impact (and quantity), especially for these new membranes.
- There is nothing that states a conventional electrode designed for Nafion PEM will be the same electrode which works for this new membrane.

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

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

- Significant collaborative efforts have been put in this work. Good selection of different teams (Government, Industry and Academia) and knowledge bases has been accomplished.
- Good coordination with other groups. Would be interesting to see if this type of technology can be applied to other HC systems, i.e., Polyfuel, Arkema, Hoku, etc. Is this technology applicable to all HC membranes or just BSPH-type?
- Very strong team, but the amount of money for such a large team seems too small.
- Lots of collaborators listed, but it was not clear what they were doing or how the technology was being applied/transferred.
- Appears to be acceptable for this stage of the project.

Question 5: Approach to and relevance of proposed future research

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

- The proposed future work seems to be rational.
- Good future work. Well thought out experiments and well executed.
- The work on block copolymers may yield important insights for many other developers of new membranes.

- With an open ended project it is important that a timeline be proposed. New ionomer design needs to be incorporated into phase separation studies. Not enough detail to evaluate whether future work will lead to the optimized MEA in the most rapid time.
- A more thorough understanding of performance and durability is needed. The source of the recoverable vs. non-recoverable performance loss should be identified.
- The project needs some refining and an additional level of experimental detail based on elucidating the mechanisms.

Strengths and weaknesses

Strengths
- Good approach towards the study of hydrocarbon membranes. Development of non-Nafion ionomers for electrode ink to achieve good membrane electrode layer contact. Good understanding of MEA water content dependence on performance.
- Have shown conclusively the importance of optimizing HC MEAs in order to obtain good performance and acceptable durability. Very important information especially for the success of HC membranes in fuel cell applications.
- Researchers and their institution are capable of carrying out all aspects of the project. Systematic study of water uptake vs. IEC is powerful.
- Very strong team of extremely competent and qualified individuals.
- Strong effort in studying non-Nafion membranes and electrodes. This is a challenging and important problem, and even a moderate success can be used as a model for subsequent efforts with other non-Nafion membranes.
- Systematic approach to characterize new materials.
- LANL has great facilities and know-how. The fuel cell team is still good, and LANL (including this work) can work the "hard" and more challenging themes. The group seems to be creative.

Weaknesses
- Fluorinated additives are being used to achieve desired water content of the ionomer. The long term implication of using fluorinated additives is not known. Cost projection of the membrane using fluorinated bis-phenol is not known.
- Unclear if water uptake versus swelling is the important parameter for understanding durability – should be reexamined to confirm water uptake/swelling is the same. Assumptions are being made to understand HFR increase (increase in interfacial resistance) – it would be important to see how/why this can be done. Should focus on hydrogen – if DMFC work is done, it needs to be repeated in an hydrogen cell.
- No evidence is presented that such membranes can achieve conductivity targets using 25 or 1.5 kPa inlet water vapor pressure.
- Project needs more team coordination and focus.
- Role of collaborators was not clear. More analysis of failure mechanisms and performance loss/limitations is needed.

Specific recommendations and additions or deletions to the work scope

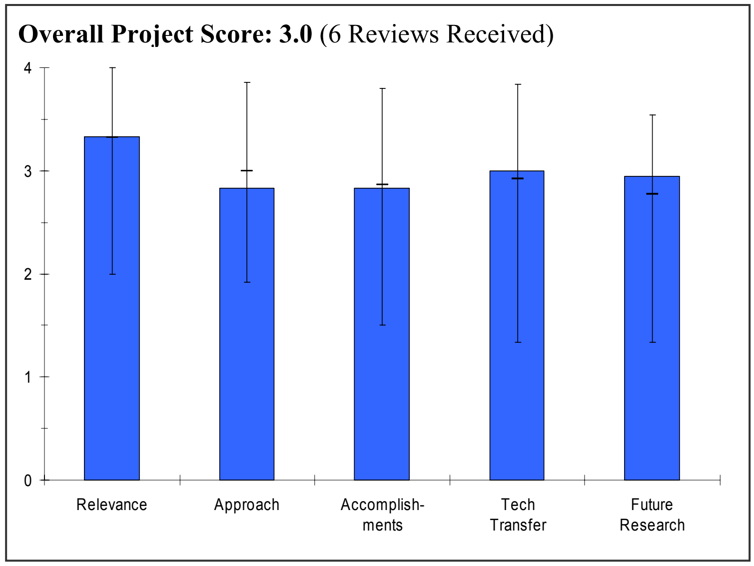
- Quantitative validation is needed to confirm that the increase in HFR is related to the increase in the interfacial resistance between the membrane and electrode layer. Confirmation that water uptake can be considered the same as swelling in regards to durability. Focus on hydrogen cells.
- Focus and extend OCV and start/stop tests to interface-optimized MEAs using Nafion in electrodes and BPSH, 6F, 6FCN membranes. Though difficult, try to obtain access to a wider variety of non-Nafion membranes. Measure fluoride emission rate on 6F type membranes in OCV and start-stop. Report performance with inlet water vapor pressures between 25 and 1.5 kPa. Work to improve performance with inlet water vapor pressures between 25 and 1.5 kPa.
- This same approach should be applied to other new membrane materials being funded by the DOE. A key development of this project should be design rules for incorporating non-Nafion polymers into fuel cell electrodes.
- Determine the true cost and fuel cell durability of the membrane possessing fluorinated bis-phenol additive. Assess the feasibility of using some non-fluorinated material to achieve similar water uptake advantage.

Project # FC-04: Advanced Fuel Cell Membranes Based on Heteropolyacids

John Turner; NREL

Brief Summary of Project

The goal of this research at the National Renewable Energy Laboratory in partnership with the Colorado School of Mines is to develop the methodology for the fabrication of a 3-D cross-linked, hydrocarbon-based membrane using immobilized heteropolyacids (HPAs) as the proton conducting moiety. HPAs and their salts exhibit high proton conductivity at low humidities (below 25% RH) and at elevated temperatures (well above 100°C). NREL will apply its understanding of the structural, chemical, and thermal properties/stability and proton conductivity towards meeting the DOE goals for advanced membranes.



Question 1: Relevance to overall DOE objectives

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

- This project is relevant to DOE objectives.
- Immobilization of highly proton conductive materials is a good direction to proceed.
- High temperature low relative humidity membranes are critical components for PEM fuel cells. The barriers and an understanding of the challenges were well presented.
- Project aligns well to cost and durability barriers.
- New membranes are absolutely required to inverse PEM durability & decrease cost.
- This work is an alternative approach of developing low-cost fuel cell membranes for low relative humidity operation.

Question 2: Approach to performing the research and development

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

- Hpas are water soluble and they can wash out very easily under fuel cell operational condition.
- PMG material, which is being used as host polymer matrix, may not be stable under oxidative fuel cell condition. It may survive in solar panels where peroxides are not formed.
- Immobilization by chemically attaching HPA with polymer is better approach.
- The development of cross-linked hydrocarbon membranes using immobilized heteropolyacids is proposed. A very worthwhile goal. The approach to achieve this goal is well thought out.
- Like many of the new high temperature membrane approaches, the breadth of the material sets and processing options is getting very large very quickly.
- Not clear that this heteropolyacid work is different than past HPA work.
- Elegant, innovative concepts. Excellent organizations of options for available resources. Need to focus on a few options only.
- The project activities are too diversified respect to the budget. The PI needs to focus on acquiring fundamental understanding to identify the opportunities and the challenges of using heteropolyacids for fuel cell membranes.
- There must be more analysis regarding on the cost projection of heteropolyacid-based membranes.

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

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

- The conductivity of HPA composite membrane drops at 80 oC.
- The conductivity at 120°C is 2 order less than Nafion under the same condition.
- No fuel cell performance given.
- Considering the project start date (FY 2005) and the funding level ($150K/yr),much work has been accomplished. Stabilization of the HPAs has occurred and membrane films have been fabricated. The films are flexible and the HPA does not leach out of the membrane. Fenton's reagent test performed shows some concerns about leaching out of the HPA. Preliminary conductivity measurements accomplished albeit at only low temps 27 to 80°C and 100% RH. Higher temperature and low RH measurements are needed.
- Given the short time it has been operating, results to date are encouraging. But the PIs have only scratched the surface of what will have to be done to allow making a judgment that there is a path to success with this approach.
- This project states to be working on improving durability and reducing cost, yet performance is shown.
- Poor conductivity threatens viability of this approach. Too much NMR & TGA analytical data & too little data analysis. Need more evaluation of data.
- Significant progress on immobilizing the heteropolyacid and characterizing the membrane has been made.
- The PI needs to do more evaluation of membrane conductivity at low relative humidity (RH) conditions. The use of heteropolyacid is to eliminate the humidity dependency of fuel cell membranes. Even though the values of conductivity measured at the saturation condition are relatively low, the more important aspect should be the characterization of the humidity dependency.
- More membrane characterization works are required such as gas crossover rates, mechanical properties, chemical stability, water uptakes etc.

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

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

- Limited technology collaboration.
- More collaboration will be better.
- Strong interaction with Colorado School of Mines and also relationship with 3M through School of Mines interaction for the recently funded High Temp Membrane Program (Topic 1). Poster FCP-06.
- May want to consider adding an industry member to the collaborative effort.
- The project has appropriate collaboration between a National Lab and a university to acquiring scientific understanding of the use of heteropolyacids in hydrocarbon membranes. Having another partners for fuel cell testing will be beneficial. anthropomorphic

Question 5: Approach to and relevance of proposed future research

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

- Planned immobilization techniques are appropriate.
- Proposed future work is sound. Increasing thermal stability is a must the range for stability should be raised to 150°C not just 120°C (an operating temperature). A better procedure for measuring conductivity is needed. Gas permeability measurements should be included. What about water uptake and membrane swelling issues?
- With limited resources, prioritization and focus on only the key issues is warranted.
- Future plan appears to address the issues and questions identified through the research, but may not have enough time to complete the work.
- Plenty of work left to be done.
- The proposed activities are forwarding to the right direction, but it is too diversified considering to the budget. It needs to be focused on understanding specific issues rather than evaluation of different materials.

Strengths and weaknesses

Strengths
- HPAs are well known for their proton conductivity at high temperature. Incorporation of this material in membrane properly may allow the membrane to operate at higher temperature.
- Good synthetic approaches are being taken for immobilizing HPA in polymer matrix.
- The goal of developing a composite hydrocarbon membrane using HPAs as the conducting moiety is very sound. The teaming relationships are very good and true collaboration is occurring.
- A tremendous breadth of materials and processes are available. Technical members are strong.
- NREL and CSM appear to be working well together.
- Excellent approach to a key PEM issue.
- This project directly addressing the goal of developing low-cost, no-humidity required fuel cell membranes.

Weaknesses
- The conductivity of HPAs seems to decrease when incorporated into polymer matrix.
- Compared to Nafion, very low conductivity at 80°C, 100% RH.
- It is very unlikely that the conductivity will increase by 3D cross linking of the polymer.
- The DOE goal is high conductivity at low RH and high temperature. While the membrane may be capable of this,a better procedure for measuring conductivity is needed. Gas permeability measurements should be included.
- When not adequately staffed, these kinds of material development projects may meander forever.
- Collaboration only between NREL and CSM.
- Too few resources – need to work fewer options and make more progress on those few materials. No discussion of system performance parameters!
- Not enough budget has been assigned.

Specific recommendations and additions or deletions to the work scope

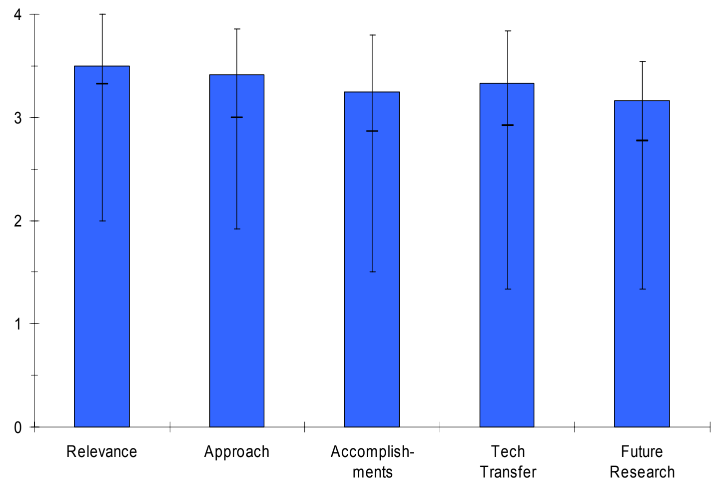
- Increasing thermal stability is a must. The range for stability should be raised to 150°C not just 120°C (operating temperature). A better procedure for measuring conductivity is needed. Gas permeability measurements should be included. What about water uptake and membrane swelling issues?
- Set some very specific targets for three of the most critical properties and a timeline to meet them. Then determine the resources needed to answer those questions in that time, and if it cannot be done, consider a Go/No-Go decision instead.
- Should either increase funding or narrow the alternatives.
- Rather than evaluation different materials, the project has to focus on understanding the binding mechanism of HPAs and the conductivity mechanism in the membrane.
- Work with the hydrocarbon membrane people to learn about what to expect in high temperature membranes. Else establish collaborations with someone in the MEA industry to understand what to expect from high temperature membranes. Until we have some real high temperature membranes, specifications cannot be made.

Project # FC-05: Enabling Commercial PEM Fuel Cells with Breakthrough Lifetime Improvements
Gonzalo Escobedo; DuPont

Brief Summary of Project

This DuPont project is utilizing both experiments and modeling to develop a better 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: 3.3 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Lowering the cost and improving the durability of membranes are key criteria for the President's Hydrogen Fuel Initiative (HFI).
- This project targets the HFI's major barrier and is completely relevant to a major component of the HFI.
- Good relevancy to DOE objectives and goal of durability. Clear definition of operating (assumed) conditions is necessary, and it should be consistently used in the durability experiments.

Question 2: Approach to performing the research and development

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

- The study of peroxide and mechanical stress together yields deeper insight into failure mechanisms. Chemical modification, reinforcement and mitigation mechanisms are reasonable approaches for reducing the identified failure mechanisms. Characterization using dielectric spectroscopy is reasonable. Study of model compounds is appropriate for accelerated tests.
- Approach is very reasonable. Model membranes have demonstrated "weak" aspects of membrane. A UTC model seems to be applicable to PFSA-type materials and insight can be gained. University of Southern Mississippi (USM) analytical work seems to be providing insight on membrane degradation mechanisms.
- The approach is refreshingly appropriate for fuel cell research. The combination of chemical degradation mechanisms elucidated by model compound studies with appropriate mechanical and dielectric relaxation studies provides a very insightful picture of what is happening. The application of the results to fuel cell and stack testing is impressive and most appropriate. Combined with e modeling effort the project demonstrates an outstanding approach that has been lacking in fuel cell research in the past.
- It is not clear how the results on the other model compounds at USM relate to the work at DuPont.
- Systematic approach to develop new material.
- Good approach showing progress towards goals.

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

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

- Little progress for the year for a project funded at this level. Conflicting interpretation of loss of mechanical properties (suggesting chain scission) and end group attack (stated as being only chemical attack mechanism). Improvements in chemical attack resistance and membrane lifetimes is compelling.
- CS/reinforcements show significant improvements in membrane durability. USM post-mortem results look very interesting. UTC mitigation technique has shown success with the DuPont material.
- The accomplishments are impressive and very satisfying. The progress is excellent on a very difficult problem.
- Good progress on membrane development with reinforced type membrane. It is necessary to show model validation data. Also it is necessary to investigate the corellation between bubble formation and real-world failure mode.
- Results of drive cycle tests with new materials are very encouraging.

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

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

- Few presentations or publications for a project of this size; a number of the cited presentations were not for this period of performance. Team has necessary strengths; however USM results seem overrepresented for fraction of budget.
- Very high. Collaboration with both USM and UTC seems to be very good. Work from all parties is leading to success of the program.
- The partnerships are appropriate and fully engaged. Excellent example of collaborative research.

Question 5: Approach to and relevance of proposed future research

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

- Proposed research feeds into program well and can bring further insight. Cost analysis is of particularly high value. At this stage of the project, modeling of combined chemical and mechanical effects is unlikely to yield fruitful results.
- USM post-mortem work does not seem to correlate with improvements seen in fuel cell experiments done by DuPont. A strong effort will be needed to interpret the differences seen between USM and DuPont results and should be followed closely to improve membrane durability. The PI did not state what will be done in order to find agreement between the findings of DuPont and USM.
- Project is close to completion. Future plans involve tieing last loose ends.

Strengths and weaknesses

Strengths
- Have shown meaningful lifetime improvements. Bubble formation in membranes following Fenton's reaction is very interesting.
- DuPont is a world leader in the field of polymers and by using their model materials, they have learned the "weak" spots of their membrane and modified them. Furthermore by introducing the reinforcement and then using the mitigation techniques of UTC, major improvements in durability were achieved. Analytical work by USM has provided even more insight.
- Very strong approach that involves an appropriate mix of chemistry, physics, material science and engineering.
- Interesting findings on the role of the side chains in the response of the model compounds to Fenton's reagent. The combination of methods to reduce the FER appears to be effective.
- Systematic approach of membrane material development.
- Leverage model base engineering.
- Well coordinated team effort.

Weaknesses

- Lack of cost analysis, contradiction of chemical attack mechanisms and observed mechanical properties/ dielectric spectrum, and lack of discussion of mitigation mechanism (while some aspects proprietary, is this based on architecture or additives? Some generalities would be useful to understand if adverse interactions exist or if the cost would be likely prohibitive).
- Do the improvements meet the DOE targets for performance and durability for 2010? 2015? If not, what will DuPont do? DuPont needs to show durability as a function of cell potential versus time instead of always FER. FER is only applicable to PFSA type membranes and does not contribute to the industry. DuPont has not talked at all about cost – how much will this cost? Will it reach the targets?
- The seeming lack of correspondence between the USM studies and the DuPont combination of stabilization methods suggests that a closer integration of the work could be beneficial.
- Not sufficient data to support model validation.
- Accelerated fluoride emissions test results were short and it is questionable whether the conditions are truly accelerated.

Specific recommendations and additions or deletions to the work scope

- Cost analysis first, delay UTC modeling work.
- DuPont needs to obtain an understanding of the results of USM and determine why such degradation is seen and how to mitigate that. If not, the membrane optimization will not be complete. DuPont needs to consider whether this material will meet the DOE targets or if more work needs to done. DuPont needs to disclose (or discuss) the cost of these materials. As we all know, Nafion is not cheap and this approach will only add more cost.
- Project close to completion.

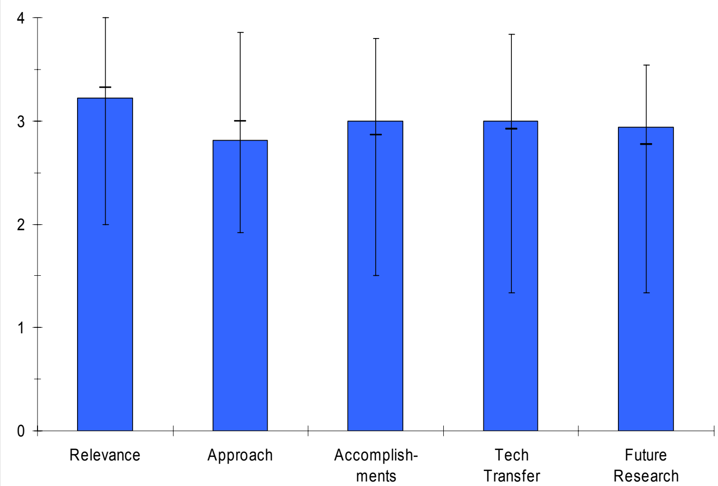
Project # FC-06: Development of a Low-cost, Durable Membrane and MEA for Stationary and Mobile Fuel Cell Applications

Scott Gaboury; Arkema Chemicals

Brief Summary of Project

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

Overall Project Score: 3.0 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Little economic and technical data were shown on why this approach will reduce cost and durability of membranes.
- This project will likely reduce membrane cost and improve durability but questions about this membrane's tolerance of dry gases are unanswered.
- Development of new membranes that are cheaper and more durable is important .
- Project addresses key barriers of cost and durability.
- Good alignment of the project with DOE objectives.
- This project corresponds well to the multi-year R&D plan.
- A durable membrane has applicability for both stationary and automotive applications.
- Blending Kynar with polyelectrolytes is NOT that novel.

Question 2: Approach to performing the research and development

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

- It is not clear that this approach can result in a stable viscous mixture. Test durations are not sufficiently long.
- It appears that this research group has developed their own durability test, but their membranes must be compared to other membranes by the same metrics, i.e. Fenton's test or cycling through OCV or testing at OCV, or simply running a fuel cell.
- More conductivity data needs to be performed at low %RH. The dry cathode experiment is not very impressive considering that the cathode makes water and that the anode is presumably humidified.
- It is a little difficult to evaluate the durability work as explicit chemistry is not described. The results appear to be going in the right direction but the approach could use a bit more of a chemicals and materials emphasis. Fast screening techniques are fine for manufacturing but do not promote fundamental understanding.
- Eliminating apparent dead end with M31 material is good.
- M40 material durability work should continue to be pursued.
- Basic properties of the polyelectrolytes should be explained.
- PVDF is a low T_g material. It gets softened at 120^oC. High temperature application could be an issue.
- PVDF binder softening may lead to the leakage and failure of the gas separating membrane and induce cross-over.

- This approach may be ok for low temperature (<80°C) fuel cell application, but questions arise for 120^{o}C fuel cell application.
- Kynar blend adds mechanical stability. Arkema is taking a systematic approach to improving performance, durability, and operating range.
- The simple blending of Kynar and polyelectrolytes has been conducted in the past, yet the researcher made no references to the pros and cons of the past efforts. How is the Arkema effort different?
- There is no substantive discussion of the resulting composite membrane re: structure; nor is the nature of the polyelectrolyte clarified.
- The presenter implies that these systems are lower cost than perfluorinated systems. Without quantification this assertion is not defensible.

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

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

- The progress since the last review seems to be slow. Many issues remain unanswered.
- 0.1 S cm^{-1} at 100% RH is readily obtainable by other materials. Cost is a huge advantage though and if these systems could be made to perform under truly low %RH conditions with real durability then this would be a very impressive project. The membrane survived 120^{o}C but did it still conduct at 120^{o}C?
- Progress appears to have been made for cost and durability.
- Current durability performance of 2100 hours at steady state appears to be well below the 2006 DOE target of 9000 hours.
- Gen-D (M40) material is a significant accomplishment.
- The proof of concept for the high throughput casting method is a good step towards large scale manufacturing process.
- Arkema identified degradation mechanisms, implemented mitigation strategies and achieved >2500 hours without degradation in *ex situ* tests. Performance is comparable to state of the art Nafion.
- Higher temperature performance was achieved; but the assertion of higher conductivity in water does not translate readily into higher performance in a stack.
- The researcher needs to clarify the differences in chemical structure (at least qualitatively) as he progresses from Gen A to Gen D of the polyelectrolyte.
- Long-term stability of the composite structure (re: durability of the polyelectrolyte within the PVDF) is not clearly explained; e.g., was there a tie-layer or a grafted interlayer polymer involved?
- How does *ex situ* sulfur loss correlate with *in situ* sulfur loss?
- The reduction in weight degradation is a significant accomplishment.

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

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

- The partner team seems to be good; however it is not clear which specific tasks each team member is focused on.
- Good collaborations with universities and industry.
- Good team of collaborators are engaged in this project.
- Collaborations include universities, fuel cell stack/system developer, and National Lab.
- Except for future plans, no current tech transfer or collaborations were evident,.
- It would be useful if the researcher made comparisons with other degradation studies of Kynar blends or with other polymeric systems.

Question 5: Approach to and relevance of proposed future research

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

- The PI did not explain how corrective action of current weaknesses will be implemented.
- More proton conduction and durability testing under harsher conditions needs to be performed with these systems.
- Continuation of M40 material work is good.
- How will they test end of life durability?
- Temperature effect of the membrane should be studied.
- Need to conduct TGA and DMA tests on membrane.
- Future work addresses remaining issues such as low RH and high temperature.
- Future plans are pedestrian and appear to be hand-offs to other companies for further testing and evaluation.
- How will the researcher distinguish between membrane performance and electrode formulations.

Strengths and weaknesses

Strengths
- The PVDF platform is an excellent scaffold for new ionomers that may need more structural integrity.
- Morphology work is a good addition.
- Focusing on M40 material.
- Easy fabrication technique.
- Good control on IEC and mechanical properties of the membrane.
- Good collaboration partners.
- Use of PVDF and polyelectrolytes allows the decoupling of conductivity and mechanical properties.
- Excellent results in the polyelectrolyte/Kynar blends.

Weaknesses
- They never explain why they are not conducting standard durability tests. They should not do only their own tests in isolation.
- There is a lot of fundamental understanding that can be learned from this research however that was not included in the presentation.
- Not much work apparent yet on end of life durability for the M40 material with respect to typical drive cycles.
- Low T_g binding material, PVDF, may not be stable under 120°C operation temperature.
- Peroxide stability of the polyelectrolyte material should be evaluated.
- Thermal effect on phase separation between the polyelectrolyte and PVDF should be evaluated.
- Need to include more information on composite structures, degradation mechanisms, polyelectrolyte chemistry, and stack testing data.
- Need more information on the manufacturing scale-up, uniformity of composite structure from a doctor-blade casting process and the definition of the electrode structure (will it be of the composite material or will it be Nafion-based?)
- Degradation tests need to be conducted over longer times. Extrapolations of short duration tests may not be adequate.

Specific recommendations and additions or deletions to the work scope

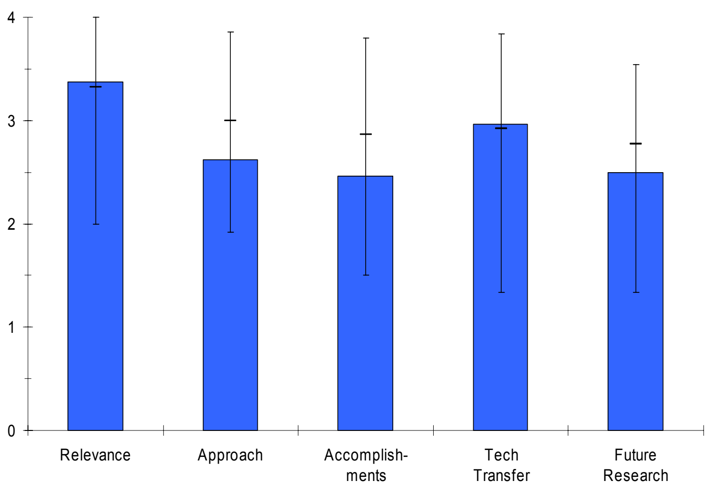
- Include more background on the economic and technical rationale for their approach over Nafion.
- Concentrate on conductivity testing and durability testing. Would be good to see membranes tested *ex situ* under much drier conditions. Not too soon to develop good electrodes for these materials as this is an important problem. Otherwise scale up is premature.
- Present durability performance to DOE targets for typical drive cycles as well as steady state.
- Gen-D (M40) material should be assessed under aqueous hydrolysis condition and Fenton's degradation condition.
- Thermal effect (~120°C) on the membrane should be studied.
- Use the Fenton's test to check durability.
- Compare Arkema systems with past PVDF blends.
- Take more than just an Edisonian approach in blend formulation.

Project # FC-07: Hydrocarbon Membrane

Christopher Cornelius; SNL

Brief Summary of Project

Overall Project Score: 2.7 (8 Reviews Received)



This Sandia National Laboratories polymer electrolyte membrane (PEM) and catalyst coated membrane (CCM) development effort is an alternative approach to address the physical property limitations of perfluoronated PEM materials such as Nafion. The limitations include its poor mechanical properties at temperatures above 80°C, high methanol flux in direct methanol fuel cells, loss in proton conductivity at elevated temperatures, and high material cost. This has resulted in a considerable amount of research to correct these material property deficiencies with an alternative polymer electrolyte. While Nafion is currently one of the state-of-the-art polymer electrolytes for fuel cells, several alternative polymer electrolyte materials have demonstrated better fuel cell performance characteristics. The polymer family of polyphenylenes represents a class of thermoplastics that has the potential of being used as a PEM within a fuel cell. These types of polymers are known for their excellent thermal and chemical stability, while maintaining organic solubility making it possible to form mechanically robust films. The chemistry afforded by the parent Diels-Alder polyphenylene represents a system that has tunable chemical structure and properties.

Question 1: Relevance to overall DOE objectives

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

- The project is relevant to the goals and objectives of DOE.
- Clearly identified technical relevance and interactions/collaborations.
- Development of HC membranes is aligned well with the Hydrogen Fuel Initiative.
- Represents long term DOE goals.
- MEA is being designed with new ideas being incorporated in both membrane and electrode.
- Project is focused on one of the most critical areas. Stable and lower cost membranes will be required for project success. Sandia is well suited to address this topic.

Question 2: Approach to performing the research and development

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

- This approach is very common.
- Sulfonated polyphenylenes are susceptible to hydrolysis under high temperature fuel cell condition.
- There is a high possibility of desulfonation of sulfonated polyphenylene, which will lead to loss of IEC of the membrane.
- The use of the SDAPP is a sound approach re: HC membranes.
- The enumeration of the different expected properties is well developed, especially with the understanding of the role of electrode versus that of the membrane material.
- The comparison with Nafion systems is well-done.
- From the presentation, it appears the technical approach is focused on SDAPP binders and not membranes. Optimization of performance through binder design has greater focus than on membrane characterization and durability.

- SDAPP-type membrane materials do seem to provide improvements in FC performance and lower gas cross-over.
- Preliminary data illustrates that adding ionomer to electrode is not going to be easy – need to incorporate this problem into the approach.
- Short on details about blends, control of permeability, porosity, electrochemical activity, etc. Also, many of the parameters are coupled, complicating the data analysis.
- Clearer definition and metric of interfacial resistance is necessary.
- It is not clear to this reviewer how these membranes are any different than prior hydrocarbon-based systems developed over the last 30 years – all seem to be highly aromatic and sulfonic acid-based.
- Such systems may impact stability with lower RH but there weren't any time-related results to demonstrate stability to peroxide, etc.
- Still need pressurized systems to maintain water management and facilitate proton transport.

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

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

- The need for non-Nafion ionomers for non-Nafion membranes is well proven.
- At high temperature (120°C, 50% RH) polyphenylene material is performing a little better than Nafion, but it is not doing that great.
- The cost of dione material is not given.
- No studies have been done on durability of the PEM material.
- Focusing on the roles of the electrode versus the membrane materials is appropriate.
- The speculation of a more "porous" SDAPP structure versus Nafion-based electrodes is intriguing.
- The researcher has a solid insight into structure-property relationships, unlike the other presentations which take a more Edisonian approach to structural variations.
- Too much focus on binder design, however good progress.
- SDAPP membranes show improvements in fuel cell performance and hydrogen cross-over. A large amount of work was spent using SDAPP as the ionomer in the electrode to minimize interfacial resistance between the membrane and the electrode, however, after all this work, when Nafion was used as the ionomer, performance was better. Very little membrane characterization and no electrochemical studies (CV) done at this time.
- Strong results in both membrane and electrode, but the rationale is not clear. A more systematic approach is needed. The membrane-electrode interface needs to be better characterized.
- Performance of baseline membrane (Nafion) is too low. Interfacial resistance could be an issue. Further investigation of this issue is necessary to fix it.
- The early stage results are promising.
- Degradation and lifetime tests need to be carried out (early life)
- Electrodes and membrane interfaces must be better understood, Sandia should focus on such fundamentals.
- Suggest using additional electrochemical tests to assess the MEA.

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

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

- Lack of interaction with industrial partners.
- More collaborative partners are needed.
- The slides clarify the roles of the collaborators well.
- Very impressive list of collaborators.
- Interaction seems to be high. Clarification of who is doing what is needed for the DOE to assess the contributions of each collaborator.
- Interacting with numerous institutions and companies.
- Role of partners and collaborators is not clear.
- PIs should consider to providing developed material to fuel cell developer/automotive OEM to evaluate under real-world conditions.
- Appears to be acceptable for this stage of the project.

Question 5: Approach to and relevance of proposed future research

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

- Too broad.
- Not enough time for evaluating four different tailor-made hydrocarbon ionomeric materials for electrode and PEM utilization.
- There is no information on types of inorganic materials to be used in organic-inorganic composites.
- Future themes were well-defined, but the details are lacking.
- Much work to be done in the field. Recommend the team show timeline or prioritization of future work.
- Too much focus on reducing the interfacial resistance when mass transport losses seem to be causing more losses. Strong need to understand what is happening in the electrode (electrochemical/gas transport) before future research is carried out. Need to correlate AFM with membrane properties.
- A more systematic approach is needed. "Optimizing" morphology assumes that they know what a good morphology would look like. A study of structure-property relationships could tell you this, but it cannot be stated a priori.
- Needs further improvement of fuel cell performance in a systematic way.

Strengths and weaknesses

Strengths
- Understanding of DOE objectives and underlying challenges.
- The steps taken to solving the problem are adequate.
- The researcher gave a solid presentation and has a sound understanding of structure-property relationships.
- SDAPP material shows promise as a membrane for FC applications. Analytical work (AFM) provides vivid images and could provide great insight.
- Strong collaboration with other institutions.
- Few projects address both membrane and electrode, so this is relatively uncharted territory. However, this is a challenging problem that needs to be addressed, and the results are very relevant to new materials development. A well executed program has the potential to impact many efforts in future years.
- Capability of membrane synthesis.
- Sandia has great facilities and know-how. The team is quite qualified to address these challenges.

Weaknesses
- More work is needed in materials study.
- Collaborations should be established to acquire better ionomeric material.
- There is no clear attempt to check on durability of the HC systems.
- There is a need to better understand the costs of these HC systems as compared to perfluorinated structures.
- Should focus near term research on key durability issues which have been displayed by hydrocarbon membranes.
- Very little membrane characterization – tensile strength, swelling, etc. No electrochemical studies of electrode surface – CV, EIS, specific FC conditions to understand electrode structure. Too much focus on using SDAPP as ionomer in electrode layer without understanding of the mechanisms.
- A more systematic approach is needed. Performance improvements are impressive, but the source of the different effects needs to be identified.

Specific recommendations and additions or deletions to the work scope

- The future work is too broad. More focused approach is needed.
- Continue the excellent collaborations.
- Use cyclic voltammetry to help elucidate structure-property relationships.
- Expand on the analyses, e.g., by operating at various oxygen pressures, to clarify theories on SDAPP "porosity" versus that of Nafion-type electrode layers.

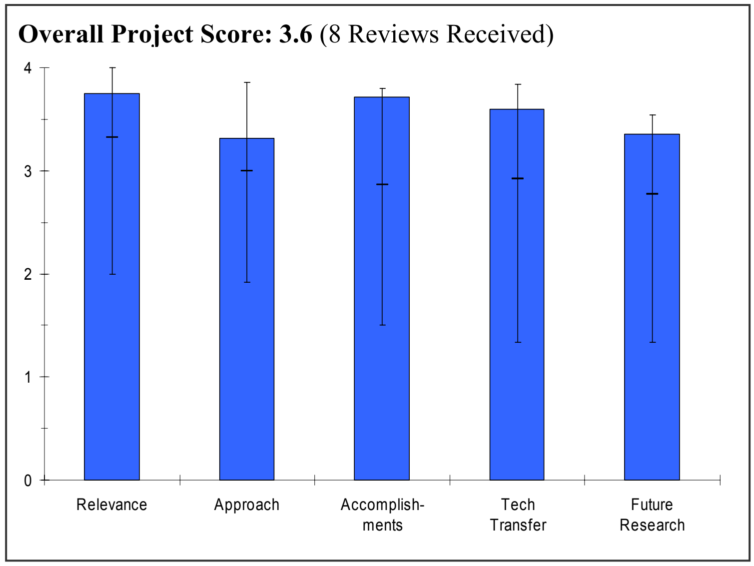
- There is a strong need to quantify the pros/cons of using SDAPP in the electrode layer – as of now, there is no benefit. Helox experiments need to be run to investigate mass-transport phenomena. Experiments need to be run to look at the interfacial resistance specifically. CV experiments need to be run in order verify how SDAPP affects the Pt surface. Serious holes that need to be filled before further work can be done with a logical approach. AFM work must be correlated to membrane properties in order to gain understanding of which morphology is beneficial.
- Need H_2O_2 stability data.
- Need to add to approach – integration of membrane and electrodes, how to make electrodes out of SDAPP.
- With so many parameters being varied, a design of experiment approach could help sort through the many effects.

Project # FC-08: MEA and Stack Durability for PEM Fuel Cells

Mike Hicks; 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, is capable of meeting 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 is on MEA component development, MEA characterization and diagnostics, and defining a system operating window. The overall objective is to develop a pathway/technology for stationary PEM fuel cell systems for enabling DOE's 2010 objective of 40,000 hour system lifetime to be met.



Question 1: Relevance to overall DOE objectives

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

- The development of fuel cells with lifetime improvements is necessary for reaching the President's Hydrogen Fuel Initiative. The project investigates GDL, membrane, MEA and stack issues with membranes being the focus.
- This project is highly relevant to the DOE Hydrogen Program. Increased MEA durability is critical to the success of fuel cells in stationary applications. The DOE goal for 40,000 hours durability should be considered a minimum requirement for success. Much of the work has applicability to the automotive sector. The cycles chosen for the accelerated testing provide stress to the MEA and are likely to be relevant for predictions of lifetime in stationary conditions. Load cycling that more closely resembles driving profiles would stress the MEA even harder and be more relevant to automotive applications.
- The focus on durability (> 40,000 hrs.) is clear and in line with the DOE program objectives.
- The realistic goals of high-volume manufacturability and low-cost are also appropriate.
- The determination of membrane-electrode assembly failure mechanisms is critical to determining the materials limitations and operating conditions accelerating failure.
- Great focus on durability, but this is the only barrier addressed.
- Project is directly relevant to the Hydrogen Fuel Initiative and fully supports MYPP.
- Project directly targets a principal barrier (durability) to the successful commercialization of stationary fuel cells.
- Project addresses one of the major barriers for Fuel Cells, durability. While this project is directed toward stationary power, the project is applicable to both stationary and automotive applications
- The PI and his team are addressing each critical technical problem which needs to be solved to commercialize PEMFC.

Question 2: Approach to performing the research and development

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

- Study of model compounds is appropriate for accelerated tests. The accelerated tests and models presented are necessary for estimating fuel cell lifetimes without long test times. Investigating reinforcement and edge effects is sensible, as these have been identified as being related to membrane failure. The synergy of GDL, MEA and stack work with membranes is not apparent.

- Accelerated testing and statistical analysis are important components of the approach. The approach is comprehensive. The work with model compounds should result in improved insights into changes in membrane characteristics upon aging. However, it must be shown that the results apply to actual membranes.
- The step-by-step approach is systematic and the linkage of structure developments to optimized system operating conditions shows the researcher's practical stance.
- The approach is holistic and very comprehensive, and the pace of the delivery points to a methodic approach to tackling the challenges.
- This project has a good, balanced approach. The approach could be improved by incorporating post-mortem microscopic characterization to correlate failure location with conditions encountered at this location.
- Approach addresses durability very thoroughly.
- At some point it would be beneficial to understand the cost implications of achieving the durability identified in this project.
- Project is sharply focused on pathways and technology to enable achieving DOE 2010 objective of 40,000 hr system lifetime.
- Project incorporates innovative dual approach of combining 1) optimization of MEAs and components for durability with 2) optimization of operating conditions to minimize performance decay.
- Approach addresses important technical barriers. 3M haa developed accelerated tests AND a correlation to real time tests which allows them to predict lifetimes from accelerated tests. New work starting with nonuniformities may be important and could feed in to requirements for manufacturing.
- The PI has a very keen understanding of fuel cell technology. Each technical barrier is being addressed very comprehensively.

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

This project was rated **3.7** based on accomplishments.

- Chemical decomposition due to peroxide has been demonstrated on model compounds. Edge failure has been shown to be mitigated by a specific (unstated) approach. GDL and stack studies either have not been demonstrated or gave uninteresting results. Segmented cell results show predictable performance in terms of current distribution at low stoichiometric conditions. Non uniformity modeling studies have not been validated at this time; reported results for uneven catalyst layers are interesting, if verifiable. Conclusions regarding reinforcements are suspect and potentially misleading.
- 3M appears to be making good progress in improving MEA durability. Based on accelerated aging tests, including load cycling, 3M has predicted better than 20,000 hrs. 3M and Plug Power identified edge failures as a major cause of early failures and indicated that design changes along the edges of the active area have eliminated the problem.
- The researcher showed a set of excellent accomplishments (from test equipment for component evaluations, through membrane / MEA developments, to system tests and lifetime modeling.)
- The GDL capillary pressure measurement development is a good example of technical collaboration with CWRU; the "lack of correlation between reinforcement and physical property durability" is well done; but this data set needs to be checked out.
- The rapid aging with ion-exchange with Fe cations and the evidence of premature edge effects are useful findings, as is the work on decay mechanisms, using model compounds.
- In general, this project has yielded very useful data on the various causes of membrane and MEA durability.
- Saratoga system test of MEA is valuable to make sure component level durability gains are maintained when incorporated into a system.
- Durability progress is very good.
- Impressive list of technical accomplishments from durability testing and procedure development to identification of membrane degradation mechanisms and operating strategies to minimize performance decay.
- New MEAs with 3M ionomer have achieved approximately 4x better durability.
- PI indicated significant improvements in durability are achievable through demonstrated pathway towards 20,000 hr MEA lifetime with 3M PEM MEAs under accelerated near OCV load cycle tests.

- Overall, progress in this project has been good. 3M has demonstrated lifetimes in accelerated tests that extrapolate to lifetimes which meet the 2005 goals. Development of GDL characterization techniques is important work. Work with reinforced membranes and lack of correlation of durability with mechanical strength appears at odds with others experience and may need more investigation to determine what the differences are and if relative humidity cycling is the appropriate test.
- 3M and the PI have shown historically very good progress that is well presented and easily comprehended.

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

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

- Case Western contributions in the area of small molecule analogues are very useful. Role of Plug Power and University of Miami less clear as non-uniformity studies are much less interesting than the membrane work presented. Significant presentations, but little in the area of publications.
- Very good collaboration between the partners is evident. The work at Case is leading to improved understanding of membrane degradation, and the Plug Power work is important third-party validation of 3M's approach.
- This talk very clearly identified the collaborators and the roles each played in the different sub-projects in this very comprehensive, overall, study.
- Outstanding collaboration with the collaborators contributing in their areas of expertise.
- Strong collaborative team.
- Leveraging the expertise of all members well.
- Project has established reasonable coordination mechanisms.
- It is not clear as to the extent of collaboration with other organizations conducting MEA and stack durability research. For example, would there be synergistic advances if this project coordinated more closely with the LANL durability activities led by Rod Borup?
- Collaboration with Plug power and use of the Saratoga system provides benefits and experience with stationary systems. University involvement at Case and U of Miami.
- 3M has a great mixture of academic (university) and other commercial developers.

Question 5: Approach to and relevance of proposed future research

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

- Project looks to finish/expand current tasks.
- Good plans for future activities.
- Again, this section was well-presented and well-focused (MEA and stack development with a key fuel cell partner; MEA degradation tests along with attempts to correlate structure and properties, statistical lifetime predictions.)
- The focus of fluoride release is important, but the need to determine whether the fluoride comes primarily from the electrode layer or the membrane is paramount.
- The description of the future plans was too vague.
- Future durability working on lifetime predictions is very good.
- May want to consider estimating cost of achieving this level of MEA durability.
- Future work schedule is logical, building upon past progress.
- No discussion was provided as to technical contingencies or off ramps if unexpected show stoppers arise.
- Future work builds on previous work and is directed at bringing the project to completion.
- The future program is well planned and follows-on to the present program. Keep them FUNDED!

Strengths and weaknesses

Strengths

- Chemical control and modifications are possible.

- 3M is attacking the problem of durability in a comprehensive manner. The work at Case is leading to a clearer understanding of the changes in membrane characteristics upon aging.
- This summary presentation is broad and very comprehensive, although some of the conclusions need to be re-checked: reinforcement and the "lack of correlation" to durability?
- Microscopic analysis of MEAs post-mortem would be a nice addition to determine failure mechanisms.
- Analysis tools are used.
- Project appears to be very focused.
- Very solid project approach and strategy.
- Impressive list of technical accomplishments to date.
- This project can draw from other concurrent projects at 3M (catalyst and advanced MEAs for enhanced operating conditions) and bring in the newest findings from them. This project has developed/advanced accelerated test protocols that are of use to the fuel cell community as a whole and has performed a lot of the work providing an understanding of membrane chemical degradation mechanisms.
- Technical team and company management.
- Great cooperation and coordination with partners.
- Super understanding of technology.

Weaknesses

- Conclusions regarding reinforcements and the data taken by GM (Gittelman) have been overstated and presented out of context. The type of reinforcement investigated involved soft PTFE which does little structurally. In Gittelman's work, the correlations with water uptake seem to suggest water uptake is more important for failure, and other reinforcement types could help this problem. Additionally, for situations where tear is important, any such reinforcement is likely useful. The statements given in this presentation regarding reinforcements may end up being very misleading to those who watched the presentation.
- There are a lot of data that indicate reinforced membranes do improve durability in contrast to 3M who concluded that the reinforced membrane did not improve MEA durability. The different conclusions may well result from the great variability in testing protocols and conditions. From the presentation it was not apparent that the definition of the accelerated testing profiles was made after considering possible failure mechanisms.
- A revelation of the 3M chemistry would help bring greater clarity to why these ionomers show improvements over traditional Nafion.
- More work is needed on the peroxide effects.
- Project only addresses one barrier.
- It is sometimes difficult to determine what work was performed under this project and what work was performed under the concurrent projects in catalysts/advanced MEAs.

Specific recommendations and additions or deletions to the work scope

- Focus on membranes, decrease work on GDLs and MEAs/stacks.
- Standardized testing conditions and protocols need to be defined so that data from different experiments can be compared on a similar basis. The USFCC work in this area should be able to guide future work in the durability.
- Well done! Just focus on your critical assumptions and issues that you have recognized.
- Add in some effort to evaluate cost.
- Increase collaborations especially with other DOE supported fuel cell durability projects.
- This was one of the best presentations I have ever attended.

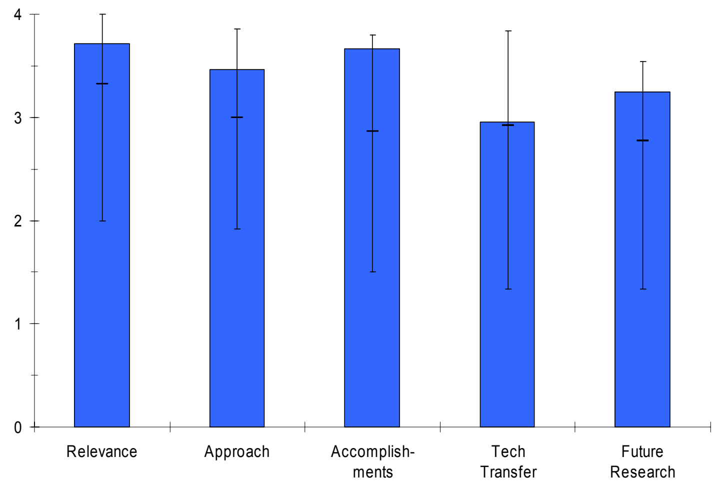
Project # FC-09: Low Pt Loading Fuel Cell Electrocatalysts

Radoslav Adzic; BNL

Brief Summary of Project

The purpose of this Brookhaven National Laboratory project is to develop low platinum-loading fuel cell electrocatalysts. The objectives are to demonstrate the possibility of synthesizing novel electrocatalysts for O_2 reduction with monolayer level Pt loadings and conduct long-term tests in fuel cells with them, commence a study of mixed Pt-late transition metal monolayer electrocatalysts, further characterize a PdCo electrocatalyst for O_2 reduction and a $PtRu_{20}$ electrocatalyst for H_2/CO oxidation, and to gain a deeper understanding of the mechanism of their catalytic action.

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.

- The PI and his team are focusing on the key problems: cost of electrode catalysts and endurance.
- Approach for reduction of PM content in FC electrocatalysts which has demonstrated ability to significantly increase the mass activity in ORR reaction.
- New addition was modification of the nanoparticles surface to improve durability.
- The project is well-targeted to reducing electrocatalyst loading, enhancing catalytic activity, and increasing durability – hence, definitely relevant to the DOE plan objectives.
- The researcher presented well and systematically, although he mumbled at times.
- The project constitutes a very well thought-out approach to one of the most important DOE objectives: high activity cathode catalysts to reduce fuel cell costs.
- The development of improved (higher activity, longer life, and lower cost) electrocatalysts is imperative for fuel cell commercialization in automotive applications.
- These activities are relevant to meeting the DOE objectives. If successful, this project could result in a critical breakthrough on the cost goals for fuel cell catalysts. The alloys being examined may have applications beyond the PEM fuel cell designs being considered.

Question 2: Approach to performing the research and development

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

- The keen understanding of the PI has his team and their expertise in surface behavior and fundamental electrochemistry making good progress.
- Well thought and systematic approach based on fundamentals.
- Well-done! The presentation was comprehensive and its focus on mixed metal systems was deliberate and systematic.
- The researcher was very clear in his delineation of the different types of structures he was investigating (Pt on Pd, then onto mixed Pt metals on Pd, then core-shell nanoparticles.)
- The correlations of structure with chemical activity were clearly presented, including the focus on O_2 reduction kinetics and mass activities.
- The project's approach is a carefully thought-out pathway to the demonstration of high catalyst mass activities.

- The need for checking the resistance of active catalysts against potential cycling damage has been very well addressed.
- The project has been characterized by excellent and rapid transfer from single-crystal results to supported catalyst RDE testing and has made a good start on MEA testing.
- The presenter did not explain the basis of the choice of elemental materials, e.g. What fundamental considerations suggest positive results using Ir, Au, and Fe?
- The presentation would be more effective if the presenter showed photographs or presented overviews of the sample materials and test fixtures.
- The approach taken by the PI appears to be practical and sound. Tolerance to methanol and a synthetic reformate is mentioned. Definition of the reformate and cognizance of impurities likely to be in transportation grades of fuels was not touched upon.

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

This project was rated **3.7** based on accomplishments.

- The work on surface stabilization is very impressive and has made a significant advancement in fuel cell science.
- Excellent progress in new compositions and durability improvement of Pt-based catalysts and new support for increased performance.
- The findings of increased mass activity and the intriguing conclusions on the behavior of Au nanoclusters on stability and durability are impressive.
- The fine-tuning of the Pt substrate in the mixed metal structures as well as the interpretation of the XANES data for chemical stability in DMFC systems represents solid work.
- The combination of theory (re: dual pathway kinetics) with experimental data rounds up a very solid body of work, which results in catalytic structures almost at DOE performance goals – good work.
- Excellent RDE demonstration of new catalysts with high activity per mass of precious metal.
- A good start was made on testing the durability of these new catalysts against potential cycling effects which are likely to be the greatest challenge that they face.
- The gold overlayer work has shown impressive, and very surprising, suppression of potential cycling damage, through an approach that may have wide applicability.
- Development of very nice data-driven theory, and thoughtful application of existing theory to explain results and guide new directions.
- The responsiveness to last year's comments about cyclic testing was excellent.
- The presenter should explain the sufficiency of the test conditions, in particular the voltage range selected, and the temperature, and convince the audience of the direct applicability to the fuel cell operating environment.
- The comparisons of activity and surface area integrity with and without gold were very compelling.
- The results with the Pt_3Fe system offer a high degree of confidence that catalyst loading and cost targets can be met.
- The work on Pd_2Co for DMFC should be a part of a different presentation – it is distracting and lower in priority.
- The technical progress is very significant and appears to show great progress towards several alternate catalysts to platinum.

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

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

- It is not clear that their good work is being transferred to other scientists and developers.
- Collaborations with P. Zelenay at LANL are clearly identified; but the other collaborations were not well-presented.
- The work with Plug Power should be further explained.
- Productive collaborations are in place and more are being brought on line.

- A previous reviewer's comment that collaborations with companies whose core business is the making of catalysts would be most valuable has an important element of truth.
- The collaboration with LANL continues to be productive.
- The process of technology transfer is not clear. When would such technologies be available in commercial components and integration in stacks?
- The results are very favorable – BNL should seek qualification by a component developer/supplier.
- There was no calculation or projection of comparative costs – a succinct statement of cost impacts would be helpful for further confirming relevance and commercial importance.
- The apparent collaboration with 3M, GM, and Plug Power is encouraging. These entities will maintain an "applied" science perspective which will ensure commercial relevance.

Question 5: Approach to and relevance of proposed future research

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

- It appears that the PI and his team have a reasonable plan for the future.
- Would be nice to include more FC test data and durability studies in MEA configuration.
- The steps forward are acceptable; but a further push to further verification in functional stacks with "established" fuel cell companies are important.
- These market demonstrations are needed to show reduction to practice for these academically exciting developments.
- Future emphasis on stabilization by gold clusters is appropriate, as this surprising effect has a "too good to be true" aspect and must be checked very carefully.
- Plan to concentrate on systems that improve the activity per total mass of all precious metals is wise.
- It would be good to maintain a fair amount of the emphasis on fundamental understanding that has yielded substantial progress to this point, even as this type of catalyst gets closer to real application.
- A clear vision of future work was not presented.
- The proposed future research path appears rational. Longer term stability testing is warranted. Hopefully, this work will include determination of any degradation by-products and if and how the by-products might leave the cell.

Strengths and weaknesses

Strengths
- Good fundamental understanding.
- Strong fundamental work.
- The study has led to appreciable progress towards DOE goals in reductions in Pt metal loadings, increases in activity and durability, as well as novel structures.
- The work presents a good balance between experimental data and plausible explanations of structure-property relationships, including modeling of certain kinetic pathways.
- The finding re: Au nanocluster effects on Pt activity and durability is intriguing and must be further explored.
- The project has generated ideas and followed those ideas to new catalysts that give very encouraging results.
- The project has productively blended theory and experiment.
- The project has successfully brought ideas forward from single crystal experiments to real supported high-area catalysts in a short time.
- This project offers critical confirmation that DOE's targets for electrocatalysts are achievable.
- The technical strength of this project is self-evident.
- Collaboration with LANL in the past on MEA testing of the materials developed as part of this project was beneficial for validation and application demonstration.

Weaknesses
- Not sufficient technology transfer.
- Given the exciting progress on structure developments, there needs to be a greater urgency to "reduce to practice" by scaling up these structures for use by some fuel cell stack developers.

- More collaborations are needed to show whether the promise of these findings could be translated into the commercial marketplace.
- The effects of contaminants have to be further explored and more external collaborations could lead to such experiments.
- In the past, substitution of Pd for Pt was perhaps perceived as more of an advance than it really would be; this situation appears to have been rectified now.
- Make sure that the reference data is as solid as the data on the innovative systems.
- Avoid drawing conclusions from RDE data at too low potentials where the mass transport correction is large.
- The presentation slides in general contained too much information, and were very difficult to read – use one plot on a slide instead of three.
- Basic material considerations were not adequately explained, i.e. Alloying element choices were not rationalized, and the results were not fully reconciled with theory.
- Test data on operating prototype stacks, effects of commercial grade fuels and the impact on fuel cell cost should be included in the approach.

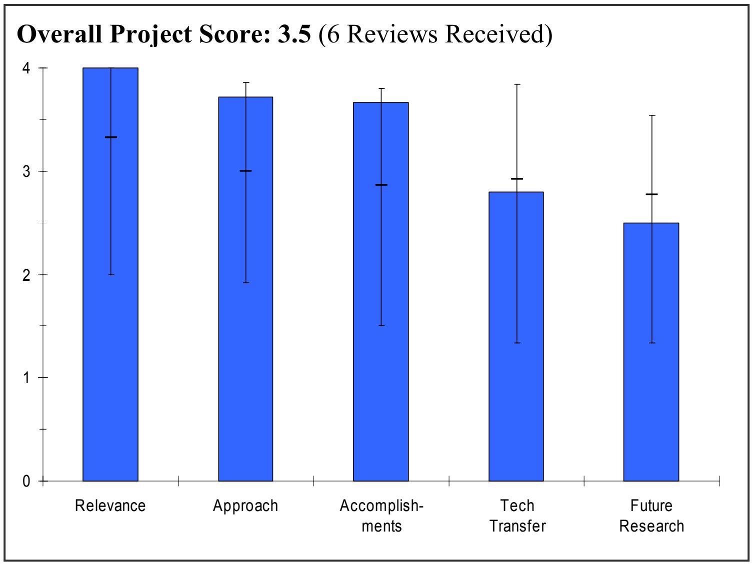
Specific recommendations and additions or deletions to the work scope

- It should include closer integration with others.
- Check on the structural and functional stability of these novel structures in real fuel cell stacks with real-life feeds of hydrogen and air (or even reformate feeds.) The effects of contaminants must be considered.
- Higher temperature operations, including cycling at higher potentials as well, could provide insights into whether such novel structures exhibit structural stability and durability.
- This project has enough useful cathode leads to follow to keep it very productive.
- Progress in transportation fuel cells requires that the best electrocatalyst people concentrate on cathodes; avoid being overly distracted by the scientifically fascinating but technically less critical issues on anodes.
- If a cost assessment for Pd_3Fe with Pt monolayer is compelling, this work should be highest priority.
- The results with gold alloying are very convincing, and these should be reconciled with theoretical calculations.
- The work on DMFC materials is defocusing, and should be re-examined in terms of relevancy, especially in light of FY 2006 budget considerations.
- Expand the operating temperature range for the catalyst. Temperatures ranging from -40 to 120°C will cover the present envisioned PEM range.

Project # FC-10: New Electrocatalysts for Fuel Cells

Phil Ross; LBNL

Brief Summary of Project



This Lawrence Berkeley National Laboratory (LBNL) project is developing new catalysts for both anodes and cathodes 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 is based on computational screening of PGM core-shell nanostructures using newly developed (under BES funding) Monte Carlo simulations. LBNL is pursuing 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.

- There is no doubt that improvements in electrocatalyst activities are critical to cost reduction of catalysts for fuel cell stacks.
- This project directly addresses routes to the improved cathode catalyst activities that are central to attaining DOE's fuel cell cost targets.
- The development of improved (higher activity, longer life, and lower cost) electrocatalysts is imperative for fuel cell commercialization in automotive applications.
- Improving the activity and stability of electrocatalysts is very important, especially to meet automotive targets.
- Fundamental project critical for further improvement of the state of the art in ORR electrocatalysts.
- This type of project is highly relevant to achieving DOE and industries goal towards commercialization of PEM fuel cells.

Question 2: Approach to performing the research and development

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

- This project has done very fundamental research that very astutely focuses in on the central problems in fuel cell electrocatalysts, so it should have a high level of impact in industrial systems.
- An excellent blend of careful experimentation and theory (both within the project and drawn from the literature) has contributed to the success of the project.
- The objectives of the work are very clear, but how they are being realized was not clearly articulated.
- The explanation of analytical methods (Auger, Ion Scattering) was very lucid.
- Details of the annealing process, including photos of test articles and equipment, as well as processing conditions and timescales, were not elaborated.
- The first half of the talk focused on Pt_3Co, but the second half on Pt_3Ni – the difficulty of preparing the latter should be explained.

- There was mention of Monte-Carlo calculations – more explanation of this study, which is critical for reconciling theory and results, is requested.
- An excellent mix of theory and experimental work, which leads to true fundamental understanding. Phil's work on single-crystal model systems has been very useful in furthering understanding.
- Combination of modeling and single crystal studies combined with characterization of supported catalysts.
- The approach generates interesting results. The possibilities are intriguing. However, nothing was indicated on how this work might be commercialized.

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

This project was rated **3.7** based on accomplishments.

- Project is coming to a close, but the closing activities are still very good. A final conclusion slide would have been very helpful for a lengthy research project.
- The alloy single crystal activity and characterization results vividly point out important pathways to higher activities in practical catalysts.
- Confirmation of structural changes at surfaces combined with excellent data were extremely compelling.
- The Pt_3Ni system and its performance offers tremendous promise for achieving DOE's automotive targets for catalytic materials in PEM.
- In addition to surface analyses, a full compositional analysis would be beneficial.
- Only real new result shown was the PtNi results. Nevertheless, these were quite impressive. Understandably, the emphasis for the past year has been in documenting the work that has been done.
- The demonstrated 10-fold improvement of mass activity as function of the type of crystalline phase.
- The progress made appears to be quite significant.

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

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

- The only criticism of this project is that it would have been great if the project had more catalyst industry partners. The real technology transfer is in the research staff joining industry and another National Lab.
- Collaborations with MEA makers, fuel cell developers, and automotive OEMs have been highly effective.
- Catalyst makers have been influenced by this project's publications and presentations but might have benefited from more direct collaborations.
- There was mention of collaborations with GM and 3M, but it seemed to have occurred in the past. What is the outlet for the latest results? What work is needed to validate the technology, scale-up, and realize commercial components?
- Third party assessment of costs would help establish the value of the developments.
- There is some interaction with industry (e.g., GM), but more interaction with catalyst companies vs. end users would be beneficial for an academic program working on electrocatalysts.
- Two industrial "partners" were mentioned, GM and Cabot. No information was supplied to indicate any active collaboration.

Question 5: Approach to and relevance of proposed future research

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

- Plan to get everything written up is good.
- PI's plan to retire will leave a big hole in the fuel cell community.
- It is to be hoped that PI's retirement will include some continued exciting science – now fully on his own terms.
- This project is wrapping up; however the study of the Pt_3Co system, in analogy to the Pt_3Ni system, should be more fully investigated.
- It would be disappointing if research in this area was decreased or side tracked. Some indication of how a future PI might proceed would have been appropriate.

Strengths and weaknesses

Strengths
- The findings are impressive. The activity improvements are very good. The development and research process is quite logical.
- The project is characterized by careful experimentation on very well-controlled systems.
- This work has blended experiment and theory in an unusually effective way.
- The project has created insights that are critical to the development of more active and stable supported electrocatalysts.
- The objective of realizing optimal utilization of precious metals is exceedingly clear, and the latest results confirm the achievability of this goal. This offers high confidence that DOE targets will be met.
- Excellent mix of theoretical and experimental work. Elegant experimental results. Advancement of fundamental understanding.
- Technical expertise and innovative analytical techniques.

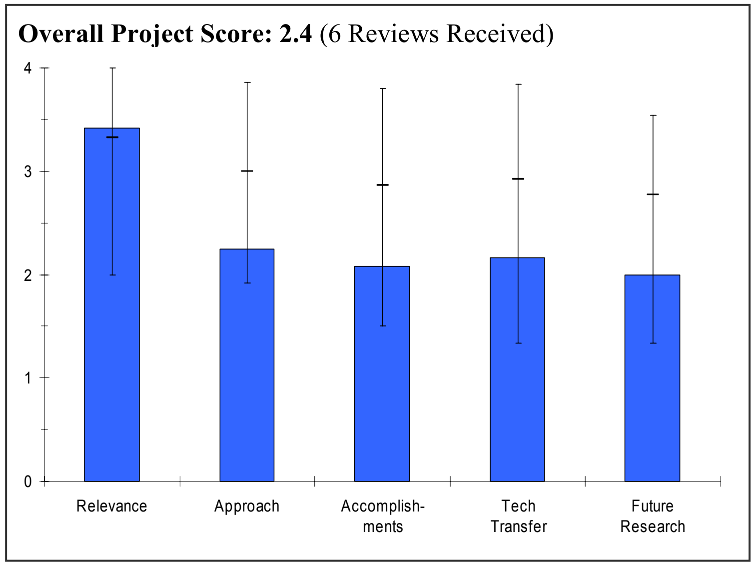
Weaknesses
- It is a little disappointing that this project did not have catalyst manufacturers involvement. GM and 3M are not catalysts suppliers. However, it was good to see one researcher joining Cabot Superior MicroPowders so one will assume some technology transfer will occur.
- The recent work of the project has not carried the single crystal ideas all the way through to high-surface-area supported catalysts.
- There is sound experimental and theoretical work published in technical journals, and very exciting results in terms of reduced costs, but the pathway to a commercial outlet is not at all articulated.
- Lack of proposed follow-on research.

Specific recommendations and additions or deletions to the work scope

- I assume that Dr. Ross will be retained as a consultant and this should be very helpful for the technology transfer part.
- Explore ways to produce Pt-alloy particles with control of exposed crystal faces in high-surface-area catalysts.
- Develop characterization tools capable of determining surface structure (nanoparticle habit) and 2nd-layer composition for high-surface-area catalysts.
- Preparation of Pt_3Co in analogy to Pt_3Ni is the logical next step.
- Analysis of comparative costs (including materials and processing steps) would help reinforce relevancy, and inspire next steps toward adoption by component developers.

Project # FC-11: Development of transition metal/ chalcogen based cathode catalysts for PEM fuel cells

Stephen Campbell; Ballard



Brief Summary of Project

Ballard Power Systems is developing a non-precious metal cathode catalyst for PEM fuel cells that will be as active and as durable as current PGM-based catalysts, at a significantly reduced cost. This project develops composition using sputtered thin films, develops dispersed, supported catalysts, and evaluation/demonstration in fuel cells and stacks.

Question 1: Relevance to overall DOE objectives

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

- This project sought an active, stable Pt-free cathode catalyst which could solve the cathode catalyst cost problem.
- Whether DOE should have put so much money into completely Pt-free approaches is questionable, but is probably outside the scope of this review.
- Project is well focused on DOE goal to identify less expensive catalysts for PEM fuel cell. Unclear how targets A, B and C can be achieved
- This project is very relevant to the objectives of the HFCIT program of addressing the key issue of cathode electrocatalysis.
- Alternative catalysts are important.
- PGM reduction is a key need and this project addresses the need.

Question 2: Approach to performing the research and development

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

- This project is neither fundamental enough nor applied enough to have maximum impact – the middle ground has not proven particularly fertile here.
- Testing was done in sulfuric acid rather than in more weakly-adsorbing perchloric acid, which is much more relevant to PEM fuel cells.
- Project approach is suitable for the planned goals. Unclear why switching to ternary system should raise OCP.
- This project appears to be well suited for a design of experiments approach.
- Chalcogenide catalysts studied in this project appear to lack required performance and performance stability. This approach shows little promise for reaching performance targets..
- At this stage of the project, testing should have been also carried out at higher than ambient temperatures.
- This project is unlikely to yield useful results.
- The justification for selecting the materials was not explained.

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

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

- The project has demonstrated good stability, if not particularly impressive catalytic activity, for several new materials.

- Little progress in identifying promising systems.
- The results are not commensurate with the amount of funding spent. More materials systems and some degree of accelerated testing of the electrocatalyst would benefit this project.
- Although a fully unambiguous evaluation of the catalysts' performance is not possible in the absence of current density data, the results presented by the PI indicate poor ORR performance of the catalysts.
- Stability of the catalysts is unsatisfactory.
- Demonstrated increase in the OCP values is a plus.
- All results to date have shown no measure of success with the chalcogenide catalysts. The project objectives appear to be in a constant state of flux. The work is mainly (90% according to the presenter) carried out at UBC/CWRU, and it is entirely unclear what Ballard has contributed.
- This is a difficult challenge. An explanation of stability data on CoS_2 would be beneficial.

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

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

- The work seemed to show a good working relationship between UBC, CWRU and Ballard.
- Will these results be used by anyone?
- This work may have provided useful background for Ballard's proprietary work on precious-metal-enhanced catalysts using materials such as these as supports, though from the outside it's not clear which effort made the real innovations.
- Good collaborations with Universities.
- Roles of the participants was not defined nor mentioned during the presentation. There is no indication of what CWRU and UBC bring to the project.
- The roles of the two partners and that of Ballard's have been unclear. More and stronger partners would help the progress.
- In view of the poor results to date and the unlikely development of a useful chalcogenide catalyst there isn't anything to transfer at present.
- Good collaboration with Universities.

Question 5: Approach to and relevance of proposed future research

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

- Either more fundamental mechanistic work or a shift to the addition of very small amounts of precious metals might be more productive than the planned continuing tweaking with non-precious-component ternary systems.
- Future efforts should include identification of catalyst active sites.
- Given the enormous challenges that the project is facing, the presented research plan does not guarantee sufficient progress in the future.
- No sensible approach other than trying some of this and that are offered. Trial and error without a sound basis of understanding can only lead to continued lack of progress.
- Is the investigation on binary systems exhausted such that it is necessary to introduce a third variable?
- Go/No-Go point is useful. Besides the OCV, other criteria were not identified.

Strengths and weaknesses

Strengths
- The project gave evidence of careful experimentation.
- Development of procedure for particle size distribution from HRTEM images.
- Interesting approach.
- The project takes on much needed work.

Weaknesses

- Slow progress in getting new experimental results.
- Results less than expected for the amount of funding allocated. This project would benefit from accelerated testing tied to better TEM to look at the catalyst stability.
- Path forward is unclear.
- No peroxide formation data.
- Why worry about normalizing the activity of non-PGM materials with respect to active area, especially where the mechanism is not the objective? The ultimate goal is activity greater than that achievable with Pt.

Specific recommendations and additions or deletions to the work scope

- Consider dropping the plans for additional non-precious-component ternary work in favor of adding a small amount of a precious metal and looking for strong metal-support interactions, if this doesn't overlap excessively with proprietary projects.
- Complement RDE measurements with RRDE measurements. Evaluate volumetric activity in fuel cell and compare with DOE target even if OCP is low
- This project is in a dire need of a major breakthrough to justify further funding; it should be terminated otherwise. There should be no stack testing at this stage.
- End the project.
- The characterizations did not appear to explain the data or direct future plans.

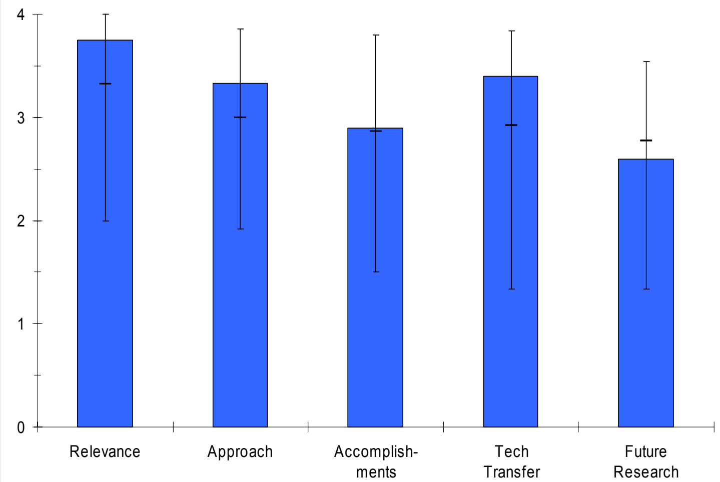
Project # FC-12: Novel Approach to Non-Precious Metal Catalysts

Radoslav Atanasoski; 3M

Brief Summary of Project

This 3M project is developing and demonstrating non-precious metal cathode catalysts to lower cost (goal of 50% less vs. target of 0.2 g Pt/peak kW) and to reduce the dependence of proton exchange membrane fuel cell catalysts on precious metals. 3M is identifying opportunities for system cost reduction through breakthroughs in the catalyst area of fuel cells, utilizing cost-effective, scalable fabrication processes. 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.

Overall Project Score: 3.2 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project is part of DOE's attempt at finding a stable, active Pt-free catalyst that could remove any concerns about catalyst cost.
- The project is relevant to the RD&D plan in the context of discovering new alloys that improve catalyst activity and durability.
- The project is of paramount importance to realization of the DOE 2010 target goals for the design of novel high surface area non-precious nanostructured cathode materials.
- Catalyst cost reduction is one of the key DOE targets.

Question 2: Approach to performing the research and development

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

- This project has followed logical approaches to the development of active non-Pt catalysts and has nicely benchmarked activities against both Pt and state-of-the-art non-Pt systems, though see the box below for a qualifier regarding a Tafel extrapolation.
- The use of 50 cm^2 MEA's in early testing lends an important element of reality to this project which is often lacking in non-Pt catalyst work.
- The low surface areas available from the vacuum approach draw into question its applicability to non-Pt systems, even though significant relative progress (10^3 fold) has been made.
- The approach is rather broad and loosely defined (i.e., "vacuum processes" and "nanotechnology"); consequently it is difficult to gauge the technical value of their approaches. The researchers appear to be well aware of the technical targets and focused on meeting them.
- It is very difficult to judge the technical merits of the approach when the materials are named "A, B, and C". The RRDE measurements should be used to screen the materials prior to fabricating them into a full MEA, rather than vice versa. With the current approach, some materials may be disregarded due to poor activity caused by intricacies in making an MEA rather than being judged on intrinsic catalyst activity.

- The PI used state-of-the-art analytical tools of surface preparation and surface analysis to form well characterized surfaces.
- The PI uniquely combined modeling and UHV studies to illuminate the electronic effects.
- The rotating ring disk electrode method is important addition to a real fuel cell testing.
- The PI must find correlation between physicochemical properties of surface atoms and stability/reactivity in an electrochemical environment under PEMFC operating conditions. To stimulate and complement the experimental studies, the PI must further develop theoretical and/or computational methods, based on reliable quantum chemical means, to perform band-structure calculations.

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

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

- Good progress has been shown in somewhat exceeding state-of-the-art non-Pt activities in full 50 cm^2 MEAs, though no major advancement of the field has yet been made.
- Use of a Tafel slope extrapolation to give a pure kinetic activity at 900 mV seems a bit questionable (if you're already not just kinetically limited at 900 mV, the catalyst has serious other problems).
- Overall, this project was a good effort on a very challenging topic. Progress was made without the ability to obtain significant interaction with an experienced collaborator with whom they had planned to work (through no fault of the project organizers).
- Actual materials not presented, which complicates understanding of the issues and results. Considerable performance loss over the first ten hours with the "nanotechnology" catalysts. Fuel cell derived results are for 58 psig oxygen, which is generous compared to conventional conditions. New higher surface area substrates are intriguing, but it is difficult to appreciate their importance with so few details. Considerable progress towards the activity targets have been accomplished, but durability continues to be a discouraging aspect.
- The interim performance milestones of 0.08 A/cm^2 at 0.6 V and 0.1 A/cm^2 at 0.7 V are too low for a project that has been funded for since 2003 and to meet the 2010 DOE targets.
- The IR correction for Figure 10 is 35 mV at 0.032A/cm^2, corresponding to an area specific resistance of 1.1 ohm cm^2. Normal fuel cell resistance would be <0.2 ohm cm^2. If this could not be lowered, it would prevent attainment of stack efficiency targets.
- Performance on catalysts has improved from the previous report.
- Production of peroxide is too high and may affect both stability of the membrane and catalyst itself.
- These types of catalysts are inherently unstable in acid solution and the project needs a stronger durability component.
- To advance the catalytic/stability performance the nature of active sites must be illuminated in more details.

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

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

- Effective collaborations are in place with National Labs and universities; since the PI is in a company with diverse fuel cell capabilities; many industrial bases are already covered.
- Technology collaborations are strong.
- Technology transfer is not applicable for this project as 3M has the capabilities for commercialization. Good collaboration with universities and Brookhaven.
- The PI has clearly indicated contribution from 3M and from collaborators/partners. 3M is uniquely positioned to transfer the knowledge from fundamental studies to the "real" products.

Question 5: Approach to and relevance of proposed future research

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

- Plans seem basically good, but it is not clear that plans are in place to increase durability of state-of-the-art non-Pt catalysts.

- Future work primarily involves understanding current materials (e.g., characterizing active sites). Paths for improving performance are not described. Durability is a critical issue, but is downplayed in the proposed future research.
- The goal of the future work should not be to achieve the 2005 DOE targets!
- Activity still requiring 2.6x improvement for 2005 target and 7x improvement for the 2010 target suggests that surveying other approaches is needed rather than optimizing the current system.
- The proposed future research is rather general. The PI needs to identify, more precisely, what experimental and theoretical methods he is planning to use in order to resolve stability and activity issues.

Strengths and weaknesses

Strengths
- The project has demonstrated the ability to make 50 cm^2 MEAs out of this challenging class of non-Pt catalysts.
- From its slow start, the project has shown major improvements by reaching the state-of-the-art for this type of non-Pt catalyst.
- The project has strong contributions from the collaborators.
- The collaborative team and characterization of the materials using spectroscopy.
- A comprehensive approach covering all aspects needed to create, characterize, control and understand a new generation of non-precious nanostructured cathode materials for low temperature fuel cells.

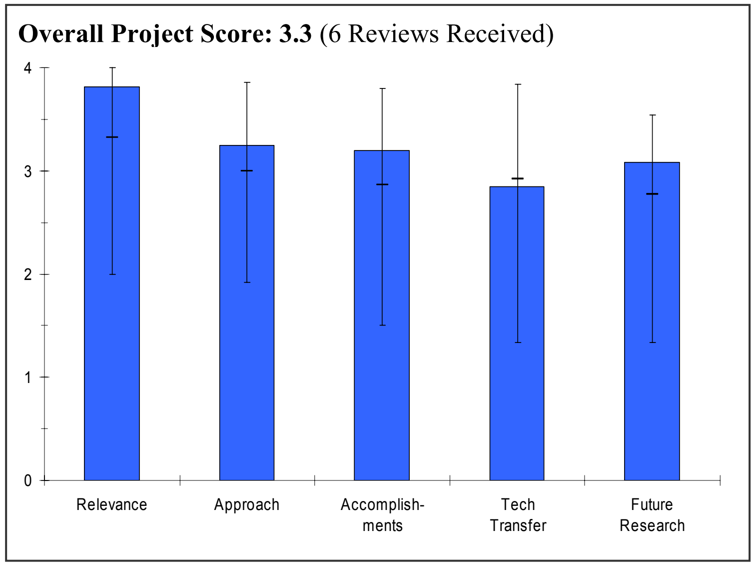
Weaknesses
- The very significant improvements made in the vacuum approach may have been good effort wasted in a direction that can't work with non-Pt systems due to the small surface area available.
- It's not clear that the project has yet advanced significantly beyond the non-Pt state-of-the-art (which was real data, not an extrapolated Tafel line), either in activity or in communicated new insights; it may now be poised to do so.
- Durability is at least as important as activity but receives significantly less coverage.
- Extremely low catalytic activity even after working since 2003 on this class of catalysts.
- These types of materials are very unstable in hostile electrochemical environments and the success of this project relies more on stability of catalysts than on catalytic activity. The amount of peroxide produced during the ORR is too high and must be completely eliminated.

Specific recommendations and additions or deletions to the work scope

- Increased attention to durability issues, even while the activity is still low, could accelerate long-term progress.
- Need basic understanding of what is limiting the performance of this class of catalysts.
- Bring in organometallic chemists to supply ligands which might be added to, or replace some of the nitrogen from the existing C/N matrix to tune electronic, steric factors.

Project # FC-13: Novel Non-Precious Metals for PEMFC: Catalyst Selection Through Molecular Modeling and Durability Studies

Branko N. Popov; University of South Carolina



Brief Summary of Project

The University of South Carolina is synthesizing novel non-precious metal electrocatalysts and metal-free electrocatalysts with similar activity and stability as Pt for the oxygen reduction reaction (ORR). The PIs are focusing on high activity for the ORR, mass production methods, corrosion resistance, low cost, and improved understanding of reaction mechanism of oxygen reduction. The active reaction catalytic sites are optimized with respect to carbon support, presence of surface oxygen groups, nitrogen content, surface modifiers, pyrolysis temperature, porosity, pore size distribution and the concentration of the non-metallic additive "X" in the catalyst matrix. 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 electrocatalysts and the metal-free catalyst as substitutes for Pt currently used in membrane electrode assemblies.

Question 1: Relevance to overall DOE objectives

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

- Development of new catalysts addresses the two key issues of fuel cell cost and durability.
- Through addressing the key issue of cathode electrocatalysis, this project is very relevant to the program objectives.
- The project is a necessary step for moving away from Pt and lowering cost.
- Outstanding.

Question 2: Approach to performing the research and development

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

- The initial approach, based on electrochemical characterization, has been logical and technically sound.
- With only a little more than one year left in the project, the PIs need to down-select and focus on the most promising pathway, and to put additional emphasis on catalyst stability.
- Research focuses on interesting materials, especially metal-free carbons.
- Testing the effect of several different factors on the performance of the Co-based catalysts has revealed interesting properties of the catalysts in the Co group.
- There seems to be little, if any, benefit from the modeling component in this project.
- Given poor "real-time" stability of the catalysts, there is no need for accelerated durability testing at this stage of the project.
- Good systematic approach that includes support in modeling and structure at other universities.
- It could be more efficient to focus on one approach. Metal-free catalysts, if not active enough, have no special attraction in this scenario.

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

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

- Significant progress has been made during the past year. Activity and selectivity has been improved greatly.
- Progress to date does not suggest that the targets will be achieved in the remaining timeframe of the project.
- Initial performance of several materials studied in this project is promising.
- Neither Co/C nor Co-X/C catalysts show sufficient performance durability.
- Effect of surface modifiers needs to be demonstrated using unmodified surfaces as a reference.
- Activating carbon by nitrogen incorporation appears to be a promising endeavor. It is unfortunate that the nature of "X" could not be communicated. The strong reduction of the peroxide generation is impressive.
- Very systematic narrowing of materials and optimization has led to good progress in activity.
- The activity achieved is very impressive. T he durability needs to be addressed.
- The activity of Co-based catalysts has been increased but it is still considerably lower than that of Pt/C. The number of electrons per O_2 should be calculated at higher E from Koutecky-Levich plots. At 0.5 V, a diffusion control is operative and at such over potential it is easy to get a 4-e^- reduction.

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

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

- The modeling and structural studies with Case Western and Northeastern complement the experimental work well.
- More industry involvement is recommended to develop synthesis processes amenable to large-scale manufacturing.
- Benefit from the modeling done at CWRU is unclear as is the role of Northeastern University in this project.
- Industrial/National Lab Involvement would help the progress of the project.
- An industrial collaborator is needed to help this project to establish benchmarks and guidance on development.
- Collaboration with other universities seems to be useful and supportive.
- Developments are still in early stages for tech transfer.

Question 5: Approach to and relevance of proposed future research

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

- Proposed approaches to improving the durability of Co-based catalysts are insufficient. In particular, without a better understanding of the causes for the observed fast catalyst performance degradation, it is not obvious at all why supporting catalysts on graphitized carbon should help the stability (applies also to metal-free catalysts).
- Well formulated approach to developing the concepts further.
- Future plans to support durability and MEA optimizations are supported by the data.
- Not clear why ZrO_2 encapsulation is proposed?
- Should focus on selected systems and fewer variables.

Strengths and weaknesses

Strengths
- The initial approach, based on electrochemical characterization, has been logical and technically sound.
- The project team is highly qualified and experienced in carrying out the electrochemical characterizations.
- Selection of uncommon catalysts, metal-free in particular, represents a strength of the project.
- USC is building a good knowledgebase on Co and metal-free catalyst work.
- Interesting innovative approach.
- Systematic approach and good progress. Very organized presentation.
- Synthetic effort seems good.

Weaknesses
- Durability studies have been rather limited – much more is needed.
- Poor stability of the catalysts and no clear path towards improving it.
- The Xs need to be revealed to allow better technical evaluation of this project in the future.
- Contrary to the PI's claim, 5% peroxide yield is too high to be practical for PEFC systems.
- It is not clear in the presentation regarding how this project is making measurable progress toward the contribution of lowering costs, increasing durability, etc. of the fuel cell stack.
- A better selection of variables for optimizing of catalysts seems necessary.

Specific recommendations and additions or deletions to the work scope

- More industry involvement is recommended to develop synthesis processes amenable to large-scale manufacturing.
- With only a little more than one year left in the project, the approach needs to down-select and focus on the most promising pathway, and to put additional emphasis on catalyst stability.
- Unless the PI can demonstrate a major improvement to the durability of Co-based catalysts, the "Co part" of the project should be deleted from the scope.
- Catalysts need to be tested for possible Pt presence, especially following the so called the "post-treatment". The nature of the "post-treatment" should be revealed.
- Catalysts testing at above-ambient temperatures is needed.
- It would be nice if all the researchers working with Co as a catalyst will collaborate with each other. It appears that they are sometimes repeating each other's work to learn similar lessons. Fewer presentation slides and a few slides on how this research is making measurable contribution toward reducing cost and durability of the stack year to year would be appreciated.
- Expand modeling efforts.
- Recommend continued support.
- More insight into the extent and impact of changes in MEA/cell design would be desirable.

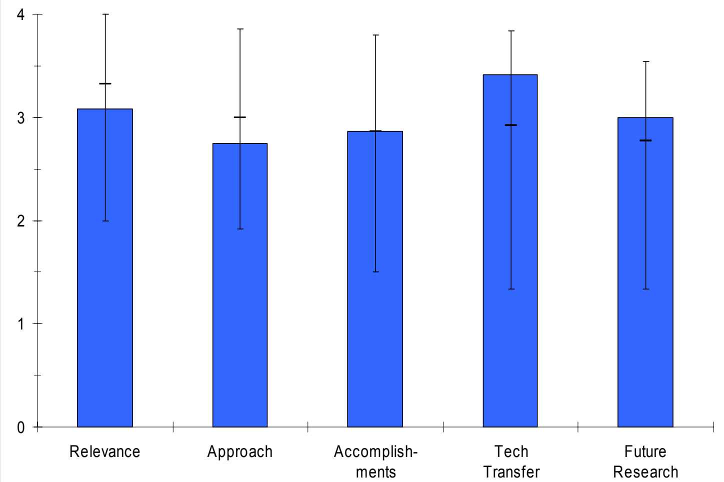
Project # FC-14: Non-Platinum Cathode Catalysts

Piotr Zelenay; LANL

Brief Summary of Project

This Los Alamos National Laboratory project on non-platinum catalysts for polymer electrolyte fuel cells focuses on the fuel cell performance, performance stability and the mechanism of oxygen reduction reaction (ORR) on non-precious metal/heterocyclic polymer composites, such as carbon-supported Co-polypyrrole composite. The project also targets the design, synthesis and fuel cell testing of surface chalcogenides, in particular Se-decorated Ru nanoparticles (Se/Ru), including replacement of ruthenium by non-precious metals in the core of the nanoparticles.

Overall Project Score: 3.0 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- This project went after the Holy Grail of fuel cell research, a durable non-Pt cathode catalyst which could potentially reduce fuel cell costs.
- It is unlikely that the no-Pt, rather than low-Pt, route is more efficient route to acceptable fuel cell catalyst costs, but the decision to pursue this path was DOE's, not that of the organizers of this project.
- The high-loaded (3mg/cm^2, replacing 0.4mg/cm^2 Pt) Ru on chalcogenide catalyst part of this project is inconsistent with the intent, if not the exact wording, of DOE goals for direct H_2 fuel cells, as it would lead to a net increase in cathode catalyst costs once Ru prices were driven up by the development of a significant new use for that metal; it makes some more sense for direct methanol fuel cells.
- Zelenay et al. are making progress on developing non-platinum catalysts through two approaches – Co-N and RuSe complexes. Although RuSe is a non-platinum compound, it still contains a precious metal (Ru) so it is not clear if the project is truly focused on low-cost, available catalysts.
- Co catalysts, if successful, would remove one of the cost constraints.
- The project objectives, if successfully met, would definitely assist in meeting the hydrogen vision.
- With the price of Pt and its availability and the price target of FCs to be competitive with ICEs, Pt replacement is a must.
- Project addresses DOE goal to design low cost non-precious metal catalysts for the oxygen reduction reaction.

Question 2: Approach to performing the research and development

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

- Approach for the metal/conducting polymer/carbon catalyst was generally good, though plotting W/cm^2 is a bit misleading when the maximum value occurs at such a low potential that the cell efficiency would be unacceptable, and were the films really as thin as claimed? For such systems, one always wants to actually measure the electrode layer thickness.
- Approach for the Ru/chalcogenide system (previously the subject of considerable study) still hasn't broken free from its direct-methanol roots – it seems improper to use 10x as much of another platinum group metal to replace 1x Pt for a H_2/air fuel cell.

- Ru/chalcogenide data should be reported against the A/mg precious metal target instead of against the A/cm^3 non-PGM target, as Ru price would rise considerably with any high-volume use.
- The overall approach of the LANL team is thorough, combining physical and electrochemical characterization. The main challenge with their approach is distinguishing themselves from the many other researchers who have tried similar materials. For instance, Zaikovskii et al. published a report this year in J. Phys. Chem. B ((vol. 110, p. 6881) on RuSe nanoparticles, with an approach and results almost identical to that of the LANL team. There are numerous other reports on metal and metal oxide – PPY complexes (see for instance Nguyen Cong, El Abbassi, and Chartier, JECS, 220, v 149, p A525).
- The LANL project never really explains how their program is different from the other publications on related materials. More importantly, all of the related papers are stuck in activity about where LANL is – how exactly do they plan to make up the other 70% of activity needed to reach DOE goals? They have one RuSe material that is Fe-doped, but Fe is not a good idea for fuel cells due to its propensity to catalyze the Fenton reaction.
- The rating of only a 2 does not reflect in anyway the quality of the work being done. It is meant as a reality check on the difficulty of trying to develop a NPM catalyst that is equivalent to Pt in all the ways required for commercially successful components. Some aspects of the research have a very good probability of adding to the general knowledge that will ultimately overcome some of the barriers in time to be effective.
- The approach is good.
- Project approach is very well thought out.

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

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

- The durability of the 10%Co-PPY-XC72 would appear to represent an advance over the typically dismal stability of non-Pt cathode catalysts, but the experiment should have been performed at a practical voltage at or above 0.6V instead of 0.4V – durability can depend quite strongly on potential.
- The claim of a high A/cm^3 value for the Ru/chalcogenide catalyst is inappropriate in spirit if not in exact wording of the targets, as this target was intended for catalysts containing only non-precious components and the quoted achievement was for a high Ru loading.
- The A/cm^3 for the Co-PPY-XC72 is a modestly good achievement in a short time, particularly if the film thickness was accurately estimated as ~10 microns. Was the loading $0.2 mg/cm^2$ of Co, or $0.2 mg/cm^2$ of the complete Co+polymer+carbon catalyst? If the former, the film was probably much thicker than the <10 microns orally quoted in response to a question. The open XC72 structure typically sets the electrode layer thickness, and 0.4mg XC72 typically gives a 12 micron thick layer. $0.2 mg/cm^2$ Co in a 10% Co catalyst would have $2 mg/cm^2$ total catalyst, of which say 1 mg is XC72, which would give a 30 micron layer thickness. If the layer is actually 3 times thicker than assumed in the A/cm^3 calculation, then the quoted A/cm^3 would be three times too high. 4.9/3 = 1.6 which is still near state-of-the-art non-Pt A/cm^3 of 2.8.
- LANL has a mixed bag of accomplishments. They cite materials stability, but they do not do rigorous cycling of their materials, such as by pushing to 1.4 or 1.7 V in the RDE (to replicate the environment that the cathode sees when an actual fuel cell is switched on). The materials are stable within the demonstrated conditions, but it's not clear if they are stable beyond that. Otherwise, they have shown improvement in the materials that they characterized.
- Considering project cost and age, good results have been obtained.
- Again, the low rating does not imply a less than stellar rate of progress towards the original objectives. The LANL group has made outstanding progress and their work continues to be very solid and reliable. But the exact wording of the rankings does not allow giving a higher number since the barriers to replacing Pt are extraordinarily high.
- The project has brought more focus this year. However, the success with the Co should be exploited further while the Ru work should be focused on decreasing the amount of Ru to below $0.01 mg/cm^2$.
- Project shows significant accomplishments concerning nanocomposite stability and durability.
- Good progress in catalyst characterization.

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

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

- The technology is at a far too early stage of development to be transferred, so the lack of technology transfer beyond the partners (which includes and industrial partner for the DMFC work) is not a cause for concern.
- This is a highly integrated program. LANL, in general, is very good about collaborating and sharing data.
- Great team.
- Perhaps the fundamental work on the Ru side has to be left to Universities.
- Good collaboration with universities. PI needs to increase collaboration with industry.

Question 5: Approach to and relevance of proposed future research

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

- Future plans look OK, though DOE might consider retargeting the project to the likely more productive direction of very low PGM-loaded catalysts.
- The LANL team is heading down a fairly well traveled path. It's not clear how they will move ahead where others have failed.
- Ru catalysts are unlikely to exceed the performance of Pt, and with similar limited supply as Pt, are unlikely to surpass Pt in cost/performance.
- Proposed Co work is in less-explored area with greater scope for breakthrough.
- Recommend trying as quickly as possible, to establish fundamental limits of any of their approaches. These entitlement values (whether ORR activities, or mass transport over potential, or peroxide decomposition susceptibility, or toxicity, etc.) should be used to prioritize the likelihood of success of the approaches, and any related technologies the research community may be trying to develop.
- Prioritization is needed. Focus on Co and other transition metals, less on Ru.
- Future work focused on DOE goals.
- DOE targets for non-precious catalysts represent a difficult technical challenge.

Strengths and weaknesses

Strengths
- Some nice materials were developed in the Co/polymer/carbon composites.
- Highly talented, able team.
- The LANL group continues to be a major source of excellent research. They should continue to focus whenever possible more on the fundamentals of the barriers facing catalyst development, with a keen eye to what material systems have the most likelihood of meeting all the 10-12 requirements for a successful electrocatalyst.
- Good novel approach, promising results on the Co side.
- Strong combination of characterization methods for catalyst evaluation.

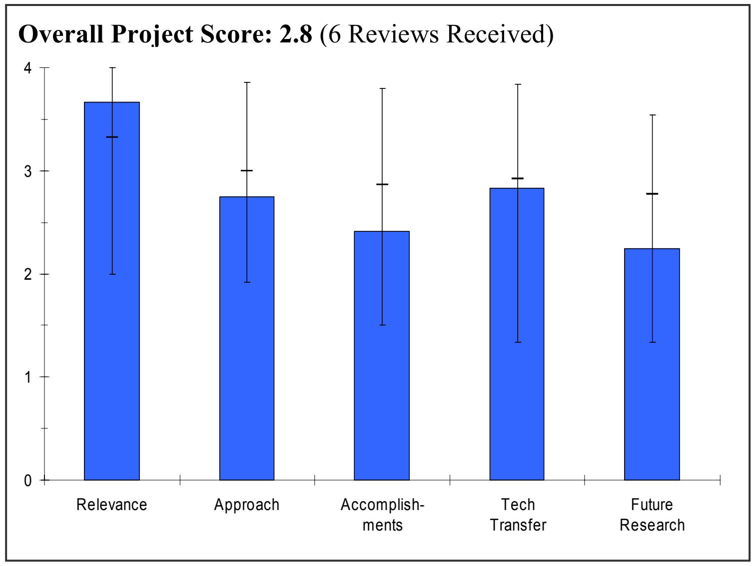
Weaknesses
- It would be good to pay attention to the intended spirit of targets and not just the literal meanings of the words -- we should all be able to recognize the difference between a precious metal-based target and a target intended for systems that are essentially without cost compared to PGM's.
- For non-Pt work in particular (and comparison to the A/cm^3 target), it is very important to measure the actual thickness of catalyst layers.
- No overt weaknesses, but philosophically, if they were to "try to develop catalysts at a National Laboratory with the preconceived notion that they would lead to a commercialized product," that would detract from their strengths.
- Reporting performance below 0.6 V (0.5V for nonprecious metal catalysts), durability included, is useless. Measure the actual thicknesses of the cathode layers to calculate the current densities.
- No collaboration with industry to evaluate catalysts in full-size fuel cells.

Specific recommendations and additions or deletions to the work scope

- Unless Ru loadings for Ru/chalcogenide system can be reduced more than 10x, consider abandoning it for direct H_2 systems.
- High Ru/chalcogenide could make sense as cathode for direct methanol if a membrane with low MeOH crossover could be developed; without this, a low selectivity for methanol oxidation just means unacceptable methanol emissions in the cathode offgas, no?
- There are several inconsistencies in the data. Fuel cell measurements on the Co-N complexes are made at 0.4 V, where they are generating peroxide. Such a fuel cell would also only have an efficiency of about 27% so it is not clear why measurements are being done at that potential.
- RDE/RRDE figures should be more clearly and consistently labeled with power density, temperature, electrolyte, sweep rate and reference electrode.

Project # FC-15: Low-Platinum Catalysts for Oxygen Reduction at PEMFC Cathodes

Karen Swider-Lyons; NRL



Brief Summary of Project

The objective of this Naval Research Laboratory (NRL) project is to attain the DOE goals to achieve 0.2 g Pt/rated kW before 2010 by focusing on lowering the amount of 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 (MSIs) with Pt, and ionic mobility of oxide supports. Objectives for FY 2006 include devising mechanism(s) to explain catalyst activity, and improving synthesis and reproducible materials.

Question 1: Relevance to overall DOE objectives

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

- Project is well aligned to the goal of reduction of PM content.
- This project's objectives cover the critical need to reduce Pt usage in fuel cells.
- Improving the activity and stability of electrocatalysts and lowering Pt loading is very important, especially to meet automotive targets.
- This project supports Pt reduction and therefore cost reduction.
- Very important: Cost reduction. Performance. Durability. All have potential for improvement.

Question 2: Approach to performing the research and development

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

- The proposed idea is novel and has potential, seems that there is some uncertainty in the synthesis methods and in ability to reproduce samples.
- The search for improved cathode catalyst activity through strong metal-support interactions is one of only a few plausible routes to very high mass activities.
- A previous approach of attempting to accomplish significant ionic conduction in the support itself appears to have been properly dropped for the recently-examined materials.
- Specific activity (per Pt area) has now been recognized as a useful parameter in the earliest stages of catalyst development, but it is important not to lose sight of the fact that for precious-metal catalysts the economically-important factor is the mass activity.
- There is not enough focus on trying to obtain a fundamental understanding of mechanisms. There are some proposed mechanisms trying to explain the support interactions with Pt, but it appears to be "waving hands." Admittedly, the ORR mechanism is very complex and difficult to study, which is why one should focus on trying to explain activity improvements based on what is known about ORR catalysts. For example, is there a change in the electronic structure of the Pt or spacing when Pt is put on these supports? A good model in this area is Dr. Ross' work, where good hypotheses for increases in activity are offered.
- If MSIs can be leveraged to reduce Pt content, that is worthwhile. Acidic phosphates and basic oxides have been selected. Trying to understand the mechanism is a good strategy.
- Generally good although details seem a bit disorganized. Little cost discussion. Reasons for strong interest in gold were not clear.
- Approach is sound, but it would be rounded out if more fuel cell tests were carried out.

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

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

- Progress was made in achieving improved mass activity, not clear how this can translate into improved materials for application in MEAs.
- Progress has been slow but real, with modest gains of specific activity reproducibly demonstrated at useful potentials.
- The very large metal support interaction gain demonstrated for gold in acid is of tremendous academic interest, but the enhanced activity is still at too low of a potential to be of any practical utility.
- Good quantity of results considering the level of funding. The results are interesting, but nothing really useful yet, since no practical new catalysts have been discovered nor has a clear improvement in mechanistic understanding been provided. Nevertheless, this is an interesting approach and now that many initial "hiccups" have been overcome, there is reason to be optimistic that future results will be even better.
- Technical progress has been slow perhaps as a result of non-reproducibility. This has been (hopefully) resolved.
- Mechanisms have been proposed. No data is presented (proposed future work).
- Overall not very impressive results. Hard to sort out "old" work from current work. Not clear that much has been accomplished in current work.
- The results this year were weak. The data on the Pt-TaPO would lead one to believe that it is a poor electrical conductor. This coupled with the low proton conductivity would indicate TaPO should be set aside for a second generation system.

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

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

- Collaboration with GM beneficial, started interaction with Ballard.
- Samples are slowly being made available to industry for testing, and meaningful dialog with at least some industrial scientists has been achieved.
- Interactions with GM have been useful and adding another "end-user" (Ballard) is good. However, some interaction with catalyst companies and/or MEA companies vs. end users would be beneficial for a lab program working on electrocatalysts.
- Good collaboration with GM and National Labs.
- Little apparent interaction with collaboration. ETEK role not really explained. University of Hawaii and Ballard roles not known.

Question 5: Approach to and relevance of proposed future research

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

- Plan is adequate as long as it is systematically executed.
- Stated plans did not seem to propose much in the way of new types of materials to be explored.
- Plans need to emphasize the continued search for stable, high mass activities.
- It is not clear how the low surface area issue with the TaPO will be resolved. The one pathway that was tried was not effective and it is not really clear how the barriers to this approach, to date, will be overcome. Once a good performing catalyst has been made, will stability (e.g., potential cycles) be established? How will durability of these new materials be addressed?
- See recommendations below about communicating the objectives on the work with Au.
- Mechanistic studies are proposed. What tests will be conducted to confirm/refute the mechanisms?
- Generally reasonable but not very well defined. No details on possible future technology transfer.

Strengths and weaknesses

Strengths
- New interesting ideas.
- Growing patience to keep at the experiments until they are reproducible and right.
- Unique approach which has promise and is not currently being pursued by others. Focus on materials that are thermodynamically stable in the environment of interest is great. Good results to date, especially for the amount of funding provided.
- Using the support to increase activity is an established approach in heterogeneous catalysis.
- Some what unique approach. Could add to knowledge base if done well.

Weaknesses
- Execution and identifying what are the key process/structure parameters that matter for the performance.
- Mechanistic discussions still seem to be largely speculation rather than driven by a convincing body of data.
- Not understanding of activity effects, to date. For example, why does Ta:P ratio of 1:1 have the best activity? Is that even being investigated? Is there a hypothesis?
- Not clear how MSI has been or will be confirmed.
- Doesn't seem well organized. Little of the work or proposed work is shown to support performance increase. No discussion on costs issues associated with TaPO.
- This project needs a new focus. The continued focus on TaPO does not appear to be going anywhere. While the phosphates are an intriguing idea, there must be some other system(s) that is more promising that a Ta based one.

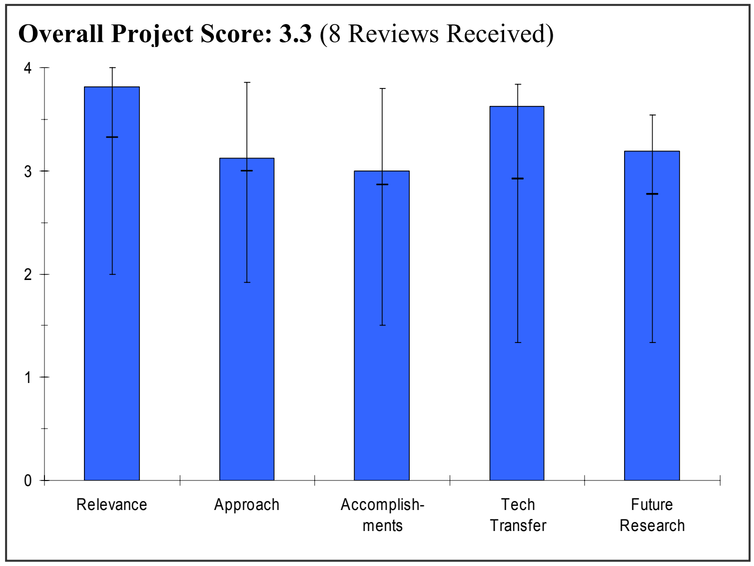
Specific recommendations and additions or deletions to the work scope

- Be careful not to let gold become too much of a time sink as long as all of its activity is below 600 mV (present activity should perhaps not be described as "works like a champ").
- It is not at all clear why Au catalysts are being studied here. I thought the reason was that it was a means to determine if there were support-catalyst interactions, i.e., it is a good "model system" since it has minimal activity in acid electrolytes. However, the summary statements on slide 19 are made about the abundance and stability of Au (relative to Pt), which implies that it is being investigated as a real commercial system. This should be made clear, is Au just a "model" or a real option? Presumably, it is the former. If, in the future, very large activity increases with Au are obtained, then it is OK to switch to stating that it is a real possibility.
- Why is "P" not circled on the table shown in slide 17 Isn't this one of the leading materials in this project?
- Try to organize better. Better explain emphasis on gold. Better relate results and planned work to cost, durability, and performance objectives.
- Redirect the entire project to a new materials system.

Project # FC-16: Development of High-Performance, Low-Pt Cathodes Containing New Catalysts and Layer Structures

Paolina Atanassova; Superior MicroPowders

Brief Summary of Project



This is a four year project led by Cabot Superior MicroPowders to develop and apply a 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. Specific objectives include completing the development of rapid testing equipment (DuPont Fuel Cells); completing high throughput synthesis of ternary alloy compositions in a discovery mode; scaling up the best performing compositions, 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 **3.8** for its relevance to DOE objectives.

- Reducing the Pt loading without performance loss is in line with DOE objectives.
- Extremely relevant to reducing the cost of PEMFCs.
- Low Pt loading is key to overcome price barriers.
- The project is focused on reducing PGM content of cathode without loss of performance or durability. Cost reduction and durability improvement are both critical to achieving DOE targets.
- Very relevant to cost, performance, durability issues.

Question 2: Approach to performing the research and development

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

- The combinatorial method being used has a major advantage over others in that the materials being screened are produced by the same process that would be used in large scale production of "real" catalysts.
- Considerable experience and capabilities in synthesis of new materials. Oriented towards scale-up and high production right from the start. Selection of compositions is logical but somewhat uninspired. A very large parameter set, but early narrowing of the selections may overlook unexpectedly beneficial compositions.
- High-throughput synthesis and testing is a very beneficial approach for rapid progress.
- Spray pyrolysis allows a variety of catalyst compositions, a rapid synthesis method and is easily scalable.
- The advantages of this unique preparation method over conventional catalyst preparation routes need to be pointed out.
- A lot of the work has been focused on the composition of the alloys. The effects of process parameters do not seem to be sufficiently addressed. How much room for improvement does the optimization of the process parameters offer?

- Structural effects are not discussed, e.g.. to what extent and with what effect does metal segregation occur during the heat treatment step?
- Rapid catalyst screening method for small scale fuel cells is a very valuable effort.
- Combinatorial screening has allowed rapid evaluation of many catalyst alloy combinations and identification of several promising systems. Quantitative performance-based decision points were used.
- Approach allows mass screening of many candidates. Durability not really discussed. Scale up issues could be important.
- Developing another method for producing catalysts (spray pyrolysis) seems a good strategy. Long-term testing will show if the combinatorial route for catalyst development is good.

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

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

- 21 ternary combinations were screened with some sweet spots found. These were selected for further testing and it seems that at least one produced a halving of Pt loading without any performance loss. The composition was not revealed. However, reports from elsewhere, e.g. GM, have already clearly established (in stacks) a halving of Pt loading using Pt-Co alloy catalyst. Unfortunately, the ternary compositions did not include any of the valve metals, Ti, Zr, Nd, Hf, W or Re, which would have made the results more interesting.
- Showing ongoing performance improvements. Achieving stable 2-3 nm particle sizes is a positive contribution, but very little durability data was provided on these compositions. Apparently other compositions (slide 14?) are sustaining kinetic activity, but may not be the same materials.
- Excellent progress in reducing PGM loading per kW.
- Huge amount of samples have been investigated.
- Performance of spray pyrolysis does not appear to be superior to current state of the art. If this is merely an alternative to conventional preparation routes, it is not necessarily an improvement.
- Durability improvement appears to be related to better availability on the carbon supports and does not seem to be related to the method of synthesis used in this project.
- Performance and durability of "discovered" alloys match those of conventional platinum at half the loading.
- Performance improvements are very good but more durability work is needed. Scale up results are very limited.
- The activity of the catalysts has been enhanced, and the initial stability test looks promising..

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

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

- It appears the Cabot and DuPont collaboration worked extremely well.
- Collaborations are quite strong.
- Excellent collaboration with DuPont.
- A good collaboration with both vertical integration and diversity. This is a good thing to see.
- Good collaboration with industry, component manufacturer and OEM involved.
- Hydrogenics (stack testing) seems to have been rather passive. Stack tests could be used for rapid screening. (different types of catalyst tested in one stack) to broaden the statistical basis.
- Collaborations include a university for TEM analysis, a stack/system fabricator, and an automotive OEM.
- Potentially very good. Partners could be more involved.
- Outstanding.

Question 5: Approach to and relevance of proposed future research

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

- Until they reveal the composition, it is not clear they have actually found anything new here. Factor of two gains from alloys are essentially already "on the books". Perhaps there will be a gain in process cost and hence the catalyst made by the spray pyrolysis method will be cheaper than from other methods.

- Last year of project. Corrosion resistant carbon results, shown on the future research slide, are very promising.
- Project under current contract is ending very soon. The proposed future work for the remaining few months is adequate to fulfill the goals of the project.
- Given the remaining duration, plan seems ok.
- It would be interesting to compare the cost of spray pyrolysis to the wet chemical route.
- Remaining effort provides a logical conclusion to the project.
- Generally good. More emphasis on scale up and cost issues for best candidates would help.
- Long-term stability tests critical and should be completed soon to assess the usefulness of the synthetic approach.

Strengths and weaknesses

Strengths
- Expertise in spray pyrolysis process to mass-produce high surface area materials.
- Project managed to screen a very large array of potential compositions with a high confidence level. Demonstrated catalyst production at larger scale rates.
- The strengths of this project and approach are the combinatorial approach and the scalability of the catalyst fabrication process.
- The high throughput synthesis process is a good development. It is good to see a common catalyst test protocol.
- Rapid, variable and scalable synthesis method.
- Rapid pre-screening.
- Excellent partnership.
- Ability to experimentally screen many candidates in a controlled and meaningful manner.

Weaknesses
- Not clear what the fraction of catalyst cost is due to synthesis chemistry and therefore what cost reduction can result from the process if there the catalyst is not truly new.
- While the screening is of value to establish baseline data sets, for the results shown, no particularly interesting or effective compositions were found.
- It is not clear in the presentation how this project contributed to fuel cell stack cost reduction and durability goals. Need further information if they used a high constant flow to generate a polarization curve.
- Performance over-emphasized, sometimes at the expense of durability.
- Too much focus on composition, effect of structure neglected.
- Focus on GDE only. This may limit the catalyst utilization. PM utilization may be better in catalyst coated membranes.
- So far too little emphasis on pursing durability issues.

Specific recommendations and additions or deletions to the work scope

- The source of the improved stability of the Pt alloy catalyst specific surface as compared to Pt should be clarified. Is the improved stability of Pt alloy surface area due to lower starting specific area of the alloy catalyst or an intrinsic property of the alloy vs. pure Pt?
- For a project that is coming to an end, a closing presentation maybe more useful to the audience. Tell us what contribution the work made in terms of cost, durability, etc. PI should report how close they came to fulfilling the original goals you promised to the DOE. Tell us what did you discover in your project that we should consider for other projects.
- Need to investigate the effect of structure on catalyst performance.
- How much improvement in performance and durability can be created by varying process parameters? Relate that data to other commercial catalysts with comparable composition/structure.
- Leaching tests ought to be included. How stable are the alloys in presence of liquid water/acid environment?
- Include at least preliminary cost discussions for promising candidates. Include durability as a higher priority issue. Push the short stack testing.

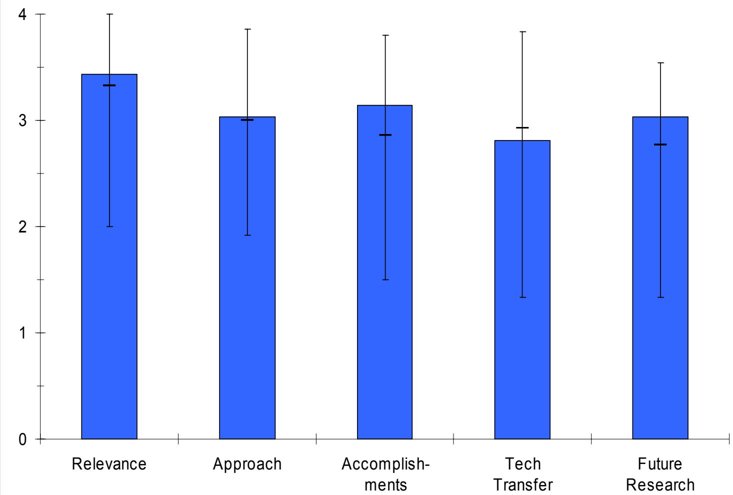
Project # FC-17: Electrode Stability

Xiaoping Wang; ANL

Brief Summary of Project

The objective of this Argonne National Laboratory project is to elucidate rates and mechanisms of loss of electrochemically active surface area (EASA) of polymer electrolyte fuel cell (PEFC) platinum electrodes. We are systematically investigating the dissolution behavior of Pt electrocatalysts in an aqueous, non-adsorbing electrolyte, mimicking the conditions encountered in the PEFC, as a function of potential, potential cycling, alloying, temperature, and other variables that are of interest in the automotive applications of PEFCs.

Overall Project Score: 3.1 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- How do the test conditions relate to real world FC cycles?
- How does cycling affect I-V curves for real fuel cells?
- Although stability of Pt in fuel cells is important, it is also an area where a lot of knowledge already exists. However, automotive applications present operating conditions that are very different (e.g., highly cyclic) relative to what has been studied in the past. In addition, PEM fuel cells have a different type of electrolyte and use different catalysts (e.g., high wt% Pt on C) than what most previous studies have focused on. Pt loss does not appear to be a major problem, but it does warrant some further studies.
- Improved understanding of Pt solubility as function of operating potential and temperature is important for improved durability.
- Project addresses DOE goals on increased durability and electrode performance of PEM fuel cells.
- Too far from real fuel cell world.
- Electrode stability is the key issue for FC successes.
- Reduction of performance degradation is high priority.

Question 2: Approach to performing the research and development

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

- It is not clear that the investigators are fully versed in the work that has been done to date. For example, are they aware that a model of Pt dissolution, including under cyclic conditions, was recently published (Darling and Meyer in JECS)? Are they aware that Pt is a catalyst for C corrosion (see, e.g., L. Roen, C. Paik, and T. Jarvi, "Electrocatalytic corrosion of carbon support in PEMFC cathodes," Electrochemical and Solid-State Letters, Vol. 7, No. 1, pp. A19-A22, 2004) and therefore will be a significant factor in their work at high potentials with Pt/C?
- Systematic approach based on model structures and combined with studies on Pt/C catalysts.
- Unclear how RRDE technique can be used to measure very small current from Pt dissolution.
- Project approach is suitable to the goals.
- The project has to focus either on the fundamentals of Pt stability or to adopt to a high throughput approach to encompass the FC reality of Pt corrosion. More detailed and in depth knowledge and critical literature review could be beneficial.

- Very interesting and somewhat unique approach.
- Not clear that same important variables (e.g. humidity, temperature, cycling rates, etc.) are being included.

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

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

- What is the reason for the difference between potentiostatic and potential cycling dissolution rates?
- OK, for just starting out. But, it is not clear that the team has done a thorough literature review.
- Good progress in identification of conditions that lead to platinum dissolution and particle growth.
- The project is new, however a better focus will help.
- Project has impressive results for the time since started.
- Good evidence that cycling through large potential is major factor in degradation.
- Pace seems somewhat slow relative to FY06 milestones. In particular, if the test apparatus for elevated temperatures is just now being constructed, achieving the 09/06 milestone of temperature effect on dissolution of Pt and Pt-Co (on carbon support) seems in doubt. Plots of dissolved Pt vs. Potential don't have error bars. At >1.2 V, the PIs conclude that dissolved Pt decrease for polycrystalline Pt, but continues to increase for Pt/C. In the absence of error bars, perhaps the differences are not all that great.

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

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

- No interactions with FC manufacturers.
- Interactions with other institutions were mentioned but not explained.
- Could be improved by collaborating directly with researchers who have already done some studies of electrode stability on cells and/or stacks. For example, is Mahlon Wilson from LANL actively involved in this project or is there just an "interaction" by using LANL data?
- The project has to expand collaborative effort.
- Good collaboration with National Labs.
- It needs real life test conditions (potential range, Nafion, O_2 vs. N_2, etc.) from stack manufacturers.
- Very little so far but possibly not much opportunity.

Question 5: Approach to and relevance of proposed future research

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

- Why higher upper limit? Do commercial fuel cells operate at 1.5 V?
- A major goal of this project should be to generate a standardized set of stability tests that can be used to assess the different alternative catalysts that DOE is funding. Ideally, these tests could be conducted on new catalysts (e.g., Pt alloys) on standard carbon supports, as well as for new supported-catalyst systems. This means that the stability tests should challenge both the catalyst (e.g., accelerate dissolution with potential cycles) as well as the oxidation of the supports (e.g., potential holds at high potentials). This would be a very welcome addition to guide and assess all of DOE's advanced catalysts projects.
- Important addition in the future work plan is the study on alloy catalysts. It may be beneficial to expand on several compositions and catalysts and to be able to elucidate effect of composition and preparation method on the alloy stability and solubility of elements incorporated.
- Project should switch from analytical stage to understanding mechanisms of Pt dissolution and agglomeration. Since Pt agglomeration rate may depend on support, it would be better to use carbon support for Pt and Pt_3Co from the same manufacturer.
- The plan does not seem to include looking for other important factors such as rate of cycling and humidity.
- Planned future work seems to give only lip service to modeling effort to extrapolate results to real systems (FY05 reviewer comment). Does ANL really plan to do this? Need to address how to make the work more representative of real systems.

Strengths and weaknesses

Strengths
- Good electrochemistry.
- Bringing access to good analytical tools to bear on a relatively important issue.
- Systematic investigation of the influence of different factors on rate of Pt dissolution.
- This work is needed.
- Impressive implementation of difficult and sophisticated laboratory work.
- Excellent team of investigators.

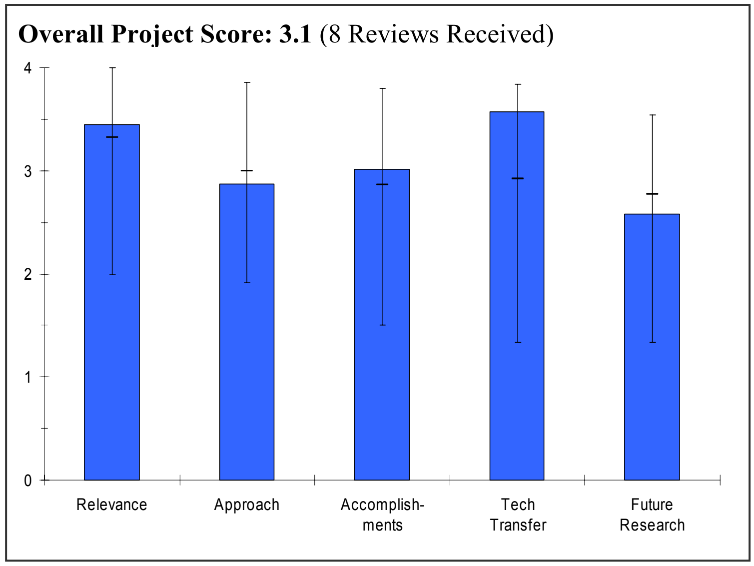
Weaknesses
- Too far from realistic conditions.
- It is not clear how much and what coordination with other National Labs is needed to prevent duplication.
- Is this project adequately funded for the work plan? $350 K seems like a small effort, amounting to a little over 1 person-year with not much left over for materials. Should try to leverage additional DOE/BES funding.

Specific recommendations and additions or deletions to the work scope

- Coordinate with FC manufacturers and catalyst developers to focus research better.
- Include studies with different wt% of Pt on C (e.g., 50 wt %), since this is the type of catalyst being used in PEM cells. Additionally, characterize the changes in the carbon support that occur, as well as measuring the loss of Pt. There will be changes in C surface area, oxidation state, and quantity (i.e., loss to CO_2).
- Include studying stability of cathode catalysts while cycling the anode between H_2 and O_2. This is the real start/stop condition. If done, the results will be very interesting (e.g., see, C. Reiser, L. Bregoli, T. Patterson, J. Yi, J. Yang, M. Perry, and T. Jarvi, "A reverse-current decay mechanism for fuel cells," Electrochemical and Solid-State Letters, Vol. 8, No. 6, pp. A273-A276, 2005).
- It would be interesting to complement platinum dissolution rate and HRTEM measurements with cyclic voltammetry to establish relationship between microscopic and macroscopic results for EASA.
- Similar work may be going elsewhere in other labs. Stability is a serious issue and establishing one large project is worth considering. Include industry in the coordination effort, if not in a leadership position.
- Consider potential gradients (rate of cycling) and humidity as possible significant variables.

Project # FC-18: Integrated Manufacturing for Advanced MEAs

Yu-Min Tsou; E-TEK



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 0.4 A/cm^2 at 0.8 V or 0.1 A/cm^2 at 0.85 V operating on hydrogen/air with precious metal loadings of 0.3mg/cm^2 that are amenable to mass manufacturing technology. Advances from this work as well as from development of a membrane which operates at 120ºC and 25% relative humidity 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 **3.5** for its relevance to DOE objectives.

- "Integrated manufacturing" implies a cost reduction but cost isn't discussed.
- Most of the project is on track to provide useful contributions. The high-temperature membrane's usefulness is less clear.
- As the technology matures, low-cost high volume manufacturability will become more and more important.
- Reduced catalyst loadings are critical to meeting fuel cell cost targets. High-temperature membranes support the R&D plan objectives.
- The project clearly addresses the key technical barriers outlined in the multi-year R&D plan.
- Low cost manufacturing processes will be key to making fuel cells commercial.

Question 2: Approach to performing the research and development

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

- The only "integrated manufacturing I can find in this work is the ion beam assisted deposition (IBAD) of catalyst onto the gas diffusion layer. The resulting performance of the MEAs is so-so, but there is no cost analysis. Is IBAD really a low-cost manufacturing process? Where is the data? Surface area is relatively low, similar to that in 3M "whiskers."
- Developed new catalysts and gas diffusion layers (GDLs), but little detail on approach. The GDL material is a cloth. Stack manufacturers may have difficulty accepting cloth.
- Three distinct tasks with separate description of the approach for each. High volume low-pt loading is important.
- The approach is well thought out and focused on the key issues. The activities are well integrated among the partners.
- Three important areas have been addressed (catalyst, membrane, MEA manufacturing).
- Approach for GDL improvement remains unclear.
- The IBAD approach seems creative and promising but cost and manufacturing issues must be included at an early stage.
- Down-selection criteria would have brought the membrane R&D work into better focus. Too many activities in parallel.

- Some fundamental hurdles should have been considered in an earlier stage (low loadings).
- Well integrated program.
- Fuel cell evaluation only used single cell and short stacks – full scale testing is desired. But, perhaps this was beyond the scope of the project.

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

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

- Some progress on making MEAs using the IBAD deposition but the results do not appear to reduce cost in any significant way.
- The results with ion beam deposition are surprisingly good for a non-impregnated structure. This may have significant advantages for some systems and for manufacturability, particularly if good cathode performances can be achieved. On the other hand, the results with the high temperature membrane are not promising.
- Have shown good performance with fine gradient GDLs. Have shown progress with IBAD electrodes. Also conducted stack tests (Nuvera).
- The project has made significant technical progress during the last year. The IBAD process has shown good results.
- A robust gas diffusion layer working at a different set of conditions (dry, wet, hot, cold, high load, low load) is crucial. The durability test at constant current density at modest temperature and full humidification is not very meaningful in this regard.
- The quality of the IBAD coating should have been analyzed more thoroughly. Does this process yield continuous layers even at very low Pt-loadings at the microscale? Discontinuities are likely to impair the durability.
- Fuel cell evaluation only used single stack and short stacks – full scale testing is desired.
- The performance of the catalysts and the membrane has been increased. The long-term tests have to be extended. That will provide information on the stability of the multi-component catalyst. Tests with potential cycling needs to be performed.

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

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

- Excellent involvement of collaborators.
- They have universities and commercial organizations as team members.
- Very good collaboration among the university and industry partners.
- A very competent and experienced team with a broad and diverse background.
- Balanced participation of academia and industry.
- Transfer of research results to industry ensured.
- Good collaboration with partners.

Question 5: Approach to and relevance of proposed future research

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

- Future work has to address the cost barrier in a more convincing and quantitative way. The membrane work should be discontinued.
- Future work is solid and builds on previous efforts but not particularly ambitious. The future direction of the high temperature work is not clear as the membranes are not providing the requisite low RH performance.
- Planning to lower Pt loadings but will need to address sealing and gasketing issues.
- Project is scheduled to end in August 2006.
- Planned future work builds on successes to date and validation of improvements in stack tests.
- Integrate as many of the individual components as possible into one MEA technology and see if results indicate an overall improvement.
- It appears adequate. It is difficult to say more without compositions of catalysts, membrane.

Strengths and weaknesses

Strengths
- Potential major suppliers of both components and stacks involved.
- IBAD and cloth GDL work are potentially useful contributions.
- Good collaborations among industrial and academic organizations. Broad experience and expertise base.
- Strong collaborative team, with good balance of university, small business, and industry participation.
- Overall project has been well coordinated and implemented.
- Good partnership with complementary skills.
- Good collaborations.
- Good technical progress.
- Appreciation for differences between hand-made and machine-made components. Reliability is inherently tied to manufacture and scale-up.

Weaknesses
- The value of the high temperature membrane effort is not apparent.
- Durability is still a concern, particularly under cycling conditions.
- The cost of the IBAD process was not discussed.
- Catalyst stability/durability needs to be better established.
- Big project, too many areas.
- Catalyst durability only superficially addressed.
- Component down-selection and integration into a single MEA technology comes if at all at a late point.
- Need full stack testing of proposed improvements. No mention of cost models, production capabilities, production metrics, large scale manufacturing.

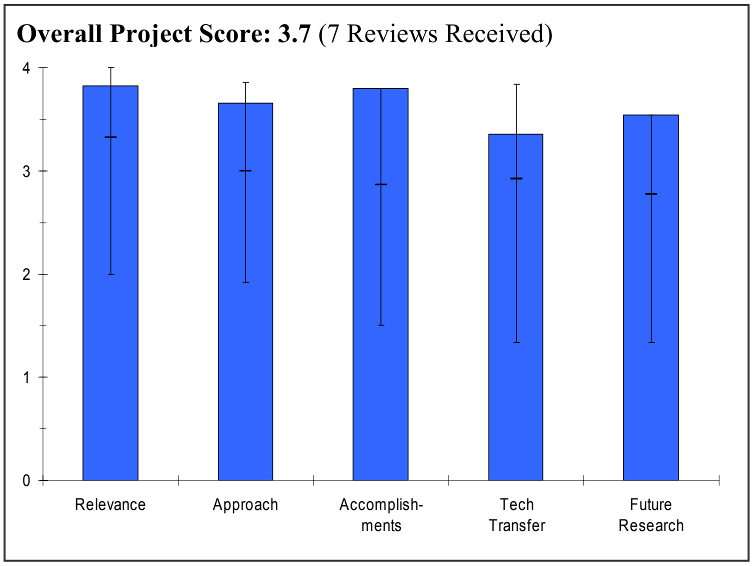
Specific recommendations and additions or deletions to the work scope

- The project should consider focusing on aspects other than the high-temperature membrane effort.
- Apparently, the project is ending in a few months. If this work continues, suggest durability testing under cycling conditions and investigations of degradations/failure mechanisms.
- The large-scale manufacturing process and the cost of the IBAD process needs to be better established.
- Catalyst stability/durability needs to be better validated.
- For a project that is coming to an end, the presentation should include a discussion of progress in terms of lowering cost and improving durability. PI should communicate how close they came to fulfilling the original goals proposed to the DOE. Tell us what did you discover in your program that we should consider for other programs?
- Focus on the IBAD route, which may also be applicable to ccms.
- Not only take into consideration the reduced noble metal content, but also the required power density (ultimately 1 W/cm^2).

Project # FC-19: Advanced MEAs for Enhanced Operating Conditions, Amenable to High Volume Manufacture

Mark Debe; 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 catalysts and 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 ultra-thin layer catalyst electrodes [nanostructured thin film]; optimize perfluorinated sulfonic acid based ionomers modified for enhanced durability at low relative humidity; match MEA components for enhanced performance under demanding conditions; utilize roll-good fabrication processes for lower cost) and to investigate approaches for membrane operation in the range of 120 < T < 150ºC.

Question 1: Relevance to overall DOE objectives

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

- High volume manufacture flexibility is very important.
- Not only is the project relevant to the DOE objectives, this team has clearly focused and targeted on the critical issues and a deliberate path towards meeting those objectives.
- The choice of perfluorinated systems shows their understanding of the long history of such systems and they understand that such chemistries are not, de facto, expensive, as is assumed by others.
- If successful, this project will result in game-changing technology.
- Highly relevant topic.
- Project seeks to solve major problems with catalysts and membranes relevant to DOE transportation applications.
- The project addresses several critical issues: membrane performance and durability, catalyst performance and durability, support durability.
- This project addresses several key issues for the fuel cell technology and as such is very relevant to the program objectives.

Question 2: Approach to performing the research and development

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

- Most of the work is done with roll-goods manufactured materials.
- 3M's approach is methodical and step-wise; more important is their recognition that ultimately, any chemical system explored must be amenable to continuous manufacturing operations.
- Their holistic approach, from polymer choice, through MEA surface innovation (their "tunable" whiskers) for catalyst support, to catalyst exploration (downselecting to ternary systems) is impressive.
- The amount of experimental data generated (including those of long-term testing under simulated load profiles) is laudable.

- The NSTFC approach is very creative and innovative with tremendous potential – excellent.
- The approach outlined in task 2 (HT membrane R&D) is impressive in itself. Care must be taken to link the activities at an appropriate time. Otherwise task 2 may remain a stand-alone development effort.
- Approach has been thorough.
- Project takes integrated approach to the problem.
- The approach has been systematic and focused.
- A few uncertainties that the PI may want to address in the future have to do with NSTFC/3M PEM operation under the condition of high humidity and low air stoichiometric ratios.

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

This project was rated **3.8** based on accomplishments.

- Impressive progress has been cited. Ternary Pt-Co-Mn catalysts show high performance and ability.
- Impressive set of accomplishments (mass activities based on their NSTF catalysts and correlations with the whisker morphologies; downselection to their Pt ternary structures; wealth of the CV cycling data to assess durability; identification of loss mechanisms for catalytic activity; equivalent weight (EW) effects of the 3M ionomer on the polarization curves, etc.)
- The progress shown by the team is solid, including the reproduction of MEA properties on roll goods from systems produced by hand.
- Outstanding results on the catalyst development. All the relevant investigations have been performed. Very meaningful and hence convincing data.
- The advantage of 3M's 730 EW membrane as compared to Nafion® 1000 EW at 120°C, 20% RH is surprisingly small. Only upon reduction of the humidity down to 10% does the difference becomes more pronounced. This deserves closer investigation.
- Sensitivity tests at lower temperature (flooding issues) should be included.
- The technical accomplishments are very high, specifically with respect to high performance at low catalyst loadings, durability, high temperature performance and high temperature durability..
- Progress to date is excellent.
- Short stack testing is good, but full stack testing is really required.
- Performance and durability of the MEA have been significantly improved.
- Impressive cycling stability test results in the last year.
- The source(s) of (i) an increase in the performance during the first ~50 stop/start cycles and (ii) the reversible performance loss should be clarified.

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

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

- Showed a list of collaborators but did not point out these contributions.
- This phase of the project was NOT as collaborative as the earlier phases; hence the low scoring.
- The presentation (unlike those of previous years) appears to be primarily based on 3M internal programs.
- Good partnership.
- The project could benefit even more from the input of the OEMs.
- While this evaluation criteria is difficult to assess from a half hour presentation, it appears that learning from collaborations has been incorporated into research and such collaboration has proved fruitful.
- The only entities missing from the formal list of partners are a stack/system developer and an automotive OEM.
- Very good collaboration with multiple partners; stronger stack developer presence would have been a plus.

Question 5: Approach to and relevance of proposed future research

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

- The project is essentially complete, except for delivery of a short stack to DOE (Argonne) and final report.
- This project may be approaching a "pre-commercial" phase.
- This project has graduated from the R&D phase and the future R&D plans appear to be less "inspired". Given the short remaining duration the plans are absolutely adequate.
- This project is 97% complete so the remaining research is limited. The proposed work beyond the time limits of this project is appropriate.
- 3M is continuing work to commercialization.
- In addition to the research already proposed by the PI, future work should also include: (i) testing of the resistance of the low surface-area NSTF catalysts to surface-active the impurities likely to be present in the cathode feed; (ii) performance recovery after NSTFC flooding; (iii) suitability of the approach to other (non-3M) PEMs.

Strengths and weaknesses

Strengths

- This systematic, comprehensive, project has to be considered a successful demonstration of the novel surface structure-property relationships in the MEA design and manufacturing.
- The catalyst selection demonstrates solid achievement.
- This project has moved the product development into a "pre-commercial" stage.
- Highly innovative approach and outstanding technical accomplishments.
- Relevant testing conditions (performance and durability) have been chosen resulting in meaningful data.
- Well focused, excellent technical progress, well positioned to commercialize technology which has been developed in this project.
- Good approach and execution.

Weaknesses

- Not many; but more stack versus cell data to confirm the progress made would have been instructive.
- More collaborative results with major stack developers would lend credence to the product development claims in this presentation.
- Make sure membrane and catalyst development efforts find their way into a combined MEA technology.
- More testing in full scale stacks.

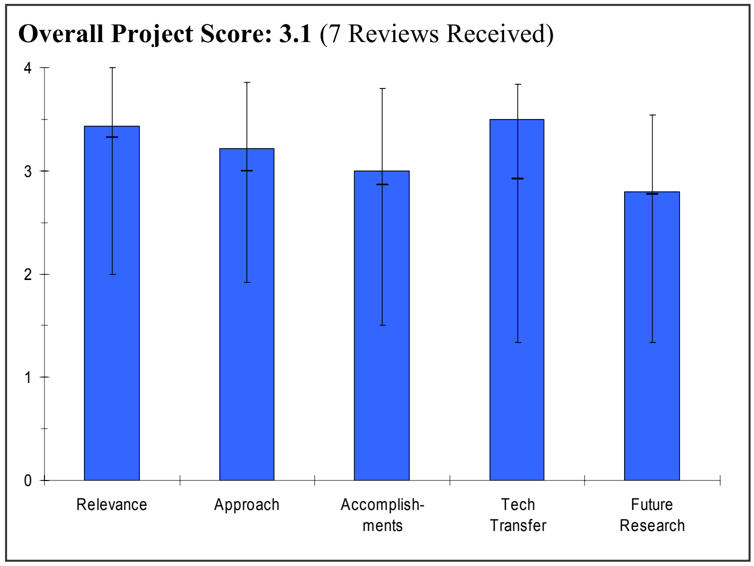
Specific recommendations and additions or deletions to the work scope

- Develop more full-stack data.
- Work with key stack/system developers.
- No information on duration of break-in stages of the NSTFC has been provided.
- This is important especially for automotive OEMs. Concentrate on the scale up of the NSTFC production.

Project # FC-20: Development of High Temperature Membranes and Improved Cathode Catalysts for PEM Fuel Cells

Lesia Protsailo; UTC

Brief Summary of Project



In the area of high temperature operation, United Technologies Corp Fuel Cells is assessing and optimizing fuel cell materials that define and influence performance and durability of PEM fuel cells at operating conditions of 100-120°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 alloy fabrication procedures are being developed; catalysts are fabricated, and electrodes in membrane electrode assemblies are optimized. Performance and durability of these catalysts are evaluated though the use of *ex situ* techniques (RDE, liquid half-cell tests) and *in situ* fuel cell tests.

Question 1: Relevance to overall DOE objectives

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

- It was difficult to fully evaluate this project. For example, one could not determine the power density or specific power of the stack sent to ANL. It would have been helpful if the presentation included a summary of membranes and cell performance vs. the DOE metrics
- The UTC team has focused on critical performance, endurance, and cost issues. The information generated is very useful to the fuel cell program.
- High relevance. Addresses key barriers of durability, cost, water management and operation at elevated temperature and low RH.
- Highly relevant to transportation fuel cell applications.

Question 2: Approach to performing the research and development

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

- The PIs made an effort to develop a downselect methodology for the polymers. Unfortunately, they built a stack too late in the project and did not realize the benefit of their new catalysts. This is a major flaw in the schedule, and UTC should have built the stack earlier to allow time for trouble shooting.
- The PI implemented a very complete and integrated technical plan which is closely coordinated with team members.
- Increased emphasis on durability studies greatly strengthened the project.
- Project focused on high-temperature operation of the MEA. Well thoughtout approach which looked at a high-temperature MEA, not just a high temperature membrane. Integrated experimental and modeling work.
- Broad approach to possible solutions with subsequent downselect to two membrane approaches. Binary and ternary catalysts pursued for stability.
- Combination of new materials and fundamental understanding of high temperature operations.

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

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

- The biggest technical improvement of the program was the extended durability of the PtIrCo catalysts
- Good effort and progress this year. Other developers should be look at the hydrocarbon membrane results.
- Excellent results in the catalyst area, made significant progress in durability and demonstrating catalyst durability at elevated temperatures.
- Determined the relationship between O_2 permeability and membrane durability; and use of the Fenton's test with hydrocarbon membranes.
- Both BPSH and composite membrane show promise, but project did not bring either approach to fully satisfactory state. Stability of PtIrCo catalyst is promising, but understanding is not complete, limiting contribution to catalyst "design." Demonstration of high mass fraction, thinner electrode useful.
- Highly innovative project. Well coordinated among the several elements.

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

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

- UTC worked well with polymer suppliers. The stack work was not as well coordinated/transferred.
- The PI has shown very good coordination with all team members.
- Good collaborative effort with significant exchange of information.
- Collaborations between Virginia Tech and UConn. on membrane development coordinated well and benefited the project. These subprojects were continued beyond the Go/No-Go when they showed some promise, rather than cut when they did not meet the target, which was a good choice since the continued work led to an increased understanding of high-temperature membranes.
- Broad collaborations. Some dropped (appropriately) based on technology downselect decisions.
- Should have collaborated with MEA company.

Question 5: Approach to and relevance of proposed future research

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

- Clearly UTC made some improvements and more work is needed.
- Project is complete and stack was shipped to ANL.
- Program ending. Good closing presentation.

Strengths and weaknesses

Strengths
- Brought some resolution to the wide number of claimed PEM membranes.
- Good team and long-term progress.
- Catalyst durability at elevated temperatures was demonstrated under this project, countering claims by some that high temperature operation would not be achievable due to limitations of catalyst dissolution.
- Broad collaborations led to useful screening of possibilities.
- Well coordinated effort integrating large number of elements.
- Good closing presentation for end of program. Clearly improved their own knowledge base.

Weaknesses

- Stack development of the new catalysts was rushed, and none of the advanced membranes could be used. The project ended up being 2 separate programs for new catalysts and new membranes, but a more integrated program toward a high temperature stack would have made more of a contribution.
- Need to bring TEM analysis into accelerated testing to look for changes to catalyst population and distribution.
- Membrane development did not meet target and a high temperature membrane was not included in the stack; therefore the stack cannot be tested under high temperature, low relative humidity conditions.
- Carrying two membrane approaches forward may have limited chances to push one or the other further.
- More discussion was needed on how this project made measurable contributions towards the DOE cost and durability targets. It is unclear how much knowledge transfer occurred externally.

Specific recommendations and additions or deletions to the work scope

- The PIs should have addressed the contributions they made to this project. It would have been good to see some lessons learned, feedback on test protocols, etc.

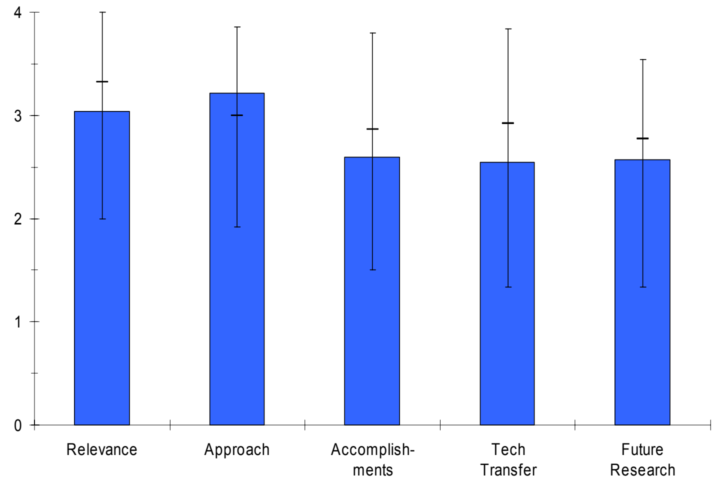
Project # FC-21: Electrocatalyst Supports and Electrode Structures

Mahlon Wilson; LANL

Brief Summary of Project

This project involves the development of alternative supports to carbon that improve the dispersion and stability of the platinum catalyst. Work is focusing on hexaboride supports which are shown to readily provide high Pt dispersions using a simple spontaneous deposition process unique to the hexaborides. Electrochemical characterization of the Pt/hexaboride catalysts demonstrates that activity is maintained even with highly-dispersed, noncrystalline Pt.

Overall Project Score: 2.8 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Need to identify metric for electrocatalyst stability and definition of testing conditions to mimic automotive usage, including start-up/shut down conditions.
- Need to identify the requirement of the supports (expected higher corrosion resistivity than carbon).
- The objectives are clearly consistent with the hydrogen vision, but either separately or together may not be sufficient or enabling. The catalysts have to be compatible and integrate with the other components in the MEA.
- Durability and performance are clearly addressed – provided the novel supports turn out stable.
- Contribution to cost reduction depends on the costs of the alternative support materials.
- Low-Pt durability is directly relevant to transportation goals.
- Minimizing use of platinum group metals (PGMs) (cost reasons) and utilizing them in a manner that will be stable, after long-term fuel-cell use (durability reasons) are valid concerns and worthy project objectives.

Question 2: Approach to performing the research and development

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

- Material selection for Pt stability is good, however, corrosion resistivity (dissolution) of support material should be considered.
- It is very early yet to know if the hexaboride approach will have any utility. It would be valuable to eliminate, as soon as possible, any mechanisms that might cause the release of Ca cations from the high surface area CaB_6 and poison the membrane. If there is, then this approach should be reconsidered.
- The use of interactive supports is an innovative and very promising approach to reducing surface mobility and at the same time increasing Pt crystallite activity. High surface area supports with sufficient stability are a prerequisite, however.
- Analytical model of Pt deposition provides fundamental insight. Exotic materials (europium) and high temperature processes covered.
- The project can be summarized as long term, high risk, and exploratory research in a novel strategy for utilization of PGMs. The researchers appear to be expert and have appropriate analysis tools.
- This is one of those high-risk, high-reward projects that may or may not be successful. But we need this kind of fundamental R&D to identify new materials, especially at the National Labs. Identifying alternatives to carbon supports to improve durability is a key enabler for fuel cells. LaB_6 has a long way to go to reach practicality, but that's OK at this point. Need the preliminary work on many materials to identify the most promising ones.

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

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

- Good to identify morphology of support carbon and electrocatalyst performance.
- Progress has been made in opening up a new area of research for potential supports to replace carbon, but no concrete results yet to indicate that performance, durability or cost barriers can be reduced by this approach.
- Good investigative work on the nature of borides and the deposition of Pt.
- Initial results from key experiments indicating increased Pt activity and reduced surface mobility are missing.
- Very limited fuel cell data. Assertion of Pt monolayer and hexaboride needs to be experimentally demonstrated.
- The PIs are asking good questions and are mindful of the ultimate performance requirements (e.g., Slide 3). It is too soon to tell whether the approach being tested will advance the state- of-the-art.
- The spontaneous deposition with hexaborides is a welcome bonus – provided the materials meet durability targets. Could pave the way to a cheaper manufacturing process.

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

This project was rated **2.5** for technology transfer and collaboration.

- Need to involve catalyst/MEA supplier.
- Might benefit from some collaboration with someone having expertise utilizing such materials in other applications. Not sure where that would be, however.
- Good cooperation betweens the National Labs.
- Input from catalyst, MEA manufacturers, and end users is appreciated.
- Interaction with other National Labs, but limited or no work with industry or academia.
- BNL, ORNL. No commercial or university partners. Good job using National Lab resources.
- Work spans three LANL groups and three other DOE labs. It is premature to collaborate with industry for tech transfer.

Question 5: Approach to and relevance of proposed future research

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

- Need to narrow the objectives. Materials should be down-selected for the next step. Support material corrosion resistivity is still necessary.
- The work appears comprehensive but perhaps should be focused on answering the most critical question first, the durability aspects of the new support materials. Assume the same activity can be obtained with Pt and focus more on just determining if the corrosion resistance of these new supports is any different than carbon – the material being replaced. Also, if there is any difference in the resistance to Pt dissolution from CV cycling up to 1.2 volts at 80°C under H_2/N_2. If the materials cannot perform better than carbon, then there is no reason to try and improve activity.
- Key experiments must be designed and conducted to show whether the alternative boride supports actually do enhance Pt activity and simultaneously reduce surface mobility.
- More focus on stability of supports is needed. CV results were interesting, but as the presenter indicated, these should give one pause. Recommend more emphasis on increasing surface area and less work on fuel cell performance.
- Major work needs to be done on synthesis processes.
- The PIs are on the right track.
- The quantum mechanical modeling is important, so it is welcome that LANL plans to expand it. Need to carefully consider if the BNL catalyst material merits further pursuit. The fact that the hexaboride support may be sacrificial to meet durability may not be a detriment. The question is whether it is better or worse than carbon, which also undergoes the sacrificial corrosion over time.

Strengths and weaknesses

Strengths
- Material engineering based on molecular level modeling.
- Highly innovative approach with great potential.
- Novel approach to develop alternatives to carbon for support materials, interesting properties.
- Elegant modeling and analysis techniques.
- Interesting hypothesis and initial results (e.g., "self healing"); research on track.
- Good long-term, fundamental R&D to develop new classes for materials for catalysts.

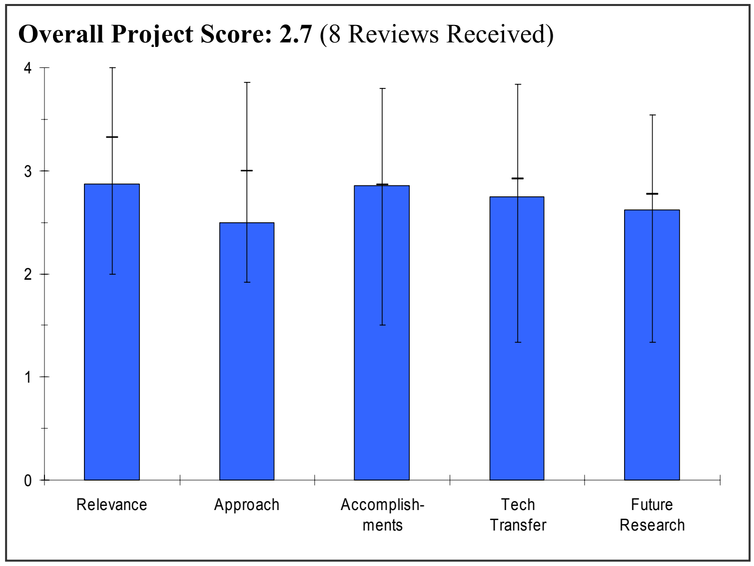
Weaknesses
- Systematic development.
- Very narrow scope on boride supports.
- It must be shown in an early stage that the presented supports hold some of the conjectured advantages. To date this does not become apparent, not even from theoretical calculations.
- Subsequently, many other aspects must be considered for fuel cell operation, e.g. H_2O_2 formation, ionomer interaction.
- Greater interaction with other groups outside of LANL/ORNL/BNL.
- Uses exotic material and processes. This could take us away from low cost. No fuel cell data (1 polarization curve). Very basic science.
- Since cost saving is one of the main motivations of project, it would be helpful to also give a back-of-envelope estimate of what the ultimate cost might be. This is particularly important because competing, novel methods (e.g., 3M's in FC-19) have similar goals (that is, Pt on C is not the most relevant baseline).

Specific recommendations and additions or deletions to the work scope

- Involve industry partners (catalyst/MEA suppliers).
- Focus first on durability of the new supports. Are they any different than carbon – better or worse?
- The interaction of Pt with the support deserves closer investigation. What exactly is the effect of the electron donating support?
- Key experiment to show reduced surface mobility and increased Pt activity (and corresponding Go/No-Go criteria) must be included. Based on the outcome of these experiments other supports than Borides should be considered.
- Important to keep the Fuel Cell Tech Team apprised of progress through yearly updates.

Project # FC-22: Fundamental Science for Performance, Cost and Durability

Bryan Pivovar; LANL



Brief Summary of Project

This Los Alamos National Laboratory project develops the fundamental understanding and technical underpinnings of technologies for improved cost, performance, and durability of fuel cell components. This effort focuses on phenomenological modeling of the membrane-electrode interface, development of hydrogen oxidation reaction models, and investigation of the platinum-ionomer interface to decrease cost, and improve performance and durability.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- Weak relevance. CO poisoning is no longer relevant.
- This work seems to be in three related areas that might be better pursued separately.
- Addressing the cause and solution of MEA delamination would be of greater value if compared with models correlating delamination with performance. Improved understanding of poisoning is important for determining mitigation strategies.
- Title rates a "4", but will need to see that their approaches apply to performance, cost, and/or durability over the next year.
- Hydrogen oxidation is not one of the major fuel cell problems with pure hydrogen as fuel. On the other hand, anode poisoning e.g., by CO, H_2S, etc., and its mitigation should be pursued as a separate topic. In this consideration, HOR investigation should be limited under this project.
- Relevant but not the same impact as many of the other projects.
- All three areas covered by the PI are critical.
- Strong project team.
- Modeling work important.
- Membrane electrode delamination does need to be studied.

Question 2: Approach to performing the research and development

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

- Basket of 3 tasks which are not interrelated. No project synergy.
- Using old fuel cell I-V curves as sources of inquiry may introduce unknown variables that cannot be easily tracked. The old fuel cells may have had other design or assembly problems that could have resulted in the odd curves. It might be better to use more modern fuel cell materials and designs, and careful break-in and leakage testing, along with careful test equipment calibration rather than using old data with uncharacteristic curve shapes as the source of inquiry. If modern materials and careful testing cannot duplicate the old, odd curve shapes, perhaps they should be discarded.
- A multi-faceted approach with some possible payoffs.
- Not clear if the delamination model is accurate or relevant.
- HOR model can be useful if applicable to lower (5-20 mV?) overpotentials due to reduced loading and/or impurities. A better understanding of the electrodes is always welcome.

- Concentrate on the cathode. Will be interested in next year's results.
- Pursue more experimental delamination studies (accelerated) to validate model-based data.
- Design and pursue modeling/experimental studies mimicking real-world catalyst-ionomer interface.
- Identify leading contributors to performance degradation and determine their relative contribution.
- Logical approach.
- Approach is very academic.
- Question whether delamination results truly represent physical delamination.

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

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

- Delamination model is poorly correlated with experimental data. CO poisoning evaluation is not relevant to H_2 cells.
- It is difficult to draw correlations between the phenomena discussed and the contaminants of interest. The modeling approach seems to have merit, but it might be helpful to have more recent parametric studies to compare to predicted results.
- Appears to be good results on the anode modeling.
- Approach to remediation of degradation or durability problems should be developed based on understanding of these problems.
- The results of the microelectrode work are particularly important accomplishments.
- Good quality data generated.

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

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

- No commercial collaboration.
- Good collaboration.
- Collaboration with fuel cell and/or membrane companies would be beneficial.
- LANL has good interaction with other labs and programs.
- Perhaps project would benefit from increased interactions with OEMs to define future work?
- Collaboration with other laboratories and universities is quite extensive.
- Industry collaboration lacking; this was partially responsible for poor relevance of some portion of the work.
- The PI could benefit form closer collaborations with industrial partners.
- Good coordination and assignments of tasks.
- Good list of publications.

Question 5: Approach to and relevance of proposed future research

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

- Limited effort projected for data correlation – this is probably the most important aspect.
- Although delamination was shown by modeling to affect performance, no work was done to understand the causes of the delamination.
- It might be helpful to try to model the cause of the delamination and validate that.
- The stated fact that methanol accelerates delamination might be a clue to the causes of delamination. It might be important to know the causes.
- Will be interesting to see what comes of the Pt-ionomer interface work.
- The future proposed approach is quite logical and fairly coordinated.
- Contingency consideration is a weak point.
- Very important to continue to correlate delimination model with experimental data
- Question whether proposed future work may be accomplished in time remaining on project

Strengths and weaknesses

Strengths
- The presenter appears to be knowledgeable and up to date on fuel cell chemistry.
- The research team and their physical facilities are excellent.
- Technical approaches employed are very appropriate.
- The PI has identified critical areas where research is needed and designed excellent test methods to address these.
- Expanding microelectrode work to peroxide generation is very promising approach.
- One of the best presentations this week.
- Overall well thought-out and managed project.
- Strong team.
- Important technical issues addressed.

Weaknesses
- The simultaneous pursuit of disparate phenomena in one project might tend to weaken the work.
- This reviewer expected more work done for the level of funding afforded.
- Little or no interaction with industry or academia. Although "applied" is in the title, it seems that the projects are not sharply focused on getting information that is relevant to a broad spectrum of investigators. The model for delamination gives expected results and doesn't seem to advance technical understanding significantly.
- The PI could work more with industrial and university partners to use these techniques to address real problems such as studying delamination under different durability protocols or with new experimental membranes, or applying microelectrode work to the development of new electrode formulations with increased durability under hotter, drier operating conditions.
- More experimental vs. model data needed.
- Total project timeline vs. budget unclear.

Specific recommendations and additions or deletions to the work scope

- Perhaps this project might be stopped, with two or three new well-defined projects to take its place. It might also be useful to coordinate it with other work examining how contaminants affect cell performance. This project seems to be using older data.
- Recommend no additional work on modeling MEA delamination. A better use of resources would be to identify the causes and solutions of delamination rather than correlating degree of delamination with performance.
- Degradation of catalytic activity, particularly at the cathode (sintering/surface area loss/flooding) should be included in the future experimental and modeling work.
- Project goals are in three areas. May need to focus more to meet deadlines.
- Include timetable, milestones, and Go/No-Go concepts into future planning thought process.

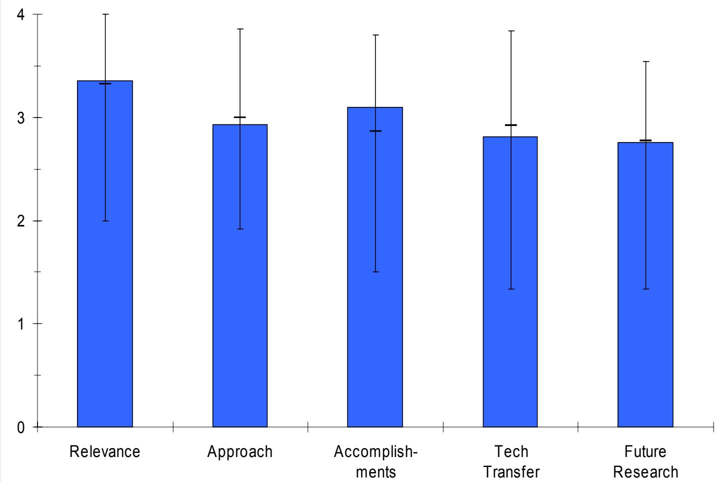
Project # FC-23: Fuel Cell Systems Analysis

Rajesh Ahluwalia; ANL

Brief Summary of Project

For this project, Argonne National Laboratory has developed a fuel cell system model and is using 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 a versatile system design and analysis tool, and to apply the model to issues of current interest.

Overall Project Score: 3.0 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Do not understand how N_2 purge is a significant technical barrier in need of new models. The need to purge N_2 due to crossover seems to be well understood and is not a large contributor to overall system efficiency.
- The system analysis of a fuel cell is an important and essential part of realizing the objectives for the President's Hydrogen Fuel Initiative. The project focus is narrow and appears to consider only one type of fuel cell system. The system analysis evaluates components that have not been fully developed and demonstrated and, as a result, the project's contribution to real world solutions is suspect and could be misleading.
- This project applies modeling to a variety of topics in support of the program. It would appear to be a modeling center of excellence for the program. If this is, in fact, the modeling center of excellence for the DOE Fuel Cell Program, additional resources would be helpful for more model validation, field data comparisons, and component data acquisition. Modeling is difficult in the best circumstances, and a high level of resources is necessary to both accomplish good modeling and to update the modeling techniques used in response to technology movement.
- Very practical effort. Doing good work on sub freezing startup, impurities in feed streams. Well organized research effort. Why is GC Tool being licensed? Making the program free will improve its adoption and use.
- The investigator is clearly responsive to the analysis needs of the hydrogen R&D plan.
- Vital work. Meets the need to have a validated model for fuel cells.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative. Applied correctly, this initiative could be extremely valuable to DOE and FCV integrators.

Question 2: Approach to performing the research and development

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

- System level freeze modeling to determine ability to start-up a fuel cell stack seems to be insufficient compared with experimental stack testing. As other presentations in this review have shown, stack level changes (e.g., GDL and MEA) have shown to markedly improve ability of a stack to start-up from freezing temperatures. These types of effects are not captured in a system level model.
- System level modeling to understand component effects on overall system efficiency could help DOE understand research trade-offs. It is not clear how the other modeling efforts (e.g., freeze, N_2 purge) will significantly facilitate DOE's understanding of research needs.

- The approach of the project is strongly tied to the use of a compressor/expander developed by Honeywell, an enthalpy wheel from Honeywell/Emprise, a membrane humidifier from Honeywell/permapure, and radiator results compared to Honeywell data. The approach appears to be to work with a Honeywell PEM fuel cell concept. On the other hand, Honeywell is a component manufacturer and not a fuel cell integrator, such as Ballard, Plug Power, GM, or UTC Power. It appears the model is a collection of Honeywell components and the project has not worked with fuel cell integrators to ascertain the quality of the system being analyzed.
- Although model validation was not discussed in the presentation, model validation is essential for good modeling. If continuous validation is in fact accomplished, this should be highlighted. If continuous validation is not done, it should be done. Feedback from the FreedomCAR Program should be used to help to validate the modeling results. Additional data from demonstration programs, including the FreedomCAR Program and others, would be helpful to validate the modeling techniques and computer programs used.
- Comparison of model and experimental data to be applauded. Outstanding body of work to understand and overcome the design limitations for fuel cell powered systems.
- Efforts should be taken to validate start-up approaches at the system level. Otherwise the approach is solid and is addressing important technical barriers. Plans for and progress in validation at the stack level appears to be appropriate at this time.
- This project is a rational approach to addressing the President's hydrogen fuel initiative. Information and modeling on boot strap starts, N_2 dilution of the fuel and system sizing are valid and applicable.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

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

- The effect of model results on the overall DOE program was not articulated.
- The project has contributed to the advancement of fuel cell technology. The project did not report the results of other researchers who have done start-up tests at freeze conditions. These researchers demonstrate unassisted rapid start. How does this project rationalize that data with their data? How does the compressor/expander operate during transient conditions? What is the effect on efficiency of cyclic load? These points should be addressed since they are an efficiency limitation of the pressurized system. The project did not identify that the enthalpy wheel is a single point device designed for 60% relative humidity at peak power. What happens at non-peak conditions?
- The results on hydrogen crossover and hydrogen effects would benefit from additional validation from actual stack performance data. Some validation of the predicted optimum purge would also be helpful to both validate the model for this aspect of the work and to see if other aspects of cell stack design should be evaluated.
- Making good progress. This effort will impact future system designs.
- The investigators have made solid incremental progress toward refining system and stack level models.
- Now that self-starting at ≤ -20°C and the role of water has been elucidated, can the analysis explicitly offer solutions to the startup issues? E.g., if the stack is heated to 0°C first, what are the implications and what is the energy impact--do we have a battery?
- The technical accomplishments and progress on this project are very good. The information on bootstrap starts from subfreezing temperatures is interesting. Consideration and recommendations should be given on how a fuel cell system would be applied these conclusions. The effect of diluents entering the fuel processing system is very important from a performance and safety perspective. The system and sizing activities are warranted to benchmark where the industry is heading versus the DOE goals.

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

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

- A greater degree of collaboration with fuel cell companies could illuminate potential model deficiencies (particularly with respect to the efficacy of a system freeze model).
- The project is putting its information into the open literature which is good. The project should work with fuel cell integrators to establish if the concept being proposed is valid. Would fuel cell manufacturers make a fuel cell like the concept proposed here? Are all the components and subsystems consistent with the real fuel cell

world? An affirmative answer would establish the credibility of this work, otherwise the project is not linked or helping the advancement of PEM fuel cells.
- Close coordination with field data is important to good modeling until the methods are validated for a long period of time with stable system design types. Additional resources would be helpful to allow additional coordination with field data provided by the program to allow fine-tuning the modeling approach. These modeling results should be coordinated and compared to LANL work on freezing cells to both validate the modeling and to complement the freezing cell work done by LANL.
- Good teaming with Honeywell, FreedomCAR and TIAX.
- The investigators have worked with a variety of other DOE programs as well as other developers to incorporate current practice into their system models.
- ANL should continue to support TIAX in their fuel cell cost assessment. Likewise the guidance from the fuel cell Tech Team must continue. A lot of effort spent on N_2 as an impurity, but it is not clear how the results have been fed back to the H_2 fuel quality specification work done by SAE and ISO. Is there a disconnect here? Do these ANL results support the maximum N_2 concentrations proposed by SAE and ISO?
- The collaborative efforts are good, but information from public and DOE sources do not appear to be leveraged well. For example, HNEI, JARI and LANL are doing impurities testing of electrodes. As part of the testing, they are periodically measuring impurities in the cell exhaust. If requested, this data could be supplied to supplement the model on nitrogen diffusion. Additional effort needs to be applied in modeling the effects of recycle and possibly cascading. The radiator studies done as part of the system work could leverage other manufacturers' public literature (e.g., Modine, Harrison).

Question 5: Approach to and relevance of proposed future research

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

- Not clear how system level modeling of impurity effects will be illuminating. It seems that experimental testing of stack sensitivity to impurities (e.g., CO, Sulfur, etc.) is required for this understanding.
- Not clear how further system freeze modeling will be beneficial, as stack level effects (e.g., GDL, MEA design) seem to be such a strong contributor to stack freeze start-up performance… (see FCP-21_Patterson and FC-30_Mukundan). Experimental testing and/or MEA/GDL analysis is required to quantify these effects.
- The project proposes to continue its strong dependence on Honeywell components. Honeywell is a component manufacturer and not a systems integrator or manufacturer of fuel cells. This approach greatly limits the scope and value of this program.
- The predicted cold startup schemes analyzed could be compared with actual stack and system design to see if the predicted cold startup methods are consistent with current system designs that can accomplish cold startup. This comparison would show if industry is using these techniques and if the techniques are consistent with the model prediction. The work at LANL on cell freezing should be considered as validation input.
- Most of the proposed work is the logical extension of the ongoing effort.
- The proposed future work included several potentially valuable initiatives; care needs to be taken to coordinate with the Fuel Cell Tech Team and industrial partners to ensure that focus is maintained on critical aspects.
- The proposed future work is relevant and appropriate. Leveraging other information sources and collaborators would enhance the effort.

Strengths and weaknesses

Strengths
- Keeping modeling expertise in one area can lead to increased speed of project development and improved results.
- Good that the modeling results are being tied to experimental data.
- The project is set up to be very responsive to the needs of the Fuel Cell Tech Team and industry. Good efforts are under way for validation of the results.
- Excellent team of investigators.
- The strengths appear to be the breadth and scope of the work. The work is applied science and meshes well with commercial efforts.

Weaknesses

- The purge recycle data has been known for several years and it is not clear that this was a contribution to the advancement of PEM fuel cells. The crossover of nitrogen is well known. This did not appear to advance the technology. Most fuel cell manufacturers are moving toward atmospheric operation which appears contrary to the direction of the model in this project.
- Modeling requires high levels of staffing and interaction with real systems to provide optimum results. Additional funding could improve this project if additional staff and resources are added both to the modeling effort and to the validation effort.
- Nitrogen contamination in feed streams leads to reduced cell performance. This is a simple dilution effect which is not surprising.
- Careful management must be maintained to ensure the same responsiveness in the future, the direction of future work can take many possible directions and the same attention to critical modeling efforts needs to be maintained. It does not appear that there are plans in place to validate start-up strategies at the system level.
- The weakness appears to be related to the leveraging of data sources and applying real world experience. Real world experience would question the use of 40 FPI heat exchangers. The concern would be fouling of the air flow paths by road dust, pollen, oxides, etc.

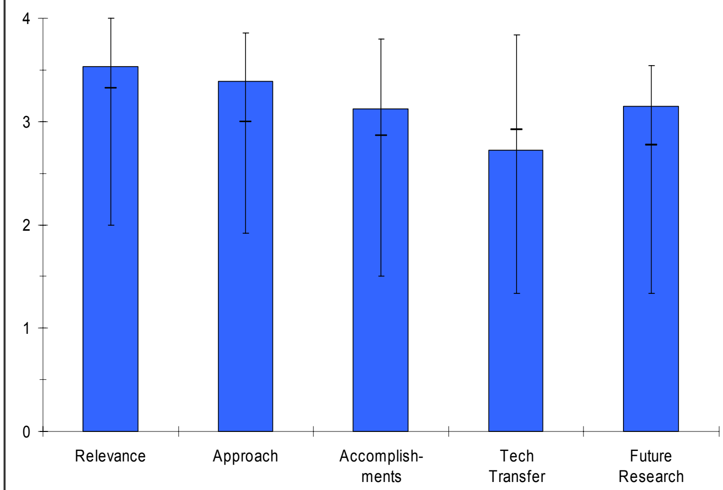
Specific recommendations and additions or deletions to the work scope

- Recommend deleting freeze system modeling and system impurity modeling, which seem to be more effectively addressed by experimental testing.
- The scope appears to be somewhat fluid, depending upon the needs of the program. This is expected.
- Romesh Kumar should update the H_2 Quality Task Force (and perhaps the Codes & Standards Tech Team) on the N_2 impurity work and determine whether it has any impact/implications for the SAE/ISO list of impurities. Future research plan should be reviewed with Fuel Cell Tech Team for input and suggestions. Review model on automotive fuel cell system with Systems Analysis Tech Team yearly.
- Expand the collaborative information sources and include more "real world" experience.

Project # FC-24: Effect of Fuel and Air Impurities on Fuel Cell Performance

Fernando Garzon; LANL

Overall Project Score: 3.2 (9 Reviews Received)



Brief Summary of Project

This project is focused on understanding the effects of impurities in the fuel and oxidant streams on fuel cell performance. The effects of fuel impurities such as hydrogen sulfide and air impurities such as sulfur dioxide and hydrogen sulfide are specifically targeted in this study. Methods to mitigate the negative effects of impurities are being developed along with models of fuel cell-impurity interactions. Collaborations and discussions with USFCC, Fuel Cell Tech Team, Industry and other National Laboratories foster a better understanding of impurity effects.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- My view is that impurities are not a high priority compared to other challenges. I understand that I may be in the minority.
- Project is focused on one of the most critical areas. Stable and lower cost electrodes will be required for long term success. We need to understand the lifetime issues of the MEAs.
- Understanding of impurities effect will be important if used appropriately in the development of future fuel purity standards.
- Excellent research effort. The PI did a great job of presenting the results of this research project and did an excellent job of synopsizing how various issues impact system design.
- Very important work relevant to both understanding, and eventually reducing costs, of FC catalysts as well as assisting in determination of fuel specification.
- The project is very relevant to the Hydrogen Fuel Initiative and it is well planned.
- Understanding effect of impurities deals with DOE goals on improvement of durability and electrode performance.
- This project addresses critical issues for fuel cell commercialization and the stated goals of the DOE.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative. Results from this research should be used as direct input into the national fuel quality requirements.

Question 2: Approach to performing the research and development

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

- Sound experimental approach.
- More fundamental than last year.
- Could recommend additional ex-situ testing with follow through electrochemical analyses.
- The research approach is excellent in that achieving the initial goals of developing analytical methods of tracing impurities and operating under a variety of conditions should allow sufficient measurements to achieve the breakthrough goal of developing models of fuel cell-impurity interactions.
- Well thought through effort. The appropriate analytical tools are being used to characterize poisoning of Pt catalysts. Nice ongoing and proposed work on competitive adsorption effects.

- Good balance of empirical testing, modeling, and team capabilities to develop fundamental understanding of impurity effects.
- The technical approach is quite focused to understand technical issues in the fundamental level.
- Only some basic clues to the technical barriers have been identified.
- Approach is suitable to the goals.
- The PI presents a good approach and excellent capabilities to perform the work.
- The research approach is outstanding. The methodology being used is very similar to the pure and applied science research of the major fuel cell developers. This approach should be considered the goal for other independent researchers.

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

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

- Progress is good and quite valuable.
- Should other sulfur-based species be investigated?
- Salt (chloride) effects on the performance might need to be looked at over a longer period of time and conflicts with existing knowledge. Should try to resolve this.
- Other organic decomposition products should be looked at.
- The impact of the GDL should be considered in greater detail. Is it a dense GDL like ETEK or a more open structure? Does it make a difference (anode or cathode)?
- The effect of water (humidification process) and the impurities should be considered, e.g., H_2S and H_2O electrochemically.
- 0.1% CO is too low – what happens if more realistic concentrations are used?
- Should coolant contamination be considered?
- Supported Pt is a major research topic – i.e., new supports. Is there any planned work on some of the new concepts being proposed?
- Impact of voltage on H_2S adsorption should be further investigated.
- This project's primary objectives and accomplishments to date seem to be an understanding of the physical science causing the barriers, with overcoming them an effort that will be mostly reserved for the future or other efforts. In terms of understanding the barriers, the research team has determined a number of possible reactions and mechanisms that are taking place, in a very controlled environment. These hypotheses will need to be confirmed before the logical leap to real world scenarios can be made with confidence.
- Making very good progress. This effort will is expanding the baseline knowledge of how impurities impact system performance and durability.
- Technical progress is quite significant.
- Coordinated experimental work leads to some valuable data that will benefit fuel cell developers.
- Given the funding level, more progress could be expected.
- Good progress in identifying conditions that lead to cathode/anode contamination/recovery.
- The technical accomplishments to date are very good. Line items need to be completed and information incorporated into complementing commercial activities.

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

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

- Better effort to incorporate field data from demonstration programs would help. Little discussion of the field data and the magnitude of the problem. How are the specific impurities prioritized, air vs. fuel, vs. corrosion?
- Appears to be acceptable for this stage of the program.
- Effort should identify the companies they are working with.
- To date the only obvious collaboration has been to use the hydrogen impurity mixture proposed by the FreedomCAR Fuel Cell Tech Team. With the end goal of influencing the development of codes and standards, increased coordination with other laboratories and industry will be essential; a similar effort is taking place as part of an Argonne project, (FC-23) for example, that will need to be integrated.

- Good teaming with FreedomCAR and USFCC. However the team interactions were not well communicated.
- Continue dialogue with Fuel Cell Tech Team, US Fuel Cell Council, and OEMs.
- Establish synergies with ANL (Debbie Myers) similar impurity studies. Perhaps a divide & conquer approach is warranted?
- This is a weak aspect of the project. Collaboration with other labs/university/industry is ignored.
- Industry collaboration, in particular could be very helpful to address practical issues.
- PI needs to increase collaboration with other institutions.
- A stronger interaction with industrial partners would benefit this program.
- Communications between commercial activities need to be improved.
- Communications and collaboration with HNEI and JARI has been initiated on testing and test methods.
- Communication and collaboration on detection methodology needs to be addressed. Specifically collaboration with the ASTM D03 committee (Raul Dominguez of SCAQMD is the chair).

Question 5: Approach to and relevance of proposed future research

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

- Additional potential sulfur gas phase species should be looked at.
- Additional air contaminants need to be investigated.
- Air cross-over is not always an option – additional operating scenarios may be of interest to investigate (voltage cycling, etc.).
- Study other organic species from the reformed process.
- Work should include other GDLs and other Pt/C supported concepts.
- Still would like to see additional *ex situ* testing, fuel cell testing can be difficult to interpret and is time and resource consuming.
- Cycling to measure recovery impacts, determining fuel performance thresholds, and further investigation of competitive adsorption effects seem to be promising areas of study, only some of which are highlighted in the proposed near-term work.
- Most of the proposed work is the logical extension of the ongoing effort. The multivalent cation work that was proposed is also very important.
- Ambitious project. Agree with direction.
- Continue emphasis on anode fuel specification with more single impurity component testing.
- Emphasize testing and understanding towards lower anode and cathode loadings.
- Investigate impact of membrane thickness? (Ex: Will O_2 crossover impact anode results?)
- Proposed future research is quite relevant to the defined objective.
- The outlined work is consistent to the current progress.
- Future effort should focus on studying the influence of impurities on HOR and ORR in half cells in order to elucidate the mechanism of impurity effects.
- Very good overall.
- The PI should verify that there are not additional air impurities that should be studied as part of the program.
- The proposed near term work plan is extremely good. Work needs to be accelerated if the DOE goals for a national hydrogen fuel standard by 2010 are to be met (California wants it by 2008). Completion of the work on sulfur and ammonia will go a long way towards this industry goal.

Strengths and weaknesses

Strengths

- Good focus on fundamental mechanisms. I would encourage continuing the emphasis.
- LANL continues to build off a strong program.
- Substantial technical expertise on multiple related areas.
- The project is a very useful test of the hydrogen oxidation models, and the development of the low-cost H_2S detector seems to be a key accomplishment, especially if it can be scaled for use by other researchers.
- Very good experimental effort focusing on relevant problems.

- The research team is very strong. Available physical facilities are excellent. Experimental approach is quite focused to relevant problems.
- Electrochemical techniques were complemented by new analytical techniques for sulfur determination.
- The PI presents a good systematic approach with focus on fundamentals.
- The PI presents excellent capabilities to perform the work.
- The analytical work and test results.

Weaknesses

- The connection with validating or modifying the specification is not clear. An effect of poisoning is good, but what does that imply about the proposed limits? Testing is not yet representative of the application, automotive with lots of start/stops.
- The project still seems to lack an ultimate goal/deliverable, as achieving the 2010 fuel cell targets (cost, durability, etc.) Simply by mitigating the impurities is impractical and beyond the scope of this project. If the purpose is to influence codes and standards, then that should be more explicitly stated and have a greater impact on the future research direction.
- Good work showing impurity effects on system performance. Additional work needs to be done to understand what is happening at the molecular level.
- External collaboration is nonexistent. Without industry collaboration, benefits will be marginal.
- Mechanism of the influence of impurities is not clear.
- Experiments should be done to break down the effects from hydrogen impurity mixture better – the PI should confirm some verifiable theories such as that the performance loss is due to resistance change from NH_4^+.
- Flow down of results and application to industry needs. By this it is meant that results are incomplete and not necessarily scalable for the various stack designs. Further collaboration with ASTM, FreedomCAR CSTT, HNEI, JARI, and USFCC should address this issue.

Specific recommendations and additions or deletions to the work scope

- The goal of developing methods to mitigate the negative effects of impurities (beyond cycling which is fairly easy and useful to perform in the controlled environment) could be reserved for future efforts that could be performed as part of a greater systems approach to improving fuel cell performance under real world conditions versus selected impurity mixtures.
- External collaboration is strongly urged.
- Extensive work on CO poisoning and its mitigation should be given special attention.
- Look at lower catalyst loadings in the range of the 2010 loading targets – this may give very different results.
- The PI's explanation of the effect of Na from NaCl (a membrane resistance effect) does not seem consistent with other work done in this area – look at Okata, T. in Handbook of Fuel Cells: Fundamentals, Technology and Applications, Vol. 3, Vielstich, W.; Gasteiger, H. A.; Lamm, A. Eds.; John Wiley & Sons: West Sussex, UK, 2003; p. 627.
- Complete the work on sulfur and ammonia. Interface with HNEI and JARI to demonstrate results are repeatable (at LANL) and reproducible (at HNEI and JARI). Test results should be combined into a single DOE report for each compound tested to be used as the definitive and referenceable DOE documents on the effects of the compounds on PEMFC (and PAFC, AFC – where applicable).
- Work with ASTM on impurities test methods. In the near term, the demonstration programs need to verify and quantify the impurities in the fuel grades being used. Laboratory test methods are lacking. ASTM D03 is generating recognized and published test methods for local commercial laboratories to follow to do the required testing. Input in the writing of these methods and validation of these methods needs to be addressed. LANL could assist in this area in several areas (e.g., the sulfur detection method discussed in this Program Review).

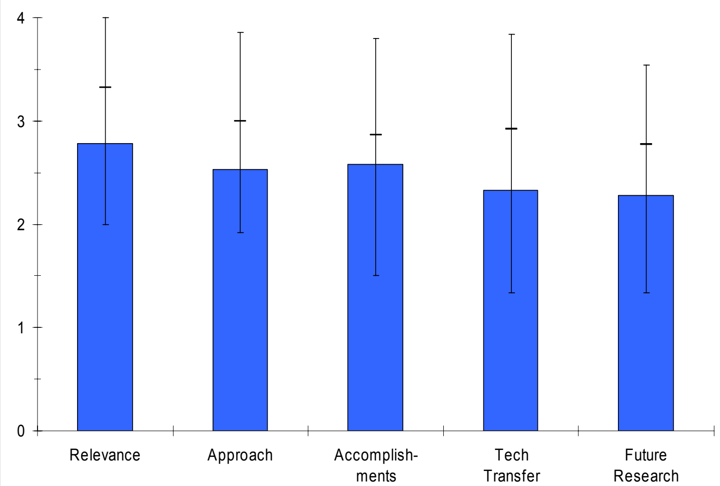
Project # FC-25: High Temperature/Low Humidity Polymer Electrolytes Derived from Ionic Liquids

Jim Boncella; LANL

Brief Summary of Project

The overall objective of this project is to develop high temperature polymer electrolytes for transportation applications of fuel cells. Specific goals include: 1) improving fundamental understanding of conduction in 'free' proton-containing ionic liquids; 2) investigating how phase separation behavior affects conductivity in well defined phase separated ionomers; 3) probing the dependence of properties on ion exchange capacity, water content and temperature; and 4) increasing conductivity at high temperature (~120˚C) and low relative humidity (<50% RH).

Overall Project Score: 2.5 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- If Polynorbornenes are not good candidates for actual fuel cell electrolytes, this research is several steps removed from usable electrolyte systems.
- Program addresses technical improvement of PEM fuel cells. Program seeks to expand range of operating parameters for PEM fuel cells for automotive applications.
- Very good idea. Ionic liquids need to be evaluated for this application.

Question 2: Approach to performing the research and development

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

- Working with admittedly weak backbone allows better materials science but takes results out of real world. Complete processing steps could point to high cost ultimately.
- Since a high level of effort was made to successfully use polynorbornenes, this effort would need to be repeated using a more suitable polymer in the future. Use of a more suitable polymer at an early stage would be encouraged.
- Program identifies the use of dihydrogen phosphate as one of the anions. The dihydrogen phosphate would lead to anion adsorption on the catalyst and reduce the activity. The approach would probably be of little value. The use of bisulfate at elevated temperatures could be a concern with the dissociation to form sulfur at the anode. This is a temperature limitation. The program addresses the formation of polymers made from the ionic liquids, but should also consider the anion effects before going too far.
- Good possibility for alternative electrolytes. I like the amount of work compared to the somewhat modest $200K/year funding. Researchers need to keep focused on the target conductivity needed for these materials.
- No clear path to a low cost or high durability membrane, promising RH performance but may not lead to a useful membrane.

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

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

- Have not demonstrated ability to create a block polymer. Water solubility will continue to be a problem.
- Progress has been made, but using an unsuitable polymer as the basis of the work is not encouraging.
- Good progress; project in early stages. Movement towards obtaining membrane for fuel cell evaluations in next year is important.
- Slow progress forward. Need to investigate more realistic anions. Needs better synthesis capabilities to integrate the IL cation onto polymers which are capable of being used in a fuel cell.
- The PI needs to do a better job on characterizing these new materials to include: transport, physical properties, etc.
- Lot of progress for the money spent. I would like to see the annual talk have more on the ultimate targets (conductivity and life). I assume the targets are in the back of the minds of the investigators, but it needs to be clearly captured.

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

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

- Good university collaborations, UMass., Virginia.
- No coordination was mentioned.
- One publication. Project does not appear to be coordinated with industry. Any reasons why?
- Moderate teaming. This project needs a collaborator who "knows" (electrochemistry, transport, and physical properties) ionic liquids such as Dr. Paul Trulove, United States Naval Academy or Dr. John Wilkes, United States Air Force Academy.
- Keep in mind an ultimate developer who would use the technology. List of collaborators is growing, which is good. More discussions with 3M, Dupont, etc. Would be valuable, but only on a precompetitive basis (at this point). The technology is new enough that it is not appropriate to go proprietary at this point.

Question 5: Approach to and relevance of proposed future research

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

- No clear plan for incorporating block copolymer. No clear plan for a stable backbone.
- Future work still continues to plan to use norbornene polymers. This is not encouraging.
- Future work identified does not show correspondence to fuel cell applications. The project needs to be directed to solve fuel cell issues. Critical issues appear to indicate that the project may not lead to advanced membrane for PEM fuel cells.
- Future research not discussed in the slides. Some future research areas where mentioned in the question and answer period.
- Still early in the development of these compounds and LANL has a good broad-based plan.

Strengths and weaknesses

Strengths
- The project team appears to understand the electrochemistry very well. They appear to be experienced and knowledgeable. They also understand why these polymers are not suitable for fuel cell electrolyte.
- Good idea to investigate ionic liquids for fuel cell applications. Covalent attachment of ionic liquids to polymers is innovative.
- LANL fully understands fuel cell requirements.

Weaknesses

- Have not achieved a stable, non-soluble membrane. No clear approach (other than "thinking with chemistry") to achieving a useful membrane. No identified backbone.
- The basis of the work is using a polymer that is admittedly not suitable for fuel cell electrolytes.
- Good work showing impurity effects on system performance. Additional work needs to be done to understand what is happening at the molecular level.
- Input from knowledgeable reviewers (3M, DuPont, etc) is valuable.

Specific recommendations and additions or deletions to the work scope

- Funding at this modest level of effort is not likely to lead to useful, practical results.
- The project should start to work on more promising polymer types.
- PI should investigate preparing and characterizing polymer/ionic liquid blends. This technique has been used previously to prepare gels.

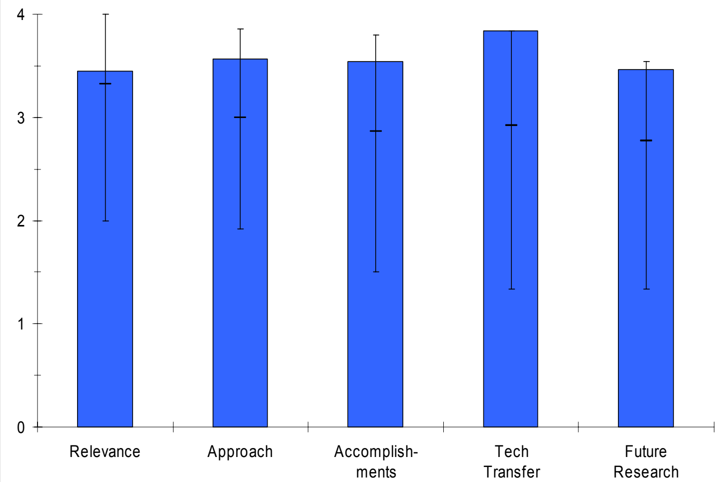
Project # FC-26: Neutron Imaging Study of the Water Transport in Operating Fuel Cells

Muhammad Arif; NIST

Brief Summary of Project

The goal of the National Institute of Standards and Technology project is to develop effective neutron imaging based, non-destructive diagnostics tools and an experimental facility to characterize water transport in operating PEM fuel cells. Objectives include providing research and testing infrastructure to enable the fuel cell industry to test commercial grade fuel cell flow field and MEA designs; training industry to enable it to use the imaging facility independently; and transferring data interpretation and analysis algorithms/ techniques to industry to enable it to use research results more effectively and independently.

Overall Project Score: 3.5 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Water management in fuel cells is required for efficient operation. This project provides a novel tool for visualizing water *in situ*. The data obtained are critical for flow field design and thermal issues. The work is important for realization of the President's Hydrogen Fuel Initiative.
- Water management is one of the key enablers to durable fuel cell design. This project plays a very significant role in the understanding of water management and facilitating design.
- Water management is a major issue.
- Understanding water behavior is critical to managing and stabilizing performance during transients, steady-state operation, and start-up/shutdown cycles. This technique facilitates this understanding.
- Neutron imaging at NIST is proving to be a valuable tool to visualize water flow in operating fuel cells. It is extremely relevant to the DOE Hydrogen Program as evidenced by the many requests for work under proprietary agreements.
- The project is highly relevant to DOE objectives.
- In-situ monitoring and analysis of water in an operating fuel cell represents a unique capability that could provide useful information.

Question 2: Approach to performing the research and development

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

- The project is well designed and the strong push to lower size scales makes the potential impact of this technique much stronger. The addition of tomography and low temperature control is likewise beneficial.
- Approach will result in geometry-specific results.
- Approach should include working towards models.
- The approach is good.
- The Advanced Fuel Cell Imaging Facility is state-of-the-art. The addition of a freeze chamber is an important addition to the existing apparatus.

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

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

- It is easily seen how the results to date can be useful for modelers and flow field designers. The project doesn't involve direct research, but the data presented in collaboration with others are insightful.
- State-of-the-art facility.
- Facility and capabilities are impressive but results to date are not.
- NIST has made crucial progress in the area of spatial resolution, gaining insight not only across the facial area of the fuel cell (x and y directions) but also in the z direction.
- NIST is completing the studies on schedule and they showed several examples of the results of their work in the presentation.
- The research team has made good progress in monitoring water generation and distribution in an operating fuel cell. It is not clear how data collected at the facility are being used by modelers who are studying water management in an operating fuel cell (i.e., where is the match/comparison of experimental data and model predictions?).
- This new facility should be very popular.

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

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

- Project inherently involves a number of other participants. The structure to be limited to 50% proprietary data, limits this aspect of the project in score, but is acceptable.
- Excellent balance of open and proprietary research.
- Are all FC publications resulting from the NIST imaging facility use listed on one site? If not, recommend listing on their website.
- Essentially all work is transferred either public information or to proprietary partners.
- NIST has a very broad spectrum of collaborations comprising component developers, stack manufacturers, fuel cell system integrators, and users. Facilities are available to everyone.
- The NIST neutron source is considered a national user facility. Therefore all of the work that NIST undertakes in this area is as a result of a collaboration with fuel cell developers.
- The imaging facility has a nice group of users and collaborators. I was surprised and somewhat disappointed that LANL was not on the user/collaborator list.
- Probably the highest technology transfer project in DOE. A very impressive leveraged project. It would be good if such technologies and lessons learned can be transferred to other facilities with neutron imaging capacity to help better manage the resource demands.
- Excellent collaboration with industry, much better than the average project. Good to see that industry is sharing in providing funds.

Question 5: Approach to and relevance of proposed future research

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

- Tomography and lower size scale resolution are certainly useful. The results shown to date suggest advancements can be made in this area. It would be useful to understand the ultimate limit of size resolution to better understand the ultimate limit of this technique.
- If dynamic gas flow capability (1-2 second response) is not yet available, consider for future development.
- Freeze/thaw planned for following year will be a strong added capability.
- Good, but should include collaboration with model developers.
- Enhancing spatial resolution will be extremely valuable. Addition of freeze/thaw capabilities is also important.
- NIST is installing an environmental chamber at the beam line that will have the capability of conditioning the fuel cell at various ambient conditions including temperatures as low as -40°C.

- More collaboration with researchers studying water management is warranted. Future research should focus on water generation and distribution in a fuel cell stack, as opposed to a single MEA.
- How do you plan to accommodate more users?
- Freeze chamber addition and tomography good.

Strengths and weaknesses

Strengths
- Novel, useful technique. Project leveraged through other funding sources to improve cost/value ratio for DOE.
- Well-deserving of their American Competitiveness Initiative recognition!
- Very powerful tool with potential for much useful input.
- NIST is able to complete the agreed upon work on schedule as presented. Environmental conditions down to -40°C ambient can be achieved.
- The facilities are excellent and the addition of a freeze chamber will enhance the Imaging facility's capabilities.
- Only 20% of the facility's annual budget is provided by the DOE. The project team has successfully attracted users/collaborators that can pay for use of the apparatus.
- There is a nice mix of users/collaborators from academia, industry, and government labs.
- A tool for all to use. A good balance between proprietary and non-proprietary work.
- Open tool available to all. More than other projects, this activity advances the fuel cell technology for everyone.

Weaknesses
- Size scales probed are still too large for a number of fundamental aspects that would be interesting.
- As currently operating, work is mostly a development tool for specific designs.
- It is not clear if this technique can distinguish the 3 states of water.
- The interpretation of data is weak. For example, experiments showed that there is a maximum in the water content of an MEA at a point between the anode and cathode (closer to the anode) but this finding is not intuitively obvious and it has not been verified through additional experimentation or by theoretical modeling.
- There is no collaboration with the fuel cell group at LANL.
- The 3D tool needs to be ready soon. It is still unclear how a 200+ cm^2 multicells stack can be evaluated with this technique on a cell to cell basis at this moment.

Specific recommendations and additions or deletions to the work scope

- Project is worthwhile, and researcher involved has provided solid results.
- Try to use the impressive capability to help produce (through appropriate partners) models and simulation capabilities.
- NIST should ensure that reactants can be conditioned to the same temperature as the fuel cell when cold start experiments are performed. This capability was not mentioned in the presentation.
- Work more closely with fuel cell developers/modelers, especially at LANL.
- Carry out experiments in a fuel cell stack.
- Please invest more resources into developing software that would help extrapolate information for large multicell stack development.

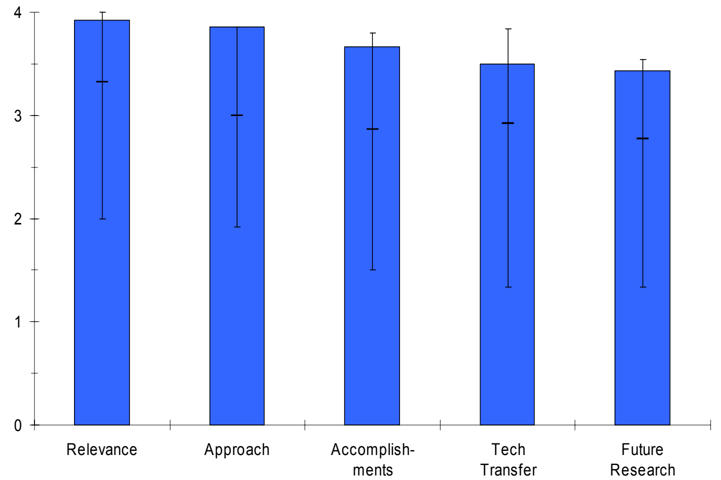
Project # FC-27: Microstructural Characterization Of PEM Fuel Cell MEAs

Karren More; ORNL

Brief Summary of Project

The project at Oak Ridge National Laboratory (ORNL) is focused on elucidating the mechanisms contributing to performance degradation of PEMFC membrane electrode assemblies (MEAs) by conducting extensive microstructural and compositional analyses using high-resolution electron microscopy and spectroscopic techniques. These analyses are performed on (1) the individual MEA constituent materials (i.e., electrocatalyst, catalyst support, membrane, ionomer) and (2) fully-fabricated MEAs, which have been subjected to a variety of processing and/or electrochemical aging conditions.

Overall Project Score: 3.7 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.9** for its relevance to DOE objectives.

- As MEA component materials are developed for the stringent automotive MEA requirements, the problems and issues require increasingly greater in-depth analyses. The ORNL TEM project is a great example of a successful program accomplishing just that. One might always argue that even without this technology analysis, the H_2 Initiative barriers will be overcome, but it may take longer and cost more.
- This project is developing key analytic tools needed by fuel cell technology developers to solve their specific degradation mechanisms.
- This highly valuable characterization technique can contribute to minimize the use of precious catalyst, and improve the durability of electrodes and membranes.
- Characterization is essential to improving the performance of the fuel cell MEA – a key component of fuel cell systems. Long-term durability is a critical issue in fuel cell development.
- The project is very relevant to the Hydrogen Fuel Initiative and of practical importance. It is consistent with the DOE's multi-year goals and objectives.
- Extremely important to have a better understanding of MEA degradation issues.

Question 2: Approach to performing the research and development

This project was rated **3.9** on its approach.

- The approach itself cannot of course develop or invent new materials that will directly remove a barrier. However this work is very critical for decision making on what material sets have the potential, or do not have the potential, to overcome critical barriers to the H_2 Initiative.
- The materials characterization capabilities of ORNL seem to be well applied to this area.
- The project clearly demonstrated the usefulness of this technique to meet the DOE target especially on MEA durability.
- Developed and used a "treasure chest" of characterization tools to study durability and performance of fuel cell catalysts.
- Technical approach is very sound and well coordinated.
- Work plan is well-designed; practically feasible concepts are being pursued.
- Provides a unique view of electrochemical aging which can lead to better designs for durability.

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

This project was rated **3.7** based on accomplishments.

- Program has made excellent progress towards meeting its objectives. The project by its nature identifies things that go wrong rather than what went right with aged MEAs. This in and of itself is very important, but cannot really chart a path to tell how a barrier will be overcome.
- The aging effect to the catalyst growth and migration within the electrode and into the membrane has been clearly validated.
- Significant progress has been made in using TEM characterization to follow catalyst degradation as a function of operational lifetime.
- The technical progress is very significant.
- Experimental data should be quite helpful to the understanding of some key technical issues.
- No remedial methodology has yet been developed; more extensive work needed for practical benefits.
- Very impressive and very important results.

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

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

- It would be interesting and potentially useful to validate results from other DOE sponsored research projects using these analytic tools.
- FreedomCAR & Fuels Partnership funds should not be going toward proprietary efforts with developers. If the developer is paying full cost, then it is OK. FreedomCAR funds directly to the National Labs should benefit everyone. ORNL seemed to be an outlier in the amount of proprietary work underway.
- Broad interaction with MEA industry was mentioned.
- The detailed procedure needs to be shared with other public and private institutes who can use this excellent technique for their MEA development.
- Extensive collaborations will both other National Labs and industrial organizations.
- Project relies on input (characterization requirements) from other organizations.
- Information gained from this project should help extend the performance lifetime of the catalyst.
- Level of industry/university/other laboratory collaboration is commendable.
- Practical issues encountered by industry partners should be given due priority.
- Good, but there could be better transfer of information.

Question 5: Approach to and relevance of proposed future research

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

- More of the same will be excellent.
- Verbal statements made in presentation clarified future work.
- More quantitative analysis is needed.
- Future work is planned to bring new characterization techniques on-line as well as continue the important collaborations that are already in place utilizing TEM characterization.
- Proposed future work is very relevant to practical issues and consistent with the outcome to date.
- Proposed collaborative work with membrane/MEA developers should be beneficial to the industry.
- Hard to fault an ordered continuation of this work.

Strengths and weaknesses

Strengths

- Expertise of the PI and ORNL TEM laboratory.
- This is highly relevant work. The industry needs to have the tools developed which can be used to understand and accelerate development of this technology. The results from this project are primarily in the catalyst degradation area which is of high relevance to performance and lifetime.
- Great technique for understanding Pt migration and clustering. Good national resource.
- An excellent microstructural analysis tool has been delivered.
- The application of existing characterization tools at the National Lab to address problems at other organizations including those in industry represents an important return on the "public" investment.
- The PI clearly demonstrates the knowledge required to interpret characterization data in terms of optimizing fuel cell performance.
- The research team strength is outstanding. Physical facilities available for the project are excellent. Collaborative partnership is most helpful and is a big plus.
- Impressive use of sophisticated techniques.

Weaknesses

- Assessment of membrane morphology has only started and it is unclear its value to the general PEM research field. Further to this, developing tools to understand PEM degradation mechanisms is an area worthy of future research. I suspect that work has been done in this area but is proprietary and not publishable.
- The only non-proprietary work seems to be what is being done with LANL. Non-proprietary work should increase.
- No collaboration activity to transfer the technique.
- The research team should have given more attention to develop remedial approach (es) to address some of the issues.

Specific recommendations and additions or deletions to the work scope

- ORNL should not apply FreedomCAR funds to the proprietary part of the work.
- The PI needs to transfer the technique to be public.
- Keep up the good work – if demand for this characterization support exceeds current capability- funding (which is modest) should be expended to help meet requests for support, particularly from industrial organizations.
- Work on new membrane/catalysts/MEAs should be extensively pursued.
- Perhaps a little more emphasis on technology transfer.

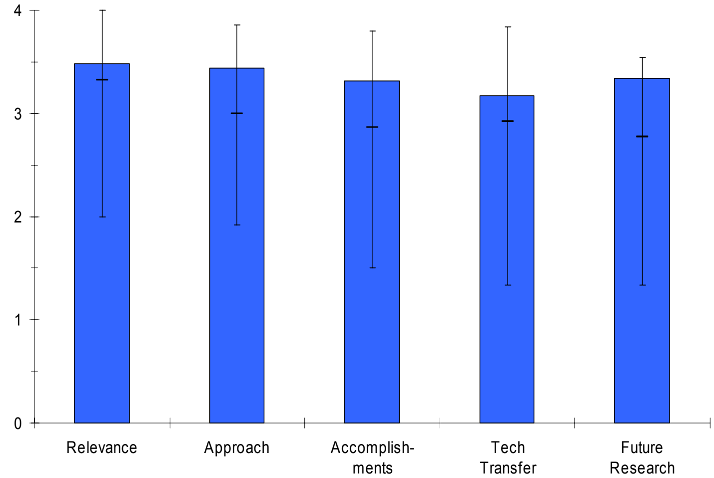
Project # FC-28: PEM Fuel Cell Durability

Rodney Borup; LANL

Brief Summary of Project

In this project, Los Alamos National Laboratory is identifying and quantifying 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. The overall objective is to meet the 2010 DOE target of 5000 hours with cycling.

Overall Project Score: 3.4 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Presentation stressed performance, not causes of degradation.
- Project is focused on one of the most critical areas. Stable and lower cost electrodes will be required for long term success. We need to understand the lifetime issues of the MEAs.
- This effort continues to pull together the critical understanding of the elements of the electrode.
- This research project examines a range of MEA durability issues. Durability is an important issue.
- Nice project. Doing a good job of trying to nail down several "real life" issues that will dramatically impact fuel cell durability.
- Fuel cell durability is one of the two or three primary enablers to FC commercialization.
- This project is directly focused on identifying the mechanisms affecting degradation durability which is a prime barrier to fuel cell commercialization. As such, it directly supports the Hydrogen Fuel Initiative and the MYPP.
- This project is a rational approach to addressing the President's Hydrogen Fuel Initiative. The results from this effort are directly supportive of industry efforts.

Question 2: Approach to performing the research and development

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

- This project seems to be working at the early stages of durability analysis. Additional work is necessary to draw cause-and-effect relationships
- Between changes in the membrane, the GDL, and the catalyst and changes in cell and stack performance. Additional work beyond this
- Effort is necessary to understand the cause of the changes being seen in the various materials.
- Sound experimental approach.
- Fundamentals continue to remain the main focus.
- The collaboration with ORNL is excellent, but even further integration of the two programs should be considered.
- A variety of different durability tests was performed. Accelerated testing of fuel cell MEAs is important, but such experiments must be carefully scrutinized to insure that the results mimic the actual degradation that will occur during long-time fuel cell operation. Although one of the objectives of this project is to "design new materials with improved durability," no such materials were proposed.

- Good solid approach characterizing the effects of fuel cell cycling, platinum particle growth. It would be very beneficial to understand the mechanism by which this occurs (physical migration or dissolution followed by deposition). Also what is driving this phenomenon? Making good progress figuring this out.
- GDL characterization effort good. Needs to be supplemented with some surface characterization (XPS, Auger, TOF-SIMS).
- Strong combination of testing capability, diagnostics and analysis.
- Project continues to provide good insights into fuel cell durability issues.
- The project is sharply focused on fuel cell durability, looking at many of the key degradation mechanisms.
- Very clear, lucid approach to the examination of PEMFC degradation mechanisms.
- Project is developing modeling activities in an attempt to further understand and correlate empirical degradation testing results. This is a good idea; however, a note of caution is that modeling development should not come at the expense of the excellent progress being achieved via durability testing. Project is looking at so many areas that there is a risk of small degradation nuances being missed.
- The approach to this project is well ground in sound engineering practice and managerial judgment.

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

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

- Membrane modeling of platinum particle size growth was compared to TEM scans, showing good correlation.
- Technical accomplishments are significant and relate well to the operating modes of the fuel cell.
- More delineation of the MEA components needs to be included (GDLs and electrode layers are being investigated and modeled, but are these representative of what the industry is developing?) Not all GDLs are the same but it is good to see they were identified.
- Need to take the technical knowledge of this excellent program and work with the industry suppliers and fuel cell developers even more closely.
- Would like to see actual mechanisms identified. To date, the group is still in a "measurement" phase.
- Can this effort develop a standardized post mortem testing protocol?
- Good to see transient behavior studied.
- Catalyst agglomeration models exist in other industries (should utilize the existing knowledge).
- Loss of hydrophobic character: General Electric showed very long GDL life 30 years ago – why is it different today?
- Lots of data were collected by this research team, but the interpretation of the results was rather weak and there was no suggestion(s) as to how one might improve the properties of various fuel cell components to minimize/eliminate degradation.
- Good progress forward. Many new important results.
- Many effects observed and measured (changes in hydrophobicity/hydrophilicity, GDL porosity, etc.).
- Causes for these effects are postulated, but need further validation/verification. Looking for the team to close the loop on the observations and insights they've gained these last couple of years to prove mechanisms.
- Excellent technical progress achieved in all areas in identifying, examining, and elucidating degradation mechanisms.
- Prodigious technical output quantifying factors limiting PEMFC durability.
- The accomplishments to date are impressive. Relevant tools are being generated and properly applied.
- Data mining may be in order. There might have been some research in the hydrophobicity of carbon substrates as part of the PAFC research in the 1980's. Review of the quarterly reports to NASA and to DOE METC during this time frame may be of value.

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

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

- Good coordination with other labs and groups.
- Should increase activities even further.

- It is not clear how industry, universities, and other National Laboratories are using the data collected by this research team. There needs to be closer collaboration with other fuel cell groups (e.g., catalyst degradation should be examined in collaboration with a fuel cell catalyst group).
- Good teaming. The PI has assembled a good team to obtain and characterize PEM fuel cell degradation. Need additional surface characterization team members.
- Strong visibility through publications, presentations and reputation in community.
- Good collaboration within various National Labs.
- Increase collaboration with industry material/component developers.
- Good interaction and coordination with other institutions and projects.
- PI may want to consider downloading a greater degree of the project tasks to other institutions if feasible given the extent of work activities.
- Collaboration to date has been excellent. Additional potential contacts could be found among the USFCC material & components working group, durability task force.

Question 5: Approach to and relevance of proposed future research

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

- It was difficult to draw conclusions regarding causes of degradation from the presentation. Although much data was presented, there was not much regarding the causes of the changes and not much data regarding the effect of the changes on cell performance. Additional work should add efforts to identify causes of degradation.
- Work as much as possible with industry standard materials.
- Better coordination is needed between this research team and other DOE-sponsored research groups. There seems to be considerable duplication of effort on many durability topics, such as catalyst particle growth and the degradation of GDL and membrane materials.
- Good future research proposed. Research ideas relevant.
- Aggressive future activity schedule.
- PI should consider narrowing down somewhat future scope to look more deeply at a few key areas and run more tests within each area.
- The list of future activities listed is impressive. Catalyst durability information may be available from PAFC research in the 1980's. Documenting and publishing suitable test protocols may be warranted.

Strengths and weaknesses

Strengths
- Excellent results regarding degradation.
- LANL continues to build off a strong program.
- Substantial technical expertise on multiple related areas.
- This research team has performed numerous experiments and has collected a large quantity of data over the past year.
- Qualitative conclusions have been drawn on some results.
- Very relevant project. Good results.
- Very strong diagnostics and analysis capabilities.
- Excellent test capability, including transient control.
- Very strong laboratory approach with prodigious and excellent *in situ* and *ex situ* testing results.
- Project is casting excellent insight to the broad picture of fuel cell degradation mechanisms.
- The focus and diversity of the team.

Weaknesses
- Limited data on causes of degradation.
- There was no identification of new materials with improved durability.
- There was little interpretation of degradation data.
- The materials cost issue was not addressed (one of the stated barriers).
- PI needs to perform additional surface characterization of GDLs.

- PI should be careful not to lose the excellent focus and progress achieved so far by attempting to cover too many areas simultaneously. Possibly consider greater use of collaborators.
- The apparent lack of reviewing previously sponsored work for the US Government.

Specific recommendations and additions or deletions to the work scope

- Spend more time on causes of degradation and mitigation in the future.
- There needs to be more collaboration and less duplication of effort. Better coordination of MEA durability studies, from all DOE-sponsored groups, is warranted.
- Evaluate hydrophobicity of the GDL in both oxidizing and reducing environments. Publish durability test methods through a recognized authority. Two options might be the USFCC and SAE.

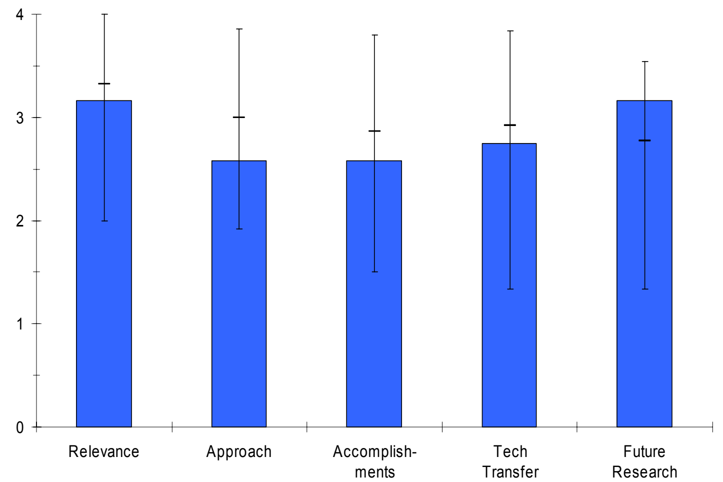
Project # FC-29: Investigating Failure in Polymer-Electrolyte Fuel Cells

John Newman; LBNL

Brief Summary of Project

This project focuses on examining various types of fuel-cell failure, including both mathematical modeling and supporting experimental studies. The issues investigated include fuel-cell water and thermal management, subzero operation, membrane degradation, and mechanically-related failure. 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 theoretical optimization of material properties, operating conditions, and possibly start-up and shut-down scenarios.

Overall Project Score: 2.8 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Presentation disjointed. Very difficult to follow presentation and understand subprojects being worked.
- Theoretical analysis is essential to address the durability issues.

Question 2: Approach to performing the research and development

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

- The approach looks good in the slides. It did not come across during the presentation. Membrane constraint result interesting.
- Model development is good, but make sure validation is a continuous process.
- Experimental validation of the model needs to be added.
- Most of the work is on modeling normal fuel cell performance instead of simulating failure modes.

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

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

- Project appears disjointed. Project has questionable relevance.
- Hard to distinguish what was done in the past year vs. literature and older modeling work.
- There is too much emphasis on modeling fuel cell performance in model operation mode. Many researchers already have modeled and reported similar results.
- No experimental validation of the modeling results is shown.

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

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

- Minimal teaming. However, funding of effort is small.

- In transferring results by presentation, it would be useful to have one chart with text (maybe small print necessary) informing/reminding the audience in words what phenomena are included in or excluded from the model.
- For this small project, it is critical that it be integrated with partners such as LANL and Georgia Tech, provided they have parallel programs on failure also.
- Industrial partners, who can commercialize the catalyst, will be a beneficial collaboration.
- Are there any opportunities to model some of the public NIST water results as a way of model validation?
- No results from the collaborators are shown. More collaboration is needed to validate the modeling results.

Question 5: Approach to and relevance of proposed future research

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

- Future research proposed is reasonable.
- Further challenge could be to model the thermal/chemical/mechanical stress coupled phenomena occurring around a pin-hole or membrane breach often found near edge of MEA.
- Do we know peroxide production vs. Temp, H_2 flux, potential?
- Can a model be developed explaining why OCV and low RH conditions are so accelerated?
- The effort as proposed is very logical.
- The PI should focus more effort on membrane degradation work, particularly mechanistic understanding.
- Investigating the major failure mechanisms is planned.

Strengths and weaknesses

Strengths
- Membrane constraint results interesting.
- Approach taken of modeling at fundamental level is sound and not easily carried out by many other institutions.
- Use of nano-size bimetallic catalysts is quite promising. Analytical tools used for screening are effective. Alternate method for synthesis.
- Water and thermal management work addresses critical issues.
- The PI offered an interesting explanation of Schroeder's Paradox.
- Planned stress model development good plan.
- The PI has very strong skills and experience in theoretical analysis of electrochemical systems.

Weaknesses
- The membrane degradation studies with Georgia Tech proposed look like work which has already been published by DuPont and GM. It is not clear how this will be different.
- More collaboration is needed for comprehensive analysis of the failure mechanisms.

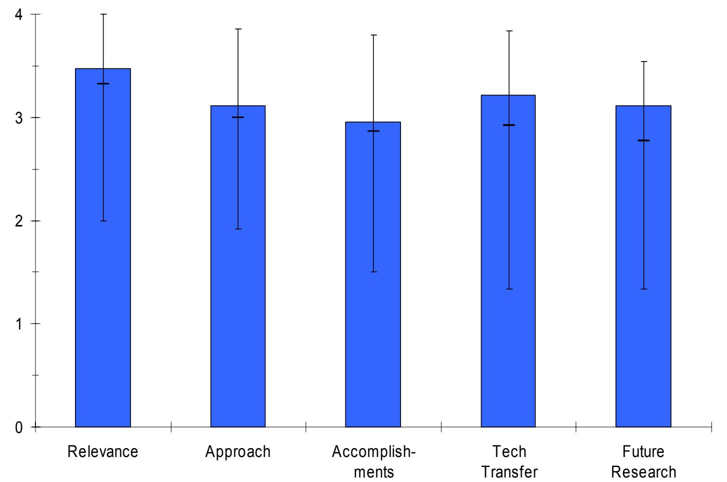
Specific recommendations and additions or deletions to the work scope

- If not already done, consider a review of similarities and differences between LBNL/Newman model(s) and ANL/Kumar system model.
- Could LBNL be used as input/"back-office" of ANL?
- Low temperature studies of membranes are probably not relevant to real cold start and freeze/thaw issues.

Project # FC-30: Sub-Freezing Fuel Cell Effects

Rangachary Mukundan; LANL

Overall Project Score: 3.1 (7 Reviews Received)



Brief Summary of Project

The overall objective of this project is to assist the DOE-HFCIT Program in understanding the role sub-freezing temperatures play on fuel cell performance and durability to meet DOE milestones for sub-freezing startup (-20°C, 30 sec, 5 MJ) and survivability (-40°C). Characterization of water in ionomer, catalyst, and gas diffusion layers is being done and degradation mechanisms are being identified including freeze/thaw cycling (ice formation), startup and shutdown, and thermal cycling.

Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Freeze start and degradation are key barriers to automotive fuel cell commercialization.
- Important work that needs to be done. Understanding cold start-up and temperature cycling induced failures is critical.
- Sub-freezing studies of PEM fuel cells are important.
- This project supports the goal of ensuring that fuel cell technology works in all weather and climates – a vital factor for wide acceptance of the technology.
- Understanding of fuel cell freezing effects are critical to fuel cell applications.
- This work is essential to having workable vehicle systems in the world market.

Question 2: Approach to performing the research and development

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

- Nice combination of material properties, necessary for understanding, and proposed mechanism development.
- Experimental characterization of water in the ionomer, catalyst and gas diffusion layer is well underway. Identification of the degradation mechanisms during ice formation, startup and shutdown and thermal cycling is also underway.
- Good approach. Doing a good job testing the worse case scenario.
- The state of water in Nafion is well understood. The research team's interest in examining non-Nafion membrane materials is worthwhile, but I do not understand why they are focusing so much of their time and effort on the BPSH material. Both U.S. and Japanese automakers have no interest in this type of membrane, for a variety of reasons.
- The series of studies on fuel cell components seems to be reasonable. Studies should be broadened to complete systems, including designs in balance-of-plant to alleviate the freezing problem.
- Extending work to non-Nafion membranes is key to future developments.
- Good research, focused on hardware effects.

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

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

- Reasonable progress so far. State of water and conductivity measurements.
- Need to test greater # of freeze/thaw cycles. Target around 1000+.
- Need more post-mortem diagnostics, ECSA, etc.
- An understanding of the state of water in Nafion at different RH and the RH effect on bound freezing water and non-freezing water was presented. Conductivity measurements have been made. Water of hydration measured. Freeze/thaw cycling of MEAs is performed. Mechanical degradation of GDLs observed. Start-up performance measured under fully humidified conditions with different GDLs. Flow problems investigated.
- Overall good technical accomplishments. The temperature cycling results are very important. DSC results are suspect, at a minimum, results skewed by over 15 degree Celsius. A little more careful experimental technique will go a long way.
- A great deal of data has been collected, but the interpretation of the results is weak and there was no suggestion as to the use of new materials. It appears that freeze/thaw cycling has little effect on the voltage-current density behavior of an MEA (for 45 or 100 cycles). Such data indicate that there is no real problem with regards to freeze/thaw events. Since there does not appear to be a degradation mechanism associated with freezing and cold start-up, perhaps this work should be de-emphasized in the coming years.
- This project is focused on identifying causes of failure in subfreezing conditions, but only indirectly with remedying them.
- Some of the experiments were not able to demonstrate performance degradation with freeze cycling, and this needs to be sorted out to better interpret failure modes.
- Interesting work. The results showing no degradation after freeze/thaw cycles are counter-intuitive, but well documented for a low number of cycles. GDL fiber breakage is another interesting result.

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

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

- In contact with both National labs and OEMs – good.
- Partners with other National Labs like Sandia and LBNL and also with GM and GE through sharing of data.
- Very good teaming. UTC Fuel Cells/UTRC, General Motors, Plug Power, and Hunter College.
- Collaborations with industry/universities/other labs are weak. Other groups are examining degradation mechanisms.
- Project spans three National Labs and two large industrial partners. There seems to be a lack of coordination with a similar project at ANL (FCP-23). Project should include coordination with suppliers of components being tested, so as to work towards subfreezing-compatible components and system designs.
- Nice work in support of industry. Other experimental and modeling efforts are going on at LBNL and ANL. The efforts could be coordinated to avoid duplication and yield a better understanding of effects. Improved measurement technique from initial data presented last year.
- It appears that this work should be better coordinated with the Argonne modeling effort.

Question 5: Approach to and relevance of proposed future research

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

- Extend start testing to colder temperatures, to -40°C. While the DOE roadmap indicates fast start from -20°C, unassisted starts will be required at lower temperature.
- Consider testing with different catalyst layers (thickness, loadings) to assess impact.
- Conductivity effects on non-Nafion® membranes during freeze will be interesting. Transient responses and mechanical property and degradation mechanism studies are indeed needed.
- Good future research plans. Understanding membrane thinning and failure is important.

- Future work, as indicated on the Project Summary slide is of limited scope and lacks details. What electrical components will be characterized? From recent experiments, it can be concluded that there does not appear to be any degradation due to freeze/thaw cycling. There is essentially no effect of cold start on fuel cell performance at high voltage (about 0.6V). So why spend time and money to look at such effects? Examination of cold starts and freeze/thaw events in a fuel cell stack (as opposed to a single MEA) would be worthwhile.
- PI indicates plans to procure environmental test chambers for component research – a worthwhile extension. Future research should also include tests on complete systems.
- Need to add analysis of freeze/thaw and start/stop dynamics. More data points to get meaningful statistics for degradation and failure rates.
- It would be good to add higher cycle testing, to simulate a full stack life cycle for an automotive system.

Strengths and weaknesses

Strengths
- Teaming and thorough experimentation looking at the fundamental steps in freeze/thaw should provide information for mechanism development.
- Good temperature cycling results.
- The research team is well qualified to study durability issues. The team has collected a great deal of data over the past year and has made some important findings, in particular that freeze/thaw cycling has little effect on fuel cell performance.
- Good start for a project that covers a fundamental program need – analysis of failure modes of FC components at subfreezing temperatures.
- Highly relevant work in support of fuel cell industry. Strong leadership by LANL and good collaboration with GM.
- Good work focused on real hardware effects.

Weaknesses
- Need better characterization of water environments in the membranes.
- There is little coordination between the work of this team and the research efforts of others who are looking at degradation issues. The team's focus on BPSH-type polymers as alternatives to Nafion is misdirected and will eventually hurt the credibility of the team and the DOE. Automotive companies are not interested in the BPSH materials. The free water content of BPSH-30 and BPSH-40 were contrasted with Nafion in one slide, but these materials have a proton conductivity considerably lower than that of Nafion, so any comparison of water content is unwarranted.
- Project coordination between this work and that at ANL not clear – potential duplication of effort. In addition to components, PI should articulate goal to develop testing program for complete systems, with and without design features for mitigating subfreezing effects.
- Lacks coordination with ANL/LBNL efforts in this area.
- Higher cycle testing is needed.

Specific recommendations and additions or deletions to the work scope

- Would be nice to see how the MEA performance is altered during freeze/thaw. ECA measurements, change in gas crossover in addition to mechanical property measurement would be nice.
- The research team should carry out fuel cell stack tests. Experiments should be coordinated with fuel cell system people, e.g., should water be purged from the system prior to shut down and freezing or should water in the system be allowed to collect and freeze, with the eventual thawing of any frozen water during start-up.? Wind down the freeze/thaw work, since experiments have shown that such events do not affect fuel cell performance.
- Addition: testing of complete systems.
- Need more data for better statistics of failure modes. Environmental chamber added at NIST should allow neutron imaging to be added for next year.
- Full system testing would be a positive addition. This might be integrated with Argonne testing of the Plug Power system.

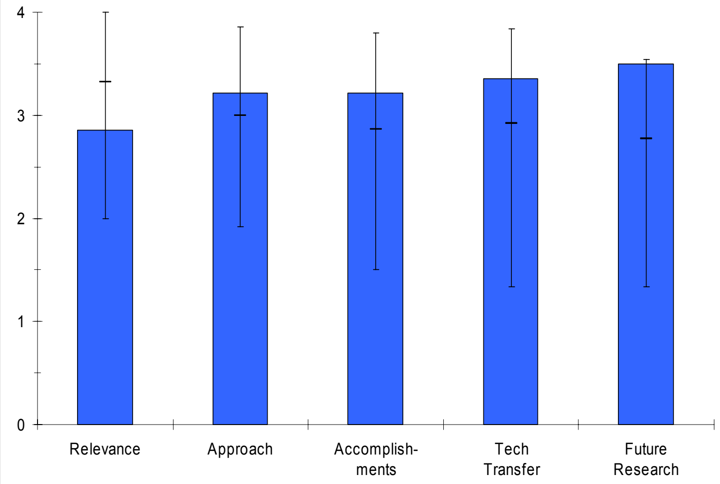
Project # FC-31: Back-up/Peak-Shaving Fuel Cells

Daniel Rodriquez; 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 is developing, building and testing three identical fuel cell back-up systems and is field testing them at three sites, including an industry host site to identify technical barriers. Other objectives include developing a cost-reduced polymer electrolyte membrane fuel cell stack tailored for hydrogen fuel; developing a modular, scalable power conditioning system tailored to market requirements; designing scaled-down, cost-reduced balance of plant; and certifying the design to Network Equipment Building Standards and Underwriters Laboratories.

Overall Project Score: 3.2 (7 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- The product is not directly related to vehicle programs but could lead to near-term high value products.
- High value products allow fuel cells to be deployed earlier than vehicle systems will accept the high cost of current fuel cell systems.
- While backup power/peak shaving applications could generate demand for fuel cell components (e.g., membranes), they would not have an appreciable affect on energy demand. Thus, the primary benefit of the program would seem to be stimulating the demand for fuel cell components, which might reduce the cost of envisioned energy intensive applications. To the extent that the market is very large for these applications, this may be of benefit to DOE. However, if the market is small, the benefit to DOE might be negligible.
- The project is not a R&D program. The project is a system integration program. Based on the presentation, the progress of the project is very good. It is a good stationary power project.
- The project aligns with the Hydrogen Fuel Initiative and MYPP.
- Focused on specific technical barriers facing stationary fuel cells: cost, durability, power electronics, start-up time and thermal and water management.
- The project is aligned with stationary power goals.
- Project looking for early market niche with better chance of hitting viable price point. Success of this project is useful, but it is not enabling for President's vision when technology breakthroughs are required in other parts of the program.

Question 2: Approach to performing the research and development

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

- This project is not fundamental research, but rather it is a product development task.
- The system is well-designed and the technical approach is excellent.
- The approach is conventional following a traditional business model of technology and system assessment, product design, validation, and field demonstration.

- Approach could have benefited from involvement of additional industry customers at the front end of the project to help scope system requirements and provide a broader customer commitment base.
- Interesting application incorporating ultracapacitors.
- Plug Power addressed technical barriers for the development of a backup power system, including design for passing testing requirements.
- The product development has necessary steps of requirements development, technology assessment, product design and fabrication, and product certification and validation. Appropriate collaborators and potential customers are involved.

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

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

- The actual certification testing of the system is interesting, but it would have little or no application to other designs since certification testing is unique to the particular design.
- It was not clear from the presentation where the final design is projected to perform against customer requirements. Thus, it is difficult to evaluate the success of this design. Future reports should clearly identify key requirements and projected (or actual) status against those requirements.
- It appears that a great deal of testing has been or will be accomplished to ensure the design will meet requirements, which is good. Unfortunately, the results of those tests were not communicated.
- The progress for the project is excellent; the system design and integration is also excellent.
- Plug Power indicated they met fuel cell system requirements, but Plug Power did not provide comprehensive information to support this claim.
- PI did indicate impressive accomplishments with regards to design validation, reliability and safety under very hazardous conditions such as fire, wind driven rain, firearms resistance, etc. to be in compliance with NEBS and UC.
- Cost is a major unknown.
- True test will be if product meets Bellsouth field demonstration and ANL baseline requirements.
- The project has made excellent progress in developing a commercial backup power PEMFC. Certification to NEBS standards is a big accomplishment.
- The product developed will be available ($14,995 each). Comprehensive commercial certification has been completed. Initial testing is positive. Products have been delivered to independent evaluators.
- Plug Power has made great accomplishments in terms of reduction in footprint and improvements to system components.

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

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

- It might be helpful to bring data from this system stack design into comparison with modeling work done at Argonne.
- This design could provide much real data for validation of modeling done at Argonne and elsewhere.
- The project is not an R&D program; the project is a system assembly and integration program. The collaboration with BellSouth, Argonne National Lab and FAA is excellent.
- Overall, collaboration with other organizations seems appropriate in scale, conduct, and variety.
- Collaborations with FAA and Bell are excellent. Collaboration for testing is good.
- Nice mix of customers, test sites, suppliers and test/certification organizations.

Question 5: Approach to and relevance of proposed future research

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

- Although this is not research, results of deployment of this high-value product might help to guide the work being done by Battelle to assess near term transition markets.

- Future product validation plans seem sufficient.
- Find a safe and reliable hydrogen source for the 5kW PEMFC system.
- Proposed future activities are self-evident via laboratory and field testing at BellSouth and ANL.
- No contingencies nor off ramps were presented should technology fail in final testing phases.
- Future research is for outside testing/validation of results. Project is almost complete.
- Project is nearly complete. Remaining activities are appropriate to project closeout. Customer validation that the product developed meets customer requirements remains to be completed.

Strengths and weaknesses

Strengths
- This high value product could help to pave the way for future lower cost fuel cell systems. The data could be used to validate modeling done elsewhere.
- Excellent system integration work.
- Logical, business engineering approach to product development.
- Technical accomplishments appear reasonable but are somewhat hard to gauge due to lack of presented data.
- Successful development project.
- Good systems level understanding.

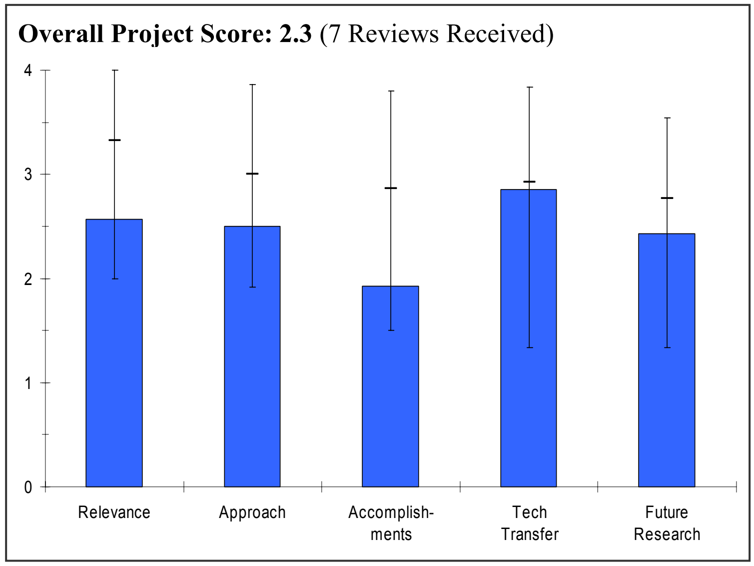
Weaknesses
- This is not a vehicle system and the work supports only one manufacturer.
- Where is the 99.99% hydrogen source?
- Broader collaboration with industry customers upfront would be beneficial.
- It is unclear whether unit with bottle storage will have a compelling advantage over peak-shaving/backup power alternatives. Business case was not developed in presentation.
- Lifetime of 1500hrs.

Specific recommendations and additions or deletions to the work scope

- Federal purchases of such equipment would allow better customer feedback if they were included in the program. Other federal purchases should be added, with other manufacturers and other products. The Battelle program may allow this to occur.
- Find a reliable and safe hydrogen source for the 5kW PEMFC system.
- Develop off ramps and contingencies for future work should testing results with BellSouth and ANL be unfavorable.
- Project is nearly complete; no modifications or extensions are recommended.

Project # FC-32: Market Opportunity Assessment for Direct Hydrogen PEM Fuel Cells in Transition Markets

Harry J. Stone; Battelle



Brief Summary of Project

Battelle Memorial Institute and its team are developing 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 transitional markets for direct hydrogen 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 **2.6** for its relevance to DOE objectives.

- The identification of transition markets is best conducted by fuel cell developers and their investors. The developers are currently actively engaged in working with fork lift companies, in developing UPS systems for telecom and other applications, supplying residential systems for demonstrations in Japan, and providing stacks and systems for bus demonstrations.
- The project assumes the transitional markets will assist in the development of a transportation fuel cell application. The project has not strongly made that case. It has not shown how the transition market will develop a supplier network or the infrastructure that will promote the transportation applications.
- It is hard to understand the relevance of the transitional markets, identified to the automotive markets – so specific examples were offered.
- Success in transitional markets will be critical to success in automotive applications. Thus, identification and support of these markets is of interest to DOE. However, the markets need to be large enough to facilitate bringing down fuel cell component cost. Thus, potential market size is an important factor to consider in this analysis.
- Near term market opportunities can provide income for stack, module, and system developers to sustain a business model prior to achieving the cost reductions necessary for widespread vehicle deployments.
- Getting a handle on the possibilities that might exist in the "early adopter" community will be of key importance in enabling the beginnings of a hydrogen infrastructure via stationary projects while work continues on meeting the road-going targets.
- The information is very relevant but the timing is a bit off. By focusing on markets in 2008 the information will be published too late for it to have much value for industry.

Question 2: Approach to performing the research and development

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

- Development of products usually entails discussions of proprietary information involving the application requirements and performance benefits that will differentiate the new system from the market place.

- Focus groups are not an effective format for these discussions.
- The approach tends to repeat the approaches that were used for other non-transportation fuel cells such as the phosphoric acid fuel cell, the alkaline fuel cell and the molten carbonate fuel cell. The project has identified a discriminator to distinguish its results from previous studies.
- The approach assumes that potential users of fuel cells know enough to provide credible information – this is not borne out by the H_2IQ project (Christy Cooper).
- The approach is reasonable.
- Should consider larger scale transitional markets (e.g., Transit), which could more quickly facilitate transition to light duty applications.
- Battelle should identify large PEM markets and ignore small PEM markets.
- This project is not a research product, but is rather a market assessment.
- Diversity of market research respondents is excellent, should provide key market driver data for DOE in evaluating near-term markets.
- The approach incorporates a number of analysis types in an effort to ensure commercialization opportunities are prioritized correctly.

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

This project was rated **1.9** based on accomplishments.

- The PI did not present any information on which to judge the status of the project. A change of direction has been implemented, but the criteria for power markets can be discussed in engineering terms that then translated into metrics for identifying the cost/performance that a new technology must meet or exceed. In assessing potential markets this understanding provides metric to establish the potential for value pricing. If the PI is thinking along these lines, his presentation did not reflect this.
- Project reported that it had completed an interim report that identified backup power applications, engineering cost models, cost drivers, life cycle cost analysis of direct hydrogen system and comparison with batteries / diesel generators. These were not reported in the review. Where are the results? The review identified future work not the results of past activities. The reviewers cannot assess the contribution unless reviewers willing to discuss results.
- Not a single one of the technical accomplishments listed was specifically discussed in the subsequent presentation. No results were presented or discussed at any length.
- It is not clear how the analysis will narrow down the primary transition market to 3. It seems from the schedule communicated that this should already be done, but the 3 primary markets were not presented, so presumably they have not been selected.
- Battelle has made a good start on an important evaluation.
- More specific highlights of interim report (available soon through DOE) would have helped in evaluation of "technical" progress.
- While there may be funding issues connected to part of the problem the results and work is moving too slow for the industry to realize maximum value. One suggestion would be to focus on 2012 instead of 2008 and buses need to be considered as part of the transition and not main stream transportation.

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

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

- By definition the project has a high degree of interaction with fuel cell developers and potential end-users.
- The project does not engage universities and laboratories.
- Battelle has produced limited publications for such a large effort. Little information was presented at the Program Review.
- There was no discussion of technology transfer or collaborations, but this measure is not really applicable to this project.
- The project is seeking input from both Federal and Private Sector customer classes. Battelle will also share its market assessment and its methodology.

- Partnerships are not formal and don't need to be; many companies have been involved in focus groups or interviews.
- There is good collaboration with a wide cross section of stakeholders.

Question 5: Approach to and relevance of proposed future research

This project was rated **2.4** for proposed future work.

- The identification of transition markets is best handled by fuel cell developers
- Project does not appear to have work plans but is responding to DOE directives.
- Future work described is reasonable but in the absence of any interim results, it is hard to say how valuable the results will be.
- Applications such as buses and scooters and fork-lift systems (and other non-car transportation systems) should be included.
- Scope was revised in February 2006 to prioritize transitional markets; fortunately the analytical market research framework makes the change relatively simple to incorporate.
- Efforts for FY07 are dependent on 2006 findings; there should be more detail and prioritization of how the potential $1.3 million left on the contract may be spent.
- Future work is mostly reactive and is guided by results and where EERE thinks things are headed. Battelle should be more directive in where the money should be spent to best accelerate or facilitate the industry penetration.

Strengths and weaknesses

Strengths

- Good effort to explore high-value near term markets.
- Very thorough analysis of potential markets via breadth of respondents involved in the project.
- Good approach to working with a broad array of stakeholders.

Weaknesses

- No results were presented even if they are internal results.
- Left out near-term high-value vehicle markets such as buses, scooters, fork-lifts, mine and construction vehicles, and similar applications.
- Future efforts that involve significant funding potential may not be specified in detail before budgets are approved.
- Not considering bus fleets as part of the transition is a mistake. They are controlled fleets and infrastructure is easier to handle.
- Study results for 2008 are too short-sighted and should be redirected to 2012. Industry has already done the 2008 analysis.

Specific recommendations and additions or deletions to the work scope

- I believe that DOE should question the allocation of valuable resources to market development activities when this is best handled by industry. The investment community and private industry has an internal reward systems that provides incentives for individuals in these companies to succeed.
- The responsible DOE / EE / HFCIT Technology Development managers should find ways to ensure that future presentations on this project are informative and meaningful.
- Add near-term vehicle markets such as buses, scooters, mine and construction vehicles, and similar applications.
- Market data on near-term transition markets should be made available as possible to industry to best leverage the government's substantial investment in this project.
- Consider adding some element of international market differences on a macro scale and only in a qualitative fashion. As an example, scooter markets are likely to take off in Asia and be a priority over there sooner than in America. Trying to get too detailed would be overly complicated but we should not ignore the international market drivers.

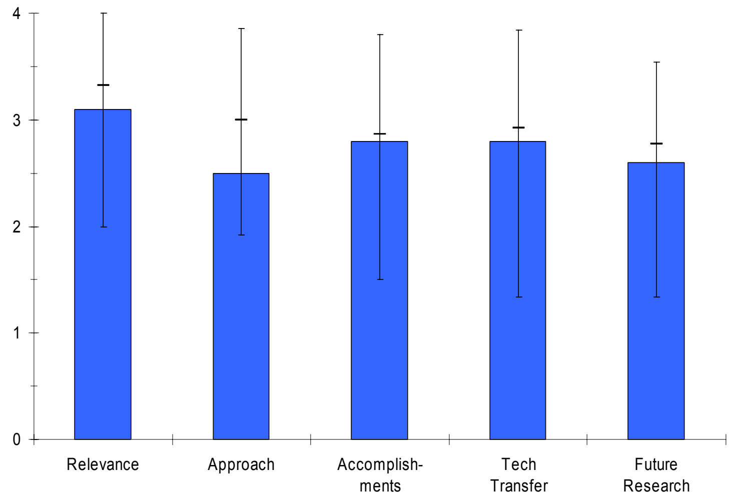
Project # FC-33: Scale-Up of Carbon/Carbon Bipolar Plates

David Haack; Porvair Corp.

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 a 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.

Overall Project Score: 2.8 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- Inexpensive and effective bipolar plates are critical for realizing RD&D objectives; it is just not evident that this project can attain the targets.
- The main factor in plate costs is increasing volume. This project is aimed at developing a high-volume process. This was a manufacturing project (prior to the manufacturing initiative).
- Bipolar plate cost reduction is quite important.
- Development of durable, lightweight, easily manufacturable, low cost bipolar plates is important enabling technology for fuel cell stacks. This project addresses these goals.
- The project is very relevant to and consistent with the President's Hydrogen Fuel Initiative goals and objectives.
- Hardware reliability/cost issues are of significant concerns.

Question 2: Approach to performing the research and development

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

- This approach primarily produces porous plates and has only a single primary customer. The technology is not yet ready for those companies that need sealed plates. The plates will undoubtedly be much too expensive to reach technical targets.
- Project focused on manufacturing of bipolar plates at high volume.
- Cost reduction: "Achieved or not" is not targeted.
- The PI is using a solid engineering approach to testing and statistical evaluation of results.
- The project is based on sound technical approach to address some technical vulnerability (H_2 permeation/sealing, hydrophobility/hydrophilicity) and cost reduction (mass-manufacturability) issues.

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

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

- Sealing progress is not sufficient as it is still a problem and there are some uncertainties with the sealing test procedure (i.e., if liquid is present as it appears, the applied pressure of 30 psig may not be high enough to

exceed the bubble point of small pinholes). In the plate forming process, there are still difficulties in achieving a full fill if the flow-field features are not within a certain size range.
- Good process control. Small variability in plate weight and dimensions demonstrated. Project has met most of the DOE targets for bipolar plates.
- Presenter did not show how the project targets have been met.
- Current status falls short of the 2010 and 2015 goals in all technical areas except one. Project is 90% complete so it is difficult to determine if further progress can be made in the time remaining or if a future project is planned. Having said this, it appears the project has made progress with the development of a material that has promise as a low cost bipolar plate, including materials development work to resolve problems with mold filling.
- Technical progress is quite significant thus far; technical milestones have been achieved in some respects.
- Manufacturing methodology, once fully developed, will greatly contribute to hardware cost-cutting benefits.

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

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

- There are interactions with UTC, but possibly only as an OEM materials supplier.
- Some coordination with UTC, including durability testing.
- Need to include additional partners in bipolar plate manufacturing, fuel cell stack development, and cost analysis.
- Collaboration is limited to one partner, but that partner is a fuel cell supplier who should be able to provide valuable feedback regarding progress and viability of the technology being developed.
- OEM partnership (e.g., UTC) is very pragmatic and technologically prudent.

Question 5: Approach to and relevance of proposed future research

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

- At this relatively late stage (90%) in a multi-year project, they are now proposing using thermoplastics instead of thermosets. Thermoplastics will introduce a whole new set of rheology (fill), shrinkage, and cycle-time issues (may be necessary to cool in the mold). Switching at this point is not likely to solve the underlying problems of cost and permeability.
- Future plans for higher throughput processing include using thermoplastic resins.
- Future plans address current shortcomings in the material development including critical needs such as finding a sealant that will withstand durability testing and improving process time to find a manufacturing process that could support high volume fuel cell production needs.
- The proposed future work is very consistent and well-coordinated with current progress. QA and FMEA initiatives are very timely and commendable.

Strengths and weaknesses

Strengths
- Have met most of the targets for bipolar plates.
- UTC as a partner will bring their long experience in bipolar plate making.
- Good engineering approach to solving the challenges of a bipolar plate done by a company that is familiar with manufacturing and working directly with a fuel cell stack manufacturer.
- OEM partnership with UTC and other fuel cell developers (not named) should be rewarding.
- Company commitment of its own resource (cost share) is BIG plus for the potential success.

Weaknesses
- Between cost and sealing issues, the sealed plates will not be capable of meeting technical targets. The porous plates may have a chance, but since they are of interest to only UTC, they would be of little benefit to the community as a whole.
- Did not show enough data on manufacturing process parameters to indicate the true potential for cost reduction.

- PI should be able to estimate and report the cost of the C/C bipolar plates at a volume level that could be agreed between DOE and the PI. While it is reasonable for the PI to state that the cost is volume dependent, this equivocation provides no information for comparison of this technology to other alternatives.
- Lack of collaboration with National Labs/university may cause some handicap in pursuing sophisticated technical measurements and understanding the fundamentals of the structural problem.

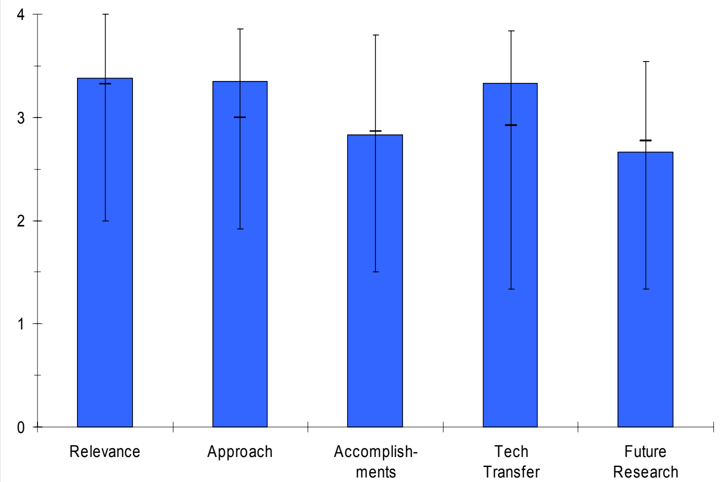
Specific recommendations and additions or deletions to the work scope

- The project is near completion, but it is strongly recommended that it is not extended in any capacity.
- Additional collaborative partners may help develop the concept to its full potential.
- Materials level durability, compatibility, degradation and poisoning possibility – these issues should be investigated early on (either in-house or with some collaborative partner).

Project # FC-34: Cost-Effective Surface Modification For Metallic Bipolar Plates

Peter Tortorelli; ORNL

Overall Project Score: 3.1 (6 Reviews Received)



Brief Summary of Project

Oak Ridge National Laboratory (ORNL) is developing nitridation surface treatments to protect metallic bipolar plates. The work is focused on formation of dense Cr-nitride surfaces on stainless steel alloys. The overall objective of this project is to demonstrate the potential for metallic bipolar plates to meet the 5000 h durability goals at a cost of <$6/kW.

Question 1: Relevance to overall DOE objectives

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

- The project is relevant to the goals and objectives of DOE.
- Metallic bipolar plates are an alternative to carbon composite bipolar plates and many automobile companies prefer metallic bipolar plates over carbon composite plates.
- Project is important to the success of the meeting the cost objectives of the program.
- Has good potential. Cost effectiveness is questionable. Could produce good durability if successful.
- Cost effective and durable/reliable bipolar plates are critical components for a fuel cell stack that is to be viable for transportation applications. Metallic plates offer the ability to be mass produced for low cost.
- Clearly, developing low-cost and stable bipolar plates is a critical component to meet the automotive goals and is a significant technical challenge. And, this addresses the key issue with metallic plates, namely, developing corrosion-resistant and low-contact-resistant materials with the potential for low-cost production. Nevertheless, it should be ensured that DOE also continue to support low-cost carbon-based bipolar plates, since it is not obvious (yet) which approach will ultimately be preferable.
- Lower cost bipolar plates are highly desirable.

Question 2: Approach to performing the research and development

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

- This approach is quite unique.
- This non-coating method could be the best solution to the surface defects caused by coatings.
- Approach is solid; significant emphasis on fundamental knowledge.
- Advancements have been made but on materials which may not be useful (Ni).
- Coating concepts and metal plates are important; more time should have been dedicated to the Fe system.
- Good attempts. Internal thermal stresses are a concern. Ni detected in MEA raises the question on the full blanketing effect of the technique.
- Project is well designed concentrating on low cost metallic plates with various coatings being evaluated for their physical properties and durability. Solid scientific and engineering approaches appear to be used to evaluate the results of testing and to develop alternatives when results are less than anticipated.
- Surface-conversion approach is good vs. coating.
- The proposed surface modification has an excellent potential to yield a low cost bipolar plate.

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

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

- Oxygen reduction problem could be very important for this technology.
- Internal nitridation is a key problem that needed to be resolved.
- There is no comparative data on coated metal plate conductivity with the conductivities of available commercial fuel cell plates.
- Progress has been good but the Fe system needs further development and evaluation.
- It is still not clear that the cost goals will be met based on the Fe system.
- Internal nitridation in the alloy can cause embrittlement. However, Vanadium addition improves the Cr- Nitride protective surface.
- When technical setbacks occurred, the PIs developed different coatings that can meet the necessary physical properties indicating improved durability. While progress has been a bit slower than could be wished, it appears that the PI has identified a combination of plate materials and alloys that are moving in the right direction of meeting the program's goals.
- Success with Fe-based materials definitely needs to be demonstrated to meet cost requirements. Good progress here, but PI still needs to show some in-stack durability of Fe-based materials.
- Use of Vanadium in low cost bipolar plates appears to be quite promising.

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

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

- Good collaboration team is in place.
- Not much collaborative work has been reported in this presentation.
- Appropriate level of exchange is in place.
- Project is collaborating with other National Labs, but more importantly with industry experts in the design and end use application of fuel cells.
- Excellent. Working with both end users (e.g., GM) and bipolar-plate manufacturer (e.g., GenCell).
- The stakeholders included have good fuel cell experience and commercialization capability.

Question 5: Approach to and relevance of proposed future research

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

- Significant amount of work has to be done in next few months as the project ends in Sept., 2006.
- The proposed work is important, but it may not be achievable due to the time constraint.
- Would continue to support additional coating research (Fe-based systems).
- Scale-up may be too premature.
- Good planning with good Go/No-Go decision.
- Future plans appear reasonable assuming that the project successfully clears the Go/No-Go checkpoint.
- Good plan, but can it all be completed by Sept. 2006? The Go/No-Go gate is a good idea. DOE should ensure the latest results obtained in this project be included in the determination if additional funding is granted in the latest solicitation.
- Long term cell and stack testing in needed to validate the technology benefits.

Strengths and weaknesses

Strengths
- Good approach. The surface defects, which are typically caused during the coating process, don't happen in this case.
- ORNL's very strong materials emphasis.
- Good concept. Could lead to good results.

- Good, solid engineering/scientific approach to the challenges at hand.
- Technical approach and industry collaborators.
- The protective nitride larger concept is very appealing. Analytical approach is very effective. Involving a manufacturer partner such as Dana helps to focus on manufacturing issues.

Weaknesses
- The technique is very new and there is not much literature precedence of this method.
- Very focused into few types of alloys. May need to do broader exploration of alloys that are compatible with the present coating procedure.
- Lack of cost effectiveness. Internal thermal stresses and deformation during the nitridation process.
- But a bit slow reaching the point where they are now. The costs reported do not include the cost of nitriding which does not appear to be fully understood. Until the total cost of the plates with nitriding can be determined and included in the total cost, it may be premature to declare that the FreedomCAR cost goals can be met.
- Potential environmental or recycling issues associated with Cr and/or nitrides? Will these Cr-based metals become a "material of concern"? Lack of long-term stability (e.g., 5,000 hrs) testing in stacks or in appropriate environments (e.g., H_2 and air in the presence of water/acid).
- Pre-test and post-test characterization of the coating after a long term cell test (>2000-5000 h) was not shown.

Specific recommendations and additions or deletions to the work scope

- Due to the time limitations, work should be focused onto the validation of the proof of concept of this coating technique to low-cost alloy material.
- Develop techniques and minimize the effect of thermal stresses and mismatch in thermal expansion coefficients between the Ni-Cr layer and the bulk of the base alloy. Consider more accurate cost evaluation methods and cost effectiveness.
- Recommend that the PI find a way to understand and estimate the total costs.
- Focus on testing the bipolar plates in long term cell and stack to further validate the potential benefits.

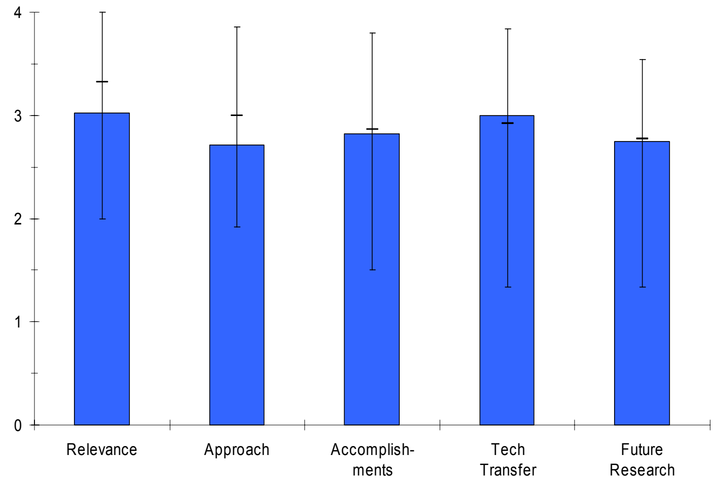
Project # FC-35: Platinum Recycling Technology Development

Stephen Grot; Ion Power, Inc.

Brief Summary of Project

This Ion Power, Inc. project will assist the DOE in demonstrating a cost effective and environmentally friendly platinum group metal (PGM) recovery and re-use technology for PGM-containing materials used in fuel cell systems. The objectives are aimed at recovering not only the PGM materials but also the membrane material in a usable form. Specific developments include processes for solubilizing catalyst-coated membranes, processes for the separation of catalyst and ionomer materials, test methods to determine vitality of the recovered materials, and partnering with the key stakeholders in this technology area.

Overall Project Score: 2.8 (8 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Long term, recycling of Pt from stacks will be critical to Pt supply.
- Platinum recycling will be very important in maintaining a viable supply of fuel cell catalyst precious metals required for full scale fuel cell commercialization.
- Recycling of platinum is critical to achieving the hydrogen vision, and directly addresses the RD&D plan objectives.
- Pt recycling is important to life cycle cost of fuel cell development.
- Ion Power has a realistic approach to recycling platinum and Nafion, which will be critical to the cost and environmental friendliness of fuel cells.
- Membrane recycling is also important however the relevance of specific research tasks to overall objectives is difficult to determine.
- The project is highly relevant to DOE's Hydrogen Program for recycling Pt from MEAs.

Question 2: Approach to performing the research and development

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

- Approach builds on Principal Investigator's knowledge of materials.
- Inclusion of impurity identification is important to materials processing.
- DMFC membranes will not represent large volume of material in the future, and uncertain why they are included.
- Rather than investing in fuel cell materials performance characterization, additional investment might be made in better understanding the recycling processes.
- More effort needs to be applied to the core of the program: recovering precious metals, ionomer and then demonstrating their vitality.
- *In situ* membrane purification is interesting, but it is unclear if it is proposed for commercialization – very sound approach for Pt recycling.
- A convincing case for the value of recycling the membrane polymer.

- Unclear number of hours of service for stacks. Aging mechanisms and recycling challenges may differ depending on the hours of service. No mechanical characterization of the recovered Nafion, which is important to ensure that it is reusable. The real recycling challenge will be stacks from vehicle usage, based on future volumes. Ultimate durability targets of 5500 hrs means that current stacks used in this project may not be realistic.
- Good understanding of fundamental issues causing chemical degradation of Nafion membranes, as applied to recycling concepts. Very good analytical chemistry. Innovative way to restore Nafion. Good economic analysis.

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

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

- Results focused on analytical methods and fuel cell characterization of materials rather than recycling processes.
- Not clear on progress related to recycling process.
- Energy analysis shown but focus on cost. Cost analysis of the project appears incomplete.
- Interesting diagnostics and analyses of end of life membrane materials that might prove useful to other DOE fuel cell durability programs.
- The project has achieved success in clearly demonstrating the viability of platinum recovery.
- Good progress in recovery of membrane ionomers.
- The "cost" model of process energy analysis seems inadequate. Environmental costs of solvents and other waste streams may not be captured. Although a postive business case is presented, the current analysis should be regarded as an estimate.
- Performance test did not seem compatible with main project objectives.
- Unclear as to how much of the platinum as a percentage was recovered by the extra techniques used on the membrane.
- Based on the presentation, some solvents were used to separate catalysts and membrane electrolyte. Unclear if the recycling membranes still have the same prosperity as fresh membranes.

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

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

- Industrially focused project. Some involvement of university to characterize materials and fuel cell performance might free up PI to focus on recycling process.
- Partner with a catalyst company to evaluate and demonstrate the viability of the recovered platinum for remanufactured electrodes.
- Very good mix of collaborators, including major MEA suppliers and stack developers.
- Good interactions with suppliers but need to get hours of use on the stacks.
- Not much evidence of effective transfer yet.
- Working with a lot of OEMs.
- Good collaboration.
- The collaboration with partners is good.

Question 5: Approach to and relevance of proposed future research

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

- Mechanical processing of membranes not discussed thoroughly; many options exist for shredding MEAs that can take fair amount of resources.
- PI did not discuss how they would minimize materials that do not contain Pt.
- Economic analysis should use future target component costs as large scale fuel cell commercialization would not occur at today's prices.

- Future plans to focus on scale up processing and to further refine the cost models are on target.
- Careful economic analysis is vital to ensure that the recycling methods developed are the low-cost choices.
- Seems appropriate especially including other membrane materials and test of a re-manufactured stack.
- Future plans are unclear.
- Branching out to other membrane manufacturers beyond Nafion is a good next step.
- Based on the presentation, the future research approaches are fair. Recycling process steps should be better described.

Strengths and weaknesses

Strengths
- Interactions with stack suppliers.
- Good combination of synthetic, analytical, and fuel cell chemistry combined with working on practical problems.
- Detailed analysis work accomplished on the different membranes.
- Showed the value of the ionomer as being potentially as valuable as the platinum.
- Investigators have great deal of expertise in understanding Nafion's chemical and physical properties.

Weaknesses
- Value proposition seems to be tied to the assumption that Nafion would remain at current cost even if mass produced.
- A small weakness is that they are focusing on Pt and Nafion, and not fully addressing alloys (PtCo) and hydrocarbon membranes (note that future plans include seeking out manufacturers of non-Nafion membranes, so this may be fixed).
- Presentation could be better organized. The procedure for recycling Pt from MEA should be the focus.

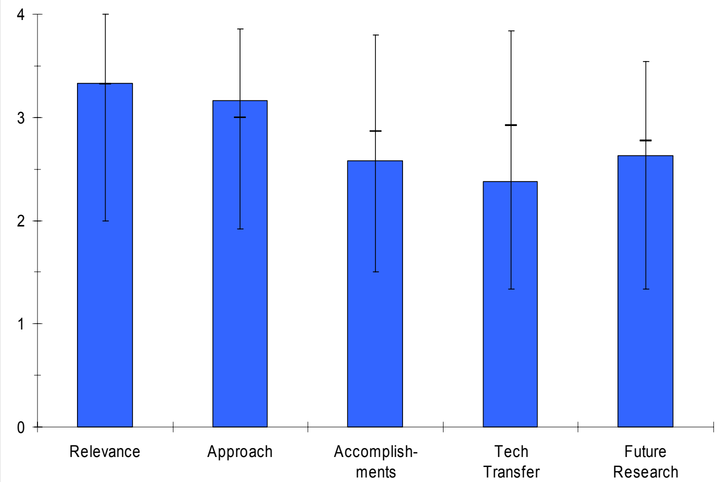
Specific recommendations and additions or deletions to the work scope

- Provide more explanation on how the recovered Nafion (or other material) would be recycled and at what cost.
- Quantifying the specific life of each membrane in hours to note the differences in chemical properties and contaminants over a time scale and for different applications would help to understand changes over time.

Project # FC-36: Platinum Group Metal Recycling Technology Development

Larry Shore; Engelhard

Overall Project Score: 2.8 (6 Reviews Received)



Brief Summary of Project

This Engelhard Corp. project is examining 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 Pt and 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 PM recovery from metal monoliths, which are used in stationary reformers and potentially in hydrogen production, is also being evaluated.

Question 1: Relevance to overall DOE objectives

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

- Working on an approach to recycle Nafion and Platinum with concern for environmental impact.
- The project is highly relevant to the DOE's Hydrogen Program.
- One of the critical elements in long-term cost of Pt with large volume production of PEMFCs is the cost of recycling the Pt. Critical to understand the chemistry and byproducts (and environmental impacts) not just the yields.
- Platinum recycling is critical to future markets.

Question 2: Approach to performing the research and development

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

- How the individual tasks fit into an overall manufacturing process was not clearly defined.
- Initial economic analysis not apparent to determine if the selected approaches had the potential to be cost effective.
- Unclear who process economics were on 100 kg of MEA. Unclear how many stacks the MEA units represent.
- Innovation not apparent.
- Based on the presentation information, the technical approaches are excellent.
- Approach is not very well defined at least in the presentation. A lot detail is given about some aspects of alternative paths, but a clear picture of the pluses and minuses of alternatives was not presented.
- Solid path to environmentally friendly process for recovery of PGM.

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

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

- Unclear which approach was dropped and whether the microwave assisted process and the mechanical preparation of the samples would handle existing and contemplated MEA configurations.
- An overview of the future process and how these developments would fit in would have helped to provide a sense of progress and what challenges remain.

- Some progress but appears to be behind schedule based on money and time spent.
- The technical accomplishments are excellent.
- Status is unclear.

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

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

- Industrially focused project. Some involvement of university to develop adsorbent.
- Working with OEMs, but not providing public information.
- Should increase collaboration with MEA production companies (such as DuPont) and PEMFC manufacturing companies.
- Unclear as to exact roll of collaborators outside of MEA donors and one testing company.

Question 5: Approach to and relevance of proposed future research

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

- Mechanical processing of membranes not discussed thoroughly, many options exist for shredding MEAs and can take fair amount of resources.
- Lack discussion of how to minimize materials that do not contain Pt to increase concentrations.
- Some future work seems to be work that should have been done by now, or is not likely to be completed by dates indicated.
- It seems premature to be certifying recovery processes before MEAs are fully commercialized; the certification would have to be redone if the MEAs change. It is not clear if there are enough fuel cells made now to make certification important.
- The future research plans are excellent.
- Very well planned future work with Go/No-Go criteria established.

Strengths and weaknesses

Strengths
- Leveraging commercial processes.
- The technical approaches are well-organized and stated in the presentation file.
- Domestic producer and refiner of Pt group metals is the prime.
- Good progress on a process to recover a high percentage of PGM.

Weaknesses
- Progress seems slow.
- Innovation unclear.
- Increase collaboration activities to possibly use some reclaimed materials in new MEAs and do some preliminary testing and comparisons to new MEAs.

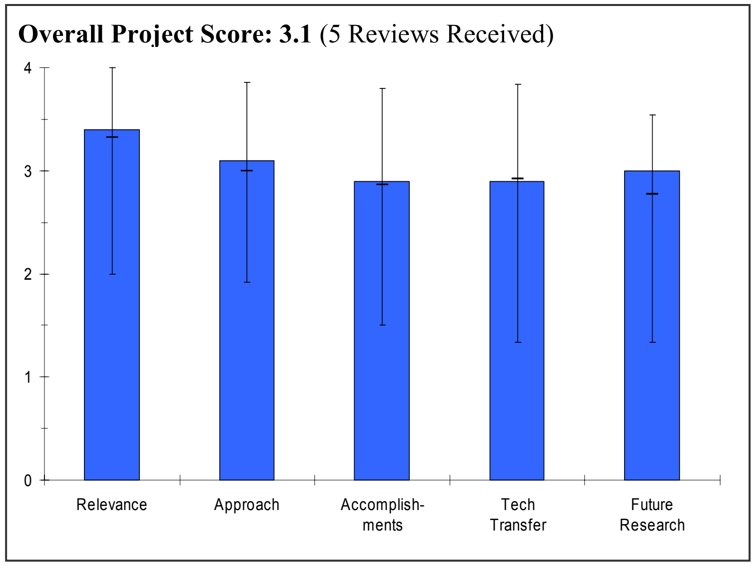
Specific recommendations and additions or deletions to the work scope

- The economic value of the MEA will provide the industry the incentives to optimize recovery technologies
- Cost analysis needs to be presented (one presumes it is in the Project Scope).

Project # FCP-08: High Temperature Membrane

John Kerr; LBNL

Brief Summary of Project



Lawrence Berkeley National Laboratory (LBNL) is investigating the feasibility of use of solid polyelectrolytes for high temperature operation that do not require the presence of water for proton conduction. Nafion and polyether polyelectrolytes doped with imidazoles are being prepared and their conductivities and mechanical/thermal properties measured. Imidazoles are covalently attached to side chains of ionomers with appropriate polymer backbones. Simplification of the overall fuel cell system is particularly important for transportation and mobile applications.

Question 1: Relevance to overall DOE objectives

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

- High temperature, non-water dependent membranes correlate well with the DOE objectives.
- Materials work to reduce fuel cell manufacturing costs is important.
- Project well-aligned with commercial issues, i.e. membrane operation with low/reduced water content (high temperature and low temperature membrane operation).
- Membranes for operation under dry conditions are critical for the commercialization of fuel cells in vehicles.

Question 2: Approach to performing the research and development

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

- PI is aware of past work using imidazole-type materials for fuel cells and understands their shortcomings. Membrane fabrication was well thought out using information from the literature.
- The approach seems to focus primarily on durability.
- Good concept and prototype design for evaluating the proposed polymer modification.
- Need to expand the scope to include activities that address/resolve likely chemical durability issues with the prototype. For example, additional review/experiments with fluorinated analogs as proposed by the PI would be appropriate for the next stage of this project.
- This is a totally novel approach, one of the few that are truly out of the box and although it is high risk, a clear path exists to solving the problem. This is outstanding research by a very competent researcher.

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

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

- Membrane characterization is very high showing high proton conduction and good mechanical properties. However, the PI is aware of the major shortcoming of this material (imidazole poisons Pt surface) but has not addressed this issue.
- While a good deal of work has been done, it is difficult to clearly see progress towards DOE goals.
- Project interruption for 1.5 years has slowed originally planned progress.
- The PI validated the concept design and documented the prototype demonstration very well.

- Need to solicit external resources (polymer manufacturers, fabricators, etc.) to identify and prioritize potential barriers to commercialization, ways to reformulate the concept with commercial materials, etc., and address those issues in the next stage of the project.
- The ionomer architecture has been worked out with few resources. A truly impressive accomplishment.

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

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

- Collaboration is with NASA and "High Temperature Membrane Working Group." Level of interaction is very low.
- Collaboration appears limited.
- Project has excellent interaction with the other National Labs, utilizing needed expertise and analysis capabilities.
- PI indicated the need to interact with polymer manufacturers as a next step to address material selection and support demonstration activities. This will be important for continued development of this concept.
- Good, more collaborators than listed on slides.

Question 5: Approach to and relevance of proposed future research

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

- Preparation and testing of MEAs is critical to assess the real value of this research – it should be a milestone with a metric, both for performance over the relevant temperature range, and for durability against some of the known failure modes of PEM.
- Membrane work has shown promising results; however, until mitigation strategies to prevent Pt poisoning are clearly stated and tried, this project is simply an intellectual exercise.
- Future research should more clearly address barriers.
- Project well-aligned with other National Lab activities.
- Excellent. A clear path towards success exists.

Strengths and weaknesses

Strengths
- Good fundamental research into non-aqueous ion transport mechanisms.
- Imidazole-type membrane materials have been clearly shown by the PI and in previous literature that proton conduction at high temperatures without water is at an acceptable level. This type of material has the potential to be the "Holy Grail" for PEMFCs.
- This project has "synergy" with other projects (e.g., FC-25). The "concept" is an "engineering solution" which has merit and potential.
- Novel out of the box approach, concept totally validated.

Weaknesses
- The *ex situ* approach to developing these materials should be augmented by *in situ* evaluation of promising technologies. To delay *in situ* work until the latter part of the project incurs unreasonable risk.
- Without validation that these type of materials can be designed in a way to prevent Pt poisoning in a fuel cell, it is unclear what new concept the PI is bringing to the table. Conjecture and hand waving is not acceptable.
- Need to address potential "de-railers" as soon as possible.
- Resolve possible chemical stability of imidazoles and backbone polymer as currently defined by the initial concept.
- Identify partners (other resources) to evaluate the prototype concept versus commercial materials and manufacturing routes.
- Not enough resources for project.

Specific recommendations and additions or deletions to the work scope

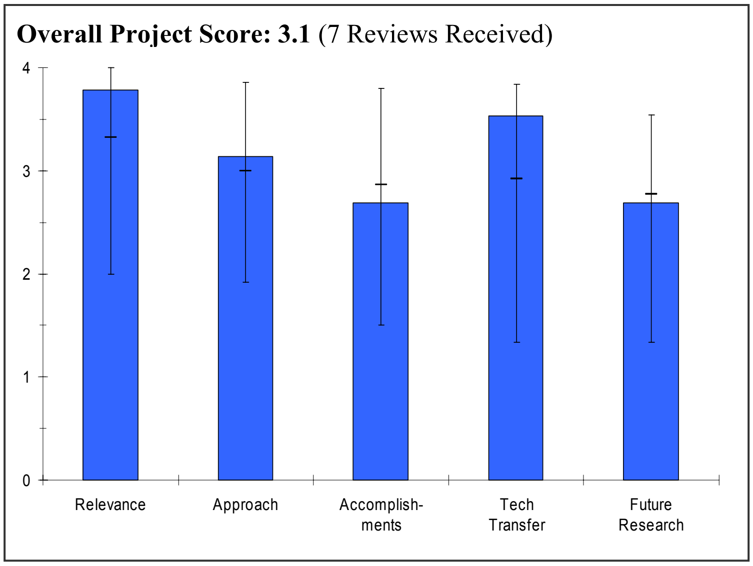
- Add a milestone for *in situ* evaluation of candidate membranes, with metrics for conductivity over a temperature range and sensitivity to known failure modes of PEMs.
- Funding for this project as stated by the PI is low and MEA work is not currently being investigated. For this to be a worthwhile program, the DOE has to either increase funding to allow the PI to pursue MEA work (validate concepts to mitigate Pt poisoning) or kill the project. Doing it halfway under the current conditions will result in useless findings (imidazole is known to work already).
- Keep project focused on addressing barriers and goals.
- Consider collaborating with an industry partner.
- This project should be combined with other National Lab programs – to better expedite the development and provide more focused activities with industry partners for commercialization of the concept. Need to create a project with "critical mass", including better utilization of resources (people and funding).

Project # FCP-09: Component Benchmarking

Tommy Rockward; LANL

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.

- Establishing standard test protocols is key to the adoption of technology. In this case, it is a useful exercise, but it is not clear if this particular test protocol is one that will be widely adopted.
- This project appears to be a "coordinating"/oversight program for all the other component analysis and characterization efforts.
- As such, this effort is clearly important and relevant to DOE's plan objectives.
- This is work that needs to be done; it is not flashy but enabling if fuel cells are to be widespread.
- The project addresses the important need for developing and qualifying test protocols that will allow valid comparison of fuel cell data.
- The project does not directly address the key barriers (cost, durability, electrode performance), but will indirectly foster development work in these areas by providing testing guidelines.
- The project will focus and streamline the development efforts of component manufacturers as they will not get sidetracked by issues related to MEA assembly (i.e. sealing, GDL bonding).
- This work will be useful in achieving the DOE goals by allowing more uniform comparison of fuel cell sub-components.

Question 2: Approach to performing the research and development

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

- It would be helpful to establish ways to modify the protocol as needed for different materials.
- The approach is vital to sharing and coordinating all the related DOE programs on component analyses. However, the simplification of certain procedures (e.g., single cell tests, gross crossover leaks, simplified conditioning, etc.) is disturbing in that more complicated problems may be missed. For example, LANL's MEA fabrication approach may work for Nafion-like membrane systems; but it would be inappropriate for non-perfluorinated systems and hence, an "apples-to-apples" comparison between different membrane families would not be possible.
- Straightforward approach to benchmarking. This is almost a National Bureau of Standards approach for fuel cells.
- The barriers (and resolutions) regarding the single cell test protocol need to be clearly identified and presented with more specific details – at least the "critical few".

- The rational behind specific test procedures should be explained more precisely (e.g., break-in period) and also the way in which a specific test protocol does affect or stress the individual MEA component.
- Guidelines on the data processing, e.g. extracting internal resistances from IV-curves, calculating Tafel slopes) and on the assessment of the MEA components should be given, too.
- For components (membrane, catalyst, or GDL) evaluation, consider developing MEA fabrication capability and prepare baseline materials.
- Develop electrochemical analysis, e.g. EIS, CV etc.
- The PI proposes a reasonable way to address a difficult question of identifying testing protocols for benchmarking fuel cell sub-components which will be relevant to numerous users.

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

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

- The team has made good progress towards the second round of testing. Still waiting for publication of the results.
- Technical accomplishments and progress in this project are hard to gauge given the "coordinating" nature of this program; but the poster and researcher did not make clear the compromises and adjustments needed to deliver on the program.
- The testing protocols for break-in, etc. are overly simplified.
- A reasonable level of activity shown – the PIs could be more aggressive and have more impact.
- The technical accomplishments were difficult to evaluate, since many of the project's collaboration activities remain "confidential."
- While the program with the USFCC (single cell test protocol) was not confidential and LANL's valuable contributions are generally known, the presentation did not provide adequate specifics regarding the accomplishments.
- It was clear that the "timing" (and completion) of activities is not totally under the project's control – and the most likely the reason for slow progress.
- The achievements are highlighted in a very general manner. A summarized feedback from the partners/collaborators on the test protocols should have been included.
- The PI has made progress in developing good testing protocols.

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

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

- Good interactions with USFCC and academic/industrial partners.
- This effort is *de facto* collaborative; so despite the difficulties, the efforts are well-done.
- Good TT/Collaborations but more could be done.
- Extensive collaboration and support for a variety of groups and companies.
- This project involves a variety of fuel cell partners with obviously close interaction.
- More industrial partners would certainly increase the value and the acceptance of the project. It is, however, recognized that industrial support for standard tests is often difficult to obtain.
- Repeatability and reproducibility of testing stations should be evaluated.

Question 5: Approach to and relevance of proposed future research

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

- Stronger leadership for the establishment of standardized testing is needed. This is only one part of fuel cell performance/durability, materials characterization, and component testing.
- More details explaining specific coordinating programs are definitely needed, e.g., procedures which clarify the performance of membrane materials versus electrode formulations, etc.
- This was a bit weak. The project appears to be passive. More aggressive participation recommended.

- Project is generally providing a "service" to others, and future activity was not well defined – since it seemed to be very dependent on the "immediate" needs of the listed collaborators/partners.
- Future plans only briefly described, appear sensible.
- Emphasize accelerated/selective durability test methods to save testing time and help identifying the "weakest link" in the MEA.
- Developing a durability protocol that will be relevant and accepted broadly by fuel cell researchers in many application areas will be difficult.

Strengths and weaknesses

Strengths
- LANL has leveraged its position as a leader in fuel cell testing to bring about this collaborative effort.
- A good first start at data-gathering and establishing "uniform" procedures for basic characterizations and collaborations.
- Important activity, high capabilities, impartial lab.
- The PI is able to utilize many LANL resources, ranging from fuel cell hardware and instrumentation, to sample preparation, evaluation protocols, and personnel needed to define protocols and evaluate the results. These resources were presented in good detail – and the listing (and discussion during the presentation) reinforced their value to the fuel cell industry and others (universities, organizations, etc.).
- The project clearly addresses a key issue!
- Many partners involved, crucial for widespread acceptance.
- Provides guidelines to inexperienced fuel cell developers and allows quick positioning against state-of-the-art technology.
- MEA testing capability.
- Many collaborations with universities, National Labs, and industry.

Weaknesses
- This establishes a testing protocol for one particular set of conditions. More work is needed to establish standardized measurement techniques across the full spectrum of fuel cell technologies.
- More details re: future standardizations would help the presentation.
- Needs to more aggressively look for business.
- The PI needs to provide more specific comments on: sources of "site-to-site" error when using the test protocol; protocol remedies to reduce errors, etc.; and protocol and/or hardware limitations.
- These accomplishments were "inferred", but there were no specifics shown in the presentation.
- Guidelines on the processing and interpretation of the obtained data should be given.
- The project lacks some additional analytical standard testing methods (CV, EIS).
- There should be a plan for releasing a proposed testing protocol for public evaluation and input.

Specific recommendations and additions or deletions to the work scope

- It is not clear that DOE funds should be used for a larger effort, but stronger leadership for standardized testing protocols is needed.
- This project needs more funding and a further in-depth series of more uniform characterization programs.
- Increase overall energy within project and keep going!
- It could turn out useful to split the test protocol up into mandatory tests and those which can be done in addition.
- Sensitivity test (temperature, relative humidity, pressure, etc.) covering a wide operating condition range could be included (possibly through a design of experiment approach). System developers could largely profit from this.

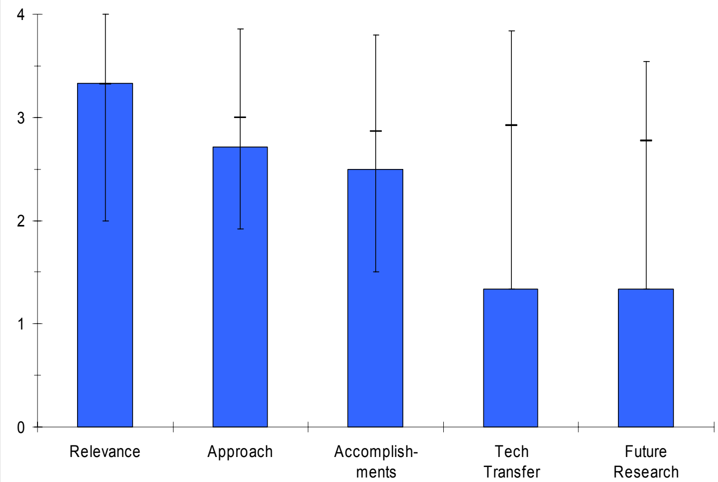
Project # FCP-13: Montana PEM Membrane Degradation Study

Lee Spangler; Montana State

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: 2.4 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- The relevance to DOE objectives is clearly stated and appropriate.
- However, the focus appears to be one emphasizing development of analytical tools for membrane analysis over truly understanding fundamental degradation mechanisms.
- The project sought to address membrane durability, which is one of the subject areas most in need of detailed attention.
- Parts of the work were probably seeing changes in the electrode layers which may have been misinterpreted as changes in the membrane (see below).
- The ostensible reason for the research is sound: the performance of fuel cells degrade over time and analysis of why can lead to longer lasting components. However, relevance is limited by experimental design flaws detailed in (2).
- Understanding of membrane degradation mechanisms is critical. Focus seems to be on DMFCs.
- The approach of database generation in real time describing fuel cell performance is very good. Correlation with durability is essential. Interesting tools proposed for characterization.
- *In situ* membrane degradation studies are an important topic for the Presidents H_2 Initiative.

Question 2: Approach to performing the research and development

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

- The technical methods and analyses are well presented and discussed.
- The emphasis, however, was more on analytical methods and tools, systems, and equipment; so the relevance of such an approach to fundamental understanding of membrane degradation mechanisms are a stretch, e.g., the linear resistive model is impressive in the equipment development to get at it; but what do voltage decrease and resistance increase tell you about where in the MEA the problems are?
- Local densification and variations have been uncovered by other researchers, without recourse to the X-ray analyses and the NMR studies.
- The project was intended to address the mechanisms of membrane degradation, but the in the electrical analysis the whole MEA was treated like a black box, not taking advantage of preexisting knowledge of how an MEA works.
- The linear resistance model used was overly simplistic. It is well known that the properties of an MEA are best modeled by a combination of kinetics, ohmic losses, and mass transport.

- The X-ray work was interpreted in terms of changes in the membrane before and after it was used, no? If so, wouldn't the high-Z Pt dominate the X-ray scattering, so that the observed changes would most likely be in the electrode layer rather than the membrane?
- The experimental design did not fully address the objective: "The overall objective is to determine membrane degradation mechanisms and how to prevent or mitigate them." The experiment reported a physical degradation mechanism (localized membrane densification), but no chemical degradation mechanisms were addressed (it is not clear how X-ray imaging was supposed to detect changes to the chemical structure of the membrane). Preventing degradation (an objective) involves not just load regulation but chemical engineering of the membrane. Objective "Investigate the potential of advanced control systems to prevent degradation problems" not addressed by experiment. Parallel/Serial interconnection method was acknowledged as poor experimental design: "The degradation of the cartridge modifies the extent to which the cartridge participates in the system and changes the excitation." "The amount and complexity of the data has been a challenge. Tens of thousands of data files per membrane exist." This was a problem of the PI's own making ... for what reason was all this data taken? What was the experimental plan and what were the results? Most significantly, what were the independent/dependent variables or the correlations between the measurables that this project was intended to study? After quantifying electrical behavior of cell with two parameters (Slide 12 – $\mu 1$ and $\mu 2$, being Thevenin voltage and Thevenin resistance), changes to these measureables were not reported as functions of anything, or even correlated with density variation data (Slide 28) or other measureables, such as changes in cell response time (e.g., Slides 11 and 23). Slide 25 hypothesizes a cause/effect (change in a time constant as function of "hydration/drying cycles"), but what excluded other possible causes such as: total operating hours, load, load oscillations/duty cycle, etc. (the presentation implies that the load is a cause, but provides no data)? Slide 38 shows "T2 maps of Nafion 117 Heterogeneity," but such data not provided as a function of other measureables; how does T2 change after membrane has been used in a cell with a specific history?
- *In situ* diagnostics are being used to elucidate degradation mechanisms. Attempting to correlate failure modes to fuel cell materials/physical aspects.
- A massive amount of data was obtained; PIs overwhelmed by these data and did not fully make use of all of it; characterization techniques are fine.
- The *in situ* SAXS experiments outstanding. However it is not clear how the NMR imaging fits in and why the use of methanol would be relevant to an hydrogen fuel cell.

<u>Question 3: Technical accomplishments and progress toward project and DOE goals</u>

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

- The technical approaches are an overkill for the conclusions arrived at.
- However, the methods development and the analytical techniques were well-discussed and the conclusions reached are on the mark; but the more detailed challenges re: membranes versus electrode layer problems were not resolved.
- There is clearly progress in the analytical developments.
- Use of a more sophisticated model would have increased the value derived from all of the detailed electrical measurements.
- If the X-ray measurements actually measure properties only of the membrane rather than of the entire MEA, then the way this was done should have been better clarified on the poster.
- It would be good to clarify why the T2 maps of the cutouts show variability in the Nafion 117 rather than possible problems with reproducibility of the results. Were similar images taken from a material known to be uniform and showed no such variability?
- Can the authors provide any physical interpretation of the two time constants?
- Work resulted in hypothesis: "Improved density uniformity may extend PEM fuel cell performance." However, this hypothesis was not tested.
- Some correlations have been established, e.g.., membrane density uniformity and cell performance.
- Modest progress; densification during use is really the only conclusion after a great deal of work.
- *In situ* SAXS measurements are a significant accomplishment.

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

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

- There is NO clear evidence of external collaborations; nor did I see cross-references to the degradation studies conducted by other DOE program participants, such as 3M or DuPont.
- The project would have benefited from detailed discussions with people more experienced with fuel cells (e.g., in AC impedance of fuel cells) at its early stages.
- There was little or no evidence of collaborative work between the university and fuel-cell companies or DOE labs.
- No collaborations were reported although a collaboration was said to be imminent.
- Given the problems in data handling, they could have brought in experts. Additional help in characterization could have been imported.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.3** for proposed future work.

- Development of the electrophoretic magnetic resonance probe could prove to be useful.
- No future work was presented.
- Disappointing. Proposed is data mining that should have been included in the current project.

Strengths and weaknesses

Strengths
- The equipment and methods developments are well-executed and explained.
- Instrumentation.
- This is a university-based project that exposed students to fuel-cell engineering issues. The project provided student experience in the mechanics of laboratory instrumentation (e.g., labview).
- The approach – online, massive databank, correlation of membrane properties with cell performance is solid. Good test configuration established.
- *In situ* SAXS.

Weaknesses
- The sophistication of the equipment and analytical techniques employed has led to fairly pedestrian conclusions re: densification and membrane inhomegeneity.
- There is no evidence of further in-depth analysis of membrane degradation beyond phenomenological explanations. Also, future plans are not articulated in the poster presentation.
- Insufficient incorporation of knowledge from the established literature as an aid to data analysis and interpretation.
- The experimental design was not responsive to the stated objectives of the project. This four-year project is concluding with little or no useful information gained that would aid in commercialization of fuel cells.
- Should establish collaborations with others doing similar diagnostic work.
- Relatively little delivered in this project. All one has is that cells densify with declining performance. This probably would have been determined with a single test. Not much further upside suggested.
- Need NMR imaging and PFGSE of water swelled and dry membrane before and after durability testing. No collaboration or technology transfer other than by publications.

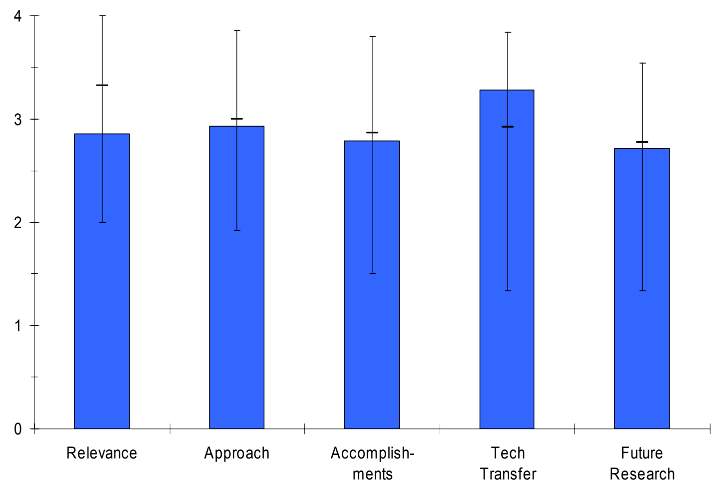
Specific recommendations and additions or deletions to the work scope

- If there is a follow-up program, use the methods developed to probe into more fundamental degradation mechanisms.
- Collaborate with the other research groups that are working on degradation studies.
- Need close collaboration with (a) fuel cell developer(s) so that all the work done here on instrumentation could lead to more solid physical interpretations of degradation mechanisms.
- Project concludes 9/2006. No additions or deletions are recommended.
- Not recommended for future support. Significantly different characterization is needed in the future as the current approach is not answering the questions posed.

Project # FCP-20: Residential Fuel Cell Demonstration by the Delaware County Electric Cooperative, Inc.

Mark Schneider; Del. Co. Electric Co-op

Overall Project Score: 2.9 (7 Reviews Received)



Brief Summary of Project

Delaware County Electric Cooperative, Inc. (DCEC) is validating the 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, DCEC is measuring and reporting technical performance, providing raw cost data and economic viability analysis, documenting maintenance and operations concept enhancements specific to residential fuel cells, sharing safety related vulnerability analysis and lessons learned, and promoting education of state and local consumers.

Question 1: Relevance to overall DOE objectives

This project earned a score of **2.9** for its relevance to DOE objectives.

- The project is not an R&D project. It is more likely demonstration program.
- Demonstration projects are beneficial for the development of alternative power sources. The program demonstrated the importance of establishing the infrastructure to supply controlled fuel products to a PEM fuel cell. The program partially supports the President's hydrogen vision in that it identified problems in the delivery network and limitations of the "commercially" available PEM fuel cells. The project did not identify a process for advancing the state -of-the-art of PEM fuel cells. The project tests to determine if PEM fuel cells are ready for commercialization and mass distribution.
- This project provides demonstration experience in a real environment. The project provided the utility co-op and the fuel cell developer with a new understanding of failure modes and product design requirements.
- The co-op also assessed the economics of this particular site, thus providing information on potential early adopter markets.
- This project provides an excellent opportunity to educate public on fuel cell technologies.
- The project fits well within the goals of the hydrogen vision and RD&D plan, maybe better classified as a technology validation or education project.
- Field testing and demonstrations of prototype and near commercial fuel cell systems is essential to moving this technology forward.
- The project to demonstrate a fuel cell home that can function independent from the local power grid is relevant to the Hydrogen Fuel Initiative in that it enables experience to be gained with fuel cells in a stationary application. Even though this project is not related to transportation, it provides valuable learning and helps to establish the fuel cell manufacturing base.
- Real life demonstrations are very helpful.

Question 2: Approach to performing the research and development

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

- The PI should discuss and cooperative the project and the cooperative with PEMFC manufacturers and production companies.

- The project did identify barriers to installation and operation of the PEM fuel cells and the limitations of near commercial PEM fuel cell systems. The project did not address properly the codes and standards issues for fuels to be used in a PEM fuel cell. The approach should have carefully regulated the quality of the fuel for the PEM fuel cell. The project was fortunate the impurity was only methanol that affected the fuel processor cleanup process. Had the impurity been carbonyl sulfoxide or other species that can be formed in a reformer, dangerous emissions could have occurred. The project did not appear to fully appreciate the safety aspects of operating a fuel cell.
- The project developed a well thought out plan and pulled together appropriate team members to implement the plan.
- The project used available technologies, but was designed to provide information to assist in development of next generation projects.
- The approach is solid and the project has performed to plan. There is good integration with other similar projects but this could be expanded.
- Approach is systematic and clearly explained in 5 categories.
- The approach is reasonable for demonstrating an early generation fuel cell. Closer coordination with the vendor may have been able to foresee and prevent the early failure caused by the methanol in the new propane tank (which is used to remove any water in the as received tank). The presentation indicates that the barrier addressed in this project is co-production of hydrogen and electricity. In fact, the project does not produce any hydrogen for export.
- Demonstration at electric co-op sites provides very useful experience and data.

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

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

- The technical accomplishments for project are fair.
- The project has identified no real barriers that were not known by the manufacturer of the fuel cell. The flicker issue indicates the manufacturer is not providing a device that delivers grid quality power. The manufacturer should know the power is not grid quality and have disclosed this to the DCEC. The DCEC should have a contract that assures the delivery of grid quality power. These are not issues requiring technology accomplishments by the PI. The thermal recovery issues are well known problems for PEM fuel cells and should have been identified by the fuel cell manufacturer. Systems analysis clearly identifies these problems which are well known by the fuel cell industry. No new technical information was obtained through this project.
- This project provided information of value to both the fuel cell developer and the utility co-op. The fuel cell developer learned about the cause of early system failures and the co-op learned about power electronics requirements and product performance. These insights will save the fuel cell developer money in the future and lead to a better product.
- Additional operating experience will provide data for product optimization leading to better performance at probably lower overall cost.
- The project has had very good results for the low amount of DOE funds invested in the effort.
- This project identified "infant stack" failure as a significant concern.
- Large cyclic loads cause short-term flicker (not due to fuel cell!).
- Thermal management became an issue.
- The project was designed, built, installed, and is undergoing testing, which is almost complete. The project ends in January 2007. Several problem areas with the system were identified that highlight the need for further development of the Balance of Plant components, in this case, the power inverters. The lack of applicable codes for fuel cell systems in the NY area was also identified; although the project was successfully sited and operated over a ten month period.
- Additional data on energy saving benefits are not presented.

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

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

- The PI doesn't have any technology transferable. The PI should work the project with PEMFC system companies.

- Interfaces for technology transfer were established. The project should identify on its website that the PEM fuel cell does not provide grid quality electricity. Is that done? The internet site should identify that codes and standards for fueling the PEM system were not followed by this project or are not available. If the codes and standards are not available, some risk must be assumed by the operator of the fuel cell.
- It is not discussed in detail but clearly the co-op and the demonstration served to bring together industry and universities.
- What I thought was most important was the transfer of technology to the market place.
- The investigators have clearly demonstrated a strong effort and good success for sharing the results of the project; emphasis needs to be maintained through the end of the project.
- This poster itself is an excellent example of technology transfer – the fuel cell system is out in the field for a year.
- Public and academic outreach.
- There was evidence of collaboration between the partners. Final reports that are available to the public will be available at the partners' web site.
- This project has utilized excellent involvement of stakeholder's media and educators public internet access provision is a plus.

Question 5: Approach to and relevance of proposed future research

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

- DCEC is working closely with PEMFC companies.
- The project should complete its analysis of the performance of the fuel cell and power storage. The project should address how to eliminate the failures in fuel delivery and identify the limitations of the fuel cell power delivery.
- The project is approaching its end and so there are limited future activities.
- I think preparation of a good final report and distribution of this information to the general technical community is important to convey the benefits of real world demonstrations and also convey the specific findings.
- The proposed future work is appropriate for successful close out of the project.
- The project will be ending in June 2006 and the fuel cell will be decommissioned.
- Follow-on projects at other organizations are being planned.
- Technical analysis will be completed by January 2007.
- Data collection ends next month and the system will be decommissioned. Operating experience and data will be analyzed and a final report will be available on the project web site.
- Additional operation on fuel cell will be valuable if resources permit.

Strengths and weaknesses

Strengths
- The company is an Electric Cooperative company.
- There exists a need for real world demonstration of new technology. This project fulfills that need to a limited extent by moving emerging technology to the consumer.
- This project has a well thought out approach and good implementation.
- A number of important lessons have been learned that will benefit future products and make them more cost effective.
- The project has identified several operational concerns for utilizing stationary fuel cells, especially in a rural application. The investigators have taken appropriate steps to ensure that the results are shared and have been successful in these efforts. The project was a very effective use of limited amounts of DOE funds toward demonstrating and validating current technology stationary fuel cells.
- DCEC performed a real-world field trial for a year, so it could identify seasonal effects such as the mismatch between space heating requirements and thermal integration of the fuel cell system.
- DCEC identified several issues and proposed options to resolving them.
- DCEC had good support from the fuel cell vendor.

- After initial problems with drying the propane tank, the fuel cell operated well. Reviewer comments from last year were addressed. A large body of data from similar projects from around the country will be assembled from which conclusions will be drawn.
- Excellent concept for residential co-generation strong team particularly for outreach. Propane is a good selection for end of grid applications.

Weaknesses

- They need to work with PEMFC companies for installation and demonstration of residential fuel cell power. What is the hydrogen source? On page 14, propane and methanol are not good for PEMFC system. Propane and methanol need to be reformed for PEMFC system.
- The project did not fully address the requirements and risks of installing a new technology in commercial/residential environment. There appeared to be little adherence to codes and standards or to the safety aspects of reforming propane in a residential/commercial environment. Safety, codes and standards need to be fully addressed.
- Multiple installations would have provided more meaningful results.
- Technical data have not yet been analyzed and reported – so understanding significance of the project will have to wait until 2007.
- Understanding the variability of propane quality and its impact on system design needs to be improved. Overall energy savings estimates are not provided.

Specific recommendations and additions or deletions to the work scope

- The project is not worth continuing.
- One reviewer commented that the use of the term "infant stack failure" might be offensive to families that have experienced SIDS. Perhaps it should be changed to “early/new stack failure.”
- Project approaching completion.
- Similar projects should be strongly encouraged to build a database of operating experience.
- This project would present better if data and experiences from its sister project in NY were included.
- Characterize all the components in propane that influence the design of the desulfurizer and then provide a strategy to match the load transients.

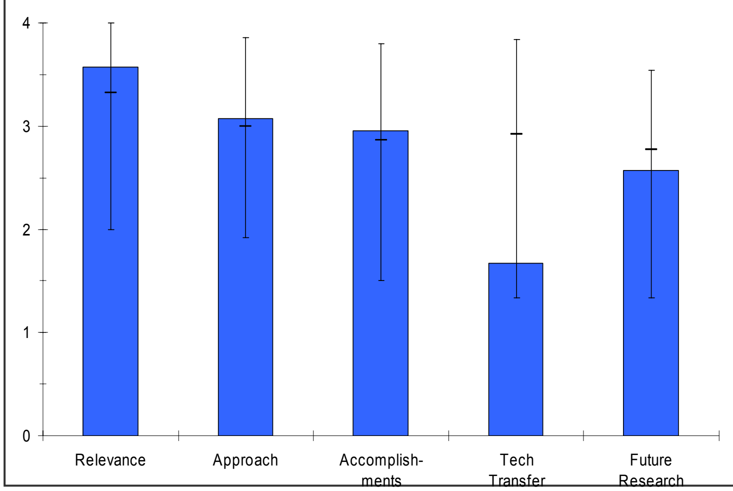
Project # FCP-23: Sub-Freezing Start-up of a Fuel Cell

Dennis Papadias; ANL

Brief Summary of Project

The goal of this ANL project is to understand fundamental aspects of the start-up process at sub-freezing conditions and to identify the key mechanisms that limit rapid start-up and lead to failure. ANL will study the effect of different start-up and shutdown protocols on fuel cell durability and performance.

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.

- Sub-freezing start-up of a PEM fuel cell is critical for success. The goal to understand the fundamental aspects is key.
- This is a new project and it shows good early progress.
- A modeling effort is needed to understand freezing effects in fuel cells better.
- Key transportation issue.
- Freeze start-up and survivability are critical to vehicular fuel cell development.
- Modeling work important.
- Experimental verification proposed.

Question 2: Approach to performing the research and development

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

- Model development and single cell experiments are proposed. The models and the experiments appear to be too general; focus should be given at first to local areas (i.e., one dimension) and then spread out. Experiments should be more carefully designed.
- The approach is a good combination of theoretic modeling and future filtering of experimental data.
- A more elaborate model is needed. Localized effects are modeled at steady state and will not give any real insight to the system performance.
- Modeling coupled with single cell validation is a good approach.
- It will be important to couple this effort with partners that can link stack issues (dimensions, response times, etc.) with single cell models.
- The researchers are not clear on what they might find or how they will use these results. There is no clear path to apply these findings.
- Good combination of experiments and modeling.
- Logical approach. Focused. Project addresses difficult issue.

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

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

- Modeling results on air flow rates and the effect on the removal of water are presented. No results on actual shut down or start-up protocols are presented. Models show water transport in GDL and flow channels Experimental apparatus appears to have been built and tested at 75°C and 25°C but not at below freezing yet?

- Based on information provided and guidance of the ANL fuel cell team, the project has made good progress.
- This is still relatively early in the program, and technical accomplishments are reasonable for this period of time.
- The project's initial results appear "on track" with regard to relating observed issues with predictions from the developed models.
- So far only baseline start-up has been accomplished.
- Project in very early stages.

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

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

- No collaborations appear?
- It is not possible to assess this at this early date.
- Project is just starting; however, I would expect that the nature of this project will require partners to assist in the validation of the proposed models, and to generate data for improving the existing models.
- Proposed collaborative efforts not evident.
- No collaborators are mentioned, how does this work coordinate with similar work at LANL?

Question 5: Approach to and relevance of proposed future research

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

- No specifics given on future work.
- The PI and his team have made a good description of future work.
- Work is largely exploratory, and the plans for studying perturbations in the system are particularly weak. A more systematic approach is needed.
- This project has a lot of potential to identify key issues and generate/inspire "engineering solutions" for fuel cell stacks.
- Work proposed important.
- More details regarding future work would be useful.
- Timeline not detailed.

Strengths and weaknesses

Strengths
- Modeling effort is underway. Experimental apparatus built.
- Project is very relevant and addresses freezing effects that need to be addressed for fuel cell commercialization.
- Project has good underlying fundamentals, both with proposed models and the test apparatus available for generating data and validating the models. The test apparatus is quite "flexible" and should allow "engineering solutions" to be evaluated and qualified.
- Good combination of experiments and modeling.
- Addresses an important topic, quantitative models that elucidate phenomena.
- Overall well thought-out and managed project.
- Strong team.
- Important technical issues addressed.

Weaknesses
- Lack of focus and clear goals. No protocols for shut down and start-up are given. Which will be tested? What are the possible prior shutdown scenarios and power draw as a function of time and temperature?
- Correlation between experimental and simulation work needs to be better developed. Experimental work seems to duplicate experiments at LANL.
- This project's viability will benefit by having partners in both the theoretical and stack fabrication areas. These resources will be needed to accelerate and deliver any understandings to resolving cold-start issues.

- Lack of industrial collaborators.
- No collaboration. Since the focus is at the stack level, and because manufacturers are reluctant to share details of their design, this work will not achieve its potential.
- More experimental vs. model data needed.
- Total project timeline vs. budget unclear.

Specific recommendations and additions or deletions to the work scope

- More details on the experiments and their priorities must be given.
- Project needs a clear vision of how these findings will be applied. Should coordinate efforts with modeling and experimental work going on at other institutions (UTC, LANL, LBNL, etc.).
- This project should include partners in both the theoretical and stack fabrication areas. These resources will be needed to accelerate and deliver any resolutions to resolving cold-start issues. This project should be closely aligned with other projects, if not combined, such that better utilization of resources and acceleration of results can be achieved.
- Need to test a large number commercial and non-commercial MEAs. Must cycle fuel cells through cold and hot conditions to understand materials durability issues.
- Add more fundamentals of transport of water during freeze/thaw rather than high level model of system.
- Prioritize to ensure continued focus.
- Disseminate results at least through publications.

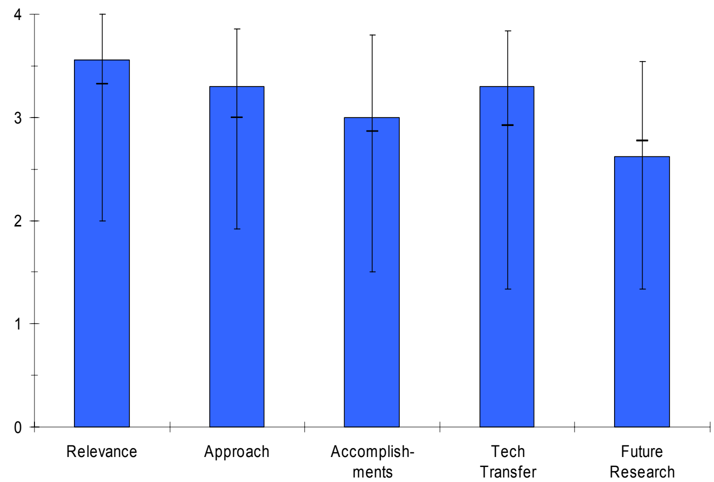
Project # FCP-25: Corrosion Protection of Metallic Bipolar Plates for Fuel Cells

John Turner; NREL

Brief Summary of Project

This project at the National Renewable Energy Laboratory (NREL) is concerned with the identification and characterization of metal alloys and coatings 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

Overall Project Score: 3.1 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Metal bipolar plates provide a possible alternative to graphite plates. At this stage of development, technology options reduces risks.
- The objectives of this project are aligned with targets for bipolar plate cost, resistivity, and corrosion rate.
- The subject matter (protecting bimetal plates from reaction with fuel cell fluids) is a very important, if less glamorous technology area that needs to be resolved.
- Corrosion work important.
- Good systematic approach to complex problem.

Question 2: Approach to performing the research and development

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

- The approach appears to encompass several parallel paths, rather than focusing the effort on one. Nitriding of steels, conductive coatings, assessing impact of cations on membrane conductivity, and then measurements in phosphoric acid (PBI-like conditions).
- Focussing resources on the most promising approach going forward may prove more effective.
- It may be more cost effective to validate the resistance and corrosion measurements in a single cell test rather than building a stack. The test setup is simpler and one could make more measurements to validate the resistance and corrosion measurements.
- Investigating plates for high temperature phosphoric acid applications is tangential. The remainder of the approach is relevant.
- The PI is trying a good collection of surface treatments.
- Should keep in mind/address the cost issue when coating metals.

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

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

- The work has differentiated different steels. It would be beneficial to validate this ranking of materials and the test methodology with single cell measurements.
- Need to improve the cost projection to include the cost of the coating and fabrication process. Are other properties important such as thermal conductivity? How will the coating work on plates with flow channels? Can one obtain a uniform coating on 3D profiles? How effective is the coating on edges or corners? Will thermal/mechanical stresses result in cracking of the coating?
- Is the tin oxide work in parallel with nitriding of the steel? How will this coating stand up to bending of the interconnect? Will cracks form? Given its use in the PV area, what is the projected cost? How will it work on 3D surfaces?
- For the budget, the work appears fragmented between too many subject areas: nitrided steels, tin oxide coatings, membrane measurements, and phosphoric acid tests.
- The investigators have identified a good range of candidate materials; beginning of life (BOL) resistance measurements are presented. Corrosion measurements must include cycling.
- The experiments, generally, show DOE goals being reached.

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

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

- Interaction with ORNL.
- Oak Ridge has been used for material synthesis, and Jadoo will be used for fuel cell evaluation; no further collaboration is suggested.
- There is a good degree of industry/government collaboration (e.g., Plug Power and NREL/ORNL). The PI realizes that project metrics also include manufacturability of plates, and partnerships in this area could be further developed (e.g., Manufacturing Roadmap).
- Should try to establish a collaboration with an industry member who can mass produce high volume metallic bipolar plates.

Question 5: Approach to and relevance of proposed future research

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

- Approach to determining adequate formability is unknown. The extent to which SnO_2:F coating will be incorporated in the study is unknown.
- Proposed research along lines of "more of the same." Future activity should also include ties with manufacturing research.
- Work proposed important.
- More details regarding future work would be useful.
- Timeline not detailed.

Strengths and weaknesses

Strengths

- The investigators have fabricated and confirmed their fabrication of materials that are worthwhile candidates for meeting the identified targets.
- Good results in important, if less prominent fuel-cell development issue.
- Covering a wide range of steel and stainless steel in testing.
- Overall well thought-out and managed project.
- Strong team.
- Important technical issues addressed.

Weaknesses

- Effort possibly spread across too many tasks for the budget.
- The failure to test with cycling prevents an evaluation of corrosion resistance. The approach requires organization.
- Needs greater ties to manufacturing research in order to minimize total, rather than only materials cost.
- Cost presentation does not account for manufacturing costs. A collaboration with industry would be useful here. When discussing cost targets, all costs need to be accounted for if possible. Cost of material is not an indicator alone when comparing to a final cost target.
- Total project timeline vs. budget unclear.

Specific recommendations and additions or deletions to the work scope

- Use more analytical tools to characterize coating. Consider additional tests of coating that might represent stresses from handling and manufacturing. Consider coating or treatments on 3D profiles.
- Interested in XPS profiles before and after stack operation. What is the impact of inclusions in the steels on the nitride or coating protection? Can these lead to point defects?
- A test plan for corrosion evaluation and formability evaluation needs to be added. Such a test plan should also define which material/coating combinations will be studied.
- Recommend adding combination of research with manufacturability (e.g., low-cost plate stamping).
- Prioritize to ensure continued focus.

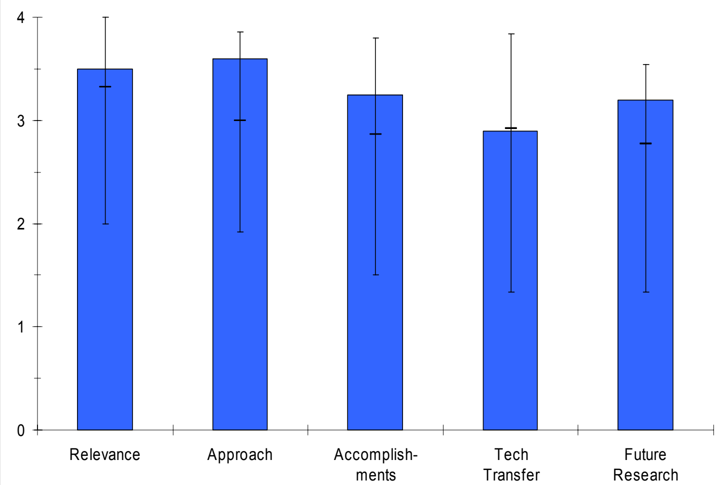
Project # FCP-26: Development of Low-Cost, Clad Metal Bipolar Plates for PEM Fuel Cells

Scott Weil; PNNL

Brief Summary of Project

To assist the DOE in lowering the cost and improving the durability of automotive PEM fuel cell stacks, Pacific Northwest National Laboratory (PNNL) is reducing the material and manufacturing costs of bipolar plates, while substantially increasing their resistance to corrosion and mitigating the release of poisonous metallic ions into the MEA. This project consists of a feasibility study to determine the potential efficacy of a clad metal approach in developing a low-cost/low-mass/low-volume PEM bipolar plate; formability testing of Nb and Ni clad metals, followed by *ex situ* validation testing of formed pieces; and fabrication of small-scale plates for short-stack testing.

Overall Project Score: 3.3 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.5** for its relevance to DOE objectives.

- Metal bipolar plates provide a possible alternative to graphite plates. At this stage of development,technology options reduce risks.
- Objectives are aligned with durability and cost targets.
- Development of durable, lightweight, easily manufacturable, low cost bipolar plates is an important enabling technology for fuel cell stacks. This project addresses these goals.
- Specific DOE targets highlighted.
- Good systematic approach to complex problem.

Question 2: Approach to performing the research and development

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

- The approach u provides preliminary data on a number of new materials.
- May want to consider if sufficient Nb would be available if fuel cells are commercialized by doing order of magnitude estimates and comparing with current or projected production volumes. Discuss with ATI Wah Chang.
- Include high level cost estimate of the boronizing treatment in the cost analysis.
- The use of clad metal material is a very particular approach. The investigators are giving this approach its best chance with Nb and boronized Ni.
- The technical concept of designing a low cost bipolar plate using a metal composite and roll cladding is attractive for its potential for easy manufacturing and low cost. The PI seems to be taking a logical approach to experimentally determining which cladding materials can best meet the needs of the program.
- Logical approach. Focused. Project addresses difficult issue.

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

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

- Both classes of clad materials still need to be fabricated at lower thickness (of cladding) in a very limited timeframe.

- Accomplishments to date seem promising with both the B-Ni and Nb cladding materials; however, the ability to form channels in the cladded materials without detrimental effects to the cladding thickness or material characteristics will be critical to assessing the viability of the technology.
- Good progress.
- Significant number of milestones met or achieved.

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

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

- At this stage of feasibility demonstration and with the limited budget, would not expect very much collaboration.
- Given the limited timeframe, the direct National Laboratory/small company collaboration is sufficient.
- Collaboration at this time is limited to the roll forming/cladding facility.
- Publication record is good. Collaborations could be deeper. Not apparent the contributions from partner.

Question 5: Approach to and relevance of proposed future research

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

- Reasonable proposed next steps to assess performance in stack environment and to consider potential issues associated with exposed edges.
- As noted above, should consider high level estimation of processing costs for the boron treatment.
- The sequence of future work is well-defined; time may not allow all of it to be done. In-situ experiments are not listed.
- Future plans are consistent with the scope and funding level of the project. Work to investigate the ability to stamp channels in the material without damaging the cladding will be important.
- Reasonably focused plan.

Strengths and weaknesses

Strengths
- Sequence of tests is well-defined.
- The investigators are capable of fabricating the needed materials.
- Opportunities for low cost, easy-to-manufacture bipolar plates.
- Cladding materials seem to meet goals of project and are low cost and plentiful.
- Overall well thought-out and managed project.
- Strong team
- Important technical issues addressed.

Weaknesses
- The project is too ambitious for the time given.
- Total project timeline vs. budget unclear.

Specific recommendations and additions or deletions to the work scope

- Breakout cost of the substrate, clad material, and any post processing. Then add the cost of finishing operations to form flow fields to provide value that can be compared with molded graphite or expanded graphite plates.
- Consider discussion of ultimate plate thickness and impact on bipolar plate thickness. This will provide data to estimate benefits relative to kW/kg or kW/L of the stack compared to projected values for carbon plates.
- The project should be extended past 9/30/06. *In situ* experimentation on cladded plates will be able to confirm whether DOE targets are met.
- Prioritize to ensure continued focus.

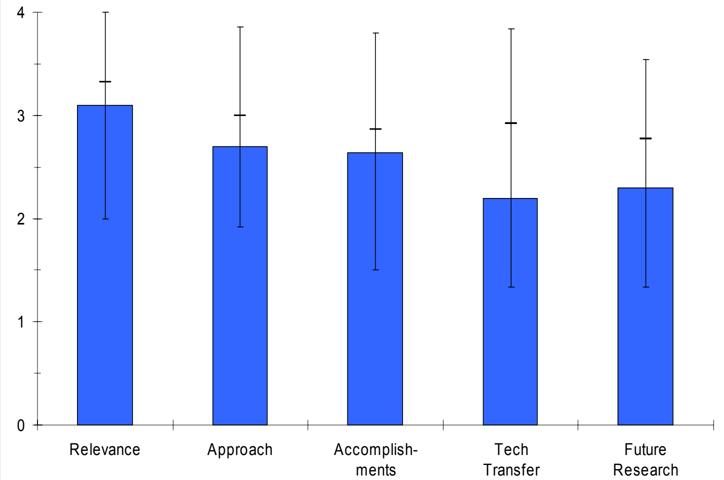
Project # FCP-27: Advanced Catalysts for Fuel Cells

S. Narayanan; JPL

Brief Summary of Project

The overall objectives of this Jet Propulsion Laboratory project are to reduce the cost of fuel cell stack components and to reduce the amount of precious metal used. Work in 2005 and 2006 is focused on developing methods for combinatorial screening of oxygen reduction reaction catalysts, identifying catalysts capable of performing at 2500 mW/(mg of precious metal), and increasing the cathode potential by 0.1 V at 500 mA/cm^2.

Overall Project Score: 2.6 (5 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **3.1** for its relevance to DOE objectives.

- The project is irrelevant for the DOE 2010 target goals for the design of novel multi-metallic cathode catalysts.
- Reduction of fuel cell catalysts cost by reduction of Pt content is aligned well with the FC plan objectives.
- Reduction of Pt loading is one of the identified RD&D plan objectives.
- By addressing the cathode catalyst cost and precious metal loading issues, this project is very relevant to the objectives of the HFCIT program.

Question 2: Approach to performing the research and development

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

- The objective and approach is neither original nor well justified.
- Addition of low weight metal such as Ni or small at % of the tried element (such as Zr) will not substantially reduce the cost of cathode catalysts. The correct approach would be to improve catalytic activity of Pt or the existing Pt bimetallic systems. With the later approach the catalyst loading and thus the cost can be significantly reduced.
- Combinatorial methods are already well developed and no need for further improvement is required.
- Systematic approach based on sputtered alloys followed by structural characterization.
- Quite late in PEMFC development to be pursuing combinatorial approach with sputtered films.
- The approach is promising and quite novel for platinum-based cathode catalysis.
- Multi-electrode array approach has helped to achieve high throughput in testing the ORR activity of various compositions.
- Transition from thin-film morphology to dispersed catalysts may prove difficult.
- Initial durability data should have been presented by now to justify viability of the approach.
- Should keep in mind/address the cost issue when coating metals.

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

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

- Catalytic activity of Pt-Ni catalysts is rather low and are not consistent with literature data.
- The effect of Zr is neither well justified/understood nor provides any guideline how to create new multi-metallic systems.

- To make any progress in the quest of developing new cathode catalysts, the PI needs to get insight into the relationships between the surface characteristics and catalytic activity. The progress should be made if the PI would combine experimental results with the existing quantum mechanical calculations.
- This is a limited scope project, the library studied was well selected, might be beneficial to compare few binary or ternary libraries.
- As usual, combinatorial approach finds a "sweet spot" in composition. Problem is, of course, that the combinatorial method of synthesis does not produce a "usable" catalyst, i.e. one that can be used in a fuel cell. Further work, actually more difficult and extensive than discovery, is needed to produce a real catalyst with this composition.
- Improvement in the relative performance of the ternary catalysts versus that of the Pt reference is promising as is the potential for lowering the Pt loading.
- At ~0.77 V, the ORR onset potential at the Pt reference, defined as the potential measured at 1.5% of the limiting current, is much lower than expected. This casts doubt on the quality of the presented data for all tested materials. Could the purity of the sputtered catalysts be a problem? Should ORR kinetic studies be performed on large stationary electrodes as those used in the presented work?

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

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

- The project needs strong collaboration with other partners because the PI does not have adequate knowledge in the field of the oxygen reduction reaction.
- No collaborative efforts were reported.
- None.
- Good partners in materials characterization. More collaboration in the materials development might be helpful.
- Should try to establish a collaboration with an industry member if they are planning on continuing this work. Is Pt/Ni new development?

Question 5: Approach to and relevance of proposed future research

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

- Future work plan has no relevance toward eliminating the barriers and advancing the program.
- Important addition proposed in the work plan is to study various alloying elements. This will allow for comparison of effects due to nature of specific alloying element.
- Where is the plan to produce a real catalyst?
- Further catalyst performance screening should be preceded by durability testing of selected existing materials.
- Acceptable purity of sputtered catalysts needs to be demonstrated using electrochemical and, perhaps, other techniques.

Strengths and weaknesses

Strengths
- Relatively well developed sputter-deposition thin film method.
- Possibly in thin film synthesis.
- Original approach to making and testing cathode catalysts with low Pt loading.

Weaknesses
- The project is lacking fresh ideas which are required for the development of new generation of active materials for the ORR.
- Late in the combinatorial game and probably duplicative over what 3M and Jeff Dahn have already done (and many others, e.g. Honda/Symex).
- Most of the research effort has gone into technique validation, which was also the focus of this project in FY05.
- Lack of durability data.

Specific recommendations and additions or deletions to the work scope

- Variations of synthesis method, e.g., evaluation of layered structures (top Pt layer, for example) combined with study of activity and durability.
- Why continue?
- Investigate the origin of surprisingly low values of the ORR onset potentials. Viability of the Pt-M-Zr catalysts for fuel-cell type cathode should be demonstrated soon.
- It is unclear in the poster if durability is considered.

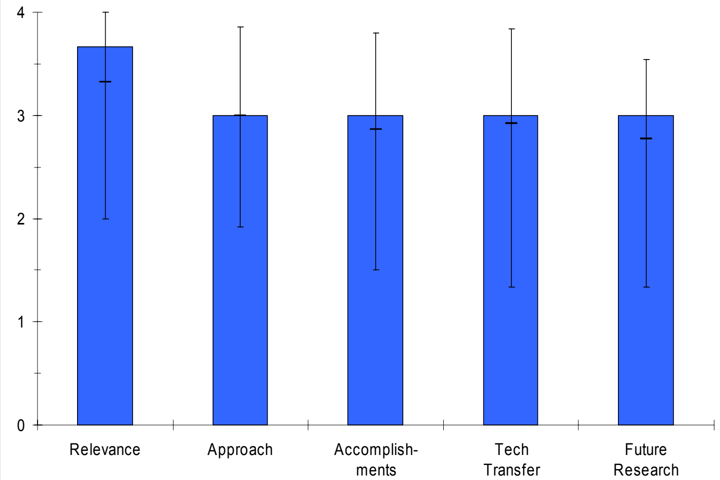
Project # FCP-28: Contaminant Effects

Debbie Myers; ANL

Brief Summary of Project

In this project, ANL will: determine the mechanisms for the degradation of PEFC performance by impurities in hydrogen and in air; predict the long-term effects of impurities on PEFC stack performance; predict the effect of operating conditions and impurity concentration on PEFC stack performance; develop strategies to mitigate performance degradation and enhance stack durability; and develop strategies to recover stack performance after the impurity is removed from the fuel or air streams.

Overall Project Score: 3.1 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Understanding the effects of anode and cathode impurities is vital to both fuel cell durability and performance as well as in determining a proper H_2 fuel specification.
- Catalyst performance and lifetime are key issues for meeting transportation goals.
- Critical study to enable fuel cell commercialization.

Question 2: Approach to performing the research and development

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

- There would be value to extending the range of temperatures studied to sub-freezing when possible.
- PI has identified a useful evaluation technique for the initial contaminant studied.
- The PI's inclusion of "how permanent the contaminant effect might be on the Pt and methods to mitigate the effect" will be a useful protocol for future contaminants studied by the project.
- Good, but it is not clear how different this is from the 10+ years of work performed at LANL.

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

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

- Project is at a very early stage. Too early to gauge.
- I would expect further refinement/additions in the evaluation technique as other contaminants are studied.
- Good, but Cl^- poisoning has been well studied by Uribe et al. Organo-chlorine compounds have not been studied in detail yet so a nice extension.

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

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

- Would benefit from more collaboration with automotive (and stationary?) OEMs to focus cathode contaminant candidate list.
- Link and develop synergies with similar LANL project (FC-24).

- The PI needs to include the FreedomCAR Fuel Cell Technical Team's feedback as the various contaminants are studied.
- Work with tech teams is good, but really needs to be coordinated with LANL so that a lot of time is not wasted repeating experiments.

Question 5: Approach to and relevance of proposed future research

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

- Too much listed for a single year. Needs multi-year funding.
- Project just starting, and has the potential to provide much-needed guidance on the contaminants issue – both for prevention methodology and selection of catalyst formulation.
- Good, but are organo-chlorine compounds really the biggest priority, especially in relation to pollutants that the early fuel cell cars will have to deal with from conventional combustion.

Strengths and weaknesses

Strengths
- Good technique and protocol for evaluating contaminant effect/duration on catalyst systems.
- Inclusion of potential contaminant's "persistence and mitigation" will be an important feature of the project.
- Component level analysis tied to stack model.

Weaknesses
- The project should include a listing of the "top 10" contaminants as defined by the FreedomCAR Fuel Cell Technical Team. The listing should include the source (air, components, etc.), expected concentration, priority for review (based on expected performance decline), etc. This "scorecard" can then be updated by the Project's results on the effect of the contaminant, including how fast the contaminant affects the catalyst system, is the effect permanent or reversible, etc. This "scorecard" then becomes a guide for industry in selecting catalyst systems as well as specifying fuel cell stack/system design, etc.
- Needs to be coordinated with past and present work at LANL.

Specific recommendations and additions or deletions to the work scope

- Need to carefully consider real pollutant and impurities that will be experienced by the early fuel cell cars in real air sheds.

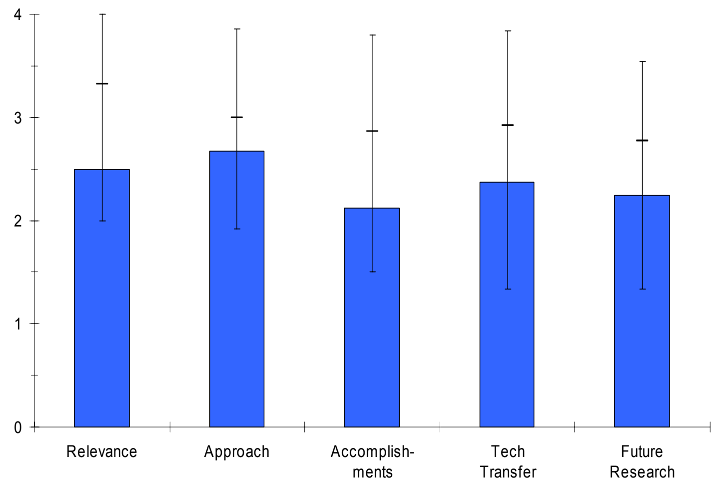
Project # FCP-29: Non-Platinum Catalysts

Xiaoping Wang; ANL

Brief Summary of Project

This Argonne National Laboratory (ANL) project is developing 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. The approach this year focused on noble metal-base metal alloy nanoparticles and base metal macrocycles attached to polymer backbones.

Overall Project Score: 2.4 (4 Reviews Received)



Question 1: Relevance to overall DOE objectives

This project earned a score of **2.5** for its relevance to DOE objectives.

- The project is irrelevant for the DOE 2010 target goals for the design of novel bimetallic cathode catalysts.
- Developing a non-Pt catalyst addresses the cost target. The use of precious metals prevents the relevance from being "outstanding".
- For practical application point of view, no other PGM with exception of Pd needs to be considered. If fundamentals of electrocatalysis are addressed than it is ok to work with Ir, Os, etc., but has to be clearly stated.

Question 2: Approach to performing the research and development

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

- The objective and approach are neither original nor well justified.
- The title is misleading; Pd, Ir and Rh belong to the Pt-group metals. Therefore, there is no justification that the synthesis process is scaleable at low cost.
- The PI has neither the knowledge nor required surface analytical tools for studying surface segregation processes.
- Of the two approaches, one is conventional (the modification of d-Bands), and the other is novel (attaching a metal to an electron-conducting polymer).
- Two unrelated topics are considered.

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

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

- Pt-like catalytic behavior is not good enough to meet the DOE targets.
- In electrocatalysis, application of the Hammer-Nørskov theory requires more knowledge than simple analysis of the DFT predicted segregation trends.
- The d-band theory is, unfortunately, still not applicable for real nanoparticles.
- The O_2 reduction mass activities for the enhanced 1r and Pd-based catalysts are below the DOE target by an order of magnitude. Work on the other approach has not been completed.
- Some good work but difficult to judge.

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

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

- The program needs strong collaboration with other partners because the PI does not have adequate knowledge in the field of the oxygen reduction reaction.
- Collaborators are mentioned, but the degree of collaboration is not well elaborated. What experiments were done with whom?

Question 5: Approach to and relevance of proposed future research

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

- Future work plan has no relevance toward eliminating the barriers.
- Testing in an MEA is an important next step.
- The bimetallic systems are failing, but there is no clear concept as to how to execute the electron-conducting polymer backbone approach.
- It seems that the focus is going to be exclusively on Pd-based catalysts in which case the stability should be immediately tested.

Strengths and weaknesses

Strengths
- The PI is a part of well respected group in the field of fuel cell technology.
- Systematic study with careful characterization.
- The investigators have the ability to generate ideas and apply recent knowledge to justify a research direction.

Weaknesses
- Whole program is weak and the PI has no experience in the electrocatalysis of a such complicated and demanding reaction such as the ORR.
- The PI did not show a path to meeting the chemical compatibility (durability) and cost goals. Pd-based system may meet cost target, but no data was given.
- The project has not produced a beneficial result.
- The only progress has been the enhancement of precious metal-based catalysts which will not satisfy cost targets.

Specific recommendations and additions or deletions to the work scope

- Provided that a reasonable direction for the electron-conducting polymer backbone approach can be devised, the project should change to this approach.
- The bimetallic work should be stopped.
- Coordinated effort with other Labs will be beneficial not only to the fuel cell testing but on the fundamentals as well.

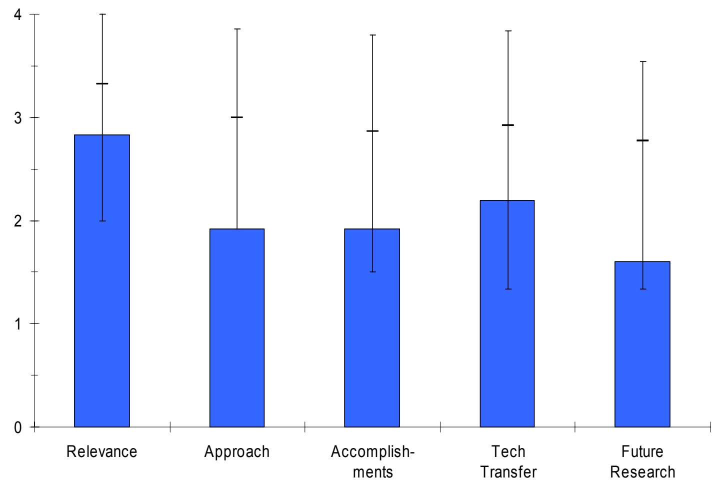
Project # FCP-40: Tungsten Oxide Cathode Catalysts

Joel Christian; OSRAM Sylvania

Brief Summary of Project

The objective of this OSRAM project is to produce a tungsten electrocatalyst catalyst with high specific power at a cost significantly lower than platinum. This effort includes evaluating the current catalyst in cathode applications and optimizing catalyst synthesis procedures to improve activity against the DOE technical targets for non-platinum catalysts. Performance evaluation is conducted at 250 hours and over 1000 hours.

Overall Project Score: 2.1 (6 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- Established as primary objective a performance metric (A/cm^3) that is not in the RD&D.
- The project is irrelevant for the DOE 2010 target goals for the design of novel non-precious metal cathode catalysts.
- This project attempts to find a stably active non-Pt oxygen reduction catalyst.
- It seems unlikely that the no-Pt route (as opposed to the low-Pt one) is the most probable means of achieving DOE's catalyst cost targets, but the decision to pursue no-Pt work was made by DOE (with additional direct input from Congress in some cases).
- The OSRAM Sylvania team is making progress with a low cost, non-platinum polyoxometalate catalyst. They have confirmed stability to 3200 h operation. A challenge with these catalysts is their low voltage.
- Development of active, low cost fuel cell catalysts are critical to the commercialization of fuel cells for transportation applications.

Question 2: Approach to performing the research and development

This project was rated **1.9** on its approach.

- Nothing new here. Tungsten-based catalysts were examined in great detail in the 1970's by numerous researchers and research groups.
- The approach has significant weaknesses; covering none of important aspects for the development of cathode catalysts.
- The PI must develop methods how to test a true specific activity of catalysts as well as to incorporate RRDE measurements to check the peroxide formation.
- The PI has no knowledge about the surface electrochemistry or the oxygen reduction reaction. The PI must to revisit old literature related to catalytic activity of the "Pt-free" tungsten catalysts for the ORR and to compare his data with the existing one.
- The PI must include theoretical models in discussing the possible active sites.
- This is an extremely difficult research area that demands great experimental care, in part because of the claimed need for total reduction of the material at very low potentials before it can exhibit full activity.
- This extreme pre-conditioning required, and prior operation as a hydrogen oxidation catalyst in at least some of the experiments, leads to a significant danger of Pt contamination of the cathode unless there is no Pt in the cell.

- The investigators properly have started no-Pt RDE experiments, but the initial data involve very low currents, only a quantitative, not qualitative, difference in the responses to O_2 and to Ar, and show plateaus at currents far too low to be real limiting currents.
- Having LANL do some experiments was a very good thing.
- While the appropriate activity target for non-precious catalysts is A/cm^3, data should still be shown in conventional electrochemical units of A/cm^2 to allow others to get a feel for the small magnitude of the currents measured here.
- The approach of this team is good in that they have teamed with LANL for fuel cell testing. They also have support from a lot of professors. Some of the materials analysis is a little inconsistent, and it's not clear how truly it reflects the *in situ* state of the catalyst. This might impede their progress, if they don't understand the mechanism.
- The activities of these materials are extremely low and a realistic path towards dramatic improvements in activity was not presented.
- The catalytic activity of POMs in general is very low. There is no fundamental background why such surfaces should be active for the ORR.

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

This project was rated **1.9** based on accomplishments.

- Objective testing at LANL shows 0.5 V at 10 mA/cm^2. This is said to be a 5x increase from baseline (!). To meet efficiency objectives there must be at least 20% rated power at 50% efficiency, i.e. at a cell voltage above 0.75 V. There is zero current at 0.75 V. 5x nothing is nothing.
- The presented activities are neither clear nor convincing. It is very difficult to separate capacitive components from a true catalytic activity.
- The best total activities achieved here are still quite small. If the electrode layer thickness of slide 11 is 10 microns, then the A/cm^3 scale is numerically equal to mA/cm^2. The activity in slide 11 appears to be roughly that of a Pt loading of 0.005 mg/cm^2 — far below a practical activity, and one wouldn't necessarily expect to be able to see such a small amount of Pt in the XPS measurements of slide 8.
- Los Alamos results did not reproduce any activity at 800 mV or above, admittedly with only a limited time spent trying.
- Because the strongly reductive pretreatment greatly increases the danger of Pt contamination of the cell, a more cogent explanation should be given as to why full reduction is essential to activation, and the additional chemical step that follows this reduction should be explained.
- The team met their own goals by passing their project milestone by 50%; however they still have a long way to go before they reach the DOE goals for non-platinum catalysts (p. 11 of presentation). The RDE performance of the catalyst seems very poor and they do not reach the limiting current expected for the oxygen reduction reaction. This result could be due to either peroxide generation or to insufficient supply of oxygen to the electrode. It's probably the latter, as they would not be able to achieve 3200 h stability if they were generating a lot of peroxide, but this needs to be sorted out. The stability of the catalyst at 0.24 V vs. 0.79 V is also not clear. The idea that the spacing between the W atoms enables the tungstate to mimic Pt is not really clear, as one could also argue that it is the d-band states of the Pt that make it special. It is not clear what the electronic configuration of the tungstate is. In summary, the team has made accomplishments, but the results are still not fully understood
- The activity was improved only slightly this year.
- Some improvement in the ORR activity has been shown but it does not make the system promising. 10 mA/cm^2 at 0.5 V is very low. Some data are not well understood.

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

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

- What collaboration?
- This is not important because these type of materials will never be used as a catalyst for the ORR.
- This technology is in too early a stage to be transferred, so the lack of industrial participation outside the prime contractor for the work is not an element of concern.

- They have done a good job leveraging expertise at LANL.
- Collaboration with LANL is a good step. Need further input from someone with a strong background in electrocatalysis.

Question 5: Approach to and relevance of proposed future research

This project was rated **1.6** for proposed future work.

- A thousand times nothing is still nothing.
- Future work plan has no relevance toward eliminating the barriers of advancing the program.
- The future work section of the presentation was dominated by a comparison of probable ultimate site densities that was hard to follow; the listed Pt metal utilization (assuming that is the ratio of surface atoms to total atoms) was 1/5 that of state-of-the-art commercial Pt catalysts.
- The proposed steps for 2007 are reasonable if the program continues.
- The path to improving the catalysts is unclear. Tungsten bronzes were first reported as cathode catalysts in sulfuric acid by Broyde in 1968, but they have still not "made it". The presentation does not clarify how they will move forward, i.e. Meet DOE's 2010 goals. Once concept on p. 15 is to disperse individual polytungstate molecules on the carbon, but there might be issues with agglomeration. How would the oxide be bound to the carbon? It seems like a good idea, but there are not specifics on how these ideas would be reduced to practice.
- A realistic path toward the vast improvements in the intrinsic catalyst activity was not presented.
- Some projections for future accomplishments look too optimistic.

Strengths and weaknesses

Strengths

- Tungsten resources.
- The PI is a part of great company which unfortunately has no experience in catalysts production for a such complicated and demanding reaction such as the ORR.
- Control over some very interesting tungsten chemistry has been demonstrated.
- Focused team, working well with LANL.
- Collaboration with LANL on testing of catalyst in MEAs at LANL.

Weaknesses

- Tungsten-based catalysts don't work as PEMFC cathodes.
- Whole program is weak and the PI needs simply to admit that tungsten based materials have no future as the OR catalysts.
- Quite small currents were measured; none at or above 800 mV at LANL.
- Overuse of the A/cm^3 units obscures how small the currents were.
- The basic concept may not be viable – others have looked at polytungstates and have never been able to make a significant impact on fuel cells.
- Low activity of electrocatalyst and overstatement of the promise of catalyst.
- Fundamental electrochemical behavior of POMs is not satisfactory.

Specific recommendations and additions or deletions to the work scope

- Never should have been funded in the first place. Terminate as soon as possible.
- Consider investigating these materials as special supports for Pt, possibly giving strong metal-support interactions yielding improved specific activity and/or resistance to voltage cycling. But be aware that in the flurry of activity on similar materials after the Bockris Pt-contaminated experiments, similar tungsten bronze materials were looked at as supports and, in systematic studies, showed no enhancement of Pt activity.
- The analysis of the tungsten XPS is a little funny. It seems to be integrated in reverse so that the W $4f^7$ peaks are 1/2 the size of the $4f^5$ peaks. Some work needs to be done to make the analysis make sense, as there might be more valuable information there. One interpretation is that there are two W(IV) species, one with a peak at 32 eV, and the other at 33.5 eV (corresponding to WO_2 and the other to the polyoxometalate).
- Project is due to end this fiscal year.

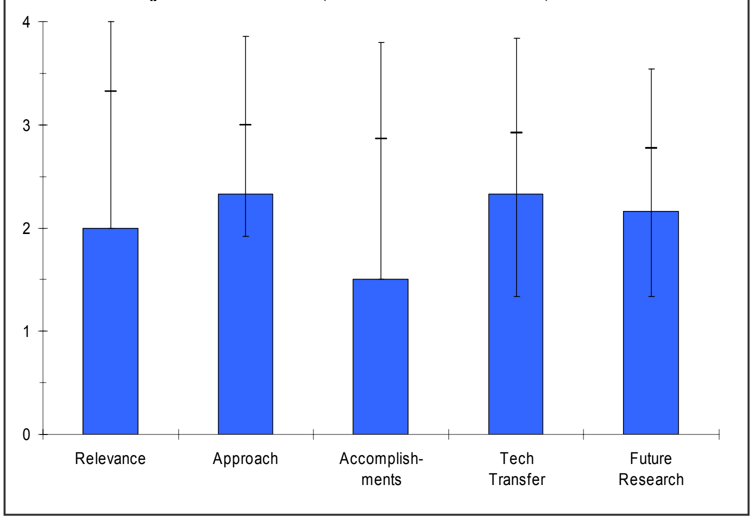
Project # FCP-42: Smart Fuel Cell Operated Residential Micro Micro-Grid Community Grid Community

Mohammad Alam; University of South Alabama

Brief Summary of Project

The University of South Alabama has completed an operational analysis of a multiple fuel cell power plant based mini-grid system. Novel materials suitable for efficient light harvesting photo-anodes are being developed. A methane reformer and a metal hydride based compression and purification process are being modeled. Fuzzy logic based energy management algorithms have been implemented to manage power demand around a threshold better. A similar neural network based algorithm is being developed to manage the water heater.

Overall Project Score: 2.0 (3 Reviews Received)



Question 1: Relevance to overall DOE objectives

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

- This project is identifying mechanisms to encourage the introduction of fuel cells into the residential buildings market.
- The relevance is not at all clear. This project does not address any major issues in the commercialization path. This work may result in minor improvements (e.g., improved control strategies) that will likely be done by industry anyway and would best be conducted on systems that are actually closer to commercialization (i.e., these are system optimizations that are best done later since there are not any real technical barriers being addressed). In any case, the work done here certainly does not justify the budgets allocated.
- The topics investigated under this effort generally support the vision and R&D objectives but do not appear to be focused on addressing critical path developments needed to meet specific measurable goals of the R&D plan.
- Most aspects of project align with the President's hydrogen vision and RD&D plan objects.
- This project will not address durability of stationary fuel cell systems, but may help lower costs by reducing needed installation capacity and mitigating market entry barriers.

Question 2: Approach to performing the research and development

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

- The approach is ok for the goals stated, however, it is mostly just modeling and data collection, with no real apparent attempt to do any model validations. Certainly, with the budget allocated, more work with actual hardware could be done.
- The approach to the research was not satisfactorily communicated.
- The approach is generally well thought out; however, inclusion in this project of Task II – "Production of Hydrogen Using Photo-Electrochemical (PEC) Solar Cells" and Task III – "Modeling of Hydrogen Production, Purification, and Storage" is questioned.
- Tasks II & III diffuse the main focus of this project, smart energy management for residential fuel cell applications.
- The task should focus solely on using smart energy management to reduce installed costs via load shedding, increasing potential fuel cell customer acceptance, and identifying optimal sizes, configurations, and control strategies.

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

This project was rated **1.5** based on accomplishments.

- Fair, but deliverables per dollars spent does not appear impressive. No real results shown, so it is difficult to judge. The lack of results is especially disappointing for a program so far along.
- The accomplishments do not appear to be appropriate for the level of funding applied to this project.
- Technical accomplishments 3+ years into the project are fair.
- Micro-grid community layout complete including sizing of fuel cells and potential energy management strategies.
- No data is available yet on the costs of a fuel cell powered micro-grid community nor other possible mechanisms for cost reduction besides lower installed capacity via load shedding.

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

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

- What is going to be done with these results, besides being published? Are there any interactions with companies that make these products? If not, why not? If so, are they actually interested in this work? Stated interactions appear to be limited to fuel-cell testing houses (e.g., HARC), not OEMs.
- The investigator clearly shared results through publications, but the poster did not make evident any in-depth collaborations with other investigators from other organizations.
- Project has fair interaction with other institutions and projects. Efforts were made up front with Alabama Power to look at load management strategies and typical home load profiles.
- PI should bring into project discussions with residential home builders to gain insight as to most likely pathways of market entry for residential fuel cell systems. No discussions to date have been conducted with builders and what their interests are with regards to residential fuel cell systems and how market barriers may be overcome.

Question 5: Approach to and relevance of proposed future research

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

- It looks like some real hardware testing and model validation may take place in Phase IV.
- Proposed future work appears to follow on to the past efforts, but insufficient details were provided to assess the future plans with any degree of confidence.
- PI indicated future research (if funding is available) would focus on implementation of the fuel cell system and the energy management system into the university housing community. This is a mistake – implementation would be best in coordination with the advice of professional builders to gain insight to best market entry points.
- The market entry point for a fuel cell micro-grid system would be in the high end home community – not average 2,000 sq ft middle class homes.

Strengths and weaknesses

Strengths

- Approach has some merit and could contribute to mitigating market barriers to fuel cell powered residential homes.

Weaknesses

- No real obvious technical barriers being addressed. The project is principally limited to modeling work, which does not seem to justify the budget allocated. The combination of projects here is also quite odd and they are not clearly related.
- The poster did a very poor job of effectively communicating the intent, progress and future plans of the project.
- Tasks II & III are out of scope and should be eliminated.
- Rigorous cost analysis is needed.
- Coordination with building community is necessary.

Specific recommendations and additions or deletions to the work scope

- The investigation of semiconductor materials for application in photoelectrochemical applications does not fit with the rest of the scope of this effort.
- A comprehensive cost analysis of the fuel cell micro-grid strategy is an essential deliverable from this task.
- This task should coordinate closely with the Zero Energy Building activities of DOE's Building Technologies (BT) Program. BT's activities are aimed at significantly lowering the energy load of homes via integrated appliance management, solar applications, improved envelopes, etc. This approach significantly lowers the energy load of a house. Integration of fuel cell and advanced energy management should be looked at in the context of advanced energy home systems not coupled with homes exhibiting conventional energy load profiles.