U.S. DEPARTMENT OF ENERGY | Energy Efficiency & Renewable Energy

SOLAR ENERGY TECHNOLOGIES PROGRAM

2010 SOLAR PROGRAM PEER REVIEW REPORT:

An Independent Evaluation of Program Activities for FY2009 and FY2010

THIS PAGE INTENTIONALLY LEFT BLANK

Dear Colleague:

This document summarizes the recommendations and evaluations provided by an independent external panel of experts at the U.S. Department of Energy (DOE) Solar Energy Technologies Program's 2010 Program Review meeting, held on May 24–27, 2010, in Washington, D.C. This report provides evaluations of the program's projects in applied research, development, and demonstration as well as analysis and deployment activities. The Program Review is an evaluation of the program's overall strategic planning, management approach, priorities across research areas, resource allocation, and individual project performance.

The recommendations of these expert reviewers are used by the Solar Program staff to conduct and update out-year planning for the program and technology platforms. It is a critical element of responsible portfolio and program management.

This report includes a description of the review process, a summary of the evaluation and recommendations, the program staff's response to those comments and recommendations, and a brief review of the results of the subprogram reviews and the review panel's feedback.

Additional details on the 2010 program peer review meeting, including presentations, are available on the Solar Program Web site at: http://www.eere.energy.gov/solar/review_meeting/program_review_meeting_2010.html.

I would like to express my sincere appreciation to the external reviewers that made this year's Program Review possible. Their work and commitment to this process will help ensure that the SETP continues to lead in the innovation and commercialization of solar technologies and fulfills the clean energy objectives for the Obama Administration. Thank you for participating in the 2010 U.S. DOE Solar Energy Technologies Program review meeting.

Sincerely,

John Lushetsky
Solar Energy Technologies Program Manager
Office of Energy Efficiency and Renewable Energy
U.S. Department of Energy

Table of Contents

Executive Summary

2010 Program Peer Review
U.S. Department of Energy
Solar Energy Technologies Program

On May 24-27, 2010, the U.S. Department of Energy (DOE), Office of Energy Efficiency and Renewable Energy (EERE), Solar Energy Technologies Program (SETP or Solar Program) conducted its annual program peer review at the Omni Shoreham Hotel located in Washington, D.C. In accordance with the *EERE Peer Review Guide*, the review provides an independent, expert evaluation of the strategic goals and direction of the program and is a forum for feedback and recommendations on future program planning.

The DOE Solar Energy Technologies Program is investing in an impressive portfolio of science and engineering research projects, grid integration and market transformation activities; and working with highly qualified researchers from academia, industry, and national laboratories. The program is staffed by highly skilled and talented individuals charged with managing a very complex research, development, demonstration, and deployment (RDD&D) portfolio.

The 2010 Solar Energy Technologies Program Review was organized via subprogram areas and was divided into the following five panels or sessions: Concentrating Solar Power (CSP), Photovoltaics Near Term (PVNT), Photovoltaics Long Term (PVLT), Systems Integration (SI), and Market Transformation (MT). Each panel for the program review was comprised of six external experts in the field, including a panel chair and five expert peer reviewers. Additionally, one overall chairperson was selected to oversee each of the subprogram panels and the entire program review process. The lead reviewers of each panel have provided oversight and guidance to ensure consistency, transparency, and independence throughout the review process. Detailed evaluations are provided in this *2010 Solar Energy Technologies Program Peer Review Report*.

Key Findings and Recommendations

The key findings and recommendations represent lessons learned regarding the Solar Energy Technologies Program (SETP) as a whole as well as each subprogram. Below are the Key Findings and Recommendations provided by members of the 2010 Solar Energy Technologies Program Review Panel.

Solar Energy Technologies Program – Overall Key Findings and Recommendations

- Certain efforts in the Systems Integration (SI) and Photovoltaic (PV) subprograms should be integrated more closely and focus on lowering overall systems costs. The Program has recognized this gap and has initiated efforts to create a new program called the “Systems Collaborative.” This new activity is aimed at integrating SI and PV subprogram efforts.

- EERE should formulate appropriate terminology to distinguish among photovoltaics, domestic/commercial hot water, concentrating solar thermal power, and solar. Once these terms are agreed upon, EERE should craft definitions for general in-house use, propagate them to the various laboratories and contractors, incorporate them in FOAs, and rigorously stick to this usage. In particular, PV and solar are not synonyms.
- A standard definition for the term "system," as it relates to photovoltaics, should be developed. Developing "best practices" is most likely the most effective first step towards developing a standard.
- The Panel feels that there are several issues that need to be addressed by the Program regarding manufacturing. Even though technologies are researched and developed in the U.S., a substantial amount of the products are actually manufactured overseas. The U.S. needs to focus on developing manufacturing expertise.
  - Research on novel manufacturing methods should be undertaken. Opportunities exist in coupled systems/manufacturing research, which would exploit novel systems or component designs that lower manufacturing complexity and cost.
  - For manufacturing, in-line testing is very important. The National Renewable Energy Laboratory (NREL) and Sandia National Laboratories (Sandia) have developed methods to optimize methods and operations. Those techniques must be harvested from the laboratories and applied to the industry.
- The Review Panel feels that it is important to maximize the benefit to the U.S. of intellectual property developed by government funding. We recognize that this is a complex issue and that there are many trade-offs with the need for open and free exchange of information and with a free market. Nonetheless it is important to initiate a study on how best to benefit U.S. industry, and possible program mechanisms that can be put in place to achieve that goal. Among some of the intellectual property concerns highlighted during this review, the Review Panel noted that:
  - for cases where government funded technology is acquired by foreign entities, it is very difficult to contractually bind companies to retain the intellectual property in the U.S. and not sell to foreign entities. It was concerning to researchers that this information is now lost to U.S. entities when foreign companies purchase the intellectual property. It is not just that the information goes overseas; it is the fact that the information is lost and no longer available to the U.S. The Panel feels that it would be desirable for this information to remain as open domain rather than to be lost entirely to a foreign entity.
- There needs to be a better mix of novel, early stage, exploratory research included in all areas of the Program's portfolio. This upstream, innovative research is important for filling the downstream value chain. Currently, the emphasis is almost entirely on RDD&D to commercialize existing technology, whereas future winner(s) in solar electricity may well be based on entirely new technology.
- The balance of funding tends to lean heavily on the industry, then the national laboratories, and finally universities (in that order) for each of the subprogram areas.
  - The Program is underutilizing an important resource in that there is a vast pool of talent and ideas in U.S. universities particularly suitable to the upstream research

described in the preceding bullet. Of course national labs and industry can and should also contribute to the upstream work.

- The Solar Repowering and Retrofit Program was recognized as an excellent way to introduce central receivers to the utility market 30 years ago, and it still has the advantage of not requiring the funding for the power block, balance of plant, and utility tie line. Documents from this program should be made available online.
- The Multi-Year Program Plan (MYPP) is out of date and should be updated whenever a major Program event happens.
- EERE should follow basic research in solar fuels, i.e., direct conversion of solar energy into useful fuels, with the idea of including research programs in this area in their portfolio at an appropriate time in the future.

Photovoltaics Subprogram – Key Findings and Recommendations

- Gaps that need to be addressed by the Photovoltaics subprogram are:
  - Increasing funding support for basic research in order to generate the talented researchers and the backlog of ideas that will be required in the future.
  - Devoting more attention to the NREL process development laboratory. Significant funding has been invested in this facility, and the Program should find the most effective use of it.
  - Focusing efforts on fewer programs and allocating more concentrated levels of funding and effort to the technologies that have the potential to reach the $/W cost goals in the targeted timeframe. Effective work can be done to reduce costs at the system level.
- Various reviewers view the role of NREL quite differently. NREL has a double role as (1) a developer and keeper of basic science and technology related to renewable energy, and (2) as the national resource for applying this knowledge to the solution of basic problems encountered in the commercialization and manufacturing of renewable energy products.
  - Basic science and technology would include computational, modeling, and metrology efforts. Some flexibility for working on short term theory and modeling projects for needs that arise during the year should be available from a contingency fund at the lab director's discretion. How often such a fund would be used and the redirection of yearly goals would need to be handled carefully.
  - Technology assistance projects would focus on solving problems common to technology implementation and manufacturing (barriers, contacts, failure analysis) and perhaps on improving specific materials which are already in commercial use. It seems that a lot of wasted industrial effort could be eliminated if it were possible to provide expert consulting services (on a day or two basis) from NREL staff at minimal cost and on a best effort basis.

  - The Process Development and Integration Laboratory (PDIL) has had significant funding invested in it, but seems to be underutilized. Increasing technology transfer projects and cooperative development agreements may present an avenue to increase funding, staff, and use of the equipment at the facility.
    - There are some internal projects and uses that should be supported as a basic part of the PDIL. The concept of making a best device, and then selectively degrading it to help close the gap between hero results and manufacturing results, is a powerful method to drive efficiency up and costs down. A strong interaction with the computational and modeling group to close the theory and practice loop can only be done in a facility such as NREL and is an effective way to save a lot of experiment time and money.
- Some of the Technology Pathway Partnership (TPP) projects were considered to be quite impressive. The TPP funding seems to have hastened the pace of CPV development for moderately large systems. It appears that there is now more available manufacturing capacity than there is demand for such systems. The unproven cost and reliability of such systems is part of the acceptance problem. To reduce costs, volume manufacturing needs to occur, which means the systems need to be purchased. It looks like some level of cost reduction or incentives might be required to get this effort off the ground.
- NREL measurement and characterization activities are immensely important and cannot be done as effectively elsewhere; however, the lab does not have performance measures in place to evaluate how they are performing or to provide them with feedback regarding what their customers think.
- There is strong support for the CRADA Program. The Review Panel feels that is a valuable and worthwhile effort, but there are some concerns over need to develop processes that give fairness of opportunity to the private sector.
- Moisture impermeable barriers seem to be needed to make thin film (CIGS, CdTe) and OPV cells reliable enough for long term service.
- Anti-reflection (AR) coating improves efficiency enough that it should be included in panels. The cost of AR coating is being addressed by several groups including Xerocoat. There may be some scope for combining the AR coating and the moisture barrier into the same film.
- Contact quality and degradation seem to be the primary reliability problems for thin film technologies. The barrier layers protect the contact/semiconductor interface. Improved materials and metallurgy may be helpful here. Metallurgy changes will need to go through extensive reliability testing before they will be accepted.
- There are several ITO/top contact replacement schemes in the different projects. They are aimed at reducing degradation, replacing indium, improving transmission, or some combination of effects. Coordination of contact degradation and transparent contact efforts seem like a necessity, since they are so closely related.

Concentrating Solar Power Subprogram – Key Findings and Recommendations

- There have been many years of low funding for CSP (and several years of zero funding for towers). CSP must achieve support parity with other renewables such as PV, wind, and biomass because of its potential to achieve a low cost of energy and the low actual risk associated with the program (in contrast to the perceived high risk by the financial industry) by substantially increasing the funding for CSP technologies over the next decade.
    - If possible, the Program needs to buy down risk. Work on the Solar Zone and Solar Loan Guarantees is clearly a step in the right direction.
    - The Program should continue funding projects that offer a lower levelized cost of energy (LCOE). This is in large part being done now, and the Department of Energy is well aware of this. It would help if opportunities can also be developed for technologies that are longer term, show promise, involve foreign participation, and have the potential for export sales.
    - Laboratory testing, demonstration, and certification of solar components and subsystems are also very important facilitators of progress toward bankability. However, larger demonstrations are required and these activities will not be funded by banks. The DOE should help fill this void.
    - There should be add-on funding to the CSP subprogram for services to respond to global warming and peak oil issues.
- The Program needs to take advantage of CSP technology and its ability to store energy. CSP can control the shoulders while PV handles only the peak load.
- For the CSP subprogram, stress and strain properties of receiver materials should be addressed by the national laboratories. National laboratories operate excellent materials facilities, and industry is not required to conduct much of the research in this area.
- The Reviewers who read the Solar Vision Study felt that it had very little emphasis on CSP or solar thermal technologies. The feasibility and societal benefits of these technologies need to be brought strongly to the attention of everyone.
- The joint programs with the International Energy Agency would better serve the U.S. citizenry if the emphasis were focused on those programs that generate U.S. jobs rather than restricting contracts to nominal U.S. companies, i.e., those with an office in the U.S. but little or no staff, employees, or capabilities.
- CSP will be largely manufactured, installed, operated, and serviced in the U.S, and this presents a great opportunity to create American jobs (unless we manage to fall far enough behind that all equipment is imported).
- CSP has the potential to quickly offset carbon production and at relatively low cost (augmentation/retrofit; relatively low, if not the lowest, LCOE of any solar power system), and with a high degree of dispatchability.

- The addition of a solar field to a conventional power plant to decrease the amount of required new energy infrastructure purchases can be a good investment for the right kind of utility situation. Most of the infrastructure will already be in place except for the solar field and some method of "bolting" it to the conventional plant.
- Of all of the subprograms, CSP has the least to lose and the most to gain from foreign collaborations, and these collaborations should be encouraged. Significant gains and benefits to the CSP market potential can be achieved through international collaborations. Asia could be a target for much of this, as it is developing CSP much the same way the United States is.
- Other than some involvement in the thermal storage aspects of the program, almost no university presence exists. While there is obviously some ease of operational aspects for DOE to do this, it greatly stifles a well-trained employment pipeline coming through the educational system that could be enhanced greatly by active involvement of universities. The requirement that universities provide a substantial cost share to qualify for a contract award is a major and often insurmountable problem, discouraging university investigators from even developing a proposal.
- There is a need for independent analysis and evaluation. The goals in the Multi-Year Program Plan (MYPP) are a reasonable guide to the evaluation of emerging technology solutions. However, where the projects stack up in relation to these goals currently seems to depend primarily on the assumptions made by the technology project managers in the context of the project managers' cost and economic calculations.
    - The Solar Advisor Model (SAM) is a first order tool to help assess the value of a particular innovation. However, it is clearly ill advised to rely on a company interested in future funding, especially venture funding, or looking for market allies and suppliers to be objective and conservative in using SAM to assess their own technical solution.
- The biggest roadblock for the Program is its over-emphasis of analytical attention to estimating costs versus assessing economic value. Because the DOE has cost goals and LCOE models, the emphasis is almost 100% on cost per delivered kWh. This is a completely inappropriate metric for any variable resource that can deliver on peak energy even part of the time.
    - Settling for comparative evaluations of variable renewables with one another, and with fuel based options using LCOE as a metric, results in a serious disservice to CSP at a stage where policy makers have to make choices regarding the allocation of limited resources, including the resources necessary to provide appropriate incentives in the early stages of industrialization and commercialization.
- A great deal of very good work was done under DOE support in the past 38 years, prior to the advent of the Internet and electronic storage. However, the "new generation" of solar workers remains essentially unaware of the lessons learned and information generated back then. At the very least, titles, authors, and abstracts of the Sandia/NREL

libraries should be available in a searchable format on line. Meaningful reports should also be scanned in searchable PDF format.

Systems Integration Subprogram – Key Findings and Recommendations

- A definition for what it means to "integrate solar into the grid" should be developed and coded into law.
- In the past, both the Systems Integration and Market Transformation subprograms were addressing codes and standards. Moving forward, the Systems Integration subprogram will be responsible for the development of codes and standards.
    - This has been a transitional year, and the Program is attempting to pull together analysis of PV and CSP activities in order to get codes and standards in place. CSP storage will also be included in this analysis.
- More work is required in the area of high penetration. It appears to the Review Panel that only utilities and laboratories are involved in these activities. Additionally, the Solar Program needs to develop a definition for "high penetration".
- Additional collaboration with utilities, the Electric Power Research Institute, and the federal government is required.
- The Systems Integration subprogram needs to ensure that outreach efforts are reaching the correct audience. Additionally, the Solar Program should increase the level of state and local outreach activities.
- Issues related to operating the grid with variable resources and how to model specific dynamic variabilities are currently being studied. Additionally, a study on how a CSP central station with storage addresses these issues should be conducted. The Program should also look at forecasting issues.
- Some of the Systems Integration projects are in the very early stages of development and minimal data were available on criteria such as accomplishments.
- It would be useful if the Systems Integration activities were disseminated to the CSP community. The CSP subprogram needs to be informed that the SI subprogram will include activities related to concentrating solar power technologies.
- It is difficult to rate projects that are just initiating their work. The Program should consider removing those projects in future reviews.

Market Transformation Subprogram – Key Findings and Recommendations

- The Market Transformation program has done an excellent job of moving high-performing communities to the next level of program implementation. This provides a critical blueprint for others to follow.
- The industry, national lab, and non-governmental partners are doing essential research and development work around the regulatory, policy, and permitting arenas which will strongly position the program for what may be the next critical step in development: the establishment of a package of essential regulatory ingredients, sound policies to drive the market, and recommended approaches for streamlining and reducing the costs of permitting.

  - The Review Panel recommends that these essential elements be introduced to the non-high performing areas of the country to help develop the necessary national market that will move PV integration from a successful pilot in isolated areas of the country to a geographically diverse and consistent market throughout the country.
- By pulling in the non-traditional players in the renewable energy market, the Market Transformation effort will enjoy multiple benefits, including:
  - Allowing for large-scale players to enter the market without a patchwork of regulatory barriers that drive up costs and reduce opportunities for broad scale implementation.
  - Driving localized manufacturing to create more American jobs to provide PV resources.
  - Building a national consensus through the success of employment and job creation associated with the renewable energy industry. This type of consensus would greatly assist national legislative efforts which are currently suffering from balkanized support around the country, particularly in the southeast states.
- Building a successful foundation of regulation, permitting, and policy will support the important research being done at the labs. As efficiencies increase, BIPV opportunities expand, and costs per watt are reduced, it is essential that the market is prepared for using these advanced technologies.
- NREL & Sandia are producing critical information that state energy offices, regulatory entities, and incentive programs would benefit from, yet there is a lack of sufficient outreach to these players throughout the country to drive these best practices home. An effort should be made to find the champions within states – whether they are utilities, administrations, legislators, or others – and develop state-specific strategies for implementing these best practices. In some cases it will be at the large IOUs, at others municipal and rural utilities, and/or the legislative or initiative process.
- The Interstate Renewable Energy Council (IREC), Solar Electric Power Association and the National Association of Regulatory Utility Commissioners (NARUC) are providing an essential service to Public Utilities Commissions (PUCs) around the country to advance utility policies. However, the Review Panel did not hear any of the Principal Investigators discuss municipal utilities and rural electric cooperatives.
  - The PUCs are certainly the most centralized mechanism for effecting large-scale change, but the Program needs to implement a process for driving these policies to the other, public utilities. Also, IREC was limiting their interaction to the regulatory process at the PUCs, yet the direction to those PUCs is often set in the statutes and frequently there are PUCs that are not willing to move on renewable energy policies without the expressed intent of the legislature firmly established. Finally, PUCs may need legislation to allow them to address electric delivery issues in a more comprehensive manner. By clarifying through statute what the goals of the PUC are, legislation can guide PUCs to more advanced energy opportunities.
- The Clean Energy Group is providing critical outreach to those states that have entities, both governmental and non-governmental, that are committed to renewable energy both from an infrastructure and financial perspective (membership includes most dedicated system benefits funds). They could be used to reach outside of their limited scope of

influence to drive policies both nationally and at the state level to increase the establishment of resources dedicated to the advancement of all renewable resources.

- The National Conference of State Legislatures and NARUC are required to provide information to all of their members without a role of advocacy. While their service is essential to provide the non-biased information that will advance the PV market, they cannot be relied upon to be advocates and should be paired with other entities that can fulfill that role.
- The North American Board of Certified Energy Practitioners provides the de facto standard of assurance that installers have a level of competency that is critical to avoiding market killing bad actors from becoming widespread. An effort should be made to determine how best to utilize their established standards without becoming burdensome to the industry and driving smaller players out of the market. Colorado has provided an attempt at this balance, and their process could be followed and evaluated for its effectiveness to determine best practices for providing a level of consumer protection currently not enjoyed in the industry.
- Assessing accomplishments for the Solar America Cities projects must take into account the city's starting point, including regulatory issues, markets, etc. Some coaching at the beginning may have helped start each city at a comparable baseline. The Program should provide leadership and coaching to get each project started on the right foot.
  - Along the lines of coaching, the Program should foster additional collaborative meetings between the cities.
  - Fostering sessions with each solar city lead to go over strategic planning could be the best approach.
- It was hard for the Review Panel to determine where non-Solar America Cities projects fit in with the rest of the subprogram. The subprogram needs to ensure that these projects correlate to what is going on at the city level.
- Some cities tried to work on many fronts at same time; others picked one issue and focused on that. The Review Panel felt that it would be best for a city to single out one really significant thing that can be replicated: its program should focus on that.
- Many cities stated that collaboration was an accomplishment, but it was difficult for the Review Panel to determine the results of the collaboration efforts. Key stakeholders, including utilities, need to be brought in to make sure that the collaboration leads to partnerships.
- An information exchange program and additional strategic planning would be beneficial. This is part of the plan going forward to expand the program. There are lessons learned from the 25 initial cities. With benchmarking, best practices for new cities going forward can be made available.

Summary of Platform Review Results:

The following series of tables ES-1A-E represents the results of the individual project evaluations for each subprogram area. Each table identifies the subprogram, and each project is identified by a unique code (PeerNet Number) for the project. Additional information on each project includes: the project title, presenting organization, and Principal Investigator's last name. Each project was reviewed by no less than three reviewers in five scored review criteria. The Average Overall Score represents an aggregation of all five criteria.

Table ES-1A: Summary of the Photovoltaic Near Term Project Portfolio

PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments / progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
PVNT001	NREL & Sandia Photovoltaics Research	Etzkorn; DOE	3.0	2.7	2.7	3.3	3.0	2.8
PVNT004	Efficiency Enhancing Layers for Photovoltaic Modules	Tsakalakos; General Electric	3.0	2.6	2.6	2.8	2.8	2.7
PVNT005	High Energy Yield Distributed Architecture for Large Commercial and Utility-Scale PV Systems	Elasser; General Electric	2.8	3.2	2.2	2.8	2.6	2.6

Table ES-1A: Summary of the Photovoltaic Near Term Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments / progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
PVNT006	Low-cost, High-throughput Si Epitaxy System for Solar Cell Manufacturing	Fu; Sierra Solar Power	2.2	2.6	2.8	2.2	2.8	2.6
PVNT007	Novel Kerf-Free PV Wafering that Provides a Low-Cost Approach to Generate Wafers from 150 mm to 50 mm	Brailove; Silicon Genesis	2.8	3.4	3.0	2.8	2.8	3.0
PVNT008	Floating Silicon Method (FSM)	Kellerman; Varian Semiconductor	3.6	3.2	2.8	2.8	3.2	3.1
PVNT009	Industrial CRADAs	Benner; NREL	3.8	3.3	3.5	4.0	3.3	3.5
PVNT010	A Low Cost Solar PV Anti-reflection Coating	McAllister; Xerocoat	3.2	3.4	2.8	3.4	3.0	3.1
PVNT011	Flexible Barrier Films	Brown; 3M	2.8	3.0	2.8	3.0	2.8	2.9

Table ES-1A: Summary of the Photovoltaic Near Term Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments / progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
PVNT012	Enhanced Growth Rate and Silane Utilization in a-Si and nc-Si Solar Cell Deposition via Gas Phase Additives	Hurley; Air Products	2.8	3.4	3.0	3.0	2.8	3.0
PVNT013	Flexible Ultra Moisture Barrier for Thin-Film Photovoltaic Applications PV	Wang; Dupont	3.2	3.2	3.2	3.0	3.4	3.2
PVNT015	Organic Photovoltaics	Lloyd; NREL	3.0	3.3	3.0	3.3	3.3	3.1
PVNT016	Low Cost, Lightweight Solar Modules Based on Organic Photovoltaic Technology	Gaudiana; Konarka	2.2	2.2	2.2	2.2	2.2	2.2
PVNT017	NREL Film Silicon Agreement	Branz; NREL	3.6	3.4	3.6	3.4	3.2	3.5

Table ES-1A: Summary of the Photovoltaic Near Term Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments / progress toward project and DOE goals (Weight = 40%)**	**Collaborations and technology transfer (Weight = 10%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
PVNT018	Low Cost Thin Film Building-Integrated PV Systems	Yang; United Solar Ovonics, Inc.	2.8	2.6	2.5	2.6	2.8	2.6
PVNT019	Wafer Si Agreement	Wang; NREL	2.6	2.6	2.4	3.6	2.4	2.6
PVNT020	Grid-Competitive Residential and Commercial Fully Automated PV Systems Technology	Peurach; Sunpower	3.4	3.8	3.8	2.8	2.6	3.5
PVNT021	Concentrating Photovoltaics	Friedman; NREL	3.0	3.0	2.8	3.0	2.8	2.9
PVNT022	High Efficiency XR-700 Concentrator Photovoltaic System	Ventura; Boeing	3.2	3.4	3.0	3.2	3.0	3.1
PVNT023	Low Cost High Concentration PV Systems for Utility Power Generation	McConnell; Amonix	2.6	2.2	2.2	2.0	2.0	2.2

Table ES-1A: Summary of the Photovoltaic Near Term Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments / progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
PVNT024	Concentrating Solar Panels: Bringing the Highest Power and Lowest Cost to the Rooftop	Deck; Soliant Energy, Inc.	2.8	3.0	2.6	3.2	2.6	2.8
PVNT025	CIGS Technology	Contreras; NREL	3.4	3.2	3.2	3.4	3.2	3.3
PVNT026	Delivering Grid-Parity Solar Electricity on Flat Commercial Rooftops	Sager; Nanosolar	3.0	2.8	2.3	2.8	2.5	2.6
PVNT027	Fully Integrated Building Science Solutions	Mills; Dow	3.2	3.2	2.8	3.0	2.6	3.0
PVNT028	Development of an AC Module System	Miles; GreenRay	2.8	2.8	2.6	2.8	2.8	2.7
PVNT029	Theory and Computational Science	Lany; NREL	3.4	3.6	3.2	3.2	3.0	3.3

Table ES-1A: Summary of the Photovoltaic Near Term Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments / progress toward project and DOE goals (Weight = 40%)**	**Collaborations and technology transfer (Weight = 10%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
PVNT030	Transparent Conducting Oxides	Perkins; NREL	3.6	3.2	3.4	3.8	3.4	3.4
PVNT031	CdTe	Gessert; NREL	3.4	3.6	2.8	3.2	3.2	3.2
PVNT032	Sensitized Solar Cells (SSCs)	Frank; NREL	2.6	3.0	2.8	2.2	2.6	2.7

Table ES-1B: Summary of the Photovoltaic Long Term Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments / progress toward project and DOE goals (Weight = 40%)**	**Collaborations and technology transfer (Weight = 10%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
PVLT001	PV Technology Incubator	Symko-Davies; DOE	3.7	3.7	3.3	3.7	3.7	3.5
PVLT002	High Efficiency Single Crystal CdTe Solar Cells	Carmody; EPIR Technologies	3.0	3.0	3.3	1.8	2.5	2.9
PVLT003	Novel, R2R Manufacturable, Photonic Enhanced Thin Film Solar cells	Dalal; Lightwave	3.0	3.3	2.8	2.8	2.5	2.9
PVLT004	High Efficiency Organic Solar Cells	Joslin; Luna	2.8	3.0	3.0	3.3	3.0	3.0
PVLT005	Thin Single Crystal Silicon Solar Cells on Ceramic Substrates	Ravi; Crystal Solar	3.3	3.8	3.0	2.0	3.3	3.1

Table ES-1B: Summary of the Photovoltaic Long Term Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments / progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
PVLT006	High Efficiency, Low-Cost, Multijunction Solar Cells Based on Epitaxial Liftoff and Wafer Bonding	Tatavarti; Microlink	3.5	3.5	3.3	3.0	3.0	3.3
PVLT007	Innovative Manufacturing of Dye Sensitized Solar Cells	Bucca; TiSol	2.3	2.0	2.5	1.3	1.5	2.1
PVLT008	ATIR Optics for Solar	Schultz; Banyan	3.0	2.8	2.8	3.0	2.5	2.8
PVLT009	Improved Solar Cell Efficiency Through the Use of an Additive Optical Downshifter	Kurtin; SpectraWatt	3.0	3.0	3.3	3.0	3.3	3.1
PVLT010	ZnMgO by APCVD Enabling High-Performance Mid-Bandgap CIGS on Polyimide Modules	Woods; Ascent	3.3	3.3	3.3	3.0	3.3	3.2

Table ES-1B: Summary of the Photovoltaic Long Term Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments / progress toward project and DOE goals (Weight = 40%)**	**Collaborations and technology transfer (Weight = 10%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
PVLT011	Process Development and Integration Laboratory	Nelson; NREL	3.5	3.5	3.3	3.0	3.0	3.3
PVLT012	Measurements and Characterization	Sheldon; NREL	4.0	4.0	4.0	4.0	4.0	4.0
PVLT013	PV Technology Incubator: Round 3	Mapes; DOE	4.0	3.3	3.3	3.7	3.7	3.5
PVLT017	Nanocoax Solar Cells	Naughton; Solasta	3.8	3.8	3.5	3.0	2.5	3.5
PVLT018	The Self Aligned Cell: Scaling Up Manufacture of a Cost Effective Cell Architecture for Multicrystalline Silicon Photovoltaics	Gabor; 1366	3.0	3.5	3.3	3.3	2.8	3.2

Table ES-1B: Summary of the Photovoltaic Long Term Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments / progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
PVLT019	Productization and Manufacturing Scaling of High-Efficiency Solar Cell and Module Products Based on a Disruptive Low-Cost, Mono-Crystalline Technology	Fatemi; Solexel	3.0	3.0	3.3	1.5	3.0	3.0
PVLT020	High-Efficiency, Low-Cost Solar Cells Manufactured Using “Silicon Ink” On Thin Crystalline Silicon Wafers	Antoniadis; Innovalight	3.0	3.3	3.3	1.8	2.5	3.0
PVLT021	High-Efficiency Bi-Facial Concentrator Solar Cells	Wojtczuk; Spire	2.8	3.5	3.5	2.8	3.3	3.3
PVLT022	University Product and Processes Development Program	Mapes; DOE	3.7	3.3	3.3	3.7	3.7	3.5

Table ES-1B: Summary of the Photovoltaic Long Term Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments / progress toward project and DOE goals (Weight = 40%)**	**Collaborations and technology transfer (Weight = 10%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
PVLT023	Organic Semiconductor Heterojunction Solar Cells for Efficient, Low Cost, Large Area Scalable Solar Energy Conversion	Grimes; Penn State University	2.3	2.8	2.8	3.0	2.5	2.7
PVLT024	Reliability Evaluation of Concentrator Photovoltaic Modules per IEC Qualification Specifications	TamizhMani; Arizona State University	2.8	3.0	2.3	2.3	1.8	2.5
PVLT025	Defect Engineering, Cell Processing, and Modeling for High-Performance, Low-Cost Crystalline Si PV	Buonassisi; MIT	3.3	3.5	3.3	3.3	2.8	3.3

Table ES-1B: Summary of the Photovoltaic Long Term Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments / progress toward project and DOE goals (Weight = 40%)**	**Collaborations and technology transfer (Weight = 10%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
PVLT026	High-Rate Fabrication of a-Si-Based Thin-Film Solar Cells Using Large Area VHF PECVD Processes	Fan; University of Toledo	3.3	3.5	3.3	2.8	3.3	3.3
PVLT027	Development of Rear Contact Technologies for Next Generation High-Efficiency Commercial Silicon Solar Cells	Rohatgi; Georgia Tech University	3.8	3.8	3.5	3.3	3.5	3.6
PVLT028	University Product and Process Development Support	Compaan; University of Toledo	3.8	3.5	3.5	3.8	3.5	3.6
PVLT029	Development of a Low Cost Insulated Foil Substrate for Cu (InGa)Se2 Photovoltaics	Eser; University of Delaware	3.0	3.3	3.0	2.8	3.3	3.1

Table ES-1B: Summary of the Photovoltaic Long Term Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments / progress toward project and DOE goals (Weight = 40%)**	**Collaborations and technology transfer (Weight = 10%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
PVLT030	Routes for Rapid Synthesis of CuGaxIn1-xSe2 Absorbers	Anderson; University of Florida	3.8	3.5	3.5	3.5	3.5	3.6
PVLT031	High Efficiency Back Contact Si Heterojunction Solar Cells	Das; University of Delaware	3.3	3.5	3.0	3.0	2.8	3.1
PVLT032	100 mm Engineered InP-on-Si Laminate Substrates for InP-based Multijunction Solar Cells	Atwater; Caltech	3.0	2.5	2.8	2.8	2.5	2.7
PVLT033	Tunable Narrow Band Gap Absorbers For Ultra High Efficiency Solar Cells	Bedair; North Carolina State University	3.3	3.5	3.3	3.0	3.0	3.3

Table ES-1C: Summary of the Concentrating Solar Power Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments/ progress toward project and DOE goals (Weight = 35%)**	**Collaborations and technology transfer (Weight = 15%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
CSP001	Line-Focus Systems Program Team: CSP	Kutscher; NREL	4.0	3.4	4.0	3.4	3.4	3.7
CSP002	Line Focus Systems	Moss; SNL	3.4	3.2	2.8	3.4	2.8	3.1
CSP003	NREL System Analysis	Turchi; NREL	3.4	3.0	3.0	3.0	2.6	3.0
CSP004	Dish Research and Development	Andraka; SNL	3.4	3.0	3.4	3.2	2.6	3.2
CSP005	Design of a High-Temperature Molten Salt Linear Fresnel Collector	Brost; SkyFuel	3.2	3.4	3.2	1.8	2.8	3.0
CSP006	Cleanable and Hardcoat Coatings for Increased Durability of Silvered Polymeric Mirrors	Clear; 3M	3.2	3.6	3.2	2.2	2.8	3.1

Table ES-1C: Summary of the Concentrating Solar Power Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments/ progress toward project and DOE goals (Weight = 35%)**	**Collaborations and technology transfer (Weight = 15%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
CSP007	Development of Next-Generation Parabolic Trough Collectors and Components for CSP Applications	Marcotte; Abengoa	2.6	2.2	2.4	1.8	1.8	2.3
CSP008	Reflector Technology Development & System Design for CSP Technologies	Schaut; Alcoa	3.4	3.4	3.2	2.8	3.0	3.2
CSP009	Advanced High Temperature Trough Collector Development	Dracker; Solar Millennium	3.0	3.2	3.2	2.2	2.6	3.0
CSP010	Development of Advanced Polymeric Reflector for CSP Applications	Smilgys; Abengoa	3.2	2.4	2.6	2.6	2.2	2.6

Table ES-1C: Summary of the Concentrating Solar Power Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 35%)	Collaborations and technology transfer (Weight = 15%)	Proposed future research (Weight = 10%)	Final Average Score
CSP011	Brayton Solar Power Conversion System	Kesseli; Brayton	2.4	2.4	2.6	2.4	2.4	2.5
CSP012	Indirect, Dual-Media, Phase Changing Material Modular Thermal Energy Storage System	Newmarker; Acciona	2.3	1.8	1.5	1.0	1.5	1.6
CSP013	Heat Transfer and Latent Heat Storage in Inorganic Molten Salts for Concentrating Solar Power Plants	Mathur; Terrafore	3.0	3.0	2.6	2.8	2.4	2.8

Table ES-1C: Summary of the Concentrating Solar Power Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 35%)	Collaborations and technology transfer (Weight = 15%)	Proposed future research (Weight = 10%)	Final Average Score
CSP014	Innovative Application of Maintenance-Free Phase-Change Thermal Energy Storage for Dish Engine Solar Power Generation	Qiu; Infinia	4.0	3.4	3.0	3.0	3.2	3.3
CSP015	Molten Salt-Carbon Nanotube Thermal Energy Storage for Concentrating Solar Power Systems	Banerjee; Texas A&M University	3.6	3.6	3.2	2.4	3.0	3.2
CSP016	CSP: Tower R&D	Kolb; SNL	3.8	3.8	3.8	3.6	3.2	3.7

Table ES-1C: Summary of the Concentrating Solar Power Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 35%)	Collaborations and technology transfer (Weight = 15%)	Proposed future research (Weight = 10%)	Final Average Score
CSP017	Development and Performance Evaluation of High Temperature Concrete for Thermal Energy Storage for Solar Power Generation	Selvam; University of Arkansas	2.4	2.4	2.6	1.8	2.4	2.4
CSP018	CSP Energy Storage – Multiple Technologies Compared	Jeter; US Solar	2.2	2.0	1.6	2.2	2.0	1.9
CSP019	Novel Molten Salts Thermal Energy Storage for Concentrating Solar Power Generation	Reddy; University of Alabama	3.4	3.4	3.0	2.0	3.0	3.0

Table ES-1C: Summary of the Concentrating Solar Power Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 35%)	Collaborations and technology transfer (Weight = 15%)	Proposed future research (Weight = 10%)	Final Average Score
CSP020	Deep Eutectic Salt Formulations Suitable as Advanced Heat Transfer Fluids	Raade; Symyx/ Halotechnics	3.8	3.8	3.4	2.4	2.8	3.4
CSP021	High Performance Reflector Panels for Concentrating Solar Power Assemblies	McCamy; PPG	3.8	4.0	3.6	3.0	3.2	3.6
CSP022	Tower Receiver Development Kris Miner	Miner; Pratt and Whitney	2.6	2.4	2.2	1.6	2.4	2.3
CSP023	Sensible Heat, Direct, Dual-Media Thermal Energy Storage Module	Newmarker; Acciona	2.0	2.0	1.8	1.3	1.5	1.8

Table ES-1C: Summary of the Concentrating Solar Power Project Portfolio

PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 35%)	Collaborations and technology transfer (Weight = 15%)	Proposed future research (Weight = 10%)	Final Average Score
CSP024	Research and Development for Novel Thermal Energy Storage Systems (TES) for Concentrating Solar Power (CSP)	Bergman; University of Connecticut	3.2	2.6	2.4	2.2	2.4	2.6
CSP025	Advanced CSP R&D: Advanced Reflectors	Kennedy; NREL	4.0	3.8	3.6	4.0	3.6	3.8
CSP026	Advanced CSP R&D	Turchi; NREL	3.2	3.0	2.8	3.2	2.4	2.9
CSP027	Advanced CSP R&D: Advanced Absorbers	Kennedy; NREL	3.4	3.8	3.2	3.6	3.4	3.4
CSP028	Sandia Advanced Concepts	Ho; SNL	3.8	3.5	3.3	3.8	3.3	3.5
CSP029	Thermal Energy Storage	Glatzmaier; NREL	3.4	3.2	3.0	2.6	3.0	3.1

Table ES-1C: Summary of the Concentrating Solar Power Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 35%)	Collaborations and technology transfer (Weight = 15%)	Proposed future research (Weight = 10%)	Final Average Score
CSP030	Thermal Energy Storage: Systems and Components	Siegel; SNL	3.8	3.6	3.0	3.2	3.2	3.3
CSP031	NREL Advanced Fluids Thermal Energy Storage	Glatzmaier; NREL	3.6	3.0	2.6	3.0	2.6	2.9
CSP032	Advanced Heat Transfer Fluid Development	Bradshaw; SNL	3.8	3.4	3.0	3.2	3.2	3.3

Table ES-1D: Summary of the Systems Integration Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
SI001	System Modeling - NREL	Dobos; NREL	4.0	3.6	3.8	3.4	3.2	3.7
SI002	Systems Modeling	Cameron; SNL	3.8	3.6	3.2	3.4	3.0	3.4
SI003	NREL PV Grid Integration	Kroposki; NREL	3.8	3.8	3.6	3.2	3.6	3.6
SI004	PV Grid Integration	Ellis; SNL	3.8	3.8	3.6	3.6	3.6	3.7
SI005	CSP Grid Integration	Parsons; NREL	3.6	2.8	2.8	2.6	3.2	3.0
SI006	Solar America Board for Codes and Standards	Sherwood; New Mexico State University	3.8	3.6	3.4	3.4	3.4	3.5
SI007	NREL Codes & Standards Lab Support	Basso; NREL	4.0	3.8	3.8	3.6	3.4	3.8
SI008	Solar Codes and Standards Support	Bower; SNL	4.0	3.8	3.4	3.6	2.8	3.6

Table ES-1D: Summary of the Systems Integration Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
SI009	Solar Radiometry and Modeling	Myers; NREL	4.0	3.6	3.6	3.6	3.6	3.7
SI010	Solar Resource Characterization	Renne; NREL	4.0	3.8	3.8	3.2	3.4	3.7
SI011	Southwest Region Experiment Station	Rosenthal; New Mexico State University	3.6	3.6	3.6	3.4	3.8	3.6
SI012	Test and Evaluation Activities	Reedy; FSEC	3.0	2.4	2.8	3.0	2.4	2.7
SI013	Systems Analysis	Margolis; NREL	3.4	3.4	3.6	3.2	3.0	3.4
SI014	Reliability R&D - NREL	Kurtz; NREL	4.0	3.8	3.8	3.6	3.6	3.8
SI015	Reliability	Granata; SNL	4.0	4.0	4.0	3.6	3.6	3.9
SI016	NREL PV Test and Evaluation	Marion; NREL	4.0	4.0	3.8	3.6	3.6	3.8
SI017	Test & Evaluation	Granata; SNL	4.0	3.8	3.8	3.2	3.4	3.7

Table ES-1D: Summary of the Systems Integration Project Portfolio

PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
SI018	Development, Validation and Commercialization of Grid Smart Inverters for Wider Photovoltaic Technology Utilization	Reedy; FSEC/Satcon	3.2	3.0	2.6	3.2	2.4	2.8
SI019	Development of Economically Viable Highly Integrated, Highly Modular SEGIS Architecture	Mensah; Petra Solar	2.8	2.6	2.6	2.2	2.4	2.6
SI020	SEGIS Smart Grid Inverter Systems Integration	Pfeifer; Apollo Solar	3.6	3.4	3.6	3.2	3.4	3.5
SI021	100kW Demand Response Inverter (DRI)	Hammell; Princeton Power	3.0	3.0	2.8	3.3	2.3	2.9

Table ES-1D: Summary of the Systems Integration Project Portfolio								
PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the R&D (Weight = 20%)	Technical accomplishments/ progress toward project and DOE goals (Weight = 40%)	Collaborations and technology transfer (Weight = 10%)	Proposed future research (Weight = 10%)	Final Average Score
SI022	PV Inverter Meets Smart Grid	Scharf; PV Powered	3.6	3.4	2.8	2.8	3.2	3.1
SI023	Smart Grid Photovoltaic Pilot	Freestone; ComEd	3.6	3.2	3.2	3.2	3.4	3.3
SI024	SMUD PV and Smart Grid Pilot at Anatolia	Rawson; SMUD	3.8	3.6	3.4	3.4	3.4	3.5
SI025	Analysis of High-Penetration Levels of PV into the Distribution Grid in California	Kroposki; NREL	4.0	3.6	3.6	3.6	3.6	3.7
SI026	Sunshine State Solar Grid Initiative	Meeker; Florida State University	3.4	2.8	2.2	3.2	2.6	2.7
SI027	Improved Modeling Tools Development for High Penetration Solar	Washom; UCSD	3.8	3.8	3.4	3.4	3.6	3.6

Table ES-1D: Summary of the Systems Integration Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the R&D (Weight = 20%)**	**Technical accomplishments/ progress toward project and DOE goals (Weight = 40%)**	**Collaborations and technology transfer (Weight = 10%)**	**Proposed future research (Weight = 10%)**	**Final Average Score**
SI028	High Penetration of Photovoltaic Generation Study – Flagstaff Community Power	Narang; APS	3.5	3.0	3.0	3.5	3.3	3.2

Table ES-1E: Summary of the Market Transformation Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the project (Weight = 20%)**	**Accomplishments and progress toward overall project and DOE goals (Weight = 40%)**	**Collaborations and information transfer with other institutions (Weight = 10%)**	**Proposed future activity (Weight = 10%)**	**Final Average Score**
MT001	Solar America Cities – Solar Boston	Belden; Boston	2.8	3.0	3.0	2.8	2.0	2.8
MT002	Sustainable Energy 2050 Plan	Giannelli Pratt; San Diego	2.8	2.6	2.6	2.2	2.4	2.6
MT003	Solar Market Transformation in Portland, Oregon	Jacob; Portland	3.6	3.6	3.6	3.8	3.4	3.6
MT004	Solar Salt Lake Project	Baldwin; Salt Lake City	4.0	3.8	3.8	3.8	3.4	3.8
MT005	Santa Rosa & Solar Sonoma County	Wright; Santa Rosa	2.8	3.2	3.0	3.0	2.4	2.9
MT006	Solar San Francisco	Broomhead; San Francisco	3.6	3.6	3.2	3.0	2.8	3.3
MT007	Milwaukee Shines for a Sustainable Solar Economy	Luecke; Milwaukee	2.8	3.2	3.0	3.2	2.6	3.0

Table ES-1E: Summary of the Market Transformation Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the project (Weight = 20%)**	**Accomplishments and progress toward overall project and DOE goals (Weight = 40%)**	**Collaborations and information transfer with other institutions (Weight = 10%)**	**Proposed future activity (Weight = 10%)**	**Final Average Score**
MT008	Seattle: The Emerald City Solar Initiative	Irvine; Seattle	3.4	3.2	3.4	3.6	3.4	3.4
MT009	Solar America Showcases, Government Solar Installation Program (GSIP)	Stoltenberg; NREL and SNL	3.2	3.2	3.0	3.0	3.0	3.1
MT010	Market Transformation Analysis	Friedman; NREL	3.6	3.6	3.6	3.4	3.4	3.6
MT011	Austin Solar City Partnership	Libby; Austin	2.4	2.6	2.4	2.4	2.4	2.4
MT012	The City of New York Solar City Strategic Partnership	Case; New York	3.4	3.6	3.4	3.4	3.4	3.4

Table ES-1E: Summary of the Market Transformation Project Portfolio

PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the project (Weight = 20%)	Accomplishments and progress toward overall project and DOE goals (Weight = 40%)	Collaborations and information transfer with other institutions (Weight = 10%)	Proposed future activity (Weight = 10%)	Final Average Score
MT013	Linking San Jose's Green Vision and Solar Cities	Tucker; San Jose	3.3	2.8	3.5	3.8	3.3	3.3
MT015	Tucson Solar Initiative	Plenk; Tucson	3.4	3.4	3.4	3.2	2.8	3.3
MT016	Minneapolis Saint Paul Solar America Cities Program	Hunt; St. Paul	3.5	3.8	3.5	3.5	3.0	3.5
MT017	SmartSolar Program: A Partnership to Serve the East Bay	DeSnoo; Berkeley	3.4	3.6	3.2	3.6	3.0	3.3
MT018	Midwest Solar City Model (MadiSUN)	Hoffman; Madison	3.2	2.8	3.2	2.8	3.0	3.1
MT019	Solar America Cities – NREL Support	Coughlin; NREL and SNL	3.8	3.8	3.8	3.8	3.0	3.7
MT020	Technical Integration	Orr; CH2M Hill	3.2	3.6	3.2	3.4	3.0	3.3

Table ES-1E: Summary of the Market Transformation Project Portfolio

PeerNet Number	Project Title	Presenter Last Name; Organization	Relevance to overall DOE objectives (Weight = 20%)	Approach to performing the project (Weight = 20%)	Accomplishments and progress toward overall project and DOE goals (Weight = 40%)	Collaborations and information transfer with other institutions (Weight = 10%)	Proposed future activity (Weight = 10%)	Final Average Score
MT021	Solar State Technical Outreach Partnership Project	Sinclair; Clean Energy Group	3.5	3.5	3.3	3.3	2.5	3.3
MT022	Stakeholder Outreach - Workforce Development	Weissman; IREC	3.8	3.8	3.8	3.8	2.5	3.6
MT023	Strategic Growth Plan	Auerbach; NABCEP	4.0	3.8	3.5	2.8	3.0	3.5
MT024	State Legislative Outreach on Solar Technology and Policy Options	Savage; NCSL	2.0	2.5	2.0	2.3	2.3	2.2
MT025	Large Scale Integration	Parsons; NREL	3.7	3.3	3.0	3.0	3.0	3.2

Table ES-1E: Summary of the Market Transformation Project Portfolio								
PeerNet Number	**Project Title**	**Presenter Last Name; Organization**	**Relevance to overall DOE objectives (Weight = 20%)**	**Approach to performing the project (Weight = 20%)**	**Accomplishments and progress toward overall project and DOE goals (Weight = 40%)**	**Collaborations and information transfer with other institutions (Weight = 10%)**	**Proposed future activity (Weight = 10%)**	**Final Average Score**
MT026	Facilitating Utility Use and Integration of Solar Electric Power	Hamm; Solar Electric Power Association	3.5	3.3	3.3	3.3	3.0	3.3
MT027	State Labs	Friedman; NREL and Sandia	3.4	3.2	3.2	3.2	3.0	3.2
MT028	NREL Environmental Impact	Turchi; NREL and Argonne	3.0	3.5	2.8	2.5	2.5	2.9

I. Introduction

Objective review and advice from peers—"peer review"—provides U.S. Department of Energy (DOE) managers, staff, and researchers with a powerful and effective tool for enhancing the management, relevance, effectiveness, and productivity of all Office of Energy Efficiency and Renewable Energy (EERE) research, development, demonstration, deployment, and supporting business management programs. The 2004 EERE Peer Review Guide[1] defines a peer review as:

> *A rigorous, formal, and documented evaluation process using objective criteria and qualified and independent reviewers to make a judgment of the technical/ scientific/business merit, the actual or anticipated results, and the productivity and management effectiveness of programs and/or projects.*

This definition is drawn from definitions used by DOE, National Academy of Sciences (NAS), the White House Office of Management and Budget (OMB), the U.S. General Accounting Office (GAO), and other federal agencies and institutions. It clearly distinguishes in-progress peer review from other types of peer review, such as merit review to select winners of competitive solicitations or readiness (stage gate) reviews to determine when a technology is ready to move to the next phase of development, as well as from other management activities such as quarterly milestone reviews or budget reviews.

A rigorous DOE Solar Energy Technologies Program Peer Review was conducted as a four-day event. The May 2010 program-level review culminated a process that involved evaluations of key activities in each of the four subprogram areas: Photovoltaics (PV), Concentrating Solar Power (CSP), Systems Integration (SI), and Market Transformation (MT). The platform reviews covered approximately 55 to 65 percent of this portfolio.

The objectives of the 2010 meeting were to:

- review and evaluate FY 2009 and 2010 accomplishments;
- provide an opportunity for Program partners to help shape the DOE-sponsored research and development (R&D) program in order that the highest priority technical barriers are addressed;
- provide public disclosure for how taxpayer funds are being utilized to further technology development and deployment in the solar industry;
- review and evaluate Program structure and approach; and
- foster interactions among the national laboratories, industry, and academic institutions conducting the R&D.

[1] Peer Review Guide, Based on a Survey of Best Practices for In-Progress Peer Review, August 2004

A. Solar Energy Technologies Program Overview

Through its four subprograms—Photovoltaics (PV), Concentrating Solar Power (CSP), Systems Integration (SI), and Market Transformation (MT) —the DOE Solar Energy Technologies Program (SETP or Solar Program) focuses on making solar electricity cost-competitive with conventional forms of electricity. The subprograms support the program goal of increasing the widespread adoption of solar electric technologies through applied R&D, demonstration, and market transformation activities. Because the goal requires an industry-wide effort, the Solar Program forges partnerships with national laboratories; universities; private companies; professional associations; other DOE programs; and federal, state, and local agencies across the nation.

The Solar Program works to develop cost-competitive solar energy systems by investing more than $170 million each year in research and development (R&D) on the two solar electric technologies with the greatest potential to reach cost competitiveness by 2015: photovoltaics (PV) and concentrating solar power (CSP). The greatest R&D challenges are reducing costs, improving system performance, and finding new ways to capture and store energy from the sun, and effectively convert that energy to electricity.

The Solar Program also ensures that new technologies are accepted in the marketplace by working to remove many non-technical market barriers. Such activities include updating codes and standards that are not applicable to new technologies, improving interconnection agreements among utilities and consumers, and analyzing utility value capacity credits for utilities. These activities help consumers, businesses, and utilities make more informed decisions when considering renewable energy and help facilitate the purchase of solar energy.

The Solar Program benefits the nation by improving air quality, developing the economy, and increasing energy security. Benefits include:

- Increasing energy reliability and security by domestic production of a solar energy supply, which promotes economic growth.
- Adding 250,000 new jobs for America in the solar industry.
- Saving $100 billion per year for industry and businesses by averting power outages.
- Improving air quality, especially for children and the elderly, by using a clean, non-polluting fuel source.
- Reducing carbon emissions by 23 million metric tons per year by 2030.
- Reducing the trade deficit by mitigating the purchase of billions of barrels of foreign oil.

The mission of the Solar Program is to conduct aggressive research, development, demonstration, and deployment (RDD&D) of solar energy technologies and systems to significantly reduce the cost of solar electricity by 2015. Once solar energy becomes economically viable for everyday use, the Solar Program foresees a future where:

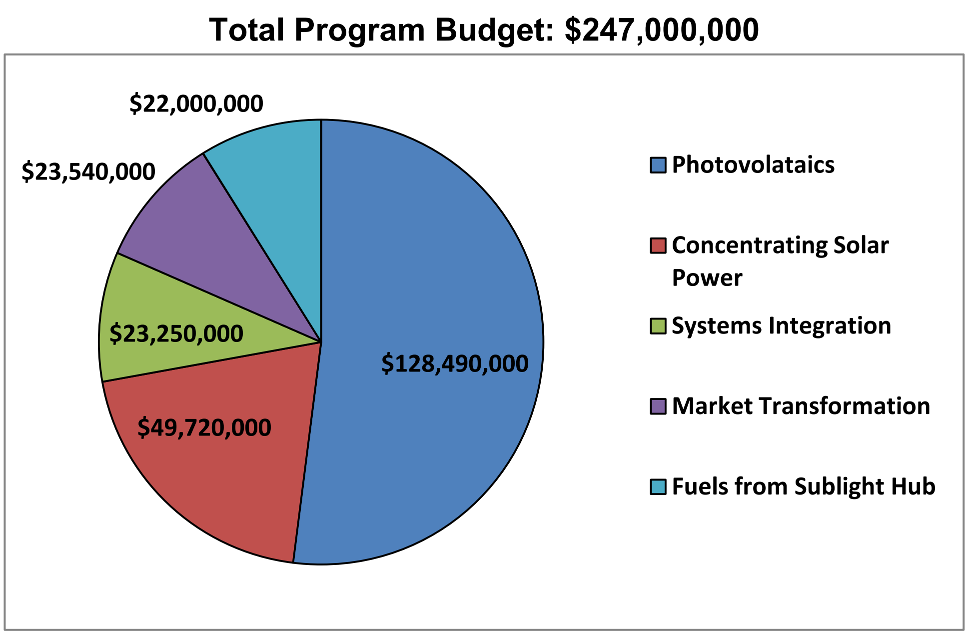
- All Americans benefit from this pure and clean primary energy resource.
- Millions of homes and commercial buildings across the nation use solar technology to provide all or much of their energy needs.
- The Sun Belt states get much of their electricity from solar power plants sited near the communities that need it.
- The southwestern states generate more electricity from solar energy than they need, enabling them to export power to other states.
- Solar power is used to produce hydrogen, which is a transportation fuel that relieves our nation's dependence on imported oil.

The primary goals of the Solar Program are to:

- substantively accelerate development of U.S.-produced PV systems so that electricity produced from PV systems becomes cost-competitive with select grid-connected markets across the United States;
- expand the U.S.-installed domestic capacity of PV systems to 5-10 gigawatts by 2015;
- develop parabolic trough, central receiver, and dish/Stirling concentrating solar thermal power plant technologies that produce electricity that is cost competitive with electricity from conventional power technologies; and
- promote market expansion of solar energy technologies.

The program budget and distribution across each subprogram is shown below in Exhibit 1.

Exhibit 1 – Solar Program Funding



II. Solar Program Peer Review Process

The Solar Program followed guidelines provided in the EERE 2004 *Peer Review Guide* in the design and implementation of its subprogram reviews. A chair committee, comprised of external experts, was established early in the process to provide recommendations and help ensure an independent and transparent review process.

Mr. Joseph Stekli of the Solar Program was assigned by the Solar Program Manager as the program review leader. Mr. Stekli managed all aspects of planning and implementation. He was supported by a planning team comprised of staff from the Solar Program, DOE Golden Office, and contractors. The Oak Ridge Institute for Science and Education (ORISE), SENTECH Incorporated, and Courtesy Associates were the lead contractors responsible for organizing and facilitating the program review. The team held bi-weekly planning meetings beginning in January 2010 to outline the review procedures and processes, plan each of the individual subprogram Reviews, and ensure that the process followed the Office of Energy Efficiency and Renewable Energy peer review guidance.

The 2010 program review meeting was held May 24 through May 27 at the Omni Shoreham Hotel in Washington, D.C. The program review meeting consisted of five simultaneous panels or sessions that were based on the four subprograms; the Photovoltaics panel was sub-divided into Near Term and Long Term technology sessions. The program review meeting consisted of technical project-level reviews of the research projects funded. Additionally, the overall structure and direction of the program and each subprogram was also reviewed.

A separate review panel consisting of five expert reviewers and one chairperson was formed for each subprogram panel or session. Additionally, an overall lead chairperson was selected to oversee each of the subprogram panels and the entire review process. The selected peer review panel members are peer experts from a variety of solar power-related backgrounds and organizations, including laboratories, industry, and academia. The "chair committee" for the program review was comprised of the overall lead chairperson as well as the chairperson from each subprogram review panel. Reviewers were screened to ensure no conflicts of interest with regard to the specific projects for which they submitted reviews. Reviewers recused themselves from projects on which they worked directly and those in which they had other relationships with project team members, and/or in instances where they had a financial interest in the matter at issue.

The following programmatic documents were made available to the reviewers via the Solar Program's Website prior to program review meeting: the Multi-Year Program Plan 2008-2012, the 2009 Annual Report, and the Program Brochure. Additionally, the Principal Investigator presentations were made available to the reviewers via the Oak Ridge Institute for Science and Education online peer review system known as PeerNet. A training webinar was held on May 10 to introduce reviewers to the PeerNet system, and to demonstrate how to navigate the site in order to access and download the presentations.

A list of program review panel members is provided in Exhibit 2.

Exhibit 2 – Solar Program Peer Review Panel

Name	Organization	Role	Area of Expertise
Bob Armstrong	Massachusetts Institute of Technology	Review Panel Chairperson	Solar Energy
Photovoltaics Panel			
Martin Green	The University of New South Wales	Section Chairperson	Photovoltaics (PV)
Syrys Ziai	QSpeed	Peer Reviewer	Near-Term PV
Terry Jester	Consultant	Peer Reviewer	Near-Term PV
Jim Rand	University of Delaware	Peer Reviewer	Near-Term PV
Dave Danielson	U.S. DOE Advanced Research Projects Agency - Energy	Peer Reviewer	Near-Term PV
Jim Mikkelson	Consultant	Peer Reviewer	Near-Term PV
Greg Smestad	Solar Energy Materials and Solar Cells	Peer Reviewer	Long-Term PV
John Meakin	Professor Emeritus University of Delaware	Peer Reviewer	Long-Term PV
Sheila Bailey	The National Aeronautics and Space Administration	Peer Reviewer	Long-Term PV
George Maracas	National Science Foundation	Peer Reviewer	Long-Term PV
David Wilt	Air Force Research Laboratory	Peer Reviewer	Long-Term PV
Concentrating Solar Power Panel			
Lorin Vant-Hull	Professor Emeritus University of Houston	Section Chairperson	Concentrating Solar Power

Name	Organization	Role	Area of Expertise
Gerry Braun	California Institute for Energy and Environment	Peer Reviewer	Concentrating Solar Power
Jim Blackmon	University of Alabama-Huntsville	Peer Reviewer	Concentrating Solar Power
Terry Peterson	Consultant	Peer Reviewer	Concentrating Solar Power
Arlon Hunt	Lawrence Berkeley National Laboratory	Peer Reviewer	Concentrating Solar Power
Bob Boehm	University of Nevada-Las Vegas	Peer Reviewer	Concentrating Solar Power
Systems Integration Panel			
Ernie Palomino	Salt River Project	Section Chairperson	Systems Integration
Christy Herig	Solar Electric Power Association	Peer Reviewer	Systems Integration
Efrain O'Neill	University of Puerto Rico-Mayagüez	Peer Reviewer	Systems Integration
Mack Grady	University of Texas	Peer Reviewer	Systems Integration
Haukur Asgeirsson	DTE Energy	Peer Reviewer	Systems Integration
Ray Hudson	BEW Engineering Corporation	Peer Reviewer	Systems Integration
Market Transformation Panel			
Tom Plant	Colorado Governor’s Energy Office	Section Chairperson	Market Transformation
Adam Browning	The Vote Solar Initiative	Peer Reviewer	Market Transformation
Stephen Frantz	The Sacramento Municipal Utility District (SMUD)	Peer Reviewer	Market Transformation
Steve Beuning	Xcel Energy, Director of Market Operations	Peer Reviewer	Market Transformation
Doug Payne	SolarTech Consortium	Peer Reviewer	Market Transformation
James Critchfield	U.S. Environmental Protection Agency	Peer Reviewer	Market Transformation

During the May 24-27, 2010 Solar Program review meeting, program management and staff presented on (1) strategic planning and management approaches, and (2) integration of analysis and sustainability in planning, direction and priorities of the RDD&D platforms. In addition, the panel chairs presented the results of each panel's evaluation to the Solar Program during the wrap up session on Friday, May 28, 2010.

An agenda for the meeting is provided in Attachment Two. A list of attendees is provided in Attachment Three. Presentations given during each of the program review meetings as well as other background information are posted on the Solar Program Review Meeting Web site: http://www.eere.energy.gov/solar/review_meeting/program_review_meeting_2010.html.

A. Solar Program Review Chair Committee

EERE Peer Review Guidelines recommend a steering committee be formed to help ensure an independent and transparent expert review of EERE reviews. The Solar Program elected to adopt this recommendation and formed a "chair committee" to guide the peer review process for its research, development, demonstration, and deployment portfolio. The Committee provided recommendations, technical reviewers, comments and direction to ensure the Program receives and publishes calibrated, independent and transparent project portfolio feedback. Specific activities performed by the steering committee were as follows:

- Review and comment on evaluation forms and presentation templates.
- Review and comment on overall implementation process.
- Review and comment on candidate review panelists for each platform.
- Review the summary results of the subprogram reviews and reviewer comments.
- Participate on the review panel for the overall program peer review.

The Committee consisted of five members, the overall Program Review Chairperson and the Chairperson for each of the four subprogram areas. Final selection was made by the Solar Program Manager and Program Review Team Leader. Dr. Bob Armstrong was selected to be the overall Program Review Chairperson, and the following individuals were selected as the chairpersons for each of the subprogram areas: Dr. Martin Green, Photovoltaics; Dr. Lorin Vant-Hull, Concentrating Solar Power; Mr. Ernie Palomino, Systems Integration; and Mr. Tom Plant, Market Transformation.

Decision criteria in selecting Committee members included the following:

- Absence of any conflict of interest (COI) as demonstrated by receipt of a signed COI form.
- Balanced representation of the diversity of expertise required to support the review process.

- Balanced representation by type of organization including research institution, private sector, government, and nongovernmental organization.
- Distinguished reputation as some of the top experts in their respective field or technology area.

Committee recommendations were provided to the review planning team as they were made throughout the planning process. As described above, the chair committee along with the peer reviewers of each review panel or session comprised the program review Panel for the May 24-27, 2010 Solar Program Review meeting.

III. Summary of the Program and Subprogram Reviews

This section includes the following: a summary of reviewer feedback regarding the overall efforts of the Solar Program, a summary or overview of each of the four subprogram activities, and reviewer feedback pertaining to the efforts of the four subprograms. Additional information and details on the 2010 program review meeting, including presentations, are available on the program review Web site at: http://www.eere.energy.gov/solar/review_meeting/program_review_meeting_2010.html.

A. *Reviewer Feedback on the Solar Energy Technologies Program*

The Solar Energy Technologies Program is investing in an impressive portfolio of science and engineering research projects, grid integration and market transformation activities and working with highly qualified researchers from academia, industry, and national laboratories. The program is staffed by highly skilled and talented individuals charged with managing a very complex research, development, demonstration, and deployment (RDD&D) portfolio.

The Review Panel recommends program management integrate certain Systems Integration (SI) and Photovoltaic (PV) subprogram efforts focusing on lowering overall systems costs. The Panel feels the creation of the "Systems Collaborative Program," aimed at integrating SI and PV subprogram efforts, is a step in the right direction.

The Review Panel also suggests that the Office of Energy Efficiency and Renewable Energy (EERE) formulate appropriate terminology to distinguish among photovoltaics, domestic/commercial hot water, concentrating solar thermal power, and solar. EERE should craft definitions for general "in-house" use, distribute the definitions to national laboratories and contractors, incorporate the definitions into funding opportunity announcements (FOAs), and rigorously adhere to this usage. In particular, it is suggested that the Program specifically note that "solar" is not a synonym for PV. It is also suggested that a standard definition for the term "system" as it relates to PV and CSP should be developed.

According to the Panel, several manufacturing issues should be addressed by the Program. Even though solar energy technologies are being researched and developed in the United States by the Solar Program, many of the products are ultimately manufactured overseas. The Panel recommends the United States focus efforts on developing and expanding capabilities and expertise in the manufacturing of solar energy technologies. Additionally, manufacturability should be considered as one of the criteria in designing and evaluating CSP and PV systems. The techniques developed and optimized by the National Renewable Energy Laboratory (NREL) and Sandia National Laboratories (Sandia) must be harvested and applied to industry.

The Review Panel feels that a study should be initiated on how intellectual property developed with government funding in the Program can be of maximum benefit to the U.S. Among concerns raised by the panel are instances in which technology developed with Program funding is acquired by foreign entities, (e.g., by purchase of a U.S. company). In these situations it is very difficult contractually to bind companies to retain the intellectual property in the U.S. and not sell to foreign entities. Of biggest concern to researchers is that this information is lost to U.S. entities once the intellectual property is purchased by foreign companies. Not only does the information end up overseas; it is lost and no longer available to the U.S. The Panel feels this information should remain "open domain" to researchers and developers in the U.S. rather than lost entirely to a foreign entity.

Regarding portfolio and budget management in each of the subprogram areas, the Panel recognized that the balance of funding tends to lean heavily towards industry, then the national laboratories, and finally universities. Reviewers feel more funding should be allocated to universities, since they have the ability to bring new tools and innovative approaches to the table. However, it was noted by the Panel that funding opportunities for universities are limited by the fact that they often lack technology demonstration capabilities, and a large part the Solar Program budget is focused on researching, developing, and demonstrating advanced technologies.

Finally, the portfolio balance should be improved by the inclusion of more upstream research on novel and innovative approaches in each of the major Program areas as well as in other solar energy approaches. The recent funding of the Solar Fuels Hub is an excellent step in this direction. Added upstream research may help the balance of funding to universities noted above.

B. The Photovoltaics Subprogram

The Photovoltaics (PV) subprogram is working to make PV competitive with conventional forms of electricity by 2015. To accomplish this goal, the U.S. Department of Energy (DOE) has created strategic partnerships that include the national laboratories, start-up solar companies, and universities. These partnerships aim to keep the innovation pipeline full by driving down costs, diversifying products, ensuring adequate supply for rapidly growing demand, and manufacturing dependable products that consumers trust.

The DOE has awarded funding in a number of areas to advance new technologies, move technologies from prototype into production, and improve the manufacturing capabilities of the technologies already being mass produced. For example, work pertaining to Next Generation PV Devices and Processes emphasizes exploratory R&D on innovative PV technologies. This work is expected to produce prototype cells or processes by 2015, with full commercialization expected between 2020 and 2030. The PV Technology Pre-Incubator project helps small businesses bridge the gap between the concept verification stage of a PV technology and the development of a commercially viable prototype by 2015. Additionally, the PV Technology Incubator projects explore the commercial potential of new manufacturing processes and products produced in pilot-scale operations. Prototypes must demonstrate cost, reliability, and performance advantages. Finally, Technology Pathway Partnerships focus on PV component and system designs that are ready for mass production and capable of lowering PV's levelized cost of electricity (LCOE).

Four national laboratories, working closely with private sector and academic partners, conduct a majority of the research and analysis in PV subprogram: NREL, Sandia, Oak Ridge National Laboratory (ORNL), and Brookhaven National Laboratory (BNL). The national laboratories use the Solar Advisor Model (SAM) as a standardized tool for assessing PV system effects on LCOE. SAM is also used within DOE's Solar Program and in industry (which offers input on the model) to analyze different energy scenarios, assess the impact of technology improvements, and move technologies from the laboratory to the marketplace. Other DOE analysis activities are designed to explore the impacts of increased market penetration, policy changes, and technology progress.

i. The Photovoltaics Subprogram Review

The PV subprogram review was held May 25 through May 27, 2010, at the Omni Shoreham Hotel located in Washington, D.C. The PV Subprogram Review was divided into Near Term and Long Term review panels or sessions. Candidates for the Review Panel were evaluated based on their subject matter knowledge in the technology area, willingness to commit the time and energy needed to serve on the panel, and freedom from conflict of interest as represented by receipt of their Conflict of Interest form. Review Panel members for the PV subprogram review included the following:

- Dr. Martin Green; The University of New South Wales (Panel Chair)
- PV Near Term Reviewers:
  - Mr. David Danielson; U.S. DOE Advanced Research Projects Agency - Energy
  - Ms. Terry Jester; Entrepreneur-in-Residence, Hudson Clean Energy Partners
  - Mr. Jim Mikkelson; Consultant
  - Dr. Jim Rand; University of Delaware
  - Mr. Syrus Ziai; Chief Executive Officer, QSpeed
- PV Long Term Reviewers:
  - Dr. Sheila Bailey; National Aeronautics and Space Administration
  - Dr. George Maracas; Program Director, National Science Foundation
  - Dr. John Meakin; Proferssor Emeritus, University of Delaware
  - Dr. Greg Smestad; Solar Energy Materials and Solar Cells
  - Mr. David Wilt; Air Force Research Laboratory

ii. Reviewer Feedback on the Photovoltaics Subprogram

The role of the National Renewable Energy Laboratory (NREL) is viewed quite differently by the various PV reviewers. Many PV reviewers see two main roles for NREL: 1) as a developer and keeper of basic science and technology related to renewable energy; and 2) as the national resource for applying this knowledge to the solution of basic problems encountered in the commercialization and manufacturing of renewable energy products. The Panel suggests that basic science and technology focus on computational, modeling, and metrology efforts. Technology assistance projects should focus on solving problems common to technology implementation and manufacturing (barriers, contacts, failure analysis) and perhaps on improving specific materials that are already in commercial use.

The Process Development and Integration Laboratory (PDIL) has had a significant amount of funding invested in it but seems to be underutilized. Increasing technology transfer projects and cooperative development agreements may present an avenue to increase funding, staff, and the use of the equipment at the facility. The Panel indicates that there are some internal projects and uses which should be supported as a basic part of the PDIL. The concept of making a best device and then selectively degrading it to help close the gap between "hero" results and manufacturing results was identified as a powerful method to drive efficiency up and costs down. The reviewers also indicate that a strong interaction with the computational and modeling group to close the theory and practice loop, which is an effective way to save experiment time and money, can only be done in such a facility as NREL.

Some of the Technology Pathway Partnership (TPP) projects were considered to be quite impressive. Reviewers note that TPP funding seems to have hastened the pace of Concentrated Photovoltaics (CPV) development for moderately large systems and it appears there is now more available manufacturing capacity than there is demand for such systems. According to reviewers, cost reduction will require high-volume manufacturing, which means systems need to be purchased. They suggest that some level of cost reduction or incentives might be required to get this effort off the ground.

The Panel noted the following gaps that require attention from and should be addressed by the Photovoltaics subprogram:

- Increasing funding support for basic research in order to generate the talented researchers and the backlog of ideas that will be required in the future.
- Devoting more attention to resolving issues with the NREL Process Development and Integration Laboratory. Significant funding has been invested in this facility, and the Program should find the most effective use of it.

- Focusing efforts on fewer programs and allocating more concentrated levels of funding and effort to the technologies that have the potential to reach the $/W cost goals on the targeted timescales. Effective work can be done to reduce costs at the system level.

iii. Summarized Photovoltaics Subprogram Response

The Solar Program appreciates and shares the intellectual property (IP) concerns mentioned by the reviewers. The Solar Program continues to work with legal council to ensure and enforce that research supported with federal taxpayer dollars continues to have a domestic impact.

In recent years, industry has seen an increase in funding percentage relative to universities. This is a direct result of the priorities of the American Reinvestment and Recovery Act (ARRA), which had greater emphasis on funding programs with nearer term impact on the economy. Moving forward, University funding percentage will likely see an increase. For example, the second iteration of the Next Generation Technology solicitation will be released this year with increased funding. A pilot program for a DOE Solar Fellowship is under development for FY2011 and the Minority University Research Associate (MURA) Program recently announced new awards.

The national labs play a critical role in both technology development and industry assistance. The Solar Program continues to work closely with lab partners to evaluate and evolve the role of the labs in moving solar technology and industry forward. The program will continue to look at avenues to increase the usefulness of PDIL through greater cooperation with industry and other researchers. Early stage research by the labs continue to be a funded through the program to both advance solar technology and support the research knowledge as well as develop the workforce necessary for the growing industry.

The Technology Pathway Partnership (TPP) projects were able to improve performance and reduce total systems costs through close collaboration among many companies in teams. Concentrating Photovoltaics is an excellent example of a technology that benefitted from those partnerships since technology expertise was required in disparate fields ranging from high tech chip fabrication to metal forming and 2-axis tracking. CPV, like other emerging photovoltaic technologies, will benefit from the recently announced Solar Demonstration Zone which will provide by a way to prove the feasibility and affordability of CPV technologies and help those technologies achieve bankability.

The three year TPP program is coming to an end in FY2010. Based on the general success of the program, a follow on program is currently under development for FY2011 and will reflect the lessons learned over the last three years.

Residential rooftop solar does represent a significant opportunity for penetration over the coming years. Communicating the benefits of solar technology continue to be a challenge SETP seeks to aggressively address. In regards to communicating payback period and monthly cost to homeowners, the Solar Program has funded PVWatts through NREL. PVWatts is a solar power calculator found at: http://www.pvwatts.org/. With very basic inputs, the calculator can give the user financial estimates as to system size and cost as well as payback period and energy costs savings.

C. The Concentrating Solar Power Subprogram

Concentrating Solar Power (CSP) offers a utility-scale, firm, dispatchable renewable energy option that can help meet the nation's demand for electricity. Worldwide, CSP activity is rapidly scaling, with approximately 14,500 MW in various stages of development in 20 countries. In the United States alone, more than 419 MW of CSP are currently in operation, with another 75 MW under construction and more than 10,000 MW under development.

The goals of the CSP subprogram include lowering costs and advancing technology to the point that CSP is competitive in the intermediate power market between 2015 and 2017 and in the baseload power market between 2020 and 2022. Research and development (R&D) is conducted through cost-shared contracts with industry, universities, and national laboratories. In addition, the CSP subprogram develops partnerships with federal and state agencies, as well as the solar industry, to encourage the deployment of CSP technologies by addressing land and transmission issues.

Since 2008, the CSP subprogram has established 40 ongoing partnerships through competitive solicitations with companies and universities by giving financial and technical assistance to each awardee. The 12 contracts awarded in 2008 focus on advanced CSP components and manufacturing concepts; the 15 contracts awarded in 2009 emphasize novel thermal energy storage concepts and improved heat transfer fluids; and the13 contracts established in FY2010 look at long-range R&D where CSP technologies could compete in the baseload power market with projects that offer low-cost power and 16 hours of storage. All of these projects represent important steps toward making CSP a cost-competitive source of power and of providing dispatchable power. CSP with storage currently does not receive the credit it deserves for its dispatchability.

National laboratories, primarily the National Renewable Energy Laboratory (NREL) and Sandia National Laboratories (Sandia), support the CSP industry with critical R&D to meet cost, reliability, performance, and manufacturing challenges. One of the most important avenues of support is through optical tool development, including the Video Scanning Hartmann Optical

Test (VSHOT) and Theoretical Overlay Photographic Collector Alignment Technique (TOPCAT). Industry partners have used both of these tools to characterize and align reflectors. Resource assessment allows accurate weather and solar insolation data to be captured through improved satellite imaging, additional ground data sites, and forecasting. Other research topics include materials; thermal storage and heat transfer concepts; reflector and absorber concepts; trough, tower, and dish-engine component and system R&D; and CSP systems analysis.

Additionally, the CSP subprogram is also co-leading a programmatic environmental impact statement with the U.S. Department of the Interior's Bureau of Land Management (BLM). A significant number of acres administered by BLM in desert areas of the southwest U.S. register the necessary levels of solar radiation for CSP development. The purpose of the environmental impact statement is to identify suitable federal land in California, Arizona, New Mexico, Nevada, Colorado, and Utah for utility-scale solar project development. The CSP subprogram is currently working alongside the DOE Office of Electricity, the Western Area Power Administration, the Western Governors' Association, and various states to identify the best locations for new transmission corridors.

i. The Concentrating Solar Power Subprogram Review

The CSP subprogram review was held May 25 through May 27, 2010, at the Omni Shoreham Hotel located in Washington, D.C. Candidates for the Review Panel were evaluated based on their subject matter knowledge in the technology area, willingness to commit the time and energy needed to serve on the panel, and freedom from conflict of interest as represented by receipt of their Conflict of Interest form. Review Panel members for the CSP subprogram review included the following:

- Dr. Lorin Vant-Hull; University of Houston Retired (Panel Chair)
- Dr. Jim Blackmon; University of Alabama-Huntsville
- Dr. Bob Boehm; University of Nevada-Las Vegas
- Mr. Gerry Braun; California Institute for Energy and Environment
- Dr. Arlon Hunt; Lawrence Berkeley National Lab
- Dr. Terry Peterson; consultant

ii. Reviewer Feedback on the Concentrating Solar Power Subprogram

Reviewers note that there have been many years of low funding for CSP, and several years of no funding for central receivers. The Review Panel indicates that because of its potential to achieve low cost of energy and the low actual risk associated with the program, in contrast to the perceived high risk by the financial industry, CSP must achieve support parity with the other renewables such as PV, wind, and biomass through substantially increased funding for CSP technologies over the next decade. Reviewers consider work on the Solar Zone and Solar Loan Guarantees as a step in the right direction but believe the Program should consider funding more projects that offer a lower levelized cost of energy (LCOE). Additionally, reviewers would like to see add-on funding to the CSP subprogram for services to respond to global warming and peak oil issues.

The Panel notes that a great deal of very good work was done under DOE support in the past 38 years. However, reviewers are concerned that the "new generation" of solar workers remains essentially unaware of the lessons learned and results of those previous analyses and studies. The suggestion is that titles, authors, and abstracts of the Sandia/NREL libraries should be available in a searchable format online, meaningful reports should be scanned in searchable format, and the existing, extensive libraries of reports could also be made available at little more than the cost of transport of the material to a central library.

According to the Panel, the addition of a solar field to a conventional power plant to decrease the fossil fuel purchases required can be a good investment for the right kind of utility situation. Most of the infrastructure will already be in place except for the solar field and some means of "bolting" it to the conventional plant, and normal concerns such as transmission will already be in place. While this may not be a universal application, there are several good situations where this could be applied, serving as a niche market, similar to some of the markets that are used by PV. This kind of effort overlaps between the conventional and renewable energy programs at DOE, and this probably complicates the administration of this effort. However, the Panel suggests there could be much for CSP to gain in pursuing this kind of activity. Of course, the significant CO_2 reduction from the conventional plant can represent even more compelling arguments for the conventional energy program. This issue was the objective of the DOE "Repowering and Retrofiting" program in the 1980s which resulted in seven utilities and seven process heat customers engaging in intense evaluations of the concept in partnership with (solar) aerospace companies and the national labs.

The Review Panel believes that, of all of the solar programs, CSP has the least to lose and the most to gain from foreign collaborations, and such collaborations should be encouraged. Since the true motive of "Buy American" sentiments is retention and formation of U.S. jobs, the panel believe the country's best interests would be better served if the DOE were to replace its current practice of awarding contracts only to nominally domestic companies (which may have only a small business office in the United States) with one aimed specifically at creation of U.S. employment. In other words, development of CSP or PV technology that is deployed in the United States will inevitably create U.S. jobs, whether or not the technology vendor is a "U.S." company.

The Panel noted that, other than some involvement in the thermal storage aspects of the program, almost no university presence exists in the CSP subprogram. Reviewers believe this is partially because of high reliance by DOE on NREL and Sandia. While reviewers conceded this reliance on the labs provides some ease in operational aspects for DOE, they are concerned that it stifles a well-trained employment pipeline coming through the educational system that could be enhanced greatly by active involvement of universities. The requirement that universities provide a substantial cost share to qualify for a contract award is viewed as a major and often insurmountable problem that often discourages university investigators from even developing a proposal.

The biggest roadblock the CSP reviewers see for the subprogram is its over-weighting of analytical attention to estimating costs vs. assessing economic value. Because the DOE has cost goals and LCOE models, the emphasis is almost 100% on cost per delivered kWh. Reviewers believe that this is a completely inappropriate metric for any variable resource that can deliver on-peak energy even part of the time. Further, the major sensitivities in determining the economic value of a CSP plant are still the same as they were when studies were conducted in 1976, which are viewed as inappropriate in the current environment of peak oil, global warming, and foreign exchange losses. Finally, the Panel notes that the ability to incorporate cost effective storage brings a unique value to central receiver and molten salt trough plants. This unique value is not related to the plant cost, performance, or even the capacity factor, but accrues through the ability to defer energy delivery until the time of demand - meaning that the plant is fully dispatchable.

iii. Concentrating Solar Power Subprogram Response

The reviewers were very perceptive in their evaluation of CSP activities. It's difficult to find any part of their review that the Program disagrees with. As mentioned by the reviewers, there is a need to buy down risk of developing innovative CSP technology, establish opportunities for industry to develop long term concepts that show a particular emphasis on reducing greenhouse gas emissions, emphasize development of thermal storage concepts, and work with international companies and institutions to further the advancement of CSP. We also need to preserve and make available to future scientists and engineers the technical work that has been done over the last 35 years.

The Program agrees with the need for additional university participation in CSP activities. One reviewer comment that requires further discussion was the assertion that LCOE is a "completely inappropriate metric." It is agreed that LCOE should not be the only metric, and since the review the Program has been trying to develop a metric that better values the dispatchability enabled by thermal storage. However, CSP must compete in the power market with natural gas, wind, PV, and other energy sources. Cost, if not the only metric, is certainly the most important one used by utilities when they decide which energy source to implement.

Overall, the reviewers did an excellent job in their evaluation of the CSP activities.

D. The Systems Integration Subprogram

The Systems Integration (SI) subprogram works with industry, universities, and the national laboratories to overcome technical barriers to the large-scale deployment of solar technologies on the grid. The subprogram is investing primarily in five areas: technology development, system modeling and analysis, solar resource assessment, codes and standards, and system testing and evaluation.

The SI subprogram is investing in new advanced inverter, controller, and energy management technologies, and is also working with industry and utilities to test and demonstrate high penetration solar applications, both in the laboratory and in the field. To better predict the impact of solar on the grid, SI is developing advanced technical and economic modeling, simulation, and analysis capabilities that will give utility personnel a better understanding of PV and CSP system power production. In addition, new ground- and satellite-based methods are being investigated for measuring, modeling, and forecasting solar radiation. Finally, SI is supporting the development of consistent solar interconnection codes and standards and transparent regulatory implementation practices. Collaborators in this work include the Solar America Board of Codes and Standards, the national laboratories, the National Institute of Standards and Technology (NIST), and IEEE.

To help ensure the "bankability" of PV systems, the Systems Integration subprogram tests and evaluates new technologies to determine their impact on system performance and levelized cost of energy (LCOE). Because component and system reliability are so important to reaching LCOE commensurate with conventional generation, the national laboratories are working diligently with industry and code-making bodies to develop test protocols to find failure mechanisms early, so that they do not affect the legitimacy of solar as a proven technology. These testing and evaluation activities are then used to enhance the development of models like the Solar Advisor Model (SAM), allow validation of component/system models, and integrate various modeling platforms for collaborative development and use.

The Systems Integration subprogram is working with NIST to develop new standards, including additions to the distributed energy interconnection standard IEEE 1547.7. The subprogram is also collaborating with the Electric Power Research Institute (EPRI) to develop new communications standards so utilities can communicate with and potentially control solar systems on the grid. The national laboratories are collaborating with the National Oceanic and Atmospheric Administration to collect high-quality solar radiation data from ground- and satellite-based measurements, to meet the need for improved atmospheric models, and to develop solar radiation forecasts. Finally, the SI subprogram is learning a great deal from the experiences of the Utility Wind Interest Group, particularly because the integration of wind power into the electric transmission system is relatively mature.

i. The Systems Integration Subprogram Review

The Systems Integration subprogram review was held May 25 and May 26, 2010, at the Omni Shoreham Hotel located in Washington, D.C. Candidates for the Systems Integration Review Panel were evaluated based on their subject matter knowledge in the technology area, willingness to commit the time and energy needed to serve on the panel, and freedom from conflict of interest as represented by receipt of their Conflict of Interest form. Review Panel members for the Systems Integration subprogram review included the following:

- Mr. Ernie Palomino; The Salt River Project, (Panel Chair)
- Ms. Christy Herig; Solar Electric Power Association
- Mr. Efrain O'Neill; University of Puerto Rico-Mayaguez
- Dr. Mack Grady; University of Texas
- Mr. Haukur Asgeirsson; DTE Energy
- Mr. Ray Hudson; BEW Engineering Corporation

ii. Reviewer Feedback on the Systems Integration Subprogram Review

The Review Panel believes that ongoing collaboration efforts between the federal laboratories, the National Renewable Energy Laboratory (NREL) and Sandia National Laboratories (Sandia), should be continued and expanded. One concern expressed by the Systems Integration Review Panel is that insufficient data and results from the laboratory research programs are actually made available to the general public due to confidentiality agreements. In some cases, the data and results must be purchased from a third party, or they have been transferred offshore. The Panel believes that the Systems Integration subprogram needs to establish a process to address this concern and to maintain more effective control of its work products. Reviewers suggest that DOE establish a clearinghouse for DOE-developed software. This clearinghouse should include a mechanism to expand outreach to disseminate the deliverable to user communities.

The Review Panel also recommends that NREL and Sandia Testing & Evaluation activities focus on new product/technology development. Also, priority for testing and evaluation support should be provided to U.S. firms.

Currently, codes and standards are addressed by both the Systems Integration and Market Transformation subprograms. The plan, which is supported by the Review Panel, is to transfer the responsibility for the coordination and development of codes and standards to the Systems Integration subprogram. Additionally, the Review Panel also recommends that the Solar Energy Technologies Program combine PV and CSP analyses activities related to codes and standards within the Systems Integration subprogram. Codes and standards activities need to include CSP storage and address safety concerns about "old" PV systems.

High penetration projects are critical to achieving DOE's Solar Program goals and high penetration activities, specifically in PV, should be expanded and accelerated. Further, the Panel feels deployment of PV and CSP technologies needs to be expedited. The information obtained from such activities and deployments should be transferred to stakeholder communities in a timely manner to ensure that decisions are made with the best available data. One issue noted by the Review Panel is that only a limited number of utilities are involved and that participation from universities is not evident in the area of high penetration.

The Review Panel recommends that universities be included and/or considered for the data collection and analyses tasks required. The panel's opinion was that software and models to evaluate high penetrations scenarios were not readily available or were difficult to use. Universities with power engineering capabilities may be a good resource for developing and or enhancing such models. Also, it would provide training to future engineers on system integration high penetration issues. Additionally, there seemed to be limited interaction with the Electric Power Research Institute and the Solar Electric Power Association. These organizations represent the utility sector and may be a means to communicate issues related to high penetration PV concerns and to request input and support for the SI program.

The Panel recognizes that a significant amount of work is currently being conducted by the international community, particularly in CSP. The Review Panel recommends that the subprogram initiate an activity to acquire the available international information and or data. Questions to be addressed include:

- What are the results or lessons learned from the international work, particularly in Europe, Germany, and Japan?
- Is the information publicly available?
- How does this information support or complement the efforts currently being conducted by the Solar Program?

The Review Panel suggests that the Systems Integration subprogram needs to continue to expand its activities regarding the impact of variable resources on electric grid operations. The development and validation of dynamic models needs to continue in order to provide insights on how solar electric technologies affect the operating dynamics of the grid; this is a significant issue for the high penetration of PV scenario. According to reviewers, the subprogram also needs to study, as a part of this effort, how a CSP central station may influence electric system grid operations.

Reviewers suggest that DOE models such as the Solar Advisor Model (SAM), In My Back Yard (IMBY), and PVWATTS should be enhanced and upgrading to depict the current state of the market and technologies. For example, the role of storage technologies needs to be incorporated into future modeling efforts. The Review Panel echoes the need for accurate, quality solar radiation data and recommends that solar monitoring, modeling, and analyses activities start to explore solar forecasting model development.

The subprogram includes several activities related to the development of advanced PV inverters. Concern was raised that information provided was inconsistent and that it was unclear how this set of activities supported the program activities, specifically the metric used to measure progress, and how risk and uncertainty were to be addressed.

Some of the general findings of the Review Panel are:

- several of the Systems Integration projects are in the very early stages of development and minimal data were available on criteria such as accomplishments and progress;
- the program needs to reduce the time between data collection and analysis and the availability for use in models or by the stakeholder community;
- DOE needs to streamline the contract process; and
- DOE needs to schedule more workshops to obtain feedback for the stakeholder community, and more specifically, utilities.

iii. Systems Integration Subprogram Response

As mentioned by the review panel, addressing the technical barriers to the high penetration of solar technologies is extremely important, and DOE agrees that more work needs to be done in this area. It is for this reason SI has decided to focus not only on high penetration issues on the distribution systems, but also create a new emphasis on transmission. This is especially important since not only is CSP becoming more prevalent, but large-scale PV is also moving to transmission scale as well. This is clearly evident from the 25 MW PV system in DeSoto, FL, and the 21 MW PV system in Blythe, CA. DOE also agrees with the panel that more work needs to be done in developing closer collaborations with organizations such as EPRI, CPUC, SEPA, UWIG, the WECC and others so that the results of DOE funded research can have the greatest possible impact. It is for that reason that the Systems Integration group is developing a "high penetration solar web portal" which will allow information from the labs and our funding awards to be readily shared with many stakeholders.

The review panel also emphasized the importance of working with international community to learn from their experience in developing a plan for the United States. To accomplish this, Systems Integration intends to work closely with countries that currently have high penetrations of solar technologies, such as Germany, Spain, and Japan. Additionally, DOE has asked the national labs to play an important role in IEA Task 14: High Penetrations of PV Systems in Electricity Grids.

This has been a transitional year for the SI subprogram. Some of these changes include the transition of Codes and Standards from Market Transformation, an increased emphasis on the impact of solar technologies on the transmission system, and the inclusion of all solar technologies, as opposed to just photovoltaics. To better integrate these new activities within the subprogram, SI is developing a multi-year program plan for four areas: balance of systems, grid integration, technology validation, and solar resource assessment. By incorporating the comments from the review panel and integrating them into this multi-year plan, SI can play an effective role in enabling high penetrations of solar technologies to be integrated into the grid safely and effectively.

E. The Market Transformation Subprogram

Beyond solar technology development and successful grid integration, continued evolution of the domestic solar market will be necessary to enable the Solar Program goal of reaching 10% to 15% solar electricity use by 2030.

The Market Transformation subprogram contributes to this goal through non-R&D activities that assist state and local governments, create a robust solar workforce, and engage utilities and consumers. Market Transformation actively engages key stakeholders and early adopters through a diverse portfolio of activities that includes education on important issues, policy analysis, and technical assistance. Innovative outreach efforts and peer-to-peer networking quickly disseminate information about best practices and lessons learned. The subprogram is also working to update and streamline regulations and expand affordable financing.

Local governments, which often preside over dense centers of electricity consumption, play an important role in accelerating widespread solar energy adoption. As one way to assist local governments, the Market Transformation subprogram created the Solar America Cities (SAC) activity. This activity's four-pronged approach identifies and overcomes barriers to urban solar implementation, allowing lessons learned and best practices to be shared across the nation:

- *Solar America Cities Partnerships* are cooperative agreements between DOE and 25 large U.S. cities to develop comprehensive, citywide approaches to increasing solar energy use.
- *Solar America Cities Special Projects*, funded through the Recovery Act, tackle barriers to urban solar energy use that were identified through the 25 city partnerships.
- *Solar America Cities Technical Analyses*, conducted by national laboratories and DOE's private sector partners, yield unbiased information and tools to meet emerging challenges and capitalize on new opportunities in local solar market transformation. Analysis projects cover innovative financing approaches, methods for streamlining solar permitting, and solar-friendly building and zoning codes, among others.
- *Solar America Cities Technical Outreach* shares best practices with hundreds of other local governments.

A well-trained workforce is critical to a successful solar market, ensuring high-quality installations, cost reductions, and continued consumer acceptance of solar technologies. As the U.S. solar installation industry continues to grow, employers are having difficulty finding qualified workers. Additionally, educational institutions face challenges in developing the high-quality training programs required to meet industry needs. In many cases, local educational institutions begin developing courses without sufficient expertise in solar technologies.

Market Transformation workforce development activities are designed to complement and increase the effectiveness of investments in workforce development made by state and local governments and other stakeholders. Additionally, DOE funding to the North American Board of Certified Energy Practitioners (NABCEP) and IREC supports the certification of solar installers and accreditation of solar training institutions. This work is based on industry-approved task analyses and is critical to ensuring safe, high-quality solar installations.

Supportive state-level policies and regulations continue to be critical to establishing an effective domestic solar market. States that lead with innovative approaches have reaped the benefits of economic development and local solar market growth. The subprogram is also working with DOE's Federal Energy Management Program (FEMP) to provide technical assistance to federal agencies that install solar technologies on federal facilities.

i. The Market Transformation Subprogram Review

The Market Transformation subprogram review was held May 25 through May 27, 2010, at the Omni Shoreham Hotel located in Washington, D.C. Candidates for the Market Transformation Review Panel were evaluated based on their subject matter knowledge in the area of energy policy, willingness to commit the time and energy needed to serve on the panel, and freedom from conflict of interest as represented by receipt of their Conflict of Interest form. Review Panel members for the Market Transformation subprogram review included the following:

- Mr. Tom Plant; State of Colorado, Governor's Energy Office, (Panel Chair)
- Mr. Steve Beuning; Xcel Energy
- Mr. Adam Browning; The Vote Solar Initiative
- Mr. James Critchfield; U.S. Environmental Protection Agency
- Mr. Stephen Frantz; The Sacramento Municipal Utility District (SMUD)
- Mr. Doug Payne; SolarTech Consortium

ii. Reviewer Feedback on the Market Transformation Subprogram Review

The Market Transformation portion of the SETP Multiyear Program Plan identifies five principal barriers to achieving market transformation: 1) Regulatory, 2) Policies, 3) Permitting, 4) Financial Options, and 5) Education. The Review Panel feels these barriers could easily be described as the foundation of any effort to advance photovoltaics (PV) as an industry in any market, but cautions that they are not all equal in their advance of the PV market. For example, reviewers note that if there is a lack of net metering and interconnection standards, costs are going to be artificially high and any financial incentives will be largely used to offset the costs of these regulatory shortcomings. Similarly, without financing options, purchase of systems will be limited to those who can both monetize the substantial federal tax credit and individually finance the upfront costs of systems – precisely the population that will least benefit from the cost reductions made possible through the tax credit and financial incentives. Finally, DOE could make money allocated to states and municipalities conditional on having certain policies or items in place. For example, certain laws and regulations must be in place prior to a state receiving federal highway funds.

The challenge identified by Panelists is to build upon the basic foundational structure to address all barriers without inadvertently building a structure that is unsustainable due to a weak foundation. A further challenge for the Solar America Cities program, according to reviewers, is that the governmental body recipients want to ensure the public is getting a direct benefit that is easily understood and communicated. As a result, focus is understandably placed on educational efforts and financing programs. The Review Panel believes the Market Transformation subprogram has done an excellent job of moving the high performing Solar America Cities communities to the next level of program implementation, and this provides a critical blueprint for others to follow.

Panelists in the Market Transformation review believe that industry, laboratory, and nongovernmental organization partners are doing essential research and development work around the regulatory, policy and permitting arenas that will strongly position the program for what may be the next critical step in development: the establishment of a package of essential regulatory ingredients, sound policies to drive the market, and recommended approaches for streamlining and reducing the costs of permitting. Reviewers suggest these essential elements be introduced to the non-high performing areas of the country to help develop the necessary national market that will move PV integration from a successful pilot in isolated areas of the country to a geographically diverse and consistent market throughout the country.

The Panel believes that by pulling in the non-traditional players in the renewable energy market, the Market Transformation effort will enjoy multiple benefits:

- They will allow for large-scale players to enter the market without a patchwork of regulatory barriers that drive up costs and reduce opportunities for broad scale implementation.
- They will drive localized manufacturing, creating more American jobs providing PV resources.
- They will build a national consensus through the success of employment and job creation associated with the renewable energy industry. Panelists suggest this type of consensus would greatly assist national legislative efforts which are currently suffering from balkanized support around the country, particularly in the southeast states.

The Review Panel suggests that building a successful foundation of regulation, permitting, and policy will support the important research being done at the labs. The market must be prepared for use of advanced technologies as efficiencies increase, BIPV opportunities expand, and costs per watt are reduced. One reviewer noted, “The best PV module in the world is meaningless if a consumer can’t enjoy net metering and simple interconnection policies.” Even if costs are reduced through technological advancements, the goal of moving toward market transformation and grid parity has not been accomplished if additional costs are accrued due to regulatory uncertainty. While these objectives should not supplant the important research being done, reviewers suggest it should be recognized that the technology is only as good as the system to which it is being introduced. Without a solid regulatory and policy underpinning, advances by the labs are theoretical and do not have an avenue for market transformation because the market is essentially shut out for the technology.

The National Renewable Energy Laboratory and Sandia National Laboratories are producing critical information that state energy offices, regulatory entities, and incentive programs would benefit from – yet reviewers believe there is a lack of sufficient outreach to these players throughout the country to drive best practices home. The Review Panel suggests that an effort be made to find “champions” within states – whether they are utilities, administrations, legislators, or others – and develop state-specific strategies for implementing these best practices. Such champions might include large IOUs or municipal and rural utilities. In other cases this effort may have to be executed through the legislative or initiative process. Through these strategies, other partners would be engaged.

The Panel feels that the Interstate Renewable Energy Council, Solar Electric Power Association, and National Association of Regulatory Utility Commissioners (NARUC) are providing essential services to Public Utility Commissions (PUCs) around the country to advance utility policies. However, the Market Transformation reviewers did not hear any of the Principal Investigators discuss municipal utilities and rural electric cooperatives and suggest that these entities could be used to greater effect when the regulatory picture is viewed in a more comprehensive policy landscape.

According to the Review Panel, the Clean Energy Group is providing critical outreach to those states that have entities, both governmental and non-governmental, that are committed to renewable energy from both an infrastructure and financial perspective (membership includes most dedicated system benefits funds). Reviewers suggest the Clean Energy Group could be used to reach outside of their limited scope of influence to drive policies, both nationally and at the state level, to increase the establishment of resources dedicated to the advancement of PV and other renewable resources.

The National Conference of State Legislatures and NARUC are required to provide information to all of their members without a role of advocacy. While their service is essential to provide the non-biased information that will advance the PV market, Market Transformation reviewers are concerned that these organizations cannot be relied upon to be advocates and should be paired with other entities that can fulfill that role.

The North American Board of Certified Practitioners (NABCEP) provides the de facto standard of assurance that installers have a level of competency that is critical to avoiding market killing bad actors from becoming widespread. The Review Panel suggests that an effort be made to determine how best to utilize NABCEP's established standards without over-burdening the industry and driving smaller players out of the market. Colorado has provided an attempt at this balance, and reviewers suggested that this process be followed and evaluated for its effectiveness to determine best practices for providing a level of consumer protection currently not enjoyed in the industry.

iii. The Market Transformation Subprogram Response

The Market Transformation subprogram agrees with the comments of the peer review committee and shares their view that these activities are an important component of the Solar Program. As they succinctly say, "the technology is only as good as the system to which it is being introduced."

The Market Transformation subprogram also agrees with the feedback that the information gleaned from the Solar America Cities experiences, the State Outreach efforts, and the National Lab analysis needs to be better disseminated to decision makers nationwide for maximum impact. To that end, we are initiating a series of new outreach activities targeting key stakeholders in a position to grow cost-effective solar markets. We are providing new dedicated funding to bring the lessons from the Solar America City partnerships to cities and counties around the country. We are expanding our state outreach efforts to address the full range of solar issues faced by legislatures, public utility commissioners, and governors. We will fund new efforts to reach utilities with updated solar technology information and new business models to support accelerated solar integration. As suggested by the peer reviewers, we will also provide dedicated support to municipal utilities and cooperatives, recognizing their unique needs.

The subprogram also recognizes the opportunity in reaching non-high performing areas of the country. Looking across the United States, there has been uneven solar adoption, with whole regions lacking an appreciable solar market. The Solar Program is responding by directly supporting regional solar market development through funding to multi-state consortia. These consortia partnerships will be able to address region-specific barriers to solar implementation by taking into account geographic differences in climate, regulations, and stakeholder needs. Advancing solar markets in all regions of the country will not only grow the aggregate domestic market, but will also add to the nationwide acceptance of solar energy.

The Market Transformation subprogram agrees that quality assurance is critical in the nascent solar market, and that we must avoid market killing bad actors from becoming widespread. We will continue to support the proliferation of quality solar training through the new Solar Instructor Training Network and other efforts to support quality assurance activities such as personnel certification and training accreditation.

Attachment One: Reviewer Feedback on the Program Review Processes

The following section contains questions that were asked to the reviewers by DOE regarding the program review process and the reviewers response to these questions. This exercise was undertaken in order to continually improve the review process from year to year.

1) **Was the information given to you before the review adequate? What other information would have been useful if provided before the review?**

It was adequate, but as a first-time reviewer it was not clear to me if we were deciding on funding for future activity or making a report card evaluation on activity underway.

Yes. Can't think of any.

The advance information was more than adequate.

Yes, adequate.

The following information, provided ahead of time, would have helped:

The purpose of the peer review process (e.g., it took some of us a while to realize that we weren't helping to decide who gets funded and who doesn't).

Summary of the presentation format ahead of time and how to link the categories in the format with the categories in the project review screens.

Some basic guidelines for how to evaluate each program's accomplishments. For instance, it is not fair to compare the progress of a SAC program in a relatively undeveloped PV market (e.g., Milwaukee) with one in a very development market (e.g., San Francisco). Perhaps a small group facilitated discussion among the reviewers would have helped. We had the opportunity at the initial breakfast but needed more structure for that discussion. You could even have some sample questions that would help reviewers organize their thinking (e.g., "Did the project's choice of what to focus on make the most strategic sense against the basic goal of market transformation?")

For the non-SAC projects, more context on how they fit into SETP's grand strategy in terms of getting all aspects of your program to work in a mutually supportive way.

Yes. But one thing would have helped, that is to make it clear that reviewing the PPT ahead of time is sufficient. I wasn't sure if there were more materials or not.

Mostly yes, but you should definitely let presenters know that when the information is not provided ahead of time, this puts their review at a disadvantage. I know this to be true for those late submittals. While the format is helpful, the supplemental slides especially when they contained technical results provided a great deal of clarity.

Information on PeerNet access and presentations was adequate. It would be useful to include a one (or two) paragraph project abstract with start date, duration and funding level to help the Reviewers frame the presentations.

The copies of presentations were very helpful in setting the stage. It was a pity that not all presentations were available.

The MYPP is a very long introduction. If it had been available a few weeks earlier, I might have been able to be much better prepared. A shorter executive summary would be helpful. The most helpful thing would have been getting the slides much earlier.

A checklist for reviewers should be made available, describing not only the parts of the review process, but also listing the documents that reviewers are expected to review before the panel convenes. For example, for me it was VITAL to read the Solar Program's MPYY 08-13. Reviewers could also have the "template" that was sent to PIs as a guideline on what information was required and they did not present, and what information was not presented just because it was not required. Written reports from each project would be useful during the preparation for the actual review period. If reports CANNOT be made available, at least parts of the proposals could be made available so that reviewers have a chance to evaluate questions or doubts, and gauge whether or not projects are complying with what they originally proposed.

The presentation slides alone were inadequate to prepare properly for the review. Previous peer reviews have sometimes included a document from each project that distilled the project objectives, funding, and current achievements onto 3 pages. These were extremely useful to reviewers for overall perspective on the entire project set.

Yes, the information provided before the review was, for the most part, sufficient. It was useful that the SETP multi-year plan was made readily available. There are, however, other things that would have been useful. For example, since the PV1 and PV2 (PVNT, PVLT) were somewhat mixed in their topics and time horizons, it would have been useful to provide all of the presentations to all of the PV reviewers before the meeting. If there were a conflict of interest, this could be handled in similar way in the PeerNet system as was done with the projects that we were asked to formally review. At the very least, a full list of all PV projects as well as the PV projects in posters should have been made available before the PV review. In addition, reviewers (and the peer review process) would benefit from learning (from DOE) the company names and technology type (e.g. c-Si, CIGS, OPV, CSP dish, PV deployment, polysilicon production) supported by other solar-related federally funded programs such as the 48C Manufacturing Tax Credit Program and the Loan Guarantee Program, Solicitation DE-FOA-0000140. Support of companies by these programs can -in some sense- be considered part of the overall DOE SETP portfolio. Only information already available in press releases need be presented.

The information was generally adequate. There was some confusion regarding logistics, but perhaps that was on my part. My other comment was that the overall objective of the review, being to assess the research portfolio balance rather than assess individual projects wasn't clearly communicated prior to the meeting. Doing so would have been helpful.

2) **Did the presentation format (i.e. how the presentations were organized in the PowerPoint given) give you the information you needed to accurately judge the work being done? What should have been added/removed?**

The format was adequate but the rote style resulted in some redundancy.

Format was fine, but there is a more fundamental issue about the lack of information from some of the presentations, primarily due, I think, to concerns about IP. Few appreciate the need to address a schedule, with milestones, etc, and some consideration of the resources. Most had a rather undisciplined approach to budget, schedule, etc.

I think the format was helpful. It was consistent with the format for reviewer rating and evaluation.

The format was fine. Some presenters just did a better job filling them out. I found that I really needed to sit through the entire presentation before I could judge the work in its totality—which make real-time reviewing a bit challenging.

See bullet #2 above. It took me a couple of presentations to discern the common pattern and relate them to the rating categories I was assigned to fill in. Maybe I'm just slow but having the structure underscored ahead of time would have helped.

Yes, the PPTs were fine.

See my previous answer and add that those presentations that didn't follow the format were again at a disadvantage and got lower reviews. How work was collaborated was also not evident. A list of collaborators is inadequate, because it was obvious that some work got additional funding from other areas.

The uniform presentation category template simplified filling out the Review Forms and comparing the Projects. They are fine as they are.

PowerPoint persuades people that everything in this world can be reduced to a number of bullets - not true. Presenters should all be asked to look at Edward Tufte's "The Visual Display of Quantitative Information"

In most cases the information was adequate. Several of the presentations had very little information on actual goals and results. Overall, there were way too many undefined acronyms. It was as if the presentations were prepared for those already very familiar with all the programs. I would have liked to see a more consistent format of results vs. goals. We know that all goals will not be met, but it is very useful to understand the causes of shortcomings.

In general presentations were good. Some presentations were more complete than others. Although a template was probably given out, it should be emphasized PIs follow it, and that their presentations address the information needed by reviewers to complete the review criteria. Some presenters did not explicitly show the impact their project had in addressing corresponding barriers. Some projects did not present the "relevance slide" Some presentations did not have explicit project goals (against which to measure success). Many presentations did not present clearly decision points or go/no go's scenarios (these are not milestones or objectives). These are potential alternate pathways in case an activity does not turn out as planned. Reviewers had to obtain this “general” information during the Q&A, wasting time that could have been devoted to better and deeper questions. An EXCELLENT presentation, easy to follow and evaluate was presentation number 009 from D. Myers. Also presentation # 010 from D. Renne was good. These presentations provided all the information evaluators needed in a clear and explicit way. These two could be used as examples, in developing templates for the next Peer Review. Some projects reported percent completion as a metric. However, on-going projects did not. A metric could be developed for on-going metric to self-assess their progress during the previous year. In the future consider asking projects to talk about some of the main problems they had during the previous year, and how they dealt with them. These ARE NOT necessarily "addressing the barriers" but rather "implementation problems". A few presenters DID talk about this sort of problems and ways they managed them. This could be shared with other projects for help with their own project hurdles. A few presentations were late (even available THE SAME DAY of the evaluation) giving the panel no time to prepare properly for questions. These presenters should be admonished that peer review is part of their responsibilities in reporting.

The standardized format did help and it provided in most cases the necessary information. It did not work so well for the presentations that encompassed wide areas of tasks, such as several of the National Lab presentations. In my opinion, those need somehow to be more restricted in scope because the many disparate project details overwhelm comprehension in the limited presentation time.

In most cases, the format and information in the presentations and in the PDFs provided before the review was sufficient. There were, however, a number of cases in which sensitive or proprietary information was removed for fear that it would be shared with competitors. This made it difficult for the peer review to be undertaken so that both constructive criticism and suggestions could be offered to the investigators, and an assessment of the project can be made to DOE program managers. There are several ways of dealing with this issue in the future: provide additional written content to the reviewers, and utilize a combination of a closed and open door presentation for those projects that warrant such a format. This was done during the 2009 review. Some of the slides showed the word "Mandatory" at the top. It should be made clear to the presenter that although the slide is mandatory, they should remove that word - even if it means creating a whole new slide with the same title and background. In addition, the initial slides of the presentation should clearly indicate under which solicitation, or project, the work was funded (e.g., University Product and Process, TPP, Incubator, ARRA etc…). In general, encouraging the presenters to provide a standard set of information that includes collaborators, funding, budget, publications and technical details seems to work well and this can be utilized and refined in the future. This provides a uniform and standardized flow of content from which the reviewers can more easily compare and contrast projects and presentations. This is desirable over a free-form presentation style.

Given that our objective was to assess the entire research portfolio, I would have preferred to see shorter presentations from every research effort and perhaps have them grouped by topic (i.e. all III-V PV together, all CIGS together, etc).

3) **Was the scoring criteria proper for this type of review? What should have been added/removed from the scoring criteria? Where the criteria weighted properly?**

I prefer a 5-point scoring criteria. With a 4-point system I must either bias the results towards the bottom of the "bell curve" or the top. I employed a consistently skeptical rubric in response to the 4-point scale. Other reviewers may have been more positive.

Yes. Yes.

The criteria were probably the right combination of quantitative scoring backed up by explanatory comments. The quantitative information should be useful in getting a feeling for consensus impressions, while the comments would tend to be more useful to the presenters. I am not sure the format made the absolute best use of the reviewers' expertise. To some extent we were asked to evaluate whether the projects were consistent with DOE programmatic goals and strategies. While it is reasonable to ask reviewers to understand the context in which the projects are proceeding, in most cases programmatic fit was determined at the time of project selection and funding, so whether or not we thought the projects fit the program's plan seems moot.

The scoring criteria could have been tightened a bit. I found the questions a bit fuzzy and perhaps—depending on personal interpretation—overlapping. I ended up coming up with my own internal definitions, but not sure if they correlate with other reviewers. Or—have a greater discussion of the questions and categories with the reviewers and form designers.

The scoring criteria worked well—well-matched to the structures of the presentations, which in term reflected a rational approach to evaluating the progress of the programs.

All fine.

I would have added application and outreach to the criteria. How the work is actually used was not part of the evaluation and those getting funding should always be considering this. I also think that team member names are important and should be added, only one presentation did this.

Scoring criteria were good. No need to change.

Scoring was proper for Research Programs and most unsuited to the presentations of DOE/NREL programs.

I found the first question, relevance to overall objectives, to be difficult to gauge and hard to comment on. It takes someone with a much better overview of the overall DOE objectives to answer this question. Also, it is very difficult to judge the relevance for the early presentations. This question makes more sense after a day or two. The approach and accomplishments questions are reasonable. Sometimes the presenters couldn't or wouldn't give enough information for me to be able to tell what was actually done. In that case, my assumption is that it wasn't done. In some cases, a much longer and more detailed presentation with more questions would be in order, but that really isn't possible in this kind of review. The collaboration question is somewhat unclear. In some cases a lot of collaboration and many partners is a good thing, but in other cases, more interfaces would dilute the effort and waste time. I tried to score this question by my view of what level made sense for a particular project. For projects with a clear near term goal, the future activities section is useful. For more research like projects, the measurable goals are often unclear. In these cases future plans need to be more flexible, but the directions need to be clearly stated and tracked.

The evaluation form (in PEERNET) must be improved to include a way to give feedback of new projects (e.g., It is difficult to evaluate Criterion 3 for projects that are just beginning). Also, some of the metrics or 1-4 scale on Criterion 4 need to be better explained. The description on Criterion 4 is ok, but for example technology transfer also includes outreach and communication with stakeholders.

I think that the criteria were well aimed and weighted about right. The one area where I think there may have been significant confusion was in the "planning" one. The standard slides seemed not to encourage most presenters to provide any perspective on plans for or implications of their projects beyond 2011(!). In my view this shortchanges the value of the research.

The scoring for the PV section is adequate. Criterion 4 can be refined and weighted more strongly to include business and tech. transfer aspects. Via this criterion, the presenters should be encouraged to give more thought as to how the project is, or will be, benefiting the American taxpayer. This can also include (but not be limited to) calculations from models such as the solar advisor model (DOE's SAM). Since this criterion would be weighted more strongly, DOE could weaken the weight of Criterion 3 on the Technical Accomplishments. In general, the criteria used for the review are close to the selection criteria, and this is highly recommended to continue to assess and consider the project as it matures and advances.

The scoring seemed to be appropriate for technical reviews of individual projects, but that wasn't the primary charter for our review.

4) **Was the schedule for the review proper? Was the Monday session helpful? Were the presentations given adequate time to present and for Q&A? Would it have been useful for the Q&A to have been closed door (for at least some of the projects)?**

The schedule worked fine. The Monday session was interesting but not vital to performing the review. Because as a reviewer I had already reviewed all timely submitted electronic presentations, much of the in-person presentation was redundant and unnecessary. I would have preferred a format where the electronic presentation allowed even more detail, but the human presentation was reduced to 10-15 minutes of what worked, what didn't and what are next steps. Then the agenda should continue to allow time for questions and answers of interest to the reviewers.

Yes. Very helpful. I think it's important for the leadership to keep us up to date on what's being done, what's needed, etc. Yes, except I could have used more time to question some of them. I did this later, and in one case corresponded several times with the PI (who wasn't able to be at the meeting). If the evaluations are going to be thorough, the evaluators must have access to technical aspects. Several of the presentations basically gave no real information. It may be necessary for only DOE civil service employees to review these, and then give the evaluators some idea of the status, without revealing sensitive IP. I did the best I could to evaluate those presentations. However, I had one other concern: there was no indication to us that indeed the information was too sensitive to reveal…it was mostly ignored as an issue, and in once case was only indirectly addressed by the presenter in response to a question. That leaves open the issue of whether the presentations that gave no real information did so because of real IP issues. One, for example, basically showed figures from work done years ago, and several gave very vague views of what the configuration was, or blocked it entirely, or presented very vague descriptions of the process, and I couldn't determine if the work had been done, and wasn't being shown, or if it hadn't been done yet. At the very least, even if the IP details can't be shown, there should be no problem stating the system/technology, etc offers certain capabilities (state the temperature range, for example), characteristics (improved physical properties), performance (such as efficiency increases relative to current technology), etc.

The Monday session was quite valuable. Time allocations were adequate. Conducting the review in public was fine given the length of time devoted to individual projects. More time for drilling down might have called for closed door in some cases.

I don't think closed-door is necessary.

Rigorous but workable. An extra 5 minutes of quiet time between presentations to enter comments on the review screens would have made the job much easier. I think you should encourage reviewers to at least get some comments entered in each box right after the presentation when impressions are fresh. Extremely. Yes—nothing felt rushed. On the contrary, an even shorter format for presentations could work so long as Q&A was not shortened as well. I did not detect any reluctance to be open on the part of the presenters, but I may not have caught some of the political subtleties. I suppose I might have been more openly critical on a couple of occasions if I knew that my questions would not embarrass the presenters in public.

All fine.

Schedule was good, Monday helpful, and close door was not necessary. However, it seems that the analysis SI013 Margolis had many arms. SEGIS and High penetration are one funding pot, but several projects, it seems that this SI013 work also had several projects.

The schedule was good. In some cases, closed door sessions would allow Reviewers to ask business & technology questions not appropriate for the larger audience.

I do not recall any significant event where closed or open door would have made much difference.

The schedule was really quite good. Some presenters (especially some of the NREL projects) had way too much to cover, and it might have been useful to split the presentations. We never actually ran short on Q&A time. There were certainly important questions which were not answered. I think some of the ideas from Friday's meeting could be implemented. The breakout room idea seems like a good one. The presenters need to know that they will be expected to answer the questions in the private forum. I feel that some of the "proprietary" excuses were really "I don't want to say "excuses.

Good timing. Wednesday was full but I appreciated that we did not have to return on Thursday. First day was important in setting the overall stage of the review process, and understand the context and other sub-programs (that I did not participate in). The time for presentations was adequate, and the breaks were properly scheduled. Closed door sessions would probably work only for the projects that had confidentiality issues. I could not judge them properly on their technical merit because the technical information was not available.

The review was too rushed. It was very useful in prior reviews to have time between each presentation for closed-door discussion amongst the committee. Closed-door Q&A would have been very useful in several cases, both for better understanding of some proprietary work and for frank feedback.

The schedule for the Peer review was very well organized and provided a relaxed setting for both presentations and Q&A. In some cases, the Q&A should have been closed door so that more technical content and more aggressive questions from the review panel could be utilized without fear of disclosing intellectual property and sensitive information. This format was done during the 2009 review and simply requires that the audience be asked to step outside for a few minutes and return for the next presentation. In addition, a format can be used where PI's are called back for closed door Q&A as is needed.

The Monday session was somewhat helpful. One recommendation would be that the highlights of the previous year's program review be presented with perhaps an indication of those projects in each of the tracks that are representative of various thrusts and tracks in the program. These can also be selected at random to avoid the appearance of picking winners. Other aspects of the Monday session could include a brief description of upcoming legislation and budgeting issues that would affect the program. This would provide valuable information for all meeting participants. It need not include sensitive or confidential information, but could represent a high level program manager's perspective and observations of what's going on in D.C. Another suggestion for the Monday session would be to design it so that the press and the public are in attendance, together with some members of Congress or their aides. Some of the presentations could be made quite visual with photographs from several of the deployed projects as well as visual representations of actual devices and representations from the funded research. A picture is worth a thousand words. In addition, attendees (and the peer review process) would benefit from learning the company names and (general) technologies represented by other solar-related federally funded programs such as the 48C Manufacturing Tax Credit Program and the Loan Guarantee Program, Solicitation DE-FOA-0000140. Support of solar-related companies can be considered part of the overall federal portfolio. Only information already available in press releases need be collected and presented by high-ranking/level DOE officials.

The schedule was reasonable. I missed the majority of the Monday session due to flight schedule problems thus am unable to comment on that portion. The level of presentation was adequate for judging the overall program quality, but was insufficient to permit full analysis of individual research projects due to proprietary IP concerns.

5) **Was the ORISE system (the software used for entering review comments and scores) useful in scoring the reviews? Was it helpful to be able to score the presentations in real-time?**

I had scored all timely presentations in advance, but there were at least three or four that having the real-time capability was useful because it allowed me to amend my evaluation. The real-time scoring capability was quite useful and should continue.

Yes. I couldn't use that feature during the presentations because I didn't have a computer. Mine had some troubles and was removed.

It really wasn't possible to score the proposals in real time and also listen carefully to the questions and answers, at least for me. It was better to take notes and thoughtfully do the scoring and commentary afterwards.

It was helpful to have the ability to score in real-time…but it was also difficult. One needs to pay attention and ask questions; hard to do that while entering data. Instead, I would highly recommend 5-7 minutes of time after each presentation for reviewers to input thoughts while impressions were fresh.

The system worked perfectly until I tried to revise and complete a couple of my review sheets from the office when I got home. But I don't rule out the possibility of user error in anything related to computers.

Couldn't do the on-line scoring in real time. Took hand notes during the presentations, and then did the on-line later. Works fine. If you try to do the on-line in real time, you will miss the entire Q&A.

No, it was not possible to do this in real time. If we could have had at least 5 minutes between presentations this might have been helpful, but the internet connection was slow and my computer bombed midway. Luckily I had my own computer and all of the available presentations downloaded. This system also prevented comparative ratings. The sessions were organized in the correct order to allow this – like work was grouped together. However, to facilitate consistent ratings across projects, a matrix of similar work projects would have been easier.

I used paper to make notes and score presentations which worked better for me. Advantage: a more detailed evaluation can be made. Disadvantage: the written notes need to be transcribed.

System worked. Ability to do some real times scoring was useful.

I cannot see the ability to score presentations in real time as useful. Unlike some people who believe that they can multi-task, I cannot pay close attention to the presentation, ask questions, and enter scores. I think I spent 20 to 30 minutes entering the score for each presentation. The ability to have the PPT slides available in real time so I could back up and review them was definitely an advantage.

It is o.k. It was confusing the first time when I was about to “logout”, perhaps this could be changed. I had reviewed the projects before the actual panel review, and uploaded answers in the systems. I did not score the presentations in real-time, I was focused on listening to the presenters, their answers to the panel’s questions and taking notes. I made final scoring later, with my notes, and uploaded them into the system. No information was available for the item named “Solicitation” Perhaps in that “Menu option” can be used to include further information about the projects (e.g., the original proposal for each). That would also help at least have an idea of the projects that are late in submitting their presentations.

The ORISE system works moderately well. It could be made far less tedious for downloading project materials en-masse beforehand. I was not able to score presentations in “real time” and I am doubtful that this is even a good idea.

The ORAU/ORISE system is very useful for scoring the reviews and was utilized before, during and after the presentations. The interface is intuitive and the webinars and staff were outstanding. The team provided computers to the reviewers who did not bring their computers and made sure that login and other technical issues were dealt with quickly. This allowed notes and scores to be entered into the system real-time during the presentations. This was invaluable and should be continued in future reviews. One suggestion to include in the PeerNet system/ORISE is a simple set of buttons and icons representing DOE search engines. This can be included in much the same way as the solar energy technologies program multi-year plan as it was incorporated for easy retrieval on the site. The office scientific and technical information provides scienceaccelerator.gov, science.gov, and worldwidescience.org. A button and icon via PeerNet can give reviewers instant and free access to prior studies funded by the federal Government, as well as information about the solar industry. This feature would match what is currently provided to reviewers in many scientific and technical journals during the peer review process. These sites can also be made available to investigators as they develop their presentation materials and upload them to the PeerNet site.

The ORISE software seemed to work well and it was helpful to be able to add reviewer comments real time.

Please provide any general comments you have that were not covered in the questions above in the space below.

It was a real pleasure meeting so many interesting people working on so many useful topics. Thanks for the opportunity to be included as a reviewer.

Above, and in my evaluations, I expressed my basic concern about some of the presentations giving virtually no real information. In addition, I recommended to Prof. Vant Hull that we advise DOE that there are a number of technologies in CSP that have not been given consideration and well deserve it. Some of these have been under development in other countries for many years, but are not considered in the U.S. by DOE. I also think DOE needs to find a way to allow foreign companies, universities, etc to work with U.S. entities. The current limitation of about 10% is insufficient, and as a result, we can't even propose on some of these promising systems/technologies.

The presenters needed content as well as format guidance. The message should be: "The format guidelines and proprietary concerns are not an excuse for giving a technically content free review presentation." The more experienced lab presenters provided a good model for others in most but not all cases. On the average the presentations had adequate technical content. Some were exceptional, both good and bad. The ones that made a particularly bad impression simply provided the minimum information required by the format and were not forthcoming in response to questions. In these cases I recommend a critical project review to determine if the project should continue. Otherwise there is no way to determine if the problem was the presenter or the failure to understand that taking public funds incurs an obligation to share non-proprietary information and insights. Companies and lab staff whose implied message is "trust me" should be vigorously challenged. Very often unwillingness to share information is a big red flag regarding the project's value to the program. Frankly, to do an adequate job as a reviewer of 32 individual projects required a bigger time commitment than I would have anticipated. Organizers of future reviews should be aware of the reviewer time commitments required to properly support the process. I would recommend that in future reviews the reviewers have time for private review team discussions and comparing notes on individual presentations. Given our diverse backgrounds, we can learn from one another, which can significantly enhance the quality of our individual contributions.

I really enjoyed the opportunity; many thanks for extending the invitation.

For me, the whole week was just a huge opportunity to meet leaders in the field, to be among allies in the advancement of this remarkable energy source, and to become acquainted with the overall aims and activities of our national energy strategy. The mix of institutional affiliations of the Market Transformation reviewers was perfect, and the depth of experience and capability of the individuals themselves was most impressive. Thank you for inviting me.

Comments above – it was a real pleasure to take part in the exercise. Thanks for the opportunity.

I was quite impressed with the time and effort put in by the organizers and the reviewers. Everyone took their jobs seriously and tried to make the process very worthwhile.

Some inconsistencies were detected regarding budgets of some presentations (e.g., budget figures that also included activities that WERE NOT part of the project being presented). Presenters should be advised to show ONLY information regarding the project being evaluated through the presentation. If information from other activities is included, it MUST be relevant to the project at hand and must be properly identified as not being part of the project. SI is very important in reducing the number of un-successful projects that become bad examples. NEED TO MAINTAIN a holistic perspective in the SETP, perspective that is attained through good SI and MT sub-programs. An important comment from the presenters was the need to acknowledge that “solar grid integration cannot be not isolated from the context of a changing grid.” The grid needs to be looked at from a more holistic perspective, and closely collaborate with utilities to develop new ways to plan, design and operate power systems in a way that grids are "renewable-friendly". In the long term the philosophy of "grid integration" should just be "grid issues" since the integration should occur much earlier, embedded in grid planning and design. This will require great collaboration among many stakeholders, including EERE's SI, MT's and DOE's office of electricity. There are many DOE-related software tools. This diversity is positive and allows dealing with various aspects of energy issues. However, there should be a "clearinghouse" website for all DOE-supported software (past and present), with descriptions, users guides and explanations of the relationship among these tools. Some basic examples and sample applications could be useful. There is an urgent need to integrate software users and to improve dissemination and communications of software tools to a broader audience. The DOE website, webinars and newsletters are natural choices. However, some stakeholders could also be used. For example education materials could be made available to universities so that professors can use them in classes and in the training of the future workforce. Graduates would have exposure and better awareness of DOE tools and programs.

Lunch: Make the lunch breaks function as smaller get-togethers where each track’s reviewers can exchange ideas about that day’s presentations. Overall: Overall, my impression of the 2010 SETP review in D.C. is good and in comparison to 2009, it was as good or better. Especially encouraging is the amount of interagency collaboration I saw, with meaningful participation from NASA, the Army, BES, ARPA-E and NSF. That's an improvement over 2009 and should be emphasized in reports on the meeting and continued in future reviews. It's an honor to make a contribution in this way, and the review was organized so that it was also a pleasure. File and Name Mix Up (as of Friday, June 4th): PVLT024 and PVPoster24 on PeerNet The mix up stems from the fact that the number for the presentation and poster are both officially "24" and both are from AZ State. There are 4 actions that needed to be taken to correct things (all described with *). The file and project title for PVLT024 on PeerNet is now (as of 6/4/2010) correct but was not correct during the meeting. I pointed out this mix up to SENTECH and ORAU/ORISE during the Program Review meeting, but the response was slow. *The PI's name in the list of projects on PeerNet was, however, not correct and should be changed on the right hand side to include the information in his name: Mani G TamizhMani [FYI- his contact info. is: Arizona

State University Manit@asu.edu; 480-584-7296 in case you need to confer with him] * The PI's name in the [reviewer] Evaluation form should also be changed and checked. This is important because there other questions may come up about this project later!*I also recommend that the PDF name be changed from PVLT024_vanschilgaarde to PVLT024_TamizhMani and that it be re-posted. *Associated with the mix up, PVPoster24 PDF file is not present on the list and the attached file should be uploaded to fill in the blank. Note that the PI's contact information is found on the front page of presentation file in case you need to cross check the information or confer with them. Transitioning DOE Funded Projects to Industry: PVLT017/ "Nanocoax Solar Cells", and PVLT024/" Reliability Evaluation of Concentrator Photovoltaic Modules per IEC Qualification Specifications" both highlight the need to consider business aspects better during R&D work funded by DOE. Resources such as those at the sites below should be made available to the PIs early on so that mistakes can be avoided and so that the full impact and benefit of the work can be made available to U.S. interests and the U.S. economy: Office of Small and Disadvantaged Business Utilization http://smallbusiness.doe.gov/ http://www.business.gov/business-law/contacts/federal/doe/ Small Business Financial Assistance www.sba.gov/financialassistance/ Business.gov http://www.business.gov/ DOE Office of Science and Tech. Info. http://www.osti.gov/ USA.gov http://www.usa.gov/Citizen/Topics/Benefits.shtml Commerce Department http://en.wikipedia.org/wiki/United_States_Department_of_Commerce. Not all SETP projects will result in business opportunities and products, but those that show promise should be identified. Their PIs should learn early on in the process about business-related resources and support that are available to them (such as from the sites above). They should be more carefully guided and educated by the DOE and its contractors on these aspects as part of their work on the project. This may take some interagency collaboration and coordination, but will increase the probability of an overall success and help prevent information and knowledge gained from U.S. federally funded work from being lost overseas. It is recommended that a panel or group be assembled (internally to DOE or externally) that can investigate PVLT017/Nanocoax Solar Cells, and PVLT024/ "Reliability Evaluation of Concentrator..." come up with recommendations for better practices. The business-related lessons from these projects must be learned and applied to other SETP projects so that intellectual property (IP) created from U.S. federally funded projects is utilized in more effective and measurable ways. Pairing a brilliant researcher as PI with a brilliant business developer (and business resources) is necessary for DOE to more rapidly reap the benefits of its portfolio. This model for R&D follows that used by Venture Capitalists as they fund, set up and sit on the boards of successful U.S. enterprises that often efficiently take R&D work from university professors to the international marketplace. DOE should more actively monitor, facilitate and be a steward for this process for those projects that it funds. Both DOE SETP and the funded investigators (PIs) should study the lessons from the stories of innovators such as: Edwin Howard Armstrong, and William Shockley. In both cases, invention was profound, but benefit was not effectively realized in a timely fashion. To learn more, type the two names above into appropriate search engines on the web and contrast the progression of those technologies to that of companies such as Bell Labs, Apple Computer and Google.

Coming into the review, I was not aware of the EERE and BES relationship and was initially concerned that the research portfolio seemed to be biased to the near term. I then realized that we weren't reviewing the BES programs. Perhaps if I'd been able to attend the Monday session that distinction would have been made clear to me. In the future, it may be worth considering having a complete DOE PV review, where both portfolios could be presented. Doing so would really allow the reviewers to get the complete picture of DOE's portfolio. Finally, I found the reviews to be very helpful and have already taken steps to capitalize on synergistic research efforts. I would suggest you continue having DOD and other PV researchers from gov't on your review panel. Doing so is a great opportunity to inform, leverage and coordinate our efforts.

Attachment Two: Program Review Agenda

U.S. Department of Energy Program Review – Opening Plenary

Omni Shoreham Hotel · Washington, DC · May 24-27, 2010

Monday, May 24, 2010 – Regency Ballroom

1:00pm–1:10pm	**Introduction and Welcome –** John Lushetsky, SETP Program Manager
1:10pm-1:40pm	**Keynote Speaker –** Henry Kelly, Principal Deputy Assistant Secretary
1:40pm-2:10pm	**Keynote Speaker –** Sam Baldwin, EERE Chief Technology Officer
2:10pm-2:40pm	**Keynote Speaker –** Robert Margolis, NREL – Solar Visions Study
2:40pm-3:10pm	**Overview of US DOE Solar Energy Technology Program (SETP) –** John Lushetsky, SETP Program Manager
3:10pm-3:25pm	**Overview of Photovoltaics (PV) Subprogram –** Minh Le, SETP Chief Engineer and Acting PV Program Lead
3:25pm-3:40pm	**Overview of Concentrating Solar Power (CSP) Subprogram –** Tex Wilkins, CSP Program Lead
3:40pm-3:55pm	**Overview of Systems Integration (SI) Subprogram –** Kevin Lynn, Acting SI Program Lead
3:55pm-4:10pm	**Overview of Market Transformation (MT) Subprogram –** Charlie Hemmeline, Acting MT Program Lead
4:10pm-4:45pm	**The Wall Street View on Solar Power –** Ahmar Zaman, UBS

U.S. Department of Energy Program Review – Market Transformation

Omni Shoreham Hotel · Washington, DC · May 24-27, 2010

Tuesday, May 25, 2010 – Hampton Room

8:15am-8:35am	**Solar America Cities –** *Boston*
8:35am-8:45am	**Q&A: Solar America Cities –** *Boston*
8:45am-9:15am	**Solar America Cities –** *San Diego*
9:15am-9:30am	**Q&A: Solar America Cities –** *San Diego*
9:30am-10:00am	**Solar America Cities –** *Portland*
10:00am-10:15am	**Q&A: Solar America Cities –** *Portland*
10:15am-10:45am	**Break**
10:45am-11:05am	**Solar America Cities –** *Salt Lake City*
11:05am-11:15am	**Q&A: Solar America Cities –** *Salt Lake City*
11:15am-11:35am	**Solar America Cities –** *Santa Rosa*
11:35am-11:45am	**Q&A: Solar America Cities –** *Santa Rosa*
11:45am-1:15pm	**Lunch –** *Luncheon Talk – Bettina Weis, Sr. Director Photovoltaics, SEMI PV Group*

1:15pm-1:45pm	**Solar America Cities –** *San Francisco*
1:45pm-2:00pm	**Q&A: Solar America Cities –** *San Francisco*
2:00pm-2:20pm	**Solar America Cities –** *Milwaukee*
2:20pm-2:30pm	**Q&A: Solar America Cities –** *Milwaukee*
2:30pm-3:00pm	**Solar America Cities –** *Tuscon*
3:00pm-3:15pm	**Q&A: Solar America Cities –** *Tuscon*
3:15pm-3:45pm	**Break**
3:45pm-4:25pm	**NREL and Sandia –** *Utility and Consumer Outreach Lab Support – Blaise Stoltenberg and Beth Richards*
4:25pm-4:45pm	**Q&A: NREL and Sandia -** *Utility and Consumer Outreach Lab Support*
4:45pm-5:15pm	**NREL –** *Market Transformation Analysis – Barry Friedman*
5:15pm-5:30pm	**Q&A: NREL –** *Market Transformation Analysis*

Wednesday, May 26, 2010 – Hampton Room

8:15am-8:35am	**Solar America Cities –** *Austin*
8:35am-8:45am	**Q&A: Solar America Cities –** *Austin*
8:45am-9:15am	**Solar America Cities –** *Seattle*
9:15am-9:30am	**Q&A: Solar America Cities –** *Seattle*
9:30am-10:00am	**Solar America Cities –** *San Jose*
10:00am-10:15am	**Q&A: Solar America Cities –** *San Jose*
10:15am-10:45am	**Break**
10:45am-11:05am	**Solar America Cities –** *New Orleans*
11:05am-11:15am	**Q&A: Solar America Cities –** *New Orleans*
11:15am-11:35am	**Solar America Cities –** *New York City*
11:35am-11:45am	**Q&A: Solar America Cities –** *New York City*
11:45am-1:15pm	**Lunch -** *Luncheon Talk – Lisa Frantzis and Andy Wickless, Navigant Consulting*
1:15pm-1:45pm	**Solar America Cities –** *St. Paul*
1:45pm-2:00pm	**Q&A: Solar America Cities –** *St. Paul*

2:00pm-2:20pm	**Solar America Cities –** *Berkeley*
2:20pm-2:30pm	**Q&A: Solar America Cities –** *Berkeley*
2:30pm-3:00pm	**Solar America Cities –** *Madison*
3:00pm-3:15pm	**Q&A: Solar America Cities –** *Madison*
3:15pm-3:45pm	**Break**
3:45pm-4:25pm	**NREL and Sandia -** *Solar America Cities Lab Support – Jason Coughlin and Beth Richards*
4:25pm-4:45pm	**Q&A: NREL and Sandia -** *Solar America Cities Lab Support*
4:45pm-5:15pm	**CH2M Hill –** *Support Activities*
5:15pm-5:30pm	**Q&A: CH2M Hill –** *Support Activities*

Thursday, May 27, 2010 – Hampton Room

8:15am-8:35am	**Clean Energy Group –** *Solar State Partnership Project*
8:35am-8:45am	**Q&A: Clean Energy Group –** *Solar State Partnership Project*
8:45am-9:15am	**Interstate Renewable Energy Council (IREC) –** *Stakeholder Outreach/Workforce Development*
9:15am-9:30am	**Q&A: Interstate Renewable Energy Council (IREC) –** *Stakeholder Outreach/Workforce Development*
9:30am-10:00am	**North American Board of Certified Energy Practitioners (NABCEP) -** *Strategic Growth Plan for NABCEP's Solar PV and Solar Thermal Certification and Certificate Programs*

10:00am-10:15am	**Q&A: North American Board of Certified Energy Practitioners (NABCEP) -** *Strategic Growth Plan for NABCEP's Solar PV and Solar Thermal Certification and Certificate Programs*
10:15am-10:45am	**Break**
10:45am-11:05am	**National Conference of State Legislatures (NCSL) –** *State Legislative outreach on Solar Technology and Policy Options*
11:05am-11:15am	**Q&A: National Conference of State Legislatures (NCSL) –** *State Legislative outreach on Solar Technology and Policy Options*
11:15am-11:35am	**NREL –** *Large Scale Integration – Brian Parsons*
11:35am-11:45am	**Q&A: NREL –** *Large Scale Integration – Brian Parsons*
11:45am-1:15pm	**Lunch**
1:15pm-3:15pm	***Poster Session in Blue Room Pre-Function Area – Solar Instructor Training Network***
	Participants: Florida Solar Energy Center at UCF; North Carolina Solar Center at NCSU; Pennsylvania State University; Hudson Valley Community College; Kennebec Valley Community College; Midwest Renewable Energy Association; The Energy Institute at HCC – Northeast; Salt Lake Community College, Solar Energy International, and Utah Solar Energy Association; California Community College Board of Governors, California Energy Commission, California Centers for Sustainable Energy, and the Labor Management Cooperation Committee
1:15pm-1:45pm	**Solar Electric Power Association –** *Facilitating Utility Use and Integration of Solar Electric Power*
1:45pm-2:00pm	**Q&A: Solar Electric Power Association –** *Facilitating Utility Use and Integration of Solar Electric Power*
2:00pm-2:40pm	**NREL and Sandia –** *State Labs – Barry Friedman and Tom Mancini*
2:40pm-3:00pm	**Q&A: Solar NREL and Sandia –** *State Labs*

3:00pm-3:45pm	**Break**
3:45pm-4:25pm	**NREL and Argonne –** *Environmental Impact – Craig Turchi and John Gasper*
4:25pm-4:45pm	**Q&A: NREL and Argonne –** *Environmental Impact*

U.S. Department of Energy Program Review – Photovoltaics: Long Term

Omni Shoreham Hotel · Washington, DC · May 24-27, 2010

Tuesday, May 25, 2010 – Regency Ballroom

8:15am-8:35am	**DOE Headquarters -** *Introduction of Pre-Incubators – Marie Mapes and Martha Symko-Davies*
8:35am-8:45am	**Q&A: DOE Headquarters -** *Introduction of Pre-Incubators*
8:45am-9:05am	**EPIR Technologies –** *High Efficiency Single-Crystal CdTe Solar Cell*
9:05am-9:15am	**Q&A: EPIR Technologies –** *High Efficiency Single-Crystal CdTe Solar Cell*
9:15am-9:35am	**Lightwave –** *Novel Roll-to-Roll Manufacturable Photonic-Enhanced Thin Film Solar Cells*
9:35am-9:45am	**Q&A: Lightwave –** *Novel Roll-to-Roll Manufacturable Photonic-Enhanced Thin Film Solar Cells*
9:45am-10:05am	**Luna –** *High Efficiency Organic Solar Cells*
10:05am-10:15am	**Q&A: Luna –** *High Efficiency Organic Solar Cells*
10:15am-10:45am	**Break**
10:45am-11:05am	**Crystal Solar –** *Thin Single Crystal Silicon Solar Cells on Ceramic Substrates*
11:05am-11:15am	**Q&A: Crystal Solar –** *Thin Single Crystal Silicon Solar Cells on Ceramic Substrates*
11:15am-11:35am	**Microlink –** *High Efficiency, Low-Cost, Multijunction Solar Cells Based on Epitaxial Liftoff and Wafer Bonding*

11:35am-11:45am	**Q&A: Microlink –** *High Efficiency, Low-Cost, Multijunction Solar Cells Based on Epitaxial Liftoff and Wafer Bonding*
11:45am-1:15pm	**Lunch -** *Lunceon Talk - Bettina Weis, Sr. Director Photovoltaics, SEMI PV Group*
1:15pm-1:35pm	**TiSol –** *Innovative Manufacturing of Dye Sensitized Solar Cells*
1:35pm-1:45pm	**Q&A: TiSol –** *Innovative Manufacturing of Dye Sensitized Solar Cells*
1:45pm-2:05pm	**Banyan –** *A Flat ATIR Optics Approach to CPV*
2:05pm-2:15pm	**Q&A: Banyan –** *A Flat ATIR Optics Approach to CPV*
2:15pm-2:35pm	**SpectraWatt –** *Improved Solar Cell Efficiency through the use of an additive nanostructure-based optical downshifter*
2:35pm-2:45pm	**Q&A: SpectraWatt –** *Improved Solar Cell Efficiency through the use of an additive nanostructure-based optical downshifter*
2:45pm-3:05pm	**Ascent –** *ZnMgO by APCVD Enabling High-Performance Mid-bandgap CIGS on Polyimide Modules*
3:05pm-3:15pm	**Q&A: Ascent –** *ZnMgO by APCVD Enabling High-Performance Mid-bandgap CIGS on Polyimide Modules*
3:15pm-3:45pm	**Break**
3:45pm-4:25pm	**NREL –** *PDIL Infrastructure, Engineering, and Integration – Brent Nelson*
4:25pm-4:45pm	**Q&A: NREL -** *PDIL Infrastructure, Engineering, and Integration*
4:45pm-5:25pm	**NREL –** *Measurements and Characterization – Peter Sheldon*
5:25pm-5:45pm	**Q&A: NREL –** *Measurements and Characterization*

Wednesday, May 26, 2010 – AM: Regency Ballroom; PM: Palladian Room

8:10am-8:15am	**DOE Headquarters –** *Introduction to Incubator Awards – Martha Symko-Davies*
8:15am-8:35am	**The Solar Energy Consortium (TSEC) –** *Solar Consortium of NY PV R&D Center*
8:35am-8:45am	**Q&A: The Solar Energy Consortium (TSEC) –** *Solar Consortium of NY PV R&D Center*
8:45am-9:05am	**University of Arkansas at Little Rock –** *Novel PV Devices Based on Polymeric and Carbon Nanostructured Materials*
9:05am-9:15am	**Q&A: University of Arkansas at Little Rock –** *Novel PV Devices Based on Polymeric and Carbon Nanostructured Materials*
9:15am-9:35am	**North Dakota State University –** *Center for Nanoscale Energy*
9:35am-9:45am	**Q&A: North Dakota State University Center for Nanoscale Energy –** *Center for Nanoscale Energy*
9:45am-10:05am	**Solasta –** *Nanocoax Solar Cells*
10:05am-10:15am	**Q&A: Solasta –** *Nanocoax Solar Cells*
10:15am-10:45am	**Break**
10:45am-11:05am	**1366 –** *Self Aligned Cell – Scaling Up Manufacture of a Cost Effective Cell Architecture for Multi-Crystalline Silicon Photovoltaics*
11:05am-11:15am	**Q&A: 1366 –** *Self Aligned Cell – Scaling Up Manufacture of a Cost Effective Cell Architecture for Multi-Crystalline Silicon Photovoltaics*
11:15am-11:35am	**Solexel –** *Productization and Manufacturing Scaling of High-Efficiency Solar Cell and Module Products Based on a Disruptive Low-Cost, Mono-Crystalline Technology*
11:35am-11:45am	**Q&A: Solexel –** *Productization and Manufacturing Scaling of High-Efficiency Solar Cell and Module Products Based on a Disruptive Low-Cost, Mono-Crystalline Technology*

11:45am-1:15pm	**Lunch -** *Luncheon Talk - Lisa Frantzis and Andy Wickless, Navigant Consulting*
1:15pm-1:35pm	**Innovalight –** *High-Efficiency, Low-Cost Solar Cells Manufactured Using "Silicon Ink" On Thin Crystalline Silicon Wafers*
1:35pm-1:45pm	**Q&A: Innovalight –** *High-Efficiency, Low-Cost Solar Cells Manufactured Using "Silicon Ink" On Thin Crystalline Silicon Wafers*
1:45pm-2:05pm	**Spire –** *Manufacturing of High-Efficiency Bi-Facial Tandem Concentrator Solar Cells*
2:05pm-2:15pm	**Q&A: Spire –** *Manufacturing of High-Efficiency Bi-Facial Tandem Concentrator Solar Cells*
2:15pm-2:35pm	**DOE Headquarters -** *Introduction to University Product and Process Development – Marie Mapes*
2:35pm-2:45pm	**Q&A: DOE Headquarters -** *Introduction to University Product and Process Development*
2:45pm-3:05pm	**Penn State University –** *TiO2 Nanotube Array-Organic Semiconductor Heterojunction Solar Cells for Efficient, Low Cost, Large Area Scalable Solar Energy Conversion*
3:05pm-3:15pm	**Q&A: Penn State University –** *TiO2 Nanotube Array-Organic Semiconductor Heterojunction Solar Cells for Efficient, Low Cost, Large Area Scalable Solar Energy Conversion*
3:15pm-3:40pm	**Break**
3:45pm-4:05pm	**Arizona State University –** *Reliability Evaluation of Concentrator Photovoltaic Modules per IEC Qualification Specifications*
4:05pm-4:15pm	**Q&A: Arizona State University –** *Reliability Evaluation of Concentrator Photovoltaic Modules per IEC Qualification Specifications*
4:15pm-4:35pm	**MIT –** *Defect Engineering, Cell Processing, and Modeling for High-Perfomance, Low-Cost Crystalline Si PV*
4:35pm-4:45pm	**Q&A: MIT –** *Defect Engineering, Cell Processing, and Modeling for High-Perfomance, Low-Cost Crystalline Si PV*
4:45pm-5:05pm	**University of Toledo –** *High-Rate Fabrication of a-Si-Based Thin-Film Solar Cells Using Large Area VHF PECVD Processes*

5:05pm-5:15pm	**Q&A: University of Toledo –** *High-Rate Fabrication of a-Si-Based Thin-Film Solar Cells Using Large Area VHF PECVD Processes*
5:15pm-5:35pm	**Georgia Tech –** *Development of Rear Contact Technologies for Next Generation High Efficiency Commercial Si Solar Cells*
5:35pm-5:45pm	**Q&A: Georgia Tech –** *Development of Rear Contact Technologies for Next Generation High Efficiency Commercial Si Solar Cells*

Thursday, May 27, 2010 – Regency Ballroom

8:15am-8:35am	**University of Toledo –** *Improved CdTe PV Modules by Atmospheric Pressure Vapor Deposition*
8:35am-8:45am	**Q&A: University of Toledo –** *Improved CdTe PV Modules by Atmospheric Pressure Vapor Deposition*
8:45am-9:05am	**University of Delaware –** *Development of a Low Cost Insulated Foil Substrate for Cu(InGa)Se2 PV*
9:05am-9:15am	**Q&A: University of Delaware –** *Development of a Low Cost Insulated Foil Substrate for Cu(InGa)Se2 PV*
9:15am-9:35am	**University of Florida –** *Routes for Rapid Synthesis of CuGaxIn1-xSe2 Absorbers*
9:35am-9:45am	**Q&A: University of Florida –** *Routes for Rapid Synthesis of CuGaxIn1-xSe2 Absorbers*
9:45am-10:05am	**University of Delaware –** *High Efficiency Back Contact Si Heterojunction Solar Cells*
10:05am-10:15am	**Q&A: University of Delaware –** *High Efficiency Back Contact Si Heterojunction Solar Cells*
10:15am-10:45am	**Break**
10:45am-11:05am	**Caltech –** *100 mm Engineered InP-on-Si Laminate Substrates for InP-based Multijunction Solar Cells*
11:05am-11:15am	**Q&A: Caltech –** *100 mm Engineered InP-on-Si Laminate Substrates for InP-based Multijunction Solar Cells*

11:15am-11:35am	**North Carolina State University –** *Tunable Narrow Band Gap Absorbers for Ultra High Efficiency Multi-junction Solar Cells*
11:35am-11:45am	**Q&A: North Carolina State University –** *Tunable Narrow Band Gap Absorbers for Ultra High Efficiency Multi-junction Solar Cells*
11:45am-1:15pm	**Lunch**

U.S. DEPARTMENT OF ENERGY | Energy Efficiency & Renewable Energy

U.S. Department of Energy Program Review – Photovoltaics: Near Term

Omni Shoreham Hotel · Washington, DC · May 24-27, 2010

Tuesday, May 25, 2010 – Empire Room

8:15am-8:35am	**DOE Headquarters -** *Introduction and AOP Discussion – Scott Stephens and Ed Etzkorn*
8:35am-8:45am	**Q&A: DOE Headquarters -** *Introduction and AOP Discussion*
8:45am-9:05am	**Omega Optical, Inc. –** *Optical Designs for Solar Power Generation*
9:05am-9:15am	**Q&A: Omega Optical, Inc. –** *Optical Designs for Solar Power Generation*
9:15am-9:35am	**Eikos, Inc. –** *Transparent Coatings for Solar Cell Research Project*
9:35am-9:45am	**Q&A: Eikos, Inc. –** *Transparent Coatings for Solar Cell Research Project*
9:45am-10:05am	**General Electric –** *Efficiency Enhancing Layers for PV Modules*
10:05am-10:15am	**Q&A: General Electric –** *Efficiency Enhancing Layers for PV Modules*
10:15am-10:45am	**Break**
10:45am-11:05am	**General Electric –** *High Energy Yield Distributed Architecture for Large Commercial and Utility-Scale PV Systems*
11:05am-11:15am	**Q&A: General Electric –** *High Energy Yield Distributed Architecture for Large Commercial and Utility-Scale PV Systems*
11:15am-11:35am	**Sierra Solar Power –** *Low-cost, High-throughput Si Epitaxy System for Solar Cell Manufacturing*

11:35am-11:45am	**Q&A: Sierra Solar Power –** *Low-cost, High-throughput Si Epitaxy System for Solar Cell Manufacturing*
11:45am-1:15pm	**Lunch -** *Luncheon Talk - Bettina Weis, Sr. Director Photovoltaics, SEMI PV Group*
1:15pm-1:35pm	**Silicon Genesis -** *Novel kerf-free PV wafering that provides a low-cost approach to generate wafers*
1:35pm-1:45pm	**Q&A: Silicon Genesis -** *Novel kerf-free PV wafering that provides a low-cost approach to generate wafers*
1:45pm-2:05pm	**Varian Semiconductor –** *Manufacturing single crystal silicon sheets*
2:05pm-2:15pm	**Q&A: Varian Semiconductor –** *Manufacturing single crystal silicon sheets*
2:15pm-2:55pm	**NREL –** *Industrial CRADA's – John Benner*
2:55pm-3:15pm	**Q&A: NREL –** *Industrial CRADA's*
3:15pm-3:45pm	**Break**
3:45pm-4:05pm	**Xerocoat –** *A Low Cost Spray Deposited Solar PV Anti-reflection Coating*
4:05pm-4:15pm	**Q&A: Xerocoat –** *A Low Cost Spray Deposited Solar PV Anti-reflection Coating*
4:15pm-4:35pm	**3M –** *Flexible Barrier Films*
4:35pm-4:45pm	**Q&A: 3M –** *Flexible Barrier Films*
4:45pm-5:05pm	**Air Products –** *Enhanced Growth Rate and Silane Utilization in Amorphous Silicon*
5:05pm-5:15pm	**Q&A: Air Products –** *Enhanced Growth Rate and Silane Utilization in Amorphous Silicon*

5:15pm-5:35pm	**Dupont –** *Flexible Ultra Moisture Barrier Film for Thin-Film Photovoltaic Applications*
5:35pm-5:45pm	**Q&A: Dupont –** *Flexible Ultra Moisture Barrier Film for Thin-Film Photovoltaic Applications*

Wednesday, May 26, 2010 – Empire Room

8:10am-8:15am	**DOE Headquarters –** *Introduction to TPP Awards – Scott Stephens*
8:15am-8:35am	**NREL –** *Organic Photovoltaics and Advanced Materials – Matt Lloyd*
8:35am-8:45am	**Q&A: NREL –** *Organic Photovoltaics and Advanced Materials*
8:45am-9:05am	**Konarka –** *Low Cost, Lightweight Solar Modules Based on Organic Photovoltaic Technology*
9:05am-9:15am	**Q&A: Konarka –** *Low Cost, Lightweight Solar Modules Based on Organic Photovoltaic Technology*
9:15am-9:35am	**NREL –** *Film Silicon – Howard Branz*
9:35am-9:45am	**Q&A: NREL –** *Film Silicon*
9:45am-10:05am	**United Solar Ovonics, Inc. –** *Low Cost Thin Film Building-Integrated PV Systems*
10:05am-10:15am	**Q&A: United Solar Ovonics, Inc. –** *Low Cost Thin Film Building-Integrated PV Systems*

10:15am-10:45am	**Break**

10:45am-11:05am	**NREL –** *Wafer Silicon – Qi Wang*
11:05am-11:15am	**Q&A: NREL –** *Wafer Silicon*

11:15am-11:35am	**Sunpower –** *Grid-Competitive Residential and Commercial Fully Automated PV Systems Technology*
11:35am-11:45am	**Q&A: Sunpower –** *Grid-Competitive Residential and Commercial Fully Automated PV Systems Technology*
11:45am-1:15pm	**Lunch -** *Luncheon Talk - Lisa Frantzis and Andy Wickless, Navigant Consulting*
1:15pm-1:35pm	**NREL –** *Concentrating Photovoltaics – Daniel Friedman*
1:35pm-1:45pm	**Q&A: NREL –** *Concentrating Photovoltaics*
1:45pm-2:05pm	**Boeing –** *Concentrator Photovoltaic Power System*
2:05pm-2:15pm	**Q&A: Boeing –** *Concentrator Photovoltaic Power System*
2:15pm-2:35pm	**Amonix –** *Low Cost High Concentration Photovoltaic Systems for Utility Power Generation*
2:35pm-2:45pm	**Q&A: Amonix –** *Low Cost High Concentration Photovoltaic Systems for Utility Power Generation*
2:45pm-3:05pm	**Soliant –** *Concentrating Solar Panels: Bringing the Highest Power and Lowest Cost to the Rooftop*
3:05pm-3:15pm	**Q&A: Soliant –** *Concentrating Solar Panels: Bringing the Highest Power and Lowest Cost to the Rooftop*
3:15pm-3:40pm	**Break**
3:45pm-4:05pm	**NREL –** *Copper Indium Gallium diSelenide Research – Miguel Contreas*
4:05pm-4:15pm	**Q&A: NREL –** *Copper Indium Gallium diSelenide Research*
4:15pm-4:35pm	**Nanosolar –** *Delivering Grid-Parity Solar Electricity for the Commercial Market*

4:35pm-4:45pm	**Q&A: Nanosolar –** *Delivering Grid-Parity Solar Electricity for the Commercial Market*
4:45pm-5:05pm	**Dow –** *Fully Integrated Building Science Solutions for Residential and Commercial Photovoltaic Energy Generation*
5:05pm-5:15pm	**Q&A: Dow –** *Fully Integrated Building Science Solutions for Residential and Commercial Photovoltaic Energy Generation*
4:45pm-5:05pm	**GreenRay –** *Development of an AC Module System*
5:05pm-5:15pm	**Q&A: GreenRay –** *Development of an AC Module System*

6:30pm-8:00pm **Poster Session (Located in the Blue Room Pre-Function Area):**

SBIR - Nanosolar, Inc. (*Printed Solar Cell Using Nanostructured Ink*); TDA Research, Inc. (*Improved Fullerenes for OPV*); NanoSonic, Inc. (*High Performance, Low-Cost Nanostructured Mirror Surfaces*); Midwest Optoelectronics, Inc. (*Novel Interconnection Process for Lightweight Flexible Photovoltaic Modules*); Luminit, LLC (*Flexible Spectrum Splitting Holographic Concentrator*); SVV Technology Innovations, Inc. (*High Performance PV Concentrator*); Luna Innovations Incorporated (*High-Throughput In-Line PV Manufacturing Diagnostic System*); Ultrasonic Technologies, Inc. (*In-Line Crack Detection in Silicon Solar Cell Production Using Resonance Ultrasonic Vibrations*); Applied Nanotech, Inc. (*Non-Contact, Printable Metallic inks for Silicon Solar Cells*); Luminit, LLC (*Multifunctional UV Curable Sol-Gel Organic Hybrid Nanocomposite Encapsulation System*); Crystal Systems, Inc. (*Material Utilization and Waste Reduction Through Kerf Recycling*)

PV Next Gen Awards – ASU (John Kouvetakis), ASU (Marc van Schilfgaarde), Cal Tech (Harry Atwater), Mayaterials (Richard Laine), MIT (Vladimir Bulovic), MIT (Emanuel Sachs), Penn State (Harry Allcock), Penn State (Joan Redwing), Rochester Institute of Technology (Seth Hubbard), Solexant (Alison Breeze), Solexel (Mehrdad Moslehi), Stanford (Yi Cui), University of Cal-Davis (Adam Moule), University of Washington (Alex Jen), University of Colorado (Josef Michl), University of Michigan (Stephen Forrest), University of South Florida (Christos Ferekides), University of California-San Diego (Edward Yu), University of Delaware (William Shafarman), University of Florida (Jiangeng Xue), University of Illinois (John Rogers), Voxtel (David Schut), Wakonda (Leslie Fritzemeier), Stanford University (Peter Peumans), NREL (Vipin Gupta)

ARRA Lab Call – NREL (*Generation Inverted Metamorphic Multi-junction (IMM) III-V Solar Cells*); PNNL (*Multilayer Window for Improved Performance in CdTe Solar Cells*); LANL (*Assessment of Silicon Nanowire Architecture for PV Application*); ANL (*Interdigitated Cu2S Thin Film Photovoltaics*); NREL (*Black Silicon Anti-Reflection: Increased Wafer Silicon Efficiency with Reduced Manufacturing Costs*); NREL (*Imaging Techniques for Statistical Process Control on a Solar Cell Manufacturing Line*); ANL

(*Transparent Conducting Coatings for Cost Effective Photovoltaics Manufactured Using Atomic Layer Deposition*); NREL (*Completion of NREL's Process Development and Integration Laboratory, including the Silicon Wafer Replacement Tool*)

Thursday, May 27, 2010 - Empire Room

8:15am-8:35am	**NREL –** *Theory and Computational Science – Stephen Lany*
8:35am-8:45am	**Q&A: NREL –** *Theory and Computational Science*
8:45am-9:05am	**NREL –** *TCO's – John Perkins*
9:05am-9:15am	**Q&A: NREL –** *TCO's*
9:15am-9:35am	**NREL –** *Cadmium Telluride – Tim Gessert*
9:35am-9:45am	**Q&A: NREL –** *Cadmium Telluride*
9:45am-10:05am	**NREL –** *Sensitized Solar Cells – Arthur Frank*
10:05am-10:15am	**Q&A: NREL –** *Sensitized Solar Cells*
10:15am-10:45am	**Break**

U.S. DEPARTMENT OF ENERGY | Energy Efficiency & Renewable Energy

U.S. Department of Energy Program Review – Systems Integration

Omni Shoreham Hotel · Washington, DC · May 24-27, 2010

Tuesday, May 25, 2010 – Congressional Room

8:15am-8:35am	**NREL –** *Systems Modeling – Aron Dobos and Christopher Helm*
8:35am-8:45am	**Q&A: NREL –** *Systems Modeling*
8:45am-9:05am	**Sandia –** *Systems Modeling – Chris Cameron*
9:05am-9:15am	**Q&A: Sandia –** *Systems Modeling*
9:15am-9:55am	**NREL –** *Photovoltaics Grid Integration – Ben Kroposki*
9:55am-10:15am	**Q&A: NREL –** *Photovoltaics Grid Integration*
10:15am-10:45am	**Break**
10:45am-11:25am	**Sandia –** *Photovoltaics Grid Integration – Abe Ellis*
11:25am-11:45am	**Q&A: Sandia –** *Photovoltaics Grid Integration*
11:45am-1:15pm	**Lunch -** *Luncheon Talk - Bettina Weis, Sr. Director Photovoltaics, SEMI PV Group*

1:15pm-1:35pm	**NREL –** *Concentrating Solar Power Grid Integration – Brian Parsons*
1:35pm-1:45pm	**Q&A: NREL –** *Concentrating Solar Power Grid Integration*
1:45pm-2:05pm	**New Mexico State University –** *Solar ABC's – Larry Sherwood*
2:05pm-2:15pm	**Q&A: New Mexico State University –** *Solar ABC's*
2:15pm-2:35pm	**NREL –** *Codes and Standards – Ben Kroposki*
2:35pm-2:45pm	**Q&A: NREL –** *Codes and Standards*
2:45pm-3:05pm	**Sandia –** *Codes and Standards – Ward Bower*
3:05pm-3:15pm	**Q&A: Sandia –** *Codes and Standards*
3:15pm-3:45pm	**Break**
3:45pm-4:05pm	**NREL –** *Solar Radiometry – Daryl Myers*
4:05pm-4:15pm	**Q&A: NREL –** *Solar Radiometry*
4:15pm-4:35pm	**NREL –** *Solar Resource Characterization and Forecasting – Dave Renne*
4:35pm-4:45pm	**Q&A: NREL –** *Solar Resource Characterization and Forecasting*
4:45pm-5:05pm	**New Mexico State University –** *T&E Activities – Andy Rosenthal*
5:05pm-5:15pm	**Q&A: New Mexico State University –** *T&E Activities*
5:15pm-5:35pm	**FSEC –** *T&E Activities – Bob Reedy*
5:35pm-5:45pm	**Q&A: FSEC –** *T&E Activities*

Wednesday, May 26, 2010 – Congressional Room

8:15am-8:35am	**NREL –** *PV Reliability – Sarah Kurtz*
8:35am-8:45am	**Q&A: NREL –** *PV Reliability*
8:45am-9:05am	**Sandia –** *PV Reliability – Jennifer Granata*
9:05am-9:15am	**Q&A: Sandia –** *PV Reliability*
9:15am-9:35am	**NREL –** *PV Test and Evaluation – Bill Marion*
9:35am-9:45am	**Q&A: NREL –** *PV Test and Evaluation*
9:45am-10:05am	**Sandia –** *PV Test and Evaluation – Jennifer Granata*
10:05am-10:15am	**Q&A: Sandia –** *PV Test and Evaluation*
10:15am-10:45am	**Break**
10:45am-11:05am	**FSEC/Satcon –** *Grid-Smart Inverters*
11:05am-11:15am	**Q&A: FSEC/Satcon –** *Grid-Smart Inverters*
11:15am-11:35am	**Petra Solar –** *Development of Economically Viable, Highly Integrated, Highly Modular SEGIS Architecture*
11:35am-11:45am	**Q&A: Petra Solar –** *Development of Economically Viable, Highly Integrated, Highly Modular SEGIS Architecture*
11:45am-1:15pm	**Lunch -** *Luncheon Talk - Lisa Frantzis and Andy Wickless, Navigant Consulting*

1:15pm-1:35pm	**Apollo Solar –** *An Advanced Grid-tied Inverter, Charge Controller, Energy Monitor and Internet Gateway*
1:35pm-1:45pm	**Q&A: Apollo Solar –** *An Advanced Grid-tied Inverter, Charge Controller, Energy Monitor and Internet Gateway*
1:45pm-2:05pm	**Princeton Power –** *Demand Response Inverter*
2:05pm-2:15pm	**Q&A: Princeton Power –** *Demand Response Inverter*
2:15pm-2:35pm	**PV Powered –** *MPPT and EMS Advancements*
2:35pm-2:45pm	**Q&A: PV Powered –** *MPPT and EMS Advancements*
2:45pm-3:05pm	**ComEd –** *High Penetration*
3:05pm-3:15pm	**Q&A: ComEd –** *High Penetration*
3:15pm-3:45pm	**Break**
3:45pm-4:05pm	**SMUD –** *High Penetration*
4:05pm-4:15pm	**Q&A: SMUD –** *High Penetration*
4:15pm-4:35pm	**NREL –** *High Penetration*
4:35pm-4:45pm	**Q&A: NREL –** *High Penetration*
4:45pm-5:05pm	**Florida State University–** *High Penetration*
5:05pm-5:15pm	**Q&A: Florida State University –** *High Penetration*
5:15pm-5:35pm	**UCSD –** *High Penetration*

5:35pm-5:45pm	**Q&A: UCSD –** *High Penetration*
5:45pm-6:05pm	**APS –** *High Penetration*
6:05pm-6:15pm	**Q&A: APS –** *High Penetration*
6:15pm-6:35pm	**NREL –** *Systems Analysis – Robert Margolis*
6:35pm-6:45pm	**Q&A: NREL –** *Systems Analysis*

U.S. Department of Energy Program Review – Concentrating Solar Power

Omni Shoreham Hotel · Washington, DC · May 24-27, 2010

Tuesday, May 25, 2010 – Diplomat Room

8:15am-8:55am	**NREL-** *Line Focus Systems – Chuck Kutscher*
8:55am-9:15am	**Q&A: NREL-** *Line Focus Systems*
9:15am-9:35am	**Sandia –** *Line Focus Systems – Tim Moss*
9:35am-9:45am	**Q&A: Sandia –** *Line Focus Systems*
9:45am-10:05am	**NREL –** *Systems Analysis – Craig Turchi*
10:05am-10:15am	**Q&A: NREL –** *Systems Analysis*
10:15am-10:45am	**Break**
10:45am-11:25am	**Sandia –** *Dish Research and Development – Chuck Andraka*
11:25am-11:45am	**Q&A: Sandia –** *Dish Research and Development*
11:45am-1:15pm	**Lunch -** *Luncheon Talk - Bettina Weis, Sr. Director Photovoltaics, SEMI PV Group*
1:15pm-1:35pm	**SkyFuel -** *Commercial Development of an Advanced, High-Temperature, Linear-Fresnel Based Concentrating Solar Power Concept*

1:35pm-1:45pm	**Q&A: SkyFuel -** *Commercial Development of an Advanced, High-Temperature, Linear-Fresnel Based Concentrating Solar Power Concept*
1:45pm-2:05pm	**3M –** *Cleanable and Hardcoat Coatings for Increased Durability of Silvered Polymeric Mirrors*
2:05pm-2:15pm	**Q&A: 3M –** *Cleanable and Hardcoat Coatings for Increased Durability of Silvered Polymeric Mirrors*
2:15pm-2:35pm	**Abengoa –** *Development of Advanced Polymeric Reflector for CSP Applications*
2:35pm-2:45pm	**Q&A: Abengoa –** *Development of Advanced Polymeric Reflector for CSP Applications*
2:45pm-3:05pm	**Alcoa –** *Reflector Technology Development and System Design for Concentrating Solar Power (CSP) Technologies*
3:05pm-3:15pm	**Q&A: Alcoa –** *Reflector Technology Development and System Design for Concentrating Solar Power (CSP) Technologies*
3:15pm-3:45pm	**Break**
3:45pm-4:05pm	**Solar Millennium –** *Advanced High Temperature Trough Collector Development*
4:05pm-4:15pm	**Q&A: Solar Millennium –** *Advanced High Temperature Trough Collector Development*
4:15pm-4:35pm	**Abengoa –** *Development of Next-Generation Parabolic Trough Collectors and Components for CSP Applications*
4:35pm-4:45pm	**Q&A: Abengoa (GO18037) –** *Development of Next-Generation Parabolic Trough Collectors and Components for CSP Applications*
4:45pm-5:05pm	**Brayton –** *Brayton Solar Power Conversion System*
5:05pm-5:15pm	**Q&A: Brayton –** *Brayton Solar Power Conversion System*
6:30pm-8:00pm	**Poster Session (Located in the Blue Room Pre-Function Area):** Lehigh University (*Novel Thermal Storage Technologies for Concentrating Solar Power Generation*), CUNY (*A Novel Storage Method for Concentrating Solar Power Plants Allowing Operation at High Temperature*), General Atomics (*Thermochemical Heat Storage for Concentrated Solar Power*), Abengoa (*Reducing the Cost of*

Thermal Energy Storage for Parabolic Trough Solar Power Plants), Abengoa (*Development of Molten-Salt Heat Transfer Fluid Technology for Parabolic Trough Solar Power Plants*), Infinia (*30-kW Maintenance-Free Stirling Engine for High-Performance Dish Concentrating Solar Power*), NREL Line Focus Systems Balance of Plant (Chuck Kutscher), 2009 ARRA lab call award winners (Dileep Singh, Joanna McFarlane, Calvin Curtis, Robert Wegeng, Elise Fox)

Wednesday, May 26, 2010 – Blue Room

8:15am-8:35am **Acciona –** *Indirect, Dual-Media, Phase Changing Material Modular Thermal Energy Storage System*

8:35am-8:45am **Q&A: Acciona –** *Indirect, Dual-Media, Phase Changing Material Modular Thermal Energy Storage System*

8:45am-9:05am **Terrafore –** *Heat Transfer and Latent Heat Storage in Inorganic Molten Salts for Concentrating Solar Power Plants*

9:05am-9:15am **Q&A: Terrafore –** *Heat Transfer and Latent Heat Storage in Inorganic Molten Salts for Concentrating Solar Power Plants*

9:15am-9:35am **Infinia –** *Innovative Application of Maintenance-Free Phase-Change Thermal Energy Storage for Dish Engine Solar Power Generation*

9:35am-9:45am **Q&A: Infinia –** *Innovative Application of Maintenance-Free Phase-Change Thermal Energy Storage for Dish Engine Solar Power Generation*

9:45am-10:05am **Texas A&M University –** *Molten Salt-Carbon Nanotube Thermal Energy Storage for Concentrating Solar Power Systems*

10:05am-10:15am **Q&A: Texas A&M University –** *Molten Salt-Carbon Nanotube Thermal Energy Storage for Concentrating Solar Power Systems*

10:15am-10:45am **Break**

10:45am-11:25am **Sandia –** *Tower Research and Development – Greg Kolb*

11:25am-11:45am **Q&A: Sandia –** *Tower Research and Development*

11:45am-1:15pm	**Lunch -** *Luncheon Talk - Lisa Frantzis and Andy Wickless, Navigant Consulting*
1:15pm-1:35pm	**University of Arkansas –** *Development and Performance Evaluation of High Temperature Concrete for Thermal Energy Storage for Solar Power Generation*
1:35pm-1:45pm	**Q&A: University of Arkansas –** *Development and Performance Evaluation of High Temperature Concrete for Thermal Energy Storage for Solar Power Generation*
1:45pm-2:05pm	**US Solar –** *CSP Energy Storage Solutions – Multiple Technologies Compared*
2:05pm-2:15pm	**Q&A: US Solar –** *CSP Energy Storage Solutions – Multiple Technologies Compared*
2:15pm-2:35pm	**University of Alabama –** *Novel Molten Salts Thermal Energy Storage for Concentrating Solar Power Generation*
2:35pm-2:45pm	**Q&A: University of Alabama –** *Novel Molten Salts Thermal Energy Storage for Concentrating Solar Power Generation*
2:45pm-3:05pm	**Symyx/Halotechnics –** *Deep Eutectic Salt Formulations Suitable as Advanced Heat Transfer Fluids*
3:05pm-3:15pm	**Q&A: Symyx Halotechnics –** *Deep Eutectic Salt Formulations Suitable as Advanced Heat Transfer Fluids*
3:15pm-3:45pm	**Break**
3:45pm-4:05pm	**PPG –** *High Performance Reflector Panels for Concentrating Solar Power Assemblies*
4:05pm-4:15pm	**Q&A: PPG –** *High Performance Reflector Panels for Concentrating Solar Power Assemblies*
4:15pm-4:35pm	**Pratt and Whitney –** *Solar Power Tower Receiver Development*
4:35pm-4:45pm	**Q&A: Pratt and Whitney –** *Solar Power Tower Receiver Development*

4:45pm-5:05pm **Acciona (GO18152) –** *Sensible Heat, Direct, Dual-Media Thermal Energy Storage Module*

5:05pm-5:15pm **Q&A: Acciona (GO18152) –** *Sensible Heat, Direct, Dual-Media Thermal Energy Storage Module*

4:45pm-5:05pm **University of Connecticut –** *Research and Development for Novel Thermal Energy Storage Systems (TES) for Concentrating Solar Power (CSP)*

5:05pm-5:15pm **Q&A: University of Connecticut –** *Research and Development for Novel Thermal Energy Storage Systems (TES) for Concentrating Solar Power (CSP)*

Thursday, May 27, 2010 – Blue Room

8:15am-8:55am **NREL –** *Advanced Reflectors – Cheryl Kennedy*

8:55am-9:15am **Q&A: NREL –** *Advanced Reflectors – Cheryl Kennedy*

9:15am-9:35am **NREL –** *Advanced Concepts – Craig Turchi*

9:35am-9:45am **Q&A: NREL –** *Advanced Concepts – Craig Turchi*

9:45am-10:05am **NREL –***Advanced Absorbers – Cheryl Kennedy*

10:05am-10:15am **Q&A: NREL –***Advanced Absorbers – Cheryl Kennedy*

10:15am-10:45am **Break**

10:45am-11:25am **Sandia –** *Advanced Concepts – Cliff Ho*

11:25am-11:45am **Q&A: Sandia –** *Advanced Concepts – Cliff Ho*

11:45am-1:15pm	**Lunch**
1:15pm-1:45pm	**NREL –** *Storage Systems – Greg Glatzmaier*
1:45pm-2:00pm	**Q&A: NREL –** *Storage Systems*
2:00pm-2:30pm	**Sandia –** *Storage Systems – Nate Siegel*
2:30pm-2:45pm	**Q&A: Sandia –** *Storage Systems*
2:45pm-3:05pm	**NREL –** *Advanced Fluids – Greg Glatzmaier*
3:05pm-3:15pm	**Q&A: NREL –** *Advanced Fluids*
3:15pm-3:45pm	**Break**
3:45pm-4:05pm	**Sandia –** *Advanced Fluids – Bob Bradshaw*
4:05pm-4:15pm	**Q&A: Sandia –** *Advanced Fluids*

Attachment Three: Program Review Attendees

First Name	Last Name	Organization
Jesse	Adams	U.S. Department of Energy
Hussam	Alatrash	Petra Solar
Mowafak	Al-Jassim	National Renewable Energy Laboratory
Harry	Allcock	The Pennsylvania State University
Allison	Aman	Navarro Research & Engineering
Doug	Anders	Boeing
Tim	Anderson	University of Florida
Bruce	Anderson	Wilson TurboPower
Charles	Andraka	Sandia National Laboratories
Homer	Antoniadis	Innovalight, Inc.
Richard	Aspinall	Princeton Power Systems
Suzanne	Atkinson	Navarro Research & Engineering/ Golden Field Office
Harry	Atwater	California Institute of Technology
Ezra	Auerbach	North American Board of Certified Energy Practitioners
Sara	Baldwin	Utah Clean Energy
Samuel	Baldwin	U.S. Department of Energy
Debjyoti	Banerjee	Texas A&M University
Swarnab	Banerjee	Princeton Power Systems
Stephen	Barkaszi	Florida Solar Energy Center
John	Bartlett	New West Technologies
Paul	Bautista	Sentech, Inc.
Salah	Bedair	North Carolina State University
Gavi	Begtrup	U.S. House of Representatives
Andrew	Belden	Meister Cousulting/City of Boston
Levi	Belnap	Rocky Mountain Solar Training Provider (Utah Solar Energy Association)
John	Benner	National Renewable Energy Laboratory
Katherine	Berg	NanoSonic, Inc.
Theodore	Bergman	University of Connecticut
Gerald	Bernstein	City College of San Francisco
Steve	Beuning	University of Delaware
Alexandru	Biris	University of Arkansas at Little Rock
Joel	Blaine	Sentech, Inc.
David	Block	Florida Solar Energy Center/UCF
Philip	Boudjouk	NDSU
Shawn	Bourdo	University of Arkansas at Little Rock
Ward	Bower	Sandia National Laboratories
Lynnae	Boyd	National Renewable Energy Laboratory
Keith	Boyle	Abengoa Solar Inc
Robert	Bradshaw	Sandia National Labs
Adam	Brailove	Silicon Genesis Corporation
Howard	Branz	National Renewable Energy Laboratory

First Name	Last Name	Organization
Alison	Breeze	Solexant Corporation
Peter	Brehm	Infinia
Lee	Brinton	Salt Lake Community College
Randy	Brost	SkyFuel
Marlene	Brown	Sandia National Lab
Adam	Browning	The Vote Solar Initiative
James	Bruner	Oak Ridge Institute for Science and Education
Pablo	Bueno	City College of New York
Tonio	Buonassisi	Massachusetts Institute of Technology
Scott	Burroughs	Semprius
Christopher	Cameron	Sandia National Laboratories
Claudia	Cardona	Luna Innovations
Michael	Carmody	EPIR Technologies
Susan	Carollo	Sentech
Dr. Pam	Carpenter	North Carolina Solar Center-North Carolina State University
Gary	Carver	Omega Optical
Mia	Casabona	Navarro Research and Engineering
Tria	Case	City University of New York
Li Han	Chan	NV Institute for Renewable Energy Commercialization
Mike	Cligget	U.S. Department of Energy
Susannah	Clear	3M
David	Click	Florida Solar Energy Center
Charlie	Coggeshall	New West Technologies, LLC
Al	Compan	Univ. of Toledo
Bob	Conner	Semprius
Miguel	Conteras	NCPV
Ian	Cooper	Ga. Institute Of Technology
Jason	Coughlin	National Renewable Energy Laboratory
Vincent	Cozzolino	The Solar Energy Consortium
Calvin	Curtis	National Renewable Energy Laboratory
Vikram	Dalal	Iowa State University
Steinar	Dale	Florida State University, Center for Advanced Power Systems
Seth	Darling	Argonne National Laboratory
Ujjwal	Das	IEC, University of Delaware
Bradley	Davis	NanoSonic
Neal	De Snoo	City of Berkley
Richard	DeBoard	National Renewable Energy Laboratory
Jennifer	DeCesaro	U.S. Department of Energy
Michael	Deck	Soliant Energy, Inc.
Joe	Deluca	Petra Solar
Michael	Diener	TDA Research
Aron	Dobos	National Renewable Energy Laboratory
Dana	Doran	Kennebec Valley Community College

First Name	Last Name	Organization
Brian	Dougherty	National Institute of Standards and Technology
Raymond	Dracker	Solar Millennium
Christopher	Dymond	enXco
Tina	Eichner	National Renewable Energy Laboratory
Jeffrey	Elam	Argonne National Laboratory
Ahmed	Elasser	GE Global Research
Abraham	Ellis	Sandia National Laboratories
Keith	Emery	National Renewable Energy Laboratory
Erten	Eser	University of Delaware, IEC
Edward	Etzkorn	Solar Energy Technologies Program
Ed	Eugeni	Sentech Inc.
Holy	Evans	Strategic Counsel, LLC
Leon	Fabick	Department of Energy
Qi	Fan	The University of Toledo
Homi	Fatemi	Solexel
Chris	Ferekides	University of South Florida
Vivian	Ferry	Caltech
Tim	Fitzsimmons	U.S. Department of Energy
Kevin	Flanagan	Luna Innovations
Stephen	Forrest	University of Michigan
Elise	Fox	Savannah River National Laboratory
Arthur	Frank	National Renewable Energy Laboratory
Lisa	Frantzis	Navigant Consulting, Inc.
Maryl	Freestone	ComEd
Jeff	Frericks	Boeing
Mark	Frickel	Sentech, Inc.
Daniel	Friedman	National Renewable Energy Laboratory
Barry	Friedman	National Renewable Energy Laboratory
Les	Fritzemeier	Wakonda Technologies, inc.
Patrick	Frye	Pratt & Whitney
Jianming	Fu	Sierra Solar Power, Inc.
Wade	Fulghum	NC Solar Center, NC State University
Andrew	Gabor	1366 Technologies, Inc.
Ross	Galbraith	Infinia Corporation
John	Galiotos	Houston Community College-NE Energy Institute
Mahesh	Gandhi	Princeton Power Systems
Jesse	Gary	U.S. Department of Energy
John	Gasper	Argonne National Laboratory
Russell	Gaudiana	Konarka Technologies
Tim	Gessert	National Renewable Energy Laboratory
Eric	Gimon	AAAS
Dean	Giolando	University of Toledo
Paul	Glatkowski	Eikos Inc
Greg	Glatzmaier	National Renewable Energy Laboratory
Adam	Goldstein	U.S. Department of Energy SETP

First Name	Last Name	Organization
Alice	Gomez	Cornerstone Government Affairs
Jennifer	Granata	Sandia National Laboratories
Louis	Graziano	The Dow Chemical Company
Craig	Grimes	Penn State University
Vipin	Gupta	Sandia National Laboratories
Ross	Guttromson	Pacific Northwest National Laboratory
Scott	Haase	National Renewable Energy Laboratory
Ron	Hamaoui	Petra Solar
Darren	Hammell	Princeton Power Systems
Steve	Hammond	National Renewable Energy Laboratory
Charles	Hanley	Sandia National Labortories
Heidi	Hartmann	Argonne National Laboratory
Paul	Heavener	Princeton Power Systems
Christopher W.	Helm	National Renewable Energy Laboratory
Charlie	Hemmeline	U.S. Department of Energy Solar Program
John	Herb	Solar Junction
David	Herrmann	CH2M Hill/Critigen
Nicoleta	Hickman	Florida Solar Energy Center
Clifford	Ho	Sandia National Laboratories
Sara	Hochman	Wilson Sonsini Goodrich & Rosati
Jeanne	Hoffman	City of Madison
Larry	Holmberg	SolarInfra, Inc
John	Hryn	Argonne National Laboratory
Seth	Hubbard	Rochester Institute of Technology
Ray	Hudson	Lawrence Berkeley National Lab
Roland	Hulstrom	National Renewable Energy Lab
Anne	Hunt	City of Saint Paul
Catherine	Hunt	Dow Chemical Company
Brian	Hunter	U.S. Department of Energy Golden Field Office
Patrick	Hurley	Air Products and Chemicals Inc.
Jonathan	Hurwitch	Sentech, Inc.
Nick	Hylla	Midwest Renewable Energy Association
Linda	Irvine	Northwest SEED
Brian	Iverson	Sandia National Laboratories
Sheldon	Jeter	Georgia Tech and U S Solar Holdings
Jin	Ji	Lightwave Power, Inc.
Steve	Johnston	National Renewable Energy Laboratory
Russ	Jones	Spectrolab, Inc.
Matt	Jones	The Boeing Company
Victor	Kane	Department of Energy
Patty	Kappaz	Sentech, Inc.
Rajan	Kasetty	Terrafore, Inc.
Peter	Kellerman	Varian Semiconductor Equipment Associates, Inc.
Henry	Kelly	Department of Energy
Cheryl	Kennedy	National Renewable Energy laboratory

First Name	Last Name	Organization
James	Kern	U.S. Department of Energy
James	Kesseli	Brayton Energy, LLC
Brian	Keyes	National Renewable Energy Laboratory
Peter	Khaemba	Department of Energy
Petra	Klein	The Solar Energy Consortium
Andrew	Kobusch	Navarro Research & Engineering - U.S. Department of Energy Golden Field Office
Yevgenity	Kocherov	Princeton Power Systems
Greg	Kolb	Sandia National Laboratories
Kevin	Krauth	Sentech, Inc
Benjamin	Kroposki	National Renewable Energy Laboratory
Chelva	Kumar	EPIR Technologies, Inc.
Juanita	Kurtin	SpectraWatt
Sarah	Kurtz	National Renewable Energy Laboratory
Chuck	Kutscher	National Renewable Energy Laboratory
Jason	Lai	Virginia Tech
Richard	Laine	Mayaterials
John	Langan	Air Products
Stephan	Lany	National Renewable Energy Laboratory
Mark	Lausten	Sentech, Inc.
Richard	Lawrence	Hudson Valley Community College TEC-SMART
Minh	Le	U.S. Department of Energy - Solar Energy Technologies Program
Marina	Leite	Caltech
John	Lemmmon	Pacific Northwest National Laboratory
Leslie	Libby	Austin Energy
Yang	Liu	Princeton Power Systems
Matthew	Lloyd	National Renewable Energy Laboratory
Sarah	Locknar	Omega Optical
Joe	Lucas	U.S. Dept. of Energy
Andrea	Luecke	City of Milwaukee
Brian	Luptowski	Abengoa Solar Inc.
Margaret	Lyday	Oak Ridge Institute for Science and Education
Kevin	Lynn	U.S. Department of Energy
Thomas	Mancini	Sandia National Laboratories
Marie	Mapes	U.S. Department of Energy
Julien	Marchal	Mayaterials,inc
Patrick	Marcotte	Abengoa Solar Inc
Robert	Margolis	National Renewable Energy Laboratory
Bill	Marion	National Renewable Energy Laboratory
Evlyn	Mark	Petra Solar
Alex	Martinson	Argonne National Laboratory
Anoop	Mathur	Terrafore
Scott	Mauger	University of California Davis
Beth	McAllister	XeroCoat, Inc.
David	McCallum	MicroLink Devices

First Name	Last Name	Organization
JW	McCamy	PPG Industries
Robert	McConnell	Amonix, INc.
Michael	McDowell	Pratt & Whitney - Rocketdyne
Michael	McElfresh	Intermolecular
Joanna	McFarlane	Oak Ridge National Laboratory
Jim	McVeigh	Sentech, Inc.
Paul	Medwick	PPG Industries, Inc.
Rick	Meeker	FSU Center for Advanced Power Systems
Mark	Mehos	National Renewable Energy Laboratory
Oliver	Meissner	The CONSILIO Group
Adje	Mensah	Petra Solar
Thomas	Metzger	Navarro Research & Engineering / Golden Field Office
Josef	Michl	University of Colorado
John	Miller	Indiana University
Mike	Mills	Dow Chemical Company
Michael	Mills-Price	PV Powered
Kris	Miner	Pratt & Whitney Rocketdyne
Camella	Mitchell	Oak Ridge Institute for Science and Education
Timothy	Moss	Sandia National Labs
Hannah	Muller	U.S. Department of Energy
Paul	Murphy	Varian Semiconductor Equipment Associates
Daryl	Myers	National Renewable Energy Laboratory
David	Narang	Arizona Public Service Company
Michael	Naughton	Solasta Inc
Brent	Nelson	National Renewable Energy Laboratory
Nathan	Newman	Arizona State University
Cyndi	Newman	Arizona Public Service
Marc	Newmarker	ACCIONA Solar Power
Kristen	Nicole	Department of Energy
Gregory	Nielson	Sandia National Laboratory
Thomas	Novet	Voxtel-Incorporated
Sven	Nuesken	Navarro Research & Engineering - Golden Field Office
Patricia	Nugent	The Dow Chemical Company
Sean	O'Connor	Apollo Solar
Maria	O'Farrell	NC Solar Center
Murat	Okandan	Sandia National Laboratories
Joreé	O'Neal	Oak Ridge Institute for Science and Education
Efrain	O'Neill	BEW Engineering Corp.
Frank	Orr	Critigen
Sergei	Ostapenko	Ultrasonic Technologies, Inc
Burak	Ozpineci	Oak Ridge National Laboratory
Alparslan	Oztekin	Lehigh University
Mike	Pacheco	National Renewable Energy Laboratory
Steven	Palmeri	DOE

First Name	Last Name	Organization
Parans	Paranthaman	Oak Ridge National Laboratory
Brian	Parsons	National Renewable Energy Laboratory
Johanna	Partin	City & County of San Francisco
Jim	Payne	U.S. Department of Energy
Doug	Payne	Con Ed
Larry	Pederson	North Dakota State University
Mike	Pellin	Materials Science Division, Argonne National Laboratory
John	Perkins	National Renewable Energy Lab
Mark	Petri	Argonne National Laboratory
Jack	Peurach	SunPower Corporation
John	Pfeifer	Apollo Solar
Denis	Phares	TISOL, LLC
Nicole	Phillips	Oak Ridge Institute for Science and Education
S. Tom	Picraux	Los Alamos National Laboratory
Bruce	Plenk	City of Tucson
Linda Giannelli	Pratt	City of San Diego
Songgang	Qiu	Infinia
Michael	Quintana	Sandia National Laboratories
Justin	Raade	Halotechnics, Inc.
Ryne	Raffaelle	National Renewable Energy Laboratory
Jim	Rand	SCE
Yana	Rasulova	U.S. Department of Energy
K.V.	Ravi	Crystal Solar, Inc.
Tirunelveli	Ravi	Crystal Solar Inc
Mark	Rawson	Sacramento Municipal Utility District (SMUD)
Ramana	Reddy	The University of Alabama
Joan	Redwing	Penn State University
Bob	Reedy	Florida Solar Energy Center at UCF
Dave	Renne	National Renewable Energy Laboratory
Elizabeth	Richards	Sandia National Laboratories
David	Riley	The Pennsylvania State University
Bradley	Ring	US Department of Energy
Steven	Roberts	Oak Ridge Associated Universities
Rick	Robertson	GE Energy
Chris	Rochester	UC Davis
Ajeet	Rohatgi	GA. Institute Of Technology
Andrew	Rosenthal	Southwest Technology Development Institute
Radek	Roucka	Arizona State University
Tommy	Rueckert	U. S. Department of Energy
Miles	Russell	GreenRay, Inc
Emanuel	Sachs	Massachusetts Institute of Technology
Brian	Sager	Nanosolar
Addison	Sanford	The CONSILIO Group
Joseph	Sarubbi	Hudson Valley Community College TEC-SMART

First Name	Last Name	Organization
Melissa	Savage	NCSL
Mesa	Scharf	PV Powered
Colin	Schauder	Satcon Technology Corporation
Adam	Schaut	Alcoa
Frederick	Schmid	Crystal Systems, Inc.
Clayton	Schroeder	ComEd
David	Schultz	Banyan Energy
Doug	Schulz	North Dakota State University
Morgan	Sell	BCS, Inc.
Rathinam	Selvam	University of Arkansas
Lisa	Sena-Henderson	Solar technologies
William	Shafarman	University of Delaware
Alan	Shaffer	Lakeland Electric
Finley	Shapiro	The Pennsylvania State University
Peter	Sheldon	National Renewable Energy Laboratory
Larry	Sherwood	Solar ABCs
Charlie	Shipp	SC Partners
Nathan	Siegel	Sandia
Brook	Simmons	Capitol Decisions, Inc.
Mark	Sinclair	Clean Energy Group
Dileep	Singh	Argonne National Laboratory
Brad	Siskavich	Spire Semiconductor
Bob	Slattery	Sentech
Russell	Smilgys	SAIC, Inc.
Steven	Sneed	Windward Commercial
Arun	Soni	Sentech, Inc.
Alok	Srivastava	GE Global Research
Ralph	Staley	DuPont Company
Amanda	Steindorf	Flagsol LLC
Joe	Stekli	U.S. Department of Energy
Scott	Stephens	U.S. Department of Energy
Blaise	Stoltenberg	National Renewable Energy Laboratory
Tom	Surek	Surek PV Consulting
Kathy	Swartz	Solar Energy International
Brad	Swing	City of Boston
Martha	Symko-Davies	National Renewable Energy Laboratory
(Mani) Govindasamy	TamizhMani	Arizona State University
Coryne	Tasca	Sentech, Inc.
Rao	Tatavarti	MicroLink Devices Inc
Samuel	Taylor	US Department of Energy
Glenn	Teeter	National Renewable Energy Laboratory
Holly	Thomas	U.S. Department of Energy Golden Field Office
Dagny	Thomas	County of Sonoma
Todd	Thornburg	ComEd

First Name	Last Name	Organization
Gregoy	Thronson	Varian Semiconductor Equipment Associates
Joe	Tillerson	Concentrating Solar Power Department
Robert	Torres	Matheson
Sarah	Truitt	Sentech, Inc.
Louca	Tsakalakos	GE Global Research
Mary	Tucker	City of San Jose
Bill	Tumas	National Renewable Energy Laboratory
Craig	Turchi	National Renewable Energy Laboratory
Joe	Tyrrell	3M
Elaine	Ulrich	U.S. House of Representatives
Mark	van Schilfgaarde	Arizona State University
Kaitlyn	VanSant	National Renewable Energy Laboratory
Sergiy	Vasylyev	SVV Technology Innovations, Inc
Mark	Ventura	Boeing
Aarohi	Vijh	Xunlight Corp.
Eric	Vollnogle	Brayton Energy, LLC
Cyrus	Wadia	Executive Office of the President
Kenneth	Walker	Luna Innovations
Qi	Wang	National Renewable Energy Laboratory
Lin	Wang	DuPont Company
Marvin	Ward	American University
Byron	Washom	UCSD
Robert	Wegeng	Pacific Northwest National Laboratory
Bettina	Weiss	SEMI PV Group
Johnny	Weiss	Solar Energy International (SEI)
Jane	Weissman	Interstate Renewable Energy Council
Andy	Wickless	Navigant Consulting, Inc.
Frank	Wilkins	U.S. Department of Energy
David	Wilt	Clarkson
Ross	Wimborne	Sandia National Laboratories
Steven	Wojtczuk	Spire Semiconductor
Bunsen	Wong	General Atomics
Lawrence	Woods	Ascent Solar Energy
Tasha	Wright	City of Santa Rosa
Jiangeng	Xue	University of Florida
Jeffrey	Yang	United Solar Ovonic LLC
Julius	Yellowhair	Sandia National Laboratories
Dora	Yen	SCE (funded)
Edward	Yu	University of Texas at Austin
Ahmar	Zaman	UBS

Attachment Four: Reviewer Comments on Individual Projects

Photovoltaic Near Term Projects

In order to ensure the anonymity of reviewer feedback, reviewer comments are listed in random order for each question

---

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT001_
Presentation Title: NREL & Sandia Photovoltaics Research
Investigator: Etzkorn, Edward

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Good review of the overall program, its content and context in the bigger Solar Budget.

In general, the projects are well aligned with the objectives.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Scott's diagram of how the projects fit into the value chain, both the larger and smaller projects was very useful and appears to fill a majority of the value chain and its challenges.

The strong motivation that typically exists in "for profit" efforts is missing at NREL. This limits the overall efficiency. I believe they are aware of this and work to minimize any inefficiencies.

The team is very strong.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good understanding of how the program fits into the $1/Watt goal.

Programs are more and more driven by metrics, which is good. This needs to be driven through the organization.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Program is built on Collaboration and Tech Transfer. This is a really great direction for the DOE- they understand the sense of urgency for the US PV Industry to recover its lead by getting the development activities commercialized. Funding near term implementation into manufacturing is essential as it appears that the acceptance of PV is growing significantly in the US.

Excellent collaboration and tech transfer.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
New projects (Supply Chain and PV Manufacturing Indicative) look good and the emphasis on collaboration with universities is great.

Continue experimenting with new programs and approaches.

<u>Project Strengths:</u>
Well thought out with regards to the value chain and commercialization.

Measurements and characterization are a strong and needed element of the NREL service.

Project Weaknesses:

Too much funding historically for OPV and amorphous silicon- these technologies have not proven to be either efficient or reliable (in the case of OPV). The focus needs to be on new, novel approaches and further commercialization and cost reduction of those technologies that are in place. The work on cross technology coatings such as those reducing reflection are good ways to improve the existing technologies. New, novel approaches need continued support coming out of the universities, national labs and companies that fund R&D.

The reliability spending seems too low for the national labs in general. There was no reliability discussion in this presentation (I believe it was reported on elsewhere).

The overall charter for NREL to invent is troublesome.

Recommendations for changes to the Project Scope:

Re-evaluate the funding for OPV to see whether the reliability issues can ever be resolved.

Continue the efforts to reinvent NREL's approach to the industry.

As mentioned elsewhere as well, it would be helpful to have the total investment in the DOE (incub, pre-incub, TPP, other) and other agencies of the federal government in PV summarized in a single location.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT004_
Presentation Title: Efficiency Enhancing Layers for Photovoltaic Modules
Investigator: Tsakalakos, Loucas

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Up and down conversion could represent a broadly applicable efficiency improvement technology across the board in PV. Strong incremental improvement efficiencies possible, but since broadly applicable could have a real impact on efficiency and cost per watt that together with many other incremental improvements can push toward DOE's cost goals.

This approach has a strong technology cross-cutting approach. GE claims that up/down conversion is one of the most practical approaches today for boosting efficiency of terrestrial flat-plate PV.

Supports program goals by addressing potential efficiency improvements. This is basically a research project to apply phosphors to PV cells to shift photon energy to more favorable wavelengths.

If successful, the program will align well with DOE objectives.

The cost vs. efficiency tradeoffs are not quantified enough to know if this has the potential to result in better LCOE or cost/W.

Company has not suggested which PV cells (Si TF, Si wafer, etc.) are better technologies based on fundamental analysis or phenomenological observation yet. It seems like they are very early in this exploration.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Technical approach looks promising, but clearly still very early. No definitive proof of concept of improvement yet (i.e. just showing that it doesn't hurt the cell performance isn't very exciting yet). Good indication that some up-conversion is happening. Would like to see the metrics more clearly laid out comparing what has been achieved relative to theoretical optimum.

Approach is to convert high energy light to useful lower energy light by the development of:
•Photoluminescent/down-converting converting layers can be applied to front of a standard solar cell without significant modification in the process
•Increased efficiency with minimal capital expenditure and cost
•Potential to be applied at module level

Some good progress on phosphor properties, but film results are disappointing.

Good team. Solid approach.

The total effect of QE, unwanted loss energy (absorption, scaterring, refl) of the wanted spectrum, fraction of down-shifted photons that are absorbed by the specific PV cell, are interlinked and complex parameters. The modeling thus far is first order and does not take into account the interaction of the above factors.
This program seems focused on CdTe, but that is likely because that is the focus of the corporation.

The technical targets for (a) material, (b) film, (c) cell, are a good initial cut at goals. There needs to be corresponding cost targets to evaluate the merits of the approach.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good technical progress. Still very early. Good team. Not much discussion on cost.

-Modeled DS film impact on multiple PV technologies
-Showed designs with > 10% relative gain

-Evaluated over 30 DS compositions and down-selected to 2 leading materials
-Model shows practical thickness of 2-50 m
-Fabricated DS materials with >90 QE referenced to starting material QE
-Demonstrated quantum splitting mechanism with excitation energies applicable to the solar spectrum
-Fabricated prototype DS films using manufacturable processes and measured excitation/emission
-DS films show promising optical properties
-Demonstrated DS films do not reduce efficiency of thin film solar cells

This project is still in its early stages. To date they have achieved no harm. It would be useful to have a clearer expectation of potential improvements to judge progress.

One to one conversion is doing no harm, but not showing any benefit. The two for one conversion is in its infancy and not well matched to CdTe. This needs to be addressed to a different active element. Much invention is needed to make this project successful.

The use of "QE" as a metric is inappropriate. The measurements need to be better defined before their impact can be understood (at least by me).

The company has down selected the material, and is performing some optimizations of the formulation based on metrology from above.
The presenter did not know why the QE efficiency is at 20% today (what were the limiters - processing conditions?)
Company does not have cost data. This makes assessing viability of the approach very difficult.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good modeling collaboration with RPI. GE can take this to market if it works, which is excellent.

Collaborations with RPI and University of Georgia.

Good use of university resources. The project appears to be aimed at internal GE use, so industrial cooperation is not likely.

The company does not appear to have plans to make this technology available to the outside, and plans to use it for its internal/Primestar production.
It is not clear if GE will in fact become a meaningful PV manufacturer.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Future work plan is good. Reasonable chance of hitting Go-No Go efficiency metric.

The following tasks were identified as future work

•Task 1:–iterative design with input from experimental results
•Task 2:–Optimization of leading DS materials system
–Pursue demonstration of DC materials system with broadband excitation
•Task 3:–Improve QE of DS films to > 40% (Key Go-No Go Metric)
•Task 4:–Develop process flow for integration with PV modules (Key Go-No Go Metric)
–Demonstrate gain on solar cells and mini-modules

It is not clear that the process parameters for film improvement have been identified. It seems that film deposition is in early stage development.

Company continues to develop the material, and metrology, to optimize the film.

<u>**Project Strengths:**</u>
Broadly applicable technology approach. Strong modeling completed. Early results for materials optical properties as promising.

Very analytical in approach.

Potential for efficiency improvement at low cost.

Very interesting problem with great potential.

Project Weaknesses:

Need to relate current and proposed performance and projected cost to impact on efficiency/cost on a module level more clearly.

Need cost and reliability targets for the project understood so that it fits into the overall goal of <$1/Watt.

Understanding how to turn good phosphor properties into useful films.

Focus on CdTe seems misplaced for the 2 for 1 conversion.

Materials problems change after deposition.

This is a very high risk program with limited results at this point.

The PI wasn't present, which significantly detracted the ability to assess the program.

Recommendations for changes to the Project Scope:

Add reliability testing? There is no mention to how stable these processes are.

No changes. Continue on the task getting the 40% QE films. This is a go/no go gate. If 40% is not achievable, determine if the best to date is worth further research.

Choose the optimal PV material to pair the films with.

Given that GE is not a meaningful player in the PV space, one option is to ask them to make this technology available others if they in fact do succeed in making a commercially viable DS film, and to make that a condition of future funding.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT005_
Presentation Title: High Energy Yield Distributed Architecture for Large Commercial and Utility-Scale PV Systems
Investigator: Elasser, Ahmed

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Not clear to this reviewer how much impact this theoretical work will have in the real world and whether this is work GE would have done in house anyhow given its strong financial interest in project level PV system optimization.

This work is technology neutral and can really improve system performance if it works. It will potentially increase energy harvest for arrays. Localized MPPT has been done before, this one appears to address the historical issues with reliability and cost.

This project is aimed at improving utility scale power output by changing the DC/DC converter architecture. So far this is a paper study which has narrow applicability.

The project goals are in line with DOE objectives.

This program addresses a key aspect (BOS/inverter) of PV program.
GE is behind many others in identifying the opportunity.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Barriers well addressed. Just not clear if the view was worth the climb here.

Great analysis of the system level architecture required for the MPPT to be successful. MPPT will be done on the module, string or string combiner level. This takes it away from the Inverter.

Approach is to establish baseline central inverter architecture 1MW or larger, mc-Si and CdTe; quantify impact of factors (Shading, Mismatch, Cabling, MPPT, Soiling…) affecting the energy yield on baseline architecture; the determine increase in Energy yield with distributed MPPT architectures; determine reliability requirement for distributed DC/DC converters; compute cost of distributed architectures as we move the MPPT from central inverter to module level.
The combination of power electronics know-how, reliability expertise, modeling capabilities and knowledge of PV plants allows GE to assess both central inverter and distributed architectures and to compare their performance.

Detailed quantification and modeling of architectures was presented. Better justification of assumptions would improve the usefulness of the results.

Disciplined, well organized approach.

The approach appears to be objective, and is not looking for a specific outcome: they are open to finding out the correct location (in the hierarchy) for the MPPT inverter.

Company is creating models for shading, mismatch, cabling, soiling for impact on energy yield, based on Matlab, Excel, etc. however the assumptions in the model are unclear/confusing in this 20 minute presentation.
Company has modeled the impact of reliability.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Some elements of the model didn't quite make sense. Application/assumptions around permanent shading didn't quite make sense in that value of the proposed approach would be to minimize problems associated with local heavy shading.

Used innovative approaches to modeling Cost, Reliability, and Energy yield:
– Developed a detailed model in BlockSim™ to assess MTBF, availability, and throughput of distributed architectures
– Developed detailed model (Spice and Matlab) to quantify impact of dynamic and static MPPT effect on energy yield for distributed architectures
– Developed Excel, Spice, Matlab, and MathCAD based tools to assess impact of shading, mismatch, cabling, soiling on Energy Yield
– Developed detailed models for cost analysis in collaboration with GE Solar to

quantify Cost per Watt, NPV, and LCOE of central and distributed architectures.

Most initial modeling is complete. Publishing the reliability study results would be useful for those who design large systems. The expected improvements suggested by the models were not clearly presented.

Very hard to establish the progress with the information given.

The company is 2-3 years behind others in identifying the components of the inverter (DC-DC boos, MPPT, DC-AC conversion, eliminating panel matching issue, etc.) that can be optimized with distributed inverters/electronics.

The company is likely behind others in implementing and/or developing compelling architectures that eliminate the cost, reliability limiters, and higher efficiency. Company has not proposed a clear wire-line or wire-less approach for communication between units (and communication to the macro grid, for per-panel inverters) Company has discussed the problem statement, and has preliminary models and tradeoffs identified. Company has developed what are likely good models (Blocksim, Matlab, Excel, etc) to compare various distributed schemes in different geographies and impairments to existing centralized solutions.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
GE is likely to deploy this knowledge.

Working with Sandia for testing hardware which is great. They are also focused on UL labs for certification. The GE team will be setting up a test site to compare standard system architecture and the MPPT approach.

This project is aimed at internal GE use, although it may be more widely applicable.

More interaction with array operators would have strengthened the model assumptions.

GE is not working with anyone else, and intends to keep this technology in house.
Given that GE today does not have meaningful presence in the PV panel market today, this approach is not a good use of government funds.

Given that GE already sells inverters commercially, it is not clear why they do not intend to make this solution available commercially.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Looking very forward to seeing the experimental results with their exciting module and system level innovations. Just the initial modeling part presented at this meeting.

FY 2010: By End of August 2010 – Complete Task 2:
Hardware Design and Requirements:
• DC/DC and DC/AC Converter Topology Selection
• Simulation, Preliminary Testing, Cost, Reliability, and Integration subtasks
Milestone: Preliminary Design Review of DC/DC Converter
Go/No Go Decision by end of August 2010
If Go Decision: Start Task 3 -
Task 3: Lab-Scale Hardware – start in 2010 and stretch to 2011
Task 4: Hardware Integration starts in 2011

Short term, it would be most useful to more clearly state the assumptions and clarify the expected improvements of the new architecture. Considering the long lead time for utility scale system installation, rapid deployment of these changes would require better quantification of the system gains.

This is mostly a tactical program, with little research content. If it gets a positive light at the go/no-go milestone, the models will be validated and updated over time. It is not clear if there is any novel circuit technology or topology for the inverter.

Project Strengths:
GE has strong motivation to develop and deploy these module/system level innovations. Modeling work is thorough and sets up the rest of work well.

Great analytics and summary of project- looks like the system reliability can be the same as that with a central inverter. GE's approach to R&D is always with an eye to commercial deployment which is important at a system level.

Moving the MPPT to module level has potential to increase energy yield by 8% to 12% over the central Inverter architecture (Location and Technology dependent)

Extensive modeling. Very systematic approach.

Project Weaknesses:
Seems to this reviewer that GE may have funded this work internally anyhow and the additionally of these DOE funds may not be very high for this project.

Needs to move fast towards hardware build and test.

Not clear that this problem is worth solving.

Showing the power gain potential at the module level with data would have strengthened the presentation.

The estimates of arrays losses were taken from literature, which is very reasonable first step. The cost estimates would have been strengthened with real world data. I suspect that alleged gains are critically dependent on these estimates, as well as the distribution of the loss estimates. For instance if the soiling were uniform (which is a reasonable assumption), then there would be no gain. The estimate that 1/2 of the soiling loss would be recovered is unsubstantiated.

What is the cost goal? Is it in sight? Very unclear on the final product.

Recommendations for changes to the Project Scope:
Cost estimates need to be reviewed to see whether the increase in system performance is offset by the increased amount of hardware.

Complete the analysis of the architectures and detail the results. Use utility scale vendors to determine if further research is warranted.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT006_
Presentation Title: Low-cost, High-throughput Si Epitaxy System for Solar Cell Manufacturing
Investigator: Fu, Jianming

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The presenter did not make it clear at all where high throughput Si epitaxy will have a big impact on developing cheaper and better Silicon solar technology. There was some discussion of UMG substrates, but no clear work on showing growth on UMG substrates (as far as this reviewer understood) or attempt to look at minority carrier lifetime, which is a big issue with low purity substrates and is not addressed by quantifying defect density alone.

This project is aimed at cost reduction in high efficiency silicon solar cells.
A high throughput, low cost Si epitaxy system would allow reduction of cost of Si materials, and enable new photovoltaic devices due to its high crystalline quality and better control of film properties.

A high throughput, low cost Si epitaxy system would allow reduction of cost of Si materials, and enable new photovoltaic devices due to its high crystalline quality and better control of film properties.

This project addresses cost reduction of C-silicon cells by reducing the cost of epitaxial silicon layers. Lower cost epitaxy on inexpensive wafers could reduce the cost of starting substrates.

It is not clear exactly how this project will significantly impact the LCOE. It seems to be an enabling technology for solar cell designs that do not exist (yet?). That appears to be two levels of risk, which may be too much.

The company's key innovation/product is the chamber/reactor design of the high throughput EPI machine. Business model is to sell equipment.
Questions not answered during the review were:
- what is the source of the cheap MG wafers/can you secure a large supply?
- will the quality of the EPI grown on the cheap substrates achieve reasonable cell efficiencies?
- thick EPI equipment already exists (such as for power semiconductor wafers). The company

did not show why having defects of ~200 EPD will be satisfactory for PV cells, while that is not the case for other semi applications.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Metrics for the project poorly defined. Poorly scoped project with ultimate goal and impact not clear at all to this reviewer.

Approach:
– Develop a silicon epitaxy chemical vapor deposition (CVD) system for mass manufacturing of photovoltaic devices, enabling new solar cells manufactured with low cost and high efficiency.
– The system throughput will be >200 WPH.
- Wafers are placed in a confined space to significantly increase the reaction of precursor gases in the reaction zone. 30% of utilization efficiency for trichloride silane according to simulation.
- Lamp heating panels are sandwiched by process chambers. This greatly reduced the power required to heat up the wafers.

The costs of producing epitaxy layers is well addressed by equipment design and relaxed uniformity requirements compared to IC epitaxy layers. Equipment cost/wafer, throughput, materials usage, automation, and maintenance have been considered.

Good team. Reasonable approach.

The company did not provide any details on metrology that correlates the quality of a given wafers to its potential PV cell efficiency or yield. There needs to be a much better understanding of defect types and density on the performance and yield of PV cells. It was not clear from the presentation how/if the substrate impacts the Assuming technical success (which is not clearly defined), the calculation for LCOE savings was not clear. (claim of 11-13c target?)

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good epitaxu achieved, low defect densities. Technical design and work are good, but impact on solar technology is unclear.

- Prototype system is functioning and capable of running process tests.
- Need more efficient and reliable operation, as well as improvement of film properties.
- Two patents were filed.

Initial equipment results look promising. Some equipment rework may be in progress.

The overall utilization of the gas seems low.
The company has expertise in EPI chamber design, and has been able to build a unit and demonstrate wafer production with 13-15um EPI, and performed basic metrology on the wafers.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Not much clear coordination and not clear who deployment partners are, but this may have just not been identified in order to keep this secret for business reasons, which is totally reasonable.

No collaborations identified.

Some unidentified partners to evaluate results.

none.

The company needs to engage will cell makers in order to understand the downstream impact, however minimum, of this new wafer on other equipment on the line.
The company also needs to engage (a) customers in order to substantiate the cost savings on cells

in $/W, etc. and (b) suppliers of the MG wafers/starting material.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Not very clear where they want to take the technology from here, who key deployment partners are, and what the ultimate goal of the project will be going forward.

Hardware Modification for Improvement will be done Q2
Preliminary Repeatability and Reliability Tests Q3
Final Prototype Modification Q4
Full Repeatability and Reliability Test, Product Release Q4-10 through Q1-11
Process Development for Specific Solar Application and Demonstrations Q2-11
Beta Site Tests Q2-11

At this early stage it is not clear what new problems will be found. The future research plan will depend on the results of tests. Better identification of the source and cost of substrates and the cost of finished epitaxy wafers should be addressed. A plan should be presented to evaluate possible film contamination from the initial substrates.

Project Strengths:
Strong Si epitaxy innovation for lower cost, higher throughput tool.

A lot of work has been done.

Goals are really strong- 11-13 cents/watt at 17-19% cell efficiency

-Prototype of an innovative high-throughput, low-cost silicon epitaxy system has
been designed and built. The system has been running for process tests.
-Completed the system assembly, installation and integration.
-Proved the feasibility of design concepts: large throughput, high gas utilization, and low power consumption.
-Initial characterization of film property is encouraging: low defect density and good thickness uniformity.
-Identified the area for hardware improvement for and film properties

Novel design approach to maximize throughput while minimizing materials and power usage.

Novel elements to the equipment approach.

Project Weaknesses:

Relevance to have large impact on any particular silicon solar technology in terms of cost and performance is not clear at all to this reviewer.

Wafer size should be scaled to the 155x155 size as most of the industry uses this and it will affect module costs. Need partners to test quality of wafers for making cells. Will need a good mono silicon cell partner to really understand the differences and potential for these wafers.

The final application for solar is really in question. Other than the peeling off reusable wafers, I see no real application for this tool, even if successful. Sounds like a development effort for an advanced VLSI tool.

Recommendations for changes to the Project Scope:

Make sure that it is clear that the application can have a strong impact on a commercially relevant silicon solar technology. Are we solving a relevant problem for a technology that could have a big impact in the solar sector?

Move forward with the project.

Quantification of expected cost of wafers earlier in the program.

More emphasis on film quality.

The company should clearly explain the source of the starting material. They should process preliminary wafers with a cell manufacturer, and have more rigorous metrology to direct and focus the development activities.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT007
Presentation Title: Novel Kerf-Free PV Wafering that Provides a Low-Cost Approach to Generate Wafers from 150 mm to 50 mm
Investigator: Brailove, Adam

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This is a very novel and creative approach. Exactly the kind of thing DOE should be funding: High risk and early. Important to keep tabs on what production metrics will be required for this to be competitive and not to fund this kind of approach too long if it looks like it just can't get there. A high efficiency kerfless wafer is a game changer if the cost can be low enough.

Relates to the DOE objectives as follows-

• 'First-order' impact on high-cost points in PV value chain: material usage and wafering
• Disruptive technology
• Potential for near-term commercialization
• Enabling technology for roadmap to very thin <100 micron wafers

A unique approach to eliminating kerf loss by cleaving thin wafers off of Si bricks. This is a major equipment development effort. The targeted area is Si usage and cost reduction.

The goals align well with DOE objectives.

If successful, this is a great solution to address the silicon content of Si-wafer based PV cells. The key issue is that their solution is focused on 111 wafers, and the bulk of the industry is on 100. This is the main weakness of this plan. If the company could pursue a variant of this technology for 100 wafers, this category would be ranked a good-outstanding.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent approach to R&D. Very clearly laid out steps and tech metrics.

Approach has been to separate the energy "breakage" or ion implantation from the cleavage of silicon. High dosage limited to only where cleave initiation is required.

Problem is that it has been developed on 111 silicon.
Negative is that it now requires an in-vacuum brick cooling technique and the cleaving requires a higher ion energy dose.

Well thought out commercialization path. Pilot cost and yield data is an important milestone in this project.

It appears that the basic technology has been carefully studied and demonstrated. The design and construction of equipment to implement this process is well underway.

Strong team. Good approach.

The company is in the early-mid stages of development. The company has developed PV cells (with partner) on their substrates, and shown little difference with standard wafers. (data not provided - described verbally)Good equipment and modeling of laser cleaving and crack propagation. The company has verified surface conditions post cleave; is tracking thickness variation control, carrier lifetime, etc.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good technical progress. Exciting early results. Definitely worth continuing support.

Hardware is now in place for implanter and facility.

Implanter design and parts fabrication & procurement is 100% complete.

Cleavage has been demonstrated with lasers to reduce energy required for cleave propagation.

The equipment construction is well under way. The project is still in the early stages of equipment testing. There is significant work to be done on increasing the beam current and demonstrating reliability and throughput.

10% of spec presently obtained, going to 50% in the R&D machine. Clear identification of the risk.

Company has been successful in producing multiple wafers removals (claim was 23) from a single wafer/ingot. It has worked with partners to produce working PV cells.
Company claims to be able to cut wafer cost (raw starting wafer) by 2X. However, it is too early to know sufficient details of the implant costs, surface re-prep post cleave, etc. to verify this claim.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Might be nice to see some collaboration with a big implant company like Varian. Not clear what solar cell suppliers are going to want 111 wafers. Important to have that collaboration in place.

Collaborating with REC on wafer supply to make cells.

Have collaborated with GaTech in the past to reference cell performance.

Partners have been lined up to evaluate the wafers and make devices.

More interaction with wafer users would strengthen the effort.

REC is a credible partner, and the ability to produce cells has been critical to verify the viability of this technology, assuming success.
Wafer handling of thin wafers throughout the line is an issue that needs to be addressed with additional customers.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Very strong path forward for increasing beam current and getting to production spec.

Implant
– Assess hardware results from initial alpha implanter operation (2010/2011)
– Assess life of components struck by beam and develop improvement plan (2010)
– Kickoff of beta tool design (2010)– Continue scale-up of beam current to spec and beyond if possible (2010,2011)
– Make improvements in equipment reliability, uptime etc. (2010, 2011)
– Kickoff factory automation design (2010)

Cleave:
– Complete cleave prototype evaluation tools (2010)
– Select and develop alpha cleave tool (2011)
– Ongoing process optimization and dose reduction (2010, 2011)

Because the equipment needs to be completed and characterized, the future research required is not yet clear. There appear to be good plans to characterize results and build on the previous work.

None.

100 wafer alternatives not being pursued in their plans.

Project Strengths:
Very novel approach to kerfless wafers. Good progress to date. Worth continued funding.

Commercially driven by collaboration with REC. Good estimates of energy required to keep costs low.

Projected cost benefit is to cut wafer costs in half.

Good progress on the equipment and characterization of the cleave process.

The limited success to date is impressive.

Project Weaknesses:

Need a good focus on what kind of throughput is needed to win economically. Need a good partner on 111 cell development/deployment. Concerns about ultimate scalability.

Is energy required to do this in excess of what is cost effective?

Identifying the path to 10x to 20x beam current improvement.

Total lack of wafer quality is troubling. Only GIT test data was referenced.

Clearer discussion of the chemical surface treatments is needed. It seems that this problem is being pushed off to the future.

The need for two tools and the low throughput represent significant cost challenges.

Recommendations for changes to the Project Scope:

Get a good 111 cell partner. Maybe partner with Varian or other ion implant equipment developer.

None. Keep funding this project at the planned level.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT008
Presentation Title: Floating Silicon Method (FSM)
Investigator: Kellerman, Peter

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Great high risk, high reward DOE project. Good size for proof of concept $3M. Kerfless single xtal silicon solar wafer will be a game changer for silicon PV. U.S. has a strong core here and should continue to support it at DOE.

Project is aimed at wafer production without any kerf- this is a deposition method which creates single wafers at a time which has significant cost reduction potential to help get silicon costs to <$1/Watt.

This is a well planned development to reduce Si substrate cost. The capital equipment cost should be reasonably low compared to some other thin wafer schemes.

The project goals are in line with DOE objectives.

If successful, this techniques has the ability to produce 100 (or other orientation) substrates with significantly lower Si feedstock. Company claims 3X reduction in starting wafer.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Good approach to equipment design. Good early modeling work.

Develop a tool that will deliver a drop-in 156mm Solar wafer substrate, enabling fast adoption by existing PV manufacturers
– Capable of future evolution to thinner and wider substrates

Capitalizes on work done on Horizontal Ribbon Growth done in the early '80s in Japan. Great literature search and references.

R&D Approach: Parallel development
– Analysis: calculations and computational modeling
– Engineering: development of required system components
– Experiment: multiple test stands to accelerate learning

Very complete modeling capabilities and very strong equipment development capabilities. The ability to separate crystallization from pulling and cooling allows for better control. Many potential problems have been identified and addressed.

Good team, reasonable approach.

Company has a good focus on modeling (Ansys, Fluent). They are exploring several techniques for controlling the meniscus (patents pending).
The company isn't far enough along yet to be able to produce a cell on a small sample, but should produce this as soon as possible.
Multi-disciplinary team with complementary skills and depth of expertise is crystal growth.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
It is hard for this reviewer to believe (very hard) that they will achieve single crystal growth with a seed. They may still get some good poly results though.

Varian has met its initial program goals as spelled out in the program.

First Si melt on 3/25/10
– Temperature scans using traveling thermocouple
– Accurate temperature control using pyrometers
– Ability to visually discern melt/solid interface on surface of melt in crucible

Modeling
Different aspects of FSM require different approaches
Solidification of sheet (heat flow and phase transition)
– First order calculations
– Computational Fluid Dynamics (CFD) two phase (solid, melt) model

Thermal environment
– CFD model – Heat flow, radiation and convection
Si flow, pumping and meniscus stabilization:
– CFD two phase FEM model (gas, melt)
Fixed mesh approach
Adaptive mesh approach
– First order calculations for pumping of molten silicon
– First order calculations for sheet flexure and buoyancy effects

Progress to date appears to be quite rapid. The ability to draw on internal resources and previous equipment development improves response time.

They are just on the leading edge of this development. The toughest problems are ahead. The quicker they address them, the quicker they can make meaningful progress.

The laboratory, and modeling and analysis (FE, temp, flow, etc.) accomplishment so far are consistent with plan, but not significant for the overall scope of this program. The preliminary temp correlation between the equipment and models are promising. The key step will come in late 2010 (Operational Step, scheduled for Sep 2010)

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Big company that can deploy this themselves if they want to. Would be good to see a cell level partner.

Partner not identified and appears that there is no university partnership.

This project is aimed at developing a commercial system to be sold to users. Technology transfer is not an issue and there is a direct commercialization path.

Company has already found a cell manufacturing partner for when wafers/substrates are available.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Good future plan. Looking forward to seeing experimental work.

Plans for upcoming year:

Sheet solidification
• Development of key process steps and control algorithms
• Demonstration of high anisotropic freeze rate

Followed by:
Process Test Stand Optimization
• System level integration and optimization
• Proof-of-Concept demonstration

They have clearly identified the future project goals and approaches.

More focus needed on the testing of the stability of the meniscus and issues of wetting and de-wetting.

Since the bulk of this project is still ahead, the plan for the future are significant. The outline of activities is well presented.

Project Strengths:
Strong high risk high reward kerfless high efficiency silicon wafer project. Well worth doing. Single crystal ability and continuous flow ability both seem pretty tough to do, but worth trying.

Novel kerf-free approach. Based solidly on learnings from literature searches and prior work to avoid problems that have already been identified. Varian has really thought about the problem and has used extensive modeling to guide the design.
Varian is already in the solar market, having developed a doping tool using ion implantation.

Planning and modeling. Rapid progress. Clear goals.

Equipment experience of the team is a plus.

Project Weaknesses:
Not clear what performance metrics are in the end (i.e. minority carrier lifetime target, cell efficiency target). Single crystal seed approach hard to believe in.

Hard to say whether the costs and quality will be there. No mention of where it fits in the overall picture of $/Watt cost reduction with specific numbers.

This is a fairly well-trod area of material science. Many of the issues the team will face working with solidifying silicon have been looked at by others. Some have been solved, some have not. An effort to learn from prior work would be useful.

Recommendations for changes to the Project Scope:
Make sure you have strong materials/cell metrics for this project that go up with time.

None, keep going with planned funding.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT009
Presentation Title: Industrial CRADAs
Investigator: Benner, John

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
NREL is really working hard to engage with industry and venture capital to get its innovations to market through this program. Bravo!

The idea of doing internal competitions for big new ideas is brilliant. I'd like to see that effort increased significantly. Great minds at NREL need some freedom to go after big new ideas.

Project is critical to the EERE Solar Program and supports the DOE RD&D objectives by taking highly developed NREL programs and bringing them out to commercial companies in a partnership approach.

Projects selected to assist industry in applying NREL PV experience and expertise to develop or improve products

This is only an overview of the program. It is hard to judge how well many programs align with the program goals.

Keeps NREL relevant. Aggressive and innovative.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Good process for gradually developing industry relationships. Need to be sure that fairness of opportunity is there at some part of the chain in the development of these relationships.

- Execute competitive process to select projects
- Fund selected NREL teams to initiate and deliver research to industry partners.
- Outreach to increase industry awareness of NREL PV capabilities
- Interface with NREL Technology Transfer and Legal Offices for IP and business issues

There appears to be a large bias to internal NREL projects in the CRADA/Innovation by Design program. Is this really an industrial focus?

Solid team at NREL.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Ampulse is a great example of a potential solar game changer coming out of this program. The inexpensive III-V thin film solar cells program could be a complete game changer and is highly differentiated from anything this reviewer has seen in the PV field.

Created Innovation by Design competition:

- Air Stable OPV
- Silicon Nanowires
- Roll-to-Roll Processing on Novel Substrate
- Inexpensive 20% Efficient III-V Solar Cells
- Inverted Metamorphic Multijunctions in Monolithic Integrated Modules

Very good results on the few projects discussed, but again, it is hard to judge the entire program from that.

The involvement of cash-on CRADAs keeps a meaningful check on progress. Formal feedback from the industry partners (obtained from an independent pollster) is needed to get this feedback.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
This program is all about collaborations.

Collaboration with Industry is the only way that this program exists.

The program seems to be squarely aimed at technology transfer.

Great industry interaction.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Having the private sector fund the Innovation by Design programs that went unfunded internally is a brilliant idea. Just need to make it fair and competitive. Also, only one investor should be involved per Innovation by Design idea at the beginning. More than that will bog the commercial decision making down too much.

Finding, forming, funding and expanding larger industry partnerships
– fixing orthogonal business norms
This will make the projects even more relevant as they will be longer lasting and less administrative in nature as the overhead portion becomes less of the total budget.

There seems to still be a lot of definition needed on how these programs will proceed in the future.

<u>Project Strengths:</u>
Entrepreneurial program focused on getting big new NREL ideas and knowledge into the market. Excellent program.

• Promotes product-focused, industry-oriented internal research at NREL.

- Provides information to enhance industry awareness of NREL technology resources.
- Facilitates linkages between industry and NREL.
- Enables partnerships to more quickly utilize NREL technology to reduce cost and increase performance of PV products.
- Stimulates new PV businesses based on NREL developed technology.
- Manages distribution of SETP resources for participation in industry partnerships by internal competition.

Programs designed for technology transfer.

42 agreements with industry is impressive.

Project Weaknesses:
Need to focus on developing a process that integrates fairness of opportunity for private sector at some point in the process.

CRADA's are too small- under $150k which makes the administration a high cost portion of these projects.

How to solicit and identify new projects.

There is an inherent conflict on taking funds from industry.

The fact that NREL management is picking the programs to accept is somewhat problematic. For instance there must be a large internal basis to populate the PIDL - does that influence the choice (of course it should not).

Involving a limited group looking of potential investors is also potentially problematic, independent review is needed.

Recommendations for changes to the Project Scope:
More money should go to the Innovation by Design program. This is the best stuff I saw at this review.

Look for larger partnerships during the next round. Continue the use of Industrial CRADA's. They help industry and NREL both.

Continuing funding the program.

Arrange a formal method of making the program public and independent review.

NREL needs to navigate access & conflict of interest with care.
VC advisory board may prove to be effective - good to try.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT010_
Presentation Title: A Low Cost Solar PV Anti-reflection Coating
Investigator: McAllister, Beth

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Incremental innovation for higher efficiency applicable across all PV techs. Team aware of critical cost/performance metrics required for this to actually decrease $/W. Good project for DOE to fund given broad applicability, will benefit whole industry.

This project has a high degree of cross cutting due to the fact that it reduces reflection from glass on any type of PV module.

This can provide an extra 3% relative energy delivery over the life of a module.

The goal of this project is to provide cost reduction by an efficiency increase in power output with an inexpensive AR coating. A manufacturing scale coating system will be built.

A low cost energy enhancing technology that can be applied across the board is good for everyone.

Company is planning to lower LCOE by increasing light throughput with an appropriate index matched coating layer on the front of the glass. The general requirement will be that the cost of adding the AR coat will be less than the increased efficiency.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent R&D approach. Well defined metrics and milestones.

This outlines the Approach and appears sound in that it addresses both the development and deployment of large scale glass coating.

• Liquid coating method is key technical risk
– Lab study of coating techniques; select two (primary and contingency).
– Emphasis on “fail fast” methodology.
• Develop selected coating methods to Pre-Pilot
– Process development unique to this project.
– Keep both paths – establish common parts (e.g. chemical delivery, cure, solution management).
– Learn enough to specify Pilot System.
• Build Pilot System
– Build and test all proposed production technologies.
– Test Production design options and select final production toolset.
• Design Production System
– Ready to build full Production alpha system.

There are measurable and well defined goals and plans to achieve them. A primary plan and a backup plan are proposed.

Good team, reasonable approach. More emphasis on reliability is needed.

Company has presented the problem well (index matching), and the potential upside. The company's has appropriate testing facilities for measuring the optical char of the films. The requirements for incremental costs are well quantified (company claims they can achieve target of $1.5/m2), which will lead to an LCOE of ~65c/kWh for the incremental 4% of power.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Clever, simple, elegant approach. Great progress to date. Uniformity is obviously an issue that needs to be worked through, but they should be given the chance to do it.

Coating application method has been down selected to two methods- roll coating and spray coating.

Extra watts can be produced at $0.65/watt and eventually $0.40/watt. Spray appears to be cheaper.

Pilot scale coating has been done at 30x60mm.

Room temperature coating so it can be done at the end of the module line.

Progress to date is promising. Improvement in process control for spray coating is underway. It is not clear how difficult the control problems will be.

The company has produced the films on large areas. Although the company claims the total uniformity requirements is about 10% across the glass, this will result in a non-uniformity that is visible to the human eye. Certain home/BPIV markets may not accept this level of non-uniformity due to the perception of quality issues.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaborations with the right partners.

Mainly equipment partnerships. No industry or university partners identified. No patent applications or licensing identified.

There are many partners evaluating the AR coating. It appears that there is a large market if the cost and technical goals are met.

The partners are appropriate for business model of equipment + consumables. Ideally the company would have had a tier-1 panel manufacturer as a beta customer at this stage.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Good path forward with milestones. Good risk mitigation with multiple deposition approaches in the pipeline.

Project plans for 2010/11:
– Complete Pre-Pilot system investigations.
– Build Pilot system: Chemical delivery & management, coating and curing.
– Engineering and design for Production System.
Key milestones:
– Pilot System installed and operating.
– Delivery of Production System design.
Main decision point will be to choose the coating method to continue to Pilot and then Production stage.

Reliability testing, remaining uniformity, and getting prototype to customer.

Project Strengths:
Incremental improvement applicable to all PV technologies. Good metrics and good understanding of what is required for adoption and to actually lower $/W. Good team.

Major cross cutting potential across all technologies.

Extra watts costing only $0.65 per watt is plus. Having 10 manufacturers test the coating and confirm power gains is also a plus.

Project Weaknesses:
Uniformity is clear issue so far, but will be addressed going forward.

Company is underfunded and seems unstable at this point. Funding for future milestones is highly dependent upon the availability of capital funds for equipment deployment.

Need better test structures. Need to understand requirements better. Optical measurements need to extend past 800nm for silicon applications.

IP strategy needs to be made clear given foreign invention.

Go to marked strategy needs to be made clear.

It seems there has been turnover and recent delays in the company. It is not clear what is really behind this, and if there is a big issue that has senior staff concerned about the viability of this program, or this is an internal issue that will be resolved easily.

Recommendations for changes to the Project Scope:
Continue with caution.

Identification of the important system parameters and responses to them should be a near term priority.

Continue funding.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT011
Presentation Title: Flexible Barrier Films
Investigator: Brown, Kathy

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Broadly applicable very small improvement in performance. Main impacts are at beginning and end of day. Not clear what total impact of this tech is on $/W. Seems small.

Relevance: The goal of the Solar America Initiative is to achieve grid-parity for solar electricity produced by PV systems. As part of PV Supply Chain Development, this program's objectives address:

Efficiency
– By improving module/packaging so that amount of light that gets to the PV cell increases

Cost
– Have an attractive cost-performance ratio (commercial viability) and replace more expensive materials

25-year service life
Useful in rigid as well as flexible PV modules

The goal is to improve power output by 4-6% using a textured barrier film. This is one of several approaches to improve PV output.

The confusing cost data made evaluating this program very difficult.

Company is planning to lower LCOE by increasing light throughput with an appropriate index matched and low-angle light recovery coating layer on the front of the glass. The general requirement will be that the cost of adding the AR coat will be less than the increased efficiency.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Very well laid out plan with many stage gate goals and metrics.

Approach includes modeling, development of weatherable materials, using long term weathering models (from 3M long term materials testing), work with partners/customers to decide on commercial viability.

Good practical approach to business/supply issues.
Strong focus on technical performance, long term durability, and cost. More work needs to be done on which material to use.

Good team and reasonable approach.

Company has all the requisite skills, equipment, and methodology to proceed with this program effectively. The modeling capability and background of weather proof films is a key asset.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good data shown that actually does show impact in the field. Not clear whether that impact is big enough to make a significant impact on $/W.

Materials choices have been made-
Polyurethanes – up to 5-10 years outdoors
Not much known about the weathering of micron-size features
Expanded program to include silicones in Q3 (3M results on UV stability)

Adding capabilities in accelerated weathering for faster and more accurate tests
However, still requires 1000 – 10,000 hours (6 – 60 weeks)

Product form factor-
Started with known models for surface features that reduce reflection and improve low-angle light scattering- data suggest 4-6% over the total wavelength spectrum.

Have improved weathering protocol to decrease time of testing to predict long term outdoor results.

Preliminary tests indicate that the efficiency gains are achievable. The accelerated life test schedule is very long, and the final material has not yet been chosen. The time to commercialize the films may be very long.

Although durability and transparency were covered, cost and moisture penetration were less so.

Company has already shown results on actual PV panels using the proposed films in Arizona and Minnesota, with 4-6% increase in efficiency. One advantage of this film is the perceived (and actual) uniformity. Company has not disclosed the cost/m2 of the film, except to say it will be cost competitive. It has also not disclosed the cost of applying the film (cap-ex and op-ex) for the equipment at the panel manufacturer's facility that will apply the film.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
3M has customers interested in the product. 3M can deploy itself no problem if there is demand.

Working with 3 potential customers who have been evaluating films.

Several potential customers have or will soon receive evaluation samples. No technology transfer is assumed, commercialization will be done by 3M.

Samples have been provided to 3 potential customers - panel manufacturers. 3M has internal partners on this project.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Pretty straight forward plan going forward to close out project.
Milestones:

– Go/No Go on July 31, 2010 – details on slide 7, plus 3M internal NPI evaluation
- By July 31, 2011 will undergo 3M internal NPI evaluation prior to commercialization

Challenges and Barriers:
1. 25 year service life
Continue to gather data, for at least 10,000 hour of accelerated testing
Continue to develop methodology for faster and more accurate lifetime prediction
2. Efficiency:
Solar industry/customer feedback is key to determining commercial viability
3M also will continue tests and analyses of module performance
3. Cost
Pilot-scale and factory runs to optimize current and newly-developed processes

Reduce risk by
– Understanding details and mechanisms of weathering
– Developing understanding of cost-performance and trade-offs
– Awareness of other 3M projects (materials, weathering studies. etc.)

The company's internal development process requires checkpoints at key times. The path forward is well understood.

Overall there was too little detail in the presentation to fully form an opinion on this point.

Company recognizes that real world lifetime tests are critical to the viability of this technology.

Project Strengths:
Good technology. Strong company. Good data in the field early on.

This project has wide benefit to several technologies in the solar space and as an industry giant, 3M would bring tremendous strength to the solar business for cover sheets. They are involved in several parts of the solar value chain already and this would strengthen their position.

Well defined goals and product plans.

Durability and optical transparency seem well covered.

Project Weaknesses:
Not clear if impact is going to be very high for this technology

Seems that 3M has not made up their mind to pursue this product. Seems odd that they would seek government dollars to do a business study?

Very cumbersome cost analysis. Difficult to understand the impact.

Very little content in the presentation.

Moisture penetration was not well covered.

Recommendations for changes to the Project Scope:
Continue because of the leverage that this has.

Continue as long as the milestones have been met.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT012
Presentation Title: Enhanced Growth Rate and Silane Utilization in a-Si and nc-Si Solar Cell Deposition via Gas Phase Additives
Investigator: Hurley, Patrick

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Greatly increasing the rate of deposition of a-Si and uc-Si could have a big impact on decreasing the capital amortization portion of thin film Si based PV modules. (It was not made clear in this presentation exactly how much a 4x increase in deposition rate would decrease this and how much it would decrease the all in manufacturing cost of a-Si modules however).

The project aims to lower the cost per watt of microcrystalline silicon. Growth rates have been a limiter.

This project is directed at cost reduction by significantly increasing the growth rates of a-Si and uc-Si, thereby reducing the amortized capital equipment cost per substrate. Improved material utilization is also addressed.

Enabling technology for some potentially new solar cell types.

It is not clear if Si TF is going to be able to meet the DOE goals of grid parity in the near term. As such, the incremental improvement (including a 2-bandgap film) of this project may not be sufficient to help with the high level DOE goal.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Good solid approach mixing good science and engineering to achieve practical outcomes.

Experimentation to screen and test several additives to test for increased deposition rate and improved silane utilization.

Growth rates are planned to improve 4x from 0.6nm to 2.0nm. Silane utilization is planned to improve by 20%.

The key issues have been carefully addressed and analyzed. The measurement and evaluation tools are in place, and clear goals have been articulated.

Good approach.

Good modeling of required additive molecules to enable the faster growth rates. The overall approach of design, synthesis, process, and analytics, appears to follow a mature and well defined methodology. The company needs to build a PV cell as soon as possible to understand the impact of the new film on actual cell efficiency.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good early results with proprietary gas additive. Not 100% clear whether generalized FTIR parameters used will translate to good solar cell properties yet, but we will see later in the program.

Using a 13.56 MHz PECVD reactor nucleation of μc-Si with additive starts at 0.592 W/cm2. Growth rates of 2.2 nm / sec have been achieved with hydrogen dilution values of 71.26

As Hydrogen dilution values decrease growth rates should increase.

Growth rates in the μc-Si phase with Additive have surpassed targeted goal of 2.0 nm/sec.

The project is ahead of schedule. A lot of data are available and the results are promising. The data analysis shows good understanding of the process parameters.

Good growth rates and layer qualities.

Company has shown meaningful results (2nm/s) in being able to increase the growth rate with their additive, which they claim they are targeting to have a comparable cost to silane (10-20c/W). A Key step is for the company to build cells based on the new films. The company needs to build a PV cell as soon as possible to understand the impact of the new film on PV cell efficiency.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Excellent collaborations with Delaware on material and PSU on characterization.

Collaborative effort with Institute of Energy Conversion at the University of Delaware and Penn State University.

Two university partners have been engaged for electrical characterization.

Not clear how the program benefited from collaborations.

The company indicated they are working with Applied, although the extent of that relationship wasn't clear.

The company needs to find a cell manufacturing partner, and have feedback on cell performance in the short loop experiments.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Good path forward to evaluate actual PV properties of films deposited at high rates with good "microstructure factors"

Looks well laid out with the following direction:

Lower hydrogen dilution studies are next with electrical testing of films

Continue screening additives in the a-Si and μc-Si phase

Measure electronic properties of a-Si and μc-Si Additive films

Measure efficiency of completed devices using Additive films

Technology transfer to Institute of Energy Conversion (University of Delaware) for large scale PV cells

Deposition plans are good. Some additional work on electrical characterization should be added.

Plans seem good.

Ongoing optimization of the additives, film deposition conditions, etc.

Project Strengths:
Very practical project that could be implemented in the near term. Good mix of science and engineering to meet practical goals of increasing deposition rate significantly. Great project.

Appears well executed, necessary for the survival of thin film amorphous silicon as a viable cost option for PV cells.

Company capabilities and experience. Working relationships with users and equipment vendors.

Goal is 4nm - and have achieved 2.1nm.

Good FTIR data vs. growth rate.

Project Weaknesses:
Wish the team were clearer on exactly what the quantitative decrease in the module $/W would be if 4x deposition rate is achieved. Some concerns that this work could be done internally and is pretty near term on a product roadmap, i.e. are DOE dollars have strong additional impact?

Financial benefits don't appear to be well understood and the question of long term viability of amorphous silicon is suspect.

This process may not have any applications in solar if the related technologies don't pull through. There may be applications for the tool in the VLSI industry.

What is the cost of the additive?

Recommendations for changes to the Project Scope:

Consider cancelling if amorphous silicon is not to be pursued. Depends on the overall DOE program structure.

Complete project then wait on the needed solar cell technology.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT013
Presentation Title: Flexible Ultra Moisture Barrier for Thin-Film Photovoltaic Applications PV
Investigator: Wang, Lin

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The thin film barrier layer could be a total show stopper for non-glass low cost thin film PV. The approach used here is very out of the box and gutsy. Many people don't think ALD can be scaled. If it is, it will have impacts well beyond this project itself. Excellent project selection with world class team.

This project is aimed at flexible CIGS with a promise to hit <$1/Watt.

Flexible CIGS can drive installed PV cost reduction and enable improved BIPV products for grid parity
– Highest efficiency among thin-film PV options (ca. 20% in lab (NREL), 13% on module)
– Lower manufacturing cost (fewer materials, roll-to-roll economy of scale)
– Lower installation cost (lighter modules, no frames)

Significant cost reduction by replacing glass with a low moisture permeability film. Potential improvement of long term reliability for thin film and OPV cells.

Important enabling technology for thin films.

It is not clear that flexible CIGS is one of the primary candidates for DOE's near-term grid parity, due to unproven lifetime issues. As such, this technology may not be a key component towards the grid parity goal. The positive side is that if the only remaining obstacle to grid-parity for flexible CIGS is the lifetime degradation due to water, and this technology is meets the WVTR and cost targets, this can be interesting. It is not clear that is the only remaining barrier to long life flexible CIGS.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent approach. ALD allows for pinhole free layers. Roll to roll approach could make ALD low enough cost and scalable for the first time ever. Proof of concept has been achieved for the material already.

Project focuses on Atomic Layer Deposition (ALD)

Initial lab results indicated that ALD-on-plastic could effectively protect CIGS cells

Program goal here is to demonstrate manufacturing feasibility of ALD-on-plastic with water vapor transmission rate below 10-4 g/m2/day at low cost
- use good scientific modeling
- test films
- project test with partners for cell efficiency and reliability on solar modules
- confirm costs
- start production capital acquisition
- start production of materials for industrial customers

Impressive process capability has already been demonstrated. The current project is aimed at creating large scale prototype and production coating systems.

Sound approach.

Company is still in "research" stage/early prod development stage.
Targets of 10E-4 for WVTR are not proven to solve all reliability problems.
Overall, the project is following a methodical process for the scope of project they have tackled.
Model to develop and sell equipment seems appropriate.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good proof of concept of the material. Good progress toward building roll to roll ALD. I love this project.

ALD modeling shows less than 10-4 WVTR for films

Prototype equipment design and build completed, including:
– Reaction kinetic analysis for process window
– Gas delivery and pressure control system
– Film transport system
– Deposition chambers and coating head
– Thermal control system
– Process control strategy

PET film supply secured

Used film recycling plan in place

The barrier properties of ALD coated plastic are beyond the requirements for most thin film PV technologies. Prototype system design and construction progressing well.

Good results.

The company has deployed their process on actual PV cells, and measured actual cell degradation at 1000 hours.
The measured degradation of 2% at 1000 hours is on 10% cells. Higher efficiency cells may have higher degradation rates.
The costs of deposition for production (uniformity, reliability, etc.) are not known yet.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good industry collaborations outside of DOE project. Company can scale this if it works.

Two university partners (US-based)
• CIGS cells aging tests using our ALD barrier films
• Exploring ALD barrier material set

Market partners (US-based)
• Working with CIGS module makers to evaluate our barrier film

Two industrial partners (US-based)
• Evaluating contingency for high throughput ALD-on-plastic

They have industrial, university and market partners involved. There is clear commercial demand for at least evaluating these films.

Not clear how collaborations helped.
The bulk of the heaving lifting in designing the deposition machine is done internally, and the company has two industrial cell/panel manufacturing partners, which is a strong asset.
The university partnership helped aging tests, and exploration of different barrier material.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Excellent plan to prove roll to roll ALD. Will be hard, but well worth a try.

Milestones appear well laid out with the following milestones:

June 30, 2010
– Prototype fabricated, testing protocol in place

December 31, 2010
– Prototype process parameters documented

Prototype optimized, with WVTR below 5E-4 gH2O/m2/day
– Documented procedure of PET film cleaning to achieve WVTR below 1E-4 gH2O/m2/day

March 31, 2011
– Preliminary design of pilot facility in place

September 30, 2011
– Prototype optimized, with WVTR below 1E-4 gH2O/m2/day
– Pilot facility design completed

June 30, 2012
– Pilot facility built

The future research plan will depend on what is learned from the initial output of the prototype system.

It seems the company may have the opportunity to explore additional features in the film, such as light trapping, index matching, etc.

Project Strengths:
Working on a very hard, very important problem. Very novel technical approach. Material should work.

Well executed with an eye to the commercial deployment of the films. Good work as always with Dupont.

Demonstrated barrier results and high customer interest.

Good progress with prototype equipment. Good accelerated weathering data.

Project Weaknesses:
Roll to roll ALD cost and scalability are the big risks, but that is why we do R&D.

It is a bit slow to market, reflecting the conservative nature of Dupont's business methodology. Dupont has had a significant place in the value chain for PV through its Tedlar brand and EVA brand (pellets used for extruded film). I believe that Dupont has seen the PV business blossom to a high volume, high reliability demanding concern. Dupont does not question the market, it probably questions the long term durability of its product.

none noted

Recommendations for changes to the Project Scope:
Keep going, it looks like a good program.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT015
Presentation Title: Organic Photovoltaics
Investigator: Lloyd, Matthew

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Although OPV still is a long term problem with the technology not even close to any real meaningful commercialization for the energy problem, it is important for NREL to maintain a deep expertise and to push fundamental science over the long term in this area. Good work is done here, core competency in OPV is being maintained in the U.S. Right level of funding.

OPV has had a promise of low cost and high adaptability to mass production. NREL has played a "gluing" role here by solving certain reliability issues with the contact designs.

The basic research parts of this project (polymer modeling, degradation mechanisms, and acting as a central clearing house for information) are valuable contributions for long term understanding of OPV.

It is unlikely that OPV will meet the lifetime target necessary to achieve grid parity LCOE in the short/medium term. However, the quality and scope of this work is necessary in achieving better efficiency and lifetime for OPV, which has applications for non-grid tied applications.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Good well-structured mix of theory and experiment. Good targeted research problems. Very strong team.

Very solid approach to understanding. Beginning with basic solar cell analysis and band gap tailoring. Work on the lifetime and improvements in degradation mechanisms have been an important part of this program.

Much of the work appears to be related to longer term understanding rather than focusing on identified barriers. This work isn't really a development/deployment effort as much as a research project. The performance and reliability goals could be more aggressive.

The combination of theoretical and applied work in conjunction with industrial and other partners is excellent, and necessary to solving the lifetime and efficiency problems of OPV. Tasks 2, 3, 6, and also 5, are ones that NREL is well positioned to do, and will not be done as rigorously and methodically and industrial firms.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Inverted device work has made some impressive leaps. I would like to see more focus on long term stability. Also, stability testing should be done with state of the art barrier layers to show what can really be done with this technology.

Accomplishments-
Demonstrated new 6.7-7% device (1cm2).

Developed models for materials design and device operation.

Synthesis and utilization of lower band gap absorbers with reduced donor – acceptor band offset.

Developed contact material with enhanced doping control for high efficiency inverted OPV devices.

Initial correlation of lifetime and cell design/encapsulation with combinatorial lifetime testing.

Very good fundamental research to understand where problems occur and to model the polymers.

Early results for efficiency, cell arch, TCO, etc. all speak for themselves.
I would grade this area as outstanding results for the scope of the research. However, there is significant work remaining to get yields, costs, and lifetimes for commercially viable products that have LCOEs close to grid parity.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Program is taking thought leadership running stability workshop. Good collaboration between theory and experiment.

Konarka, UCSB, Plextronics, Dow Chemicals

Good coordination and partnerships.

Significant interaction with many groups. Central coordination of many OPV efforts.

Excellent dissemination of information and partnerships with commercial and other partners.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Good future research plan with well-defined incremental performance metrics.

Future work for 2011

Modeling - Key focus in this task will be to continue to extend our
computational modeling ability

New Organic Materials - A key focus will be on development of new lower
gap polymers

Contacts - This will be done for conventional configurations as well as inverted devices.

Lifetime Characterization - Extended effort to delineate the key mechanisms for
degradation as a function of device configuration and characterization of
devices with new organic materials.

Characterization - Development of new rapid assessment techniques for measuring the acceptor donor properties.

Solving the degradation problems by, for instance, improved barriers may reduce or eliminate the need for some of the proposed planned research and improvement areas. Applying the best recent results from outside NREL should be addressed sooner rather than later.

The ongoing and future research is a hallmark of this program.

Project Strengths:

Important to keep this core expertise in NREL. Program is at the right level. Shouldn't be much bigger until the fundamental stability issue begins to get some real traction and people have some big ideas that start to look promising. Good collaboration with industry. Good mix of theory and experiment. Real innovation happening here.

Well done work, good analytical base.

Modeling and fundamental understanding.

Project Weaknesses:

OPV still is primarily a science project. It is not clear how much is happening in BES and how that work relates to or could enhance this work.

Question is whether to fund OPV at all at any level- this is a question to be discussed at the program level.

Pursuing too many areas. Understanding of degradation of contacts and how to prevent O_2/H_2O contamination.

Recommendations for changes to the Project Scope:

More focus on big new ideas for stability would be very welcome to this reviewer.

Keep it going for another year to see if the efficiency exceeds 10% and lifetime can exceed 10,000 hours.

See how well these devices perform with much better barriers.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT016_
Presentation Title: Low Cost, Lightweight Solar Modules Based on Organic Photovoltaic Technology
Investigator: Gaudiana, R.

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Konarka has been working on this problem for a very long time and MAJOR limitations in efficiency and more importantly stability remain, with a 2.5x increase in efficiency and a 20x increase in lifetime needed for commercial viability. It does not seem that they currently have the ideas that can overcome these barriers, so it is questionable whether a DOE investment at this time is a prudent one in a private company.

OPV has promise to be a low cost, mass production technology which has potential to produce solar modules at less than $1/Watt.

Project addresses reduced cost by OPV performance and reliability improvement. Module manufacturing techniques are implemented. Project goals are 7% efficiency and > 10 year lifetime. The program aligns well, but is not succeeding. Focus on BIPV does not meet EERE goals.

The value of this company's program is to deploy best known OPV technologies (developed in house and elsewhere) on production grade equipment, and to understand the impact of manufacturability on device performance/reliability/cost/yield. Their technology is not likely to achieve grid parity in the short/medium term.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Pushing a few incremental improvements that don't really address the fundamental problems.

Approach is extremely experimental with virtually no analytics to support the fundamental degradation mechanisms. The tests are laid out in a systematic way, but the project does not include the necessary material science modeling and analytical support to understand why the modules degrade.

Good focus on building useful modules at the lowest possible cost. The major barriers to overcome, reliability and system cost, were identified early and received most of the effort.

Although the work may be well intentioned, the results are poor.

The company can spend more time in understanding the underlying mechanisms for degradation : they need to create more controllable and observable conditions for the tests. For example, why is the degradation of the rooftop panel very flat, and then at ~3000 hours there is a sharp drop? There are enough basic unknowns that uncontrolled roof-top panels are not the right conditions to understand the degradation mechanisms at this point.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Incremental progress has been achieved on stability.

Good progress on back contact replacement with grid contact.
Poor progress on efficiency milestone of 7%.

Modest progress on stability data for modules.

Demonstrated > 2000 hrs. 85/85 with glass encapsulation. ITO replacement in use. The project shows that an adequate barrier will significantly improve OPV reliability. The project is ~ 70% complete at the end of the program. The target efficiency of 7% was only half met (3-4%).

The change of materials and lack of reasonable efficiencies make the program's chance of success very small.

The project is one of the leaders in producing large area panels on production equipment. Continued work on production machines, and root causing problems, will be necessary to get to high efficiency and long-life flexible cells.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
It wasn't made clear what kind of partnerships the company has and for what purpose.

Partners are University of Delaware and NREL.

Some collaboration with NREL and U of Delaware, but largely an internal company effort.

It was not clear how the interactions helped.

Good relationships with NREL and U of Delaware. The company works with many suppliers and partners outside the scope of this DOE project. As a commercial entity, of course the company has limits in its ability to share information with suppliers and partners.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Not clear that the company has any big ideas going forward to solve the core problems with this technology.

The project ends on June 30, there are still milestones not met, so Konarka will continue working on these items.

Program ends in June, but the project will continue at Konarka.

$3.64MM DOE, 60% company share.

They are in an invention mode, which is needed but hard to effectively plan for.

There are many areas of collaborative and in house research on their plate.

<u>Project Strengths:</u>

The strongest ongoing OPV private sector effort.

There is some reasonable reliability data here for dry exposure.

Clear focus on cost and reliability along a path to commercialization. Demonstration of good reliability with glass barriers. Such barriers are costly, but they demonstrate that an adequate barrier points the way to improving OPV operating life.

One year's time to a commercial BIPV product. It is very hard to believe that the technology is ready for this.

Further the BIPV market is very small and cannot support any significant manufacturing effort. Glass encapsulated product at 4% is at least something.

Project Weaknesses:
No big new ideas that clearly address the magnitude of the current shortcoming of OPV in efficiency and especially lifetime.

4% efficient modules at best were presented. Perhaps some promise for flexible barrier films for OPV.

Unable to implement some risky goals

Could not reveal the LCOE in the BIPV application.
They have changed active material (to n-type fullerenes). No improvement in efficiency.
Module efficiency of 4% vs. goal of 7%.
Funding from other government agencies (NIST 35%, DOD 35%, DOE 35%)

Recommendations for changes to the Project Scope:
Recommend to focus on funding differentiated new approaches that actually have promise to hit the lifetime metrics.

Project is ending, no further work should be funded on this.

The project should end on June 30.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT017
Presentation Title: NREL Film Silicon Agreement
Investigator: Branz, Howard

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
One of the most exciting research topics NREL is pursuing. If they can get thin Si films approximating single crystal performance (and can get sufficient light trapping), thin Si can be a game changer in the next 5-10 years and push us toward $1/W installed price.

This project aims to displace silicon wafer technology with film silicon to meet the target of <$1/W.

Growth of thin crystalline silicon films on inexpensive substrates addresses cost reduction and reduced silicon usage goals.

Good, but not radical, approach.

Ambitious goals to significantly reduce Si waste for Si-based PV cells.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent approach. Very well laid out area cost and efficiency targets.

Well thought out approach with great analytic support to understand the fundamental material science. The approach with the Hot-wire CVD has been thoroughly analyzed.

The project will address the needs for >15%-efficient cell performance

Additionally, the project will develop a high -rate nanocrystalline Si film for near-term PV use-need high quality film, preferably 100 orientation.

The project is well focused on developing the technology for thin silicon solar cells with reasonable (15%) efficiency. Starting substrates and deposition methods are identified.

Good team and approach.

Excellent modeling and metrology of experimental results (lifetimes, thickness control, etc.)

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Early results on HWCVD an virtual single crystal results are very exciting and collaboration/deployment through Ampulse is great tech transfer. Top notch work.

Have demonstrated high rate, low defect Hot-wire film silicon.

Developed good method for defect counting.

The initial results of growth on glass with seed layers are very encouraging. Growth rates are adequate, but the defect density is quite high. The major cause of defects has been identified and some steps to minimize defects have been taken.

Good progress on dissimilar substrates. Less work on single crystal would be good.

Good results shown on glass and Si wafer. Key challenge will be yield, and ultimately cost/W (LCOE).

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Very strong tech transfer to Ampulse. Good partnerships with Corning, Sharp, Uni-Solar, Xunlight.

Corning, Sharp, Standford, Columbia are all partnering on this project.

This project has many collaborators (too many?). There are many opportunities to test seed layers and for supporting interested industrial partners.

Good partners.

Good list of academic collaborators and industrial partners, including cell manufacturers - a key step to move this technology and the short loop experiments in the right direction.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
It is clear that light trapping will be very important going forward and more money should be devoted to this program to continue the great work on Si thin film and add advanced light trapping work.

Work for 2011 includes:

Implement improved low- defect density epitaxy in PV cells on glass

Evaluate quality of epitaxy and PV cells on newly-acquired seeds

There are many possible future research paths. Some prioritization will be needed. The plan to limit exploration of seed layers will help to reduce the distractions.

$3.8MM per year DOE.
4 um thickness with 5X light trapping is goal.

Inexpensive substrate and epitaxial growth. Hot wire CVD.

<u>Project Strengths:</u>
Thin crystalline Si could have the efficiency to get thin film silicon down to $1/W fully installed price. The HWCVD/virtual single crystal substrate approach is very promising to get there. Howard Branz is doing great work here.

Well executed work, very thoughtful and analytical.

Reviewer 11:
Progress to date and understanding of the problems.

10.5% efficiency on Glass with PECVD is a good champion cell result.

Corning glass based champion cell is a good result.

Good deposition and thicknesses with hot wire.

Project Weaknesses:
None. Some more light trapping work would be good.

Cost targets need vetting with an industrial partner, perhaps Sharp or Unisolar?

More attention needs to be placed defects that cause shunting, and exploring mitigation strategies. Fixing them after they are formed could be potentially useful across the Si industry. It could me more valuable than making "perfect" films.

Too much dependence on hot wire may overly constrain the program.

Recommendations for changes to the Project Scope:
Increase budget to enable more work on light trapping.

Add a cost modeling task- all inclusive with seed layer materials and equipment depreciation

Better definition of the light trapping approaches to be investigated. No more work on dead single crystal wafers.

Review: EERE 2010 Solar Program Review

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT018
Presentation Title: Low Cost Thin Film Building-Integated PV Systems
Investigator: Yang, Jeffrey

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Although it is not clear whether a-Si based thin film modules will ever get to sufficiently high efficiency to enable lowest cost thin film modules, this work at Uni-Solar showed very strong and quick results indicating that this technology is still worth the DOE continuing to invest in. However, this program may be excessively large for this kind of DOE grant.

Amorphous silicon has always looked like a promising way to reduce costs and improve manufacturability of PV devices in large scale.

Meet LCOE 2010 goals by improved thin film cell efficiency and cost reduction of the rest of system on a large scale manufacturing line.

More domestic manufacturing would be good.

Without breakthroughs in the MJ cell proposal in the future it is not clear how the company's current technology will approach grid parity.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Well laid out approach. Roadmap is very clear.

The project includes a two-prong parallel approach:

– Improve the deposition rate, using Very High Frequency PECVD technique on Ag/ZnO back reflectors.
– Implement nc-Si:H multi-junction technology in commercial products.

Use these two improvements to produce modules in excess of 10.5%

Strongly focused on implementation of the cell improvements and simple installation procedures.

Little was presented on this, other than champion cells.

Company has not put sufficient effort in methodically showing the failure mechanisms, and therefore expected lifetime under various environmental conditions.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Increase in almost 1.5% absolute is very impressive. Great accomplishment.

Good results on VHF deposition- looks extremely stable and at a high rate making stable, lower cost cells.

Good results on raising efficiency with the reflective back contact.
Modules made with 35% relative increase in power. Stable at >10.4%

Achieved large area uniformity.

Most of the projects goals have been met. Improved cells have been developed, process equipment is being modified, and the LCOE target is close, if not achieved. There are enough identified improvements to reduce LCOE even further.

75KW demonstration systems have been installed.

It is not clear what the actual cost of the USO material is and how LCOE is determined.

Although progress was noted in champion cells, this was a big program and manufacturing impact was unclear.

Company's technology is in very preliminary stages of deployment, and the LCOE and reliability/lifetime (which will impact LCOE) are not well understood.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Uni-Solar will deploy this technology if it works at scale.

Collaboration on this project with the following partners:

– PV Powered
– Solectria Renewables
– NREL
– Colorado School of Mines
– University of Oregon
– Syracuse University

Some collaboration with vendors, NREL, and universities. USO is the manufacturer using the technology, so transfer is not an issue. Products are currently being sold with this technology

No clear benefit from collaboration.

The company needs to identify collaborators that will help with long term reliability testing and bankability. Not clear that the university partners will help with that goal at this point in time.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Clear continued roadmap for R&D to get to higher efficiency presented.

Project is ending this summer.

Need to implement these improvements into commercial products.

This project is almost completed. Several future improvements have been demonstrated and will be implemented going forward.

$20MM SAI TTP. 50% cost share.

Three junction stack with ITO on stainless steel.

All improvements are very evolutionary. Where was the risk / gain.

Going to 170 watts in maybe one year time with new deposition and silver back reflector.

Long term plan for reaching grid parity for 2015 is to develop a new MJ cell. This is a significant technological undertaking.

Project Strengths:
Strong efficiency improvement over a short period (almost 1.5%)

Good results for VHF and Back contact work.

Producing a final product meeting the LCOE goals. Future improvement path identified.

The apparent growth in domestic production capacity is a strong positive. It is not clear what the goal was however. 180MW is present capability (60 MW was the capacity in 2007).

12.5% champion cell with NC is compelling. This would take module to 195W. All champion cells are good.

Flexibility of NC silicon?? Yes, but no proof was offered.

$3.5 to $4 installed price today.

Project Weaknesses:
Project is very large, does it need to be this large?

Didn't implement into commercial production.

Only modest improvements in efficiency (136W to 144W). This is disappointing. Much was made of future gains, but they are just far enough out of reach to question their viability.

Recommendations for changes to the Project Scope:
Don't fund unless this work is implemented into commercial production. Amorphous silicon needs to hit this efficiency to be viable and compete with the CdTe and CIGS modules being

made.

The Program should reevaluate the current level of funding versus the overall benefits of this effort

No breakthroughs are expected.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT019
Presentation Title: Wafer Si Agreement
Investigator: Wang, Qi

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
It is not clear what the purpose of the wafer Si work at NREL is. Is this a place where industry can use state of the art tools to evaluate their material/wafer quality or is it a place to invent disruptive new technologies and try to get them out to the market? Much of the work seems like relatively obvious incremental work that I can only imagine has to be being undertaken by industry already. At only $2M, probably good to keep this to keep core competency at NREL in wafer Si.

This program addresses the full value chain of making silicon solar cells which of course dominates the PV market today.

Some tasks (CZ puller, Si heterojunction scale up, black silicon) appear to be replicating capabilities which are available elsewhere or which could be easily contracted out to commercial operations.

Support of the si industry is good, but the program is too broad.

Excellent articulation of problems to achieve grid parity with wafer-based Si PV cells, and good collection of near-, mid-term approaches to addressing the components of the cell cost/efficiency. (Black Si, direct write contacts, HJ/IBC cell).

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Not clear what core goal of program is.

The crystal growth evaluation of novel feedstock types is quite good to partner with industry.

The HJ/IBC cell project has some potential for a commercially viable cell process.

Black Silicon AR process is great to improve solar cell performance.

Direct write contacts are also highly leveraging to increase solar cell performance.

The tasks are wide ranging, without specific focus on solving any particular problem. Many of the tasks seem to be catch up projects to give NREL capabilities which exist elsewhere.

Program is too broad.

Very thorough methods for making key measurements of intermediate variable and results (strong metrology resources) to help direct activities.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Fair technical accomplishments on a-Si HJ work.

Accomplishments are broad-

Good results in HJ last year and black Si this year.

Delays in Cz puller, mc passivation and HJ this year.

Some good results are available for some of the tasks, but several tasks have been significantly delayed. Some of the missing equipment could have been foreseen and procured earlier to avoid the delays.

Work seems too broad to succeed strongly in any one area.

The results so far have been OK, and the next step needs to include the high volume manufacturing cost (at a specific design point) in order to be able to measure progress and optimize the choice of technologies.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good interaction with industry on Si testing.

Good partnerships with industry-

Suniva
JA Solar
Applied Materials
SRI
1366
BP Solar
Optony
Sixtron
SolarWorld
CaliSolar
GT Solar
MEMC
Applied Nanotech
Dow Electronic Materials

Many collaborators for many tasks. It is not clear what level of participation the collaborators have.

Very broad industry team.

As with most NREL groups, lots of interaction and dissemination of the work to university and commercial entities.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Not exactly clear where the program is going from here to this reviewer.

Future work includes the following-

2011 Direct written front contact of c-Si heterojunction solar cells
2012 High efficiency > 21% heterojunction solar cells

Too many disjoint projects. The silicon feedstock task should be left to a standards organization like SEMI. The other tasks should be reoriented to solving more fundamental crosscutting problems or to support particular technology transfer needs.

$1.8MM DOE.

Heterojunction work is good.

Testing different MC wafers is too applied work for NREL. Very material dependent.

Continuation of many of the activities outlined in the plan.

<u>Project Strengths:</u>
Important to have some core competency in wafer Si in national labs.

Really supportive of industrial partners.

Good technical knowledge and breadth. New equipment capabilities.

Good support of the Si industry in the US.
Hetrojunction work is good.
Black Silicon is good.
Direct Write is good. Instead of trying to invent direct write, why not evaluate the tools of others.

Working on Cu instead of Ag would be good.

Project Weaknesses:

Not clear what the role of NREL here is: testing or technology development. Would be good for NREL to explicitly determine what the goal of this agreement is and how it fits in with the private sector.

Too broad for resources that exist- consider downscaling the number of tasks. Increase funding for the scaled down list.

Definition of the highest priorities for reaching DOE near term goals.

Project is too broad and too applied for a national lab.

Why do silicon feedstock evaluations? Evaluate individual feedstocks - for fee? Publish this work?

Recommendations for changes to the Project Scope:

Ramp up the HJ/IBC work.

Finish the Black Silicon work with optimized cell performance.

Reduce the very applied portions. Sharpen the research goals for each area.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT020_
Presentation Title: Grid-Competitive Residential and Commercial Fully Automated PV Systems Technology
Investigator: Peurach, Jack

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Strong incremental improvements in efficiency and installation cost at SunPower are important to keep U.S. based PV industry competitive. Not 100% clear that SunPower wouldn't have funded this itself 100% had DOE not stepped in.

SunPower's TPP addresses the whole value chain to achieve LCOE reductions

This project provides improved efficiency and reduced cost to meet the LCOE targets. Commercial and utility scale systems are targeted.

Would be outstanding if there were more manufacturing in the US.

A leader in commercial technology and LCOE.
Earth abundant substrate and material.
Excellent lifetime.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Great approach. Very broad innovation across the board.

Broad approach to decreasing LCOE-

Material utilization & cost
Simplified manufacturing process sequence:

-Reduction in # handling steps
-Elimination of process materials
-Improved overall OEE

Efficiency
Modified design for higher efficiency
-Leverage silicon, module, system and installation costs
-Higher field performance (kWh / kWp)

Modules
Development of large-format commercial modules
-Standardized 96- and 128-cell formats with improved, stackable frame design
-Incorporation of improved or lower cost materials, e.g. thinner glass, AR coating,
-Co-development with systems-level products to achieve cost reduction targets

Inverter
Evaluation of micro inverter integration:
-Design for reliability through component selection and elimination

Systems
BOS Cost & Installation Efficiency/Modularity/Standardization
Integrated module/system product design
-Product standardization possible through vertical integration
-Modular/unitary design focus for system-level products to reduce redundant components and overall BOS costs
-Scalable design/analysis approach through automated tools

Clear focus on efficiency, standardization, and cost reduction for BOS.

Disciplined approach and execution.

Great methodology throughout the development cycle - consistent focus on LCOE in all activities.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Great results. 23% cell very impressive. Great innovation on module and installation side.

Key Accomplishments
•23%+ solar cell efficiency
•World-record panel efficiency 20.4%, resulting in 50% more energy as compared to conventional solar panels at 339 Wp
•Production release of large-format modules resulting in significant LCOE reduction
•Modular system-level products released to enable rapid deployment and improved installation efficiency
•Initiated integration with micro inverter technologies
•Production release of expanded automated design and simulation tools

Best silicon cell and panel efficiency. Designed for rapid installation and minimal labor cost. Large scale systems installed. Manufacturing capability improved.

New products and higher efficiencies.

Results of the products speak for themselves.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
SunPower will deploy these technologies in the field. Not clear whether the TPP program resulted in synergy across the value chain or not here from the presentation.

Working with DOW, TIGO energy and SolarBridge

Good supply chain integration and contract manufacturing partnerships. Still some work to be completed on inverters.

No apparent benefit from work with partners.

For a commercial entity, there is good sharing.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Not 100% clear where they will go from here, but understandable in that this is proprietary information.

Project is ending this fall. Implementation of a few tasks will take place concurrent with the final part of the project.

Project is nearly complete, but manufacturing improvements and efficiency improvements will continue.

Project is winding down.

$24MM DOE share, 50% cost share

Project Strengths:
Strong incremental innovation. Good to support a U.S. PV technology leader.

Well thought out and executed.

Developing and deploying multi-MW capacity systems. Production capacity installed.

Impressive high efficiency results at both cell and module.

New commercial rooftop module.

75MW module line in CA. 100 people work there.

60% of modules have ARC now.

Project Weaknesses:
Not clear if SunPower would have done this without DOE support anyway. Questions on whether this money has a strong additional impact on the company.

Probably could have had more partners.

There was little apparent risk in the activities presented. Little benefit for collaboration with others.

Recommendations for changes to the Project Scope:
No recommendations- well done.

More risky steps for future work.

The DOE should consider how to leverage the excellent infrastructure of the company to fund breakthrough developments that utilize that infrastructure. Incremental money from the DOE to do incremental work is not the best use of funds.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT021_
Presentation Title: Concentrating Photovoltaics
Investigator: Friedman, Daniel

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Dialing up the efficiency of the PV converter in CPV is a strong lever on system cost per watt. NREL continues to lead the industry and have strong tech transfer. Very impressive impact.

High efficiency concentrator cells have a large leverage of minimizing materials usage and reducing cost for PV electricity generation.

Improved efficiency and reduced cost are critical for CPV growth.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Great approach. Clear targets for efficiency in 2010, 2015. Great track record.

Guided by industry in cell technology-

- Power output
- Thermal management
- Materials use mitigation / cost improvements
- Compatibility with system optics
- Reliability

Many problems and opportunities are being addressed. Better quantification and focus on the few most critical would be helpful.

Focus on technically rich elements is positive.

Depth of experience in models, structure, materials, processing, architecture, and metrology of MJCs - a key asset for continuing to move this segment of the industry forward.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The NREL CPV team keeps innovating and breaking records in CPV PV converter efficiency. Very impressive.

Results include

Improved back surface reflector
Good quantification of reliability issues
Improved lattice mismatch problem with graded interface layers
Created an advanced model of tunnel junctions which appears unique in designing the cell performance and losses

The goals are not clear. It is hard to judge progress against goals. Several possible improvements are underway.

Highly reflective in the IR is a good technical achievement.

Already a proven leader in developing the IMM and other MJC cell technologies, continues to push the technology forward. At this point other startups and large companies are in a position to make meaningful technology and innovation advances in MJCs.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Excellent tech transfer to industry.

Partners include industrial and university partners:

RF Micro Devices

Interagency DARPA/DuPont

University UC Santa Barbara

Several collaborators are mentioned. The RFMD collaboration results are excellent because they lead directly to the cost reduction and technology transfer goals.

Good partners and good interaction with them.
Broad collaborations and linkages between experimental and theoretical results in the research and commercial community (RFMD, Dupont).

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Clear targets and goals for 2015.

Plans include the following
2011
Launch comprehensive program for elevated-temperature cell.
Temperature dependence of: cell QE and current-matching; materials parameters; degradation rates

2012
Predictive understanding of IMM cell performance to 350K.
Develop predictive understanding of the performance of 1.8/1.4/1.0-eV IMM cell at temperatures from 300–350K
over spectral range of 350–1400 nm and concentration from 1 to 1000 suns.

The 5 year plan is not very specific. The goals through 2013 appear to support the program goals. Focus should shift the high temperature studies and reliability evaluation so that these cells can be more rapidly applied to real CPV systems.

1.9MM DOE, no cost share

Lead the industry in devices, materials, and reliability efforts.

Good leverage with semiconductor III-V partners such as RFMD, and internal work on reliability.

Project Strengths:

Team continues to show technical leadership and break records.

Good technology platform and analysis. Tunnel junction work is really good. Lattice mismatch work is fundamental to successful multi-stack cells in this technology space.

Cell performance and technology transfer.

Rapid transfer to industry. Good involvement of the telecom industry.

Good approach to the basic elements of device development.
Guided by industry on many cell features, but it is not clear how that information transfer takes place.

Project Weaknesses:

Would be good to see more work on how to get basic materials cost out of rest of the system. Perhaps this doesn't make sense at NREL given how many different approaches there are currently.

Still a long shot from a cost standpoint- no cost info available- has any been done?

The goals are not clear or well defined.

Cost discussion was very weak.

Work needs to be done to tighten the relationship between the research goals and real world importance of the work.

Recommendations for changes to the Project Scope:

Need some cost goals that make this project relevant to the DOE $1/Watt goal by 2015.

The scope of the project should be limited to working on things that cannot be done by industry. Cell optimization projects should move to industrial partners.

Maintain this level of investment.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT022_
Presentation Title: High Efficiency XR-700 Concentrator Photovoltaic System
Investigator: Ventura, Mark

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
CPV continues to be a strong horse in the PV cost raise. Definitely important for DOE to continue funding. Lots of approaches and not clear who may win.

Goals for the project fit well into the DOE objectives of reaching grid parity.

• Advance production solar cells technology & cost reduction
• Drive efficiency from 36% to 40% by 2010 and to 45% by 2015
• Realize >2x reduction in cost
• Increase production capacity to 1 GW/year by 2015.
• Implement advanced high concentration non-imaging optical system
• Advance reliability and cost of the tracker system and balance of plant
• Achieve Grid Parity - $0.15/kWh LCOE by 2010 and $0.07/$kWh by 2015

Large and expensive TPP project to demonstrate CPV cost reduction and system integration.

Good alignment with DOE goals.

Project has been trying to figure out how to leverage the large investment in the space MJC program to develop grid-competitive terrestrial PV plants.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent approach to R&D. Clear performance targets.

Approach is a system's level approach to deployment of the high efficiency cells by:

• Demonstrate 40% efficient production cells & production automation
• Partner with U.S. company to enable manufacturing scale-up & commercialization
• Demonstrate Proof of Design outdoor exposure performance stability for first year
• Improve module efficiency & demonstrate with Proof of Manufacturing production
• Demonstrate process & cost down via pilot production using robotic assembly line
• Demonstrate BOS installation and cost via 100 kW power plant deployment

The focus is to achieve the $0.15/watt 2010 goal by solving the problems met along the way.

Good approach.

In general, the company follows a very thorough and well managed development process. The company needs to find a commercial partner (SES now) to quickly deploy several MW projects and learn how to optimize installation and OPEX.
The project is still in "early" stage of "the rubber meeting the road" despite the large investment by Boeing and DOE.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
2% absolute efficiency improvement in PV converter very impressive. Great progress on this project.

Accomplishments that have been achieved under the project are as follows:

Optics:
-Advanced design reduces pointing accuracy requirements
-Lower cost of all associated system components required for accurate tracking

Inverter:
-Deployed 260kW product to PV market
-Improved reliability reduces operations and maintenance costs

Tracker:
-Design integration/ parts reduction lowers cost
-Automatic calibration to cancel sun pointing errors

Progress in cost reduction through automation

Achieved 38.5% efficient cell structure

Several major engineering challenges met, including cell efficiency, reflective optics design, robotic assembly, and tracker design.

Completed robotic assembly and prototype field plans look like good progress.

The company is a leader in high volume cell production of MJCs, but has to continue to innovate. Other startups are being aggressive in pursuing novel techniques to achieve higher efficiency or lower cost/W cells.
Company has not achieved large deployments to prove the validity of the LCOE targets.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Great collaboration with optics designers. Really a stellar example of collaboration and tech transfer.

Collaboration with Colorado University and Cal State Northridge and University of California at Irvine.

Most of the work is internal to Boeing and Boeing-owned companies.

Appears to be good collaboration with team members.

Collaboration is generally with installation and commercialization partners, and not with technology developers. This is acceptable given the current stage of the program.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Clear targets for getting to 40% efficiency.

Solar cell efficiency- Driving towards 50%
•Manufacturing processes- mirrors, coatings, high volume assembly systems
•Next generation optical architectures
•Next generation module & tracker
•Reliability testing

Program is near completion. Installation and monitoring of arrays remains.

$19MM DOE, 50% cost share

30% module efficiency based on the optics area, only direct normal radiation.

Company is working on both lower risk improvements for cost/W (larger wafer, etc.) as well as larger steps, like 4J cells.

Project Strengths:
Great collaborations. Excellent progress to date. Good chance of success.

Well executed project and cell efficiencies are outstanding

Good improvement in cell efficiency over the program.

First 100kw deployment is underway now. Second system with CO University.

Good alignment with goals.

Project Weaknesses:
Doesn't appear to be focused on delivering low LCOE as the original intention of the SAI program. The overall details of the cost model was not presented, making it hard to determine if the goals are being met.

Recommendations for changes to the Project Scope:

Project is ending this year.

Capture results from the prototype and report on cost effectiveness.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT023_
Presentation Title: Low Cost High Concentration PV Systems for Utility Power Generation
Investigator: McConnell, R. D.

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This presentation was a series of press releases. Not sufficient detail to show taxpayer what was done with their > $15M.

Amonix has a project that addresses the need for low cost PV.

The program objectives:
Develop a high-volume (30 MW/year), low-cost manufacturing facility
Redesign components of the Amonixhigh-concentration PV system for high-volume, low-cost manufacturing and installation
Incorporate high-efficiency multi-junction solar cells into redesigned system
Conduct reliability testing of redesigned system components

This is a large and expensive TPP project which supports CPV progress towards LCOE goals.

Successful completion of the goals would align well with DOE objectives.

Company has had a long term commitment to CPV.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Not enough detail in presentation to evaluate.

Good multi-prong approach to focus on overcoming the historical problems with CPV
1. Design of a new power module

2. Manufacturing- set up a new automated manufacturing site
3. Shipping- optimize the new module for ease of shipping
4. Installation/Performance and Reliability- field testing for these attributes

This is largely a manufacturing development project with improved concentrator cells and robotic assembly. Complete system design and installation efficiency have been well addressed.

Although it appears progress has been made, little data was supplied to offered to support that.

Project is focused on micro-optimizations, rather than a clear/comprehensive set of engineering methods to reach more optimal solutions.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Not enough detail in presentation to evaluate.

Results:
The DC (module) efficiency is close to 29% (as verified by Sandia), whereas the AC (system) efficiency of 25% includes inverter losses.

Very little information was available concerning actual results. A lot of "advertising" and press release information was presented.

Tangible growth in capability and jobs is impressive. Detailed technical and cost achievements were not presented.

As a long-time leader, company has significant deployments or CPV.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Not enough detail in presentation to evaluate.

UNLV, NREL, and Sandia partnerships

Collaboration with UNLV, NREL, and Sandia. It is not clear what was contributed by these partnerships.

Not offered.

Company does not seek synergistic partners, and work with outside entities mostly as suppliers/subcontractors.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Not enough detail in presentation to evaluate.

Future Directions

Complete IEC 62108 qualification and UL 8703 safety certification

Expand reliability studies through system deployment, compilation of reliability data and investigation of lifetime prediction methodologies

System reliability testing and certification. No specific goals are provided.

$15MM, 60% cost share.

What is the performance over the year?

Biggest drop in LCOE is next year? What is driving that.
Why is reliability testing late? Tweaking the package.

Reduce costs.

Company is tactically focused on lowering LCOE (perhaps an appropriate choice for them).

Project Strengths:
Not enough detail in presentation to evaluate

Good focus on commercialization by installing and measuring cells and modules.

30MW/year facility. 90 employees is a strong positive.

Project Weaknesses:
Not enough detail to evaluate.

Cost projections appear to not be vetted well against experience.

Lack of information as to actual accomplishments.

No data presented on installations, even though they have been in the field for over a year.

Lack of reliability testing.

Unclear on cost out activities.

Recommendations for changes to the Project Scope:
Company should be required to report enough data/results to show that they actually achieved something with this money from the DOE.

Project is ending this summer- no cost extension should be granted.

N/A Project funding is essentially complete.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT024
Presentation Title: Concentrating Solar Panels: Bringing the Highest Power and Lowest Cost to the Rooftop
Investigator: Deck, Michael

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Rooftop high concentration CPV is a very unique and smart approach, given the higher cost of avoided power on rooftops. Very smart investment by DOE to see if this approach has merit.

Soliant mission aligned with DOE SETP
– Reduce LCOE to beat retail grid without special solar subsidies

Supports LCOE goals by addressing rooftop CPV system design and reducing installation costs. The goal is to beat retail grid LCOE.

Successful project will align with EERE goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Good research and development approach.

Approach is quite commercial which is novel in the CPV sector-Soliant is striving to overcome the large barrier to central power station and large tracking array fields normally required for CPV.

• Tracking CPV for the commercial rooftop
– Mounts like a flat plate
– Low profile
– Superior area efficiency

– More time at peak energy

On the engineering side-

- set up small factory
- produce small amount of product for UL testing
- produce arrays for rooftop testing

Project is well focused on overall integration and cost reduction for the total installed system cost.

The case for strong commercial promise was not made.

The company has brought in expertise from the automotive, semi, and other high volume mechanical industries to develop the product.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Not much quantitative data presented on performance. Looks like manufacturing is getting more automated and up and running, which is good.

Soliant has achieved its key objectives under this program
– Designed a system for reliable, low-cost rooftop CPV that installs like flat plate
– Demonstrated energy production capabilities of that system
– Proved manufacturability at pilot scale
– Field-tested and accepted by customers
– Shows path to LCOE under $0.08/kWh

They are however late on delivering the larger array format

Designs completed and panel deployment in progress. A prototype manufacturing capability exists.

Good progress has been made.

Due to the early stage of this development, many questions still remain unanswered, including the LCOE targets, suitability and acceptability of a "bulk" object on rooftops, and the speed at which other technologies are moving forward.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration between cell suppliers and customer.

Partners included:

• Technology Partners
– 3M – supplier
– Spectrolab (TPP partner) – supplier
– Isuzu Glass – supplier
– Danaher Motion - supplier
– SunEdison (TPP partner) – field test
– Emcore – supplier
– GE – technical solutions
– MIT (TPP partner) – advanced research

• Channel Partners
– Sunlight Electric
– Morrow-Meadows Alternative Energy
– Meridian Green Partners
– SunEdison
– Turiprojecto

• Customers
– Caltech
– Chevron Energy Systems
– MBK Enterprises, Inc.
– A major entertainment company
A large network of suppliers and technology partners has been created.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Clear goal to get to higher concentration.

Future work is limited to this near term plan as the project ends in September 2010.

Plans & milestones through Sept 2010
– SE-1000X design complete and prototypes tested (7/2010)
– Safety certification of SE-500X complete (6/2010)
– Additional field test results (6/2010)
– Solid order book (8/2010)
– Progress towards full receiver assembly automation (8/2010)

The project is nearly complete. Demonstration of results and certification remain.

More details on reliability testing would have strengthened the presentation.

Requirement of direct sunlight (vs. diffuse light) rules out many locales.

Project Strengths:
Very creative CPV approach on rooftop. Beginning to move into some real pilot manufacturing.

This project has made a lot of progress in a short period of time.

It has been well thought out and executed.

Predicted completion of UL listing.

Following automotive example is smart.

Path from $0.39 to $0.08 for LCOE.

Project Weaknesses:
Not much technical data showing real progress in this project.

Need more effort on reliability studies and understanding of what is needed when modules are placed on the roof. Perhaps this work has been done, but it was not apparent in the presentation.

Multiple redesigns were required.

No cost data was presented.

Very little performance data was presented.

Weak capacity growth plans.

Recommendations for changes to the Project Scope:
Project is ending, no further work is planned after the project end.

Continue only with full milestone completion.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT025
Presentation Title: CIGS Technology
Investigator: Contreras, Miguel

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
It is important for NREL to maintain a core competency in CIGS type of thin film materials. Would be good to see a shifting focus to more earth abundant materials.

This project directly addresses reduced cost and improved efficiency by understanding the limitations of present CIGS manufacturing processes.

The large number of start-ups in the US need help.

Project is embarking on significant efficiency targets for CIGS at the wafer scale size, with expected ability to affect LCOE in a meaningful way.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Well laid out approach to solve specific CIGS related problems. It is not clear that the work being done is onto any particularly promising solutions for "closing the gap" in cell vs. module efficiency in CIGS.

Approach is as follow:

Approach to narrow the gap between laboratory efficiency results (20%)
and commercial modules (<13%):
• Work closely (tech transfer) with CIGS manufactures to improve their processes and materials to narrow the performance gap.
• Use new capability (CIGS PDIL cluster tool) to study issues that limit

performance in an industrial setting.

Approach to develop reliable and stable CIGS products:
• Study the degradation of each component layer of the CIGS solar cells subjected to heat and humidity (accelerated damp heat 85˚C/85 RH test).

Approach to increase production volume
• Development of metrology/sensors to improve large scale issues such as device efficiency, yield, throughput and materials utilization. Studies geared toward high speed deposition of CIGS.

Clear focus on understanding and closing the gap between record research cells and commercial cells. The reliability work and sensors for manufacturing control will be critical for future improvements.

Very rigorous and methodical approach, including a highly controllable and observable multi-target chamber that can eliminate the uncertainties of non-vacuum intermediate steps. This is necessary to separate out fundamentals device effects from manufacturing effects/exposure to air/H20/etc.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Why did IBM do CZTS first and not NREL given our capabilities at NREL? Is this program underfunded?

Accomplishments

-Developed quality control tool for CIGS absorbers. The approach will lead to real time feedback on CIGS quality which can save money (and lower production costs) by enabling quick process optimization and product quality monitoring.
-Began development of optical non-destructive techniques (ellipsometry) as applied to CIGS fabrication
- Developed Protective Metal Oxides for protecting the TCO window on top of CIGS cells which is extremely stable in Damp Heat

Identification of causes of and potential solutions to contact degradation and reliability problems are well under way. The PDIL tool is operational. The sensor project is progressing well. The cooperation with industry and manufactures is very strong.

Explaining the basic properties for CIGS is very useful program.

This project will produce significant results once the new equipment in PDIL is fully up and running. The parallel focus on the TCO on reliability is also very important.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration with industry. Contreras is a critical knowledge bank in CIGS technology at NREL.

Good industrial collaboration-

JLN Solar
Applied Quantum Technologies
Dow Chemical
Climax Molybdenum
DuPont

The program demonstrates very good coordination and cooperation among industrial partners, universities and other groups at NREL. The program has done an excellent job of working with many groups to understand and solve problems.

Good group of industries involved.

As in most projects at NREL, excellent outside collaboration and dissemination of information to the outside.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Reasonably well laid out plan. Moving toward earth abundant which is a good move.

Future Work- has direct relevance and is built on prior accomplishments and responds well to industry needs.

These include-

Begin full operation of tool in PDIL

Build and assess prototype lifetime tool and technique for application to manufacturing lines

Build a library of optical constants (n,k) for CIGS and related compounds

Continue to develop TCOs that are tolerant to humidity and/or present superior physical properties to existing TCOs
Accelerate preliminary work on kesterites $Cu_2ZnSn(S,Se)_4$ started in "Seeds" program

Validating and using the PDIL Cluster tool to resolve the commercial vs. lab results gap by determining which contaminants or morphology problems are most deleterious should get high priority. Reliability and contact improvement methods are equally important. The sensors work has potential for improving manufacturing yield. Some level of new materials work, coordinated with other NREL and external efforts is useful for understanding longer term improvements.

Continuing industry support is good.

Based on the scope of this program, significant results should be coming from this effort in the coming months.

<u>Project Strengths:</u>
NREL needs to continue to have a core competency in CIGS type materials due to the ultimate efficiency potential. Good move into earth abundant CIGS

Well executed and responsive to Industry needs- seems to be well directed

Clear focus on understanding and resolving the limitations of CIGS manufacture.

Correlation with lifetime is benefit.

A new TCO that is fault tolerant would be a great achievement for a National Lab.

Emphasis on reliability is very good.

Lack of focus on champion cells is a positive. Time to enable the industry.

A systematic study of impurities could be a positive contribution across the board.

Project Weaknesses:

Would like to see more leadership coming from this group. It seems like this group is trailing industry, not leading it.

Very broad, perhaps should be focused down in scale with more resource on fewer items.

The team cannot help every group that comes in the door.

Why go to new materials in this project.

Recommendations for changes to the Project Scope:

More focus on earth abundant to get ahead of the curve and regain some technology leadership at NREL in this area. Combinatorial computational and experimental approaches are particularly ripe now.

Continue funding, but ask for a critical prioritization-

1. Contact stability
2. Optimum properties for component level materials to reference back to the 20% cell properties.
3. Sensors and metrology for quality control

Stay the course for industry support.

Test new materials with a new program.

The program should try to secure more funding from the commercial partners.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT026_
Presentation Title: Delivering Grid-Parity Solar Electricity on Flat Commercial Rooftops
Investigator: Sager, Brian

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Low cost thin film PV is a key enabler to get to $1/W.

Project aimed at reducing cost/watt to below $1. Scale up of roll-roll processing with an aluminum back sheet.

This project is focused on reducing the installed cost of industrial scale PV systems. It focuses on cell manufacture, mounting hardware, and installation costs.

If the goals laid out are achieved, this program is well aligned to DOE objectives.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Great R&D approach. Company cleared shared what they are doing and what has been achieved.

Tough to tell what the approach was-

Build large scale machine
Test layers and cells
Test module designs and installation methods- frameless laminates
Certify modules

The major focus areas are cost effective manufacturing of cells by a roll to roll process, reduced panel hardware cost, and efficient installation procedures. A systematic approach has been taken to integrate these areas.

The focus for this program should be exclusively on meeting the product performance goals at the module level. Any R&D work on installation is misplaced. No one will doubt that the modules can be deployed using the lowest cost technology available. Get the product out in any configuration and measure the performance.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Great R&D approach. Company cleared shared what they are doing and what has been achieved.

Tough to tell as there is only one set of production cell data presented. There is also no analytical data on the film properties. No statistics of how the manufacturing process is working.

Nanosolar has commenced production of utility-scale solar panels

•Nanosolar panels have successfully passed TUV and UL tests (IEC61646) and is Class A Fire Rated
•Test installations being deployed in current performance year.

The total system design is quite appealing. The nano-particle RTP system is operating. Panels have been fabricated and approvals have been completed. The 8% to 10% efficiency needs to be improved for this system to be competitive in the future. The demonstration systems need to be installed and monitored to prove the concept.

The lack of any field performance is not explainable. Placing a few modules in the field and recording data is a very easy thing to do for such a large effort. The missing data implies that the results must be very poor.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
NanoSolar has capacity to bring its own innovation to market.

–Sunlink LLC, subcontractor. Co-development of Sunlink-Nanosolar mounting system for lowered system cost and streamlined installation
–NREL, technical monitoring and validation of cell and panel performance
–Sandia National Laboratories, validation of panel performance

Collaboration is basically the supplier chain and evaluation labs.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Not too clear what next steps are for NanoSolar technology wise.

Future plans are very commercial-

Deployment of two pilot projects (commercial rooftop, carport)
•Measurement of deployment process and analyses using activity based costing
•ISO9001 cell and panel factory certification

The program is nearing its originally scheduled end. The installation and verification of the product remain.

It is very hard to judge where they stand with so little performance data presented.

Where is reliability? Is there any reason to believe they will be more reliable than other CIGS modules. What are the plans in this critical area?

<u>Project Strengths:</u>
Good progress on technology. Very open discussion of what has been achieved and what

challenges remain.

Have started producing panels and getting real installation data

Well integrated system design.

Roll to Roll RTP is huge.

Data indicates 10% efficient on 2000 cells.
15% champion cell is good.

11% at 200 cm2
Certification testing is complete.

Project Weaknesses:
Still using glass, which gives a cost basement.

Costs are not well understood- this should be addressed with the accounting methodologies described.

Delay in manufacturing and installation of systems.

Focus on installation is misplaced.

Lack of bank funding for installations was completely predictable. The lack of pilot installations is hard to understand with the availability of both money and manufacturing capacity and is disingenuous.

Glass/Glass modules with conventional materials is heavy cost burden to carry on low efficiency modules.

Recommendations for changes to the Project Scope:
Project is ending- no further work is planned under this contract after the final date of Q3 2010. The Program should reevaluate the current level of funding versus the overall benefits of this effort

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT027_
Presentation Title: "Fully Integrated Building Science Solutions for Residential and Commercial Photovoltaic Energy Generation"
Investigator: Mills, Michael

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
PV shingles could be the game-changer that gets PV to true grid parity on rooftops. Very exciting upside, long slog to get this done and Dow has committed. Deserves to be supported by DOE.

Aimed at low residential rooftop LCOE, one of the target programs of the SETP program.

This project focuses on installed residential PV system cost reduction by incorporating power generation in building materials.

2015 manufacturing goal of 200MW aligns with DOE objectives.

Company claims to target lowering the installation costs component of the LCOE with the roof integrated solution.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Very clear long term path to get to successful product. Impressive patience.

Roof shingle design beginning with CIGS-

Design the module using standard roofing materials
Obtain certification
Commercially deploy modules for reliability data.

Reduce installation costs.

The complexity of integrating PV into shingles is vastly more difficult than just producing PV modules. Many of the barriers have not been faced by other technologies. The vast range of necessary considerations and problems encountered seem to make a focused effort difficult.

Solid team and well-reasoned approach.

The company has identified many of the "practical" and not necessarily hard core engineering problems that need to be solved for roofers to install the cells quickly and efficiently.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Great progress on efficiency, deploy-ability, and packaging.

Accomplishments are as follows-

Over 10,000 functioning prototype shingles have been produced along with required system integration pieces.

Fundamental modeling of CIGS degradation with emphasis on acceleration factors for single and multi-mechanistic processes

The number of areas to be addressed beyond the thin film solar cell problems have made the risks of this project higher and the delays are understandable. The progress is less rapid than the original schedule. The technical solution of using glass as a barrier is understandable at this time, but it appears to be an expensive, but perhaps necessary, long term solution.
Installations of the current version of the shingles are minimal. The cost is unclear, so it is hard to judge any potential success in the installed cost and LCOE areas

Limited evidence of a meaningful decrease in system costs. The incorporation of glass makes the product similar to products made with crystalline silicon in the past.

Company has only installed in the thousands of units, despite the significant resources by the company and DOE applied to this program.

Company has not shown sufficient detail on the actual costs of the "module" and installation.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Excellent engagement with downstream installers/home builders. Critical for ultimate success.

Global Solar and Pulte / Lennar

Most of the collaboration is in the supply/user chain. Two large home builders are engaged in the project.

Good interaction with the builder community.

The partnerships are focused on the practicalities of getting support from a handful of home builders, who will be key in adopting this and achieving learning and economies of scale for new constructions.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Key issue will be getting good enough barrier layers, which company knows well. Not a clear solution here.
Dow Solar awarded up to $140 million for DOW POWERHOUSE™ Solar Shingle commercial facility from the Michigan Economic Development Corporation

Dow is scaling up to more than 200 megawatts of capacity by 2015 from this first commercial facility

Dow and the California Institute of Technology have a multi-year research collaboration aimed at lower PV materials costs.

The future plans are focused on building manufacturing capability and new materials. To get better short term market penetration, it seems that more effort should be applied to verifying operating efficiency and foolproof installation.

A clearer path to the very low costs needed for eventual success would have strengthened the work.

Company has opportunities for reducing cost of the barrier materials.

Project Strengths:

The exact right problem to be working on. Excellent progress. Good patience shows that they actually might get there.

Very focused commercial program:

The Beta-version of Dow Solar Shingle has been designed to incorporate many of the learning's from code testing, installer trials, Dow Solar Advisory Council, and recent Dow Reliability Summit.

Extensive outdoor testing (Midland, Miami, Phoenix) coupled with accelerated and highly accelerated testing continue to support the reality of a 30-year useful product power output life.

Targeted product introductions (IBS 2010) has confirmed demand for a “true aesthetically pleasing” BIPV product capable of significantly impacting the distributed power generation in North America.

Dow is investing engineering resources now to scale the manufacturing capacity to meet the market demand for the Dow POWERHOUSE™ shingle.

Dow is also putting about 30% of the product being produced in demonstration arrays which will help predict lifetime based on real data.

Asphalt signal product is a great target.

One commercial installation has been completed.

Emphasis on reliability is very positive.

10,000 shingles manufactured.

Different installations around the country are very good.

Project Weaknesses:

Barrier layer is still a challenge and is not clearly addressed in this work.

Cost info doesn't seem to be well understood and there is no viable flexible front sheet option at this point.

Very little progress on barrier layers for package.

Going to glass as front surface makes the final product very similar to roof tile products that have been made for years.

Other than installation ease, it is not clear how this product will lower costs.

Recommendations for changes to the Project Scope:

Keep going at the planned rate and project scope

Complete the program if the milestones are made.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT028_
Presentation Title: "Development of an AC Module System"
Investigator: Miles, Russell

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Module level power electronics innovation is critical to get to DOE cost goals. Underinvested in so presents a good opportunity for DOE to make a strong impact.

Inverter reliability Eliminate the weak components in today's inverters, to achieve a 25-year useful lifetime

BOS Cost & Installation Efficiency Minimize mounting components to drop installation time in half

Modularity & Standardization Integrate PV module, inverter, mounting, wiring and data

Systems Engineering Remove the complexity from system design by eliminating all DC circuit issues

This project supports reduced system cost by simple modular inverters and improved inverter reliability

Lack of cost information made this impossible to judge.
This program addresses a key aspect (BOS/inverter) of PV program.
The company has not quantified the cost (LCOE impact) of their solution.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Clear goal and approach to R&D.

Create a fully-integrated AC Module System, with a high-reliability micro-inverter (>25 year lifetime), fewer materials, rapid mounting system and integrated data acquisition

Focus on simplifying BOS
– Integration of module, inverter, mounting, wiring, data
Focus on Inverter Reliability
– Elimination of weak components with known failure mechanisms
– Thorough testing
– MTBF 500 years

The focus is on reducing marginally reliable components and simplifying installation. If done successfully and cost effectively, these improvements will help increase rooftop PV penetration.

Very few technical details were included to understand barriers.

Company had not explained the rationale for selecting their current architecture, out of a pool of various architectures. The 2nd phase of future work (No electrolytic C, soft switching, etc.) is already under development by others.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Not much/any real performance data to evaluate from.

Green-Ray has done the following-

High Reliability Inverter
• Goal: useful lifetime of 25+ years
• Accelerated Testing suggests: 50+ years (Phoenix)
• MTBF 500 years

Module level performance data
• Every AC Module reports its performance
• Smart Grid functionality (2-way communication capability)
• Web display of data

Residential Mounting System
• No racks or rails
• Very few parts
• Cuts install time in half

Wiring Scheme for 120, 120/240 and 120/208
• One version for all service voltages

The system design of modularity and ability to interface to 120/240/208 volts is very useful. Some demonstration systems are complete. Reliability improvement is estimated from models, but little accelerated stress test data or real life test data is available.
No data is available for inverter efficiency vs. single inverter units.
No cost or selling price data is available.
The cost and efficiency are critical factors in the success of such a project, and should be available in order to judge progress toward the goals.

Although designs and concepts appeared good, actual accomplishments were hard to judge.

The company indicates that they have completed lifetime testing and deployed inverters already. It is not clear to this reviewer how many units have been installed in the field and for how long.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good downstream collaborations/partnerships.

Industrial partners- large companies which is a strong endorsement of the project

• SANYO Energy
• National Grid

• Berkshire PV Systems
• Geogenix

The collaboration is among the partners needed to test and verify the system.

Interactions with installers is a positive. More interaction with other supply chain groups would have been an improvement.

Work with the DOE and Sanyo as partners; detailed synergy unclear.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Clear what needs to be done to get to some real prototypes with data.

Future work-

Complete Safety Certification

Market Launch
• 2,000 units, 3Q – 4Q 2010
• Manufacturing cost reduction
• Document lesson learned on fielded AC Module

Systems Long-term testing
• Temperature cycling
• High temperature testing

The program ends in 6 months. The completion of the current goals will be difficult in that time frame. Cost reduction and reliability verification are the key areas to complete.

$3.3MM, 50% cost share.

Plans seem reasonable.

Several of the activities identified for the future are already being developed elsewhere.

Project Strengths:
Right problem to be working on. Plug and play is key.

GreenRay's AC Module System is
• Simple- easy to understand and work with
• Safe- No live wires, no dc wiring

This project addresses the whole BOS part of the value chain

Modular and flexible system design.

Reduced installation time is great. Elimination of the DC components is a positive.

Project Weaknesses:
Not much if any data yet to show whether this approach will work.

Project is late due to

– Technical challenges developing an inverter with no electrolytic capacitors
– Lengthy process of testing and delays in Safety Certification

This is a very broad topic to address this whole portion of the value chain

There appears to be no specific cost target in $/Watt or LCOE

Schedule and lack of information on real status.

What value is the history record. None of my other appliances need it.

No certification yet.

No prototype test data.

No lifetime data presented.

What about the cost of the inverter itself?

What is the breakdown of alleged BOS savings (beyond installer hours).

What about the inverter performance.

Cost will only be competitive with present inverters (not better).

Recommendations for changes to the Project Scope:
Project ends in 2010, keep going until it is done.

Fund only to the extent that the goals are made.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT029
Presentation Title: Theory and Computational Science
Investigator: Lany, Stephan

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Theory group at NREL is a very important core competency to guide experimental work to come up with real breakthroughs. I would like to have seen a list of success stories from the past of where this group had a big impact on some specific experimental discoveries.

This project supports the long range program goals by providing fundamental device and materials understanding. The project purpose is stated as:

Develop scientific understanding of PV relevant materials properties
Address barriers facing existing PV technologies
Provide theory guidance for promising directions of PV R&D
Accelerate the advances in the long-term goal of employing solar cell technology efficiently and cost-effectively.

If properly coordinated with other groups, this effort can be invaluable in reducing costly experiments and saving significant time when trying to solve PV materials problems. The basic modeling capability is critical for long-term improvement and understanding.

Fundamental knowledge of materials is important to the EERE effort.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Good interaction with experiment and theory groups at NREL was clear from the presentation. Not 100% clear how they decide which problems to focus on though.

All of this work is aimed at fundamental understanding through both computational theory and imaging diagnostics. The work on looking at abundant, non-toxic, inexpensive alternative materials is well placed and timely. The subject is so broad, hard to quantify the approach; the work is closely tied to the experimental work at NREL.

The projects are focused directly on known problems and basic capabilities. The defect imaging project should probably eventually move to the measurements and characterization group. The potential problem with Theory and Computational Science is that choosing what to work on needs to be reviewed carefully because the areas to explore are so much larger than the budget can support.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good technical accomplishments. Good strong interaction between theory and experimental Good results on problems that are prioritized. Seems there should be a list of projects that are agreed upon through some collaborative work with either universities or industry.

All portions of the program seem to be providing significant results.

Incremental learning was demonstrated.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Excellent internal collaboration at NREL

Seems like it is just collaborating with internal NREL groups. There may be more synergy with other research groups around the world.

This work is widely applicable and seems to be well coordinated within NREL to address the needs of other groups.

The collaboration with researchers at NREL was noted, but did not seem to play a very large role.

Working together with experimentalists was noted as a path to bring theory results to practice.

I expected to see more of this interaction.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Not 100% clear where they go from here, but I'm sure they will do good work.

Good plans, again would probably benefit from a priority set of areas which are defined either by the most important limiters for near term PV cells or prioritized to help set the platforms for new and novel PV devices.

Considering the budget, continuing along the current success path is a good plan. It would be helpful if some method to quickly apply some of the groups' knowledge and capabilities for problems which arise outside the current yearly schedule.

I would expect higher emphasis on new materials and less on existing materials (silicon for

Project Strengths:
Impressive and important core competency at NREL. Good interaction between theory and experiment.

Really good science and fundamental understanding. This is rare work in this arena and should be supported fully.

Top quality scientific understanding and modeling.

Building fundamental materials knowledge is important role for National Labs.

Project Weaknesses:
Would love to see a bold theory/experiment combinatorial search for earth abundant thin films with high efficiency. Would be a great NREL signature initiative and could be the nexus for a lot of other research.

Not directly connected to any one priority set, perhaps there is a better way to do this.

It is hard to understand the predictive impact of the work.

More experimental collaboration would be a positive.

Recommendations for changes to the Project Scope:

Again, very good fundamental work that needs to be supported. There may be a better way to prioritize what is worked on through either an oversight group or collaboration with other groups.

The payback of these projects seem large compared to the cost. Applying more resources in these areas would probably be a worthwhile investment.

Work may be best focused on new materials and replacement materials.

This group needs to be maintained (not cut), and if possible grown with a few very high caliber talent. Some of the skills and competencies take years to develop and are critical to the overall progress in PV and beyond. The group needs to have a process to maintain a tight linkage/correlation with experimentalists, while dedicating most of its time resources to working on the theory: the theoretical focus should not be diluted with too much emphasis on experimental and phenomenological busy work.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT030
Presentation Title: Transparent Conducting Oxides
Investigator: Perkins, John

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Good impact in that these materials can apply across the board. Not clear whether or not this is something the industry could do just as well as NREL to me though.

This work is aimed at creating TCO's that are reliable and inexpensive. This work has strong cross-cutting potential between thin films and OPV.

High quality contacts are critical for improving efficiency and reliability of most PV technologies.

Good cross cutting project that can help many different solar projects.

TCOs are a horizontal technology that needs to be solved for most PV technologies.
Clear and relevant goal of finding a solution that doesn't have dependencies on raw material with potential production limitations (or prohibitive costs at high WW PV production volumes.)

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Strong and focused approach. Good medium and long term metrics.

This project is well laid out and follows a logical process development path which is near term and valuable to industry partners-

State of the Art TCO Materials
Task 1: Discovery and Optimization of Improved TCOs

Task 2: Amorphous Transparent Conductors

New TCOs for Specific PV Technologies
Task 3: Optimization of TCOs for Device Specific Applications
Task 4: Interfacial TCO Properties and Stabilities

Scale Up New TCO Materials
Task 5: Scale Up and Large Area Film Optimization

The technical problems to be addressed are well defined and understood. The approach is, perhaps, too heavily oriented toward experimentally changing deposition conditions and then evaluating the results.

Good approach. Good technical discipline.

As with most NREL projects, very well organized and executed.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Great technical progress. Delivering on goals.

Summarized results

New TCOs have been identified that have superior performance.

New process approaches have been validated that are better integrated with PV production.

A wide variety of new materials show the potential with sufficient development to be transformative in PV cost and performance.

TCs have expanded definition to include composites, nanostructured materials and amorphous materials.

Very good progress has been made on the FY10 goals. The real success of these contacts can only be determined when they are successfully applied to the PV technologies they are designed to support.

Very good incremental building of knowledge.

Excellent progress so far on the candidate material. Program may want to focus on a limited number of target PV technologies (ex, WSi + one other) to achieve best progress on those, of the budget for the year is limited.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good interaction with industry.

Heliovolt
Univ. of Colorado
Corning
Colorado School of Mines
Arkema U
university of Denver
Konarka
SoloPower
DOW
Cambrios
WPAFB

A wide range of partners are directly involved with assisting and using this project's results.

Excellent collaboration with in NREL and with Industry.

Seemingly great transfer of technology through publications and work with large and small companies.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Very clear work to be done going forward.

Future plans are to -

Investigate new TC materials
– low temperature processing, tailored chemistry, work functions

Direct integration of improved TC's in Si, CdTe, CIS and OPV devices

Uniform large area processing with performance of best small samples
– PDIL with new large area deposition systems

In-situ diagnostics to optimize reliable materials growth

The proposed future research plans are not very specific. Performance goals and transfer to other group expected results are needed

Plans are good.

Additional exploration of materials, and new equipment to help explore more options.

Project Strengths:
Strong program. Cross cutting impact on all of thin film. Good progress and results toward goals.

This is great cross-cutting work on a fundamental layer required for any thin film or OPV cell.

This project addresses areas that are common problems for most PV technologies. By doing so, understanding of general problems can be achieved and duplication of effort can be reduced.

Enabling technology for many PV materials and products.

Good collaboration with NREL device CIGS team.

Large group of external collaborators.

Project Weaknesses:

Not clear that this could not be done just as well within industry.

None noted.

It is not clear that one size fits all is the best approach for the optimization of transparent contacts for individual technologies.

Incorporation of Carbon Nanotubes should only be done with new resources. The existing effort should not be reduced.

Recommendations for changes to the Project Scope:

Should consider increasing funding if it would help- if the resources are needed.

Fund this effort at a higher level.

This is a near ideal project for National Labs. It should be held out as an example for others.

It is not clear if there are overlaps and redundancies with other group within NREL/DOE on TCOs, but this is a project that should be considered as a candidate to have significantly higher resources to make a "big leap". The target should be to find a TCO that can scale to 100 GW of annual production of solar panels.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT031
Presentation Title: CdTe
Investigator: Gessert, Tim

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
An effort to greatly increase the champion cell efficiency in CdTe and push the envelope of efficiency for this well deployed thin film PV material could have a game-changing impact. Would love to see some effort on tandem cells as well here.

Supports the better understanding and efficiency development for CdTe materials which have proven to be the first materials structure to allow for <$1/Watt production cost.

This project addresses improving the efficiency and reliability of one of the commercial thin film PV materials.

Supporting the US CdTe industry is very good.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Right equipment is finally in place to do great work. Not a lot of progress in this report, but well set up for future success in the near future.

Approach is to improve cell current through better glass and TCO's and buffer layers. This will be followed by the improvement work necessary to increase cell efficiency. This work will start by comparison between epitaxial and polycrystalline CdTe devices to understand materials and lifetime. This is a great approach in understanding the building blocks necessary for improving CdTl efficiency.

The focus areas directly address the known shortcomings of the current technology.

Correlation of accelerated life test modeling very valuable.
Attempting to root cause the unexplained 150mV very valuable.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Few specific accomplishments here, but expect a lot going forward with PDIL.

Have demonstrated ability to produce CdS/CdTe devices on 3”x3” substrates with efficiency >11% on commercial glass substrates.

The CdTe PDIL tool completion and installation are delayed. The other device work is progressing on schedule.

Good incremental progress shown.

High efficiency and target record of 14-15% when the PDIL comes up are very meaningful and ambitious.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
NREL has strong industry interaction on CdTe. Had a spinout even.

Nine CRADA partners- and the following University partners:

CO. School Mines
CO State U
Vanderbilt U.
Univ. TX El Paso
Univ. MN

There is significant involvement and funding from other organizations for this work.

Lots of interaction with industry noted.

Significant collaboration with many commercial partners, due to the high commercial interest in CdTe.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Goals are very clear going forward. Lots of great ideas to dial up CdTe champion efficiency.

Near term work that is planned looks well throughout and can be executed

CdTe PDIL Tool Process Validation

outline CdTe Device Production/Research-Combine all aspects of superstrate device fabrication into one subtask. New CdTe-related TCO research funded through nonprogram sources

Higher-Performance CdTe Alternatives- Enhance subtask to investigate alternatives to present superstrate CdS/CdTe technology

CdTe Cell-Level Reliability-Continue to develop methods and metrics to understand and improve CdTe cell-level reliability

The near term plans for CdTe work are in line with the program goals. Careful coordination with other future materials efforts at NREL will be needed to prevent overlapping efforts.

Plans seem good.

Project Strengths:
The right problem for NREL to work on. Still lots of room for innovations to create big leaps in champion cells. Uniformity type of issues (like those in CIGS) are perhaps better for industry to tackle. Looking forward to major success going forward.

Really working the base issues on lifetime and efficiency inhibitors. Well thought out and executed with good analytical and theoretical balance.

Focus on improving the efficiency and reliability of CdTe cells and transferring the knowledge to industry.

Project Weaknesses:

Not a lot of progress yet. Will take a little time.

Needs more resources. Too many CRADA's?

Too much

Recommendations for changes to the Project Scope:

Plus up this program. Could have a quick impact and get adopted by strong U.S. PV leaders.

Fund higher so that more work can be done.

More emphasis on reliability. Consider three efforts, one on reliability, one on champion cells, one on moving industrial cells.

NREL should focus its activities in this area on a small number of broad-impact activities, to help and disseminate improvements in cost and efficiency to multiple commercial entities, and have the companies with the deep pockets take them to manufacturing as quickly as practical.

Review: EERE 2010 Solar Program Review
Presentation Number: PVNT032
Presentation Title: Sensitized Solar Cells (SSCs)
Investigator: Frank, Arthur

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Good long term program to put a few dollars into to keep the core competency going. Not likely to show commercial relevance in a big way for 10+ years.

This work centers on further development and understanding of Sensitized Solar Cells. Potential for large area/low cost devices has been what has driven this work and it fits well with the DOE goal of <$1/W in the next five years.

This appears to be a research project to understand some of the basic operation and limitations of sensitized solar cells.

Obtaining the goals set out would be good for next generation of thin film products that could come from the US.

It is not clear how long it will take for this technology to achieve meaningful commercial deployment.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Good solid, stay the course approach.

Approach is based on fundamental understanding-

− Understand material & structural effects on the relationship of physical/chemical processes of device to cell stability & efficiency

− Identify the most-promising cell materials & configurations for highest device durability & efficiency
- Develop cell assembly techniques for different device configurations combined with accelerated aging tests

The degree of maturity for the ssc is a lot lower than for most of the other technologies reviewed, so much of the effort is learning without a lot of specific deliverables.

Overall approach to tackling the whole device may be too big to tackle. Focusing on certain enabling elements of the device should be considered.

Solid experimental and theory work, given the very small team.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Hitting or approaching efficiency and lifetime goals. Would like to see more focus on understanding what limits lifetime. If it is the edge seals, let's focus more there.

Demonstrated 6.7% efficient cell with nearly 1000 hour stability

Evaluated various cell components & device configurations on device stability & performance

Evaluated NREL CdTe TCOs for sensitized solar cells which looks promising.

Reliability is close to the goals and some evaluation of contacts is in progress. There are few performance indicators against which to judge progress.

Good incremental progress is being made.

The efficiency results speak for themselves. The main challenge at this stage is to understand degradation mechanisms and lifetime.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
No need for tech transfer now for the most part. Too early.

Collaboration with two partners-

Office of Science (SC)/Basic Energy Science (BES)
CU Boulder

Moderate internal to NREL and external collaborations.

I expected more interaction with the other development efforts.

Appropriate collaboration with early stage partners (BES).

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Good plans/roadmap going forward. Well laid out targets.
Demonstrate 7% device with 2000 h stability

Study effects of cell sealing in moisture-free environment on device stability & performance

Investigate the stability & performance of solid-state sensitized solar cells.

Study effects of new dyes on device stability & performance (via collaboration)

Optimize nanotubes on TCOs with improved electrical & optical properties

The future goals are vague and do not lead to what appears to be a viable PV cell for meeting 2015 LCOE goals. The goals are basically to provide incremental improvements in efficiency and reliability.

The small team seems to be taking on too many elements of the device. It would be better to focus on a few elements and work on the invention needed for this new solar cell to be successful.

Significant opportunities remain.

Project Strengths:
Good long term program. Very strong team. Doing a lot with a little funding.

There has been significant progress here- over 6% efficiency which has some stability potential.

Good incremental progress on the overall device development, but there is an enormous amount left to do.

Project Weaknesses:
Not clear what lifetime limiter is. Is it edge seals? If so, more focus there would be good.

Project weaknesses have more to do with overall view of SSC's and how it fits into the long term program.

Too broad based for such a small program.

Recommendations for changes to the Project Scope:
I think the overall question of how much of the DOE SETP program should be spent on OPV. As the cell efficiencies are still so low, recognizing that there has been a lot of money both public and private invested, one has to question the effectiveness of this support. Perhaps OPV is a in its early phases and is best solved as a lab problem and there should only be monies spent at NREL and no SETP monies spent on commercial companies working on SSC's or OPV's. This deserves a lot of discussion and vetting with regards to how it fits into the SETP program. If it is determined that SSC's/OPV's should be worked at the lab level, then the support should be increased in order to see where these materials can go.

Investment in this program beyond some minimal level maintaining understanding of efforts underway elsewhere should not be of high priority.

Consider narrowing the focus to a few enabling elements.

This is a skeleton crew necessary to maintain a presence in this segment of the PV space. This effort should be maintained, and if resources permit, should be increased slightly (but not significantly yet).

In order to ensure the anonymity of reviewer feedback, reviewer comments are listed in random order for each question

---

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT001_
Presentation Title: PV Technology Incubator
Investigator: Symko-Davies, Martha

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This type of program is excellent for supporting DOE goals.

An appropriate use of federal funds to assist start-ups. Must be recognized that, as with small businesses, many will fail.

Very well-aligned with program goals. Very diverse set of technologies, and rigorous. Industry focused with basic research.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Hard to do this for a whole program, but the work overall in the program is well-chosen.

Selection and monitoring are well thought out.

Good

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Again there are sufficient program goals in each project that provides excellent feedback. Well-designed program.

Difficult to assess in the time available. An entire review meeting could be devoted to this program.

Good

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Excellent programmatic encouragement to collaborate. It is done well by some and less by others.

By definition this program coordinates with industrial partners.

Good

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
There is mostly a clear path to achieving the stated goals in each project. Some are at such an early stage that it is difficult to assess how well they are doing.

Intelligent learning from past experiences.

Good

Project Strengths:

I wouldn't change either the pre-incubator or incubator program. They have been well designed.

Putting federal money where it can do most good. Feds control, but industry carries out the work with profit-making incentive.

Good

Project Weaknesses:

Several times, Martha defended the review saying that the projects were already reviewed. This misses the benefit of peer review.

Recommendations for changes to the Project Scope:

I wouldn't change this program.

Both DOE and the investigators should study the lessons of the following innovators: Edwin Howard Armstrong, and William Shockley. In both cases, invention was profound but benefit was not effectively realized in a timely fashion. Pairing a brilliant researcher as PI with a brilliant business developer (MBA?) is necessary for DOE to more rapidly reap the benefits of this approach. This model for R&D follows that used by venture capitalists, as VCs fund, set up, and sit on the boards of successful U.S. enterprises that often efficiently take work from university professors to the international marketplace. To learn more, type the two names above into http://www.wikipedia.org/

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT002
Presentation Title: "High Efficiency Single Crystal CdTe Solar Cells"
Investigator: Carmody, Michael

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Difficult to assess the commercial impact of this cell even if all milestones were met. The combination of having to have a Si substrate and the MBE growth make this problematical when comparing to the MOCVD growth of IMM III-Vs.

Possibly result in a competitive technology but many difficulties in the process.

The idea to use a CdZnTe is sound and can contribute an alternative to III-V concentrator cells. This is in line with SETP goals.

The technology under development is appropriate only for concentrated solar power applications, due to the cost of materials and fabrication technology (molecular beam epitaxy). As such, this technology needs to be compared to high efficiency (>40%) III-V technologies at the system level. It is not obvious that the proposed technology offers a clear cost or performance advantage in this comparison, given that the cell cost in a concentrator system is a fraction of the system cost.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The approach is well thought out and the goals are being met. The only reservation is that achieving all the goals may not be enough to reach the overall goal of lower cost high efficiency cells.

Many processing problems to be overcome.

A reasonable R&D plan based on achieving specific values of fundamental materials’ parameters. While the approach to the CdZeTe cell is reasonable, its relationship to the bottom Si cell is less well thought-out and the technical barriers less detailed.

The technical development effort has been well constructed, conducted, and directed, although attention to current generation (quantum efficiency analysis, etc.) would be appropriate. The performance of single junction devices has been promising; however, the data suggest tremendous difficulty in reproducibly doping the binary material. The development requires doping control of a ternary compound, which could be difficult as well. This leads one to suspect that achieving uniform device performance over an entire Si wafer will be challenging.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Goals have been met as projected; however, later goals may prove to be more difficult.

Some impressive progress but many hurdles to be overcome.

Technical progress and accomplishments were noteworthy (10% efficient devices) and barriers to creating a proof of concept device were overcome.

As mentioned before, encouraging single junction device voltage performance has been demonstrated. The challenges in achieving doping control, improved currents, stability at elevated temperature, and high current densities are significant. Should these all succeed, it is not certain that this technology offers a marketable advantage over current III-V devices. Development of an economic model to flesh this out, say compared to IMM with substrate reuse, would be useful in determining future support.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Lacks any collaboration. Would benefit from a university partnership, e.g., Univ. of Toledo Nothing about other groups being involved other than NREL.

It is unclear as to the contributions of the partners. Several publications and patent applications were the result of the work thus far.

This work is very preliminary in nature. No collaborators are included but should be considered as the development matures, particularly for device testing, etc.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Future plan is well devised. Future barriers may prove to be more problematical. MBE growth on a silicon cell rather than a silicon substrate may prove problematic.

A very long way to go.

Need to establish the expected efficiency of the tandem stack with Si.
Need to explore the strange shape of the I-V curve for the single junction devices on Si and determine the junction properties.
Thermodynamic limit to voltage also determined by material luminescent efficiency. Need to determine the optimum orientation of CdTe and Si substrate [e.g. (100) or (111)].

The future effort description was limited to the remaining FY. Those plans were fine, but nothing was supplied to assess development beyond this FY.

<u>Project Strengths:</u>
Systematic approach to improving cell efficiency. Cost effective approach from DOE's perspective in that the cost share is 50%.

II-VI very attractive semiconductors but also difficult to deal with
Addressed throughput (MW/yr) and the results (in theory) seem favorable compared to other triple junction approaches even though MBE is used. There is a strong emphasis on thermodynamic limits and basic charge transport properties, as well as considerations of defect tolerances and lattice mismatch compared to III-V PV. P-type contact is to p-Si rather than the complex contact used in TF CdTe. Good p-hall mobility of 400 cm2/V-s.

The proposed development is novel, as most CdTe work is focused on poly-crystalline forms. The effort is making reasonable progress and initial device performance is acceptable.

Project Weaknesses:
Two things inhibit commercial development - MBE growth and silicon substrate.

Complexity

Has not achieved efficiencies higher than that of Si itself. Unknown expertise in Si cell processing. Unknown effect of Cd, Zn and Te diffusion in a Si cell. Low carrier lifetimes in nanosec time range seem to be a challenge unless single xtal are used for CdTe. The approach does not seem to be relevant to one-sun PV modules due to throughput and efficiency limitations.

The claim of advantages of the proposed approach over III-V's bears closer examination. It is not clear that this technology offers a cost or performance benefit, should it be successfully developed, compared to high efficiency III-V devices.

Recommendations for changes to the Project Scope:
Try growing on a silicon cell.

No suggestions

Optimize bottom Si cell separately and characterize efficiency. Establish or purchase state-of-the-art Si baseline cell for use in stack. Need to establish the expected efficiency of the tandem stack with Si. Determine effect of Cd, Zn and Te diffusion in a Si cell. Construct an energy band diagram to explain I-V curve shape. Need to study quantum efficiency (QE) vs. wavelength or photon energy. Need to determine the optimum orientation of CdTe and Si substrate [e.g. (100) or (111)].

If the program is to continue, performing an economic analysis for this technology in a concentrator PV application and comparing it to III-V devices would be useful.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT003_
Presentation Title: Novel, R2R Manufacturable, Photonic Enhanced Thin Film Solar cells
Investigator: Dalal, Vikram

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Difficult to envision the impact of the technology on the cost of future cells.

Would indeed make a substantial contribution to thin film cells in general.

This technology could be used for many types of thin film solar cells if it proves itself here. The approach is potentially low cost and high volume & throughput. It is well-aligned with SETP program goals for thinner active materials in PV.

The goal of increasing optical coupling, if successful, should allow solar cells to be produced with thinner absorber layers, reducing material usage and perhaps improving efficiency.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The photonic substrate seems well developed. The integration with the cell is more difficult. The barriers are being addressed.

Good direct attack on the processing technologies.

The approach uses fundamental optical parameters and goes on to measure associated electrical (a:Si PV cell) parameters. This is a reasonable and logical approach. Although they use a planar cell as a control, they do not go far enough to compare their approach to one that uses a simple back reflector.

The program has actually two different development efforts, textured substrates for photonic enhancement and a-Si deposition on a new low temperature polymer (PEM). Mixing these together makes assessment of the progress for either technology problematic.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Results show improved current but the cell efficiency is still low.
Convincing demonstration of photonic approach.

Quantum efficiency results are encouraging (in IR region) but not outstanding. Tandem thin film (TF) Si tandem devices are encouraging and show proof-of-concept.

The program has demonstrated low efficiency devices on the PEM and some marginal improvement in current for devices with photonic enhanced substrates. No comparison was made for devices grown with varying thickness absorbers nor alternate back surface optical treatments (BSR, diffuser, etc.).

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Collaboration with Iowa State only. Would benefit from collaboration with amorphous silicon manufacturer.

Pleasing to see university-industry cooperation. NREL appropriately involved in measurement.

Iowa State is participating in a meaningful way to characterize materials. There has been some publication and patent activity that for this stage is reasonable.

The PI has teamed with a venture startup firm interested in photonic enhancement to PV. It might be worth considering adding a commercial a-Si manufacturer to the team. This might allow better assessment of photonic enhancement benefits and tech transition should the development prove useful.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Good progress on substrate.

Clear logical approach to making further progress.

The plans are to continue to go in the direction established thus far. The optical efficiency and stability should, however, be brought into the study soon. They do not expect any barriers, and so plans for mitigation are low. One potential barrier is the optical interface with the PV device itself (both performance and longevity).

Plans do not address the issues raised in previous comments. It may be difficult to fully understand the benefit of the technology developed at the conclusion of the program if this plan is followed.

Project Strengths:
Substrate shows promise but cell structures need to be further developed.

Universality of approach to thin film solar cells.

It can be used later for plasmonic devices with, e.g., Ag dots. It can also be used for other PV technologies.

Enhanced light trapping is a useful tool that can be used to improve material utilization and potentially efficiency.

Project Weaknesses:
Would benefit from collaboration with amorphous silicon manufacturer.

None detected

Unknown interface, recombination and stability issues at the optic/cell. Work needs better control experiment with simple diffuser. They've done it, but have not reported it yet. Limited to low temperature deposition of cell to preserve structure of film. Presenter says "no" but this needs verification. Performance nowhere near the 4n^2 limit. Optical enhancements are modest only. They need to frame (in later reporting) their work in terms of prior literature from H. Kiess of the PSI and others.

It would be good to focus development on photonic film on Kapton thereby splitting off the effort to develop silicon on low temp PEN polymer substrate.

I-V plots should be presented with Jsc as the current axis to allow comparison.

The current increase in the QE is very modest and presents no real improvement compared to traditional diffuse back surface reflectors.

Recommendations for changes to the Project Scope:
Accomplished all goals as set forth.

The interface with the PV cell needs to be studied and imaged. Results need at least a few measurements at NREL to confirm I-V and QE results.
Compare current density, mA/cm2 instead of mA and compare work to a "control" with a reflector that is used in industry. The work needs better control experiment with simple diffuser. Establish whether the approach is limited to low temp. deposition of PV cell to preserve structure of film.

Drop either the PEN effort or the photonic enhancement effort so that sufficient resources can be committed and appropriate plans constructed.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT004
Presentation Title: High Efficiency Organic Solar Cells
Investigator: Joslin, Steve

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Excellent beginning to raise efficiency of organic cells.

Is there really any plausible use for a PV device well below 10% efficiency ?

A fundamental approach to address voltage limitation of OPV devices. Somewhat aligned with SETP program goals to explore OPV. If OPV is to have a future with this DOE program, this kind of basic study related to both materials properties and device performance is critical. Given the lower stability of OPV devices as a whole there are, however, some doubts that organic PV cells can ever be well-aligned with grid-connected kW or MW levels of production.

The objectives of this program are well-aligned with the DOE objectives.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project is in the early stages. The baseline has been developed.

Clearly well skilled in organic chemistry but is there really any hope of an energy conversion device based on complex polymers ?

Nothing said about the availability of the various metallic elements added.

However one must keep in mind Yogi Bear's remark, "forecasting is very difficult, especially the future."

Good - Focuses on voltage limitation using C60-type materials already in standard use in OPV. They are using synthetic chemistry, electrochemistry and band engineering to tailor and tune energetics and output voltages. This is logical.

I am not an OPV expert, but the approach outlined in the presentation appears to offer some advantages for systematically altering the consistent molecular species to positively impact device performance.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
So far so good. The next goal will really tell the story.

Impressive range of molecules characterized.

Electrochemistry helped establish LUMO & HOMO levels and energetics to confirm desired engineering design of molecule. Some technical barriers and establishment of baseline materials and devices have been achieved. Good use of baseline device using standard/state of the art materials. A 5% efficient device has been made with the voltage enhancement effect suggested.

The program has demonstrated reasonable performance devices over very, very small area. There has been some indication that the devices are somewhat stable.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Collaboration with Plextronics. Also assisted with NREL advice.

Suitable, other organizations closely involved with the project.

Partners provide materials for second part of cell (polymers) and produce larger PV devices. This critical aspect is addressed. Communicated with PV community about methods to report energetics. Very useful suggestion of standard reporting. Patent discussion with Virginia Tech. Some publications have resulted.

The team includes Plextronics, which is not only providing cost share to the effort but also offering a ready on-ramp to commercialization of technologies developed in this effort.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
The planned work is sensible.

Logical development program but is the goal line of practical value ?

Systematic, incremental but logical.

The future research efforts are aggressive in both efficiency and area increases, but well-constructed.

Project Strengths:
New approach to reaching higher efficiencies.

Knowledge of and ability to handle complex molecules and polymers.

Tailor and explore stability with side groups. Good emphasis on standards for reporting HOMO & LUMO levels. NREL measurements for efficiency and QE are encouraging. They are addressing stability of devices though contacts and deposition conditions. This project is complimentary with other grants and projects that they are undertaking. High value added.

Novel chemical approach to tuning OPV device levels. Having Plextronic as a collaborator has many advantages, including direct opportunity to commercialize new technologies.

Project Weaknesses:
Could use some university collaboration to facilitate characterization.

Doubtful in my mind that a commercial PV device will result from this or any other OPV program. Is any device of about 12% efficiency of commercial viability even if the cost is vanishingly small ?

Lu and Sc availability is a potential issue. Have not yet clearly demonstrated Voc (voltage) enhancement.

The goal of OPV efficiency of 7% is aggressive, but, even if successful, will likely be too low to have significant market impact.

Recommendations for changes to the Project Scope:
Expand to include university collaboration.

Begin to focus on stability of materials and devices compared to baseline.
Establish structure/stacking of new materials.
Report on effect of purity on performance and stability.

None

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT005
Presentation Title: "Project Title – Thin Single Crystal Silicon Solar Cells on Ceramic Substrates"
Investigator: Ravi, K.V.

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Difficult to assess the cost of epi growth followed by ceramic bonding.

Would be the ultimate technology for crystal SI cells.

Combines best of wafer and thin film Si. They are using a technique that is being explored in other groups around the world and the United States needs to explore this approach (Si wafer re-use) as well. This then represents a good match to SETP goals.

The goal of the effort is a technology to reduce the material and energy required to produce a moderate efficiency (>15%) silicon solar cell. This is aligned with the EERE goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Project is focused on addressing technical barriers. However, goals may have been set too low to impact industry.

Aware that barrier to contamination from ceramic must be solved to get acceptable efficiency.

Ceramic is the "handle" and is deposited after epi cell is deposited on a Si wafer that could be re-used by lift off. Good use of characterization (uW PCD) to understand impurity doping and diffusion into Si.
Good use of PC-1D to model expected results for the device.

Reasonable quality Si has been demonstrated via the developed technique to epitaxally deposit and separate off thick Si epitaxial layers. The proposed use of printed/sintered ceramic support may not be the optimal solution. The ceramic has low thermal conductivity, blocks the formation of full area back surface optical features (BSR, diffuser, etc.) and most likely undergoes sufficient volumetric change upon sintering to stress the Si material.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The barriers are being addressed and are on schedule. Further improvements may be harder to make however.

Should be able to meet August 2010 milestone.

They have demonstrated the process and device. A 7% efficiency value is good even though wafers bow/bend. They identified major causes of efficiency problems and have some plans to address them.

Reasonable quality epitaxial Si films separated from the growth substrate have been demonstrated. These films are being degraded by the ceramic support (strain and impurity diffusion), thus alternate support layers may be required. Cell efficiency is moderate at this point (7%), but they have achieve large area devices, which can be particularly problematic for epi lift-off processes.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Would benefit from collaboration with a silicon cell manufacturer.

Is it so evident that involving other groups will be beneficial?
NREL testing gives confidence in results claimed.

There do not seem to be any collaborations or partners. There are no publications or patents in evidence. The IP on this may be difficult to separate from prior art.

They have no development partners, although teaming with a silicon PV manufacture may be worth considering, should development continue.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Problems are well understood.

Well-organized and logical approach to meeting goals.

Good plans based on barriers and efficiency results encountered. They are considering other oxides and even the use of other "handles" to make a module. There are decision points based on results.

The future development plans are reasonable, but continue the focus on the printed ceramic support substrate. As mentioned previously, it may be appropriate to consider alternate support materials.

Project Strengths:
Project is oriented toward overcoming barriers of thin cells and a ceramic substrate.

Capitalizes on established Si technology.

Si TF - A potential easy replacement of existing wafer process with high expectations for stability and efficiency that are intrinsic (and established) in this material.

The program has demonstrated 4um per min deposition rate of Si homoepitaxy with acceptable material properties. The approach may offer material utilization, energy and cost advantages, if successfully developed

Project Weaknesses:
Difficult to assess whether the manufacturing costs can be reduced while achieving a reasonable efficiency.

None identified unless complexity of entire process proves limiting.

Impurities from oxide seem to determine performance.
Determine or estimate expected throughput (MW/y) and correlate to expected cost per peak watt using SAM or equivalent software.
Re-use of wafer - It is unknown how many times the template can be re-used.
At present, the low short circuit current densities and efficiencies would not allow competition with wafer-based or TF PV.

Cost and efficiency of epitaxy - 50% gas utilization
Ceramic volume change upon solidification leading to strains in Si - limits ability to incorporate BSR which is required for efficient operation.
Ceramic material has low thermal conductivity, which may increase operating temperature
Yield issues of separation/lift-off could be challenging
Substrate repolish/reuse will add to cost of the method... not addressed

Recommendations for changes to the Project Scope:
Focus on cell improvements.

Identify the reasons for bowing. Study effect of thermal history and CTE. Address barriers with things such as mixed and nanoparticle oxides and processing history. Determine process speed in MW/y as well as yield and breakage rates. Need to confirm efficiency values at NREL or certified lab and determine long wavelength quantum efficiency. A rear reflector should be used.
Need to establish how many times the wafer can be re-used.
Consider alternate Si film support materials.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT006
Presentation Title: "High Efficiency, Low-Cost, Multijunction Solar Cells Based on Epitaxial Liftoff and Wafer Bonding"
Investigator: Tatavarti, Rao

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Has the potential to reduce cell costs.

Very impressive solar cell results. Makes use of established III-V technologies and wafer bonding.

Major question will be the production costs.

Epitaxial release for small III-V based concentrator solar cells could reduce the cost of the cells and help to meet DOE and SETP (PV program) goals. There is a potential re-use of substrate and good materials utilization of low availability elements. Reliability is a key goal of this project and an essential aspect of SETP program goals.

The ELO and Wafer bonding technology proposed offers the potential for very high efficiency multijunction devices in addition to potential material utilization improvements. Both of these are aligned with EERE goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The work is very focused on barriers and is technically feasible.

Limitations identified and a logical approach developed to overcome obstacles.

Well-designed and feasible R&D path based on prior devices for solar cells and LEDs. Technical plan is well-divided and barriers are addressed sufficiently. Commercially viable and based on

LED and prior III-V solar cell work. Approach uses a rational examination of surface roughness and utilizes high res. imaging as characterization. There is an emphasis on triple junction device design prior to fabrication part of the study.

The proposed research effort is well structured with a logical progression. Additional activities may be considered to assess the electrical resistivity of the wafer bonded interface, given that these devices will operate under solar concentration. In addition, assessment of thermal stability of the bonded interface will need to be addressed, but perhaps in subsequent efforts.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Goals have been met. Project has just begun. Related work indicates that the goals are achievable.

Results to date show very good progress towards goals.

Some hardware and test devices have been fabricated and delivered. Characterization via AFM is a good start and a logical measurement. Imaging of S-activated wafers is a useful check. Wafer bonding is an emphasis. A systematic approach is in evidence.

The program has only just begun, but much of the required preparatory work has already been performed, thus this program would be expected to move quickly thru its initial development milestones.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
University participation on wafer bonding is excellent.

Presume that cooperation with UCLA has real value.

Not completely or clearly presented. UCLA is doing some wafer bonding and analysis. No patents or publications are in evidence. The study is at the beginning/initial stages.

The program includes an expert in wafer bonding (Goorsky - UCLA). Should the program be successful, it will be critical to pull CPV module manufacturers into the development effort.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
The problems are well understood.

Convincing grasp of potentials and possible obstacles.

Goals for next steps and performance reasonable but very incremental. Only minor thought has gone into mitigation strategies for potential barriers and risk mitigation.

The program plans are well constructed.

Project Strengths:
The material utilization - and thus costs - is excellent. Establishing the manufacturing processes and producing a high efficiency cell will be critical.

Basic ideas have been demonstrated and confirmed with NREL measurements with devices from other groups. The work parallels known LED manufacturing structures and suggests confidence in the approach. If successful, it could allow the avoidance of Ge substrate. This would allow a better (lower Eg) cell at the bottom of the triple junction stack, thereby potentially improving efficiencies compared to state-of-the-art. There is a good emphasis on basic properties like surface roughness.

Project builds off of reasonably well developed ELO technology.
Approach offers "low-cost" option for wafer bonded multijunction cell.

Project Weaknesses:
Difficult to estimate the overall cost of the finished high efficiency cell. However there is industry work with LEDs that indicates possibilities.

Project just started in March 2010.

Concern about electrical resistance and stability of wafer bonded interface at high current

densities should be addressed as soon as reasonable.
Wafer bonding has been used for small area devices (LEDs), but use for larger area may reduce yield. This was not addressed in the program plan.
Wafer bonding occurs prior to ELO, which brings in different challenges/risks and eliminates much of the ELO technology heritage that this program relies upon.

Recommendations for changes to the Project Scope:
Project just started - no recommendation for changes.

Differentiate this work from that already done in literature and by other projects funded by federal government. Large area is new but other aspects need to be reported and differentiated in reporting.

Review: EERE 2010 Solar Program Review

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT007_
Presentation Title: INNOVATIVE MANUFACTURING OF DYE SENSITIZED SOLAR CELLS
Investigator: Bucca, Daniel

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Difficult to assess if in fact this will result in a cheaper cell with a reasonable efficiency. Also difficult to assess the cost of the process itself.

Potential applications beyond dye sensitized solar cells. For DSSC does potential efficiency have commercial application ?

This may not align with SETP since it does not directly address major limitation for DSSC (DSC) PV, which is stability. TiO2 film in the technology is not limiting, so this does not address a known issue with the technology. The approach does, however, offer a potential pathway to lower cost transparent conductive oxides (TCOs) that are a cross-cutting concept necessary for all thin film PV.

The program is focused on the development of a novel deposition technique for dye sensitized solar cells. Successful development in this program does not assure commercial success of these devices and significant additional development is required. Nonetheless, this project is aligned with the EERE goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Might be more applicable to displays or other products. Some progress on technical barriers - not sure that all the barriers are identified.

Is the relevant solar cell capability available to this project ?

The claim is that manufacturing cost and efficiency are improved. This is not the key approach to overcoming the barrier in this PV technology. Based on issues they are addressing, the R&D is good for large area DSSC and perhaps TCOs.

The proposed flame deposition technology is novel.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Has not reached goals. Insufficient data presented to accurately judge this program.

Efficiency presently meeting goals but question that improvement can be maintained?
Baseline is in line with state of art for this DSC PV. AFM studies provide a good mechanism for useful feedback. Cells contain some shunt and series resistance losses.

The program has demonstrated moderate efficiency dye sensitized devices, but the PI and staff may lack the expertise to assess the interrelationship between the device performance and the deposited material.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Lacks collaboration.

Seem to be little concerned to integrate film production with solar cell production.

Not clearly presented. There is a potential for new equipment IP.

The project really needs to include a research organization with expertise in dye sensitized solar cells. It will be virtually impossible for the deposited material to be fully characterized and optimized for this application without this assistance.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Again lack of data prohibits a fair analysis.

As above - must achieve better integration with the cell and module production.

It is not clearly presented what is next compared to what's already known. It is not clear how this will impact the limitation of DSC to grid-tied PV.

Future plans were not included in the presentation

Project Strengths:
Difficult to assess.

Film growth

Can use the technique to make AZO and TCO that can be used for other PV techs. There is a good understanding of the necessary sheet resistance for TCO.
SEM & imaging work is a plus.

Novel method of flame decomposition deposition for forming TiO and AZO films in large area. The films are intended for manufacturing dye sensitized solar cells.

Project Weaknesses:
Lack of presented data. If this is a proprietary problem, I suggest that a more restricted review would be better.

Integration with cell production.

Ultimate limitations on DSSC cells.

They are making larger area DSC devices of similar efficiency than is already in the literature.
The stability is likely unchanged. There is nothing new here under the sun.
Slow deposition rates on order of one hour may limit economic viability.

Device performance is current limited, but PI and staff appear to have limited ability to address this fundamental performance limitation. Project does not address stability of the dye sensitized cells. Deposition rate seems to be very low.

Recommendations for changes to the Project Scope:

Cannot assess.

Much better integration with solar cell production.

Change focus to allow the TiO2 film to accept quasi-solid state hole conductor to make the DSSC more reliable than liquid based junctions. The DSC field has already been moving in this direction with reasonable results. Need to estimate process speed in terms of TCO and DSSC (MW/year) to determine if rates and throughput are reasonable. Use SAM model or equivalent. Establish whether the TCO would be reasonable compared to needs of other PV techs. such as CdTe, CIGS and TF Si. Report on transmission and sheet resistance compared to other TCO deposition techniques.

They would be well-advised to team with an experienced dye sensitized PV developer, thereby allowing them to focus on their core competency of flame synthesis and layer deposition.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT008
Presentation Title: ATIR Optics for Solar
Investigator: Schultz, David

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This doesn't appear to give significant cost advantages over competing technologies.

Very broadly applicable technology but questions about performance with respect to other technologies.

The project focuses on costs, but does not yet produce a convincing argument about its expected LCOE when tracker costs are included at the tight acceptance angles observed. It is somewhat aligned with SETP goals.

Optical concentrators offer improved performance and reduced PV materials usage at the expense of increased complexity (tracking) and use of only direct beam solar insolation. Overall this program is aligned with EERE goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Reliability is a critical issue that is not being addressed. Commercial barriers are being addressed.

Is it worth tracking with this optical efficiency and degree of concentration ?

Many of the reliability and performance barriers are being addressed or are in process. There is a focus on long term performance testing. The barrier of tracking accuracy is not being addressed. Most of the 2-4 degree trackers are quite costly per unit area and are built around concentration factors over 100. This is a critical aspect missing from the analysis.

This program is really more of a product development effort than an early stage R&D effort. Nevertheless, for a technology in this state of maturity the approach is appropriate.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Insufficient data to support the proposed cost reductions and reliability.
First results show that the approach is workable.

Certainly the optical and electrical efficiency results to date are encouraging, but not significantly different than state of the art concentrators or flat plate systems. The project leaders are aware that the next steps involve reliability, measurement standards and longevity. There are concrete plans to test these aspects using known and accepted protocols.

Prototype modules have been assembled and characterized and are being prepared for testing by outside organizations. Performance is as predicted.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Partners with both university and industry.

Very strong cooperating partners

There is strong evidence that the project leaders are in communication with government, institutional, industry partners and stakeholders. The results of this communication and collaboration were not made clear. The project is in its early stages but one paper is being planned.

The company has assembled a good team of collaborators, which given the maturity of the technology is appropriate.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
More work is necessary to justify investing in this program.

Well aware of the steps that must be taken to reach goals.

The project leaders are aware that the next steps involve field testing reliability, measurement standards and longevity. This is a reasonable approach to future work. There are concrete plans to test these aspects using known and accepted procedures. If the project focuses on cost per unit area, then this must be addressed with models such as DOE's SAM, and/or experimental or tabulated cost results. This aspect was not clearly emphasized.

This is a rather mature product. Future development efforts are focused on reliability and manufacturability rather than concentrator performance improvements.

Project Strengths:
Will have a product for reliability testing and intends to scale up to commercial production within 2 years.

The approach would fit somewhat well into known value and supply chains for Si and other materials. Emphasis is on reliability and field testing in next stages. Good consideration of Cap Ex costs of overall module. A uniform beam (output) is expected.

This program has prototypes out in testing at customer sites. Reasonable optical performance (84%)

Optical elements should be easily produced, injection molded

Project Weaknesses:
Once again insufficient data was presented. Not sure if goals will be met on schedule.

Need to track is seen as major obstacle to deployment.

5X concentration for 2 to 4 degrees acceptance angle is far from the realized and ideal limits for non-imaging solar concentrators using TIR. A system with n=1.5 and C = 5 would yield an acceptance of 17.5 deg. C=28x for an acceptance of 3 deg. This would mean that other optical designs with similar height to aperture ratios would outcompete the system. One must also account for costs due to the fact that known cell manufacturers would have to supply cells with more detailed contact grid structures to avoid series resistance losses. The cell must be bonded to the optic and this may result in reliability problems at elevated temperature and irradiance levels. Unknown overall system costs. The module might look inexpensive, but the LCOE might not be competitive. Mediocre overall efficiencies to date.

Recommendations for changes to the Project Scope:

Compare this project's optic to literature designs and literature data for similar concentrators with similar optical parameters. Search for work by R. Winston, and also H. Ries and Ralf Leutz. It would be recommended to compare the project's optic to non-imaging and imaging designs: 1) at the same concentration ratio (5x) and 2) same acceptance angle (2-4 degrees). If the project focuses on cost per unit area, then this must be addressed with models such as DOE's SAM, and/or experimental or tabulated cost results. This should be expanded to estimate LCOE including the cost of a tracker with the necessary acceptance angle.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT009_
Presentation Title: Improved Solar Cell efficiency through the use of an additive optical downshifter
Investigator: Kurtin, Juanita

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Somewhat difficult to evaluate if the result will be cost effective. A beginning has been made on addressing the technical issues but the cost issue will have to be addressed eventually.

Ability to adopt for various cell technologies very important.

This is a modern implementation of an old idea - fluorescent concentrator/down shifter. If successful, it is an easy-to-implement idea that can improve the blue and UV response of several types of solar cells. It is thus long term, but well-aligned with SETP goals.

The technology under development (optical down shifter) may enable higher efficiency for thin film devices, thus is aligned with EERE program goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Focus on luminescent material.

Well-designed research program.

Project leaders understand all the major challenges in this approach and seem to have a new and promising material that has the desired properties that prior models have predicted are necessary for success. There is logical R&D path that focuses on quantum yield for luminescence and the film coating aspects. There is a good mix of basic/materials and device measurements. Thus far, the project has focused on QE for the device and not the final AM1.5 efficiency. The effect might be so small as to make the overall economics for cost reduction unappealing.

The program is well constructed and addresses the critical development items for this technology.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good progress towards project goals.

Stokes shift of 75 nm (wavelength) and luminescent quant yield 85-95%. Other properties for the fluorescent materials in cover sheets are favorable. These include QE for the overall device. The prediction is validated.

The program has demonstrated very encouraging initial results. Questions remain regarding ultimate performance, stability and cost effectiveness.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Working with foundry and module manufacturer.

Good range of appropriate partners and collaborators.

Collaborations are adequately addressed in terms of role and results. Partner supplies modules/cells. Collaborators at Berkeley Molecular Foundry. There is a good mix between industry and university. There is some discussion at this early state of IP and publications.

Have informal partnership arrangements with module and luminescent material developers.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Goal of a 1% gain.

Clear logical plan of research.

Incremental advancements planned but are reasonable based on established baseline. All measurement capabilities have been established with the exception of AM1.5 solar conversion efficiency for the overall system.

Proposed development plan is reasonable but does not address alternative formation methods/materials that can address the economic viability question.

Project Strengths:
Good focus on understanding film quality.

General application of approach to various solar cells.

Approach has been tested over the years and has shown some promise. Theoretical studies done by this group indicates higher QE in blue/UV.
QE shows 0.3% + boost. Focus is on reliability with in-house equipment.

99% PLQY in downshifter dye in liquid form. In solid film form. PLYQ reduces to 85+%. predict ~1% efficiency boost, have observed ~0.4% in CIGS.... These are all very good results.

Project Weaknesses:
Need to establish if this is cost efficient and reliable.

Approach is not new. Success depends on whether the small increases are justified in terms of overall economics. Stability and efficiency results encouraging but not sufficient for favorable LCOE. It is unclear if Si, CdTe or CIGS would yield the best overall match to material.
Due to the review format, the composition of the key fluorescent material could not be disclosed or commented upon. This could cause unexpected outcomes later.

Cost of the precursor material is currently 10x higher than needed for economic viability. It is difficult to see that scale-up necessarily provides this level of cost advantage.

Recommendations for changes to the Project Scope:
Project is only 30% complete.

Focus on overall AM1.5 solar conversion efficiency and determine if this is in line with QE measurements. Have NREL validate some of the measurements. Continue with stability tests for times approaching 10k hours (accelerated tests as needed). Need to use SAM model and have discussions with DOE to determine the potential cost/m^2 and if this translates to favorable $/Wp advantages compared with baseline devices. DOE and investigators also need to consider materials availability issues for inorganic quantum textured downshifter.

Review: EERE 2010 Solar Program Review

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT010
Presentation Title: ZnMgO by APCVD Enabling High-Performance Mid-Bandgap CIGS on Polyimide Modules
Investigator: Woods, Lawrence

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Roll-to-roll process with different materials will support DOE objectives.

Reel-to-reel and potential efficiency of CIGS are both key to DOE objectives.

Addresses use of Cd in CdS in CIGS devices. The material and substrate might be useful for other thin film PV as well. This aligns the project well with SETP program goals.

The program is aimed at improved CIGS efficiency and environmental impact, which are both aligned with EERE goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Barriers are well understood. Program is ambitious.

Excellent controls and recognition of progress limiting effects.

Logical goals based on bandgap of CIGS absorber. Project is targeted for R2R and to look at the replacement of CdS by ZnMgO (window and buffer layer). A buried homojunction is expected. Multivariable exps. are utilized with a large variable space. Some of the barriers may involve oxide growth and chemistry at the interface under temperature/irradiation.

The program looks to use ZnO and MgZnO as replacements for CdS in midgap CIGS materials. The plan also included development in both stationary and roll to roll deposition equipment.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Progress has been made toward objectives.

Good progress to date and every indication that this will continue towards project goals.
A 7-8 % efficiency from 5% a few months ago is encouraging. Good use of control exps. with CdS only. Good emphasis on depth profiling and also on band diagram. The investigators consider Na incorporation but do not yet have a full knowledge of best species and distribution. Oxide migration and formation of CuO, InO and GaO unknown.

The development has not resulted in tremendous improvement in CIGS device performance. In addition, the results from the stationary chamber contradict the results from the roll-to-roll deposition system. Given the complexity of CIGS formation, this difference probably shouldn't be surprising, but it does bring into question the value of performing CIGS development on anything but a production tool.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
NREL has provided support.

Sensible cooperation with NREL and others.

No collaborators but NREL has offered assistance and produced significant findings. Project too new for IP or publications.

They have no partners but are fully prepared to transfer any commercially viable developments themselves.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Very focused program.

Systematic and logical approach to knocking off problems.

Study of effects of Na. Continued study of parameter space. Modest efficiency improvements are planned and reasonable. Stability and interface studies will be critical aspects and are not sufficiently emphasized.

The proposed plans are reasonable, but I would suggest eliminating work in the stationary deposition system as indicated in (3)

Project Strengths:
Oriented toward manufacturing environment. Cost sharing is effective for DOE.

Already making the CIGS cells to be coupled to TCO work here. Studying depth profile of oxide layer. Studying ZnMgO by modeling I-V and band diagram.

Good goal to replace CdS layer in CIGS with something more environmentally appropriate, while enabling higher bandgap CIGS.

Project Weaknesses:
Could benefit from university participation focused on metal oxide.

Efficiency still on the low side. Oxide migration and formation of CuO, InO and GaO unknown. Unknown stability of TCO-CIGS system. Interface migration is likely. Modeling to date indicates a complex and non-ideal interface.

Recommendations for changes to the Project Scope:
Stability with oxide in contact to CIGS is very important and needs to be addressed. Need to understand reliability and diffusion based on oxide/CIGS interface. Presence & diffusion of oxygen may cause interface changes over time. Show overall band diagram for whole device and over time.

Focusing all CIGS development efforts on production-like tools seems like a reasonable approach that may save significant limit redevelopment efforts for transferring processed developed in stationary chambers to roll-to-roll coaters.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT011
Presentation Title: "Process Development and Integration Laboratory"
Investigator: Nelson, Brent

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The ability to fundamentally understand all aspects of manufacturing and in particular cell interfaces is critical to achieving DOE objectives.

Excellent planning for system integration.

The fundamental question is whether a government laboratory should be in the business of imitating industrial production facilities. How many kWatts of modules will NREL ever supply to the market ? On the other hand, there is no question that centralized analytical facilities are in everyone's interest.

The EERE MYPP cites an increased focus on R&D for validating new technologies/products, cost reduction and production scale-up challenges. The PDIL is a potentially big step to assist in this direction, because it can provide a stable and repeatable fabrication and characterization platform. It addresses some reliability issues at the cell level, but not necessarily at the module or mini-module level. It fulfills this and NIST goals because it can study interfaces in detail during several process steps. It has the potential to complete its intended use if the right collaborations and projects are brought to it. It is premised on the idea that "if you build it they will come," but the proof has not yet been firmly documented in terms of up-time and utility.

The program is assembling a set of integrated capabilities that should enable new and hopefully enabling research developments.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent capability. Needs more personnel.

Undeniably high quality work conducted by skilled researchers. The fundamental analytical approaches and techniques will prove invaluable. Staffing and equipment for the "production" facilities is much more questionable.

The question is the degree to which industry will respond to procedures, materials etc. developed at NREL.

The approach to R&D is a systems-level integration of deposition, characterization, and cell analysis. It is certainly unique in the world, especially when combined with the measurement and characterization expertise already at NREL (e.g. cell efficiency certification). It is unclear if the plan has enough staff or outreach to convince the community to come use the facilities. This would only come with several proven studies that clearly indicate utility, performance, flexibility and output based on known NREL baseline (e.g. patented?) processes and recipes. The presentation says that IP issues are addressed, but this process should be transparent and better documented up front. Analogies to similar facilities at other federal sites (e.g. NIST) should be utilized and communicated to the PV community.

In reality, no R&D plans were presented. The subject of the presentation was the assembly of a common use laboratory with novel equipment capabilities.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Extra personnel would greatly benefit project. Also any additional necessary equipment would leverage the capability of the facility for many technologies.

NREL has an impressive record in the analytical and characterization area.

Most bays and chambers seem to be completed and the claim is 70% operational. Some PV material systems are not fully on-line. There has been some utilization by OPV researchers so that that can utilize a single standard. Several case studies were presented that show the potential for problem solving activities. Given the time and amount of funds spent so far, the progress has been slow. This is perhaps understandable with the complexity and novelty of the tasks.

The facility assembly process and data integration process is being performed well.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Superb interaction with industry. Provides a range of expertise as well as equipment to address industry barriers.

Numerous research and production groups have benefited from interaction with NREL.

Not presented in enough detail. Several case studies were cited, but there are IP issues that stand in the way of a complete disclosure of capabilities. The potential is there with sufficient staffing and collaborations.

There is evidence of collaborative work beginning to take shape. Efforts will need to be made to increase this level of collaboration and facility utilization.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Capable of multiple projects and support that will be invaluable in solving industry problems. Only possible limitation is in having sufficient personnel to fully utilize the facility.

Clearly a well thought out program. The outstanding question is whether NREL should be in this "manufacturing" activity as opposed to funding industry and academia.

Continuous advances in analytical tools will undeniably be extremely valuable.

This is a phenomenal project integrating both science and engineering that does not have enough vision moving ahead. The plans are for increasing the number of PV materials served and improving the data mining and extraction capabilities. A logical course would be to ready those capabilities that are most needed by industry in the near term. A case study that is free of IP issues should be undertaken, documented and published for each of the following: wafer Si, TF Si, CdTe, CIGS and OPV. This can be made using NREL/NCPV team recipes already in the patent or scientific literature.

The plans for completing the facility are very good.

Project Strengths:

Excellent capability for industry. Versatile design. Must ensure that the facility keeps up with hardware and software development used by industry.

Analytical capability.

World class lab with a one-of-a-kind opportunity to solve problems. A common and standard sample size is used. There is a focus on interfaces and controlled atmospheric conditions for multiple steps in a fab process. Without a clean room one can maintain a controlled atmospheric environment throughout process and characterization. There are collaborations with NREL NCPV experts on the PV materials.

Excellent vision and mission. The tools being developed are state-of-the-art. The group is well organized and well-led.

Project Weaknesses:

Be sure to have adequate equipment to address an array of problems and personnel to fully staff it.

Project's education and outreach aspects, and communication with the PV industry and R&D community. Demonstrations of system utility cannot be easily communicated without fear of IP issues. Question to be answered: Can the lab be tied to a production environment? Methodologies, strengths and weaknesses are not clearly presented for each major PV material system. There are not enough investigators and staffing (0/10) to effectively demonstrate the full capabilities.

Cross contamination is a concern and perhaps will be a limitation in experimental flexibility. Having dedicated staff for each bay seems critical to establishing the tool capability and making collaborations effective.

Recommendations for changes to the Project Scope:
Recommend additional personnel devoted to facility. Have a base budget to maintain SOA equipment and diagnostic tools.

Education of the U.S. PV community (including students) about the PDIL needs to be undertaken to improve its outreach aspects. This can be accomplished via workshops and presentations at the PDIL that include demonstrations of cell fabrication and characterization with a full description of the output data along the way and at each step. Collaborative publications should be one measure of success from this activity. Even though PDIL equipment is largely on-line, industry and potential user input at this stage would foster collaboration and steer the facility's fine tuning towards the most useful directions. The facility represents the state-of-the-art, but it does not seem that the 'key has been turned' and its performance measured, recorded and communicated in general technical terms. Future plans should integrate aspects such as (outside NREL) customer needs, funding, and communication with NCPV teams. Performance parameters should include those used for the particular material or measurement system. For example, for CIGS one can use deposition time, material utilization, efficiency of the devices, reproducibility, speed, etc. For characterization, measurement time might be important but other Process Performance Measures (PMs) should be developed. Each aspect of each bay should have a stated PPM. Comparisons of these PPMs to industry standards/expectations should be made as they are available. Customer/user feedback on the facility should be collected and posted for every use of the equipment. This can be edited so as to protect potential IP. To facilitate and expedite the process, a grant could be made available for user(s) to accomplish the activities and goals stated in this Project Scope section. Publications and workshops/conferences (at the site) should be a priority.

It may be appropriate to coordinate the development of standards with SEMI and their standards activity..... and it sounds like that's already moving forward.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT012
Presentation Title: Measurements and Characterization
Investigator: Sheldon, Peter

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
There is far-reaching application of this capability that is absolutely essential to DOE's goals. There is a world-class, unique capability for measurement of cells and modules. In addition, there are significant diagnostic capabilities and development of new measurement techniques.

Multiple examples of the value of the analytical capabilities to industrial and academic groups.

This is what federal laboratories should be doing.

This NREL NCPV group (and series of capabilities) is essential to NREL, DOE and the goals of SETP. On aspects of efficiency, cost and reliability, there is no substitute. This group is one of the key aspects that can only be performed at national facilities such as these.

This research group has performed excellent work in support of the DOE RD&D, particularly with regards to increasing cell performance and materials characterization.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
As new designs and materials are added, this capability is essential to understand the potential.

Multi-year history of excellence in measurement and analytical developments.

The approach is through cutting-edge science and engineering with the right mix of collaboration and capability transfer. The capabilities are diverse and innovative, but the approach is integrative (e.g. a systems approach) so that challenges and barriers in the PV industry can be understood and solved.

This group has pioneered new characterization techniques and continues to do so. I can't wait to see the new 9 zone solar simulator.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Absolutely critical capability for DOE.

Statements on the value of NREL cooperation could be obtained from numerous companies and academic organizations.

The project leaders presented a diverse array cutting across all PV technologies and approaches, which serves as proof that their approach has led to real world understanding of PV devices and materials. Over 60 milestones were completed, so not all can be detailed. The measurement and characterization activities represent five projects in one presentation. Several examples were presented that provide convincing proof that the group provides essential services to NREL, PDIL and the PV community. There are some Process Performance Measures and productivity measures used to determine performance of teams.

The efforts of this group have supported virtually every important PV technology development. This is a national resource that needs to be supported and protected.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Always has been excellent in providing collaborative work.

Transfer of cell performance techniques and analytical systems to other organizations has been invaluable.

The group displayed a publications list and a contact list that are both extensive and substantial. There were also examples where capabilities had been successfully transferred to industry leaders so that some of the load can be taken off of the NREL team. This establishes U.S. leadership in the science, engineering, and understanding of solar cells.

The group has many examples of collaborative research advances as well as development and transition of advanced characterization techniques.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Essential capability.

The group is clearly aware of future needs and improvements.

A very logical description of future work was presented that makes incremental but essential refinements on the capabilities across all sub-areas of the measurement and characterization team. There is a diverse set of tools and techniques that need to be maintained. They also presented an outline of goals related to the PDIL, another essential activity for the measurement and characterization team.

This group continues to push the development of world-class and world-first characterization and photovoltaic support techniques.

Project Strengths:
This capability is essential to the whole PV community.

Quality of equipment and personal.

Long term expertise, abilities and equipment development that is not found in other labs or in industry under one roof. There is a commitment to publication and collaboration inside and outside NREL. There is a strong commitment to maintaining and protecting the IP of partners & users. They have developed key new techniques and correlated them to other measurements. They have transferred knowledge about equipment to equipment manufacturers.

The group provides world-class characterization support. Their interactions and technique development efforts have resulted in new capabilities and supported technology breakthroughs.

Project Weaknesses:
Lack of funding

None

Lack of staff and funding that is flat, in sharp contrast to the increased need for services caused by increased SETP programs and funding. This is a drain on productivity.

The group needs additional resources to fully utilize PDIL tools as well as reduce wait time for standard PV measurements.

Recommendations for changes to the Project Scope:
Suggest providing a customer survey that will indicate how well the service capacity has been addressed. This is an extremely valuable program for the entire photovoltaic community and should have better funding and staffing. This is a very cost effective program.

Emphasize value of the group so that DOE program managers, the public, and policy makers can understand the essential aspects that the group and its capabilities bring to innovation and development in the U.S. solar industry. Record and collect customer feedback for every use of the group's capability. They need to capture qualitative utility and performance of the sub-labs in the group using performance measures that they create, modify and utilize. This can be done in such a way as to document how users feel about the capabilities while maintaining and protecting IP. What is done with the cell and module PPMs should be replicated in the other four sub-groups.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT013_
Presentation Title: PV Technology Incubator: Round 3
Investigator: Mapes, Marie

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Program provides opportunity for industry to improve products geared toward achieving DOE objectives.

A highly desirable use of federal funds. Directly attacks the areas of PV that will benefit directly from federal dollars. Cost-sharing sufficient to discourage those just looking for the federal trough.

Business plan. Pre-commercialization. Has been on round 1 (finished) 2 (companies we are hearing) and round 3 (just ARRA-funded).
Diverse portfolio.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The projects are well-selected and in general are a good investment of DOE funds.

This project is one step removed from "Approach to performing the R&D."

Good

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The technical achievements will of course vary from project to project, but the overall design has been well done by DOE.

In sum this program does "measure research progress against performance indicators"

Good

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Collaborations have been encouraged. Might try to put even a little more emphasis on that in subsequent programs.

"Collaborations and technology transfer with other institution" - this is the program

Good

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Attention has been paid by DOE to establishing measurable goals.

Every reason to believe the value of this program will be maintained.

Good

Project Strengths:
This is an excellent program and deserves future support.

Good use of federal dollars - I am much in favor of universities and industry actually doing the research and development work. There are areas where government in-house makes sense, e.g. nuclear and some aspects of space, environment etc. However, solar - and specifically PV - should be a straightforward commercial venture, assisted where appropriate by the federal government.

Project Weaknesses:

Recommendations for changes to the Project Scope:
Both DOE and the investigators should study the lessons of the following innovators: Edwin Howard Armstrong, and William Shockley. In both cases, invention was profound but benefit was not effectively realized in a timely fashion. Pairing a brilliant researcher as PI with a brilliant business developer (MBA?) is necessary for DOE to more rapidly reap the benefits of this approach. This model for R&D follows that used by venture capitalists as they fund, set up and sit on the boards of successful U.S. enterprises that often efficiently take work from university professors to the international marketplace. To learn more, type the two names above into http://www.wikipedia.org/

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT017_
Presentation Title: Nanocoax Solar Cells
Investigator: Naughton, Michael

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Potential for low-cost thin film cells.

Potentially the ultimate solar cell using amorphous Si:H.

Novel and innovative Si nanocoax (nanopillar/nanowire) PV architecture that has been successfully implemented to a 10% efficient cell in a short time. It has a good potential to reduce cost of Thin Film Si to a LCOE of below 8 cents/kWh. This shows how nanotechnology can allow new routes to ultra-high PV efficiency at reduced cost and reduced material consumption. It can help maintain the United States' technological edge in PV and clean tech. The approach can improve other thin film PV absorbers besides a-Si. All these aspects are extremely well-aligned with SETP goals. Unfortunately, the IP has been partially transferred to a foreign company due to the involvement of a large venture capital firm. This has potentially damaging implications to DOE's goals.

Approach focuses on nanorod solar cells as a method of reducing costs and improving performance (12 to 15% single junction a-Si).

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent work. Would have liked to see more data.

Very impressive science and technology with potentially large pay-off.

The project emphasizes modeling and simulation, as well as the interplay between experiment and theory. It utilizes known materials and production techniques to a novel geometry based on lessons learned from both the PV and nanotechnology fields.

The proposed geometry is novel, based on polymer nano-pillars covered by metal, pin PV and TCO. Based on sub-wavelength nano-pillar pitch. Reducing thickness of the a-Si reduces the impact of Stabler-Wronski and reduces the amount of Si material required.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Initial work was well done. Technology is promising but is being sent to China.

Very convincing approach with non-government funding in the future.

All tasks are fundamentally met to date. With metallized nanopillars: 8-10% efficiency and with NREL verification. Degradation has been shown to be less than for flat a:Si (TF Si) cells. Replicated substrates with Nano-Imprint Lithography show promise.
Hot electron effects are possible and would allow high efficiencies past those of two junction devices.

Very good results, demonstrating >10% initial performance on Si. Deposition on poly is at ~6%. The devices also demonstrated good voltage.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Some university and national lab collaborators as well as unidentified industry. The problem is that the IP license went to China.

Excellent array of cooperating organizations.

Coordination includes Venture Funding, Boston College, a Swiss partner (IMT/ EPFL), MicroContinuum (Cambridge, Mass.), Argonne National Laboratory, NREL and others. One patent issued, seven patents pending, 11 provisional patent applications pending. Several publications resulted thus far on basic science/optics. The results speak to the ability to collaborate to solve real world challenges involving implementation of an idea.

Organization is teaming with local universities and other organizations to leverage facilities. They have also teamed with national/international organizations for technology support and commercialization development.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
No future other than university research. IP license was sold.

The future appears to be outside federal funding and reporting.

Project terminating due to transfer of some of the IP from university to a Chinese entity. $1.3mm returning to the taxpayers. The loss of high quality innovative work is reminiscent of the transistor work of William Shockley. In this and the present Solasta case, the innovator is a good scientist but poor business decisions are made. The future is unclear for this work.

Proposed pathway for use of tandem junction PV in a nanopillar appears to be potentially problematic.

Project Strengths:
Excellent work was done on a promising technology.

The intrinsic strength of having the vertical nano-structure for PV operation.

10% NREL verified efficiency.
10% initial efficiency with single junction a-Si pin PV in under a year.
Optical modeling.
Passivation of interface recombination.

The project has demonstrated very encouraging single junction results.

Project Weaknesses:
Work sold to China. Would recommend DOE include legal requirement to prohibit this sort of thing happening in the future.

Transfer of some of the federally-funded IP from a university to a Chinese entity. Poor business decisions leading to potential loss of innovation and technical leadership in the U.S. PV industry.

Recommendations for changes to the Project Scope:
Include legal language in projects to prohibit tech transfer abroad.

Question: How do you make the initial nanopillar now? - Making them by drilling Si wafer, one gets 10% efficiency. Doing it by polymer replication, 6%. Need to focus on the lower cost process for making nanopillars and improve bring the polymer replication process up to the performance of other approaches.

Target for efficiency is 12% is achievable. Stability is still an issue that needs to be taken farther out to 10k hours and IEC protocols. Ready for initial field testing of small devices and modules. The project is an especially good candidate for what Secretary Chu announced on Mar 29, 2010: A $37.5 million joint U.S.-Chinese clean energy research project. This may mitigate the potential loss of the technology. In addition the lessons from this project must be learned and applied to other SETP projects so that IP is not lost to the U.S. for federally-funded projects. It is recommended that a panel be assembled that can investigate this situation and come up with recommendations for better practices. In addition, the other nanopillar and nanowire based PV projects should be carefully examined, nurtured, and guarded as a sub-portfolio of the SETP. There are still opportunities for recovery of this situation and approach. Both DOE and the investigators should study the lessons of the following innovators: Edwin Howard Armstrong, and William Shockley. In both cases, invention was profound but benefit was not effectively realized in a timely fashion. Pairing a brilliant researcher as PI with a brilliant business developer is necessary for DOE to more rapidly reap the benefits of this approach. This model for R&D follows that used by venture capitalists as they fund, set up, and sit on the boards of successful U.S. enterprises that often efficiently take work from university professors to the international marketplace.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT018_
Presentation Title: The Self Aligned Cell: Scaling up manufacture of a cost effective cell architecture for multicrystalline silicon photovoltaics
Investigator: Gabor, Andrew

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Lack of details on processes makes it difficult to assess the value of this work to DOE. It's not good enough to say this was reviewed by others and expect this panel to make an analysis on insufficient data or take someone else's word for it, sight unseen.

An incremental approach based on multi-crystalline wafers or Edge Defined Growth, etc.

The project seeks to extend the cost reduction history of multi-crystalline Si wafer-based PV. Since this represents more than 80% of the PV market, this is a critical task that is essential to SETP goals (both near- and long-term).

Program addresses texturing problem of polycrystalline Si and the development of optimum front contacts (metallization material, localized diffusion and narrowed grid finger). These technologies are aligned with EERE goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The barriers as presented seem to be addressed. However, once again, there are no details to support the self-assessment.

Systematic and successful approach to advancing the technology.

Reasonable, feasible, innovative and easy to penetrate throughout the U.S. PV industry. The approach to lower mc-Si costs is two-fold: texture the front surface in a controlled way and develop an Ag paste process that can be fired through SiN for metallization. These two branches are both low capital expenditures and are sharply focused on both state-of-the-art industry needs and materials science.

Developed printed/fired seed layer process for formation of front grid. This is followed by plating. Front process development is self-aligned via groves formed in the wafer surface. Development of a low-cost Ag replacement is an appropriate goal for addressing cell cost issues.

Details of the back surface texturing were not presented, thus it is difficult/impossible to assess the viability of their technology approach.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Data is lacking. If one accepts that the technical data support the conclusions then good progress has been made.

Moving along well.

Objectives to date met. Demonstrated desired properties for both branches of R&D. Scaling up to MW level. Demonstrated encapsulated large area multicrystalline cell efficiencies ~18% with both desired properties/aspects.

Improvement to conventional processes in reflectance is slight to date, although they claim their process to be more manufacturable.

High-efficiency (18%) poly cell efficiencies in large area (6") have been demonstrated.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
MIT spinoff. Would benefit from some industry collaboration.
Necessary interaction with some equipment makers.

No funded partners. Some characterization support from MIT & NREL.

Working with potential customers such as wetbench provider. U.S. paste manufacturers are customizing Ag seed-layer pastes.
Patent applications in evidence.

Excellent collaboration with equipment providers thru development and trials of new technology.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
The focus has shifted goals and will be reassessed.

Difficult to assess, as the information given is very limited.

The investigators are equipment developers. They plan to transfer the findings to those that can quickly scale it up. Plans for remainder of this project are reasonable and feasible. This project is likely to be considered for other DOE FOAs.

Future plans focus on validating the results obtained in previous work, i.e., establishing reliability, etc.

Project Strengths:
Good results for patterning and contacting.

Strong emphasis on materials science and process control.

Nicely focused program addressing two important advancements. Work performed appears to be of high quality.

Project Weaknesses:
Would benefit from industry collaboration.

Limited in-house capabilities for rapid scale-up (at present).
Cost comparison and throughput unknown at present.

Recommendations for changes to the Project Scope:
Almost finished, so no recommendations for change.

Use SAM or other software to estimate cost/watt (peak) for this and competitive approaches at 1, 10, 100 MW volumes. Verify stability of textured and fire-through concepts (times>5k hours) with accelerated and field testing. Continue to examine and study relevant interfaces and apply device models to predict expected performance. Potential future supply chain project.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT019_
Presentation Title: "PRODUCTIZATION AND MANUFACTURINGSCALING OF HIGH-EFFICIENCY SOLAR CELL AND MODULE PRODUCTS BASED ON A DISRUPTIVE LOW-COST, MONO-CRYSTALLINE TECHNOLOGY "
Investigator: Fatemi, Homi

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Lacks data to support cost reduction.

Promising non-wafer Si cell with high efficiency. High yield process.

Silicon (Si) technology can be brought down in cost/Wp with these kinds of projects. The goal is a thinner cell with wafer re-use. This lowers one of the highest materials costs for Si PV and thus this project shows a good alignment with SETP goals. It is unclear about ultimate scale-up and throughput.

Proposal addresses both cost and performance goals for the RD&D program thru Si epi film transfer process with substrate reuse. Epitaxial deposition of cell structure avoids subsequent diffusion processes and offers opportunities to improve cell design and performance.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Only general barriers were identified. However, accomplishments were listed.

Effective technology for thin Si cells.

Good emphasis on Cap Ex and cost. Substrate re-use is a key element and necessary if Si PV is to compete with TF PV. Good emphasis and baseline for defect density.

Release of textured samples is remarkable and enables the reduction of subsequent process steps. Plan includes module assembly development.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Both template and cell development progress were confirmed.

Pilot line set up and performing well.

12-15% efficiency cell achieved. Good VOC, JSC, FF and efficiency. Carrier lifetime and materials properties good and support further work. Defect levels lowered.
Epi-released cell and [10] wafer re-use demonstrated. In situ doping a promising step. BSF approach is good. Question: is this Ag or Al? Be clear. Plated solar cell.
Good testing of mechanical properties of the thin cells. Mini-module produced.

Demonstrated up to 6" layers and reuse of template 10 times. Cells of 15% have been demonstrated on 156mm x 156mm cells. Cells have been assembled into modules / mini-modules that appear to be functioning well. Stated pilot line process yields of >95% is very remarkable.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
In-house only.

No direct interactions with other organizations.

Not clearly presented. Not clear how this would transfer to allow rapid, cost-effective PV. No publications or patents indicated.

This is an effort wholly contained within the corporation, thus collaboration is not necessarily required. The company is also providing a significant share of the project funding (~75%), thus collaboration would not be expected.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Highly leveraged project. The barriers are being addressed and progress is being made.

Very promising for future large-scale production.

19% efficiency is within reach. Fifty re-uses of tempate/wafer is a good goal. Ramp to 3 MWp reasonable.

Advancements in cell efficiency (19%) goal and 50x reuse of substrates.

Project Strengths:
Good work reflected through pilot line production. The yield and efficiency is very good. There is significant potential.

Process appears capable to reach limiting efficiency with thin Si.

Has done SIMS to profile impurities.
An eye towards breakage rates for thin wafers.
Surface passivation and recombination is being characterized and watched.
Investigators seem to know all the tricks of the trade for state-of-the-art Si processing.

The effort appears to have addressed all aspects of the mission...cost reduction thru substrate reuse, device performance and module manufacturing. They project $0.70/W when brought into production.

Project Weaknesses:
Would like to see an analysis of costs.

The role of the Pyramid cell is not completely clear. Approach would need efficiencies over 17% and high throughput rates to compete with PV industry in 2015.
Need to have an overview slide for overall process as well as cross section of the cell(s). It is unclear about ultimate scale-up and throughput (MW/y).

Recommendations for changes to the Project Scope:
Might solicit NREL's help to increase cell efficiency.

Need to be clear about ultimate scale-up and throughput (MW/y). Use the SAM or suitable software to estimate the costs/watt (peak) for 1, 20, 100 MW/y volumes. Verify yields and breakage for 50 kW and 1 MW/y volumes (estimate as needed). Obtain UL certification(s) on mini-module and report findings to DOE. Need to have NREL-certified QE and efficiency for cell and mini-module. Need to have an overview slide for overall process as well as cross section of the cell(s). Need to have future reviews of this project contain more technical details.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT020_
Presentation Title: High-Efficiency, Low-Cost Solar Cells Manufactured Using "Silicon Ink" On Thin Crystalline Silicon Wafers
Investigator: Antoniadis, Homer

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Increasing efficiency in a cost-effective way will benefit DOE goals. Hard to quantify the cost benefit at the cell level when you are adding/replacing a process. You have a 1% efficiency improvement in the cell.

Could be applied to most Si crystal cell production lines.

In principle, the establishment of an efficiency increase of 1-2% for Si solar cell technology is within the scope of DOE and SETP objectives. It is not clear from the presentation if this approach can truly result in lower $/Wp or lower LCOE. Goal of 18.5 % efficiency is a good one for Si but perhaps low given the trajectory of Si efficiency.

Program focuses on ink-printed technology for developing selective emitter high efficiency (18%) single and (17%) poly crystalline Si solar cells. This approach offers both cost savings and potentially higher efficiency.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Reliability is the critical issue. Data was not shown to support this, but it was indicated some of that has been accomplished.

Demonstrated ability to rapidly achieve project goals with well-controlled techniques.

The claim is that the barriers were addressed and goals for phase 1 were met, but certified or established efficiency measurements were not presented and SEM and structural analysis was not presented. Details about the capped Si nanoparticles were omitted due to IP concerns. It is thus difficult to review and determine if the claims are substantiated.

Ink-printed approach to developing highly doped regions offers options for new device designs (printed all back contact Si PV) that offer higher potential efficiency.

Deposition has been moved to screen printing in order to facilitate introduction of this technology to existing fabs. The company did develop ink jet printing, but its throughput is significantly lower.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Lack of data to support the accomplishments.

Meeting program goals.

Cell quantum efficiency was presented and looks promising. Stability of the blue response was not presented. Y axis on solar conversion efficiency histogram was based on 100 cells. This is significant. Chevron testing is cited for outdoor tests but is not as valid as UL, IEC and ASTM international protocols for PV module testing.

The program has been making good technical progress, meeting all efficiency gates. Development has been accomplished on a 10 MW pilot line. The pilot line is used for customer technology transfer.

Baseline cell has demonstrated superior UV spectral response and nearly 19% efficiency large area cells. Cell reliability testing has been initiated as well as module testing.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
No partners but some support from NREL. Most work done by Innovalight. Working now with some customers.

Basically only NREL for analysis etc. However will be cooperating with customers in the future.

Collaborations only with NREL. Mostly communication with potential customers for the Si "ink." Patents have been mentioned.

The research organization is providing a significant cost share, thus coordination/collaboration may not be appropriate. The business plan is to license their process technology to existing silicon solar cell manufacturers. It will be necessary to partner with such manufacturers in the very near future.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Insufficient data. Lack of detailed plans.

Future steps well thought out and organized.

Continued approach to improve efficiency further for their customer's cells. It is difficult to offer suggestions for improvements of their material because so little about it was disclosed. Format for the review was not appropriate for this project.

Plans for applying advanced developments to back surface improvements. In addition, reliability testing of cells and modules is continuing.

Project Strengths:
Increases efficiency with minor adjustments of the production line. Simple process for application. It is a project with a 64% cost share so it is clear there is a commitment.

Applicability to numerous Si cell manufacturing systems.

Printed silicon sounds good. Good that QE was done and shows better UV resp. Cell efficiencies are reasonable when compared to PV industry products.

The project appears to offer a low-cost approach to higher efficiency solar cells. The business plan is to license the materials and processes to existing solar cell manufacturers. If successful, this should impact a significant amount of existing solar cell manufacturing infrastructure.

Project Weaknesses:
Needs reliability data. Cost-benefit analysis would be useful. Without a market analysis, unsure of whether this would be readily accepted by industry.

Printed Si on wafers does not necessary address DOE goals. The project presentation (slides) presented more claims than sound materials science and engineering.

Recommendations for changes to the Project Scope:
Not enough detail to assess.

The slides seem to have been mixed up with those more appropriate to a presentation to a marketing or venture capital group and contained very little back-up or scientific content to verify claims. Thus, it was difficult to offer constructive suggestions. Questions: Do the printed Si materials contain SiO2? Are they dense or porous? Are they stable?
Was the mechanism modeled or verified by measurements? Were the results verified by NREL or were they obtained with known UL, IEC and ASTM international standards for PV cell and module testing? Suggest the use of software such as DOE's SAM to verify $/Wp or lower LCOE. Suggest modeling using PD1D to model expected results to compare with those experimentally obtained. An SEM x-section and other imaging data is needed (top and side view) would be a requirement.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT021
Presentation Title: "2010 DOE Solar Program Merit/Peer Review High-Efficiency Bi-Facial Concentrator Solar Cells"
Investigator: Wojtczuk, Steven

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
A cost analysis would be useful to determine if bifacial growth is less costly than other methods.

Will be a significant contribution to concentrated PV.

Increasing multi-junction efficiency and lowering cost of CPV cells is within the goals of the SETP. Growing the cell on the front and back of the GaAs wafer would alleviate the need for Ge (which is a materials availability issue). This is also relevant to Program goals.

The proposed technology offers an alternate pathway for producing SOA MJ PV devices. It is hard to see the economic advantage of this approach... the amount of epi is the same, but split into two epi growths. Having two epi growths should increase costs and reduce throughput.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent work on producing close to record efficiency.

Very skilled device production driven by analysis.

They are using a process close to the standard process for III-V solar cells. It is innovative to deposit on both sides of GaAs wafer. Doping and cell design studies are reasonable and logical.

The approach is incremental rather than revolutionary. The inclusion of a substrate in the eventual device may or may not be an advantage. The competing technology (IMM) has the same efficiency, but may have the advantages of substrate reuse (reduced cost) and better thermal coupling (reduced operating temperature leading to higher cell efficiency)

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Achieved cell efficiency. Testing is being provided by industry partners.

Bordering on record efficiencies for multi-junction III-V cells.

They illuminated the device through the GaAs wafer and obtained a 41% + NREL-confirmed efficiency. This is a significant achievement. Presentation slides contain too much detail (without enough text/presentation) for full assessment of all details. Milestones were met.

They have demonstrated very good cell performance, which is expected based on the maturity of their materials and their process. They need to develop a high bandgap TJ in order to improve the GaAs Jsc.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Industry partners. Lacks university participation.

Substantial involvement of other manufacturers.

Partners are customers for the devices. Sample cells for evaluation in modules supplied to at least one company supported by DOE funds. Unclear if necessary feedback would be made available to future DOE reviews. Patent and publication situation unclear.

The business plan is to sell cells, thus collaboration with CPV module manufacturers would be appropriate but are not part of the program. Spire has a history of good technology development with little or no commercialization.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Project is essentially complete and has achieved its goals.

Well-designed development program.

Future work would raise the efficiency to 42% and test the reliability of the devices. Continued study of bowing and bandgap tuning is planned.

The program future efforts are focused on improving Jsc to achieve 42%. In addition, the program will focus on cell reliability.

Project Strengths:
Offers a more manufacturable, high-efficiency cell.

Experimental capability

NREL measurements show close proximity to targets/goals. There is a firm understanding of contacts, tunnel junction, doping and device design.

The program has demonstrated very good performance devices.

Project Weaknesses:
Cost is ~$10/cm2.

Unknown reliability, throughput and scale up possibilities. 10 MW/y now.

The cost and performance advantage of this approach compared to IMM with wafer reuse is not clear. Assuming the latter technology is successfully developed, there may not be much of a market for this technology.

Recommendations for changes to the Project Scope:
No change.

Future work should raise the efficiency above 42% and test the reliability of the devices.

Continued study of bowing and bandgap tuning is necessary. SAM or equivalent model should verify that the cost per watt of this approach would be competitive with existing CPV cells.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT022_
Presentation Title: University Product and Processes Development Program
Investigator: Mapes, Marie

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Excellent way to tie university and industry together.

A perfect use of federal dollars. The universities should always be strongly supported in science and engineering.

Overview talk

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Involving universities in industrial problems is a good way to leverage dollars and improve products.

Program does not perform R&D directly. The categories in this review form do not match this type of program and report. However the three year project duration is appropriate for graduate education support.

Good

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
In general the projects were well chosen.

See remarks above.

Consideration should be given to a "totally new" material for PV, much as was done in the 1970's.

Good

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Great way to encourage collaboration.

Excellent idea to midwife university-industry collaboration.

Good

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Keep it up.

Maintain this very valuable program
Good

Project Strengths:
Accomplishes the goals set forth.

Makes best use of university and industry R&D

Good

Project Weaknesses:

Underfunded. Some issues about publications by university personnel.

Recommendations for changes to the Project Scope:

Increase funding. Consider not drawing problems directly from a specific manufacturer but focusing on the problems in a more indirect way that would not limit publication.

Both DOE and the investigators should study the lessons of the following innovators: Edwin Howard Armstrong, and William Shockley. In both cases, invention was profound but benefit was not effectively realized in a timely fashion. Pairing a brilliant researcher as PI with a brilliant business developer (MBA?) is necessary for DOE to more rapidly reap the benefits of this approach. This model for R&D follows that used by venture capitalists as they fund, set up, and sit on the boards of successful U.S. enterprises that often efficiently take work from university professors to the international marketplace.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT023
Presentation Title: "Organic Semiconductor Heterojunction Solar Cells for Efficient, Low Cost, Large Area Scalable Solar Energy Conversion"
Investigator: Grimes, Craig

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Stability and efficiency are both required to support DOE goals. There is good understanding of these goals but it is difficult to assess how well the work will achieve them.

I see no promise for a deployable solar cell, no matter how much research is conducted in this area.

Supports the goals of innovation, low cost, and potentially stable polymer and dye sensitized semi-solid state solar cells. The project is exploratory and it is not yet clear if this dye-sensitized PV approach can lead to stable, grid-connected solar cells that possess similar efficiencies as existing products.

Focus on use of inorganic nanostructures and light capture techniques to increase organic PV device performance and reduce the levelized cost of electricity.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project is very well-designed and the barriers are understood.

All very clever chemistry, but the goal is a deployable PV system.

Use of resonance transfer, solid state organic hole conductors and a focus on device energetics (energy band diagrams) is sharply focused and innovative when taken together with the use of an oriented inorganic electron acceptor (TiO_2). Technical barriers are identified and are systematically being studied with spectroscopy, imaging and tailoring of organic molecules for sensitizers. The approach is feasible as demonstrated by results thus far. There is one barrier that is not being explicitly studied, and that is stability and reliability. Since this is the Achilles' heel of this DSC technology, it is of some concern.

Development of titania nanotubes on flex substrates as a basis for cell fabrication. The aligned nanotubes offer the opportunity to orthagonalize the light absorption and charge transport.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Device performance is still low but a path for improvement has been defined.

Goals are very limited and so progress has little practical importance.

All device performance indicators are favorable with the exception of stability. Technical objectives have been met for some of the goals, with others pending further analysis.

The program has developed very good titania nanotubes. Have demonstrated 4% device efficiency.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Both university and commercial partners.

At the basic chemistry level the coordination with other groups is good.

Technical partner is synthetic organic chemist. This is necessary and critical. Another prior partner has dropped out but new partner has joined. Publication record is exemplary.

Partner with University of Kentucky and a spin out from Honeywell called Photonic Fuels. Kentucky has focused on organic dye material development and coordinated with the cell development efforts at Penn State.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Still very broad in identifying necessary improvements.

"Realization of the proposed technology." Does realization actually make any contribution to DOE goals ?

Future goals are well described, but various pathways to understanding stability and reliability have not been broken down into their component parts.

Plans for completing the remaining 30% of the program are appropriate.

Project Strengths:
Interesting potential for higher efficiency and stability.

An oriented TiO2 with a solid state hole conductor is still the best approach thus far to achieving stable DSSCs (DSC). The investigator has used a variety of theoretical and experimental techniques to demonstrate that the configuration and geometry is feasible.

The devices developed appear to be relatively stable.

Project Weaknesses:
Still broad in your approach.

What appear to be the ultimate limitations of an organic PV device. If indeed an 8% cell is the minimum efficiency that could ever be deployed for utility electricity, it would seem that organic PV will never get there.

The approach shows the same low efficiencies of most OPV and DSC cells. It is yet unclear if the configuration and architecture can indeed lead to more stable DSC compared to those that are state-of-the-art. This needs to be established.

Recommendations for changes to the Project Scope:
Focus on the charge transport issues. Widen to include new dyes.

Describe in some detail the various R&D pathways to understanding stability and reliability in this device. Break them down into their component parts and offer mitigation and research pathways (e.g. stability of organic components, sealing, energetics, etc.). To reap the full benefit of having funded this project, the investigators should be given whatever time is necessary to determine if the configuration and architecture can indeed lead to more stable AM1.5 illuminated DSC compared to those DSCs that are state of the art. A positive outcome for stability would be encouraging. A negative result would be instructive and would provide useful information to assess the viability of applications to future DOE FOAs.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT024_
Presentation Title: Reliability Evaluation of Concentrator Photovoltaic Modules per IEC Qualification Specifications
Investigator: TamizhMani, Mani

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This is for Reliability Evaluation of Concentrator PV Modules per IEC Qual. Specs.

Valuable to the community to shorten qualification time.

Necessary testing for deployment of CPV modules.

The work of Mr. Tamizh-Mani involves testing cost and a turn-around time for IEC-certified CPV module testing. Implementing the necessary equipment and developing the U.S. expertise for an international CPV measurement protocol would increase industry and investor confidence in CPV. This would lead to more outside funding and R&D for deployable CPV systems. This unique project is therefore critical for SETP goals. There is, however, a concern that the results will not be made available to U.S. companies that can take over this testing role. In addition, there is evidence that a foreign company (TUV Rheinland PTL) which is in part running the ASU testing endeavors has a selective advantage compared to other potential U.S. entities. Since this is a project funded by U.S. federal funds, this is of some concern.

Comments are for ASU - TamizhMani
Program is focused on the development of a testing laboratory for certification of CPV components. Cost of CPV certification is 2x that of flat plate PV and turnaround time is 3-4x.

Streamlining in component certification should reduce the cost of developing and deploying CPV systems.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Needs a cross-check for evaluation (e.g. a system already tested by NREL).

Competent approach to setting up testing facility for CPV. Conditions specified by IEC.

The equipment and protocols conform to IEC (international standards) for CPV testing. The work is logically laid out and highly focused on providing useful information to CPV module producers (aka customers). Both outdoor and indoor tests are done and this is essential.

The approach is to develop a lab based upon the IEC testing standards that have been developed. The testing includes eight different stresses and the project has encompassed all of these tests.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Still not fully functional due to lack of funding. The second chamber will reduce the testing time from nine months to three months.

Goal - reduce costs by 65%

Fulfilled Task 1 requirements.

One chamber not bought as DOE funds lacking.

The investigators were unable to present substantive technical results due to confidentiality with partners and the format of the review. Due to confidentiality reasons, the test results were not shown in this presentation. Some temperature cycling curves were shown that show that the capabilities exist.

Completed the outdoor and preliminary indoor testing. Funding flow issues has limited the lab development, thus it's missing one chamber. In spite of that, they have performed a significant number of tests.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Information is being transferred to a German company. This should be avoided with DOE-funded programs.

Not clear whether any cooperation exists except for supply of modules for testing.

Two major U.S. CPV system providers are partners. These partners, in turn, are working on related DOE-funded projects.

The program has established collaborations with two solar CPV companies.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
If this reverts to a university test site then much of the value will not be realized.

Will run out within a few months.

Improving testing time is a reasonable goal, as is full IEC CPV qualification testing. This should be pursued.

The plans are to complete the laboratory. A private German company will utilize the facilities to complete certification.

<u>Project Strengths:</u>
Needed testing capability

A necessary aspect of the DOE program.

Once this project is running fully with complete qualification testing, it can be continued or taken to private U.S. industry. Providing a reasonably priced, fast turn-around time IEC certified CPV module testing facility within the U.S. would increase PV industry confidence in CPV and foster further R&D.

Project Weaknesses:

Expertise is being transferred to a German firm. Would like to see collaboration with U.S. company to do testing.

Due to confidentiality reasons, the full test results and technical aspects were not shown in this presentation such that an assessment of weaknesses is difficult. This in itself is a weakness of the work, in that it cannot be subjected to peer review and the potential improvements that such review can provide.

Presentation didn't include substantial test data due to proprietary concerns. How does this work for future efforts, particularly with regard to student involvement and the needs for peer-reviewed publications?

The use of these facilities and tests by a German company, under agreement with ASU, is troubling.

Recommendations for changes to the Project Scope:

Orient project to address U.S. testing options.

Make sure that the results of this work will be made available to potential U.S. companies that can take over this testing and certification role within the United States. PI Mr. Mani G. Tamizh-Mani handed one reviewer a business card that listed Germany's TUV Rheinland PTL as his employer. If it is found that a foreign company, which is in part running the ASU PV testing endeavors, has a selective access to the results of the federally-funded research from this project, then this should be further investigated and remedied. Question: How does information about the testing equipment and learning gained from the study flow to potential U.S. entities that might like to start a company to do CPV IEC certification? How will work continue into the future at the facility? How is off site and on site work accomplished and differentiated? Can this work be conducted by the PV group at Underwriters Laboratory in San Jose, CA?

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT025_
Presentation Title: Defect Engineering, Cell Processing, and Modeling for High-Performance, Low-Cost Crystalline Si PV
Investigator: Buonassisi, Tonio

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Improving efficiencies would support DOE goals. Need efficiency data to support the premise.

Valuable to significant fraction of present Si cells.

To improve mc-Si and upgraded metallurgical grade Si is a reasonable method to continue to improve the efficiency and reduce the cost of existing PV modules that dominate the market today. Since multi-crystalline silicon material dominates the PV market and can be deployed relatively rapidly, this project is essential to the SETP near- and long-term goals.

The focus of the effort is to improve the material quality of multi-crystalline Si material in order to improve the performance to near that of single crystalline material.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The barriers are well understood and progress has been made in improving lifetimes.

The group is well aware of performance limitations and has convincing approaches to solving them.

Approach is to improve Si material minority carrier lifetime to ~100 μs range and study the role of Fe as impurities and dislocations as intrinsic defects. This is accomplished by a careful control of temperature profile over time and the utilization of lifetime mapping in mc-Si. This is rational, reasonable and was detailed in a block diagram so that it makes a convincing and sequential scheme. The drawback is that the investigators do not have a way to cast mc-Si so that learning can be more effectively returned to immediate follow-up experiments. Progress could be delayed.

The program is focused on advancement of material minority carrier lifetime. Initial work focused on identification of lifetime limiting defects. An iron precipitate has been identified as a primary lifetime limiter.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Need to support lifetime increases with efficiency measurements. Good understanding of defect mechanisms.

Showing good progress towards long-term goals.

Study has shown proof-of-concept based on controlled temperature annealing and P diffusion. Good results also achieved with hydrogen passivation. Lifetimes in 60-100 us time ranges. Efficiency improvements realized in practice from these steps. Very promising approach supported by evidence.

The activity has identified a process to enhance the Fe out diffusion during dopant diffusion into the dopant over layer, thereby removing it from the bulk of the wafer. They have also developed a high temperature anneal that removes >99% of the dislocations. These processes have been applied to wafers into from a variety of positions within the boule. Although it was not presented, these changes resulted in a 1% gain in device performance for the worst performing cell areas. This same process can be applied to poly ingots.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
MIT led with industrial partners. Collaborations with many academic partners as well as 3 national laboratories.

Good interactions with other research and production groups.

Argonne National Labs and several other world-class labs. Many international collaborations. Several substantial industrial PV partners. Development of high-temperature in-situ μ-XRF sample stage can have potential IP and tech. transfer implications that can lead to additional DOE funding for Supply Chain projects and metrology. Several technologies have been transferred to industrial partners. This needs to be further detailed to make sure that U.S. companies benefit. One partner is largely foreign and transnational. Many publications and talks. It is unclear how the results are propagating and if U.S. industry can benefit more than competitors. Patent applications in evidence.

Several collaborations have been established, in particular with foreign research and commercial organizations.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Not enough data on future plans.

Clear ideas of where to go in the future.

Plans are to improve conversion efficiencies by implementing novel device architectures & processing steps. This needs to be further detailed to allow an assessment. It is unclear who would most benefit from the results. There must be a balance between allowing the results to go into open literature and completely suppressing them from other U.S. PV players who could rapidly transfer the knowledge into products.

Future plans are the payoff of the preceding work, namely demonstration of improved device performance.

Project Strengths:
Understanding of defects critical to improving device performance.

Analytical capability and fundamental understanding.

Models utilized to understand impurities and improve device performance
Project has produced useful fundamental understanding of mc-Si processing.

Project Weaknesses:
Need device measurements.

Some technological expertise and insights have been transferred to industrial partners, only some of which may be U.S. interests. The drawback in the work is that the investigators do not have a way to cast mc-Si so that learning can be more effectively returned to immediate follow-up experiments. Progress could be delayed. The PI is a brilliant researcher, but there needs to be a brilliant business manager making sure that taxpayers are reaping the most benefit from the work.

The focus has been on obtaining high material lifetime, but variation of the device performance may be due to grain orientation and its effect on device formation (diffusion rates, contact formation, etc.).

Does the thermal budget of these developed processes limit the cost advantage of poly over single-crystal Si, with the cost advantage being primarily driven by reduced thermal budget?

Waiting until year three to demonstrate improved cell performance seems late and inappropriate.

Recommendations for changes to the Project Scope:

No change.

Feedback from characterization & analysis to additional casting work needs to be improved with on-site furnaces and casting capabilities. The work should move on to PV devices as scheduled. Both DOE and the investigators should study the lessons of the following innovators: Edwin Howard Armstrong, and William Shockley. Here, invention was profound, but benefit was not effectively realized in a timely fashion. Pairing a brilliant researcher as PI with a brilliant business developer is necessary for DOE to more rapidly reap the benefits of this work. This model for R&D follows that used by venture capitalists as they fund, set up, and sit on the boards of successful U.S. enterprises.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT026
Presentation Title: "High-Rate Fabrication of a-Si-Based Thin-Film Solar Cells Using Large Area VHF PECVD Processes"
Investigator: Fan, Qi Hua

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
High-rate deposition will lower cost.

Very valuable approach to the improvement of a-Si:H solar cells.

PECVD is good method for large area Thin Film (TF) Si devices. The findings about stability in this study are surprising and could be disruptive and revolutionary to the PV field. Since such an approach would yield stable, low cost TF PV, this project is well aligned with SETP goals. It is yet unclear if the efficiency is significantly better than current commercial TF Si approaches.

High rate deposition of a-Si solar cells addresses the desired cost reduction and efficiency improvement.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Data supports approach to overcoming barriers.

Difficult to see how the technical could be improved.

Very commercially promising. A logical study based on sound materials science and a firm understanding of the use of the deposition equipment. Unclear if stability results will hold up under IEC or outdoor testing protocols.

Development starts on small, 4x4” deposition system with RTSE, then moves to larger static deposition chamber (3' x 3 3'), and finally to roll-to-roll deposition. The project addresses a-Si, a-

SiGe and nc-Si deposition. The eventual products are either a-Si/a-SiGe or a-Si/nc-Si.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Excellent progress toward goals.

Very satisfactory progress towards long-term goals.

A 10% efficient cell is reported but not confirmed.
There is a good understanding of parameter space for the deposition.
The uniformity goal was reached over reasonable areas such that scale up is feasible. It is unclear how the results can be improved to be competitive.

The program has demonstrated cells with the performance goal (a-Si/a-SiGe). Problems in producing nc-Si cells in roll to roll, thus that approach was dropped.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
University and industry collaboration.

Appears to only be cooperating with a captive production facility.

A company has been spun off. A good collaboration between university and industry. Several patents and publications are in evidence. Unclear if there is ability to rapidly ramp up if results are promising.

The project is linked to Xunlight, a spin out from UT, thus development transfer is well-coordinated.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Project is well devised.

Future activities appear to be well planned.

Transfer and scale-up would produce a useful product. Stability results need to be emphasized as potential barrier at high throughputs in production.

The future plans are focused on both transfer of laboratory developments in addition to additional laboratory developments.

Project Strengths:
Excellent interaction with UT and Xunlight. Able to explore manufacturing environment as a test for in line capabilities.

Higher deposition rates (8 A/s) than currently in the PV industry.
Real time monitoring of the deposition by ellipsometry can improve yield and cost. A good mix of deposition modeling and device characterization.

Project Weaknesses:
No major weaknesses.

Result may not translate to improved cost/Wp.
Perhaps this work cannot easily transfer to R2R (roll-to-roll) process. No confirmed efficiency and quantum efficiency testing or field work. SiGe may have materials availability issues.

The ability to develop a process on 4"x4" deposition chamber and then assume transfer to roll-to-roll is problematic.

Recommendations for changes to the Project Scope:
Might want to explore the high rate deposition at NREL if possible.

Need to firmly establish stabilized efficiency over longer times (approaching 10K hours). Use accelerated testing as needed with and without UV.

Compare and contrast process speeds and efficiency with Oerlikon, Applied Materials and Unisolar PV modules already sold. Use SAM or suitable model to confirm whether the results translate into lower cost/Wp.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT027
Presentation Title: "Development of Rear Contact Technologies for Next Generation High-Efficiency Commercial Silicon Solar Cells"
Investigator: Rohatgi, Ajeet

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Interesting analysis comparing module cost and efficiency to reach the DOE goals of LCOE.

Approach with significant potential to improving high efficiency Si cells.

Project attempts to raise crystalline silicon solar cell efficiencies beyond 20% in a manufacturable way, with good surface passivation and a series of device and process improvements. This is critical for allowing the PV experience curve for Si to continue to effectively compete with TF PV. This project creates basic but critical understanding to meet SETP goals.

The program addresses development of high efficiency (20%) of a low cost module fabrication technology.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Very well-designed program to address technical barriers and transition to a manufacturing environment.

Experimental ability coupled with sound theoretical understanding will guarantee continued progress.

Logical and systematic. Screen printing, BSF, selective emitter and other approaches that can be used alone or in series in implementation.

There is an understanding of the need for negative charge within passivation at the back surface. Two approaches are explored to achieve this in order to mitigate risk. Well thought out and highly organized.

The approach focuses on development of localized back surface contact via a dielectric layer deposited on the rear surface. Development of this dielectric layer is complicated by the need for passivation, which can be degraded by charging of the dielectric layer.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Project is very close to attaining performance goals. Already demonstrated on small area cells.

Meeting project goals.

The approach reduces B-O pairs leading to light induced degradation, L.I.D.
The second task demonstrated that the structure does reduce bowing. A high-quality BSF was produced. The third task was opening the dielectric. Combining what was learned leads to small area Si cells that are 20%. This result was NREL-verified and very encouraging. Since only two other commercial approaches have this performance, this gives the industry new capability and serves to solidify the assertion that this will be the norm in the near future.

A novel SiCN dielectric film has been developed and deposited. The dielectric appears to be comparable to SiN for highly doped material, but poorer at lower doping for n-doped material. For all p-type material, the dielectric was slightly inferior to SiN.

Demonstrated >20% with two different back surface approaches. Modeling efforts support the efficiency improvements. Moving to larger area devices ran into shunt leakage which has been somewhat recovered (19.2%)

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Direct partnership with industry. Full university publication capability and collaboration by commercial company to provide source material for passivation capability.

Interacting with suitable commercial partners.

Several companies are involved, mainly to provide materials and provide some capabilities not at the university. This project represents an ideal university-industry collaboration. Not sure where IP and publications lay.

At this point, the collaborators are all materials manufacturers. Once development has progressed further, it will be important to incorporate cell manufacturers.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Plans are focused on barriers.

Past experience would indicate that future progress is assured.

World-class work based on both science, engineering. Did not clearly identify risks going forward.

Very good results have been demonstrated on small area cells. The future efforts will address the challenges for forming these high efficiency devices on large area cells and to develop manufacturable (i.e., tolerant) processes and strategies.

Project Strengths:
Strong project with excellent ties to industry.

Quality of technical staff and management.

Used SAM to verify goals.
Project addresses surface recombination velocity at the materials science level.
The use of QE and theoretical modeling was exemplary. Established history in the PV field.

The program has a strong fundamental understanding of the technology under development.

Project Weaknesses:
There is a small amount of O in the SiNC that needs to be explored.
There are some problems to transfer knowledge and process to larger wafers but an understanding of basic mechanisms is there to support that. It is not clear what the trade-offs will be on efficiency, cost and throughput.

Recommendations for changes to the Project Scope:
Would not change.

Explore use of SiNC for front and back passivation. Collect statistics on cell efficiency, breakage, process rate and throughput and feed back into cost model to estimate cost/Wp likely at high volume and in comparison to current industry benchmarks.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT028
Presentation Title: University Product and Process Development Support
Investigator: Compaan, A.

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The advantage of reducing both Cd and Te supports DOE objectives. No cost analysis was presented.

Closely coordinated with the most successful solar cell manufacturing operation in the United States.

The project represents an outstanding collaborative experimental approach to CdTe solar modules from the materials utilization, cost, manufacturing processes, and efficiency perspectives. Reducing CdTe thickness is well within the goals of higher materials utilization for TF PV while maintaining efficiency and cost aspects.

Development of APVD CdTe for improved efficiency and lower production costs directly address the RDD&D goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Excellent potential for application to manufacturing.

Systematic and well-reasoned series of tasks.

Very logical R&D plan that is well implemented thus far. It is a good balance between innovation, applied science and engineering (with process monitoring).

Good coordination of device development between the glass manufacturer and device manufacturer. Optimization of glass for reduced CdTe thickness (< 1 um). Incorporation of

RTSE offers opportunities for improved devices and tighter process control. The program includes many separate elements of device development.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Barriers are well understood and excellent progress has been made.

Excellent progress on the program goals. Every reason to believe that future goals will be met.

By any performance measures (including the applicant's), the project has made significant progress toward the ultimate goals. Barriers are understood and ground work will allow a more facile approach to systematically addressing the remaining challenges. Barrier height independent of thickness.

Use of optimized glass has allowed >10% CdTe of 1 um by sputtering. The efficiency increases up to ~14% for 2+ um material. It's not clear that this dramatic reduction in efficiency is worth saving 1 um of CdTe. The thicker absorber layer is beneficial due to the separation of the back surface layer from the junction area. Optical Emission Spectroscopy didn't work out, but the equipment was used for characterizing the deposition volume above the substrate. Use of RTA for heating substrate to 400C for chloride treatment (30min) appears to be operational.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Industry partners and university collaborators are full participants. The University of Michigan and UNLV supported analysis.
Pilkington participated with glass treatment. Calyxo participated in production line issues. Excellent use of partners.

Broad range of suitable cooperating organizations.

Mix of university and industry partners is outstanding, with contributions documented from each participant. A glass company is involved, and other universities. Coordination has produced useful devices with state-of-the-art efficiencies. At the same time, fundamental and applied studies allow the results to be later transferred into a more production-oriented environment. One collaborator is in Talheim GmbH. Several publications have resulted and IP work is ongoing.

Collaborators include a variety of corporate and university collaborators. The primary corporate collaborator is a foreign entity with a U.S. presence.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Tasks are defined and appropriate.

Future well planned out and would anticipate that goals will be met.

Remaining barriers are understood (identified) and the base-line studies form a solid base on which to understand changes in process and conditions that can address the barriers.

The program has many components (glass development, chlororide treatment, etc.), but it appears that the primary obstacle to overcome is back contact development.

Project Strengths:
Excellent project, particularly with the strong correlation to manufacturing processes.

Directly involved with the major solar cell production facility in the USA.

A known PV material system with proven efficiency and cost benefits. Stability tests performed and are encouraging.

Project Weaknesses:
The "control" for the back contact was not clearly presented and compared to existing concepts for this PV material system. Also not presented were the advantages over existing manufacturing approaches as well as expected process speeds related to ultimate LCOE of this approach.

Recommendations for changes to the Project Scope:
Suggest that a cost analysis for the production of the modified cells would be advantageous.

Compare APVD CdTe devices to state-of-the-art devices (literature values) and compare expected LCOE values based on DOE's SAM software or equivalent analysis. Focus on back reflector and understanding back contact.

It seems like additional work should be focused on formation/passivation of the back surface and related contact.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT029
Presentation Title: Development of a Low Cost Insulated Foil Substrate for Cu (InGa)Se2 Photovoltaics
Investigator: Eser, Erten

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Potential for lower cost thin film cells and modules.

Critical research if CIGS is to become a major player in large-scale PV deployment.

An applied science project. Roll-to-Roll (R2R) based substrate for CIGS, but it could be used for TF Si and others. This project aligns well with the goals of SETP and the need for high throughput TF substrates and PV modules.

The development of a low-cost, insulated foil substrate should enable CIGS monolithic module development. This should provide reduced costs.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
It is not clear whether the new dielectric is advantageous.

Well-established abilities in roll-to-roll film and CIGS cell production.

Methodical and logical, from substrate to Mo coating to devices and modules. Go/No Go decisions every year. Selenization of films reasonable. Phase 2 would produce a scribed 10-12 % CIGS module on the flexible substrate.

The project is focused on the development and verification of CIGS module formation on a Dow Corning SS/SBR substrate. The program plan is well-constructed to retire risk at incremental stages.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The barriers are approached in a technically sound way.

Essentially meeting all goals and likely to continue doing so.

Majority of milestones are completed. Efficiency levels are 11-15% and confirmed by NREL. Substrate, Na incorporation and other aspects all completed. It was found that Na is important for higher efficiency but investigator does not really understand how and why. Low shunt resistance and reasonable series resistance exhibited in IV curves. Quantum efficiency shows some non-idea performance in red and IR.

The program has demonstrated >15% cell on this substrate material. The deposition has been moved from static chamber to roll to roll with reasonable uniformity. Many challenges in forming monolithic module on this coated material. The insulating SBR layer is subject to damage.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
One industry partner (Dow Corning).

Appropriate cooperating companies.

Dow Corning (provides silicone +SS substrate that can go to 600 C). No publications in evidence. Not clear who would ramp up if the project was successful and whether the process speeds and performance are competitive.

The project is a collaboration between the University of Delaware and Dow Corning. This is an excellent team for development of the core technology, although inclusion of a commercial CIGS manufacturer would be appropriate as the program matures.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Focused on reducing barriers.

Anticipate continued progress using well-established techniques.

It is not clear that the Na is mobile and what its role is in charge compensation and performance. Without this being folded into the project, the efficiency levels may be reached but not understood. This would mean transfer to production may be confounded. Uniformity of coating across and along the web measured and is encouraging.

The plans going forward are to improve device efficiency. Module formation appears challenging. Given the success other CIGS vendors have achieved in monolithic module formation, it's not clear the advantages of this project.

Project Strengths:
Very promising cells with a relatively low cost production potential.

NREL-confirmed efficiency, IV and QE. Looking to produce a full interconnected PV module with roll-to-roll (R2R), but going about it very carefully and systematically. Careful consideration regarding matching of coefficient of thermal expansion, CTE. Good uniformity of efficiency along length of roll.

Project Weaknesses:
It was not clear to me that the new dielectric is a beneficial element in light of the integration problems.

It is not clear how this approach compares from a cost/Wp and throughput standpoint from what is already commercially available from CIGS companies such as Global Solar. This needs to be clarified. Some roll-off of quantum efficiency at 800-950 nm (this needs to be explored). Still more of an applied research (empirical) project than focused on understanding and basic research.

The case for the economic and/or performance benefit of this insulated foil was not presented.

Recommendations for changes to the Project Scope:
Would suggest that the project would be strengthened with a CIGS manufacturer as a partner.

More fully describe risks to interconnect strategy and describe back-up strategies presented. Focus also on understanding the role of Na and the factors that control effective selenization. Describe how this approach compares from a cost/Wp and throughput standpoint to what is already commercially available from CIGS companies such as Global Solar. Stability tests should be performed ASAP to confirm that the substrate is worth the effort.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT030_
Presentation Title: Routes for Rapid Synthesis of CuGaxIn1-xSe2 Absorbers
Investigator: Anderson, Tim

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Valuable to increasing cell efficiency and useful to the whole CIGS community.

Impressive science and engineering attack on the economic production of MWatt scale CIGS modules.

This is both a basic experimental and theoretical approach to improving the understanding of CIGS PV formation. Capitalization & yield is highest cost for this kind of PV material. There is a need to increase CIGS process throughput without sacrificing quality and yield. Taking an experimental approach to obtaining thermodynamic and kinetic data can lead to rational and easy-to-implement changes in the U.S. PV industry. This is highly needed for this material and is in line with DOE SETP goals.

Fundamental understanding of CIGS formation processes is critical to enabling the production of high performance devices over manufacturing scale processes. By understanding the formation pathways and rate limiting steps, it may be possible to decrease the formation time and improve large area device efficiency.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The barriers are well understood and the effort is sharply focused.

Clear identification of the major barriers and the areas where better fundamental understanding will make key contributions.

Most of the work is based on selenization. This is appropriate based on current trends in industry. They are looking at lower temperatures for similar reasons. They are also going to look at mobilities, rates and higher temperature reactions.

The approach is to use high temperature XRD to explore the constituent phase diagrams. The program does not include Na, which has been shown to be important in device performance.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The goals were approached systematically. Excellent work.

Excellent progress on a very modest budget.

Milestones largely met. A database was created that can be built upon to understand reactions and phases. Some understanding of ink-based CIGS formation has been made. It is not clear how this has directly resulted in efficiency or cost improvements yet.

The program is developing excellent fundamental understanding of constituent component reaction rates. This data, in addition to other inputs, are being used to create general phase diagrams for the constituents.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
ORNL and NIST are full participating partners. Industry partners were the recipients of the work but also provide perspective.

Very productive relationships with other groups.

Collaborations with government labs and major U.S. CIGS PV companies. Transfer of understanding to industry collaborators. Several publications in evidence. One patent involving CIGS inks. It is not clear what cost and benefit is measured for the information to collaborators.

The program includes both national lab collaborators as well as a variety of CIGS manufacturers. It would be great to see NREL CIGS researchers and materials included in the program.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:

Future plans will improve understanding and hopefully provide a path for both higher efficiencies and faster manufacturing.

One of the more solidly science-based projects described at this meeting.

A good fit to literature delta G and H. Plans for expanding the information and using a HT X-ray machine are good. Good practical background to build further understanding.

This program is addressing very complex and important questions. The program is taking ever-increasing steps in complexity.

Project Strengths:

Excellent capacity to work with industry with additional funding. This project is essential to improving the understanding of CIGS formation.

Solid science underpinning.

Thermodynamics and some kinetics for understanding this complex PV system. The investigators considered nanoparticle (ink) effects. The methods in the study could be applied to other promising PV compounds.

Project Weaknesses:

Processes may be kinetically controlled and this needs to be further described/reported. The relationship between equilibrium thermodynamics and non-equilibrium processes need to be described for this material in one easy-to-use model (input parameters: temperature, composition, temperatures, particle size and time). The sensitivity of HT XRD may not be good enough to pick out desired processes for short times. Mobility and diffusivity is also going to be dependent on the host phase and other dopants. Lastly, it is difficult to directly measure the results of the study, since knowledge and expertise are distributed across the whole U.S. CIGS

PV field.

Recommendations for changes to the Project Scope:

Add funds to incorporate more industry collaboration. Good potential to improve process and therefore reduce cost.

Nanoparticles vs. bulk materials should be reported side-by-side to contrast differences and opportunities.

Need to also understand the role of Na and dopants in CIGS cells. Attempt to directly measure & report the results (affect) of the study even given that knowledge and expertise is distributed across collaborators in the U.S. CIGS PV field.

Recommend continued funding

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT031
Presentation Title: High Efficiency Back Contact Si Heterojunction Solar Cells
Investigator: Das, Ujjwal

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Unclear how this would impact existing manufacturers to reduce cost. No cost analysis provided.

Close interaction with major module producer. Very appropriate academic project in direct support of module production.

Takes two very promising commercial Si cell approaches and attempts to combine passivation aspects. Since a 19-20 % Si cell is expected to be the industry standard, this is well-aligned with SETP goals.

Develop back contact Si cell technology compatible with thinner Si wafers. Successful development should address the program goals. Use of thin wafers requires that low temperature processes be developed for passivation, etc.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Barriers are well understood.

Solid attack on key production and performance limits.

Studied stability of charge carrier lifetime. Also utilized light beam induced current, chemical analysis, and a host of state of the art characterizations. All of these are approaches. The goal is mainly focused on voltage, but may neglect trade-offs with current density.

The program is focused on low temperature front and back surface passivation as well as heterojunction formation.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Objectives have been met but device performance is not indicated.

Meeting program goals.

Good results with front and back contact passivation.
Passivation is achieved, but not a good contact/contact resistance.
Surface cleaning was found to be important. This is a solid platform for future work.

An effective front surface passivation, manufacturable stack has been developed and demonstrated on thin Si wafers. Performed optical modeling to maximize transport thru the passivation layer. ZnS appears to have advantages optically but is not fully developed.

Performance of cells limited by non-ohmic p-type contact to SHJ structure. A modified cleaning process has been developed that appears to have solved the problem.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Limited collaboration with partners.

Close coordination with SunPower, which should ensure successful transfer from IEC to SunPower.

Sunpower supplies the wafers; Jim Rand as consultant. Unclear if and how other collaborators are important. Some evidence of patents and publications. It is unclear who will benefit most from the study and how improvements will be measured in the U.S. PV industry as a result of the study.

The effort has as a partner SunPower, the primary industrial source for back contact-type silicon cells. This participation assures developed technology is both aligned with production requirements and will be rapidly incorporated into production.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Difficult to assess where this will go in terms of manufactured cells.

Major areas requiring further progress clearly identified, as are the appropriate research thrusts.

This project is not unlike the work of Georgia Tech. It can be compared and contrasted with that study. The directions in this study are sound, as is the plan for future work.

Focus of future efforts is to improve FF back passivation and contact development.

It would be good to understand the cost advantage of this technology; will the benefit of a thinner wafer offset the additional costs of heterojunction growth and new passivation deposition? That benefit remains unclear. In addition, the commitment of SunPower toward this new process is uncertain.

Project Strengths:
Excellent characterization

Close relationship with SunPower and internal capabilities at IEC.

This product provides fundamental and applied knowledge about Si device configurations that have already shown commercial promise. The progress on voltage output is encouraging.

Project Weaknesses:
Unclear how this work would lower cost for existing manufacturers.

It is unclear how this relates to work that is already state-of-the-art. A side-by-side comparison (as a control device) should be made. The goal is mainly focused on voltage, but may neglect trade-offs with current density and FF. Throughput and process speed are not being considered so that a potential MWp/y can be estimated.

Additional technical interactions with SunPower are advised, specifically with regard to contact development.

Recommendations for changes to the Project Scope:
More in-depth collaboration with SunPower would seem appropriate.

Need to explore effect of doping on Voc and stability of output for thinner cells. Will the fields cause ion mobility?

Extend and continue the surface recombination velocity measurements.
NREL SiN passivation work should be utilized (Richard Arhenkiel, Steve Johnson et al), as found on the NREL publications page. Present a model of the device that can be validated by the experiments and can serve as a basis for understanding the claim of a 20% efficient device.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT032_
Presentation Title: "100 mm Engineered InP-on-Si Laminate Substrates for InP-based Multijunction Solar Cells"
Investigator: Atwater, Harry

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
No cost analysis to verify that this would result in lower COE.

Certainly in-line with DOE goals.

A CPV approach with improved MJ efficiency. There is a potential for 50% efficiency with materials more abundant than current approaches for CPV. This is long term, but well-aligned with SEPT goals.

The plan is focused on developing a very high efficiency device (50% target) for CPV applications. Target of project is use of LC between GaAs and InP to enable the development of four junction "LM" devices. The approach uses exfoliation and attachment to silicon substrate.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Very little device data that would verify the merit of the process.

Long range research with potential high pay-off.

The work is building up systematically from the novel substrate to more complex devices.

The approach focuses on substrate development and AlInAs material and device fabrication. The plan is to use exfoliation process (SMARTCUT) to transfer nearly fully strained epilayers to alternate substrates as subsequent growth templates, but there's a problem. The SMARTCUT process requires the transfer of thick (>3um) films in order to allow for the removal of ion implanted damaged material formed during the SMARTCUT process. It is not possible to grow fully strained epilayers to that thickness for the full alloy range from GaAs to InP. In addition, the epilayer bonding process is made to a SiO-coated Si wafer. This choice presents electrical resistance issues at that interface as well as thermal expansion issues that will crack the overlying III-V device.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
With Spectrolab growing the cells it would seem that better cell efficiencies would be expected. Not sure all the barriers have been identified.

At this very early stage there has been some admirable progress.

An alternative to Ge-based substrates or InGaP metamorphic CPV approach has been identified.
The InGaAsAlP system with an InP/Si laminate that is more lattice matched has been demonstrated.
There is a good understanding of defects.
Temperature stability of InP/SiO2/Si shown to be near 725 C.
The first solar cells produced are over 5% efficiency (not great, but encouraging).
Demonstrated an ion cut and a wax-based process for substrate re-use.

The epi transfer material has a high density of bubble defects that will need to be eliminated

Silicon oxide is incorporated at the substrate interface for bonding purposes. May be problematic for high current transport (CPV application). The use of a silicon substrate introduces thermal expansion mismatch concerns.

InAlAs cell performance is poor... the attribution of poor device performance due to poor windows may only be partially correct. The aluminum content of the demonstrated device is significantly lower than that required for the target 4-junction device, thus the problems in device performance will only get worse.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Spectrolab is the industry partner. No other university participants. Proposed collaboration is outside the United States.

Appropriate cooperation with Spectrolab.

Caltech and Spectolab are involved. Basic information is being shared on materials. Project still new, so publications and IP are still in progress.

The program is partnered with Spectrolab, which has expertise in these materials as well as production of related devices.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Consultation outside the United States.

The project has some very worthwhile long-term benefits.

Incremental and logical from present position.

The development of the InAlAs cell in FY11 does not support the high efficiency device. Proposed InAlAs/InGaAsP/InGaAs device does not address the high efficiency goal.

Project Strengths:
Work is unique in offering alternative lattice constants if the problems can be overcome.

Worthwhile exploratory research.

Re-use of InP substrates. A new kind of lattice relaxed substrate for CPV cells. A firm understanding of the materials science and film formation.

Project Weaknesses:

It is difficult to predict if this will achieve the higher efficiencies. Other methods of providing thin films might prove more advantageous. There is a formidable array of problems to overcome.

New materials and architecture have significant risk as the multi-junction (MJ) is built up. Unclear if there is enough MJ device modeling experience on part of PI or if that knowledge will be sufficiently provided by partner.

Reviewer 18:

Recommendations for changes to the Project Scope:

Would recommend teaming with MicroLink.

Further determine why the quantum efficiency is 50 for the InAlAs solar cell. Determine causes of series resistance and explore the role of contact resistance and doping. Continue with cost analysis to validate promise of the approach for CPV that can be deployed at large scale.

Continue to measure basic electrical properties of the materials. Present MJ device modeling to validate experimental approach a future direction.

The most interesting portion of this effort is the transfer of elastically strained epitaxial layers to alternate substrates, although that concept really requires a chemical approach to film transfer not SmartCut. I would suggest eliminating Si substrates and incorporate a TCE-matched material.

Review: EERE 2010 Solar Program Review
Presentation Number: PVLT033
Presentation Title: "Tunable Narrow Band Gap Absorbers For Ultra High Efficiency Solar Cells"
Investigator: Bedair, S.M.

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
Increasing efficiency supports DOE goals.

Promises sufficient efficiency improvement in CPV to be worthwhile.

The project aligns well with DOE and SETP objectives in terms of improved efficiency and LCOE for concentrator devices and incrementally improves (potentially) the current MJ III-V CPV cells made by the partner.

The goal of reducing the GaAs bandgap in a TJ device should address the goal of increasing cell performance.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Barriers are well understood and addressed.

Impressive experimental developments

The approach has already proven its feasibility. The research is well-designed. Commercial feasibility is not yet determined and has not been adequately presented. Middle cell is an issue.

The approach uses strain-balanced superlattice to incorporate InGaAs lower bandgap absorber. This approach has been looked at extensively and is in commercial production by a firm (QuantaSol) in the United Kingdom as a single junction device.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Well-designed project. Technical progress is steady.

Superlattice developments are a significant contribution.

Project goals to this stage have been achieved and are promising. Barriers to this stage have been adequately addressed.
Has shown that changing bandgap of GaAs based layers does not degrade Voc.
Staggered QW is novel and pushes QE to longer wavelengths.

The addition of strained layer superlattice has resulted in current increase and their open circuit voltage is very encouraging (~0.95V). The staggered well thickness is an interesting addition to the technology development.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Partnered with Spectrolab, which did some cell processing in parallel with NCS. Spectrolab grows the top cell of the tandem.

Closely coordinated with SpectroLab.

It is unclear how the partners are participating in a meaningful way thus far. Transfer of technology is planned. Several publications have been the result of the work. Spectrolab (partner) measures the cells.

Collaboration with Spectrolab is appropriate for both development of technology and technology transfer into production.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Plan is well-devised.

Barriers identified and solutions well thought out.

Next phase research plans are reasonable. A plan for mitigation of risks was not presented. Volume/throughput speeds of the process compared to existing approaches have not been described. Reliability and stability for > 100X AM1.5 illumination for times > 1000 hours should be related to parameters such as thermal and mechanical stress under concentrated light.

The plan incorporates both superlattice developments as well as integration into the full triple-junction cell. The plan should include having Spectrolab produce the full superlattice enhanced solar cell device.

Project Strengths:
Readily transferable to industry.

Based on known materials and processes. Initial proof of concept has been demonstrated.

The program is making very encouraging technical progress.

Project Weaknesses:
Approach provides incremental increase in efficiency.

Unknown analysis and linkages to LCOE and overall system efficiency for CPV. Efficiency and characterization not certified by NREL or other suitable outside lab. More advanced characterization of stoichiometry and device imaging (e.g SEM, TEM and AFM) were not presented. The project may produce an incremental improvement in triple junction efficiency, but it has not yet demonstrated proof-of-concept for the whole stack. It is also unclear if the approach will be high volume/throughput or reliable under conditions approaching those of deployment.

Recommendations for changes to the Project Scope:
No change.

Avoid use of cell tracking numbers in presentations.
Report current density instead of raw current.
Report/compare results in next review period in terms of theoretical efficiency of each device in the stack. Emphasis should be placed on demonstration of proof of concept for the whole tandem stack. Estimate volume/throughput speeds of the process compared to existing approaches. Lastly, it is time to test the reliability and stability for > 100X AM1.5 illumination for times > 1000 hours. Determine parameters related to thermal and mechanical stress. It is recommended software such as DOE's SAM be used to analyze full potential of the systems that can be assembled with the device under study.

In order to ensure the anonymity of reviewer feedback, reviewer comments are listed in random order for each question

Review: EERE 2010 Solar Program Review
Presentation Number: CSP001
Presentation Title: Project Title: Line-Focus Systems Program Team: CSP
Investigator: Kutscher, Chuck

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This project successfully addresses several key DOE objectives, including, per the Multi-Year Program Plan: "…transfer R&D concepts…to the market place…and assist U.S. industry." This project also supports the DOE focus on lowering costs, primarily by providing, for example, optical evaluation/alignment tools, outdoor efficiency tests, field measurements, optical models, and standards. Within the context of DOE and industry interest in trough systems, it therefore rates a score of Outstanding ("Project is critical to EERE and supports objectives").

Focus on parabolic trough collector field cost as it affects LCOE is a good one. Dealing with trying to develop means of assessing the performance of systems by measurement of the optical performance is good, particularly those approaches that involve remote or semi-remote assessments. Impacts of storage on plant performance are questions that need answering. Emphasized the impact on getting more examples in the field and the possible impact this will have on costs. This was also very good.

In relation to the amount of funding allocated to the project, it appears to have a relatively high value to cost ratio. Funds appear to be deployed efficiently.
Very well aligned with DOE program goals.
I believe that most aspects of this project group (It's too diverse to be accurately termed "a project") are indeed critical to the EERE Solar Program and that they all support DOE's RD&D objectives. The breadth and number of the project activities make it difficult to comprehensively evaluate.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project primarily involves refining and using VSHOT TO-GO, LS-2 characterization, FOA support (Alcoa, SolarCAT, and NREL), validation (i.e., uncertainty analysis and planned reference mirror tests), heat loss tests (on-going since 2006), software updating, inert gas/hydrogen tests, outdoor testing of troughs, receiver infrared imaging, distant observer tests of the field (tower, balloon, or aircraft), LANSIR, complete re-write/modernization of the SolTrace (ray trace) code, support to standards development, and completion of a roadmap. Overall, this is an outstanding effort ("Sharply focused on technical barriers; difficult to improve approach significantly").

Importance of the impact of optical losses on LCOE is quite clearly shown. Also concern about thermal losses is quite good. Concern about hydrogen release inside tubes is very important. I presumed that some other presentations would address this work in more detail but by and large didn't. Seems that when the comparisons are made between laboratory heated tubes and field heated tubes, this results in some differences because of circumferential temperature variations on tube would be different due to the absorbed solar flux. Appreciate the comment that the resistance across the vacuum is very large, but there would still become circumferential variation because of the flux absorbed. Cleaning effects should be addressed also. I am very concerned about polymeric-based coatings and their life time, particularly in high flux, high ambient temperature operation.

Concern that evaluations are made in Golden compared to what might be made in a higher flux location.

The project aspects described in the review are uniformly well-designed, with feasible and commercially promising approaches.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
Their Technical Accomplishments and Progress are rated Outstanding ("Excellent progress toward objectives; suggests that barriers will be overcome"). They have partnered with industry

to determine the main areas of R&D, developed and analyzed VSHOT, characterized troughs, completed an uncertainty analysis for VSHOT, conducted IR surveys of the receiver (led to replacement of 20,000 receivers, at a cost of $15M, but with an increase of 15% in the IRR), demonstrated a novel inert gas approach for decreasing heat loss (with hydrogen evolved in the receiver), conducted a clever "ambient" heat loss test by monitoring the temperature time-dependence, developed a new optical test stand (tests planned for 7/2010), measured overall efficiency, developed a load/optical performance tool (LANSIR, approximately 80% complete), re-written SolTrace, have developed and will demonstrate the DO technique at the Cameo facility, developed an acceptance test, and have supported standards development (ASME and SolarPaces). They note that VSHOT is labor intensive; this is a barrier, but is not noted explicitly, but it is recognized. Their bar chart for cost improvements is notable.

Most of the important accomplishments show proper timing. However, this is a little hard to judge in the relatively brief presentation and associated materials.

The review demonstrated excellent progress toward overcoming the targeted barriers with nearly all sub-projects reportedly on schedule.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This effort is Outstanding ("Close, appropriate coordination with other institutions; partners are full participants, publications or presentations"), in that they have outside interactions and work with over a dozen groups (essentially all of the trough companies, both domestic and foreign, as well as subsystem suppliers), have supported these both in laboratory and field tests, supported development of standards (ASME and SolarPaces) developed a collaborative roadmap, conducted alignment in the field that resulted in real performance improvements, and have seven publications. They have also licensed VSHOT to two companies.

Seems to have quite good interactions with industry. Some interaction with Sandia. Nothing mentioned about universities.

There was relatively little reference to experience with new plants in Europe, and one assumes they are a potential source of data and experience that would buttress limited recent US experience.

The NREL group provides truly outstanding collaborations with the industry and others in CSP

RD&D.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This project cites several key milestones achieved or planned for 2011, but it lacks specificity and is scored Good ("Plans build on past progress and generally address overcoming barriers"). The presentation lists near-term and recently completed milestones in the seven major areas (collector, receiver, outdoor, efficiency, etc. testing), but it does not show a detailed plan for future work. It is necessary in DOE plans to address barriers and uncertainties; they do recognize the uncertainty aspects of the VSHOT tool, and this is notable. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) is not shown; however, in general their work supports major DOE goals for troughs, and is well-aligned with industry.

Their ability to conduct work of this nature is unquestioned, and the personnel are exceptionally accomplished and experienced. It is reasonably clear in each of the seven main areas what they plan to do in 2011, and what the milestones will be at a "top level", but they do not explicitly identify barriers or show how they will be resolved, or what the alternative development pathways will be. Although their plan lacks this level of specificity, it at least breaks down the milestones into each of the primary areas, and with their past accomplishments, it is safe to assume that they will achieve these. With these considerations in mind, this area is judged good.

They have outstanding accomplishments, are collaborating well with others, especially industry, have developed clever techniques (optics, thermal, inert gas, etc.), and are clearly staffed with exceptionally talented and experienced personnel. But, the planned future effort, although shown in each area in terms of milestones is clear, the specifics as to how this will be conducted are not addressed.

Seems quite reasonable. I have concerns about potential (and existing) overlap between Sandia and NREL. Seems that both are growing in some similar areas. Included are the optical measurement capabilities. Even if the laboratory developments were a result of ARRA funding, which does not directly impact the DOE CSP budget, it will lead ultimately to drains on the latter in potentially overlapping ways.

Well-presented description of current work.

The project group has effectively planned future work very logically incorporating appropriate decision points, considering critical barriers, and attempting to mitigate risk through alternate development pathways.

Project Strengths:

They have outstanding past performance and achievements in seven key areas, with close collaboration with others, especially industry.
Addressing key aspects of cost impacters. Good analysis of what these are.

The project addresses costs that apply to all applications, i.e. improving collector optical and thermal efficiency has a direct cost reduction impact. Improving efficiency enables higher temperature which in turn reduces storage capacity for a given storage material volume. Testing and characterizing components that may be included in plants, e.g. HTE's offered by new vendors is an important support to an immature market and contributes to increased investor confidence. The project is advancing the state of the art by investigating and validating new, more economically meaningful approaches to characterizing trough efficiency.

Highly qualified staff and (recently) good support-facility funding.

Project Weaknesses:

This project requires a more developed plan, task definition, schedule, budget allotment to a lower level than the total funding, and definition of the barriers and alternative pathways to overcome these barriers.   Given the lack of specificity needed for a peer review of the planned work, this area is judged a minor, but not a major weakness, primarily because the milestones in each of the major areas is listed relatively explicitly.

Some of the details in question: circumferential variations in temperature are not clear. How are problems decided upon it not clear. From the materials furnished, it seems that publications are not as numerous as might be expected from an effort this large. Also, it seems that few to none are in archival journals.

Not a project weakness, but I did not understand the heat loss comparison chart.

There is a question of how well the heat losses from internal heating compare with real on sun losses arising from the direction of delta T and other radiation effects. This is probably small but should needs to be confirmed.

The need to closely support the industry's nearer-term interests dilutes NREL's potential impacts on longer-term developments.

Recommendations for changes to the Project Scope:

More details on what will be done, when, and at some level of budget, labor, etc., is needed. Proposed future work should, at a minimum, be addressed at least to the subtask level for the major milestones, and preferably shown on a Gantt chart. There are no Go/No-Go decision points noted; these should be included. They mention the labor intensive aspects of VSHOT, but do not address how to improve this; if this aspect is resolvable, it should be a part of the project Should do some field work in high flux regions of the southwest in addition to characterizing systems located in that area.

There is a need for this project in particular to develop and disseminate a better understanding of project risk as perceived and monetized by investors and financial analysts. The best cost reduction strategy for troughs is plant design, construction and operation experience, and anything this RD&D project can contribute to providing tools and data allowing project risk assessment to be based on quantitative analysis rather than rumor and perception will mitigate contract failures and delays.

Over time project emphasis should shift to tools and protocols for characterizing field efficiency vs. collector efficiency. If it can be shown that field efficiency is a simple and invariant function of collector efficiency, that would suffice, but if there are factors that result in degradation in field efficiency diagnostic tools should be developed that allow plant owners and operators to ascertain field efficiency in the commissioning process and then monitor variations and trends over the subsequent years of operation.

I see some tendency to embrace some longer-term goals, now that the budget can support such in addition to the direct industry-related work. I believe that is a good direction.

U.S. DEPARTMENT OF ENERGY | Energy Efficiency & Renewable Energy

Review: EERE 2010 Solar Program Review
Presentation Number: CSP002
Presentation Title: Line Focus Systems
Investigator: Moss, Timothy

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This project successfully addresses several key DOE objectives, including, per the Multi-Year Program Plan: transfer R&D concepts…to the market place…and assist U.S. industry. This project also supports the DOE focus on lowering costs, primarily by providing optical alignment tools and trough test bed capability to improve performance by aligning the receiver and the trough reflector modules and obtaining thermal data during operation. Within the context of DOE and industry interest in trough systems, it therefore rates a score of Outstanding ("Project is critical to EERE…and supports…objectives").

Alignment improvement is quite an honorable effort. It appears that a good way has been developed to do this. Also, the development of a trough experimental facility can be quite useful.

The project appears to deliver good value in relation to annual costs.

These near-term project activities are clearly relevant to the DOE objectives and will provide LCOE reductions to the extent that industry makes use of the results and tools.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project primarily involves refining and using TOPCAT, bringing the trough collector test bed back on line, using these tools to support industry, and developing an improved closed loop tracking system. TOPCAT is the only system available today for full field alignment of the SCA to the receiver (e.g., LS-2 and -3, but can be modified for other systems) and can also be used in the factory; it has been shown to work in the field, relatively rapidly, and is of real interest to industry, in part because they demonstrated a 3.5% improvement in the output, based on better alignment. They plan to compare TOPCAT with VSHOT. They have developed a prototype

closed loop tracker, but this is not described, and further development is limited by time and budget. The rotating platform test bed has been brought on line and is the only system capable of testing up to 350 C. It is scheduled to be used for testing the SkyTrough module soon. The project will increase the size of this test bed to accommodate larger troughs. Project personnel will assist in FOAs.

The project does not list the barriers associated with the tracker (other than time and budget) nor how these will be addressed. The approach to achieving the key objectives is addressed in part through collaborations with approximately a half-dozen groups, and an effort to license the technology is underway. In the discussion, they mentioned linear power tower and advanced polymeric collectors, but with little in the way of specifics. For example, do they intend to use the rotating collector to test the linear power tower? Also, what is the nature of the tests to be conducted with the polymeric collector? Is it basically alignment? Alignment and overall performance? This lack of specificity makes it difficult to judge this aspect of the project.

The overall evaluation is good ("generally effective but could be improved; contributes to overcoming some barriers").

Seems like work is going on just to prolong work going on. Some aspects reported are not clear how the division of labor between NREL and Sandia is made.
One barrier is clearly addressed, i.e. the lack of other quick and easy field tools to check and correct loop alignment.

The activities are sharply focused on near-term CSP barriers with significant commercial promise.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
Their Technical Accomplishments and Progress are rated Good ("Significant progress toward objectives and overcoming one or more barriers"). They have developed, provided, used, and are in the process of refining TOPCAT and the rotating platform. Although they state that they have developed a tracker prototype, nothing about it is described, perhaps for patent filing reasons, but this is not stated. The alignment and performance improvement cited is good and shows progress. Overall, the level of detail is lacking and can only be surmised. For example, they cite that the platform has been improved, but give no examples.

Not clear how much progress has been made compared to other years in dealing with the TOMCAT system. I was given the impression that the system has been developed and now the researchers are attempting, apparently successfully, to keep the money flowing. One aspect that concerns me about this report and several of the others from both NREL and Sandia is the very fuzzy definition of duties. NREL seems have been assigned the line focus studies, but here is a Sandia project that is dealing with line focus issues.

In the presentation there is initial mention of industry interest in closed loop control but the subject is never addressed in terms of how the project is or should address the topic.

All activities appear to be on schedule. TOPCAT's degree of industry impact seems to depend upon satisfactorily licensing it to a 3rd party, which is evidently stalled/slowed.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This effort is Outstanding ("Close, appropriate coordination with other institutions; partners are full participants, publications or presentations"), in that they have outside interactions and work with over a half dozen groups, have conducted alignment in the field that resulted in real performance improvements, and have one patent disclosure and one publication.

Concerned about general overlap with NREL. Has some industry participation. Apparently no universities are involved. Publications appear to be quite sparse, and nothing was mentioned about publications in the archival literature. I take this to indicate that little new is happening.

There appears to be reasonable coordination with NREL and selected collector developers.

Existing collaborations appear very fruitful.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This project cites several key milestones achieved ("all milestones on schedule") for 2010, and yet they state that time and budget constraints limit the work on the tracker, making this inconsistent. The effort for 2011 and beyond is outlined, but it lacks specificity and is scored Fair ("Plans may lead to improvements, but need better focus on barriers"). Plans to conduct tests with TOPCAT on LS-2 and SkyTrough on the platform are noted, but with no detail.

The presentation lists some near-term and recently completed milestones, but it does not show an effective plan for future work, and it is inconsistent in terms of the accomplishments for the tracker. It is necessary in DOE plans to address barriers and uncertainties; it is not clear that these are recognized.

Their ability to conduct work of this nature is unquestioned, and the personnel are exceptionally accomplished and experienced. However, it is unclear what they plan to do, what the milestones will be, how they will identify and resolve barriers, and what the alternative development pathways will be. The lack of specificity in their plan makes it necessary to judge this area as Fair.

One of their past accomplishments is outstanding (e.g., TOPCAT alignment improved performance 3.5%). Overall, there is no question of the worth of the alignment and rotating platform and tracker. But, the planned future effort is unclear in terms of specifics.

Not clear where this work has to go, other than keeping things going in the project.

It would seem desirable to form an advisory committee with broad participation to guide the design process for upgrading the rotating platform. If the tool is only useful to some collector developers and not others then its funding by DOE would be questionable.

Near-term planning is very good. Longer-term view is less clear.

Project Strengths:
Outstanding past work to develop TOPCAT and use it to improve performance.

Having an alignment system is quite important.

Conceptually, the diagnostic capabilities being supported appear to have valid uses.

TOPCAT is a very useful tool, there should be some way of encouraging private industry to use it to set up a service to improve trough field performance.

Highly qualified staff and innovative techniques.

Project Weaknesses:

This project requires a more developed plan, task definition, schedule, budget allotment to a lower level than the total funding, and definition of the barriers and alternative pathways to overcome these barriers. Given the lack of specificity needed for a peer review of the planned work, this area is judged a major weakness.

It seems like the technique is pretty well developed, and it may not require a lot of additional work. Very concerned about overlap between this work and work at NREL. Also concerned about doing work that is publishable.

There is limited information about the extent of likely use of the capabilities into the future. Some sort of industry survey would seem appropriate as a means to justify the DOE annual expenditure in supporting the capability.

Impact may be limited if industry is not motivated to adopt results.

Recommendations for changes to the Project Scope:

The degree of effort (personnel, labor hours, major tasks, schedule, etc.) is not shown. Although in general their work supports major DOE goals, as noted above, their proposed future work should, at a minimum, be addressed at least to the subtask level for the major milestones, and preferably a Gantt chart. There are no Go/No-Go decision points noted. There is no discussion of barriers such as those that may be associated with the increased size of the platform, or how they will reduce the data reduction time. It would be helpful if these aspects were included in the scope.

One area in particular deserves careful attention: tracker improvements. This is mentioned, but with no elaboration, and even the basic principle is not explained, or why the approach/design was selected, and if other options were considered.

The efforts to establish a commercial vehicle for TOPCAT were not sufficiently described to reach a firm conclusion, but it seems likely that additional SNL management support for those would be useful.

Also, a bit more longer-term vision would be good to have.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP003
Presentation Title: Project Title: NREL System Analysis
Investigator: Turchi, Craig

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This systems analysis project successfully addresses several key DOE objectives, including, per the Multi-Year Program Plan: to fully incorporate CSP efforts into the SAI, transfer R&D concepts…to the market place…and assist U.S. industry. This project also supports the DOE focus on lowering costs, primarily by providing tools for a consistent comparison of options and a means to estimate LCOE. This Criterion is rated Outstanding.

Considering market realities is important. Capital costs are certainly very important. So driving effects are very appropriate. However, the ability to come up with this kind of information is quite suspect because of the proprietary nature of it. While we would all like to know the future, we should understand that the future is impossible to know. Lots of DOE money is going into this thrust.

The project will contributed to better understanding of the potential for repowering of existing plants.

It is very relevant but should be better.

The modeling provided by the NREL System Analysis group is vital to the EERE CSP program.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Although their current and past Approach to Performing R&D has been Outstanding, as seen in the Accomplishments, and their planned effort is on track, the overall evaluation is Good ("generally effective but could be improved; contributes to overcoming some barriers"),as substantiated in the following comments. The approach to achieving the key objectives is addressed in part through collaborations with the SAM model development group,

WorleyParsons, several financial analysis groups, and Sandia; this is a sound approach. Their approach also rightfully addresses cost and performance drivers, financing mechanisms and indirect costs, and how technology advances could impact policy. This approach is critical to making informed decisions as to resource allocation and the future of CSP options in the market place. They are assessing price targets, current costs, and estimating benefits.

What is not very clear is the exact nature of the R&D approach for 2011 and beyond. The approach is mentioned at a top level for 2011, primarily as general milestones. Past accomplishments show that they unquestionably have the ability to develop systems analysis tools, but the nature of this is not shown as a task breakdown, milestone, or Gantt chart, with barriers, Go/No-Go decision points, etc. For example, it is not clear how they can assess the overall project cost uncertainty given the range of component and subsystem costs and their uncertainty. There may be some unstated appreciation for the market penetration with first plants, and how this could involve very different tax incentives, discount rates, etc.; these could, in principle, be inserted into the models, but there is no discussion of this. The need for central receiver models is noted, but there is no mention of dish concentrators. If they have this capability, it is not shown. Also, there is no list of the optional designs in these categories, and therefore it is unclear what will result in the planned effort. Given that these issues are not addressed, the overall evaluation is dropped to Good, even though the work accomplished to date is Outstanding.

Appears that the best tools available are being used for this analysis, but they are probably not good enough. The study is skating on quite thin ice because of the lack of data on what companies are thinking and what their costs are. Also, it not clear how the ReEDS program allocates locations. Appears a great deal of stock is being invested in this program, so it had better reflect some reality.

Project does not address technical barriers per se except indirectly by assessing value of technology improvements.

Data uncertainties should be analyzed and explained. Serious financial decisions depend on the accuracy and viability of the solar data.

The tool set being developed is very sharply focused on the key barriers.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
Their Technical Accomplishments and Progress are Outstanding. They have developed, provided, used, and are in the process of refining four basic tools: ReEDs, Solar Prospector, SAM, and trough detailed cost models. Providing these comprehensive systems analysis tools is critically important to assessing options and allocating resources. They also assist in the use of these tools; for example, they cite their running ReEDS for Solar Vision and other studies, releasing an updated version of Solar Prospector, documenting detailed solar trough cost models, assisting EPRI with solar-augmented fossil fuel plant evaluations, and, adding this to Solar Prospector. The tools are relevant and comprehensive for troughs, and in due course will very likely include other options, such as dish concentrators and central receivers.

Probably about as good as it can be, but the results can be carried many directions using data that are highly speculative. Care can be exercised on the first point, but the second one is not very comforting.

The project has considerable value in the context of national studies such as solar vision and REF. It has less value in other contexts.
The SAM code needs serious upgrading. Including towers into the SAM model should be a high priority.

Progress is significant for the level of investment. Some milestones have been delayed by funding priorities.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This effort is Good ("some coordination exists; necessary coordination could be accomplished easily"), in that they have outside interactions and work with the groups cited above. They have provided their codes, and assisted in their use, in several instances. They note that obtaining full input from financial analysts is an understandable barrier, in that these are proprietary IP. There is no mention of any publications, or how to acquire or use the codes being developed, or how industry and others can work with, benefit from, or collaborate with this group.

Does involve some appropriate kinds of entities (EPRI, at least in the old days, not clear about present), Worley Parsons, etc. Nature of the project makes it very difficult to involve companies. So companies are quite scarce. The project could involve university input, but it does not. Publications are not mentioned, so it is unclear if this means there is none, or that they just were not mentioned. I am very concerned if it is the former.

Reasonable collaboration with others but would recommend outreach to individual utilities and ISOs

Some collaboration is listed, but an active outreach program should work with model users to better determine how the program is being used and how it can be improved.
If there is no collaboration with NASA(none is listed) it should be instructed to bring more understanding of the accuracy and uncertainty from satellite derived data.

There is much evidence of close coordination with others, which is essential for effective modeling.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area cites several key milestones, but it lacks specificity and is scored Fair ("Plans may lead to improvements, but need better focus on barriers"). Three areas in particular deserve careful attention: dish concentrators, central receivers, and solar hybrid (augmentation/retrofit, etc.). There are many reasons for this need, including the potential to lower costs, assist in market entry, meet the expressed needs and interests of the utilities (e.g., EPRI, etc.) and industry (Infinia, SES, various central receiver groups, etc.). The presentation lists some milestones, but it does not show an effective plan for future work. It is necessary in DOE plans to address barriers and uncertainties; these are not noted. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) is not shown. Although in general their work supports major DOE goals, as noted above, their proposed future work should, at a minimum, be addressed at least to the subtask level for the major milestones, and preferably a Gantt chart. There are no decision points noted. There is no discussion of barriers such as verification of the analyses and codes and ensuring that the comparisons of the major options are comprehensive and valid. They do not address how to ensure ease of availability of these codes and if they are "user friendly" and if they are fully documented. There appears to be a tacit assumption that the codes are correct, but no mention of how the output will be strongly dependent on the input data, and no examples of

validation by some sort of comparison. Their ability to conduct work of this nature is unquestioned, and the personnel are exceptionally accomplished and experienced. However, it is unclear what the refinements will be, how they will add other CSP options (various Dish and Central Receiver concepts), etc. This effort might even involve subcontracting to outside groups, including industry, to provide cost data, support with system optimization tools, etc. The lack of specificity in their plan, including alternatives to overcome barriers, such as validation of the input data on costs, or assessing the overall system cost uncertainty based on the input cost uncertainty, makes it necessary to judge this area as Fair.

The past accomplishments and capabilities of the personnel are outstanding. Overall, there is no question of the worth of these systems analysis codes and these are clearly needed and should be supported and expanded to include the full gamut of CSP options. But, the planned future effort is unclear in terms of specifics, especially in terms of what types of dish and central receiver models and configurations will be addressed, how they will obtain input data for these, how they will ensure that comparisons are for valid plant configurations, relevant sizes, etc., and how the input cost data, and uncertainty in these data, will be managed.

Not clear how new cost information will be determined so that it can be incorporated. While some strategies are indicated, in my opinion it is very difficult to assess how these can be evaluated.

Project plans to conduct economic comparisons in the context of repowering of existing plants in the western US are both appropriate and a cause for concern in the project context. The application is of significant interest for a variety of reasons, including specifically the sunk cost of the existing thermal plant and avoidance of the need for new transmission lines. However, the current study approach to CSP economic valuation would simply compare the LCOE of the solar augmentation with the avoided fuel cost of the existing plant. Augmentation projects could result, but their relative profitability vs. properly valued standalone plants is an open and important question. They will simply be a foil to facilitate extending the life of existing coal fired plants by positioning them behind a solar fig leaf.

Need to include more advanced thermodynamic cycles including Brayton. It is not considered to be included until 2012. Tower model should have been incorporated earlier to reduce the uncertainty in the potential deployment of this technology.

Plans appear reasonable. More discussion of developments beyond FY11 would have been useful.

Project Strengths:
Outstanding past work to develop needed systems analysis tools (apparently, focused on troughs to data) and to assist in their deployment and application. With this background, they should be able to expand the codes to include Dish and Central Receiver systems of various types.

Costs are very important.

The project appears to have value in relation to generalized economic assessments that result in LCOE comparisons. It appears the project managers have a good feeling for the factors that influence LCOE, e.g. weighted average cost of capital. When questioned, the project manager seemed to understand some of the economic factors that enter into utility and regulatory consideration of power purchase agreements.

The SAM model and the solar prospector are very useful tools but needs further improvement.

Highly qualified team. Strong interactions with CSP community.

Project Weaknesses:
There is no discussion of how these codes will be developed so as to cover the range from initial market entry, with presumably higher costs, to a mature industry.
Given the level of specificity needed for a peer review of the planned work, this area is lacking. However, the work accomplished to date is outstanding and valued by industry. It can be safely assumed that good work will be accomplished, but the lack of any level of detail for the 2011 effort and beyond is a major weakness.

Costs are kept very secret by companies, and the latter are not even sure about the ultimate direction these may be. So the validity of the results is quite suspect. Also, the effect of learning on the plant costs are not easily defined. For example, the nuclear power industry in France was anticipated to show cost decreases because of learning, but the opposite trend has been shown to exist in this fairly well-matured industry.

The main weakness is that the project may do a serious disservice to the CSP industry by reinforcing the widely held but false perception that CSP offers no economic benefits to the electric system beyond the avoided average cost of bulk electricity from other sources that is displaced over the course of a typical year. LCOE can be compared to this value, but the comparison is not valid in any specific case. What is valid and necessary is a careful assessment of the economically optimum plant configuration as a function of grid penetration and storage capacity and configuration. LCOE is not a valid metric for plants that have dispatchability that allows for delivery of electricity during periods when marginal costs of generation on the electric system are high. In fact LCOE for conventional power plants varies over an extremely broad range from a few cents per kWh for baseload resources to at least an order of magnitude more for peaking resources, and there are no specific benchmarks for conventional intermediate and peaking resources against with CSP LCOE can validly be compared. The stated premise of the project is that CSP costs are too high, but there is no mention of or attempt to quantify the cost of CSP plants configured to maximize overall economic value to the electric system.

Recommendations for changes to the Project Scope:

There is essentially no plan, no task definition, no schedule, no budget allotment to a lower level than the total funding, no definition of the barriers and alternative pathways to overcome these barriers, and no definition as to what types of Dish and Central Receiver systems will be added, and how this will be accomplished, and over what time frame. For example, there will hopefully be a plan as to how to ensure that the barriers associated with obtaining valid cost data and its uncertainties will be managed in the code and code inputs, and how the codes will assess the financial viability (and range of uncertainty associated with these) of the full range of various options, especially different types of troughs (these may be in the code, but it is not clear), dish concentrators, and central receivers.

I don't trust the results of this work very much. Should consider repowering applications, as these would only involve the unknowns of the solar field.

I would really like to see equal emphasis on economic value. The program is driving toward lower LCOE but it should also be driving for higher economic value where the benefit cost ratio in the near term for development investments may be higher.

DOE FOA's required comparisons using the SAM model but for projects involving towers or Brayton cycle power systems this is impossible. This must be fixed.

There should be a thorough discussion of the errors and uncertainties in the DNI data and its relationship with the satellite derived data.

Try to increase interactions with EIA to improve NEMS' handling of novel generation technology adoption.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP004
Presentation Title: Dish Research and Development
Investigator: Andraka, Chuck

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:

The Dish R&D program has successfully addressed essentially all of the primary DOE objectives, which are, per the Multi-Year Program Plan: to fully incorporate CSP efforts into the SAI, improve storage technologies, transfer R&D concepts…to the market place…and assist U.S. industry. The DOE focus is on technology development to lower costs, pursuit of thermal storage to enhance dispatchability, exploring advanced concepts, and reducing barriers to market penetration. Essentially all of these objectives are addressed through collaborations with SES, Infinia, and Brayton systems, diagnostics tools, engine and controls testing and support, Infinia's dish and thermal storage system with potential for dispatchability (i.e., 30 KW dish), deployment of a 1.5 MWe dish field partnership with Salt River, optical analysis and collaboration, a CRADA, MOU, FOA support effort, and contracts for a cost study (Sargent Lundy) and optical development work at two universities, etc.; together, these efforts resulted in a Federal Laboratories Consortium Award for Technology Transfer. The Relevance Criterion score is Outstanding.

It is good to have work on going on related to systems other than troughs and towers. Both Stirlings and Braytons are appropriate thrusts to pursue in exploring possible cost savings.

EERE has a stake in the success of all of the CSP projects in active development in California and the southwestern US. The SES projects in California, because they have a high profile, may, rightly or wrongly, be seen by investors and the public as bellweathers of the viability of CSP as renewable supply option for the region.

This work seems very responsive to the DOE plan, however see comment below.

This project is likely critical to Stirling-Dish CSP commercialization and as such is important to the EERE Solar Program. However, its scope is relatively narrow and the number of potential beneficiaries appears smaller than many other projects.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The past Approach to Performing R&D has been excellent as seen in the Accomplishments. What is not very clear is the exact nature of the upcoming effort for 2011 and beyond. This plan is only mentioned at a top level: support to large scale SES deployment (pending funding), expansion of optical tools, and continuing operational support to SES and Infinia. Past accomplishments give credence to the ability to perform this function, but the nature of this support is not shown as a task breakdown, milestone, or Gantt chart, and can only be surmised to be more characterization and test support, mentoring, etc. Given the level of specificity needed for a peer review of the planned work, this area is lacking. However, the work accomplished to date is outstanding, recognized by the federal labs, and valued by industry. It can be safely assumed that good work will be accomplished, but the lack of any level of detail for 2011 effort lowers the overall score from an otherwise Outstanding to Good ("generally effective but could be improved; contributes to overcoming some barriers").

Seems to be quite industry-led, and this could be a good or bad thing depending on limitations on information flow dictated by the companies. Not clear what of the many talents at Sandia are used for this work other than furnishing additional people for the field evaluations and working on alignment issues. Another issue is that, being closely tied to industry and their concern about proprietary information, not much can be said specifically (at least nothing really quantitative was given in the presentation) about the reliability. For systems that include a lot of smaller (less than say, 100 kW) engines in installations that amount to MW, the reliability really needs to be quantified. Of course I am sure the companies wish to guard this information, and possibly the Sandia people don't know the numbers say, for example, on the Maricopa project. It would seem like this could be the Achilles heel of these systems if it is not satisfactory.

It is difficult to objectively evaluate effectiveness. It would be desirable if information came to the public domain that allowed an objective technical evaluation of SES progress. The information presented was anecdotal and incomplete relative to this need. Specifically, no cost information was referenced, and no non-proprietary information may be available. The developer's reported enthusiasm for DOE and Sandia support is to be expected. Venture investors care primarily about managing investor perceptions of the technologies and companies in which they are invested. The DOE/Sandia involvement, whether it is technically valuable or not, conveys a priceless positive assessment of the venture's value to next stage investors and critically important deployment partners and investors. It is greatly in the technology venture's interests to retain DOE/Sandia involvement as a part of its exit strategy.

Many technical barriers have been addressed, but I am concerned that due to the rapid scale up there may be technical problems arising from underestimation of issues encountered in deployment. Those include engine lifetime, Dish alignment problems, and controlling large blocks of dishes.

To the extent that the project can be described in non-proprietary terms it appears to be targeting significant barriers and making substantial progress, indicating an effective approach. However, it is difficult to assess the unseen proprietary details.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments have been Outstanding, in that the technical barriers are being addressed with industry (optical tests/characterization, field deployment, resolution of a likely high number of day to day problems, deployment/operation support to the 1.5 MWe SES field, etc. There is also a high degree of commercial potential. This is a team effort with industry, with three major players. There are a few late items, but these are not the responsibility of Sandia. Overall, accomplishments and progress have been Outstanding.

Project goals seem a little loose. Mainly operating systems. Suppose that good feedback to the companies is present.

Agree with Sandia that the completion and initial operation of the MW scale deployment is a major step on the path scale up and technology validation path.

Apparently, this is a very well-run program, with substantial improvement in engine and dish technology achieved in the last year.

The metrics and progress are not fully disclosed, but project partners who are paying the majority of costs appear to be satisfied.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Outstanding. The project involves industry (three major solar power system developers), NREL, and two universities, with CRADA, MOU, and contracts (cost study-Sargent LundY) over a broad area. Technology Transfer is recognized by a Federal Laboratories Award. There are three papers and three patent disclosures. The collaborations with SES in particular resulted in an important joint accomplishment (i.e., the 1.5 MWe site deployed quickly with high availability) with essentially no DOE funds.

Certainly working closely with companies who are developing systems. Clearly close collaboration. Some mention of another lab (NREL) and universities for optical aspects. However, the university listing seems to be mainly through students from the universities, rather than universities themselves. A few publications are listed, but nothing of an archival nature.

As the value of early prototype equipment recedes, Sandia's primary role is understood, based on the presentation, to be transfer of technology to the private sector venture's technical and engineering teams. This is an appropriate role as long as it does not unfairly disadvantage other competing ventures.

There is very close collaboration between the Sandia staff and SES.

This score is a compromise between (1) the obviously close, evidently effective collaboration between the project and individual industry players and (2) the fact that the coordination is limited by the proprietary wraps surrounding all of the work.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity. The presentation does not show an effective plan for future work, and there is a degree of uncertainty associated with the larger scale deployment associated with the SES commercialization (especially, funding and schedule); this uncertainty is not addressed by, for example, an alternate pathway, and although this specific effort is not Sandia's

responsibility, it is necessary in a plan to address uncertainties, especially those that are recognized. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) with Infinia is not clear, and is supported primarily by an MOU, which is not discussed. Since Infinia is developing a thermal storage system, which supports a major DOE goal, this area, at a minimum, should be addressed at least to the subtask level, if not a list of milestones and a Gantt chart. The lack of a dish is noted, but no mention is made of potential alternatives or ways to work around this barrier. There are “bullets” for “Solar School”, which is not described, advanced receiver development (including heat pipes), for which the Sandia effort is not noted, etc. The ability to conduct work of this nature is unquestioned, and the personnel are exceptionally accomplished and experienced, but the lack of a plan, including alternatives to overcome barriers, makes it necessary to judge this area as Fair (“Plans may lead to improvements, but need better focus on overcoming barriers.”).

The past accomplishments and capabilities of the personnel are outstanding. The nature of the work being done by the universities is not described, nor the value of the contracts. Overall, there is no question of the worth of this project, but the planned future effort is unclear.

Basically the future work appears to me to be basically "more of the same."

The view presented is that the project is "industry led", and the same could be true for many other CSP efforts in the US today. The project's history and circumstances are unique, and that may justify a DOE role that is passive in regard to planning. Clearly, in the context of a single company's efforts, DOE cannot have a major planning role relative to its technical support of company funded RD&D. It would be a loss if the SES effort were to stall or fail, but it would not necessarily doom the dish-engine option, so some level of planning for work that would support the broader technology dish-engine development community would be appropriate.

Operation of the Maracopa plant should provide useful information for the planned large scale up in deployment.

Near-term plans appear good. Longer term was not much discussed.

Project Strengths:

Outstanding past work to aid dish systems development.

Assisting companies with operations is quite valuable.

Apparently improvements in the SES engines are quite good.

Seems to have a very high promise in the current engine design.

Sandia's trust relationship with SES is based on a long term shared commitment to the success of the technology, and to a significant extent Sandia holds a significant and valuable share of "corporate technical memory" developed at public and private expense over multiple decades.

The closeness of this collaboration tends to produce advocacy in lab staff. I am concerned that this advocacy could distort DOE's role in favoring this technology.:
Highly qualified staff and facilities support.

Project Weaknesses:
There is essentially no plan, no task definition, no schedule, no budget allotment to a lower level than the total funding, and no definition of the barriers and alternative pathways.

Surmise there is a tight control on most of the important parameters by the companies involved. Makes it very difficult to evaluate progress.

Some contingency planning based on objective, transparent metrics would be in order. DOE should recognize the critical need for independent assessment of the CSP projects in development in the US. Contract failure is a major issue for state level policy makers, and their lack of visibility to realistic, independent assessments, inevitably will result in policies based on unrealistic expectations, whether positive or negative. Policies based on unrealistic expectations regarding scale up risk, project time timelines, and a host of other issues, may be at cross purposes with those of the DOE program.

The closeness of this collaboration tends to produce advocacy in lab staff. I am concerned that this advocacy could distort DOE's role in favoring this technology.

Proprietary nature limits leverage of results.

Recommendations for changes to the Project Scope:
Some work could be done on alternative optical approaches done over 20 to 30 years ago, which were successfully deployed at Solar One, etc., but are not apparently known or of interest, but could have potential benefit. There are no decision points noted, or discussion of barriers to the realization of the proposed technology; this is especially important for the Infinia thermal storage system, but could also involve such activities as cleaning the SES dish (automated cleaning is noted as a need, but not addressed). The Brayton work could be important, since this offers an alternative to the Stirling cycle, potentially with somewhat higher efficiency, but there is very little mention of what will be done, or when, or by whom. This aspect should be considered in

the project scope.

Need to focus on quantifying reliability.

The information presented was inconclusive regarding the continuing need for intimate Sandia technical support of the SES venture. This does not necessarily imply a present project weakness but it should be recognized that the SES venture will likely continue in some form long into the future because of the value of embedded intellectual property and experience, whether or not current projects in development are realized. It would be good for DOE to provide some guidance regarding the need for the project to either identify new ways of providing technical support to SES or ways of transitioning to the types and level of support being offered to other CSP companies and the industry as a whole.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP005
Presentation Title: Design of a High-Temperature Molten Salt Linear Fresnel Collector
Investigator: Brost, Randy

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This molten salt linear Fresnel program addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel concept that uses the molten salt as the heat transfer fluid, and thus is directly stored without a heat exchanger. In principle, it would benefit from parallel efforts to improve molten salt. If successful, a 40% decrease in LCOE could result. Thus, this Criterion is rated Outstanding.

Involves concepts of a linear Fresnel system and with those of molten salt system seem like two almost unrelated issues. Not too much is said about the salt system. Is it a two-tank system or what?

The research hypothesis is that a linear Fresnel configuration could be superior economically to current line focus commercial offerings. It is difficult to infer from the results as presented exactly why the models suggest a significant economic advantage for the linear Fresnel approach, and until the models have been validated in the field, there is reason assume the error bar on the results may exceed the projected economic advantage. Nevertheless, the project has value in providing models that can be adjusted based on field experience.

An interesting approach that should be pursued to better evaluate its possibilities.
This project aims to lower LCOE by enabling use of molten salt in a linear Fresnel collector, which would facilitate adding TES. This clearly supports the EERE Solar Program objectives.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is good ("generally effective but could be improved; contributes to overcoming some barriers"). The upcoming effort for 2011 and beyond is described, including a Go/No-Go decision point. They are conducting computer analyses for the optics that give every appearance of being thorough and well-founded. They emphasize LCOE and are attempting to develop a novel concept with potential improvements on cost. They rightfully and objectively note that their comparison is against conventional troughs, and that improvements in trough technology could impact their design. They state that loads are far lower than for conventional troughs; this is not addressed in detail, but appears to be true in principle. They have a Reflectech film on a flexible mirror panel. They have conducted over 50,000 simulations. The receiver doesn't rotate, which makes it easier to use a molten salt. They have a baseline layout, but they admit that this is subject to some variation due to sensitivities. If successful, they will have a potentially higher performance linear collector, operating at a higher temperature, with a molten salt, without the complexity and cost of rotating joints. They couple their analyses with IRR estimates of various field layout/loop options, which are understandably not shown in detail, given the commercial nature of their work; these calculations are also shown in comparison with conventional troughs.

Seems to be almost entirely centered on the optical characteristics and the optimization of the parameters that will improve the optical performance. This needs to be done, if it has not been done earlier. Having salt as the working fluid may have some impact that is not clear on the basic thrust of what we have seen in the presentation.

The project could be more effective if the design and environmental conditions that favor the linear Fresnel approach were explored or at least mentioned. For example, wind loadings appear to be very important, so one would guess the LF approach would have better relative performance at sites imposing higher design wind loadings. Likewise, some work to understand trough configurations that minimize wind load related economic penalties would be in order. Finally, the value of the project would be enhanced by the existence of an independent capability to check model results and challenge assumptions. Reliance on a technology advocate to evaluate the relative advantages of concepts being advocated does not seem adequate under a reasonable expectation that DOE demonstration support will be closely contested, and the key will be DOE support of cost and performance validation efforts

Extensive modeling to narrow the large number of possible configurations is definitely the right first step.

The technical approach is innovative and appears very well conceived with significant commercial promise if successful.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments with respect to the optical analysis and the field layout are rated Good ("Significant progress toward objectives and overcoming one or more barriers"). There is a potential for commercial application, especially given that they are in this business. They are to be commended for analyzing and initiating development of a novel, albeit not completely new, concept. They have been emphasizing the optimization of the overall configuration and making progress towards understanding this. They have a successful trough design, and that provides assurance that if this approach is determined to have cost benefits over conventional troughs, they will be able to integrate this in Phase 3.

The program should have some elements that relate to linear Fresnel system characterization, and this may be giving insights to improved performance. It is certainly giving insights to how the field design can be optimized, and this is good to have. Comparison to trough field performance is one of the better things being done. Kudos on those aspects.
The progress toward completing the modeling phase seems satisfactory, and the presentation was informative. It would be helpful in future presentations to spend time on economic and finance assumptions driving weighted average cost of capital, as these assumptions would NOT be the same for proven vs. developmental concepts and would NOT favor the developmental concepts.

The analysis covered a number of important issues to better define an optimum design to deploy and test.

Progress to date appears on track.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Fair. The project involves NREL and apparently an informal relationship with Sandia. Technology Transfer is not shown. There are no papers or

patents/disclosure noted, but they do note that some patents are forthcoming.

Some coordination with NREL and Sandia is indicated, but the nature of this is not well spelled out. No other coordination is listed. No publications or presentations are listed.

Clearly, the extent of internal review and collaboration within the company is of interest, as it appears that SkyFuel is also developing projects around trough collectors. The lack of any internal company context for the work is a concern. Why is SkyFuel interested in the LF concept and what choices are ahead for corporate funding of further development. The informal relationships with NREL and Sandia are mentioned but the level of coordination is difficult for reviewers to evaluate. The value of the work hinges to a great extent on the capacity and likelihood of follow through into more expensive development phases. The presentation provides no information supporting evaluation of such capacity or likelihood.

There should be more cooperation with manufactures, developers and they should publish their results.

Development appears aimed at commercialization by the contractor alone. Collaborations are only with NREL and SNL.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity. The presentation does not show an effective plan for future work, and there is a degree of uncertainty associated with the exact system size, layout, and performance, relative to proven trough designs. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) is not clear. The key decision points are noted, but there is little discussion of barriers to the realization of the proposed technology; this is especially important for the receiver and field integration associated with wind induced oscillations, size of the receiver, etc. The receiver may be cheaper since it lacks rotating joints, but it is not clear that it won't be more difficult to inspect, remove/replace, etc. The lack of a plan that at least addresses all aspects of the future work needed to have this concept ready for a pilot plant, including alternatives to overcome barriers, makes it necessary to judge this area as Fair (e.g., "Plans may lead to improvements, but need better focus on overcoming barriers.").
The effort to analytically determine the system configuration is apparently being done well, and although this is important, it is only one aspect that deserves attention. Additional work may have been conducted, but is not shown, on loads, deflection, flow loss, life, etc.

Movement to system design is a natural next step. Not clear how much of this has been done already as part of the analysis that has already taken place.

The work was presented in the context of a project plan and the current status was clearly identified. The lack of reference to other linear Fresnel efforts was puzzling, to the extent that such efforts might serve to validate assumptions or identify critical modeling issues. No barriers to successful development were identified, which suggests an uncritical approach to the risks and challenges ahead if the work is to be the basis for further development. Likewise, some level of error analysis is appropriate in the model development phase of a project like this.

Project appears effectively planned. Future work logically incorporates appropriate decision points and considers barriers to the realization of the proposed technology.

Project Strengths:
This is a novel approach with some potential to reduce costs relative to conventional troughs, and a possibly more practical approach to the piping configuration for the molten salt, in that no rotating joints are required. There is a high degree of industrial cost sharing.

Linear Fresnel analysis and the comparison of it to trough results is quite good.

The model appears to support design optimization efforts, and the results suggest interesting directions for field validation work.

Highly qualified team. Innovative use of optimization modeling.

Project Weaknesses:
No effort is shown involving a broader range of issues than the basic analysis tool for the optics and the layout; these issues include the receiver design, integration of the system with a complete field, advantages of the piping relative to rotating joints, pressure loss, loads, thermal loss, etc.

The engineering and financial strengths underlying the modeling effort were not clearly illuminated, raising concerns that key assumptions may not be subject to competent critical review.
Also, market context was weak. For example, it seems obvious that the LF concept might have a good application on sites shared with wind turbines.

Not clear how widely results will be useful.

Recommendations for changes to the Project Scope:

The receiver of a conventional trough is somewhat protected from wind loads and thermal loss, whereas the raised receiver is subjected to potentially higher loads and losses; this aspect should be addressed. Some advantages and disadvantages are not addressed to a level that would allow some cost estimates to be made relative to conventional troughs. For example, automated cleaning is somewhat easier since the receiver is not an obstacle, but the mirrors may be more difficult to reach. In addition, having the receivers closer to the ground will result in more rapid soiling, and if too close to the ground, will actually collect small pebbles, etc. For example, I placed several dirt/dust collecting cones mounted on 20' poles at China Lake, Sandia Albuquerque, etc., in the late 70s and monitored them for several years. There was a very much higher accumulation rate of wind-blown dirt/dust/particles close to the ground. Such an accumulation, and exposure, with a film reflector, could be an issue in terms of life and I think needs to be addressed; having the mirrors closer to the ground is better in terms of loads, but it does result in greater exposure.

An independent effort to validate model assumptions and provide a sanity check on model results would add to the overall credibility of project's conclusions and recommendations for further development.

This effort could be improved if it involved more players to broaden opportunities for innovative inputs. However, such an approach would likely not be attractive to the current investigators.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP006_
Presentation Title: Cleanable and Hardcoat Coatings for Increased Durability of Silvered Polymeric Mirrors
Investigator: Clear, Susannah

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This project addresses several important DOE objectives, including, per the Multi-Year Program Plan: technology development to lower costs and exploring advanced concepts. …transfer R&D concepts…to the market place…assist U.S. industry and reducing barriers to market penetration. If successful, some decrease in LCOE could result
However, the potential for improvement, and likelihood, are not quantified, nor are potential reductions in installed cost shown, even if the goal of a low cost, durable silvered polymeric mirror is achieved. Thus, this Criterion is rated good.

Advanced performance, low-cost coatings for silvered surfaces are quite pertinent for advancing the goals of the solar program.

It could be that the project is critical to the program, but the proof of that was left to the reviewer.

20 year life time sounds like an aggressive goal.

The project supports the objectives of the EERE Solar Program by aiming to reduce capital cost and O&M of current glass mirrors. This could reduce LCOE of all types of CSP.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Good ("Generally effective, but could be improved; contributes to overcoming some barriers"). However, this project could be improved if the haze measurements were correlated with specular reflectivity loss; the haze measurements are probably sufficient for screening, but they do not translate directly into optical performance,

which is the main parameter of interest. Other aspects are addressed (durability, cleaning, and adhesion), but again, it is difficult to determine quantitatively if there will be a long life, high performance mirror, with a system cost reduction, or how much this will benefit the LCOE. They state that they will address LCOE in the remainder of 2010, and that there is a Go/No-Go decision this fiscal year. The laboratory work is of high quality, and they may have identified two coating classes that may provide interesting results.

Optimization of the three critical parameters (cleanability, abrasion resistance, durability in accelerated aging aspects) seems to be very appropriate. Of course the latter one causes the most problem in knowing how well it can be characterized. 3M should be as well versed as any organization in being able to handle this kind of work, both because of past experiences, and their current capabilities.

The project appears to be well designed. The presentation identified the key coating attributes being considered, and certainly 3M can be assumed to be in a very strong position to identify the relevant candidates for evaluation.

Well-designed approach, however the technical feasibility of this approach is a question. A twenty year life time is a very aggressive goal.

The technical approach draws upon significant contractor experience with polymeric coatings and chemistry, and it is well focused on critical barriers.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments with respect to the coatings is essentially that of having conducted an apparently large number of tests with different coatings, and comparing the results for durability, haze (after weathering and abrasion), adhesion, clean-ability, etc. The project may result in one or more candidates that have sufficient potential to justify further work. However, to date, the results are essentially qualitative, in terms of projected life, cost, and performance and the results aren't directly applicable to assessing LCOE, or specular reflectivity, especially over time. For this reason, this criterion is judged fair. ("Modest progress in overcoming barriers; rate of progress has been slow").

Seems to be on track. Series of quantified performance comparisons were given that showed the ability of a variety of formulations to meet the requirements.

Given that the use of silvered polymeric mirrors is not a new subject and there is a fair amount of presumably relevant field experience, one might hope for a better illumination not only of the material options and their relative performance in certain key specification categories but also some sense of the absolute standards that would apply to their use in actual collectors.

Materials show some progress but are not yet very convincing. The current testing regime should show better results in order to continue. I am concerned that the haze measurements should be more carefully defined in terms of how much energy goes into what solid angle. If there is much forward scattering outside the acceptance angle of the receiver the performance could be significantly degraded over predictions based on the "haze" measurement.

Progress to date is very encouraging and suggests that new coatings will be developed to have suitable durability and improved cleanability for non-glass mirrors.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored fair ("A little coordination exists; necessary coordination would take significant effort"). It should be noted, however, that the PI, and 3M, are unquestionably highly capable of conducting work of this nature, and it is important to once again consider polymerics with improved life, performance, and improved LCOE. At this stage, extensive coordination with others in terms of assessing the materials aspects is not needed. The project involves NREL through a CRADA and a potential deployment in the field with Abengoa. There are no papers or patents/disclosures.

Working with Abengoa on applications. Also has a CRADA with NREL. Not clear all of what is going on with the latter. Perhaps this is sufficient. No publications or patents. Perhaps this is typical for this type of longer term work.

No mention was made of efforts to secure input from possible future customers or manufacturing interests within 3M. This is surprising since typical good practice in product development efforts of this sort does have reference to customer requirements and also technology and manufacturing considerations. Perhaps these elements are considered proprietary and sensitive - however, the project is being funded primarily by DOE, so one might hope for some visibility to them.

I would like to see more collaboration with trough, heliostat suppliers and substrate manufacturers.

There is significant interaction with NREL and promised interactions with Abengoa. However, there could be other beneficial collaborations with the others who are exploring non-glass mirror options.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area is judged Outstanding ("Plans clearly build on past progress and are sharply focused on barriers") , in large part because there is a disciplined approach to developing and tracking the schedule and budget for specific tasks. The schedule shows Go/No Go decisions, evaluation of alternatives (i.e., associated with barriers), deliverables, milestones, and status. In this regard, the proposed future work is very well planned. Something of this nature should be required of all of the projects.

The effort would be more directly useful to the DOE goals if the measurements were quantitative, in terms of specular reflectivity, potential cost improvements were developed for the material as deployed on the module, and life was determined, for example, in the number of years of high performance, leading up to potential replacement.

Performing field evaluations seems to be the only general thrust for the 3rd phase of the project. While this is certainly needed, it seems to the layman that this will not yield a lot in one year. If it does (for example because of premature failures), this will be a bad omen.

See weaknesses. I do not believe enough attention is being given to the future steps necessary to establish silvered polymeric films as a viable commercial option.

The future plans were not well defined or articulated.

Planning for near term appears good. There is little evidence presented on thoughts of longer-term prospects.

<u>Project Strengths:</u>
This is a thorough approach in terms of screening potential coating candidates, and it is well

supported by 3M funds. There is a high degree of industrial cost sharing (about 56% of DOE funding).
Dealing with an important problem. High performance, low cost reflector coatings are critical to the program.

Well-credentialed company proceeding systematically

Highly qualified contractor.

Project Weaknesses:
Lack of specific data on performance, life, or projected cost that can be used in cost models to assess whether or not the material selected, if successful, is appropriate to meet LCOE goals.

Implicit in this, but to be expected, is the inability to make longer term reliability assessments of performance. This is not specifically a weakness that can be easily addressed.

See recommendations below.

Not evidently engaged significantly with much of the interested community.

Recommendations for changes to the Project Scope:
What is needed is a better level of coordination on the impact that the new polymeric would have on the overall LCOE.

Would be good to compare performance relative to competing products. The latter do not have to be named specifically. However putting this in proper perspective would be quite valuable.

It is unlikely that silvered polymeric films will be used in commercial projects until warranties are available that reduce project technology risks, real and perceived.

In other industries such warranties are based on accelerated life test and specific warranty terms. The tests essentially confirm the product's long term durability and performance under simulated field conditions.

It is not too early to include in such a project tasks that address:
1. Long term performance standards, e.g. < X% reflectivity loss after 20 years
2. Torture test protocols for accelerated life simulation.
3. Reference to any warranties and standards that can and are provided and applied by glass/metal mirror manufacturers.

I recognize that it may be asking too much for 3M to deal with an issue that may not be getting sufficient attention by the industry emerging to supply solar mirrors as a whole, but it would be a good exercise within the project scope to try to anticipate the specific, quantitative standards solar mirrors will have to be warranted to meet if they are to be used in so called "bankable" projects.

This project could benefit from broader collaborations in the mirror-user community. Ideally, it would also involve others with polymer film experience to enhance possibility of innovation.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP007_
Presentation Title: "Development of Next-Generation Parabolic Trough Collectors and Components for CSP Applications"
Investigator: Marcotte, Patrick

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This next generation trough program addresses several important DOE objectives, including, per the Multi-Year Program Plan: …technology development to lower costs, and exploring advanced concepts. However, there is a major barrier in that the cost reduction must be a factor of two over the current cost, and the effort is focused on the collector field, which is 34% of the total LCOE. Thus, even a factor of two reduction in the collector field cost would not achieve the cost goal, and the anticipated cost reduction is about 10 to 20%. Even if the receiver and heat transfer fluid costs (i.e., about 25% of the total) are greatly reduced, it is still difficult to justify how this approach will be successful in reaching the LCOE factor of two cost reduction. This is a well-funded effort by a major trough company, and they plan to continue this effort. The new design offers potential improvements in cost and torsional stiffness, and this might improve overall plant performance. However, the lack of a quantitative assessment of how this effort provides the needed overall cost reduction is an important consideration in judging this project. Based on the above, this criterion is judged to be Fair ("Project partially supports the EERE Solar Program and DOE RD&D objectives").

Looking at three existing concentrators for decreasing costs in the short term. Longer term they are looking at various new kinds of trough concentrators.

Some cost saving features of advanced trough designs under study may find their way into future plants if demonstrated through the project.
Lower cost collectors are a priority for near term deployment of solar thermal troughs.

The project objectives describe support of EERE Solar Program goals; however, the presentation does not make sufficiently clear what are the specific quantitative outcomes to be achieved.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Fair ("Has significant weaknesses; may have some impact on overcoming barriers"). The stated plan includes benchmarking (re-examine requirements and costs), systems analysis (optical, FEA, and thermal modeling) and testing (full loop), and new concept development (system re-optimization and new designs and techniques). Three trough designs have been considered. A particular aluminum space frame design was selected and is now being refined for a third generation, pre-commercialization version. However, there is virtually no quantitative data provided on these designs, or why one was selected over the other two. There are no specific examples of the types of component improvements made. Two mini-collectors are in the process of being installed at the Lakewood test site. Various tests are noted (NREL two-axis tracker, which is presumably the rotating platform), laser radar dimensional characterization, Cameo demonstration project, etc. However, no details are provided. Based on this relative lack of plans, and uncertainties (i.e., VSHOT, DO, or some other approach to be used by NREL, further study of wind loads (with no mention of any results to date, etc.), the plan as shown is vague and essentially described in broad general terms. For a project having a cost of the order of $4M for the two phases, there should be real, quantitative results. Without this, the R&D Approach is judged Fair.

Primarily performing, in my opinion, quite small incremental improvements in a number of aspects of their designs. Also are developing physical evaluation systems. I think this work is clearly technically feasible, as all three trough designs exist. However, I am not convinced that this is leading very quickly to the stated goal. The project has a very high DOE contribution for something that is almost more the realm of demonstration and less in the realm of basic research. The main thrust seems to be larger aperture area, and the wind loading trade-offs are recognized. Many design considerations are mentioned, and alternative designs were evaluated. However, the discussion of design issues was vague, and quantitative information primarily came in the form of projected LCOE impacts. Based on the level of technical detail and/or analysis presented it is difficult to say the project is well-designed and commercially promising. Most likely, Abengoa will have confidence to apply some of the lower risk design features being evaluated in test loops, etc.

It is very difficult to determine what progress has been made. There were vague statements about development of new space frame collectors but no drawings or photos were presented. There were many references to other ongoing projects and planning and "planned testing" but no results were presented. It is not clear that anything was accomplished and it is between 6 and 12 months behind schedule.

Discussion of approach mentions goal of _developing_ quantitative requirements but does not adequately describe how this is to be done.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments with respect to the space frame are rated Good ("Significant progress toward objectives and overcoming one or more barriers"), even though there is a general lack of specificity, even though they have built and tested it. There is very little real information provided (weight, cost, optical performance, durability, etc., etc. There is a potential for commercial application, especially given that they are in this business. The reason for the lack of specificity may be due to the competitive nature; if so, this needs to be stated.

They apparently have a successful trough design in terms of meeting structural and optical performance requirements, but, it is not clear what these are quantitatively.

I think goals are being reached, but, in my opinion, those goals are far from lofty.
Visible progress metrics are lacking. Seem to be focusing in on advanced aluminum frame. Could be a good direction but no way based on information presented to say whether it really is. Hard to say progress is significant. Activities seem diverse and related and may be integrated. The Program should reevaluate the current level of funding versus the overall benefits of this effort.

No progress was presented.

Some modest achieved cost reductions are mentioned (e.g., "10-20%" in module cost) but most accomplishments seem to be speculative reductions (e.g., "Identified design opportunities to reduce SCA cost 20-30%")

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Fair ("A little coordination exists…"). The project involves NREL and a limited relationship with Sandia. Technology Transfer is not shown; there are no

papers or patents noted.

These aspects not very well defined in the presentation. I am assuming that there is some collaboration with some people in the National Labs (VSHOT, application, for example), but little is really defined.

Internal collaborations seem to be the strong suit. Hope they are effective. Not much information about them.

Almost no collaborations with entities outside the company other than those that are mandatory for the project.

External collaboration is limited to NREL, SNL, and subcontractors.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:

This area, as shown, lacks specificity, but, they plan to install and test modules in the field and therefore more detailed plans are clearly being used. The key decision points are noted, but there is little discussion of barriers to the realization of the proposed technology. The lack of a plan that at least addresses all aspects of the future work needed to have this concept ready for a pilot plant, including alternatives to overcome barriers, makes it necessary to judge this area as Fair (e.g., "Plans may lead to improvements, but need better focus on overcoming barriers.").

The effort to build and install and test a trough system is apparently being done well. However, the material presented for evaluation is too vague to really make a well-reasoned judgment.

In my opinion little in quantitative value is being gained compared to the expenditure of funds. This is certainly the case when this placed alongside many of the other projects being funded by the project.

The budget of Phase 3 seems better defined than the scope. How could the budget be known if the scope has not been defined? Well, maybe it is defined but it wasn't shared with reviewers.

No significant discussion of future plans.

Evidence is given for longer-term planning, but it does not appear well coordinated overall.

Project Strengths:
A prototype trough will be tested for performance and these will be installed and tested in the field.

Involves one of the major forces in the industry.

Abengoa is a leading system integrator with projects in operation.
None, other than goals.

Large parent company, good relationships with NREL, SNL.

Project Weaknesses:
The major weakness is the lack of specificity. However, some of the decision points are noted, and major milestones are shown on the schedule.

Not very impressed with the cost/benefit ratio of the work.

No evidence of systematic approach claimed by the presenter. Lack of progress metrics looking back and forward. Feels like a very loosely managed effort considering the relative size of the DOE commitment.

Presentation inadequately provides unified vision of strategy. Appearance is of a grab-bag of projects.

Recommendations for changes to the Project Scope:
Quantitative data is needed on these designs, including why one was selected over the other two. Specific examples of the types of component improvements made should be provided. Various tests are noted (NREL two-axis tracker, which is presumably the rotating platform), laser radar dimensional characterization, Cameo demonstration project, etc. However, no details are provided; these are needed.

If the aluminum space frame really is the key, then at least develop a specification and detailed plans for design, testing and evaluation of it to see if the specification is met and whether there is a compelling objectively verifiable argument for going to Phase 3.

The Program should reevaluate the current level of funding versus the overall benefits of this effort.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP008_
Presentation Title: Reflector Technology Development & System Design for CSP Technologies
Investigator: Schaut, Adam

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This Alcoa "wing box" program addresses several important DOE objectives, including, per the Multi-Year Program Plan: …technology development to lower costs, and exploring advanced concepts. However, as noted in several of these evaluations, there is a major barrier in that the system cost reduction must be a factor of two over the current cost, and the effort here is focused on the collector field, which is approximately 34% of the total LCOE. Thus, even a factor of two reduction in the collector field cost would not achieve the cost goal, and the anticipated cost reduction (or goal) for the "wind box" design is about 20 to 25%, which they claim is about a 10% reduction in LCOE. Even if the receiver and heat transfer fluid costs (i.e., about 25% of the total) are greatly reduced, it is still difficult to justify how this approach will be successful in reaching the LCOE factor of two cost reduction. This is a well-funded effort by a major corporation, with a substantial cost share, and they apparently plan to continue this effort. The new "wing box" design offers potential improvements in cost through use of Alcoa aerospace manufacturing experience. However, it is difficult to see how their apparent range of anticipated cost reduction provides the needed overall cost reduction of the system. Based on this reasoning, and their manufacturing and design capability, this criterion is judged to be Good ("Most project aspects align with the EERE Solar Program and DOE RD&D objectives").

20% field cost decrease goal is very good. Very important to the goals of the Federal solar program. Also very laudable is a new approach to trough designs.

It is difficult to evaluate the contractor's estimates regarding cost improvement based on the information presented. Alcoa's experience with similar applications of their basic material would help calibrate the prospects for success. Technical intuition suggests the wingbox approach has promise.

New trough designs need to be developed and tested. This aerospace approach could offer substantial advantages in cost and performance if it is successful.

Project appears highly relevant to DOE objectives for PT and sharply focused on PT cost reduction via novel collector design and materials.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Good ("Generally effective, but could be improved; contributes to overcoming some barriers"). Their Phase 2 approach includes: prototype design with a full CAD model, with FEA imposed loads to address stiffness, detailed design of the "wing box", prototype build, detailed fabrication and inspection, including a laser tracker, validation at Alcoa and NREL for wind loads/deflections (96 strain gages, etc.), optical performance (VSHOT), optical efficiency (SIMTA), and production cost modeling.

Good idea to consider new approaches to trough reflectivities, but this is not being done here, as application of commercial products is being done. However the abilities to develop a new approach that does not require special field alignment aspects is particularly good. The fact that the manufacturing technique is easily scalable to larger systems is a good characteristic.

The barriers are intuitively obvious, i.e. the failure of lighter weight structures to preclude wind related mirror surface deflections. The risk is that initial designs that have good cost potential will have to be modified based on measurements at the expense of cost savings.

The project was well planned and executed. Alcoa used significant company resources above the contract amounts to complete the prototype.

Approach is highly systematic, well-designed, feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments with respect to the "wing box" are rated Good ("Significant progress toward objectives and overcoming one or more barriers"). Details of the CAD, FEA, and optical performance are illustrated in sufficient detail to provide clear evidence of the degree of detail exercised in this design, fabrication, and test project. In particular, the optical performance of the "wing box" met the requirements (VSHOT, DO, etc.).

Alcoa has developed an optional trough design that successfully meets structural and optical performance requirements. There may be eventual issues that will need to be resolved. For example, although infrequent, rain and ice formation on structures can occur at many if not most of the potential sites. The Solar One Martin Marietta heliostat mirror assembly had a tendency to collect water into its enclosed substrate. This may be a potential problem with the ‘wing box”, in that cycling of moisture in and out of this design, with condensation, and possible freezing, could lead to damage and distortion of the reflector surface. This issue may need to be addressed at some point.

The basic design of the a new trough approach is laudable. It seems as though they have made excellent progress related to that. I am guessing that some of that was done prior to the contract, but nonetheless that is a good accomplishment. It is particularly valuable if it does, indeed, decrease field costs.

Progress reported includes conceptual design and prototype fabrication.

The prototype was completed and met or exceeded the company's own criteria. The one disturbing part of this project is the mirror accuracy; 90% collection in 25 mrad does not appear to be adequate for small collector diameters unless it meant to be used with a secondary concentrator.

Project appears on track and has met or exceeded project milestones to date.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Good (“some coordination exists; necessary coordination could be accomplished easily”). The project involves NREL, DLR, and a co-development relationship with Acciona for Phase 3. Technology Transfer is not shown; there are no papers or patents noted.

Seems to have participation with CSP Services (DLR) and NREL. Picking up some type of partnership with Acciona Solar, the nature of which is not clear in the presentation. No other collaborations are noted. No publications are noted.

It's concerning that two thirds of the funding is from DOE for a design that apparently is being developed to a single customer's (Acciona's) specifications. Acciona's involvement is a mixed blessing. Their experience is very relevant, and their interest is encouraging. On the other hand, DOE should not really be investing in a particular company's competitive advantage in this indirect, somewhat disguised way. If Acciona is to be helped, it should be directly not through the device of funding the development efforts of a captive potential supplier. The Program should reevaluate the current level of funding versus the overall benefits of this effort.

Coordination with NREL, DLR spinoff are good and prospects are good for Acciona collaboration in Phase III. Other PT vendors could be usefully added to the project.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area is relatively well defined in terms of the Phase 3 overall goal of installation, with Acciona (with in-kind cost sharing) and field testing with systems validation. They list seven tasks for the Phase 3 effort involving the “wing box” design refinements/optimization. There is some uncertainty, subject to their collaborative effort with Acciona. Overall, the plan to provide the “wing box” should be well-within Alcoa capabilities; although not stated, it is assumed that the design is congruent with Acciona requirements without undue modification and re-work. Overall, this area is judged to be Good (e.g., “Plans build on past progress and generally address overcoming barriers”).

The effort to build and install and test a new trough design based on Alcoa manufacturing and aerospace experience is good and the effort to date is successful in terms of meeting the optical and structural loads requirements.

In reading between the lines a bit, they will be taking this out for some kind of field evaluations in conjunction with Acciona Solar Power. Several tasks are slated for the test loop taking place with ASP.

The proof is in phase III and in this instance there is evidence in the presentation that technically valuable information and data will be shared with the industry, the public and DOE. I note that Phase III, with critically needed support by Acciona will provide visibility to technical - but not economic - performance. I have difficulty squaring this with DOE's interest in economic information needed to direct future program efforts and support the entire trough industry.

Apparently the module needs a better optical surface. Since the mirror cannot be adjusted it must be adequate as installed. Further optical characterization and solar testing should be undertaken.

Planning through Phase III appears excellent. Little evidence given of plans beyond.

Project Strengths:

They have developed a successful prototype trough with a "wing box" configuration that appears to offer some potential cost reduction. They have tested this thoroughly and plan to move this into Phase 3 tests with Acciona.

A new approach to trough design has been developed, and this design holds a great deal of promise.

Alcoa knows aluminum.

This approach could offer substantial advantages in cost and performance if it is developed successfully.

Very experienced contractor team, effective collaboration with NREL.

Project Weaknesses:

The major weakness is the lack of quantitative analysis that their approach, combined with other subsystem aspects, will indeed lead to the major factor of two cost reduction that is the stated need to achieve the DOE goal.

Perhaps my weakness: I don't have a good feel on how this really stacks up in a variety of characteristics with more traditional designs.

Lack of benefit to broader trough industry unless Alcoa will be free to offer the collector to customers other than Acciona. Be good to clarify this.

The slope error seems to large. The big question is can this type of collector be cost competitive. The meager cost benefits were only given in terms of reduction in LCOE. More needs to be done to determine mass production costs.

Recommendations for changes to the Project Scope:

An analysis showing that the LCOE improvements cited are achievable is needed.

Add manufacturing cost projection task to Phase III

Formal inclusion of other PT system vendors at some level would be useful.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP009
Presentation Title: Advanced High Temperature Trough Collector Development
Investigator: Dracker, Ray

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This Helio Trough program addresses several important DOE objectives, including, per the Multi-Year Program Plan: …technology development to lower costs, and exploring advanced concepts. However, as noted in several of these evaluations, there is a major barrier in that the system cost reduction must be a factor of two over the current cost, and the effort here is focused on the collector field, which is approximately 34% of the total LCOE. Thus, even a factor of two reduction in the collector field cost would not achieve the cost goal, and the anticipated cost reduction (or goal) for the Helio Trough design is not shown. However, they are also designing the collector for a higher intercept (approximately 0.9 achieved), higher tolerances, and for use with higher temperature fluids other than VP-1, and thus overall there could be additional reductions, as they assert, in the total system (less area, less thermal loss, lower pressure drop, etc.). However, this is not quantified. They state that the use of high performance laser-aligned jigs are an important factor in reducing costs by achieving the high tolerances required. They completed installation of a complete test loop (40 collectors) and are testing these fully integrated into the SEGS V system. With this degree of commitment, and a high level of cost share, together with a design that potentially meets the requirements for higher temperature heat transfer fluids, this Criterion is rated Outstanding ("Project is critical to EERE…and fully supports DOE RD&D objectives").

This project will improve plant performance by improving the precision of manufacture and increasing the aperture. This is one of those projects that seems to be moving incrementally (seemingly VERY incrementally) along the development curve. I am not convinced that this advances the state of the program objectives very far.

Cost sharing percentages are not indicated and would be relevant to evaluating the benefits of the work to DOE's program.

Initial field experience with troughs using molten salts for heat transfer is a critical path issue for the program.

Developing lower cost, higher performance collectors is key to achieving DOE's cost goals for this technology.

The presentation describes the DOE objectives but inadequately specifies quantitative goals for the current project relative to those objectives, merely saying, e.g., "capital cost reduction is critical...."

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"). They are in the process of developing a complete evaluation for a relatively large loop of 40 collectors, at SEGS V, as the step towards a full system test.

In my opinion the objective of this work, other than focusing on bringing down costs, are not very clearly defined. To refine the objectives a little, they are making a precisely laid out through with a wider aperture.

The discussion did not emphasize technical barriers and perhaps should have addressed the concerns that came up repeatedly later in the review, i.e. the potential damage resulting from salt solidification in the receiver tube and the design provisions to avoid either or both freezing or damage. However, the presenter was considerably more forthcoming than others regarding the practical design considerations for the new collector, and of course failure to effectively deal with these considerations would leave barriers unresolved.

It is hard to tell how well the technical barriers were addressed from the information given. The performance data are sketchy. The field modeling capability needs to be improved as the graphs show several instances of the measured efficiency exceeding the modeled predictions.

The technical approach is evidently very effective in maintaining the project schedule and specified milestones.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments and progress are rated Good ("Significant progress toward objectives and overcoming one or more barriers"). They have concentrated the effort on the high tolerance collector and achieved the design, fabrication, installation, and some degree of testing. They had various obstacles to overcome, which are noted, but not in detail, such as the jig assembly in the field. There has also been some delays (August 2009 slipped to December 2009 and cost issues ("…the program is not on budget"). Overall, however, they have installed an impressive alternative design and are able to test it in a full scale field, with experienced personnel. This is a substantial amount of progress.

I have mixed reactions to this accomplishments noted in this report. I feel that the organization is probably making satisfactory progress as was outlined in their statement of project objectives. However, I do not feel that the objectives of this project are moving the DOE program that far forward.

Having quantifiable optical design metrics is refreshing. Meeting targets necessary for the collector to achieve overall design goals is a significant step.
They built the field and have it running. That is a good accomplishment.
However, they need to provide more performance data to be able to judge the success of the approach. Only in the Phase 3 future work do they reveal that they are using a secondary reflector. The design should have been explained and illustrated in the beginning of the presentation.

Design and construction progress appears very satisfactory. However, progress toward the DOE goals was inadequately quantified to judge overall performance properly.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Good ("some coordination exists; necessary coordination could be accomplished easily"). The project involves Solar Millennium as the lead, with NREL and Sandia, and to some degree, Flagsol. Later, in Phase 3, this collaboration with Flagsol

would be more extensive, and likely involve molten salt.

Indicates some unspecified collaboration with NREL and Sandia. Also indicates collaboration with NextEra and Schott. No other collaborations noted. Does not note any publications.

There appears to be good collaboration within the Solar Millennium team and its component suppliers. The strategy is apparently incremental changes to the basic metal/glass collector used previously by Solar Millennium, so presumably existing vendor relationships are coming into play.

They should have more involvement with the National labs for advice and independent verification of the performance of the approach.

This is a proprietary development effort.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area is relatively well defined in terms of the Phase 3 overall goal of installation, with Flagso and field testing with systems validation. They give a relatively complete list of task, but there is no schedule. Overall, this area is judged to be Good (e.g., "Plans build on past progress and generally address overcoming barriers").

Their efforts to build and install and test the 40 collector loop are commendable. There is at least a potential capability to achieve higher HTF temperatures and lower overall parasitic losses (pressure drop, etc.), with this approach, and use of high tolerance jigs may be part of the solution.

Focus on significant improvements is so obvious to this reviewer.

The plan to adapt the collector to salt operation at an existing SEGS plant as Phase 3 makes sense and seems to result in a relatively aggressive overall schedule. However, details and issues of the salt adaptation are not discussed.

They gave a list of tasks but no explanation for the motivation. What does "Evaluation of incidence angle modifier" mean?

Planning through Phase III appears reasonable.

Project Strengths:

They have developed a successful prototype 40 collector loop and installed it and are in the process of conducting tests, in addition to various optical and loads development tests. They have Flagsol as a partner to help move this into Phase 3. They are contributing about 80% of the DOE funding as their cost share.

Involves a major supplier of trough collectors in the DOE program.
Experienced team tackling first step in incremental system improvement, a collector optimized around molten salt heat transfer.

Getting the task done.

Well qualified project team.

Project Weaknesses:

Does not move the solar program very far along, even if the objectives are exceeded to some degree.

There is no reference to system studies that would provide context and guidance to the collector specification.

Not talking about it with real information.

Inadequately specified objectives in terms of DOE Solar Program goals.

Recommendations for changes to the Project Scope:

Needs to push the state of the art/science of trough systems in ways that only Solar Millennium could define.

The project results will be used in a plant Solar Millennium is developing. DOE should provide continuing support for design refinements until design for the commercial plant is frozen.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP010
Presentation Title: Development of Advanced Polymeric Reflector for CSP Applications
Investigator: Smilgys, Russell

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This project addresses several important DOE objectives, including, per the Multi-Year Program Plan: technology development to lower costs and exploring advanced concepts. …transfer R&D concepts…to the market place…assist U.S. industry and reducing barriers to market penetration. If successful, some decrease in LCOE could result. They have developed a polymeric mirror and conducted some tests that show it survives with virtually no loss in specular reflectivity the equivalent of 30 years of scrubbing. However, the potential for improvement, and likelihood of long term survivability, are not quantified, nor are potential reductions in installed system cost shown, even if the goal of a low cost, durable high reflectivity polymeric mirror is achieved. In addition, their own cost estimates show that this approach is essentially the same as current polymeric mirrors. This Criterion is rated Good ("Most project aspects align with the EERE Solar Program and DOE RD&D objectives").

Advanced performance, low-cost coatings for reflective surfaces are quite pertinent for advancing the goals of the solar program. This project seems to be very closely aligned with DOE goals.

Little or no analysis is offered to support the claim that use of the film reflector will result in achieving DOE goals. Specifically, Abengoa, a member of the team does not affirm this claim in their presentation. The development is in the context of Abengoa interest in using the film, but there was little indication of Abengoa's active involvement in the project nor is there an indication that Abengoa is developing a collector that would use the film reflector which in Abengoa's collector development project is referred to as an "advanced concept". Abengoa is cost sharing but does that mean no other customer will have access to the film reflector being developed here if the development is successful.

This is relevant to the program providing it can lower costs and meet reliability requirements.

The project supports DOE objectives, but its goals are not stated quantitatively enough to judge how effective it may be.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Good ("Generally effective, but could be improved; contributes to overcoming some barriers"). This project determined that the scrub-abrasion test did not cause loss of reflectivity, but the equivalent number of washings was not stated, and therefore the "30 year" equivalent is not substantiated. It is not clear what the substrate is and how this new film will be bonded to it. No mention is made of such issues as UV degradation. In effect, they have succeeded in their primary objective of developing a polymeric mirror with high reflectivity and a protective layer of aluminum oxide. However, it is not clear how this will be incorporated into the mirror module/collector. They have partially demonstrated that the overcoat of aluminum oxide has met some of the goals, but it seems that additional work is needed (integration, test, exposure, etc.)

Other than reflectivity and scubbability, few insights about the application are indicated.

The emphasis seems to be on reflectivity and abrasion resistance. What about reflectivity over 20 or 30 years? What about cleanability? What about the substrate and adhesion to it?

It is difficult to evaluate this project. It seems that SAIC has contracted with Marian to use its roll coater to produce rather standard films with an aluminum oxide top coat. This hardly seems like a half million dollar effort. It is not apparent what R&D was performed or what the money was used for.

At one point the presentation states the costs of the film are comparable to other coatings, then that the approach will lower the LCOE. These two statements were not reconciled.

Approach appears effective. More quantitative cost goals would be useful.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishment with respect to the polymeric film is essentially that of having developed the E-beam/ion source vacuum deposition process in an un-stated, but apparently 12" width,

which would be scaled up to 24” widths in Phase 2, with the presumed ability to scale this further to 48” sheets. The costs of this approach are stated to be comparable to other polymeric reflectors.

This effort traces back to an initial effort in 2003 under NREL sponsorship; to date, they have shown only one aspect of how this film will perform, and that is for the scrub-abrasion test. There are other factors of major importance, and for this reason, this criterion is judged Fair. (“Modest progress in overcoming barriers; rate of progress has been slow”). However, the reasons for this progress may have little to do with the basic design and their capabilities, and be based primarily on priorities and limited funding. If so, these essentially programmatic barriers were not mentioned.

Sound like intellectual property and business view differences between partners is a big issue on this project. It could end of delaying progress. Otherwise, involvement of several organizations is technically going quite well.

Encouraging reflectance and abrasion resistance test results are presented. Both represent significant barriers to success of film mirrors.

There needs to be far more testing and independent validation of the film performance. There should also be outdoor and uv testing. The viability of the film was not convincing enough to go to the next phase.

Progress toward demonstrating durability and optical performance is impressive.
What is potential cost impact?

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Good (“Some coordination exists; necessary coordination could be accomplished easily”). The collaboration is between Abengoa, SAIC, Marian Swisher and Associates, and NREL. This in itself is sufficient for a score of Outstanding, except for the requirement that there be technology transfer. No publications are cited.

NREL is involved. It seems that it is essentially a formal part of the effort. Otherwise, it appears that only the principals are involved. No papers or presentations are noted.

The project seems to be highly collaborative, though as noted above the level of actual participation of the various collaboratives is not clearly visible. NREL involvement in independent third party tests is a plus.

The explanation if the level and type of participation by Abengoa (other than providing matching funds) was completely missing.

This is a proprietary development effort. More rapid industry impacts would possibly result from a more collaborative approach.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area is judged Fair ("Plans may lead to improvements, but need better focus on overcoming barriers"). They note that there is a need for addressing certain risks (scaling up the beam source, for example). However, the work does not address the broader needs for testing this material bonded to at least one if not several types of substrates, together with a variety of laboratory tests for integrity and life, as well as long term and/or accelerated exposure tests. There is no schedule, Go/No Go decision points, definition of specific tasks, Gantt chart, milestones, deliverables, or status. In this regard, the proposed future work is outlined, but not planned. Phase 2 and 3 are essentially reduced to a few bullets on a single page.

The effort would be more directly useful to the DOE goals if it addressed the full module and a complete range of tests. They apparently have sufficient material to at least conduct tests in a Weatherometer, of simulated rain (at different pH levels, etc.) and accelerated UV exposure of the polymeric material, and preferably, of coupons simulating the module design with the different substrates, adhesives, etc.

the measurements were quantitative, in terms of specular reflectivity, potential cost improvements were developed for the material as deployed on the module, and life was determined, for example, in the number of years of high performance, leading up to potential replacement.

Apparently the next phase of the work will take the product development to a 48" size and initiate outdoor testing. Other than reflectance tests and abrasion tests, no failure mechanisms are being investigated.

The next steps seem to emphasize transfer of technology to production and field evaluation. What is missing is the critical issue of accelerated life testing. The product has no chance in commercial use if the customer, presumably Abengoa must base its decision on relatively short term degradation results with a single non-production batch of material. The lack of attention to accelerated life testing, not to mention criteria against which to test, is a concern.

This approach needs to be independently vetted before investing further funds for scale up.

There is tantalizing suggestion regarding self-cleaning coatings in future work,
but no specifics were offered.

Project Strengths:

They have demonstrated a high reflectivity film with some potential for durability, based on maintaining the reflectivity over the course of simulated soft brush scrubbing-abrasion tests.

High quality reflector approaches are being examined. Great abrasion resistance seems to be present in the product being developed.

The presentation was clear and team and related collaboration appears to be strong notwithstanding a question about the seriousness of Abengoa's interest and its rights as a potential customer.

Well-qualified team. NREL collaboration.

Project Weaknesses:

The project lacks specific data on known or projected performance, life, or cost for the module that could be used in cost models to assess whether or not the material developed, if successful, is appropriate to meet LCOE goals.

Seems like some other failure modes might be used to evaluate durability of the material.

Also and especially, see comment above regarding accelerated life testing.

The viability of the film was not convincing enough to go to the next phase.

Closed IP development.

Recommendations for changes to the Project Scope:

Analyses showing the potential cost reduction and its effect on LCOE are needed. Tests are required that cover such areas as UV, pH, temperature and humidity cycles, peel tests, etc.

Would be good to compare performance relative to competing products. The latter do not have to be named specifically. However putting this in proper perspective would be quite valuable. If Marian will be the eventual manufacturer, then it would be good to have a task identifying the product qualification process they would use and their initial estimates of price (not cost!!) to collector manufacturers.

This approach needs to be independently vetted before investing further funds for scale up.

The stated need for contact cleaning is troubling. There was no mention of whether this type of cleaning is something that the solar industry is willing or capable of performing or how the cost of this procedure will affect the LCOE.

Also the problem with the brushes carrying material from previous cleaning that badly degraded the film cannot be fixed by continuously cleaning sub-micron particles from the brushes. This issue must be addressed.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP011_
Presentation Title: Brayton Solar Power Conversion System
Investigator: Kesseli, Jim

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The Brayton Solar Power Conversion program addresses essentially all of the primary DOE objectives, with the exception of the focus on thermal storage. The Multi-Year Program Plan objectives include: …. fully incorporate CSP efforts into the SAI, improve storage technologies, transfer R&D concepts…to the market place…and assist U.S. industry. The DOE focus is on technology development to lower costs, pursuit of thermal storage to enhance dispatchability (in this case, dispatchability is provided by a very novel, and potentially cost-effective approach), exploring advanced concepts, and reducing barriers to market penetration. Essentially all of these objectives are addressed through the basic design and aided by collaborations with SolarCAT, SNLA, NREL, and ORNL. There is a high cost share input from the prime. The Relevance Criterion score is Outstanding (Project is critical to EERE Solar Program and fully supports DOE RD&D objectives).

I feel that the investigation of a Brayton application to solar is a very desirable step in furthering the EERE Solar Program goals. This is a project that is moving the frontiers a great deal more along the improvement direction.

This is a project that cries out for independent systems and economic analysis. The idea seems to be to couple solar dishes to a CAES reservoir that is charged independent of the solar energy source using off peak grid electricity. Then, instead of feeding the compressed air to the combustion chamber of a commercially available utility scale combustion turbine, the compressed air is delivered to the focal zone of a dish concentrator where it enters a high temperature solar receiver to be heated and fed to a dish mounted Brayton turbo-alternator that of a micro-turbine gen-set the various elements of which are being developed for purposes of the project and which is somehow capable of operating on natural gas. The basic question is how could this possibly compete economically with a ground-mounted commercial micro-turbine adapted for CAES? Another question is how could the ground mounted micro-turbine based CAES configuration compete economically with configurations using larger, more efficient and least expensive combustion turbines? Not only does this concept require a minimum investment of a hundred million dollars or so to prove out and put the receiver and Brayton engine/alternator into volume production, it would appear to have no commercial potential at all relative to alternative configurations based larger commercial Brayton turbines. The fact that this project is being funded calls the technology development priorities of the whole program into question, unless of course the project has a purpose other than to develop a commercially viable system solution.

This project seems to be outside of the main goals of the EERE solar program. It involves solar in a way that may be considered peripheral. The concept uses off peak electricity to provide energy for the compression of air,(which it is assumed to be available at a very low cost - not supported by the presenters) that it stores in natural cavities (salt domes - not widely available except at the developers site). This compressed air is then routed to dish solar receivers (why not towers?) through an extensive system of pipes to individual dish receivers (that require separate development because of their large size). The heated air is then expanded through either a two or four stage turbine (choice not discussed) with a recouperator (a key element that is not discussed) to recover some of the heat. The high temperatures required to run the turbines could undoubtedly be provided at much lower cost with natural gas. This is an overly complicated system with very large demonstration costs (note the last supplemental slide shows current development costs at about $32 million dollars) that is nowhere near a realistic demonstration (the single dish collector is still under construction after two years).

The project nominally supports DOE Solar Program objectives; however, it's not clear that the specific applications of Brayton engine and CAES are truly commercially promising.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The past Approach to Performing R&D has been Good ("generally effective but could be improved; contributes to overcoming some barriers"), as seen in the Accomplishments. The approach involves emphasis on the receiver and PCU, with a Dish to be integrated later. However, this approach also could be used with a central receiver. Possible advantages include augmentation/retrofit with an existing combined cycle power plant, operating with the compressed gas, heated to a temperature acceptable for use in the existing gas turbine in the receiver. A combined cycle, especially with the heated air introduced into the gas turbine, would increase the total efficiency and energy converted to electricity by the solar field, and for moderate temperatures, not require that the existing gas turbine be modified at the gas generator or combustor. This concept would be congruent with one option developed for the USAID

funded/DOE managed “Noor al Salaam” program as part of a follow on effort to the U.S./Israel Science and Technology Foundation (USISTF) through the Department of Commerce in the 1990s. In short, a stand-alone Dish Brayton is one of several options that deserve consideration. It is also not clear that “testing the boundaries of scale” to reduce cost is workable, and in fact may increase costs as the Dish size increases. The receiver and alternator work is commendable, cavity heat loss tests at different angles, the use of a window on the receiver to decrease loss, and other such tests are particularly notable. Also, they have a recuperated/hybrid combustion rig, which is an important subsystem. Decoupling the compressor from the turbine is also a good approach, especially with the night time, low cost power availability. Another strength is that access to salt dome cavern storage has been secured. In several respects, this concept is somewhat similar to that conducted by the Israelis (Weizmann, Rotem, etc.) and McDonnell Douglas and later Boeing and UAHuntsville; there may be technical synergies from this prior work. Overall, this is a novel, potentially low cost and practical approach that avoids a number of cost and technology issues associated with thermal storage. Although there are other versions of this basic approach that are not being addressed by this project, their planned approach is a good start.

What is not very clear is the exact nature of the upcoming effort that is stated to be completed by January 2011; there is apparently no plan to conduct DOE funded effort beyond this date. This plan is only mentioned at a top level. Past accomplishments with the receiver, turbine, and partial work on the Dish give credence to the ability to perform this function. However, there is no task breakdown, milestone, or Gantt chart, and it can only be surmised that there will be an integrated test with the man-made storage system; tests with the salt dome are unclear. Given the level of specificity needed for a peer review of the future planned work, this area is lacking. However, the work accomplished to date is outstanding, and it can be safely assumed that good work will be accomplished, but the lack of any level of detail for 2011 and beyond lowers the overall score from an otherwise Outstanding to Good.

This project is interesting because it appears that DOE is getting quite a lot of bang for the buck. Not only are Brayton machines being developed, but there are other aspects of the project that are moving forward with this work. Included is a large dish with an air heater receiver, as well as the development of an approach to CAES. Both of these other aspects are something that should be examined. Of course, it is not necessary to drive the Brayton cycle with a dish, but it certainly is a good thing to examine. The partnership crafted for this work is able to provide a very significant amount of cost share...may be among the highest totals in the program.

It is not in my opinion commercially promising. See above.

This concept, while in the category of a "neat idea" has not been thought out in terms of realistic financial requirements for solar thermal systems. The complexity and cost of the totality of the subsystems has not been considered. The receiver was tested and claimed to be high efficiency with internal heaters when the receiver was in a face down condition without wind. The realistic receiver losses will be much larger than the ones reported. There is no consideration shown of the costs of piping and insulation, thermal losses in the piping, flexible high temperature swivels to connect to the receiver, reliability of those flexible joints at near 900C, the low thermal mass of the air, the pressure losses in the cavity storage (including pressure losses due to the air cooling), cost of dish development, wind survivability of the large dishes, and so many other important aspects of the system.

Technical approach appears effectively designed to address project barriers.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments have been Outstanding, in that the main technical barriers are being addressed (receiver, window, turbine, alternator, air bearing, Dish, etc.). There is also a high degree of commercial potential, in part because of the extensive storage potential. . There are a few late items, but these can be overcome. Overall, accomplishments and progress have been Outstanding (Excellent progress toward objectives; suggests that barriers(s) will be overcome).

Apparently the dish development (not part of the funding for the Brayton aspect of the project) is moving more slowly than would be preferred. This is the down side of having a multi-faceted project, not all of it funding by the same source.

The overall presentation is reasonably clear and technically substantive, and some quantitative metrics are referenced and related data presented.

Poor planning started with proposing the concept before a suitable dish was available since this appeared to be a major cost component. I saw no realistic results of turbine testing, only what appeared to be a thermodynamic calculation with overlaid claims of high achieved efficiency. There are many un-addressed problems involving the receiver, what alloys what maximum temperature, hot spots due to dish characteristics, convective losses, and the size, weight and cost of the receiver. There is no mention of the size of the cavity for compressed air storage, so I do not know whether it is suitable for a 1mW, 10mW, 100mW or 1000mW project. For any system tests this must be specified to proceed.

Progress to date appears limited by the lack of a suitable parabolic dish to demonstrate the other components. Component development otherwise appears on track.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Good ("Some coordination exists; necessary coordination could be accomplished easily"). The project involves industry (two solar power system developers), NREL, ORNL, and SNLA. No technology transfer is noted; it is assumed that there are various reports, presentations, technical papers, and possibly patents associated with this, but none are noted. It appears that funding for the pilot plant has not been secured.

Good partner in the work in the sense that a number of other aspects are brought to the table that greatly broaden the thrust. Coordination is not doing as well as it might have in some of the timing aspect.

Collaborative relationships appear to be appropriate and extensive. Collaborators may not, however, have reason to question or be concerned about the likelihood of commercial success of the overall system. They may be content to have the opportunity to ply their skills and apply their tools and products.

While SNLA and NREL are mentioned as partners, no other evidence of their involvement was supplied.

This is a proprietary development project and it appears to have limited vision of and participation in the broader EERE solar program.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity and appears to involve a modest amount of additional DOE funds for phase 3. The presentation does not show an effective plan for future work, and there is a degree of uncertainty associated with the larger scale deployment associated with a pilot plant and follow-on commercialization (especially, funding and schedule); this uncertainty is not addressed by, for example, alternate pathways, such as potential use, or at least tests, at a central receiver test site (SNLA, for example). The degree of effort (personnel, labor hours, major tasks, schedule, etc.) is not discussed. This area, at a minimum, should be addressed at least to the subtask level, if not a list of milestones and a Gantt chart. There are no decision points noted, or discussion of barriers to the realization of the proposed technology. The Brayton work and the novel storage approach, possibly with augmentation/retrofit, could be a very important and cost-effective approach to market entry. Their ability to conduct work of this nature is unquestioned, and the personnel are exceptionally accomplished and experienced. However, based on the accomplishments, the very high leverage of DOE funds, and the market potential, this area is judged as Good ("Plans build on past progress and generally address overcoming barriers").

The past accomplishments and capabilities of the personnel are outstanding. Overall, this a particularly notable project that deserves more consideration and has the potential to provide a high conversion efficiency at low cost; however, the planned future effort, especially beyond January 2011 is unclear.

Seems to be headed toward a very large range of end results, and the integration of all these aspects will not only benefit directly the Brayton development, but it will also benefit a number of other aspects that could be quite valuable to the CSP program.

It would seem to me that the cost goal of $1000/kWe is wildly unrealistic and therefore irrelevant to design choices that may have to be made as the project proceeds.

I can't see a realistic next stage to this project without very large additional funding (which I don't recommend).

Plans through Budget Period 3 appear good. Little evidence presented for longer-term planning.

Project Strengths:
Outstanding concept and excellent past work.

Development of a pertinent Brayton cycle machine as well as dealing with a number of other aspects that could have good application in the CSP program.

The project is imaginative and entrepreneurial.

Experienced engine developer.

Project Weaknesses:
There is essentially no plan shown; there are some uncertainties associated with how a system of this type will be treated in terms of financial, tax, and other such economic aspects.

The allied parts of the program (not funded by DOE) may slow the progress on the Brayton portion of the work (funded by DOE).

Possible inability of the project team to objectively compare their solution with commercial options having the same functionality.

Lack of coordination with mainstream CSP RD&D efforts.

Recommendations for changes to the Project Scope:
Consideration could be given to alternative to approaches, of which at least one has been under development for over 20 years (USISTF, McDonnell Douglas/Boeing, Weizmann Institute of Science, UAH, Rotem, etc.). This work involved a volumetric quartz receiver that has been extensively tested.

Shift to assessing which project tasks have relevance to more promising system solutions.

Terminate the project without spending any more tax payer money.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP012
Presentation Title: Indirect, Dual-Media, Phase Changing Material Modular Thermal Energy Storage System
Investigator: Newmarker, Marc

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The dual media, phase change program addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel concept that uses the molten salt with phase change, coupled with a heat exchanger. In principle, it could benefit from parallel efforts to improve molten salt properties. However, there is very little information provided by which to judge the potential, and there are a number of problems associated with ensuring that the solidified salt is removed from the heat exchanger surface and that no damage is caused. They claim that the FEA model shows that the system can store heat in a volume that does not exceed that of a conventional two-tank system, but this is not shown in any detail, and is thus unsubstantiated. Thus, it is not clear how this can achieve a substantial cost reduction, even if the issues of phase change are resolved cost-effectively. The term "it is believed" is used to justify the concept, but there is virtually no substantiation; the SAM work is to be done later. Overall, this Criterion is rated Fair ("Project partially supports the EERE Solar Program and DOE RD&D objectives").

Phase change storage, if successfully developed and demonstrated, could improve overall CSP economics, depending on related costs and performance.

No information.

Project aligns with DOE objectives.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Fair ("Has significant weaknesses; may have some impact on overcoming barriers"). The work to date and the upcoming effort for 2011 are noted as tasks, but virtually no information is provided on such effects as salt removal, or the difficulties this could involve, or what the fundamental mechanisms are (shrinkage/expansion, temperature gradients, temperature transients, physical removal by various means, etc.). Even the TRNSYS/FEA analysis is only mentioned, with the comment that it shows that the volume will not exceed that of the conventional two tank system. There is no treatment of the module design; it is even unclear what the size is, or if the additional material costs for the presumably metal enclosure exceeds that of a two-tank system. Granted, this effort has been underway since September 2009, but there should be more reported results. In the material provided, there are no real data, and only one figure shows projected results (TRNSYS projected 93% efficiency). This lack of information almost makes the R&D approach score lower, but since this work is being done by a respected solar company, the benefit of the doubt is given to them for their capability and thus the score is Fair.

Possibly because of the project's early stage, it appears no significant design or material choices have been made, no criteria have been established to evaluate options, and the project appears not to have even identified specific conceptual solutions to overcome the major obvious technical barriers, to say nothing of the more subtle ones. Certainly, if the project team is qualified to undertake the development that is underway, some technical insights could be offered to suggest promising approaches and the pros and cons of specific technical choices.

No information.

Technical approach appears effective, although many details of the design appear not clearly worked out to date and it is not clear that the contractor team has thoroughly mined the prior art.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments with respect to the concept are rated Poor ("Little or no demonstrated progress towards objectives or any barriers"). There are no accomplishments noted, and only vague comments about "…it is believed….modeling is promising…potential to test…", etc. The project is stated to be on "on track", but it is difficult to evaluate or justify how this can be the case if no real results are shown.

Top level goals are consistent with DOE program objectives, but metrics that would allow evaluation of basic technical choices are not revealed.

No progress reported.

Progress to date is limited to modeling and specific hardware details appear uncertain.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Poor ("Most work is done at the sponsoring organization with little outside interaction"). The project does not note the use of any outside organization. There are no presentations, papers or patents/disclosures noted.

No collaboration or coordination with technically qualified partners is mentioned.

No evidence presented of collaborations.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity. The presentation does not show an effective plan for future work, and there is a degree of uncertainty associated with the exact system size, layout, and performance, and no indication that there is indeed a cost reduction potential. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) are not shown; only top level tasks are shown. Some key decision points are noted, but there is little discussion of barriers to the realization of the proposed technology; this is especially important for the storage concept. The lack of a specific (not vague) plan that addresses those aspects of the future work needed to have this concept operate as desired, especially including alternatives to overcome such barriers as salt removal, makes it necessary to judge this area as Poor (e.g., "Plans have little relevance toward eliminating barriers or advancing the program").

The effort to date is vague, with no real progress shown in terms of either the design, or the potential for cost reduction. This work may have been done, but it is not shown.

Three phase 1 tasks are identified in general terms and seem appropriate.

Plans for current project are satisfactory, but longer-term is unclear.

Project Strengths:
Acciona is would seem to have reasonable motivation to investigate thermal storage and develop a solution consistent with their collector technology and project experience.

Experienced PT contractor team.

Project Weaknesses:
Virtually no results of a specific nature are shown; given that this project has been conducted since September 2009, this lack of results is a major weakness. Of the mere 14 slides presented, only one shows "data" and that is a TRNSYS result indicating 93% round-trip efficiency.

Nothing is revealed that would suggest Acciona has the competencies to successfully undertake the on-going project.

Lacks connection with potential collaborators.

Recommendations for changes to the Project Scope:
Quantitative results should be shown.

Based on the presentation, it is unlikely that any specific technical information will find its way into the public domain over the course or at the end of the project. Accordingly, DOE funding should be limited to tasks and sub-tasks that do not use or result in proprietary information.

The Program should reevaluate the current level of funding versus the overall benefits of this effort.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP013
Presentation Title: Heat Transfer and Latent Heat Storage in Inorganic Molten Salts for Concentrating Solar Power Plants
Investigator: Mathur, Anoop

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This program addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel concept that uses the molten salt with a simple phase change that in principle results in a pump-able slurry that can potentially improve the heat transfer at the water to saturated steam heat exchanger. They show an overall system schematic, with their emphasis on HX-1 (water passes through the HX-1 tubes and is heated by salt that solidifies into a slurry. They have examined many salts and some coatings, and have an initial selection. In principle, there could be some improvement in the heat exchange, but this is not shown, and therefore projections as to how this will improve the overall LCOE are not traceable. Also, it is unclear that pumping a slurry is practical over the long term, and that it can be done without a high degree of sensitivity to any conditions that would change the slurry characteristics or, at worse, cause solidification in the pumps or on the heat exchanger. It is not evident that there is a real cost advantage, even if they are successful in finding the right eutectic, coating, and components (pumps, pipes, possibly also requiring anti-stick coatings, etc.).

Overall, this Criterion is rated Fair ("Project partially supports the EERE Solar Program and DOE RD&D objectives").

Uses an innovative approach to both increase the heat transfer rate (25x-100x). Also decreasing the storage volume. Finally a non-stick coating will be adopted. All of these aspects could be quite valuable to the CSP program.

Superior technical content, clarity and relevance. Presentation was thorough, understandable and specific as to design and material selection criteria. Combined with parallel efforts the work will result in better overall assessment and development planning for phase change storage.

Supports DOE goals to develop lower cost storage system for towers. Since towers are now part of program this is presumably an element of the program (since towers were not in 2008 program description).

Project aligns with DOE CSP goals and presents quantitative potential benefits.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Fair ("Has significant weaknesses; may have some impact on overcoming barriers"). The work to date and the upcoming effort for 2011 are noted as tasks, but virtually no information is provided on how or why the salt eutectic and additive prevent adherence to the outer wall of the tube, or if there are any pumping issues associated with this slurry, or the difficulties this could involve, or what the fundamental mechanisms are (shrinkage/expansion, temperature gradients, temperature transients, physical removal by various means, etc.). At various points, different metrics are stated (25X vs. 100X on heat exchange, 33 vs. 31 down-selected, and 3 and 5 met criteria). There is no explanation of how the heat transfer coefficient is determined or what the flow rates are at the heat exchanger surface that are needed to achieve the increased heat transfer rate, and how this compares with the stated objectives (25X or 100X, it's unclear) or how these objectives were determined. The system improvements are asserted, but not substantiated (20 to 30% reduction in salt, 30% reduction in specific cost of the TES, 60% reduction in container size (which is not described, and may only apply to HX-1, but this is unclear, and may imply that it's for the entire salt system), 20-3% improvement in efficiency, with a resulting decrease in LCOE of 6 to 9%. Lack of substantiation for these assertions is a major weakness. The goal of less than $20/kw-hr should actually be $15, and since it's currently stated to be about $40, it's unclear how they achieve this with a 30% reduction. Thus, overall, the rationale is not well supported. Down-selecting the salts from a literature search, together with a qualitative test for "sticking" and down-selecting the coatings is practical and a necessary first step. The potential advantage of lower salt phase change temperature allowing additional heat to be captured during low solar irradiance levels is notable, but the 0.6% stated efficiency is a minor improvement. They showed a number of figures of concentration profiles in the tanks and indicated, but did not explain, the "intelligent controller". They showed the various salt morphologies vs. temperature and mixture, indicating that the slurry is achievable. They also showed some SEM results for different salt morphologies. However, the results shown do not support the improved heat transfer rate, reduced size and volume, or several other of the assertions (anti-sticking, etc.). The primary concern is that by not showing that the HX-1 improvements, even if achievable, lead to the stated improvements in the overall system, it is difficult to judge that this approach is correct. In short, they lack sufficient justification and substantiation.

Conceptually pretty well laid out. The devil is in the details. Quantifying the stickiness sounds extremely ad hoc. This needs some better focus than was described in presentation. Another aspect is the assumption of the existence of a slurry, and the importance of that not totally solidifying the mix. If the latter can be realized, that is quite valuable. I am concerned about if this can really be achieved, but if it can be, that is great.

Attention to coatings, stratification, additives and slurry pumping show evidence of attention to practical design considerations.

Their approach seems to be increasing the heat transfer coefficients from 25 to 100 times existing systems; this seems like a very aggressive goal. Most of the improvement is to come from developing a "nonstick coating for salts" at high temperatures, developing scrapers to peel off the salt, bubbling air, and finding a eutectic mixture with good properties for pumping back to the thermal storage tank. No progress was reported in these areas. The approach seems to be to have subcontractors do the experimental work while Terraform develops models and evaluates possibilities.

Technical approach appears systematic and well-designed to address barriers.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments with respect to the concept are rated Fair ("Modest progress in overcoming barriers; rate of progress has been slow"). They state that 100% of Phase 1 has been accomplished, but this is unclear, as stated, and may be their goal. The primary accomplishments described appear to be a literature down-select of salts, a further down select to 3 of these, and 2 coatings selected from 13. They also note that an intelligent controller for thermocline control is one accomplishment. There are some thermal analyses of the thermocline, and some treatment of the morphologies (SEM, etc.). However, the main objectives of reducing cost by improvement in heat transfer, although asserted, are not substantiated.

Not really clear how progress measured up to goals. It appears that the first may have been little more than doing some background work and preliminary screening. Seems as though a very large number of tasks are outlined for 2010. I don't have a good feel for what criteria will be used to evaluate progress. Many of the tasks sound quite open ended.

Phase 1 objectives addressed technical barriers and were achieved.

They identified candidate salts in the first phase of the project. They also identified candidate surface coatings (through the collaboration with JPL?) but are the coatings good for all salts they are considering? No measurements of this important aspect were reported. The experimental setup they describe looks like something I could set up in my lab in two hours and indicates no means to make the required measurements. They also show several SEM micrographs but do not give any indication of what they are or why they were made (salts? surfaces? eutectics at elevated temperatures? these might all be interesting but they appear to be window dressing). Elaborate temperature diagrams within a storage tank are provided but no indication of the type or development of the thermal model are given (the color bar at the bottom has no relation to the diagram).

Progress to date appears satisfactory.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Good("Some coordination exists; necessary coordination could be accomplished easily"). The participants are stated to be: Terrafore, UCR, PWR, JPL, and the Univ. of Minnesota. However, although a matrix shows the roles, the roles are not described; no breakout of the budget or deliverables for the participants are shown, and only one individual (Venkatasetty) is named. Apparently some kind of collaboration with Sandia is planned. Two patent filings/disclosures and three presentations are noted.

Has a number of partners: industry, laboratories, and universities are involved. This seems to be a very nice balance. Not clear how they are all coordinated even through there is a matrix given for each of the tasks. Each of the tasks has at least two of the participating organizations involved, and a couple have even more (reporting is, of course, expected to find all organizations involved).

The chart showing issues and team member competencies and responsibilities is an excellent way of demonstrating collaboration, and the team included diverse and complementary organizational capabilities and cultures, effectively organized.

There is very little indication of inputs from the partners. The "Cash Cost Share: $395K. 28% by Terrafore, 20%each by UCR, PWR"; I am quite surprised that the Univ. of Calif. Riverside contributed ~$80K in cash to this project- how did this come about?

Contractor team appears to provide good collaborative leverage.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Proposed future work is listed as goals/deliverables with end dates, which is good. The presentation does not show an effective plan for future work, and there is a degree of uncertainty associated with the exact system size, layout, and performance, and little if any quantitative evidence that there is indeed a cost reduction potential through the increase in heat transfer and the eutectic mixture. The degree of effort (personnel, labor hours, major tasks, schedule, etc.)

are not shown; only top level tasks and due dates are shown. Some key decision points are noted, but there is little discussion of barriers to the realization of the proposed technology. The lack of a specific plan, in addition to top level task-due dates, that addresses those aspects of the future work needed to have this concept operate as desired, especially including alternatives to overcome such barriers as salt removal, pumping issues, etc., makes it necessary to judge this area as Fair (e.g., "Plans may lead to improvements, but need better focus on overcoming barriers").

The effort to date is provides a credible, albeit unproven, optional salt mixture, and a possible anti-stick coating. But, quantitative evidence or analysis showing that the heat transfer improvements are real, and that the LCOE cost reductions occur as a result of these optional mixtures and the concept of flowing the salt across HX-1 (their current emphasis) are lacking. This work may have been done, but it is not shown.

In general, it sounds quite well fleshed out, but the tasks are sufficiently defined in a fuzzy manner that it seems difficult to judge if they are carried out successfully or not.

Not a lot of detail on future work but high confidence in planning based on Phase 1 results.

With the lack of current progress I find it surprising that the following:

• Build a scaled model experiment by July 2010
• Conduct tests with coated tube heat exchangers between July and December 2010
• Quantify adhesion properties of coatings before August 2010
• Characterize salt morphology near and during freezing by October 2010

can be accomplished by the stated time targets.

Planning through project term appears good.

Project Strengths:

Some results of a specific nature are shown (exact mixtures and coatings from the down select are not shown, but is apparently evident to them).

Looking for phase change substances that only form a slurry can be a valuable search.

Team effort apparently well-coordinated.

Diverse, experienced team.

Project Weaknesses:
No quantitative results supporting the LCOE reductions are shown.

Not clear how the parts will actually be performed and, more importantly, assessed.

None noted

Recommendations for changes to the Project Scope:
Quantitative results for LCOE reductions need to be developed and reported.

Important for team to evaluate the results of other projects addressing phase change storage on a real time basis with the opportunity to change course if there is reason to do so.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP014
Presentation Title: Innovative Application of Maintenance-Free Phase-Change Thermal Energy Storage for Dish Engine Solar Power Generation
Investigator: Qiu, Songgang

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The Phase-Change Thermal Energy Storage for Dish Engine Solar Power Generation program addresses essentially all of the primary DOE objectives, including the focus on thermal storage. The Multi-Year Program Plan objectives include: …. fully incorporate CSP efforts into the SAI, improve storage technologies, transfer R&D concepts…to the market place…and assist U.S. industry. The DOE focus is on technology development to lower costs, pursuit of thermal storage to enhance dispatchability (in this case, dispatchability is provided by novel integration with the dish engine, which may be a potentially cost-effective approach), exploring advanced concepts, and reducing barriers to market penetration. Essentially all of these objectives are addressed through the basic design and aided by collaborations with Penn State and Thermocore. There is a 20% cost share input from the prime. Load following (intermediate power) and easier transitions and less power variation of the engine during cloud transients, etc., are additional advantages. Both of these can lower LCOE. High temperature heat pipes and high temperature phase change storage make this a possibly practical approach, and there may be some additional advantages in terms of flux distribution. The detailed models of the heat flux match "bulk" methods, and are credible. No special pumping, etc. is required, and for this size system, heat pipes should work at the various orientations; this aspect is easily addressed by Thermocore. LCOE with 6 hours storage is projected to be 11.6 cents/kw-hr, which is acceptable for intermediate power generation. The concept, use of a proven dish and engine and passive heat removal with heat pipes is a practical approach. However, the project is behind schedule due in part to protracted negotiations with manufacturing partners. In view of this level of practicability and competitive intermediate load power costs, he Relevance Criterion score is Outstanding (Project is critical to EERE Solar Program and fully supports DOE RD&D objectives).

Goal of adapting TES to dish-Stirling system is quite laudable. Even though this is adapted initially only to a 3 kW system, a successful concept will be quite valuable. Any kinds of solar power systems where storage can be adapted will be more favorably endowed than similar systems without storage.

Dish engine systems are an excellent option for community scale solar deployment, and even small amounts of thermal storage could provide significant economic value in addition to cost savings calculated on an unit energy delivered basis. It is really important that the CSP program develop an ability to quantify such economic value because SAM does not do it, and SAM driven design choices in some cases may lead to technical and economic sub-optimization. In any event the storage development here may be decisive in preserving a role for CSP in the community scale solar electricity market that may turn out to be the largest segment globally over the longer term. LCOE improvements, though perhaps not the main benefit of the design, certainly can't hurt if they are realized.

Concept will be an excellent improvement on dish Stirling performance.

Project quantitatively supports DOE objectives and appears to present potential to add dish-Stirling to CSP technologies with TES capability.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The past Approach to Performing R&D has been Good ("generally effective but could be improved; contributes to overcoming some barriers"), as seen in the Accomplishments. The approach involves emphasis on the TES (modeled, and under contract to Thermocore); the TES is integrated with the existing 3 KW engine, and will be integrated later. The TES concept and results to date are commendable; for example, the optical performance requirements can be relaxed, hot spots are eliminated, a high temperature salt has been selected, and sizing results are validated in part by "bulk analysis" methods. Integration with the enlarged dish should not pose any special problems, and no change is needed for the engine. Although the Dish system is not in SAM, it is anticipated that it will be, and then they can refine their LCOE, but their own analysis indicates that with storage, the LCOE will be substantially reduced. In short, this system should work as projected, and may achieve the cost goals for intermediate power.

I feel that the way the issue is being addressed is very appropriate. Seems as though all of critical aspects are being considered in a reasonable manner. The use of an appropriate phase change material with a heat pipe to put the heat in and take it out is very good.

There wasn't much emphasis on technical barriers, and obviously the path to a full scale system using the proposed phase change material is challenging in any number of aspects, and the scale up steps add another layer of risk. We can assume the technical barriers are being addressed, and they probably are, but sometimes it is important to admit what could go wrong and give yourself the opportunity to mitigate the technical risk as much as possible

Not much technical detail but design and approach seem very reasonable.

Technical approach is systematic and well-designed to address barriers. It appears feasible, but is relatively high in risk.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:

The accomplishments are rated as Good, in that the main technical barriers are being addressed (i.e., TES material, heat pipes, dish re-sizing, and resultant costs). There is also a high degree of commercial potential, in part because of the additional (up to 6 hours) storage potential. There has been a delay in terms of negotiations with manufacturing partners and a longer than anticipated fabrication schedule; this is not described in detail, nor are means for overcoming these barriers addressed. Overall, accomplishments and progress have been Good ("Significant progress toward objective sand overcoming one or more barriers").

Very well-conceived project. It is not a universal solution, but for their system it looks quite appropriate.

Seems clear the design is complete and ready to fabricate. The modeling results presented suggest reasonable attention to performance targets. It will be interesting to compare actual performance to model results.

Behind schedule for contractual reasons. Little information about the construction details or design but no reason to doubt they can complete the project.

Progress to date is limited to modeling and equipment design, but the project appears well on the way toward hardware building and testing.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Good ("Some coordination exists; necessary coordination could be accomplished easily"). The project involves Penn State (thermal and phase change analysis) and Thermocore (fabricate the TES module). Infinia will integrate the TES with the engine and dish and conduct tests. One patent has been filed. No other technology transfer is noted; it is assumed that there are various internal reports, subcontractor reports, etc., but no presentations or technical papers are noted.

Involvement of an outside consultant, industrial partner and university laboratory is a good combination. No publications (except patent application) has been put forth yet...perhaps it is too new for the type of project it is.
Team seems to be appropriate with each member contributing critical skills.

Collaborators are all subcontractors. Collaboration with Sandia would be well advised.

One patent application is filed. Contract team is diverse, but wider collaboration would likely be useful to accelerate progress and industry adoption.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity in terms of milestones, deliverables, Gantt chart, etc. The presentation does not show an effective plan for this future work, and there is a degree of uncertainty associated with the TES deliverable. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) is not shown. This area, at a minimum, should be addressed at least to the subtask level, if not a list of milestones and a Gantt chart. There are no decision points noted, or discussion of barriers to the realization of the proposed technology. The ability of Infinia and Thermocore to conduct the design, fabrication, and test of the TES, integrated with the dish and engine is unquestioned, and the companies are accomplished and experienced. They will refine the LCOE analyses as soon as SAM is available for dish engines. Based on the accomplishments and the market potential, this area is judged as Good ("Plans build on past progress and generally address overcoming barriers").

The past accomplishments and capabilities of these two companies are outstanding. Overall, this a particularly notable project that deserves more consideration and has the potential to provide a high conversion efficiency, coupled with a proven dish and engine, at a more competitive intermediate power LCOE.

Testing in the lab as well as on sun seem to be appropriate next steps. This will help in potentially developing a more effective approach.

The project is going through a logical progression of tasks.
The future plans look good as long as the subcontractors perform.

Plans through project term appear good.

Project Strengths:
Outstanding concept for providing extended power generation with a dish engine.

The addition of a thermal energy storage, particularly PCM, is a valuable step for dish Stirling systems. Looks like quite a good design concept.

Builds on prior work by Infinia and Infinia's design and test experience.

Qualified, diverse team.

Project Weaknesses:
There is essentially no detailed plan shown for future work, although at a top level it is clear that the main thrust is to build and test the TES with the dish engine, and this is well within the capabilities of Infinia and Thermocore.

It is currently only for a 3 kW system. I don't believe this is a major drawback, as one usually wants to look at a smaller system and then move later to a larger system. They are developing a large dish system, so this should fill this need.

Dish engine systems are complex and face numerous and diverse technical challenges, esp. with respect to long term reliability, productivity and availability. Difficult to bring all of the necessary skill sets into a start-up company, and this increases risk if there are weak areas, where out-sourcing cannot be successful given the limited purchases required for development work.

Recommendations for changes to the Project Scope:
This area lacks specificity in terms of milestones, deliverables, Gantt chart, etc. These should be

shown, including an effective plan for this future work, degree of effort (personnel, labor hours, major tasks, schedule, etc.) at the subtask level, list of milestones and a Gantt chart with decision points noted.

Might be good to have phase change material options at this stage in case of unforeseen problems with current choice.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP015
Presentation Title: Molten Salt-Carbon Nanotube Thermal Energy Storage for Concentrating Solar Power Systems
Investigator: Banerjee, D.

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This molten salt carbon nanotube program addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel concept that uses the molten salt nano-tube to increase specific heat and thermal diffusivity, so that less thermal storage material is needed, tank volumes are reduced, heat exchanger performance is improved, heat exchanger size and cost are thus reduced, and these improvements are obtained with only small percentages of nano-tube materials. The resulting overall reduction in thermal storage costs could be of the order of a factor of two. Thus, this Criterion is rated Outstanding ("Project is critical to the EERE Solar Program and fully supports DOE RD&D objectives").

This project is considering the economic impacts of increasing the thermal performance of thermal energy storage materials by use of nano-particle additives. Enhancing the thermal conductivity and specific heat of solids and liquids is a very good idea and will furnish important insights to the program. A project of this type is very critical to the EERE Solar Program.

The fundamental understanding of the phenomena that result in enhancement of specific heat and other thermal properties is at an early stage of development, so the relevance is good but not necessarily to near term objectives, esp. as the benefits to nitrate salts, where there is as body of field experience, are minimal or relatively modest.
This is potentially a very important development, leading to a significant decrease in thermal energy storage costs.

The project generally supports DOE CSP goals but does not quantify benefits in terms of potential LCOE reduction.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:

The Approach to Performing R&D is Outstanding. They have an initial systems analysis, conducted extensive tests for specific heat and thermal conductivity, with many different spherical and tubular nano-particles. They have conducted sophisticated molecular model simulations; these have the potential to allow the design of specific nano-particles. These studies indicate a first order reduction in costs (of the order of 40%) They have also demonstrated long term compatibility (over 5000 hours), and it appears that perhaps the nano-particles improve corrosion resistance. They list several Go/No-Go decision points that are appropriate, such as compatibility, meeting the property improvement goals, economic analyses, etc. They tested various methods for mixing the nano-particles and determined that aqueous solutions were preferred, and they determined that some additives produced the nano-particles; both approaches are potentially low cost.

PI seems to have some very good background in nano-particle effects on properties. The approach being used is quite good based upon my limited experience in related areas. Consideration of both the theory and the actual results is very good. Both specific heat and thermal conductivity need to be considered as originally proposed, in contrast to the advice given the PI by the NREL personnel.

The comparative evaluations are useful but only one dimension of a multi-dimensional puzzle. Technical barriers are not discussed but it's hard to accept that there are none.

Well run project, although quite a lot of money for a university program. Might consider some outside testing or collaborations to enhance effort.

The approach is very logical and focused on the technical barriers to enhancing performance of low-cost TES.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments are rated Good ("Significant progress toward objectives and overcoming one or more barriers"). There are a few issues that deserve further study, which they note, including dispersion, keeping the nano-particles in suspension, and pH control. They have made much progress in determining the specific heat and thermal conductivity of a wide variety of potential nano-particles and salts. The only issue is that since this approach can be used with dish engine and central receivers, it is important to consider higher temperatures. There results to date are in the range of about 300 to 500 C; extending these tests to about 650 C is needed.

There is a significant potential for commercial application, based on their preliminary systems analysis. They are to be commended for a comprehensive test program to assess the properties of a large number of options.

My interpretation of the results is that the gains in property values are not that outstanding and that the costs may be quite high to achieve them. Nevertheless it is definitely work that needs to be performed, as my interpretation may turn out to be in error. One of the milestones in 2011 is focused on economic evaluation, and this should give more insights.

Improving heat transfer and heat capacity without significantly increasing cost would advance DOE program objectives. The project confirms the potential to do so without identifying exactly how.

Good progress toward objectives In the next stage it is critical to establish whether there is long term stability of the nanoparticle suspensions. This may be a very difficult problem and must be addressed before any significant deployment of this approach.

Results to date show promise for significant alteration in TES media with potentially low cost impact.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Good, even though they do not have a collaboration at this stage. The do, however, have about a dozen graduate students (majority of whom are PhD candidates) and they have extensive Technology Transfer, in the form of papers/presentations (8 are noted). It is surprising that no patent applications are being filed; their work appears to be suitable for at least one patent. They are also to be commended for providing extensive detail in a total of 57 slides, far more than the usual peer review material presented for solar CSP.

In the presentation it was noted that there are no collaborations going at this time. However, my guess is that there will be some (based upon the many contacts the PI has as listed in the supplementary material) and I have no question that several papers will result from this work.

Seems to me there could be more collaboration especially with industry but also with other university research groups that may have answers to some of the fundamental phenomena questions.

Needs to develop more collaborations with solar industry and national laboratories.

A university project such as this one could easily encompass more a collaborative approach. The PI says he has none. However, there are 8 listed publications.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity. The presentation does not show an effective plan for future work. The specific level of effort and tasks normally expected for R&D project is not shown; for example, they need to show aspects such as the key personnel, labor hours, major tasks, schedule, deliverables, Gantt chart, etc. However, the key objectives, tasks, and decision points are noted at a high level; given that they have conducted a great deal of work in a relatively short period of time, it is evident that they will be able to continue the R&D. Even with the lack of a detailed plan this criterion is judged to be Good ("Plans build on past progress and generally

address overcoming barriers.”).

The effort to analytically and experimentally determine the properties of the nanofluids is being done exceptionally well.

The future work is a little fuzzily laid out, but that is somewhat to be expected in a project like this that is covering quite uncharted waters. The PI will be building a test loop and performing an economic analysis, and both of these things could yield extremely valuable insights.

Future work as outlined in Chart 16 seems appropriate.

The strong emphasis should be on measuring and enhancing the stability of the suspensions or finding a way of replacing/recycling the nanoparticles.

Plans through the contract period appear good.

Project Strengths:
This is a promising approach with significant potential to reduce costs relative to conventional thermal storage systems.

Exploring a relatively uncharted area that could have quite positive implications for the EERE Solar Program. Some benefits have already been demonstrated.

Promising area of investigation

Qualified investigator.

Project Weaknesses:
The only weakness, judged to be minor, is the apparent lack of a well-organized, specific plan to conduct the future work.

The major weakness is implicit in any new research area, and it just part of the way things go in that situation. It is not clear that a real benefit to the CSP program will result.

Need for better understanding of phenomena as well as material and property stability and life cycle issues, including cost.

Possible lack of beneficial coordination with mainstream CSP program.

Recommendations for changes to the Project Scope:
A specific effective plan for this future work, degree of effort (personnel, labor hours, major tasks, schedule, etc.) at the subtask level, list of milestones and a Gantt chart with decision points noted.

Focus on the fluids that are in use or will likely be put into use before nano-particle enhanced performance is demonstrated. In other word put more emphasis on what can be done to enhance nitrate salts. Improving other salts not yet being considered for commercial use doesn't accomplish much other than to slightly increase their likelihood of being considered at some point.

Emphasize long term tern stability testing of nanoparticle suspensions.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP016
Presentation Title: CSP: Tower R&D
Investigator: Kolb, Gregory

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The Solar Power Tower R&D program addresses essentially all of the primary DOE objectives, which are, per the Multi-Year Program Plan: to fully incorporate CSP efforts into the SAI, improve storage technologies, transfer R&D concepts…to the market place…and assist U.S. industry. The DOE focus is on technology development to lower costs, pursuit of thermal storage to enhance dispatchability, exploring advanced concepts, and reducing barriers to market penetration. Essentially all of these objectives are addressed by SNLA, including collaborations with SolarReserve/PWR, BrightSource, eSolar, Sener, Abengoa, and DLR. It is particularly important that this type of effort is being re-initiated after several years absence, in part due to the potential importance to achieving DOE goals and an upsurge in industry interest. Overall, the relevance criterion is rated Outstanding ("Project is critical to the EERE Solar Program and to DOE RD&D objectives").

Variety of tower technology support. Towers are becoming quite popular with developers. So support to that in the DOE program makes sense. Includes road map definition.

The presentation confirms a strong and extremely valuable complement to private sector efforts focused on near term project development.

With power towers back in the program this work is essential in guiding the R&D efforts in this area.

This project is critical to the EERE Solar Program and fully and quantitatively supports DOE RD&D objectives.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D has been Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"). This sharp focus on technical barriers is in part seen in the Accomplishments. Past accomplishments of the key personnel and the extensive test equipment at SNLA make it clear that their support will be of substantial benefit to the industry and technology. They have developed, with about a dozen representative industrial and R&D groups, a detailed roadmap for tower development issues, needs, barriers, etc. This in itself is highly commendable. The draft, with a substantial number of TIOs, is nearing publication and dissemination. It is assumed that this roadmap will be broadly disseminated. The potential LCOE cost reduction is estimated to be about 40%; this is a substantial improvement, and if successful, could reduce central receiver LCOE to a level comparable to that of sequestered coal fired plants. There are a number of important results that result from their approach, which are listed in the following criterion evaluation. Here, it is noted that they have combined the roadmap, various systems analyses, advanced concepts (next generation systems), flux mapping tests, "1-mile heliostat tests", etc., to assist in driving down costs. They also rightly point out that reducing heliostat costs are critical, but they also address thermal storage relative to towers, and were able to make a new observation about the relative effectiveness.
The approach for the future work from the roadmap is detailed and well planned and is in the process of being prioritized. The only additional effort I can recommend is associated with air receivers, combined cycles, etc., as mentioned above.

Is addressing issues related to the tower design, heliostat design, and thermal storage. All of these elements are critical to the advancement of tower market penetration. The pathways to some really good improvements have been shown.
Outstanding with important caveats. There is considerable risk of trying to do too much with too little in the way of budgetary resources. Based on the budget information presented the project will quickly be spread too thin. Priorities of the reconstituted and re-emergent power tower industry should be strongly influenced by the testing and validation needs of projects in development followed by most likely to be adopted next generation concepts.

There is no high temperature gas receiver component in this work. There is an operating tower gas receiver operating in Germany and in spite of the difficulties with thermal storage gas receivers should be a component of this program.

The project components are well-designed, technically feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments have been Outstanding ("Excellent progress toward objectives; suggests barriers will be overcome"). The technical barriers and prioritized needs are being addressed with industry and documented in the roadmap. Accomplishments noted include: 1-mile heliostat test, steam receiver model upgrade, performance modeling software, solid particle receiver test, lessons learned from Solar One, tower vs. trough analysis, "near-baseload" analysis, LCOE reduction with storage, increased salt temperature tower receiver analysis (underway), and heliostat CAD and error correcting software. It is noted that the latter, although not described in detail, is apparently similar to software developed many years ago and used with heliostat and dish systems by McDonnell Douglas, and this work is patented. In principle, errors noted by various means can be used in a matrix analysis to develop error coefficients that are then applied to improve aim points through the course of the day. This was shown in several applications; one that was published was by Stone and Lopez, for the McDonnell Douglas Dish Stirling system tested at Solar One/Two. This prior work is noted here primarily to avoid possible infringement issues if the approach is used commercially.
Seems to be on the right directions in most of this work.

There has been limited time and budget to restart the CSP effort, but of course the facilities at Sandia provide a good foundation. The emphasis seems less on barriers than opportunities. Barriers, including scale-up risks and the kinds of issues that were exposed in Solar One and Two should have a high priority.

The tower road map exercise was a useful tool to reevaluate the program at this time.

The falling particle receiver should not be the only high temperature receiver under development - there are many open questions regarding its efficiency, particle transport, and long term viability.

Excellent progress is evident.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Outstanding. The project involves extensive industry and laboratory participation through the roadmap, on-site tests, collaborations, and plans to extend these collaborations further. Technology Transfer is shown in four papers and one patent (pending).

Apparently a lot of industrial participation, but little else. It is really disappointing not to see university involvement, as there has been quite valuable contributions from universities in the past to some of these problems. All too typical of National Lab characteristics. Papers have been forthcoming.

Coordination with DLR and the emergent power tower development companies seems to be getting appropriate support and attention.
Collaborations and publications are excellent.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area apparently has a moderate degree of specificity, primarily through the roadmap development, but only high level tasks and dates are shown. The presentation does not show a specific plan for future work. Such a plan would include such factors as the degree of effort (personnel, labor hours, major tasks, schedule, etc.), milestones, deliverables, budgets, risks/barriers/decision points, and how these barriers would be addressed and resolved. It would be helpful to have a Gantt chart, for example. The ability to conduct work of this nature is unquestioned, and the personnel are exceptionally accomplished and experienced, and the test facility is world class. The lack of a well-organized plan, including alternatives to overcome barriers, deserves consideration as this project moves ahead. In spite of this lack, this area is judged as Good ("Plans build on past progress and generally address overcoming barriers.").

The past accomplishments and capabilities of the personnel are outstanding. Some work could be done on alternative central receiver approaches, as noted above. Overall, there is no question of the worth of this project, but the specifics of the planned future effort is unclear.

Generally very appropriate. Cannot offer many suggestions for improvements.

I don't see the merit in so much emphasis on advanced concepts even though their theoretical improvement potential is significant. If the projects in development fail, improvement possibilities will be moot.

Near-term and longer-term planning are outstanding.

Project Strengths:
Outstanding past work to aid central receiver development.

Exploring a range of issues that are relevant to tower technology, and the latter are growing in importance in the solar market.

Leadership of Greg Kolb and others who have stuck with the technology in spite of the lack of funding support and scale up activity in the market.

World-class, very experienced research team.

Project Weaknesses:
There is essentially no specific plan, but they have developed a roadmap, and this may resolve this weakness. However, no specific plan was shown so that the funding, personnel, timing, deliverables, barriers and alternative pathways, etc., could be evaluated.

No involvement with universities.

There appears to be a thrust to target the ability to supply heat to supercritical steam cycle. This is conceptually attractive, but the emphasis is too much on the benefits and not enough on the realistic steps needed on the path to full scale plants. It should be remembered that the necessary scale up steps were not achieved with lower temperature cycles even on the strength of much more robust budgets of the past.

Recommendations for changes to the Project Scope:
A specific effective plan for this future work is needed, including factors such as the degree of effort (personnel, labor hours, major tasks, schedule, etc.) at the subtask level, list of milestones and a Gantt chart with decision points noted. Additional effort in advanced concepts is warranted. For example, directly heating air in advanced receivers, considering combined cycles, augmentation/retrofitting, and hybrid solar (especially with gas and use with gas turbines and combined cycles) are all important issues that have had little consideration in the U.S. Some

of this type of work has been done in Israel, Spain, and Germany. Little has been done in the U.S. The SNLA effort is a logical place to conduct an objective appraisal of these options, and perhaps provide R&D support if is determined that these options have merit.

If foreseeable budgets will not carry dramatic efficiency improvements to the finish line at commercial scale, it would be better to focus on system concepts that have venture funding and try to scale the Federal investment to a meaningful percentage of the private sector investment.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP017
Presentation Title: DEVELOPMENT AND PERFORMANCE EVALUATION OF HIGH TEMPERATURE CONCRETE FOR THERMAL ENERGY STORAGE FOR SOLAR POWER GENERATION
Investigator: Selvam, Panneer

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The high temperature concrete program is partially aligned with several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel concept that uses high temperature concreted. However, given the many uncertainties and potential problems, this project does not address DOE goals effectively. There are a number of issues, including:
1. the cost of the concrete, relative to conventional molten salt, is not shown to offer a real cost advantage;
2. the uncertainty regarding life (spalling, etc.), and the limited number of cycles tested indicates a 50 reduction in tensile strength, but it is not clear if this is the minimum, or if it continues to drop;
3. concerns over thermal transients and the effect on the concrete cracking,
4. the uncertainty as to whether or not the concrete can be encased in a metal tube, or if it will strongly adhere to a fin or auger, needed in part to improve the heat transfer;
5. use of the concrete with the HTF running through it is, in principle, similar to the Solar One "Oil and Hot Rock" thermal storage system. The use of more expensive high temperature concrete is difficult to justify for this version.
Overall, since there is no strong evidence of a significant reduction in thermal storage costs, and since there are a number of barriers and risks, this Criterion is rated Poor ("Project provides little support to the EERE Solar Program and the DOE RD&D objectives").

Attempting to evaluate concrete as a thermal storage medium, hoping for low cost results. This could be a good avenue to pursue as shown by DLR.

Cost of heat transfer fluids is clearly a barrier, and the project may deliver technical solutions that are in the economically competitive range.

Lowering the cost of thermal storage is an important goal. Whether this approach can achieve that goal needs an answer.

The project addresses lowering cost of TES, points to low cost of concrete as a storage medium, and focuses on demonstrating its physical suitability for this application. However, the full cost of a concrete TES system would involve other significant elements and the design is not yet complete enough to estimate them. Therefore, this is a speculative effort that is insufficiently supported by evidence of the potential cost savings.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Fair. This criterion relates to the degree to which technical barriers are addressed, the project is feasible, and commercially promising. This project has significant weaknesses and at best may have some minor impact on overcoming barriers. First, there is little cost justification, other than the projected material cost for concrete, compared to molten salt, and even that lies somewhere in between the low and high cost values they show. However, it is not clear what the source is for this cost. It is not noted what the normal temperature range is for concrete, and thus it is not clear if the improvement to roughly 500 C is significant or not. It is noted that molten salt may not be appropriate with concrete, due to the pumping rate and high thermal expansion, and that therminol may be appropriate, but the operating temperatures are low. This almost rules out their approach altogether. There is a path forward for improvement (quartz sand, fly ash, etc.), but the result even if these work is not encouraging for TES. The additional cost and complexity of augurs, fins, or metal tubes, and the same issue of concrete cracking, make it questionable as to whether this will offer any real potential for TES LCOE reduction. Also, oil flowing through hot rock was conducted at Solar One (thermocline). This should at least be considered in this project. Finally, if this project has some support from industry, based on their evaluation, then that may justify continuation. Lack of a quantitative systems analysis showing that this approach has promise, coupled with the technical uncertainties, makes this a questionable approach for TES. It may have numerous applications in buildings, especially in terms of fire resistance, but its application for TES is not likely to be practical.

I don't sense the PI really has much of a vision of what needs to be done. It seems to me that he thought concrete would be a good application for thermal storage, but that he has very little other insights to the situation. His background seems to be void of a lot of the expertise it would take to develop this concept to it maximum potential.

The major foreseeable technical barriers are addressed, though long term cycling to prove lifetime over expected multi-decade plant life would probably be needed prior to commercial introduction.

The real issues in this approach have to do with long term reliability. There should be more focus on increasing the number of test cycles, evaluating the potential for interface separation problems, and continued strength testing after many thermal cycles. The cost of adding fins or other protuberances should be investigated.

The approach has focused too much on concrete's physical properties and not explored early enough the implications of the rest of the system on total cost.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments are rated Fair ("Modest progress in overcoming barriers; rate of progress has been slow"). The project has apparently three mixtures that are shown on Slide 11 to withstand "more than 600 C", but it is not stated what this means. Is there one temperature cycle? Several? Does "withstand" mean no degradation in tensile strength? This chart is unclear and has little substantiation. Similarly, the molten salt/concrete exposure test results are not given; this appears to be qualitative in nature, but little is provided that can be assessed.

While the objectives are laid out and seemingly being accomplished, I was very concerned with some of the approaches used and the PI's general knowledge of many issues as displayed in his presentation and handling of questions.

Research progress is measured against performance indicators, i.e. temperature and heating rate, but retention of minimum material properties over large numbers of thermal cycles has not been evaluated.

They have made progress toward evaluating concrete for this application but should have a definitive answer on the economic viability in the near future.

The experimental progress on demonstrating suitable concrete mixtures has been adequate. The model development appears to support the concept; however, it's unclear how or if the model has been validated.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Fair ("A little coordination exists; necessary coordination would take significant effort"). The university is the only budgeted participant; some advice/technical assistance is noted for SNLA and NREL. The project has apparently resulted in a disclosure, but it is unclear from the way it is stated if a company has been formed to sell mixes or if that is a goal. There are two papers accepted for presentation on May 20 (Conference on Energy Sustainability), and these have apparently been given.

Interacting with NREL and Sandia. Concerned that they may not be able to assist very much.

While the approach seems logical and the effort technically well informed, collaboration is limited to national labs. It would be desirable to have some level of interest on the part of potential technology users.

Need to have more contact with solar thermal companies, concrete experts, and other researchers in thermal energy storage systems.

There has been very little outside interaction aside from NREL and SNL. A university project such as this one could greatly benefit from much greater collaboration with industry and other academics.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity. The presentation does not show an effective plan for future work. The specific level of effort and tasks normally expected for R&D project is not shown; for example, they need to show aspects such as the key personnel, labor hours, major tasks, schedule, deliverables, Gantt chart, etc. However, the key objectives, tasks, and decision points

are noted at a high level. This area is judged Fair ("Plans may lead to improvements, but need better focus on overcoming obstacles")

It is highly questionable that this project will be practical and effective for TES.

Has good sounding thrusts for the future. Not at all confident that they will be carried out in meaningful ways.

The project does not seem to have targets that tie to economic value in a system context, so decisions to undertake future phases will not have reference to a compelling economic value proposition.

Planning for contingencies is generally good, but the big question of potential system cost reduction is evidently delayed until late in the project.

Project Strengths:
Concrete is something that should be examined for storage.

The work is proceeding systematically with attention to fundamental technical specifications and failure mechanisms.

Innovative PI.

Project Weaknesses:
This project does not appear to reduce LCOE, even if it successfully meets the life and performance goals. There are substantial uncertainties regarding life (spalling, tensile strength loss, thermal expansion induced cracks, limited temperature cycles, etc.). It does not offer detailed, quantitative data on such issues as tensile strength as a function of thermal cycles, compatibility with molten salt, cost and durability issues associated with tubes, fins, and augers. There is no industry interest apparent in the presentation.

I am concerned that the PI doesn't appear to have the kind of background needed to evaluate this approach appropriately. Not only is he not apparently familiar with the solar applications, is he apparently not familiar with thermal process fundamentals. Not clear that he is familiar with work that has gone on previously.

Lack of comparative context. What is the baseline configuration and what is the alternative configuration using high temperature concrete.

Lack of collaboration and industry connection.

Recommendations for changes to the Project Scope:

It would be beneficial to develop data on spalling, tensile strength, etc. vs. temperature cycles, compatibility with molten salt, and costs.

The background of the PI needs to be greatly enhanced with something more (another, more experienced, person might work).

Concrete cost is in a range of cost for nitrate salts but what is the specific quantitative target? Is it \$3/kWhth or \$12/kWhth? Nail this down before doing more expensive lab work.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP018_
Presentation Title: CSP Energy Storage – Multiple Technologies Compared
Investigator: Stephens, Jake

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This program to assess and develop two candidate thermal energy storage systems addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel concept for thermal energy storage and heat recovery from sand; it also explores thermocline thermal storage. Both approaches couple analyses with experiments to determine performance. The "sand shifter" approach is, however, not shown to a sufficient level to understand the basic approach or results to date; the thermocline storage has a long legacy of past development work, but this is not addressed with any detail. Lack of such details are apparently due to the proprietary nature of the work; this, however, makes it very difficult to fully evaluate the work done. This Criterion is rated Good ("Most project aspects align with the EERE Solar program and DOE RD&D objectives").

I really cannot assess what needs to be done because of the PI's concern about IP. I am not sure how the concept is generally supposed to work. So it is very difficult to evaluate it. I have ranked it lower than it may have been had I known what is up with it.

Being able to handle sand efficiency could be quite valuable to the CSP program for systems where sand is used as a receiver absorbent and/or storage medium. This might be an excellent approach to this issue, but it not at all clear.

The project appears to be conducting parallel development of two alternative HT TES options. The $40/kWth seems high relative to targets for other projects addressing HT TES, so the extent to which the project aligns with and support DOE goal is not clear.

This concept is probably worth exploring for an option for the falling particle receiver. However Sandia (the developer of the falling particle receiver) seems to have no role in this project - why is that?

The project presentation contained too little information to determine how well the project may support DOE objectives.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Good ("Generally effective but could be improved; contributes to overcoming some barriers"). The presentation provides objectives and approach, and there are six organizations involved, including three universities, a utility (APS), and a solar company. Effort has been underway since Fall 2008. The Phase I design effort is noted as being complete. Many accomplishments are asserted, but there is no data. For example, SandShifter has moved from "raw concept to feasible design", but no explanation of the heat transfer and configuration are shown. Again, this lack is apparently due to the proprietary nature of their work, but it is difficult to evaluate unsubstantiated assertions. Therefore, it is assumed that they are conducting the work in accordance with the stated objectives and approach, and therefore this criterion is evaluated as Good, but it is difficult to appreciate this without reported results; this grade is given in effect on the basis of trust, but there is no way to verify this aspect of their program based on the presentation. As an example, they state that one of many accomplishments is the "Cost estimation of the preferred design", but even this cost number is not shown.

Cannot judge if the project is well-designed, technically feasible, and commercially promising because of keeping all critical details under wraps. My suggestion is if protecting IP is a primary consideration, that perhaps the people with the IP should develop it on their own.

The technical barriers specific to each approach are not clearly or quantitatively identified. The storage concepts and related work are discussed in vague, general terms that offer no real visibility to the design elements that might involve technical risk.

The work seems very unfocused. Exploring particulate thermoclines and moving sand are quite disparate technical issues (other than the application). There should be a technical focus to this work to identify the critical barriers in implementing this technology and a path presented as to how to overcome them. This does not seem to have been done.

The project presentation insufficiently described the approach to judge its effectiveness.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments are rated Fair ("Modest progress in overcoming barriers; rate of progress has been slow"). Other than a few schematics and a CAD 3D model drawing, there is nothing in the accomplishments that is specific; there are no details. The project shows a fourth month slip in the expected completion date, but with no explanation for the cause. It is thus difficult to determine if any barriers have been overcome. There is also a puzzling aspect to the list of presentations, papers, and posters (five total, plus two patent applications), which indicate that at least some aspects of the details of the system have been presented, but none of this type of work is shown in the presentation. It is understandable that the patent applications would not be discussed, especially to the level that would be an enabling disclosure to one skilled in the art. However, there is a serious disconnect in not having any details, if there are about five papers/presentations that have been prepared for open dissemination. The presentation states that the SandShifter results for costs have been determined, and then state "Expect to exceed $40/kwh-th target". Stating that they have developed the costs, and then "expect" to exceed the target is puzzling. Furthermore, the target is substantially less than $40/kwh-th. Overall, this presentation is very difficult to assess. Statements are made that they are " Ahead of schedule", but this is difficult to judge; there is no schedule with milestones, deliverables, Go/No Go decision points, etc.

Very hard to evaluate.

One missed opportunity relates to the lack of comparative information for the two concepts. Why a project would be pursuing two very different alternative concepts without actually using project results to compare them is not explained.

Very disjointed effort. No quantitative results forthcoming. They must define the problem they are trying to solve.

The project presentation provided insufficient information (not even accurate budget status!) to gauge progress.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Good ("Some coordination exists; necessary coordination could be accomplished easily"). The project involves a solar power system developer, three universities, a utility, an industry partner, and apparently collaboration with national labs. Two patent applications have been filed. Five papers/posters/presentations are cited. However, no details are provided, even for the relatively more detailed, publicly available information presumably given in the papers/posters.

Some collaboration with Sandia was noted here and in another presentation. That is the only good sign about this project that was shared with the reviewers.

The project includes a significant number of appropriate partners but actual coordination is not easy to assess.

UTRC seems to be both subcontractor and is listed as cost share contributor. This should be clarified.
Sandia should be involved to advise on materials, thermal issues, technical details (moving particles up and down a tall tower), etc.

The project is evidently a proprietary development effort with no outside interactions and no issued patents.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area is judged to be Good ("Plans build on past progress and generally address overcoming barriers."). The presentation lists barriers and upcoming key milestones, but without some substantiation, it is difficult to provide an evaluation on this. The specific level of effort and tasks normally expected for R&D project is not shown; for example, no time-line, labor hours, deliverables, Go/No-Go, decision points, etc.; in short, there is no Gantt chart. Essentially only the key objectives are noted at a high level.

The effort claims that the project is addressing a potentially breakthrough TES technology (SandShifter) and in effect re-assessing thermocline storage, both analytically and experimentally, but without some indication of real results, it is difficult to evaluate the real progress.

Hard to evaluate.

Plans are to construct and test prototypes in the next phase. There is no indication of how the test programs would be conducted and what barriers they would overcome.

Insufficient information was presented to judge the quality of planning.

Project Strengths:

It is difficult to find a strength simply from the objectives, list of barriers, and key milestones as provided. It is not even clear that exceeding the TES $40/kwh-th is plausible, but it would have to greatly exceed this to be a substantive improvement. The real "target" for DOE is $15/kwh-th. Thus, it is difficult to assign a strength to this project.

Could be quite a good approach.

The project participants seem to have relevant capabilities and interests.

Private funding.

Project Weaknesses:

There is no treatment of even conceptually reaching higher temperatures. There is no mention of materials, even including what type of sand is to be used. The cost goals do not appear to be in line with the DOE target.

IP requiring the masking of details is not good for a publicly-funded project.

It may be that the project has not proceeded far enough to produce anything beyond proposal level detail, but the lack of even a coherent technical explanation of how the "sandshifter" works and how a themocline would be achieved is worrisome.

Extreme secretiveness (for a DOE-eligible project).

Recommendations for changes to the Project Scope:

Consideration of higher temperatures is needed. A basis for their cost projections is also needed, or at least an explanation of the analysis and some means for verifying their approach, even if they are constrained from providing an explanation of the processes, heat transfer, mechanism for moving the sand, TES thermocline materials, and a host of other issues. For example, they could at least provide some indication of the general size and power level, and from this at least some comparison of the total weight and thus, to first order, cost for the storage and heat exchange system. The specific level of effort and tasks normally expected for R&D project should be shown, such as the key personnel, labor hours, deliverables, Go/No-Go, decision points, etc.; in effect, a Gantt chart is needed. In short, as evaluators, we are expected to give an objective and thorough appraisal of project results and plans; to do this, we must have some level of detail. There is virtually no detailed information provided, and the relatively high evaluation scores I give in the above are based primarily on trusting the partners to do good work. But I am not comfortable with this situation.

Need to release more technical details for proper review of progress.

Cost models should be developed before the project proceeds into hardware phases, and some overall system context should be provided for comparison of the two storage concepts. Are they actually comparable or would they operate in different temperature regimes with different heat collection systems? The fact that this question has to be asked after listening to the presentation and reviewing the slides does not bode well for application of any project results in the market.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP019
Presentation Title: Novel Molten Salts Thermal Energy Storage for Concentrating Solar Power Generation
Investigator: Reddy, Ramana

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This novel molten salt program addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel concept that treats the molten salt mixtures at a fundamental atomic level to increase specific heat and thermal diffusivity, as well as operational temperature range, so that less thermal storage material is needed, tank volumes are reduced, heat exchanger performance is improved, heat exchanger size and cost are thus reduced. This analytical approach is coupled with experiments to determine properties. A systems analysis is also used to assess performance potential. This approach helps conceive and develop a wide range of thermal storage options, and thus improves the chances of success. The potential resulting overall reduction in thermal storage costs could be significant, and the same basic salts could be used, but with better properties, in planned systems. Thus, this Criterion is rated Outstanding ("Project is critical to the EERE Solar Program and fully supports DOE RD&D objectives").

Addressing some of the major issues related to the application of high temperature salt systems for storage and other applications in CSP: included are melting point (decreasing it), cost (decreasing it), and generally characterizing their properties.

Without knowing what the design and operational implications of ternary vs. binary salt eutectics, it is difficult to assess the degree to which the project supports DOE's goals. How likely is it that the project developers and designers will adopt an alternative to current salt storage medium? What are the economic implications of lower melting points? Are the candidate salts available in commercially necessary quantities?

Low melting point salts are an important consideration for high temperature trough receivers. Since salt can operate at a higher temperature than oil and trough performance is improving, having a salt HTF for troughs is important. However, trough receivers are horizontal and do not drain as readily as tower salt systems. Therefore salts that freeze at much lower temperatures represent less of a financial risk in case of failure.

The project fully supports the DOE objectives for lowering TES system costs.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Good ("Generally effective but could be improved; contributes to overcoming some barriers"). They use fundamental thermodynamic modeling in a systematic way to develop, assess, and screen potential ternary mixtures. These then undergo tests. However, in the main presentation, there is no mention of thermal conductivity, k, but in the supplemental slides, it is mentioned and one curve shows somewhat higher values (and the drop off of binary values with T). It is unclear why little attention is given to k, since it is important to heat exchanger sizing and heat transfer from the salt to the heat transfer fluid. It is also necessary in a study of this type to consider higher temperatures; if the mixtures offer the improvements found, and can operate at higher temperatures, then this would aid other CSP systems (dish, central receiver, and advanced high temperature trough systems). Thermal stability is shown as a small percentage loss, but, it is not clear if this is maintained at higher temperatures, and there may be issues with loss from dissociation and volatility/vapor pressure of some of the constituents at high temperature. The basis for the economic assessment is lacking, and refers to EPRI, but it is unclear how this is done. It would help to show that their model is valid by comparison with other results, or perhaps base their results on the Solar Advisory Model or some other analysis approach that is in common use.

This is examining materials for application both as a TES material as well as a heat transfer fluid. While it would be good to find material that will satisfy both requirements, it may not be that one will do so. It seems that a variety of the appropriate questions are being asked in the work. Use of both modeling and experimentation is viewed quite favorably.

The analysis presented is clear and informative, and analytical results are presented with reference to measured results. The assessment reported on is quite convincing and the result is important if in fact the cost comparison between the existing nitrate salt mixture and the recommended ternary eutectic is valid with error bars on the cost that are less than the cost difference.

Project seems focused on finding and evaluating the best low melting temperature salts. Theoretical work seems solid and useful in determining candidates.

The project approach efficiently produced an evidently suitable candidate salt for much improved TES economics. However, the disclosed candidate appears to be previously known so it's unclear how significant this exploration is.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:

The accomplishments are rated Good ("Significant progress toward objectives and overcoming one or more barriers"). They assessed 18 salt options, focused on six ternary mixtures, conducted detailed tests of one particular candidate, succeeded in identifying versions with lower melting points than current binary mixtures, quantified the major properties (specific heat, viscosity, corrosion effects on steels, and thermal stability), and show some economic comparisons. They note that three mixtures are near the DOE goal(l$15/kw-hr (thermal) and three were near $18/kw-hr. They identified a TES that benefits from lower temperature preheating.

They have made much progress in determining the specific heat of ternary mixtures, and their fundamental atomic model (Gibbs free energy, etc.) is excellent. They may have determined thermal conductivity (this is unclear, with only the supplemental chart showing curve, but with no data points, error bars, etc., and thus this aspect is difficult to evaluate. The only issue is that since this approach can be used with dish engine and central receivers, it is important to consider higher temperatures. Extending these tests and analyses to about 650 C is needed.

There is a significant potential for commercial application, based on their preliminary systems analysis. They are to be commended for a comprehensive analysis and test program to assess the properties of a large number of options and for providing detail in several areas, with a total of 41 slides.

On one hand it appears that very good progress is being made. However, when the PI was questioned about several of the aspects, he indicated that they will be revisiting several of the measurements to clear up inconsistencies.

The presentation conveys a convincing, data driven argument for the availability of lower cost and technically preferable alternatives to current salts used for storage. However, engineering and materials issues that might weigh in favor or against the preferred alternative salt mixture are not discussed.

Seems to be making steady progress in the theory and experimental aspects of project. The necessary thermal measurements seemed to have been performed.

To date a seemingly suitable candidate has been identified and synthesized.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Fair ("A little coordination exists; necessary coordination would take significant effort"). They show UTRC as a partner for three tasks (atomic/molecular modeling of properties, fluid flow modeling, TES optimization and system modeling), but this is not explained and it is unclear what UTRC contributed to the work reported. It is surprising that no patent applications are being filed; their work appears to be suitable for at least one patent.

Not clear to me that sufficient coordination with others who are knowledgeable is taking place. Although four groups were indicated as collaborating, I am not sure they are all distinct entities. It may be good to involve another knowledgeable group to assist in evaluating work performed.

Project does not seem to interact with industry or laboratory partners. Creates concern about researchers' visibility to practical issues and questions influencing material selection.

Working with UTRC as subcontractor but should be talking to NREL, Sandia, and possibly other potential high temperature trough developers.

The project has had little outside interaction with industrial players besides its contract partner or other universities. A single paper has evidently been submitted for publication.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area is judged to be Good ("Plans build on past progress and generally address overcoming barriers."). The presentation shows a task plan and schedule for prior and future work at a top level, and it appears that this is being followed. The value of the UTRC subcontract and its duration, deliverables, etc. are not shown. The specific level of effort and tasks normally

expected for R&D project is not shown; for example, in addition to the time-line, it would be beneficial to show aspects such as the key personnel, labor hours, deliverables, Go/No-Go, decision points, etc. However, the key objectives and tasks are noted at a high level.

The effort to analytically and experimentally determine the properties of the ternary mixtures is being done well. Results already indicate a possible improvement.

Seems to be reasonable defined, but it involves a lot of backtracking, apparently, to firm up some of the measurements made earlier.

Plans for phase 2 address key questions of corrosion and heat transfer properties.

Planning through corrosion and other compatibility testing is good. Longer-term planning is not evident.

Project Strengths:

This is a promising approach with potential to reduce costs relative to conventional thermal storage system binary molten salts.

General topic is one of significant importance to the CSP program.

Supplemental slides provide excellent exposition of material property dependencies and modeling principles.

Well-qualified investigator.

Project Weaknesses:

Extension of their approach to cover thermal conductivity explicitly is worthwhile, and may have already been done, but it is difficult to assess this aspect. Consideration of higher temperatures is needed. A basis for their cost projections is also needed, or at least an explanation of the analysis and some means for verifying their approach.

Need to have some outside group assisting with the work to assist in picking up shortcomings in the approach.

Market and industry connectedness is a gap that needs to be filled or at least addressed.

Lack of DOE CSP Program and industry connectedness.

Recommendations for changes to the Project Scope:

The specific level of effort and tasks normally expected for R&D project should be shown, such as the key personnel, labor hours, deliverables, Go/No-Go, decision points, etc.

Involve another knowledgeable group to assist in assessing progress and making suggestions for future work.

The project effort should reference and account for the results of other projects addressing the same objective using different approaches. How can the CSP industry be expected to evaluate conflicting but ostensibly equally credible storage material recommendations resulting from parallel DOE funded projects?

Review: EERE 2010 Solar Program Review
Presentation Number: CSP020
Presentation Title: Deep Eutectic Salt Formulations Suitable as Advanced Heat Transfer Fluids
Investigator: Raade, Justin

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This deep eutectic molten salt program addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel approach that allows a very wide range of mixtures to be evaluated quickly and cost effectively; this automated approach provides experimental data on the major properties. Their use of an essentially robotic power dispensing approach greatly speeds the process of screening mixtures (four mixtures and higher, in particular); a maximum of 500 blends/week can be processed. The ability to increase the temperature range is stated to result in a $0.02/kw-hr cost reduction, per a Sandia study.

This approach helps conceive and develop a wide range of thermal storage options rapidly enough to allow mixes of four or more salts to be considered, and thus improves the chances of successfully achieving better performance. They have one such mixture now, with a much wider temperature range (less than 75 C melting point, stable up to at least 500 C). The potential resulting overall reduction in thermal storage costs could be significant, and the same basic salts could be used, but with better properties, in planned systems. Thus, this Criterion is rated Outstanding ("Project is critical to the EERE Solar Program and fully supports DOE RD&D objectives").

The goal of developing a heat transfer fluid that operates over a wide range of temperatures, including having a low melting temperature, is clearly a benefit to the solar thermal power industry, and hence, of course, to the EERE Solar Program.

It seems almost certain that salt mixtures with low melting points and stability at 500 degree C would significantly enhance CSP economics if their costs were in the same range as current salt mixtures.

Low temperature melting salts are important for salt cooled trough collectors to avoid accidental freeze up. This work addresses this directly.

This project fully and quantitatively supports the DOE TES goals.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"). The essentially automated approach of mixtures of at least three, and as many as seven, salts is novel, cost-effective, and timely; this approach appears to be an enabling technology for developing improved salt eutectics. Their discovery of a novel salt mixture with an extended temperature range is an important result. They are filing a patent on this. They stress specific heat, viscosity, and stability. However, there is no mention of thermal conductivity, k. It is unclear why k is not determined, since it is important to heat exchanger sizing and heat transfer from the salt to the heat transfer fluid. They do mention that in Phase 2 they will screen for secondary properties; perhaps k is included in this. It is also necessary in a study of this type to consider higher temperatures; if the mixtures offer the improvements found, and can operate at higher temperatures, then this would aid other CSP systems (dish, central receiver, and advanced high temperature trough systems).

Examining the heat transfer and thermophysical properties is certainly an initial step that is needed after developing candidate materials, and it seems that this is one of the first steps being considered here.

The approach taken is highly systematic, comprehensive and well explained.

Very sharply focused R&D effort based on automatic measuring equipment that can screen thousands of samples.

Approach using combinatorial analysis and rapid characterization appears to be extremely effective for screening candidate materials.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments and progress are rated Outstanding ("Excellent progress toward objectives; suggests barriers will be overcome"). They screened over 5000 blends and found approximately 150 with improved properties, and one in particular with a very low melting point. This is more than an order of magnitude more than any other reported results. In principle (per their response to one of my questions), this same approach can be used with nano-particles in these salts, and this could provide additional options.
The only issue is that since this approach can be used with dish engine and central receivers, it is important to consider higher temperatures as well as lowering the melt temperature. Extending these tests and analyses to about 650 C would be beneficial. They do show some data that indicate temperatures as high as 550 are practical for HiTec, but that it has an unacceptably high melting point (142 C), but with their approach, they achieve both low melting point and thermal stability at high temperatures. They also note that one test (viscometer, limited to 300 C) will be modified for higher temperatures.

There is a significant potential for commercial application, based on their results to date, and the approach is a breakthrough on assessing mixtures of more than two or three salts. They are to be commended for a comprehensive test program to assess the properties of an extremely large number of mixtures.

This is extremely hard to judge from both the prepared slides and the presentation. I actually had the feeling that very little has been accomplished beyond items that could have been contained in the proposal. They have screened several candidates, presumably from sources such as existing literature, but as nearly as I could tell that is about all that has happened (or at least reported).

The project has identified candidate materials with superior properties to those currently in commercial use in CSP plants.

Through their screening procedures they have identified a salt combination based on Li-Na-K-NO3 and Li-Na-K-NO2 that they believe is a good candidate for trough HTF and storage medium. The physical characteristics, corrosiveness to the materials involved, and the long term stability need to be determined.

Discovery of 70C MP salt stable to >500C could be a breakthrough for PT systems.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Fair ("A little coordination exists; necessary coordination would take significant effort"). They show SNLA as a partner in Phase 3 for field testing. They state that they will work closely with customers/industry partners, and imply who these are with a display of their logos. They have applied for a patent. It is unclear in the presentation, but touched on in the presentations, what the role of Symyx is. Apparently, they have acquired the technology and show that they will continue to work with them, as noted in the task/schedule chart.

Has a large number of system developers noted. Not clear how close the collaboration may be. No indication of publications or presentations. I hope someone from DOE is monitoring this project closely to check on progress.
Outreach to the CSP industry to date appears to have been limited. What information will prospective customers need beyond that already developed through the project?

The relationship with solar developers is not spelled out and probably needs to be expanded to better understand the needs of the users.

This is a proprietary development project, but evidence of industry connections to disseminate practical results was presented.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area is judged to be Good ("Plans build on past progress and generally address overcoming barriers."). The presentation shows a task plan, schedule, roles of the participants, and milestones for prior and future work at a top level, and it appears that this is being followed. They show the relative duration and tasks

The specific level of effort and tasks normally expected for R&D project is not shown; for example, in addition to the time-line, it would be beneficial to show aspects such as the key personnel, labor hours, deliverables, Go/No-Go, decision points, etc. However, the key objectives and tasks are noted at a high level, and they are to be commended for discovering a novel eutectic; this result, in itself, makes the effort to date very worthwhile.

The effort to rapidly determine the properties of mixtures by essentially automated means is being done well and is an enabling technology that has much potential for future formulations, including nano-particles. Results already indicate a possible improvement in a salt that has very low melting point and is stable at high temperatures.

The project indicates moving to commercialization. Not sure the various steps to doing this are well thought out.

Proposed phase 2 efforts are appropriate and the implication is that there will be outreach to trough project and technology developers. It is difficult to evaluate the project team's ability to bring the new material to market. Is DOE assistance needed in support of field tests of the new material?

Plans include further testing and commercialization.

Plans clearly build on progress and envision full commercialization processes.

Project Strengths:
This is a proven, rapid, and highly promising approach with potential to improve molten salt properties and reduce costs relative to conventional thermal storage system binary molten salts.

Basis of the project is quite desirable, and they may be asking the right questions.

Materials screening model is theoretically sound and capable of evaluating large numbers of options even extending beyond ternary eutectics.

Innovative tools and investigators.

Project Weaknesses:
Extension of their approach to cover thermal conductivity explicitly is worthwhile, and may have already been done, but it is difficult to assess this aspect; it was not reported. Consideration of higher temperatures is needed. Additional work on cost projections is also needed.

Details of the layout of the project path to be followed is not clearly laid out in the presentation. Didn't like the answer to the question: what is an optimized solution? Saying it is whatever the customer will buy seemed to imply that the presenter really didn't have a very good idea what the target might be except low viscosity and low cost.

Cannot comment without knowing more about Halotronics and its commercial or commercialization experience.

Recommendations for changes to the Project Scope:

Inclusion of thermal conductivity is needed, and to the extent practical, data for properties at higher temperatures. Outreach to material customers should be complemented by outreach to material suppliers. Sandia's use of the recommended material in test loops is planned but commercial industry should have advisory input and full and timely visibility to field testing and test results.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP021_
Presentation Title: High Performance Reflector Panels for Concentrating Solar Power Assemblies
Investigator: McCamy, James

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This project addresses several important DOE objectives, including, per the Multi-Year Program Plan: technology development to lower costs and exploring advanced concepts. …transfer R&D concepts…to the market place…assist U.S. industry and reducing barriers to market penetration. If successful, some decrease in LCOE could result.
They have developed a glass mirror and conducted tests on reflectivity; one very interesting aspect is that the reflectivity improves after heat treating, which means that the glass can be mirrored prior to bending. This is a substantial improvement in cost effectiveness and is far easier as a high rate process. The encapsulant is lead-free and results indicate that it may offer long life. Given that glass mirrors are the only proven means of achieving long life and that this work applies to all three CSP systems (trough, dish, and central receiver), this work is very important. Further, it has a very high cost-share and is based on on-going PPG R&D. This Criterion is rate Outstanding ("Excellent progress towards objectives; suggests that barrier(s) will be overcome")

Goal is to develop mirrors that use a different type of process compared to other, more conventional, types of mirrors. These different types of mirrors would hopefully be of high performance and low cost. Included will be the ability to withstand bending of the glass upon which the mirror surface is mounted.
This reviewer considers the fact that this company wanting to be involved in mirror development could be a major benefit to the DOE program and the various companies in the CSP technologies development business.

Fundamentally, the project engages an additional major glass manufacture in developing and potentially offering solar mirror material to the CSP industry.
This will potentially exert competitive pressure for lower cost, more durable and better warrantied material over time, provided PPG decides to enter the market.

Producing the coating on flat glass as part of the production line and bending the glass later promise significant cost reductions for trough applications.

This project aligns very well with DOE CSP goals for both towers and troughs.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"). This project determined that their lead-free encapsulant appears to offer long-life. These tests are currently limited to one year of exposure and have not undergone accelerated life tests, but they have initiated durability tests.

They used a full test protocol based on inputs from customers and NREL. This approach is commendable. This work is based on significant PPG funded development, and thus offers substantial leverage to the DOE funded effort. They couple this with a LCOE analysis that shows a 5% reduction; this is very significant, since the cost increase for the glass with improved reflectivity is marginal. They found a surprising result: the reflectivity improves after heat treating, apparently by improving the morphology of the silver coating. There are also some environmental benefits.

Aiming for the development of three types of glass. To this observer, it would seem that these three types will cover most of the solar kinds of applications. These developments are somewhat removed from their normal business, but it appears that they have the expertise to pull this off in fine form.

Development of an protective encapsulant is an important aspect related to the longevity of the mirror. The research team seems to have a quite a good handle on the methods needed.

Bending is another aspect of importance, and they appear to have some way of bending the coated mirror.

The emphasis on long term mirror deterioration resulting from water ingress at mirror edges is very valuable, especially as it is being coupled with accelerated life test that can eventually be the basis for warranties that lower the cost of capital for projects.

Very well organized development plan. Apparently they have found a superior encapsulant material that promises long mirror life.

Technical approach is sharply focused on critical barriers to lowering cost and demonstrating reliability.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
This project has provided an improved glass with higher reflectivity, less environmental impact, essentially no cost increase for the bent glass version, and an actual improvement in the reflectivity after heat treating. There are also related production runs of this glass, in addition to, but separate from, the DOE effort. Having a major supplier conduct work of this nature, and in parallel provide commercial solar customers with this glass is commendable. It is particularly noteworthy that the process does not require changes in production processes. It is my understanding that it only requires sufficient orders to make the modifications on their existing lines. It is also note-worthy that the most recent encapsulant exhibits substantial improvement over the earlier versions; this indicates real progress. The project is on budget and schedule, but a no-cost extension was needed. This extension was needed to develop the breakthrough encapsulant. This criterion is judged Outstanding (Excellent progress toward objectives; suggests that barriers will be overcome”).

Seems to be quite good progress. The one area that needs attention is the development of bending into the needed parabolic shape that yields the necessary accuracy of the profile. This apparently is not an easy task to accomplish. Long term weatherability resistance is difficult to represent as has been indicated in other presentations in this review.

Environmental durability accomplishments are significant but need to be evaluated in the context of best commercial practice for mirrors encapsulated with lead free material. It is not clear how far along PPG is relative to say, Flagsol.

Apparently making good progress, but IP restrictions don't allow much to be said about the coatings or results.

Progress to date on lead-free lower-cost manufacturing appears outstanding.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Outstanding ("Close, appropriate coordination with other institutions; partners are full participants"). The collaboration is between various customers and NREL. This collaboration resulted in an agreed-upon test protocol. They also were able to combine this effort with their commercial glass manufacturing, and supply customers with the improved glass. This could help lower costs more quickly than a purely R&D effort, and is to be commended. They have at least six patent applications being filed and more are being developed. Although no publications are cited, the patent applications in many respects provide this kind of information, and for a commercial product, it is appropriate to file first, and publish later.
No collaborations noted. At this point things of this sort do not appear to be needed. Several patents have been filed.

External interactions seem to be limited to NREL which is not a good sign, since companies with field experience with metal glass mirrors would seem to be a good source of market understanding and requirements.

Their only outside interactions appear to be with NREL.
They have no publications but do have 6 patent applications

NREL is significantly involved in testing and 5 patent applications have been filed. More CSP industry engagement would likely be beneficial.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
They show a task and schedule, Go/No Go decision points, and top-level definition of future tasks. They do not show a Gantt chart. The ability to manufacture glass thinner than 2.1 mm, with higher reflectivity, is not addressed. This criterion is judged to be Good ("Plans build on past progress and generally address overcoming barriers")

Excellent test protocol developed with NREL and customers. Achieving improved reflectivity, allowing silver coating in the flat state and then bending, with use of existing manufacturing lines, is commendable. Having a successful lead-free encapsulant is particularly important.

Working on bending ability and scaling up are indicated for future work. These are important needs.

Not much insight or specificity is offered regarding longer term plans, probably because PPG is not yet committed to be in the market.

Well-designed development program.

Planning through Phase III appears good. Longer-term plans were not presented.

Project Strengths:
They have demonstrated a high reflectivity glass with a lead-free encapsulant

An American manufacturer entering this important CSP market is extremely laudable.

First tier glass manufacturer bringing expertise and major cost sharing.

Very experienced contractor, good working relationship with NREL.

Project Weaknesses:
None identified.

Lack of customer input to product requirements.

Recommendations for changes to the Project Scope:
The only recommendation for additional work centers on the lack of an approved, consensus approach for determining durability, which PPG would be able to address and likely offer suggested solutions.

More emphasis on working with customers to identify investor needs for risk mitigation related to long term mirror degradation.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP022
Presentation Title: Tower Receiver Development Kris Miner
Investigator: Miner, Kris

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The tower receiver development program addresses several important DOE objectives, including, per the Multi-Year Program Plan: …technology development to lower costs, and exploring advanced concepts. Its stated objectives are to address the barriers of performance, reliability, and O&M cost. However, there is nothing quantitative in the presentation that is reported as having resulted from the 1.5 year effort. For example, assertions are made that they have addressed improved materials and simplified the design. But nothing is provided to substantiate this; there are no data on improved absorptivity, emissivity, reflectivity with the materials and coatings for the receiver or the heat shield, or what the basic types are. No design improvements are shown that related to simplification or cost reduction. The majority of the presentation relates to the past work at Solar Two, which was excellent, but it is difficult to see what improvements have been made because none are noted in the presentation. Statements that vaguely address the goals are made, such as "designing in durability…will be implementing…will be monitoring…high absorptivity/low emissivity for sunlit regions…select tube coatings…innovate design…finalized two tube materials…finalized four coatings…have no substantiation. The statement "no data to present at this time" makes it difficult to reconcile the selection of materials and coatings. They are apparently ready with a new design that will be considered at the June 29 CDR, but nothing is provided on this design. There may be a reason for not presenting at least some detail, such as IP concerns, but if so, this should have been stated. In spite of past successes, the lack of quantitative results for accomplishments from this program during this 1.5 year effort makes it difficult to evaluate this project. Based on the above, this criterion is judged to be Fair ("Project partially supports the EERE Solar Program and DOE RD&D objectives").

Addressing the economic viability through increasing performance of the receiver is being addressed in this project. Generally this involves taking the previous Solar 2 design, upgrading in various ways, to improve manufacturability, reliability, repairability, and performance.

Note: DOE's share of project costs, not counting the use of tax payer funded test facilities, is twice the contractor share.

The project could be valuable and relevant. However, there was not enough information in the presentation to reach a defensible conclusion. System context was completely lacking, e.g. scale, functionality, cost, of potential commercial applications. Apparently, the receiver fits Solar Reserve's plans, which were not described even in general terms. Presentation contained no system context including such important details as whether the receiver working fluid is water/steam, nitrate salt or other. The presentation provided no technical insight at all, e.g. regarding materials selection, specific problems encountered at Solar One and Two, and leading to a reasonable assumption that anything learned in the test program would be protected as proprietary and thus be of no value to the industry and its potential investors, customers and suppliers. The project thus unfairly favors a single company, placing it under no apparent obligation to provide visibility to publicly funded research even to DOE's peer reviewers.

Liquid salt receivers are an important element in tower program and should be pursued.

The project well supports the DOE CSP tower goals by addressing key barriers to lowering cost and increasing reliability.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Fair ("Has significant weaknesses; may have some impact on overcoming barriers"). The stated plan includes the comments from the presentation stated above, plus "validate manufacturablity of large scale receivers, test receiver…, design for lowering the LCOE", but how this is accomplished is not stated, and no information is provided. It is not known how many candidates were evaluated, how they plan to "validate", what the test plan consists of, or even if there is one, and no example is given for how the design will lower LCOE. Coming from PWR, with its many years of success, this lack of substantiation is particularly acute.

There is virtually no quantitative data provided on their design, or why it was selected over alternatives, or if there were any, or how it differs from Solar Two. There are no specific examples of the types of improvements made. For a project having a cost of the order of $1M for the last 1.5 years, there should be real, quantitative results. Without this, the R&D Approach is judged Fair.

Several technical barriers are noted in the presentation, and a variety of thrusts are being worked to attack them. However, since the fundamental basis is the Solar 2 receiver, and no apparent gross changes (many lesser changes),the work in the current project is very much an incremental effort on all fronts. This makes the evaluation of the basic thrusts a little more difficult to assess.

Unable to evaluate based on presentation.

It is hard tell how much progress because of IP restrictions.

The technical approach appears well-designed, technically feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments and progress are rated Fair ("Modest progress in overcoming barriers; rate of progress has been slow"), even though they state that they are ready for a CDR within weeks, the lack of specificity, even though they have built and tested receivers for Solar One and Two and have unquestionable capability, makes it difficult to give them credit for the effort on this particular project, based on the material presented. The statement is made "very encouraged by design", but with the exception of a CAD overview, which was not discussed, but only shown, there is very little real information provided (improvements in weight, cost, optical performance, durability, design changes from Solar Two, etc., or how their presumably improved approach reduces O&M costs, etc. The schedule states that long lead items should be close to being on order; none are identified, and thus it is not known what they are or if they are in the process of being ordered. No cost evaluation results are shown. No "manufacturablity assessment" is presented. There is a potential for commercial application, especially given that they are in this business and a major player, and are therefore critical to the success of central receivers in the market place, but it is difficult to see how the funds expended have contributed to improvements in the design, cost, performance, or operation, based on the information provided. In short, the description of this project is very disappointing.

As a follow up to the point made in Criterion 2 (above), the progress is a little hard to assess. The tasks sort of cover the water front on all issues related to receiver design. All in all, though, it seems as though things are moving fine.

Unable to evaluate based on presentation.

It is hard tell how much progress because of IP restrictions. It is not clear that they have accomplished much of anything other than designing a shorter version of the Solar 2 receiver with (maybe) different and cheaper materials. I cannot tell if this is a good economic decision or will result in receiver failures. Almost, if not all, of the photos are from Solar 2 that were taken many years ago.

Progress to date is limited to receiver redesign and test planning, but those activities appear to be on track.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Fair ("A little coordination exists…"). The project involves SNLA and "sub-tier vendors", but the extent of this is not stated, nor are vendors identified. Technology Transfer is not shown; there are no papers or patents noted.

Appears that about the only collaboration is with the Sandia Solar Test Facility. No patent applications or publications have resulted from this work at this point.

It is too early to judge. However, very little apparent likelihood of collaboration based on presenter's reluctance to share information and provide forthcoming answers to reviewer questions. There could be a lot of explanations but he fell far short of communicating a willingness to share information. It does not suffice that the project and Sandia are the only recipients of information from the projects. If there is a plan for Sandia to do what the project team does not intend to, i.e. share non-proprietary information and data, that could mitigate this reviewer's negative judgments about the project, but would suggest an awkward and not particularly effective approach to protecting the public interest in technology transfer.

There appears to be little or no coordination outside of Sandia Lab and their subcontractors (who are not referred to).

It is disconcerting not to see more collaborative involvements in this project, given the high level of DOE funding. It would very likely benefit from wider exposure in the tower community.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area provides a top level schedule with clear decision points, sub-task breakout, and budget allocation. This part of the project is commendable, and indicates, as expected, that the management disciplines are being used. However, without reported results, it is difficult to see that results have been achieved, and this raises concerns over how they will complete the design of an improved receiver, fabricate it, and test it. There is no doubt that a receiver will be built and tested, but there are doubts as to how it will be an improvement over Solar Two. They state that if budget is added, the receiver can be tested longer, but they do not state how this will provide additional information that is needed, or what tests are not being done because they lack the additional test time. Nothing involving a test plan is presented. No parallel coupon tests of any of the four coatings or the two materials will be conducted, or how they will decide which one of these options will be used for the receiver, or if they will try more than just one of these. The key decision points are noted, but there is no real treatment or discussion of barriers to the realization of an improved receiver (better O&M, lower LCOE, better optical/thermal performance, longer life, etc.). However, they do show a schedule that addresses the future work to be conducted. Overall, this area is judged as Fair (e.g., "Plans may lead to improvements, but need better focus on overcoming barriers.").

The effort to build and install and test an improved receiver is claimed, but not substantiated with any data; only a CAD drawing is shown, but this is not discussed in the context of any improvements that have been developed. The material presented for evaluation is too vague to make a well-reasoned judgment.

It appears that the later aspects of the project are involved with finalizing the design and moving toward an actual test rig to be evaluated at Sandia. This seems to be quite appropriate. Having test results will be very valuable.

See above. A lot of the presentations were technically thin, perhaps as a result contractor interpretation of format requirements. This one felt like malicious compliance. It was not just thin but vacuous. This particular contractor should be in the best possible position to provide technical leadership and deep technical insight based on DOE's past investment in its receiver development work. Under the circumstances, there can be no excuse for keeping DOE and the power tower technical community in the dark regarding essential project and component parameters, system context and customer technical specifications applicable to the test object and test program.

Other than testing the receiver there is very little content in the future plans or objectives.

Plans through Phase III appear good. No longer-term plans were presented.

Project Strengths:

They are apparently on schedule to complete the design of a prototype, potentially improved receiver. This will be tested on-sun for performance at SNLA.

Will be good to have a new version of a solar central receiver unit in operation, building on lessons from Solar 2 about the design aspects.

Given the lack of private sector investment in power tower research or commercialization, it would be of interest to know if the people involved in the previous Rocketdyne tower receiver efforts are involved in the new effort or if the project is simply a new start with no reference to internal corporate memory. If there is some connection to past experience, that would be a strength.

Legacy of Solar II project learning.

Project Weaknesses:

The major weakness is the lack of specificity. However, some of the decision points are noted, and major milestones are shown on the schedule.

If the technical quality of project design and test effort mirrors the superficiality of the presentation, the project will provide no value to the DOE program.

Lack of CSP community inputs to enhance innovations.

Recommendations for changes to the Project Scope:

Quantitative data should be provided on their design, including why it was selected over alternatives, or if there were any, or how it differs from Solar Two. Specific examples of the types of improvements made should be cited. For a project having a cost of the order of $1M for the last 1.5 years, there should be real, quantitative results.

The Program should reevaluate the current level of funding versus the overall benefits of this effort.

This project should be re-reviewed under circumstances that the work can be discussed.

Incorporate project reviews with SNL tower CSP workshops to provide broader perspectives.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP023
Presentation Title: Sensible Heat, Direct, Dual-Media Thermal Energy Storage Module
Investigator: Newmarker, Marc

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The dual media, phase change program addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. Much of the presentation is virtually identical to their CSP012 project for the molten salt module with phase change, coupled with a heat exchanger. In principle, it could benefit from parallel efforts done by others to improve molten salt properties. However, there is very little information provided by which to judge the potential. They claim they are evaluating the different filler materials, but they do not address what the evaluation criteria are. Thus, it is not clear how this can achieve a cost reduction, even if the appropriate material is selected, and apparently the only test planned is one of compatibility with the HTF, and even this is uncertain and depends on budget. The term "it is believed" is used to justify the concept, but there is virtually no substantiation; unlike the CSP012 project, there is no mention of conducting a SAM analysis. Overall, this Criterion is rated Fair ("Project partially supports the EERE Solar Program and DOE RD&D objectives").

The project does not seem to be very far along and the budget is much smaller than for other projects being reviewed. One might hope that Acciona's field experience would come into play, but the effort to date seems to be materials selection related. Mostly only proposal level information was presented and even at that the technical content was practically nil.

No useful information.

The project nominally supports DOE TES goals, but it's unclear how much innovation is involved, as much of the proposed work seems to repeat prior art and the presentation did not make clear how the contractor intends to improve upon that work.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Fair ("Has significant weaknesses; may have some impact on overcoming barriers"). As with CSP012, The work reported to date and the upcoming effort for 2011 are noted as tasks, but virtually no information is provided on performance, size, configuration, etc. They mention a "lattice" structure, but with no details it's difficult to even see what this means, and why it provides lower pressure drop and better heat transfer over other options. There is no treatment of the module design; it is even unclear what the size is, or if the additional material costs for the presumably metal enclosure exceeds that of a two-tank system. Granted, this effort has been underway since September 2009, but there should be more reported results. In the material provided, there are no real data, and only one figure shows projected results (TRNSYS projected 93% efficiency, as with CSP012). This lack of information almost makes the R&D approach score lower, but since this work is being done by a respected solar company, the benefit of the doubt is given to them, as with the evaluation for CSP012, for their capability and thus the score is Fair.

The effort to identify serviceable materials seems relatively focused.

I cannot tell if anything was done because the speaker gave two content free talks.

The technical approach seems reasonable, but it's unclear that it benefits as much as possible from researching prior work on similar concepts.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments with respect to the concept are rated Poor ("Little or no demonstrated progress towards objectives or any barriers"). There are no accomplishments noted, and only vague comments about "…it is believed….modeling performed…began FEA…confirms…not to exceed…two tank …", etc.

Project seems to be just started. Progress appears minimal.

No progress was reported.

Progress to date seems commensurate with resources expended.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Poor ("Most work is done at the sponsoring organization with little outside interaction"). The project does not note the use of any outside organization. There are no presentations, papers or patents/disclosures noted.

No partners or collaborators identified. This is correctable. This sort of project would benefit from an independent technical advisory committee focused on the project. Obviously members would need to sign NDAs.

Collaboration isn't even mentioned in the presentation. This is a pity.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity. As with their earlier CSP012 project, the presentation does not show an effective plan for future work, and there is a degree of uncertainty associated with the exact system size, layout, and performance, and no indication that there is indeed a cost reduction potential. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) are not shown; only top level tasks are shown. Only the Phase II and III Go/No Go decision points are noted. There is little discussion of barriers to the realization of the proposed technology, such as the cost comparison with a two tank system, or what, if any, advantages are provided by the modules. The lack of a specific (not vague) plan that addresses the filler material, evaluation criteria, tests, etc., makes it necessary to judge this area as Poor (e.g., "Plans have little relevance toward eliminating barriers or advancing the program").

The effort to date is vague, the presentation in several places is identical to that for CSP012 and no real progress is shown in terms of either the design, filler material selection and evaluation criteria, or the potential for cost reduction. This work may have been done, but it is not shown.

No way of telling.

there is no effort to share any meaningful future plans.

Plans for the project appear reasonable. Longer-term plans were not presented.

Project Strengths:
Acciona long term commercial interest in the results.
Contractor experienced with PT systems.

Project Weaknesses:
Virtually no results of a specific nature are shown; given that this project has been conducted since September 2009, at least some results should be available. This lack of results is a major weakness. Of the mere 14 slides presented, only one shows "data" and that is a TRNSYS result indicating 93% round-trip efficiency; the other slide, showing properties, is merely a summary of information from sources, and even these aren't identified, nor are the results explained. For example, they list thermal shock, but give no more information on what it means.

Reviewer 6:
Does Acciona have the technical skill sets to address materials related issues not yet encountered in their commercial work?

Apparent isolation from prior art and the CSP community.

Recommendations for changes to the Project Scope:
The potential for cost reduction should be shown; quantitative results should be developed and cited.

Add an independent technical review task.

The Program should reevaluate the current level of funding versus the overall benefits of this effort.

Encourage wider interactions to improve benefits from prior art and increase potential innovative inputs.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP024
Presentation Title: Research and Development for Novel Thermal Energy Storage Systems (TES) for Concentrating Solar Power (CSP)
Investigator: Bergman, Theodore

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The novel thermal energy storage system program addresses several important DOE objectives, including, per the Multi-Year Program Plan: improve storage technologies, technology development to lower costs, pursuit of thermal storage to enhance dispatchability, and exploring advanced concepts. This program has a novel concept that uses thermosiphon and/or heat pipes in the thermal storage tank. Typically, this is applied to phase change materials, such as salts. The heat pipes can potentially improve the heat transfer to and from the TES. In principle, there could be some improvement in the heat exchange rates, and some improvement in the amount of energy stored and recovered, and the heat rates. It is not evident that there is a real cost advantage using heat pipes instead of conventional heat exchangers, but they plan to address this aspect.

This Criterion is rated Outstanding ("Project is critical to EERE Solar Program and fully supports DOE RD&D objectives").

Consideration of increased affectivity means of getting heat in and out of phase change storage is a very desirable focus area. Similar to what is being done on a smaller scale for dish Stirling systems, the use of heat pipes is proposed.

An enthusiastically presented project. The technologies involved are relevant and the project team seems to have relevant competencies. However, there must have been some analysis to suggest the concept had something to recommend it economically, and the results of that analysis were not apparent. Either way the concept just seems to have too much complexity, e.g. chart 17, to have any hope of being affordable. Is there a way to simplify the technical concept and retain the elements that are most promising relative to cost reduction?

Not clear that approach works well for thermal discharge. Cost and complexity argue that this technique would not be a cost effective way of providing thermal storage.

The project nominally supports CSP TES goals, but from the Q&A after the presentation is became clear that the concept and its economic benefits remain speculative. This appears to be a high-risk project.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Good ("Generally effective by could be improved; contributes to overcoming some barriers"). The advantage of this approach as stated lies in the reduction in the thermal resistance, but no analysis or data are shown that substantiate this. However, in subsequent emails with Dr. Faghri, additional explanations were provided that indicate to me that there is real promise, and he is clearly a leading expert in this field. However, it is not clear from the presentation what the major thermal resistance is and it appeared to me that it was unlikely that it's at the inner wall of a conventional heat exchanger, and more likely it's in the phase change material. It is noted that predicting the thermal resistance in the PCM is not easy, but, there are at least some results that could be used to bound the problem and give some indication that the overall thermal resistance with the heat pipe will lead to TES improvements. Not discussing the overall thermal resistance in the presentation, and the improvement available from heat pipes, is a major issue and should be addressed; it is reasonable to approach the problem this way, but the importance is such that it should have been addressed in the presentation. The subsequent emails did address this, but, it's my understanding that our role as reviewers was to address the work as reported, and therefore I am unsure as to how much of the subsequent correspondence I can take into account, in all fairness to other projects. But I am fully convinced that this project has the right people working on it who will address these concerns and others as well.

Lack of any treatment of a conventional heat exchanger pipe with fins, compared to their fluid loop with a series of heat pipes is another issue that should have been discussed. In particular, the system with a bank of heat pipes or thermosiphons (wicked or not) needs to be addressed in terms of the density, correlation with the required rate, potential improvements due to the essentially isothermal nature of the heat pipes (or thermosiphons) in the condensation region, etc., and especially how these approaches compare with more conventional approaches (e.g., finned heat exchanger with HTF passing through it, arranged within the TES). There is also some degree of uncertainty that the presenter and I discussed after the presentation regarding the heat removal from the TES. Charging the TES with the heat pipes was clear. What was not so clear was the configuration of these pipes and the radial and axial heat flux limits for the heat pipes operated in the reverse direction, and how this is considered in their study. Height and flux limits were not addressed; this may not be an issue for short heat pipes, but it was not addressed, and should have at least have been noted as an issue, if not a barrier. For example, if short pipes are used (as shown in the high temperature heat pipe), how will these be integrated with a utility scale TES system? It appears that the bulk of the work was with charging the TES, and little was done on recovering the stored heat, which in several respects is the more important problem. Later emails with Dr. Faghri made it clear that he fully understands this aspect, and that it will be treated in due course; he also agreed with my question about a difference in the axial heat flux for charge and discharge. I think it's safe to assume this will be covered in their work and good design practice will be used to ensure that the proper size, number, etc. are selected. However, I still wonder about the cost aspect, even if this is proven to be a superior approach.

However, the work on the heat pipe aspect and its integration with charging a phase change material is thorough. Whether or not a thermosiphon is appropriate for both charging the TES and later heating the HTF with the stored heat was not treated. Possibly this approach is to be used with near-horizontal thermosiphons, but even that raises some concerns. It is apparent that they have all of the experience and capability to address this issue (and others) and are developing the codes needed to determine such aspects as the PCM melting and solidification, heat pipe limits, sizing, etc., but these were not discussed in detail. Several of these aspects were unclear for both thermosiphons and heat pipes. For example, they show one slide of heat discharge from the TES entering the middle part of the heat pipe, but do not address any aspects such as the amount of fluid, possible capillary limits or radial heat flux limits at this central region. (A subsequent email touched on this in terms of the flooding limit for thermosyphons.) They note an "up to 6 cents/kwh LCOE impact" in one slide, but give no substantiation for this; there is clearly some potential reduction in using latent heat, but the issue here is the use of heat pipes to improve the latent heat TES, in addition to the basic improvement in TES from use of latent heat; this potential cost aspect should be addressed, and is an important barrier, especially for relatively high temperature heat pipes. It seems that at least preliminary estimates could have been made.

They note that novel heat pipes are being fabricated by their industrial partner (presumably, Thermacore); this is good approach.

Overall, this area is rated as Good ("Generally effective but could be improved; contributes to overcoming some barriers")

A great deal of analytical expertise exists on this project. Less clear about the ability of the crew to handle types of experiments that would be needed to show effectiveness of the approach in high temperature systems. The involvement of heat pipe companies is a good. Proposing to look at thermosyphons was not a very smart move for this kind of application.

It appears the technical barriers are understood and addressed at the bench scale, but some attention should be given to the costs of overcoming them in full size systems. It is likely the project will result in a small scale proof of concept with all components including the heat pipes needing to be scaled up. What is a realistic estimate of the cost of taking the approach all the way to proven long term operation at the 100MWt scale?

Effort not well focused on identifying critical pathways and attacking key pathways to overcome obstacles.

The technical approach seems to lack some focus on the critical issues of likely cost of this solution and performance barriers to heat extraction using the proposed techniques.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
They note that ATP was delayed about 6 months (March 2009). Thus, this report covers about one year of effort. The notable accomplishments include: materials review, the melting front model and how it is an improvement over earlier models and includes free convection, comparison of the model with the wax melting profile and heat pipe effectiveness for charging the TES, periodic charging and discharging, a journal article, and the effort to fabricate a novel heat pipe. The latter deserves some additional discussion; the novel aspects are unclear. They state that the heat pipes increase the amount of energy stored; it is likely more accurate to state that they appear to increase the rate of transfer of energy into the PCM, but even this is not substantiated by comparison with a conventional heat exchanger with a flowing fluid for their comparison curve. It is not clear what the “5 heat pipe” vs. “no heat pipe” means and thus it is unclear why this curve is important. It indicates an improvement, but it doesn’t provide real substantiation for the design approach. The predicted melting of tin appears to be thorough, and is apparently included as part of the validation of the model, and it’s noted that temperature, pressure, and velocity impact melt (and solidification) rates, but the purpose for this is not clear with respect to the TES and the design of this particular system. As noted above, additional effort to estimate the LCOE improvement is needed, even if this can only be estimated from existing models, and will require updating later. Given that their program is about 1 year in duration, the accomplishments and progress are rated Good (“Significant progress toward objectives and overcoming one or more barriers”).

A great deal of analysis has been performed. Not at all impressed with the very simple kinds of experiments performed at this point. Was quite concerned that the presenter had problems with the one of the reviewer's questions. The presenter should have been able to give an appropriate answer or show that question was irrelevant. Didn't seem to be able to do either one.

A lot of good work on a lot of aspects. That's the problem - too many developmental aspects. This is a heat transfer research lab's dream project. Who would take the hand-off?
Made progress but not in certain key areas.

The modeling and initial experiments have produced some useful results but the project team, having spent about half of the budget, still seems to lack persuasive evidence of likely technical and economic success.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of their effort is scored Good (“Some coordination exists; necessary coordination could be accomplished easily”). The participants are stated to be: Thermacore, Hamilton Sundstrand Rocketdyne, and Aavid Thermalloy. Two peer reviewed journal articles are cited for their work.

Working with heat pipe manufacturers is very good on a project of this type. Little concerned about possible lack of insights when it comes to high temperature experiments. I am sure that this group will be quite prolific in publishing results.

Two universities, no industry. At least the universities could organize an industry advisory committee. If that proved infeasible it would say something about how far over the commercial horizon this research actually is.

It appears that the collaborators are mainly subcontractors.

Little evidence of interactions outside of the project team was presented, but there were two publications.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Proposed future work is listed as goals/deliverables, performance status, notes, some high level Go/No Go decision points, etc. There are some general dates for these, but no detailed schedule (Gantt chart). There is a degree of uncertainty associated with the heat pipe and TES system size, layout, and potential performance improvements, and little if any quantitative evidence that there is indeed a cost reduction potential through the increase in heat transfer rates (decrease thermal resistance). The degree of effort (personnel, labor hours, major tasks, schedule, etc.) are not shown; only top level tasks and due dates are shown. Some key decision points are noted, but there is little discussion of barriers to the realization of the proposed technology; some of these are noted above primarily as my questions. In spite of the lack of a specific plan, it is apparent that the team and PI have the capability to conduct the work. This effort is shown as top level tasks with due dates; these address those aspects of the future work needed to have this concept operate as required, and I believe, in part based on subsequent email correspondences, that the issue of heat removal from the TES will be thoroughly addressed. Therefore, this criterion is judged as Good (e.g., "Plans build on past progress and generally address overcoming barriers").

The effort to date is provides a credible option using heat pipes and/or thermosiphons, coupled with a better model for determining the PCM melting characteristics. But, quantitative evidence or analysis showing that the heat transfer improvements are real, and that the LCOE cost reductions occur as a result of the use of heat pipes in the TES are lacking in the presentation. It is unfortunate that this project faced a six-month delay in ATP.

Moving toward a high temperature experiment is appropriate. I have some concerns about how that experiment will be set up, and how the proper kinds of results will be sought.

The future plans seems defensible but do not identify and attack specific barriers which are presumably TBD based on the early phase work.

Need to redirect research to better establish core issue; the discharge cycle.

I would like to see more emphasis on defining and demonstrating performance and economic metrics for success of the project.

Project Strengths:

Good results are obtained for one year of effort in the modeling of the TES PCM melting and in the initial system design and use of heat pipes.

Heat pipes need to be evaluated for transferring thermal energy into and out of high temperature phase change storage materials.

Clearly, a well-motivated and competent group of researchers.

Qualified project team.

Project Weaknesses:

Little treatment is shown of the heat pipe/thermosyphon limits, how these will operate with heat removal at the central region, whether or not the thermal charging and discharging have comparable rates, how large the system is, and how many heat pipes are needed, and the projected costs. It was not stated that the thermosyphons can have wicks; this was learned from subsequent emails. No comparison is shown with a conventional heat exchanger with fins. No quantitative results supporting the potential LCOE reductions are shown. However, I am convinced that in due course these, and other issues, will be addressed, but the lack of treatment, even if only as barriers to be addressed in future work, caused me to express a number of concerns. I think overall my concerns will be addressed, and this project deserves further support.

Presentation did not give me high confidence that the investigators may be able to carry out high temperature experiments that will be needed as the next step in this line of development.

No market or cost context.

Lack of external interactions and inputs.

Recommendations for changes to the Project Scope:

Additional quantitative analyses and/or data should be shown for the heat transfer limits and types, how a wicked thermosyphon differs from a heat pipe with a wick, and how the axial heat flux varies depending on orientation and whether the TES is being charged or discharged. A comparison of the cost against a conventional heat exchanger with fins is needed, and a projection of potential reductions in LCOE.

Try to interest industry in order to calibrate interest in the basic concept and identify practical design issues that could derail commercial acceptance and project finance if the design remains so complex.

The Program should reevaluate the current level of funding versus the overall benefits of this effort.

Encourage quantification of potential for success and benchmarks of such.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP025_
Presentation Title: Advanced CSP R&D: Advanced Reflectors
Investigator: Kennedy, Cheryl

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The advanced CSP project successfully addresses several key DOE objectives, including, per the Multi-Year Program Plan: "…transfer R&D concepts…to the market place…and assist U.S. industry." This project also supports the DOE focus on lowering costs and exploring advanced concepts, primarily by providing, for example, optical evaluation tools, extensive data on reflector materials, laboratory and field measurements, novel reflector materials, development of standards, and a long term, very extensive activity with dozens of other organizations and researchers throughout industry, universities, and other national laboratories, including both U.S. and foreign. Within the context of DOE and industry interest in CSP systems, it therefore rates a score of Outstanding ("Project is critical to EERE…and supports…objectives").

Looking at a variety of topics related to reflectors and other optical components related to the CSP. Very important for the program. Particularly cleaning issues.

Clearly the project provides critical support to an industry that cannot support private test, qualification and certification laboratories at this time. Having NREL validation of materials selection and technical performance of critical components can be very valuable to project developers seeking financing.

This is one of the most important and cost significant areas of the program.

The project contains many subprojects, most of which are critical to the EERE Solar Program's success. The numerous components of the project make it difficult to comprehensively cover the whole in a reasonably concise presentation. This one erred significantly on the side of presenting too much information!

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The past and future Approach to Performing R&D are Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"), as seen in the Accomplishments. It is also clear that the upcoming effort for 2011 and beyond is based on improving the laboratory, tools, data archiving, support to industry, and expanding the number of skilled personnel needed to continue to develop, monitor, disseminate, archive, and support one of the key areas in CSP: achieving and maintaining high performance reflectors. This project deals with a wide variety of reflector issues, including mirror characterization and testing, life predictions, advanced coatings, anti-soiling and environmentally friendly cleaning, and reflector and durability standards. Involvement of many organizations is a particularly notable important part of the approach. Both near term and long term, more basic research are discussed. Detailed tasks with due dates, status, and milestones are presented. Tools for performance measurements are made available throughout the solar program. The data base is extensive; making it web-based is a very important aspect and is particularly notable.. Development of a specular, spectral reflectometer is important; the comments on the limitations of the portable reflectometer have long been recognized (accuracy, sensitivity to how it was used, etc.), but there were few alternatives; now a better approach will be available. Outdoor testing, corrosion, etc. are also critically important. The observation that lead based backing coatings are generally effective, but newer coatings present challenges, and the efforts to achieve durable, long life, high performance reflectors is one of the most important aspects. The mirrors represent a significant fraction of the cost, and improving their performance has a first order effect on the major cost aspects of the major cost of any CSP system: the collector. This projects approach for many years, now being improved greatly, properly stresses near and far term R&D, collaboration, support to industry, data collection and dissemination, etc. It is clearly an outstanding aspect of the DOE solar program.

Quite varied in scope. Seems as though all work focuses on very important practical issues. Not clear to me how this can be carefully prioritized. Also not clear how this is coordinated with related work that might go on at Sandia.

The attention to lead free materials is important and well represented in the presentation. Product stewardship is a major emerging issue for solar energy in general and has been for some time with PV. It is excellent to see NREL addressing related issues. Addressing them more holistically should be a future goal. To the extent the presentation involved metaphorical drafts from a fire hose, there is a plausible concern that the overall effort may lack a logical progression of work to first address the most commercially relevant testing as a matter of priority. Is there a way to differentiate between requests for help that are critical to active projects and requests that essentially support non-project related and more generic research?

Suggest enhancing angle dependent specular reflection and absorptivity measurement capability to include wavelength dependence.

My impression is that the project components are generally very well-designed, feasible, and commercially promising. However, the blizzard of slides with so many details makes it difficult to assess this critically.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
This criterion is judged Outstanding (Excellent progress toward objectives; suggests that barriers will be overcome"). Much of the accomplishments are listed in the above. In addition, the amount of work accomplished for the personnel available, and up until recently, some limitations in facilities, make this project and the personnel even more impressive. Recently, they have acquired and installed about $750K in new equipment, plus adding more personnel. One other notable aspect is their support to a cost analysis (Abengoa, etc.), which was extensive. It is also notable that the presentation is 76 slides, the majority of which are rich in real results, data, accomplishments, and plans.

There is no question that the work that is going on here is, in general, quite important. It absolutely needs to be done. Less clear to an outsider is what constitutes good progress toward goals, and if other pathways would result in better progress.

The project provides essential data for use in improving system level performance.

Excellent job in providing measurement surfaces to the solar community and carrying out research tasks to support the increased sophistication of the program.

Many examples of good progress were presented, but too quickly to fully understand them and weigh their importance. With some care to winnow the presented data, I think that this would have been an "Outstanding".

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This is judged Outstanding ("Close, appropriate coordination with other institutions; partners are full participants, publication or presentations"). The collaborations are extensive. The foreign collaborations alone are listed on several pages. There are seven recent publications. Technology Transfer is thus extensive, thorough, and very supportive of CSP.

Seems to be collaborating with almost all organizations with any kind of hand in optical types of issues. Makes quite elaborate equipment techniques available to the industry. Publishes and patents quite regularly.

Only concern is how division of labor takes place between this lab and Sandia. This is especially true since both have apparently tapped very large streams of ARRA funding for equipment and general laboratory upgrades.

Technology transfer is almost inherent in the industry interactions associated with the NREL test capabilities. It is good to see NREL able to support so many requests within the available budget. It suggests a well-managed lab. The industry is fortunate to have access to the lab capabilities acquired with DOE support over the years. One wonders if it might be worthwhile to have a more formal industry advisory process than is currently apparent.

They have many collaborators due to the role this work plays in solar development. There is also a significant publication list.

Coordination with others in the CSP community is clearly outstanding in this project.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Proposed future work is listed as goals/deliverables, performance status, notes, some high level Go/No Go decision points, etc. There are some general dates for these, but no real schedule

(Gantt chart). Since this is partly due to the fact that they provide a very high degree of support across the CSP industry, much of which cannot be scheduled a year or so in advance, having task statements, goals, etc. is sufficient. This area is judged as Outstanding ("Plans clearly build on past progress and are sharply focused on barriers")

Excellent program.

By and large it is to do more the same, but now with improved facilities. Will continue to be a great service to the industry.

Future plans are clear and appropriate. However, the project should begin to anticipate a stable commercial market and adjust its strategy accordingly. More emphasis is need on service life prediction, including transparent models and accelerated test protocols. Independent testing to confirm design life estimates, failure and warranty criteria and support warranty decisions should be an emerging focus, along with efforts to support cradle to cradle product design and materials selection.

I would urge the rapid development and a measurement program for the Spectral Specular Reflectometer. Significant mirror degradation can occur due to spectral changes in the specular reflection with little indication from conventional measurements.

Both near-term and longer-term planning shows a sharp focus on critical barriers.

Project Strengths:
Systematic approach to developing data base, novel reflectors, and test capability. Superior support/collaboration with industry.

Very strong service to the industry.

High bandwidth coverage of the relevant issues.

Well qualified and very experienced PI, as well as world-class support organization.

Project Weaknesses:
Need for more attention to data for use in long term performance and degradation prediction.

Presentation style.

Recommendations for changes to the Project Scope:

Maybe need more technical staff and to start working with industry to identify the current testing that can be taken over by private laboratories as the industry is able to support them.

Continue or expand this program, it is at the core of the technical mission of the CSP program.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP026
Presentation Title: Project Title: Advanced CSP R&D
Investigator: Turchi, Craig

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
This s-CO2 power cycle analysis project addresses several key DOE objectives, including, per the Multi-Year Program Plan: to fully incorporate CSP efforts into the SAI, transfer R&D concepts…to the market place…and assist U.S. industry. This project also supports the DOE focus on lowering costs, primarily by providing an assessment of a possible advanced power cycle and related analysis tools. Although a relatively low funding level, the use of the University of Wisconsin grad students(s) is a cost effective way to determine if supercritical CO2 holds real promise. This Criterion is rated Outstanding.

Seems to have a primary focus on supercritical CO2 Brayton cycles. Very little has been done, but the area is being initiated. There are some indicators that this cycle has some promising performance characteristics, so it seems to be a good move to look at it in some details.

Appears effort is just getting started. The arguments for interest in s-CO2 seem to be related to efficiency and complexity. Some preliminary cost trade-offs would be appropriate.

Advanced power cycles should be an important element to the future CSP program. While they are not called out explicitly they fall under the general topics of performance and capital cost headings. Future energy plans should probably deal with this issue.

This project, while still speculative, appears promising to significantly advance EERE CSP cost goals if it is successful.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
This is a relatively new project (presumably with ATP of October 1, 2009), and although two papers are cited, there are few results that can be reported at this time. However, it has the right combination of an early, detailed analysis of the thermodynamics, combined with systems aspects, and it involves an existing tool, plus collaboration with UW and SNLA. The overall evaluation is Good ("generally effective but could be improved; contributes to overcoming some barriers"), as substantiated in the following comments.

The approach to achieving the key objectives is addressed in part through collaborations, especially with UW, but also with a recent FOA. Their approach also rightfully addresses performance as determined from a comprehensive model. Some cost aspects could be addressed as well, to substantiate the LCOE cost reductions. This project to date compares the system efficiency with that of Rankine and helium Brayton cycles, primarily, as a function of temperature, and this is a good start. However, it would be relatively easy to incorporate air Brayton and combined cycles as well. It is also important to include pressure ratio, and ensure that these are consistent with the overall system. The lack of any discussion of the pressure ratio and the temperature limits for a real system makes it difficult to evaluate some of the practical aspects and to ensure that the comparisons between cycles and working fluids are consistent.

This work is clearly important to at least consider at the trade study level and can benefit from nuclear industry developments. What is not clear is the exact nature of the R&D approach for 2011 and beyond. The approach is mentioned at a top level for 2011, primarily as general milestones (model the system, use TRNSYS, etc.). Past experience at NREL and UW show that they unquestionably have the ability to develop this analysis, but the nature of this is not shown as a task breakdown, milestone, or Gantt chart, with barriers, Go/No-Go decision points, etc. Also, there is no consideration or comparison of optional approaches (air Brayton, combined cycle, if for no other reason than simply having a more complete comparison). Given that these issues are not addressed, the overall evaluation is Good ("Generally effective but could be improved; contributes to overcoming some barriers").

Seems to be quite a generic study to probe some aspects of the cycle performance.

Probably not far enough along to definitively identify technical barriers to s-CO2, but if we already know it only applies to Brayton, that is not encouraging.

Is this work duplicating the Sandia work? It seems doubtful that supporting one graduate student and attending a few conferences is a cost effective way of enhancing the work. Is Brayton work to be part of project? This project needs a clearly defined goal.

The technical approach appears well-designed, feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
This project is new and still in the planning and early execution stages, and apparently UW is not yet under contract ("contract under negotiation"). Their Technical Accomplishments and Progress are therefore difficult to judge, and the funding level is relatively low, but at this stage of "trade study" that funding level is appropriate. The option of super critical CO2 is relevant and they are likely to be able to at least conduct the cycle analysis and consider such aspects as use as the HTF or perhaps only as the working fluid, pressure ratios as well as temperatures, practical limitations on both, and in due course will hopefully include other options (trough, Dish, air Brayton, combined cycle, etc.), if for no other reason than simply a more comprehensive comparison. Given the capabilities of the organizations and personnel, and the nuclear industry interest (and the likely leverage of that development), the benefit of the doubt leads to a score of Good ("Significant progress toward objectives and overcoming one or more barriers").

Work is moving along on a low level. Comparisons have been made for the efficiency of several advanced cycles as a function of turbine inlet temperature. The supercritical CO2 cycle. It showed better performance over a range of temperatures where central receiver plants would operate.

Good start, but I wonder why some of the conceptual alternatives receiving venture capital support are not included in the advanced concept category. It would seem there is a rich opportunity to make a contribution by assessing their development costs and risks, though the companies promoting them might not like the results being in the public domain. Does DOE have a responsibility to help investors assess project risk by conducting independent assessments of generic approaches never deployed commercially at scale?

Very little work has been done, but this is a new project so it is hard to assign a score.

The project is still in early stages, but progress is evident.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This effort is Good ("some coordination exists; necessary coordination could be accomplished easily"), in that they have outside interactions and work with UW and SNLA. Two papers are cited.

Collaborating with the University of Wisconsin, Sandia, and Enginomix. A couple of presentations/papers have been given already.

Limited funding, limited collaboration. Why not expand to support work at more universities? It would be an investment in developing engineering talent for the future industry. Collaboration intent is shown not only in CSP community but also in related nuclear power projects.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area cites several a few key task objectives, but it lacks specificity and is scored Fair ("Plans may lead to improvements, but need better focus on barriers"). Several areas in particular deserve attention: potential use, or recommendations not to use, s-CO2 with the three CSP systems, a more general comparison with other options (air Brayton, combined cycle, both as standalone and hybrid, etc.), pressure ratios, temperature limits, etc. The presentation does not show an effective plan for future work. It is necessary in DOE plans to address barriers and uncertainties; these are not noted as specifics (e.g., development cost of the turbine, time and costs required to develop to the stage of potential use by the solar industry, any problems encountered by the nuclear industry that are relevant, how this compares to conventional systems, likelihood of achieving the apparently predicted LCOE cost reduction of 12% (and, what this was compared to), etc. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) is not shown. Although in general their work supports major DOE goals, as noted above, their proposed future work should, at a minimum, be addressed at least to the subtask

level for the major milestones, and preferably a Gantt chart. There are no decision points noted. The approximate value, and labor level, associated with the UW contract is not shown. The anticipated ATP is not shown.

There is no discussion of barriers such as verification of the analyses and codes, which appear to be limited to s-CO2, such as running cases other than s-CO2 that can be compared with other generally known results for conventional systems. Their ability to conduct work of this nature is unquestioned, and the personnel are well-qualified and experienced. The lack of specificity in their plan, including specific barriers and alternatives to overcome these barriers, makes it necessary to judge this area as Fair ("Plans may lead to improvements, but need better focus on overcoming barriers").

Overall, there is no question of the worth of at least a "trade study" and systems analysis of s-CO2. But, the planned future effort is unclear in terms of specifics.

Some tasks are outlined for the year to come, but what they actually involve is not at all clear. They really don't have much meaning to someone unfamiliar with the thoughts of the PI.

See criterion 3 comment. This project could benefit from an industry advisory committee, or perhaps an investor advisory committee.

The proposed FY 2011 milestones future FY 2011 future plans do not agree. Is Brayton part of the plan or not? Develop a TRNSYS-based code for what?

Planning is excellent and sharply focused on key barriers.

Project Strengths:
Novel power cycle, and possibly HTF, that deserves consideration.

Exploring new directions for CSP.

Looking at high solar percentage NG hybrids is directionally correct in the context of emerging climate policy and related economic levers.

NREL and experienced university partner with collaborative approach.

Project Weaknesses:
This project needs to develop a plan, task definition, schedule, budget allotment to a lower level than the total funding, definition of the barriers and alternative pathways to overcome these

barriers, how this will be accomplished, and over what time frame. It can be safely assumed that good work will be accomplished, but the lack of any level of detail for the 2011 effort and beyond is a major weakness.

Doesn't seem to have much of a foundation in recent work and no reference to all of the advanced concept work done in Europe in the past couple of decades.

Recommendations for changes to the Project Scope:
Project would benefit from a specific plan, including task definitions, schedule, budget allotment to a lower level than the total funding, definition of the barriers and alternative pathways to overcome these barriers, how this will be accomplished, and over what time frame. A more general comparison with other options (air Brayton, combined cycle, both as standalone and hybrid, etc.), pressure ratios, temperature limits are needed, in addition to the focus on developing this alternative working fluid.

Some repowering concepts could be categorized as advanced. Should the project look at them?

What is the programmatic role of this project? It seems parallel to the effort at Sandia but is it duplicating or supplementing that more extensive work? Is this an advanced power cycle program (if so there should be more emphasis on Brayton cycle) or dedicated strictly to the advanced CO2 cycle? These are programmatic decisions.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP027_
Presentation Title: Advanced CSP R&D: Advanced Absorbers
Investigator: Kennedy, Cheryl

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The advanced absorbers project successfully addresses several key DOE objectives, including, per the Multi-Year Program Plan: "…transfer R&D concepts…to the market place…and assist U.S. industry." This project also supports the DOE focus on lowering costs and exploring advanced concepts, primarily by providing, for example, evaluation tools, extensive data on absorber materials, new absorber materials that appear to offer high absorptivity and low emissivity, development of standards, and a long term, very extensive activity with dozens of other organizations and researchers throughout industry, universities, and other national laboratories, including both U.S. and foreign. In particular, the CRADA with Schott is particularly commendable in terms of industry collaboration. Within the context of DOE and industry interest in CSP systems, it therefore rates a score of Outstanding ("Project is critical to EERE…and supports…objectives").

Coatings that will boost absorptivity and lower emissivity is the focus. Particularly for high temperatures (e.g. towers) this could be a very good supplement to the CSP program.

Major funding seems to be industry and CRADA which is good. Hard to tell which effort were funded which way.

Advanced absorbers are an important element in CSP because optical losses can affect the LCOE for all technologies.

This set of projects generally supports the EERE CSP goals for lower LCOE. The relationships between them is not very clearly described in the presentation. Also there appears to be some overlapping description of facilities/staffing developments between the two Kennedy presentations, which is confusing.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The basic approach is very similar to that for the companion effort on reflectors, much of the evaluation is essentially identical to that for the earlier presentation, and overall this project deserves the same high evaluation score. Thus, the past and future Approach to Performing R&D are Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"), as seen in the Accomplishments. It is also clear that the upcoming effort for 2011 and beyond is based on improving the laboratory, tools, data archiving, support to industry (especially the CRADA), and expanding the number of skilled personnel needed to continue to develop, monitor, disseminate, archive, and support one of the key areas in CSP: developing advanced absorbers. This project deals with a wide variety of issues, including characterization and testing, life predictions, advanced absorber coatings, and development of standards. Involvement of many organizations is a particularly notable important part of the approach. Both near term and basic research are planned. Detailed tasks with due dates, status, and milestones are presented. Tools for performance measurements are made available throughout the solar program. Eliminating a large number of potential materials and developing some with promise (Cermet, etc.) is notable. Building up the lab capability (personnel, equipment, etc.) is critical and deserves full support. Field testing at high temperature in air is also critically important, and appears to be a part of the CRADA, but there is little detail on this. This project's approach over the past years is now being improved greatly; it properly stresses near and far term R&D, collaboration, support to industry, data collection and dissemination, etc. In the presentation, there are a few instances of erroneous comments on the companion reflector task, but this is understandable. The presentation and supplement slides provide good evidence of a successful approach. This project is clearly an outstanding aspect of the DOE solar program.:
I have no background in the physics/chemistry related to this type of development, so I cannot evaluate this thrust very well. However, it is clear that this general effort is something that really should be done.

Obviously industry funded efforts can be assumed to address real barriers facing the sponsors.

Need capability to measure angle dependence of absorption and emmitance, especially for thin film coatings.

The project approaches to characterizing and fabricating selective surfaces appear well-designed, feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
As with the companion reflector materials project, this criterion is judged Outstanding (Excellent progress toward objectives; suggests that barriers will be overcome"). Much of the accomplishments are listed in the above. In addition, the amount of work accomplished for the personnel available, and up until recently, some limitations in facilities, make this project and the personnel even more impressive. It is notable that the presentation has 45 slides, the majority of which are rich in real results, data, accomplishments, and plans.

Primarily set up with a number of pieces of equipment. Seems as though there is a lot of set up kinds of efforts are required, because this seems like a totally new thrust. Also some concern about possible overlap with Sandia.

Modeling efforts have produced meaningful results and no doubt efforts to operationalize measurement capabilities will pay dividends.

Good technical progress was demonstrated, especially in the tube receiver development with Schott Solar.

There has been significant progress toward several project goals in the recent period.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This is judged Outstanding ("Close, appropriate coordination with other institutions; partners are full participants, publication or presentations"). The collaborations are extensive. The foreign collaborations are extensive; the CRADA with Schott is commendable. There are seven recent publications, but only one is specifically for receivers, as opposed to reflectors, etc. The round robin standards task is shown as complete, but there is little information on what this is. Overall, Technology Transfer is extensive, thorough, and very supportive of CSP.

Work with one industrial partner is about all of the interactions that seem to exist. Using samples from a variety of developers is also noted. Several papers are listed in the supplemental slides, but very few are related to this particular effort.

There is an apparent need for a policy to determine what work NREL should do in support of industry and what work industry should do for itself. I didn't get a sense of what this policy might be. It might be the first question for an industry advisory committee for the project.

NREL by virtue of its role has a multitude of collaborators. The patent and publication record are outstanding.

Project collaborations and publications are outstanding.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
As with the companion reflector project, the proposed future work is listed as goals/deliverables, performance status, notes, some high level Go/No Go decision points, etc. There are some general dates for these, but no real schedule (Gantt chart). Since this is partly due to the fact that they provide a very high degree of support across the CSP industry, much of which cannot be scheduled a year or so in advance, having task statements, goals, etc. is sufficient. This area is judged as Outstanding ("Plans clearly build on past progress and are sharply focused on barriers")

Excellent program.

Major effort seems to be, as was the case with reflectors, support of industry related to making measurements. I cannot evaluate the technical aspects of the other future plans (primarily developing selective surfaces). There is certainly value in developing appropriate coatings for tower applications.

Working toward standardization of testing methods is very important.

In fairness, ramping up to support and strike a balance between industry's near term needs and long term testing that would be better done in shared facilities, is a challenge. Set aside some time to plan.

Near-term planning is good. Longer-term plans were not described fully.

Project Strengths:

Systematic approach to developing data base and test capability. Superior support/collaboration with industry.

Development of high temperature selective coatings is a very desirable goal. So this work is applauded.

The project leader certainly has impressive bandwidth

NREL team and collaborative environment.

Project Weaknesses:

Possibly planning and prioritization.

Peer review presentation could be better organized to highlight only most significant elements.

Recommendations for changes to the Project Scope:

I'd really like to see an industry/university advisory group for this effort.

Continue the good work. It seems like a lot of work for the amount of DOE funding, but the CRADA and allied programs bring people and facilities beyond that level.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP028
Presentation Title: Sandia Advanced Concepts
Investigator: Ho, Clifford

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The Sandia advanced concepts project addresses several key DOE objectives, including, per the Multi-Year Program Plan: to fully incorporate CSP efforts into the SAI, transfer R&D concepts…to the market place…and assist U.S. industry. This project also supports the DOE focus on lowering costs, primarily by addressing cross-cutting technologies. This project covers a broad area: troughs, optics, modeling, solid particle receiver, molten salt heat transfer, selective absorbers, and FOA support. There are many partners, including six universities. Over 20 publications, including 4 technical advances and 1 patent have resulted from this effort since the last peer review. The funding has increased from $500K to $1.9M this year to cover four new tasks. This Criterion is rated Outstanding ("Project is critical to the EERE Program and fully supports DOE RD&D objectives").

Deals with three major areas: decreasing capital costs, improving performance, and decreasing technology risk. More specifically the issues involve advanced collector analysis, optical methods, advanced modeling generally, solid particle receiver modeling, molten-salt heat transfer, and selective absorber for tower receivers. Most all of these have particular emphases on modeling with some assistance from experiments.

The presentation was clear with good explanations of the rationale behind the major thrusts. Does not appear Sandia and NREL are defining "advanced concept" the same way.

This collection of projects fully supports the EERE CSP goals and is critical to the Program.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The overall evaluation is Good ("Generally effective but could be improved; contributes to overcoming some barriers") as substantiated in the following comments.

The approach to achieving the key objectives is addressed in part through collaborations. Each area has its own funding and milestones. More detailed comments are provided below for the major areas of effort.

Advanced trough analysis: Two collaborations (SkyFuel and Abengoa) are cited. Larger aperture troughs are being investigated, with fewer troughs being required, or with larger receiver tubes, less pumping power cited as advantages. FEA analyses were conducted for wind and gravity loads effects.

Optical Methods: Collaborations formed with PWR and NREL. Various optical methods developed and used; a toolkit for industry is being developed. However, past optical methods used at Solar One and at Sandia are not addressed; this is puzzling, since they were used successfully. This is perhaps an oversight. Another approach used by McDonnell Douglas and SES in the past, again, successfully, is also not noted.
Advanced Modeling: Numerous collaborations cited. Applying stochastic models to developing most important aspects for future R&D. Developing a combined structural, optical, and thermal approach to predict effects of thermal and wind. Have established rankings or sensitivities for various CSP effects (average insolation, absorptivity, heliostat cleanliness, etc.). Integrating deformed shapes, slope errors, etc. into the ray trace code. In the process of validating this code with tests. Considering glare/glint aspects.

Solid Particle Receiver: Developing design concepts and conducting CFD analyses; collaborations with Georgia Tech and DLR. Parametric and performance analyses conducted. Cite advantages of particle receivers (more efficient power cycles, cheaper thermal storage, lower LCOE). However, these assertions lack specific substantiation. There is also the question as to why solid particle receivers are superior to direct impingement receivers or to volumetric (air) receivers. The higher efficiency power cycles are not defined, but it is assumed that they may include Brayton, and possibly Brayton with Rankine as a combined cycle, but this is left unclear. The impression is that solid particle receivers are promising, but why they should be studied to the exclusion of other approaches, with over 20 years of development experience (primarily in Germany, Spain, and Israel) is not stated. There should be some level of justification for solid particle receivers in the presentation, in addition to statements as to their improved performance and cost reductions.

Molten Salt Heat Transfer: Preparing a report on lessons learned, Solar Two results, etc. for dissemination.

Selective Absorbers: This effort is particularly interesting because it addresses improved coatings (selective surfaces, at higher temperatures), and improved application methods, such as solution methods and thermal spray.

Overall, this effort is relatively comprehensive, but it does have some apparent "holes" with respect to other promising alternatives for optical evaluation and alignment, advanced receivers, and reporting of results (e.g., receiver coatings optical properties, etc.). Given that a number of alternatives and results are not addressed, the overall evaluation is Good ("Generally effective but could be improved; contributes to overcoming some barriers").

There are barriers addressed, but as best I understand them they are not as concisely defined as they might be. It is the kind of project I would like to have...somewhat fuzzily defined. The finite element analysis of the commercial trough designs is something I am surprised that the developers had not done earlier. They certainly should have. Summarizing the lessons learned from Solar 2 is quite a good idea. It is too bad something like this wasn't done earlier. Alignment of heliostats is a good area for work. Need to make sure what work had been done many years ago for possible assistance with that. The advanced modeling is one of the really fuzzy aspects of the work. The glint and glare analysis is another important one, particularly considering the interest of the outside agencies. Solar particle receiver is another aspect that is less focused in my opinion.

Systematic approach to identifying barriers and opportunities. All of the tasks, except perhaps particle receivers appear to have near term potential for use or reference by the industry.
The technical approach in all project areas is well-designed, feasible, and commercially promising. (And the presentation was comprehensive and comprehensible!)

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
Technical accomplishments are covered in about 20 publications and for the most part, well summarized in the presentation. They track milestones in a formal manner, with scheduled and actual completion dates, status, etc.; this is a well-managed approach with accountability and is to be commended. They are on schedule and within budget, and have formed a substantial number of collaborations. Additional details are provided for each area.

Advanced trough analysis: Established two collaborations (SkyFuel and Abengoa) and analyzed gravity loading on LS-2.

Optical Methods: Four ASME papers, developed a matrix for optical methods for the tool kit, and held a meeting with NREL on optical methods. Apparently, however, industry was not involved and past optical methods used at Solar One and at Sandia may not have been addressed.

Advanced Modeling: Numerous collaborations formed. Three major areas of accomplishments are noted, including: applied stochastic models to developing most important aspects for future R&D, developing a combined structural, optical, and thermal approach to predict effects of thermal and wind, established rankings or sensitivities for various CSP effects (average insolation, absorptivity, heliostat cleanliness, etc.), in the process of integrating deformed shapes, slope errors, etc. into the ray trace code (note, however, that this has been done in past work by others). In the process of validating this code with tests. Considering glare/glint aspects. Recommended using CDF of 30 year data rather than TMY data.

Solid Particle Receiver: Evaluating C-shaped and face down configurations, conducting CFD analyses; collaborating with Georgia Tech and DLR.

Molten Salt Heat Transfer: Preparing a report on lessons learned, Solar Two results, etc. for dissemination.

Selective Absorbers: Although they have characterized some coatings, determined some structural and optical properties (not specified, no data shown) and considered the morphology, they gave no results indicating that they are achieving an improved coating. They may have such results, but they were not shown. I could not find a paper related to this activity in their citations.

The overall rating is Good ("Significant progress toward objectives and overcoming one or more barriers").

Considering the fuzziness of some of the goals of the work, I think good progress is being made.

Program seems productive in terms of disseminating results. This is very important and a refreshing contrast to some of the FOA presentations.

Progress toward objectives is excellent.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This effort is Outstanding ("Close, appropriate coordination with other institutions; partners are full participants, publication or presentation"), in that they have outside interactions and work with about 15 organizations, including foreign, State, and 6 universities. Twenty publications and one patent are cited.

A large number of partners are listed, ranging from universities, through various solar system developers, to the California Energy Commission with many others in between. Quite an impressive list. It probably is the case that most university interactions are via interns hired at Sandia.

Not easy to guess the exact nature of the collaborations but the project seems to be oriented toward tasks that would involve dialog with partners.

Collaborations and publications are outstanding.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area cites numerous key task objectives, milestones, and has a formal tracking system that shows planned and actual completion dates, status, etc. The presentation does not show a detailed plan for future work, in the form of a schedule (Gantt chart), but it does address barriers

and uncertainties. The degree of effort (personnel, labor hours, major tasks, schedule, etc.) is not shown, but the funding for the six tasks is shown. They state that they are on schedule and within budget, but actual data (status of the schedule and percent funding remaining, etc.) are not shown. Their ability to conduct work of this nature is unquestioned, and the personnel are well-qualified and experienced. The lack of a detailed plan in the presentation may be an oversight. The projects objectives and task breakout are clear. This area is judged as Good ("Plans build on past progress and generally address overcoming barriers").

Broad, comprehensive study of critical aspects, but some options, past work, and alternative approaches are not noted.

Most of the future work is something like "continue what has been taking place."

Agree with the emphasis on cross-cutting tools models and technologies. As with the NREL advanced concept work, the question might be whether some of the active commercial ventures in the US are not actually pursuing advanced concepts, so the question might be how could this project complement and supplement private sector efforts on these concepts and should it conduct independent technical and economic assessments of the generic approaches to flag technical and economic risks that could be addressed in partnership with the project developers.

Plans clearly build on progress and are sharply focused on barriers in both the near term and longer term.

Project Strengths:

Well documented results, very high degree of collaboration, and proven capability to accomplish the objectives.

Excellent project team and world-class facilities.

Project Weaknesses:

Certain alternative receiver design and optical techniques are apparently not being considered. The NREL/SNLA meeting on optical measurements, etc., could have involved industry and others. This could have resulted in additional information and techniques that may be applicable, especially since some of these passed DOE acceptance tests at the time, and others were shown to meet the requirements needed at that time.

Not sure there particle receivers are part of a solution having affordable development costs.

Recommendations for changes to the Project Scope:

A more detailed plan for future work, including a schedule (Gantt chart), with the degree of effort (personnel, labor hours, etc.) is needed. Industry was not involved and past optical methods used at Solar One, McDonnell Douglas, and at Sandia may not have been addressed; these should be included in trade studies and may still have merit.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP029
Presentation Title: Thermal Energy Storage
Investigator: Glatzmaier, Greg

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The NREL TES program addresses essentially all of the primary DOE objectives, including the focus on thermal storage. The Multi-Year Program Plan objectives include: …. fully incorporate CSP efforts into the SAI, improve storage technologies, transfer R&D concepts…to the market place…and assist U.S. industry. The DOE focus is on technology development to lower costs, pursuit of thermal storage to enhance dispatchability, exploring advanced concepts, and reducing barriers to market penetration. Essentially all of these objectives are addressed are addressed. The performance and cost modeling and the process and components integration lab are the primary efforts. There is substantial collaboration, and part of their effort will support and evaluate FOA results. Overall, this criterion is judged to be Outstanding ("Project is critical to the EERE Solar Program and fully supports DOE RD&D objectives").

Thermal storage understanding is quite important. What is going on in this project is somewhat unfocused--a little bit of everything. It is the be-all, end-all project on storage.

The project is certainly relevant and apparently was driven by an opportunistic response to stimulus funding of "shovel ready" facility investments an national labs. What was missing in the presentation was how it relates to the many FOA thermal storage projects. It seems technically related to some but not most. I would have to say after listening to and review all of the other high temperature storage efforts that the need is for a program, not simply another option being evaluated throughout the usual phases.

Thermal energy storage is a key part of the CSP program.

This project supports the DOE CSP cost reduction goals both directly and by supporting other EERE contractors.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Good ("Generally effective but could be improved; contributes to overcoming some barriers"), as seen in the Accomplishments.

The approach involves emphasis on the TES performance model, full-plant models, and, very importantly, a process lab/test capability. The only additional aspect I'd suggest is that the model be capable of dealing with advanced concepts, such as nano-particles, and that it be capable of incorporating various means for coupling the heat exchangers. The means for transferring heat into and from the HTF is of course strongly dependent on the configuration. CFD results can, to some extent, be applied cost-effectively to a subset of the whatever configuration is used (heat pipes, thermosyphons, finned heat exchangers, etc.), but the overall conduction and convection of the melting and solidifying TES material may make it necessary to model the entire system. This could be a very challenging problem.

Developing performance models and experimental facilities for storage studies. It is sharply focused in the sense that it is investigating molten salt, phase change, and steam systems. Generally, though, the focus does not seem terribly sharp.

Certainly thermocline storage is an important potential near term option, and experience at the pilot scale will eliminate the barrier of no recent experience pilot scale experience. Specifically it will the project will support model development which in turn will support investigations of alternative sizing and configuration of thermal storage as grid penetration increases.

This is a mostly model based approach with some testing facilities under development.

The technical approach is well-designed, feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments are rated as Good, in that the main technical barriers are being addressed; to date, they have the two tank and thermocline models and cost analysis is on track Effort to develop the test capability is on schedule and it is particularly noteworthy that they have an

additional $880K for this. Overall, accomplishments and progress have been Good ("Significant progress toward objectives and overcoming one or more barriers").

They have been working their way through analysis of a variety of types of storage units using CFD analysis. So far, as I understand it, only thermocline with loose-fill packing has been completed. They have also been seeking and securing funding to set up three test loops for the various kinds of storage. They were successful with that and will be finalizing designs.

Apparently the work is progressing, but results in the form of graphs, predicted performance, and costs were not presented. This makes it difficult to judge progress.

Technical progress to date appears on track.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Good ("Some coordination exists; necessary coordination could be accomplished easily"). The project involves Sandia, DLR, Abengoa, and EPRI. The latter is particularly noteworthy. One technical paper is cited.

Interaction is mainly with high level organizations: Sandia, EPRI etc. May have more interaction with others next year. I guess because of being in a building mode, not much is noted in terms of presentations and publications are quite sparse.

Conceptually, all of the current FOA projects should collaborators and probably would benefit if their project managers could serve on an advisory committee for the NREL effort.

Collaborations with other contractors are outstanding, but focus on facilities development has evidently limited publications.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity in terms of milestones, deliverables, Gantt chart, etc. The presentation does not show a detailed plan for this future work. The degree of effort (personnel, labor hours, etc., for each task) is not shown. This project, at a minimum, should show the effort to the sub-task level. They do provide the major milestones for the tasks. But it is unclear if they plan to incorporate other TES concepts (heat pipes, conventional heat exchangers with fins, nano-particles, etc.). There are no decision points noted, or discussion of barriers. A program having the opportunity to develop a versatile, virtually generic test capability, especially with substantial additional funds, and also developing a suite of analysis tools could be better focused using a Gantt chart, with decision points and clearly showing where the effort results in overcoming barriers. Based on the accomplishments and the market potential, this area is judged as Good ("Plans build on past progress and generally address overcoming barriers").

The past accomplishments and capabilities of the personnel are excellent. Overall, this is a particularly notable project in that it offers an objective means for analyzing and testing TES concepts.

Seems quite straightforward. Basically build things and generate models. Doesn't appear to be too carefully controlled but will ultimately succeed.

Future work is effectively planned in a logical manner.

It is important to complete the inclusion the thermocline and phase change in the storage model they are working on.

Planning through FY11 appears good, but longer-term plans were not presented.

<u>Project Strengths:</u>
Good combination of analysis and test capability for TES and good collaboration.

Covering a wide range of topics.

New test facility able to support a thermal storage development program if one were to be funded in the wake of the FOA projects.

Well-qualified staff and world-class laboratory.

Project Weaknesses:

There is essentially no detailed plan shown for future work, although at a top level it is clear what the main thrusts are. There is a need to incorporate other TES concepts. The CFD analysis for some may prove to be daunting, if it must address the entire volume in order to determine free convection aspects, as with certain optional concepts (heat pipes, etc., as noted above.) Covering a wide range of storage topics.

Seems like there could be some significant overlap with the experiments being built at Sandia.

Lack of outreach to project developers, utilities, and esp. FOA projects.

I assume that project outputs will accelerate as facilities development is completed.

Recommendations for changes to the Project Scope:

A more detailed plan for future work, including a schedule (Gantt chart), with the degree of effort (personnel, labor hours, etc.) is needed. It is unclear if they plan to include other TES concepts (heat pipes, heat exchangers, nano-particles, etc.); these deserve consideration.

Not sure if it is a change, but some attention should be directed at how storage (say of any of the three approaches being considered) is optimally incorporated to a physical plant. Some approaches may be straight forward, some may not be.

Fund a technical advisory committee, esp. in relation to the model development work.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP030
Presentation Title: Thermal Energy Storage: Systems and Components
Investigator: Siegel, Nathan

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The Sandia TES systems and concepts program addresses essentially all of the primary DOE objectives, including the focus on thermal storage. The Multi-Year Program Plan objectives include: …. fully incorporate CSP efforts into the SAI, improve storage technologies, transfer R&D concepts…to the market place…and assist U.S. industry. The DOE focus is on technology development to lower costs, pursuit of thermal storage to enhance dispatchability, exploring advanced concepts, and reducing barriers to market penetration. Essentially all of these objectives are addressed. Overall, this criterion is judged to be Outstanding ("Project is critical to the EERE Solar Program and fully supports DOE RD&D objectives").

Developing a molten salt loop for evaluation of a variety of components for these kinds of systems--somewhat of a new version of what was done several years ago at Sandia. This could probably be quite a valuable contribution to the state of the art in system design in industry. The primary outcome will be defining appropriate equipment and method designs for these kinds of systems. The work on freeze/thaw situations could garner interest an application from a wider range of audience.

Project seems well aligned with next generation trough systems envisioned by the trough industry, cf. the Solar Millennium FOA project.

Thermal storage is critically important element of the program for the success of CSP.

This project aligns well with DOE CSP cost reduction goals and addresses critical barriers to using molten salt in both troughs and towers. It also provides valuable support to other EERE contractors.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"). The study of what component hardware works, how it responds to freeze/thaw, which components are of interest to industry, how to develop better hardware based on test results and experience of the personnel, and development of a versatile test capability make this a very important project with the expectation that very useful results will be obtained before a plant is deployed. This approach of testing components prior to installation in the plant has a critical leveraging effect and reduces real and perceived risk. There is substantial collaboration and they are developing the ability to support FOA awardees.

I don't anticipate that, with the prior experiences with molten salt at Sandia and Solar 2, this should offer too great of challenges. No real sharply focused technical barriers, but gaining better technical insights is of value. Perhaps the freeze/thaw work is cutting edge.

Failure to design a collector to protect the receive from the effects of freezing events is identified as a barrier. Otherwise not so sharply focused on technical barriers.

Clearly defined goals and approach.

The technical approach is well-designed, feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments are rated as Good ("Significant progress toward objectives and overcoming one or more barriers"), in that the main technical barriers are being addressed. To date, they have begun updating the facility, developed approaches including use of impedance heating, exploring tube bending (may be facility/set up/fixture dependent), beginning to explore "fill" state of the HCEs, have completed a design document, rightfully stress reconfigurability in the design (e.g., skid mounted hardware, etc.), placed a contract for facility design, etc. These results are notable for a relatively new effort (October, 2009).

Its ability to evaluate various molten salt components will be of value to the industry. Also, understanding freeze/thaw studies, if they are carried out in a meaningful way, could be valuable to the industry as well as have some theoretical value. It will be very important to couple the experiments with some simple finite element analysis to garner a better understanding of the importance of the various physical phenomena at play.

Project is at an early stage. Reasonable progress toward facility design and set-up.

They are making good progress - need to complete salt loop.

Progress to date has been on track.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Outstanding(“Close, appropriate coordination with other institutions; partners are full participants, publication or presentation”) The project involves NREL, Texas Tech, SQM, Abengoa, and Schott. Their roles are described at the main task level. Nine FOA awardees are cited for future collaboration. Two papers are cited. Collaborations are outstanding and there were 2 publications.

Working with industry partners as well as one university. That is probably about all that is needed. This is not the kind of project that I would expect a large number of publications. This is certainly true of the salt loop design and operation. Possibly the freeze/thaw studies will evoke a lot of interest with a more general group. But the tech transfer surely come from user group presentations.

Surprising that there is no apparent partnership or collaboration with Solar Millennium which seems to be taking the approach of testing their molten salt collector in a SEGS context. Will collector developers actually use the Sandia facility or will Sandia simply conduct tests using collectors that come with the facility.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
The presentation does not show a detailed plan for this future work. However, it does show the roles for the collaborators and it describes the main tasks. The basic layout is shown and there is no doubt this can be successfully designed and assembled. They will address other CSP systems in addition to troughs. A program having the opportunity to develop a versatile, virtually generic test capability to evaluate components and especially failure modes in a relatively short period of time (available 2011) would benefit from use of a Gantt chart, with milestones, decision points, etc. This may exist, but it is not shown. Based on the accomplishments, definition as to the major roles of the collaborators, and tasks, this area is judged as Good ("Plans build on past progress and generally address overcoming barriers").

Overall, this is a particularly notable project in that it offers a versatile means of testing components early on and thus reducing risks. One can think of this in terms of the "for want of a nail, a horse was lost….etc.", in that failures of some relatively small part of the system has a major impact on the overall success of a plant, and thus the approach taken in this project is critical to DOE's RD&D objectives.

Basically it is to continue the work started. Hopefully the test loop will be up and running, and hopefully the freeze/thaw work will be supported by some very careful efforts in simulation of the physics at play.

Freeze/thaw management research seems to be well laid out. Not sure what happens after that.

Need to track down mechanism of tube deflection during freezing.

Planning is good and includes the longer term beyond the present contract.

Project Strengths:
Excellent objectives and approach for reducing risks by considering molten salt components.

Looking at freeze recovery from trough receiver elements could yield very valuable information.

Additional molten salt experience

Well-qualified investigators and numerous collaborative inputs.

Project Weaknesses:
There is essentially no detailed plan shown for future work, although at a top level it is clear what the main thrusts are. This seems to be needed, especially given the relatively tight schedule for the test facility. There is a need to ensure that they can incorporate other TES concepts and materials, but this appears likely, although this was not noted in the presentation.

Seems like there might be some overlaps with a lot of the work going on at NREL. This needs to be carefully coordinated.

Incomplete connection to the industry and projects planned for use of salt as a heat transfer fluid.

Recommendations for changes to the Project Scope:
A more detailed plan for future work, including a schedule (Gantt chart), with the degree of effort (personnel, labor hours, etc.) is needed.

It is important to coordinate efforts as widely as possible in the salt coolant and storage field.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP031
Presentation Title: NREL Advanced Fluids Thermal Energy Storage
Investigator: Glatzmaier, Greg

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The NREL Advanced Fluids - TES program addresses essentially all of the primary DOE objectives, including the focus on thermal storage. The Multi-Year Program Plan objectives include: …. fully incorporate CSP efforts into the SAI, improve storage technologies, transfer R&D concepts…to the market place…and assist U.S. industry. The DOE focus is on technology development to lower costs, pursuit of thermal storage to enhance dispatchability, exploring advanced concepts, and reducing barriers to market penetration. Essentially all of these objectives are addressed. Increasing the temperature range, exploring new TES/HTF concepts, conducing molecular model analyses, and developing a materials lab are all excellent objectives. There is substantial collaboration, including six organizations, and with both Spain and France as collaborators. Part of their effort will support and evaluate FOA results. Overall, this criterion is judged to be Outstanding ("Project is critical to the EERE Solar Program and fully supports DOE RD&D objectives").

Main issue revolves around developing useful higher temperature fluids. These are for both heat transfer applications and thermal storage applications. A big concern that will be noted in almost all of the criteria slots is: there seems to be a lot of overlap between this and several of the other projects in the program.

Seems to be fundamental work complementing FOA projects.

In spite of the Project Title: Thermal Energy Storage project the work to be reviewed is apparently: NREL Advanced Fluids. Advanced fluids then applies to HTF's and storage fluids but not the larger program. These topics are relevant to the program since using liquid salt and other fluids is an important issue, especially for high temperature trough receivers.

This project fully supports the DOE CSP LCOE reduction goals both directly and via other EERE contractors. The prospect of discovering fundamentally new HTF candidates is exciting.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"). The combination of a materials lab to determine properties, molecular modeling, thermodynamic modeling, consideration of nano-particles, and extensive collaboration, including FOA support, make this an excellent approach. The only additional aspect I can recommend be considered is ensuring that the viscosity of the materials is determined, since this can profoundly affect pumping power, heat transfer rates, free convection in molten fluids, etc. All major properties of importance are noted except for viscosity.

Somewhat wide ranging thrust from developing new fluids (nano type, which is a real task in itself and that is being addressed in another project), measuring properties using new staff and new equipment, all the time, apparently, and developing instrument protocols.

Not much discussion of technical barriers.

The project is not well focused. Its overall objective are reasonable but there is a lack of defined goals below that. For instance, what is goal of the nanoparticle work? If it is to increase the heat capacity of the fluid by encapsulating nanoparticles, the effect on the heat capacity will only be significant if a large fraction of the fluid is made up of nanoparticles. That would almost surely be prohibitively expensive, and thus a poor goal. If it was to increase the thermal conductivity of the fluid, similar reasoning applies with a similar conclusion.

The technical approach is well-designed, feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments include addressing the range of temperatures and properties needed for improved TES/HTF, the first round of nanoparticle analysis and synthesis is complete, and plans are on track for the lab, analyses, etc. Overall, accomplishments and progress have been Good ("Significant progress toward objectives and overcoming one or more barriers").

Seems to be moving along in not too clearly defined pathways. Seems as though there is a quite a bit of overlap with other contracts that are in place.

Effort to date seems to have involved mostly getting organized.

There is very little evidence of work accomplished. The barriers identified were "High capital costs and Plant performance". There was very little progress addressing those barriers. A modeling activity was mentioned but no description of what was being modeled. I am not sure what "developed instrumentation protocols" means. How does the task "identified and modeled salt formulations with higher volumetric Cp than $NaNO_3/KNO_3$" fit with the extensive theoretical and experimental work underway elsewhere in the program (and reported on at the review)?

Progress to date appears on track.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Good ("Some coordination exists; necessary coordination could be accomplished easily"). The project involves about a half dozen organizations, including plans to involve researchers from Spain and France. Four technical papers are cited.

Indicates that some of the work supports NREL, SNL, universities and industry partners, but the details of this are virtually nonexistent. Four papers/reports are listed.

Reasonable number of partners considering early stage. In principle all trough system and component developers should be advisers.

Several Partners were listed but interactions were mentioned and no acknowledgement if contact with other work being performed relevant to their task.

Collaborations are outstanding and there were 4 publications.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity in terms of milestones, deliverables, Gantt chart, etc. The presentation does not show a detailed plan for this future work. The degree of effort (personnel, labor hours, etc., for each task) is not shown. This project, at a minimum, should show the effort to the sub-task level. They do provide the major milestones for the tasks. Based on the accomplishments and the market potential, this area is judged as Good ("Plans build on past progress and generally address overcoming barriers").

Overall, this is a particularly notable project in that it offers both advanced analytical and materials lab test capabilities needed throughout the entire CSP thermal storage system options. It is particularly notable that molecular modeling and nano-particles are included.

Didn't note any decision points. Appears to be basically a hunting expedition that goes where ever, hopefully in a useful thrust.

Future plans seem to re-plough some of the ground covered by the FOAs.

The suggested milestones for FY 11 only very generally address the barriers. There should be stronger motivation for pursuing the nanoparticle work. The "pathways to 0 – 500 C HTF" should be better defined and the work should be coordinated with other ongoing projects that have already made significant progress toward this goal.

Near-term and longer-term plans are outstanding.

Project Strengths:
Good combination of analysis and test capability for TES/HTF for troughs, dish, and central receiver thermal storage systems. Excellent collaboration.

Testing support for trough industry

Well-qualified investigator team, numerous collaborative inputs, world-class laboratory support.

Project Weaknesses:
There is essentially no detailed plan shown for future work, although at a top level it is clear what the main thrusts are.

Concern about overlap with Sandia (this might be considered collaboration) and other contractors in the program.

Eclectic mix of near and long term topics.

Recommendations for changes to the Project Scope:
A more detailed plan for future work, including a schedule (Gantt chart), with the degree of effort (personnel, labor hours, etc.) is needed.

Conduct project reviews with industry advisors.

The project goals and methodology should be better defined and the work should be coordinated with other ongoing projects.

Review: EERE 2010 Solar Program Review
Presentation Number: CSP032
Presentation Title: Advanced Heat Transfer Fluid Development
Investigator: Bradshaw, Bob

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the explicit goals and objectives of the EERE Solar Program Multi-Year RD&D plan. (Weight = 20%)

Comments:
The Sandia Advanced Heat Transfer Fluid Development program addresses essentially all of the primary DOE objectives, including the focus on thermal storage. The Multi-Year Program Plan objectives include: …. fully incorporate CSP efforts into the SAI, improve storage technologies, transfer R&D concepts…to the market place…and assist U.S. industry. The DOE focus is on technology development to lower costs, pursuit of thermal storage to enhance dispatchability, exploring advanced concepts, and reducing barriers to market penetration. Essentially all of these objectives are addressed. Increasing the temperature range, conceiving and evaluating new fluids, especially encapsulated/nano particles, etc., conducing molecular dynamics analyses, and conducting experiments on these are all excellent objectives. There is some collaboration. Part of their effort will support and evaluate FOA results. Decreasing the melting point is an important objective, and could be an enabling technology for more cost effective thermal energy storage and HTFs. Overall, this criterion is judged to be Outstanding ("Project is critical to the EERE Solar Program and fully supports DOE RD&D objectives").

Primarily involves work that is oriented to the development and characterization of various salts for storage media. Included are thrusts to bring down the freeze temperatures of storage materials and heat transfer materials. Using PCM encapsulations are also being considered. Some of the topics seem to overlap with some of the other projects' scopes?

I like the fact that the thrust mostly complement rather than duplicate the FOA efforts and also get into topics of interest to both troughs and towers. Effort seems mostly exploratory rather than industry supportive but that's ok.

It is important to develop higher temperature heat transfer fluids that can be used with troughs to improve the efficiency hence lowering the LCOE. We also need higher temperature HTF's for advanced tower receivers. This project supports those goals.

This project is critical to the longer-term EERE Solar Program as it supports both the current DOE RD&D CSP objectives and future breakthroughs in HTF science for troughs and towers.

Criterion 2. Approach to performing the R&D – the degree to which technical barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The Approach to Performing R&D is Outstanding ("Sharply focused on technical barriers; difficult to improve approach significantly"). The combination of experimental determination of properties, long term corrosion tests, molecular modeling, , consideration of nano-particles and encapsulated phase change materials make this an excellent approach.

Seems to be a general search of the frontiers of molten salt. This is about as sharply focused as the barriers get. Generally evaluating new devices for experiment determination of performance, and developing techniques for calculating various characteristics. Then the computations are compared to the measurements. Seems as though the most ground breaking thrust is the concept of encapsulation.

Work seems well focused on addressing the technical barriers by a combination of theory and experiment.

The technical approach is outstandingly well-designed, feasible, and commercially promising.

Criterion 3. Technical accomplishments and progress toward overall project and DOE SETP CSP program objectives – the degree to which research progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 35%)

Comments:
The accomplishments include addressing the range of temperatures and properties needed for improved TES/HTF, long-term corrosion tests, one patent filing/pending, one patent awarded, achieving lower melt temperatures using alkali nitrates/nitrites, achieving 700 C salts for central receivers, seven technical papers, encapsulation, etc., are commendable.
Overall, accomplishments and progress are rated Good ("Significant progress toward objectives and overcoming one or more barriers").

I find the progress is a little hard to assess. Several things are going on, and statements about accomplishments are given, but the latter are quite indefinitely defined

("Discover/characterization of low-melting nitrate-nitrite molten salts as HTF"). Apparently there is quite a bit of overlap of some thrusts with other contractors in the program.

Demonstration of an encapsulation method is an accomplishment, but it feels like there is a lot more work ahead to prove out the technology.

The effort has progressed on several important problems and has provided some solutions, notably, the discovery and characterization of low-melting nitrate-nitrite salts, work on defining and testing molten salts for high-temperature applications, computational models for salt properties and a encapsulated metal PCM approach

Significant progress toward objectives has been achieved in several project areas.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by paper presentations, patent applications and licenses. (Weight = 15%)

Comments:
This aspect of the project effort is Good ("Some coordination exists; necessary coordination could be accomplished easily"). The project involves NREL, SQM, Abengoa, and FOA awardee support. Extensive publications are noted, with one patent awarded and one pending.

Some collaborations are mentioned, but do not seem to be either to numerous or too intimate: "informal discussions with U. Wisconsin and U. Alabama." Several published paper or presentations are listed.

Technology transfer out is perhaps not as important as technology transfer in. Sandia has good exploratory targets but may not have all of the solutions at hand within Sandia.

Apparently they have good collaboration with others working in the field and have published several papers covering their work.

Collaborations are very good and there were 7 publications as well as 1 patent and 1 patent application filed.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This area lacks specificity in terms of milestones, deliverables, Gantt chart, etc. The presentation does not show a detailed plan for this future work. The degree of effort (personnel, labor hours, etc., for each task) is not shown. They do provide the major milestones for the tasks. Based on the accomplishments and the market potential, this area is judged as Good ("Plans build on past progress and generally address overcoming barriers").

Overall, this is a particularly notable project in that it combines sophisticated analyses with determination of the major properties of candidate materials. The goals are applicable for all CSP thermal storage system options. It is particularly notable that molecular modeling and nano-particle/encapsulation are being addressed.

Basically continue more of the same: "Evaluation of high temperature salts..."; "Computational modeling of molten salts"; "Evaluation of molten salt hydrates."

Future plans seem to be along the same tracks as current plans, which is appropriate given the early stage of development in the exploratory areas.

They propose to more or less continue their work that has been successful up to now.

Near-term and longer-term planning is outstanding.

<u>Project Strengths:</u>
Good combination of analysis and test capability for advanced fluids

Development of more strength in measurements and computations.

Exploratory work that can continue beyond the end of a project.

Well-qualified investigators, good collaborations, world-class laboratory support.

<u>Project Weaknesses:</u>
There is essentially no detailed plan shown for future work, although at a top level it is clear what the main thrusts are.

Seeming overlap with other a few other projects.

Probably are some. None obvious.

Recommendations for changes to the Project Scope:
A more detailed plan for future work, including a schedule (Gantt chart), with the degree of effort (personnel, labor hours, etc.) is needed.

Exam carefully the various tasks being performed and indicate any that seem to have close similarity to those being pursued in other projects.

Opportunity to work with universities. Some sort of outreach would be appropriate.

In order to ensure the anonymity of reviewer feedback, reviewer comments are listed in random order for each question

---

Review: EERE 2010 Solar Program Review
Presentation Number: SI001
Presentation Title: System Modeling - NREL
Investigator: Dobos, Aron

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
The software products NREL has developed are an important outreach tool that are widely used in North America and is very much in line with EERE objectives.

These models allow several levels of analysis, from DOE to other analysts and to users.

These modeling programs are important.

SAM focus on LCOE is very significant.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Outstanding effort in creating easily accessible software tools for the solar industry to use. I do see challenges with incorporating utility rates in the SAM because of diverse and complicated rate structures.

Models need more and planned user group feedback - beyond industry to utilities. This would allow for continued improvements towards user friendliness.
With volatile cost and market changes in the industry there needs to be a better or faster update mechanism.

All three models should have results - within 5% of each other. Since different resource databases are used this is often not the case.

These are important tools supporting activities that address barriers.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
It's important for the modeling to be updated and the product to evolve with the solar industry as it moves to larger utility scale systems that have unique challenges such as PV shading and CSP. Still can't forget about the little systems that continue to be installed with evolving products. It's important to keep the outreach fresh.

Same comment as criteria 3 - user groups are essential.
Web based or downloadable models can provide a lot of user information. Instead of total downloads, unique downloads and type of people would be important.
SAM still seems to be targeted as a DOE decision tool which was its original intent, but it has become an industry user tool and this needs to be well recognized by DOE moving forward.

Unique modeling tools. Especially valuable is the SAM focus on LCOE.
Could do a better job in presenting how the work performed last year was related to the barriers being addressed. Also could improve ways to measure progress against performance indicators.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Great collaboration with the other national labs. Continued outreach to educate the public and the utility industry on the tools is important.

These models have done an excellent job at collaboration, except with other model developers. I'm not sure if making a new rate structure database beyond CPR is the right way to go.

More publications would be good. More collaboration with industry would also be beneficial.

The project needs more interaction with Industry; after all, utilities are a key stakeholder in PV system integration. For example, the Solar Aggregator work is very important, utility participation must be sought aggressively if the tool is expected to impact utilities' perspectives on large scale integration to their grids.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Future research is well planned out.

Financial risk - bankability essential for the future
The aggregator will cause even more use by utilities and stresses the need for utility feedback mechanisms

Unclear what the detailed plan is for new features and capabilities. This should be more specific.

Go/no-go decision points not clearly presented. These are different from milestones or objectives.

Project Strengths:
Strong suite of software tools that are easily accessible to the public and industry. Because the industry is moving to larger utility scale project it's good to see a project plan to address that.

Public models, especially at the tri-level of PV-Watts, IMBY and SAM are essential for continued cost efficient market development in the US. These models have continued to maintain a degree of analytical rigor and generally kept up with the market trends

Provides an important set of tools for solar community.

The integration of finance and policy considerations in the software tools is very important work.

Project Weaknesses:
Differences in resource data and therefore predicted performance. Assumptions in SAM should be more transparent.

Validation of models with real world data would strengthen the model's acceptance.

Usability of tools can definitely be improved.

More collaboration with industry would be beneficial.

Project is not effectively integrating stakeholders, especially utilities. Presentation did not include validation work. Could also improve ways to measure progress.

Recommendations for changes to the Project Scope:

Based on actual experience with program results error, NREL may consider implementing a more rigorous testing process of the software tools accuracy prior to release. Having clear defined assumptions on underlying data that the software uses is very important for the industry.

SAM should be scoped beyond the DOE decision focus and collaborations beyond labs and solar industry to utility industry

Include model validation activities.
Add mechanisms for additional industry input.

Stakeholder engagement could occur in collaboration with Sandia for the overall benefit of the modeling effort

Review: EERE 2010 Solar Program Review
Presentation Number: SI002
Presentation Title: Systems Modeling
Investigator: Cameron, Christopher

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Very relevant to EERE program goal. Accurate and objective models backed by accurate data are the foundation for accelerating the adoption of large scale solar systems. DOE is considered to be an objective party in the solar industry. Keep it that way.

The project is using impressive statistical methods to predict the performance of panels and equipment, including lifetime and degradation. This work can feed into standards for equipment.

First, the presentation did not follow the format the others did, which would have made it easier to evaluate. That said, LCOE is obviously dependent on validated models, but validation was not as evident as analysis of differences. Accurate system modeling is critical for solar community.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
In addition to continuing the model developing and refining static once, the development of dynamic modeling has become very important as larger scale solar system are growing in importance.

Same comment as criteria one and looking at the slide 2, challenges, barriers, and problems, the content of the slide as well as the presentation only partly addresses the title of the slide

Focus on LCOE is excellent.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
This program has started focusing on larger solar systems installation that have unique challenges. This is certainly moving in the right direction.

It appears that a good deal of analysis has been done, but it is not evident that this has made it back into the models and the project has been ongoing since 2004. Lots of good progress in supporting SAM model.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good to see continued collaboration with NREL and industry on model validation.

It appears that they have had industry (BP Solar and First Solar) review the matlab process. The Sandia work notes that it answers user questions and enhancement requests for SAM, yet when this question was asked of the NREL SAM presentation, this was not mentioned.

Good partners.

Very good idea to organize a workshop. Perhaps it could include NREL's modeling work as a way to reach out to stakeholders in a more collaborative form (Sandia & NREL).

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This project is going in the right direction by focusing on larger solar installation that have unique challenges that need to be understood.

Future engagement of developers is too late in the process

Future plans could be more specific.

Go/no-go decision points not clearly presented. These are different from milestones or objectives.

Project Strengths:

A strength is the solid modeling development and industry relationship and the focus on large solar systems. When working with industry, it is important to continue be objective and display impartiality. Outreach to users and industry is good.

A scientific statistical approach toward developing performance models.
Brings Sandia expertise and experience to modeling activities.
Planned industry workshop will be important.

Good work on evaluation and validation of the models. Very good work on the SAM user support.

Project Weaknesses:

There is still a need for more large system electrical models as that's where the growth is. This may require additional funding.

Recommendations for changes to the Project Scope:

Continue with complete system evaluation. Would like to understand the system O&M for the life of solar projects by component such as inverter, arrays, combiner boxes, and etc.

Model validation is essential, but this validation should also be transparent to the users of the models in the form of sample data sets within the model

Some of the future plans seem quite ambitious (such as dynamic array model) and should be planned and budgeted in more detail.

Stakeholder engagement could occur in collaboration with NREL for the overall benefit of the modeling effort

Review: EERE 2010 Solar Program Review
Presentation Number: SI003
Presentation Title: NREL PV Grid Integration
Investigator: Kroposki, Benjamin

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Outstanding work this past year on PV integration. Excellent outreach activities to the utility industry. Continued outreach is important. This is in line with EERE objectives.

With utility market emerging as the main US market, grid integration barriers resolutions are essential.

Grid integration and high penetration PV activities are critical.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Great focus on PV integration to help reduce cost and market barriers. Standards work on distribution system is critical to this industry to continue to grow. Understanding HP PV and getting acceptance on voltage right through is critical to the utility industry.

This work has actually defined an approach towards the analysis. HI grid is not typical or even a weighted representation of most of the US grid.

Completely focused on important utility grid related issues.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Excellent collaboration with industry on large PV systems to help further the understanding of HP PV with real projects. Keep up the good work. It is great to see additional funding go this activity.

Possibly the evaluation should be made relative to the budget - then outstanding, but urgency is key

The PV variability working group is especially significant.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Continue collaboration with industry and utility scale projects to better understand HP grid integration.

No university participation. Each project should have 10% at least of university participation. That is how the future generation of engineers will be educated in renewables.

IEA collaboration is excellent. As well as targeted IEEE journal issue. This is well read by utilities.

Good partners and good publications (especially special issue of IEEE Power and Energy Magazine).

The projects need more interaction with Industry; after all, utilities are a key stakeholder in PV system integration. More dissemination to a wider audience is also needed.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Understanding system protection issues and making recommendations based on science, accurate models and field demonstration is critical for effective outreach to the industry.

Urgency of grid integration is key and getting new distribution grid protection and coordination design guidance is essential because even after it is developed the conservative utility acceptance will take time.

Continuing this work is important.

Project Strengths:
This group is well recognized to be the leaders in all aspects of grid PV integration. Continue the focus.

Good collaboration with Sandia who is focusing on Transmission and NREL on Distribution.

Achieved good agreement between feeder measurements and simulations, which gives confidence in the work.

The work is clearly laid out, addresses essential barriers towards DOE goals and most importantly is replicable by users such as utilities.

A focus on easing grid integration and reducing barriers for high concentration of solar is critically important.

SMUD and HECO projects are vital in determining the impact of high PV penetration on individual distribution feeders. The HECO project gives a “worst case scenario” being in an islanded system (geographically as well as electrically)

Project Weaknesses:
Not much of a weakness, but being an objective voice in how HP PV projects can be managed on the grid (with field demonstrations) is important to the solar industry.

If the project could be accelerated with more funding this is essential. Better platforms for information from IEA and collaborating countries might be helpful

Project is not effectively integrating stakeholders, especially utilities.

Recommendations for changes to the Project Scope:
Continue expanding the large PV grid integration knowledge and publish as quickly as possible the learnings from these activities.

More geographic diversity for utility collaborations is needed. HI is not the "Canary in the coal mine". Utilities understand a "weak grid" and failures there will not necessarily impede integration development on the more networked grid in the continental US. Also solutions in HI may not be transferable to the mainland grid

Consider expanding variability activities including additional workshops.

Put more emphasis on dissemination efforts. Perhaps this task can be coordinated with other NREL projects. Instead of multiple one-day workshops on a couple of topics, could held a few 2-day mini-conference with similar topics and audiences.

Review: EERE 2010 Solar Program Review
Presentation Number: SI004
Presentation Title: PV Grid Integration
Investigator: Ellis, Abraham

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Relevant to EERE goal of focusing on Transmission scale projects. This is an important topic that needs to be addressed.

The project has too many objectives.

Addressing the transmission issues is timely. While there is minimal transmission connected solar now, it is emerging and this is an operational arena that could be a show stopper for solar because of the US standards for reliability.

Solar codes and standards are a key factor in supporting the DOE goals.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Well planned out approach. Great to see a lot of industry collaboration. Keep up the good work.

The project is real for the utilities involved and Sandia has been an essential collaboration.

Focus on codes and standards, which in some cases are barriers at this time for larger solar deployment.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good to see there is a focus on Transmission integration. There is a need for dynamic modeling and understanding how solar variability interacts with the grid.

Too many objectives.

This work has gotten the attention of utilities and utility industry groups

Important contributions to US and international standards.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good improvement on collaboration activities.

One weakness is the need for more ISOs and transmission regions with the work. So far only CAISO is mentioned

Broad collaborations - excellent participants. Important publications.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
I think there needs to be an evaluation on how large solar systems interact with load. Is it real the concern with variable generation or does it behave like a variable load?

The future and potential work is timely and should not be delayed. Documentation of differences between US and international experiences could accelerate the interest which is essential.

Good to see that there was a review of priorities in 2010 to ensure alignment with most important issues.

Project Strengths:
Good interaction with NREL and collaboration with industry. Sandia focus on transmission and NREL on distribution is a good split.

Timely, integrates with councils (WECC and NERC) essential to acceptance.

Supports really important codes and standards activities.

Broad stakeholder participation.

Sandia's PV integration work includes both impacts at the distribution and transmission level, which sets it apart from NREL's work. The work on the statistical nature of variability must be continued. Kudos on the creation of the "renewable energy modeling taskforce" it is very important to keep an entity or mechanism to share best practices and avoid re-inventing the wheel. Good number of publications and presentations

Project Weaknesses:

Need additional focus and understanding (and publication)on the effect of large variable output DER on transmission interconnected projects. Is there a real concern and if so, what are the parameters?

Greater collaboration needed with RTOs and ISOs

Seems spread a little bit thinly in supporting many activities.

Reviewer is worried about the planned separation of the transmission and distribution work. This presents a potential weak link for the overall success of PV integration studies at Sandia.

Recommendations for changes to the Project Scope:

Well planned approach as defined.

Finish the first year, but then trim out 1/2 of the objectives. Else, none of the objectives will be satisfactorily addressed.

While storage is the panacea for the grid, energy management on a large scale is being considered as the less costly grid operational measures by many ISOs.
Stronger coordination with ISOs is needed, which may take increased funding.

Keep evaluating and updating priorities as project proceeds.

Project personnel should ensure that close coordination and communication is kept between the distribution and transmission work once these are separated during next year.

Review: EERE 2010 Solar Program Review
Presentation Number: SI005
Presentation Title: CSP Grid Integration
Investigator: Parsons, Brian

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
This project is critical to EERE as the CSP industry is rapidly expanding. This is a new program that will be evaluated on what is proposed.

Hardly any technical information given. Seemed to focus only on tariffs.

CSP and storage dispatch coupled with transmission rates and policy is important for emerging solar market

Integration of CSP into the utility grid is an important topic.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Off to good start. Good to see a combination of dispatch and policy recommendation. It is important to have solid data to back up recommendations and thus collaboration is important.

integration with gridview very important. Evaluator is not knowledgeable of other models, but consideration of the most used models and how to integrate analysis results should be considered.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good start with a report on Optimal CSP and Storage Dispatch.

It seems to be just getting started.

Has just started. Good publication resulted already.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Early outreach is on the right track and needs to continue.

Inter-lab collaboration is not evident. PV variability seemed to be covered by Sandia - Xmsn and NREL distribution. Is this a repeat? More collaboration with ISOs and RTOs needed

Small list of partners.

Scarce collaborators and dissemination efforts. It might be o.k. to have few direct collaborators, but it is very important to engage other stakeholders at least as an outreach activity

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This is an important topic. Need to make sure that there is financial analysis intertwined in this project set.

Future plans seem to be a "continuation" with a tripled budget.

Plan for PV variability database and WECC solar data are important future topics.

Project Strengths:
Policy, dispatch, forecasting and CSP system optimization are important topics.

Transmission policy and tariffs will be essential.

Facilitates CSP projects.

The storage dispatch and the policy activities are essential in the future integration of CSP to the power grid.

Project Weaknesses:
LCOE is not identified as a driver.

Collaboration with operators is not as evident as collaboration with WECC. While WECC is important so are the RTO's and ISO's

Future plan/budget details should be generated.

It was not clear, not even after the answer to the panel's questions, the use of the new budget, which increased from $335K to $1M. There seems to be other activities other than CSP also included in that $1M figure. Need to provide clear information during reporting about the project being presented.

Recommendations for changes to the Project Scope:
This is an important topic. Need to make sure that there is financial analysis intertwined in this project set.

The presentation had little to do with the topic. The project needs focus.

More Wind and solar combined on integration. Is it possible for DOE departments like solar and wind to share this resource of work?

Clarify budget for 2011 (presentation was not clear on this).

Project should improve dissemination efforts for next year.

Review: EERE 2010 Solar Program Review
Presentation Number: SI006
Presentation Title: Solar America Board for Codes and Standards
Investigator: Sherwood, Larry

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Codes and standards are a very important element of continued growth of the solar industry, especially on the small residential/commercial projects. This needs continued focus.

It became apparent to me in Washington D.C. that standard-making is high priority for PV.

Codes and standards are essential to meet DOEs goals. However, building structural codes are still not addressed.

System modeling and validation is an important aspect supporting the DOE objectives.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Good focus on barriers to solar adoption. May want to perform an annual gap analysis to catch potential barriers early.

The approach of collaboration and organization is well done. It seems that users have been left out. Additionally, though this web site central repository is essential and a major contributor, I wonder if some user market research might make the information more accessible and therefore used more.

Focus on modeling and validation is excellent.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
On the technology front, a strong focus on arc fault detection and recommendations needs to be done.

Unable for me to judge. The question that DOE needs to ask is "given the $6M in funding, how much are they doing to help in PV standards?" For example, the ABC web site - good, but is it used or ignored?

The work out of this group has long been needed. The "Potential Impacts of AMI on RE Policy" seems out of context.

Really good list of accomplishments.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
One element that I think is missing is outreach to the utility industry. The utility industry needs to be educated on PV codes and standards when they interact with their customers. The utilities for the most part live in a different standards environment. There appears to be good integration with NREL & Sandia.

An impressive list, but again are they influencing standards?

It seems that the steering committee is mainly made up of recipients of the funding. The advisory committee is broader, but it is not evident how much input they have.

Broad collaborations with excellent participants. Many publications

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Should consider annual gap analysis to see if quick course correction is needed. Need stronger focus on arc fault detection communication.

Policy research plans seem out of context. For instance:
- Rate impact of net metering
- potential advanced metering infrastructure
- Billing and Payment policies for Solar DG

Solid plan going forward.

Planned International Conference in integration of RE and DER will be important for communications about these topics.
Planned publication of high resolution data will be valuable for many stakeholders.

Project Strengths:
Strong advisory committee that could include additional resource tied to the utility industry. Could be IOU, Munies or Cops. Interaction with national SDOs is important.

Obviously an important topic. But I cannot evaluate their effectiveness.

Coordination of codes and standards has long been needed

Supports advances in important system modeling and validation topics.

Broad stakeholder participation.

The ABC work is extremely important, as it brings together PV stakeholders to reach a consensus on best practices and standardization of the industry and policy efforts.

Project Weaknesses:
Could have more utility industry outreach.

It seemed strange that the lead organization, NMSU, did not present the work.

The information and web site seems targeted towards users who already know what to look for and where to look.

For next year it is important to make sure actions taken are enough to avoid the delays mentioned on slide 24

Recommendations for changes to the Project Scope:
Should consider annual gap analysis to see if quick course correction is needed.
Could use more education and outreach projects that would include web based education and education of the utility industry in residential and commercial codes and standards.
Add a task to provide rigorous testing and evaluation standards for PV equipment (panels, inverters, etc.), including accelerating lifetime and mean-time-between-failures. One-star, two-star, three-star, four-star quality. 3rd party testing to make sure foreign "junk" doesn't get in that claims to have passed the standards but actually does not. Encourage Consumer Reports to develop articles that rate PV equipment.

There needs to be a matrix of codes, standards and safety listing relative to system, technology, market segment, and/or individual part of solar system. It is understood that these codes and standards are sold by the organizing bodies, but annotations on the content in the standards etc. is allowed. Additionally users need to understand what is required and what is the best practice.

:

Review: EERE 2010 Solar Program Review
Presentation Number: SI007
Presentation Title: NREL Codes & Standards Lab Support
Investigator: Basso, Tom

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
The NREL codes and standards support is critical to the solar industry. They are continuing to drive the DER interconnection standards that are critical part of the NIST smart grid initiatives.

Important to keep both NREL and Sandia working in the standards arena.

Very relevant to DOE program.

Support for interconnection codes and standards is critical.

IEEE 1547 updates are especially important.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Continue driving technical and non-technical DER integration. NREL is in a very good leadership position within the technical community such as IEEE and is considered to be an objective leader.

NREL lead, with DOE support, this initiation and accelerated development of IEEE1547. It is evident that there are now gaps which can be barriers. This leadership and accelerated approach should be reestablished and include EPRI's efforts at acceleration for inverter communication standards.

This project addresses the key barrier of the need for updated interconnection codes and standards.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Outstanding progress in the interconnection standards world by leading the P2030 activities and establishing P1547.8. This group is helping to drive as fast as possible the standards needed for HP solar.

More workshops such as the May 2010 are needed, but with a format that results in action items and timelines and interested party lists

Impressive list of accomplishments.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Great collaboration work with the technical industry. This group is considered the objective leader in all issues relating to interconnecting distributed energy resources to the electric grid.

Collaboration with the grid integration analysis could be stronger with crosscutting results.

Important list of partners. Especially important to work with IEC.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Important to see that a report on dynamically controlling inverters is on the slate. Industry is in need of this type of document. HP PV is a very important topic this group needs to focus on. Continue with outreach and workshops.

what is missing is the strong plan for continued gap analysis, workshops, and focus on accelerated standards development to prevent barriers caused by standards gaps

Future interconnection standards activity will continue to be vital.

IEEE 1547 updates (especially 1547.8) are really important and needs to happen quickly.
Go/no-go decision points not clearly presented. These are different from milestones or objectives

Project Strengths:
Nationally recognized objective team leading the standard activities of interconnecting DER to the grid. Ties into IEEE and NIST are important. Education of stakeholders is another strength.

NREL was a leader in the development of 1547 and has the intellectual mass and historical knowledge to continue this work, which now has gaps in applicability to HPPV

Great use of a relatively small amount of money!

Focuses on key challenges and barriers of interconnection codes and standards.

There should be more activities like the Denver workshop. Utility participation in that event was outstanding.

Project Weaknesses:
Does this group have enough resources to accelerate the standards development they are being asked to do?

1547 now has gaps in applicability to HPPV; possibly an increased budget could help towards supporting an accelerated update that is more applicable to the emerging market

Low funding.

Recommendations for changes to the Project Scope:
Acceleration to resolve standards gaps

Consider increasing budget to accelerate interconnection codes and standards development.
IEEE 1547.8 support should be prioritized.

Need to find out what organizers did to get 48 utilities to participate, need to let other workshop organizers know and keep looking for ways of integrating more utilities into solar grid integration efforts.

Review: EERE 2010 Solar Program Review
Presentation Number: SI008_
Presentation Title: Solar Codes and Standards Support
Investigator: Bower, Ward

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Support for codes and standards is in line with EERE objective.

This is a modest budget project but a very important subject. Includes a knowledgeable researcher with long-time experience on PV. Pointed out that DC arcs may be a fire hazard.

Critical to DOE goals and objectives

Provides key guidance to standards bodies including domestic, international, and National Electric Code (NEC) that can greatly impact the growth of solar. This is an ideal role for DOE to play.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Good to see this group being involved in national codes and standards activities. Arc fault detection standard should be founded on science where this group can provide considerable expertise.

Approach is mainly coordination and intellectual contributions to codes and standards development. More outreach could strengthen approach, such as workshops and forums outside of the standards group.

Very focused on the barriers that presently exist with codes and standards.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Continue creating models that represent real world arc faults to provide basis for standard development. Not sure if arc fault detection process was tackled early enough to influence codes and standards??

Where are the accomplishments accessible to user groups beyond the labs and consultants?

Good accomplishments across an array of PV related codes and standards.

New 2011 proposed changes in the NEC could harm PV industry and it is important that feedback was given to the decision makers there.

Special concern is the arc fault detection requirement - this could broadly impact PV implementation costs and complexity (and confusion in the market) if adopted.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration within labs and industry groups.

More user collaboration and reporting out is needed

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Good to see additional funding for FY10. Future research not well defined in the slides. This group is performing good research on arc fault and communication to SDOs.

Not clearly reported other than ongoing.

Continuing input to codes and standards will become even more critical as solar grows, especially NEC.

Little information about future work.

Project Strengths:

Active participation in codes and supplying scientific data is important to the solar industry.

Expertise in the subject and standards are critical to PV success.

This work is essential, especially the NEC work. However, more organized reporting is needed.

Critical contributions with a small budget.

Detailed involvement with important standards, especially NEC.

Safety is fundamental. As the presenter correctly pointed out, "as more aging PV systems are out there, fires might be more common"

Project Weaknesses:

not clear what the future path for this program is?

More organized outreach is needed.

Small budget.

There seems to be other activities other than Codes and Standard included in the $350K budget. Need to provide clear information during reporting about the project being presented.

Recommendations for changes to the Project Scope:

Focus on arc fault detection techniques and sensor/product testing.

Outreach and garnering input from users would strengthen the position in codes and standards development

Apply additional resources to support NEC changes that will facilitate and not be a barrier to solar adoption.

Arc fault work should be expanded.

Extremely important to balance the urgency of PV and other solar technology deployment with safety considerations. However, particular interests or unreasonable equipment requests should be weeded out of the process. Should maintain transparency and inclusive participation of PV stakeholders to minimize safety issues and maximize rational decision-making based on facts at hand.

Review: EERE 2010 Solar Program Review
Presentation Number: SI009
Presentation Title: Solar Radiometry and Modeling
Investigator: Myers, Daryl

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Base data is critical for an effective solar program. As solar projects have increased in size, they require major funding that requires traceable data for bank-ability this program provides.

consider expanding to more sites make data available

DOE needs to maintain a strong activity in solar radiation measurements. Clearly this project builds upon experience and expertise.

Accurate data and understanding uncertainty is now essential for that new buzz word, but ultimate market decider of "bankability". DOE diminished this work in the past and not the three year recent high resolution data is privately held, which adds to cost and becomes a market barrier.

Accurate resource measurement equipment is critical.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Good approach to traceable solar data. It would be good to see additional stations from different part of the country to be included in the data set.

Especially tabulating instrument metrics without making specific recommendations. Public web site maintenance have in the past been used extensively, and even though the web site data may be maintained, is the web communications approach maintained?

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Good work on radiometer calibration and performance evaluation of solar irradiance instruments. It would have been good to see more sites around the country participate in irradiance measurements.

These are large tasks and grinding work.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration with industry. Participation in the CSP Best practices handbook is important contribution. Not sure if there is collaboration with Sandia?

No university involvement?

The right collaborations are established and maintained

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
A new hire, even a shared one, is good to ensure growth of the requirement for bankable solar data. I would like to see solar data from more regions in the country.

Most of these products are legacy products and must be maintained

Project Strengths:
Solid science providing traceable solar data to the industry that requires bankable data for large solar projects.

An activity that must be continued.

Again these are legacy products that are necessary for continued market development and uncertainty calculations for investors.

This was the best presentation I reviewed. An excellent presentation format, easy to follow and evaluate since it included all points evaluators needed to look at. Everything was there. Should be used as an example or template for presentation for the next Peer Review.

Project Weaknesses:

Need more solar metrology stations around the country.

Updating user friendliness of public data.

Recommendations for changes to the Project Scope:

Figure out a way to increase the number of site around the country that can provide bankable data for funding large solar projects.

Consider combining with resource characterization activity.

Review: EERE 2010 Solar Program Review
Presentation Number: SI010
Presentation Title: Solar Resource Characterization
Investigator: Renne, Dave

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Base solar data has become critical for financing of large solar projects. With the future trend to larger projects, the quality of solar data (bankable) is a critical component that NREL can objectively provide. Accurate forecasting methodologies are also an important aspect of this project that is required by electrical system operators.

The solar radiation data base is necessary for evaluating PV sites. The activity must be continued.

Resource is the foundational support for investment certainty. The forecasting will become even more important as smart grid functions expand and solar technologies are required to integrate with other intermittent loads and generators.

Solar resource information is critical to the success of solar.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Well defined program. The satellite modeling shows great promise and may turn out to be an answer to short term forecasting without large proliferation of metrology stations.

The approach seems sound and well balanced between ground source measured data and continued analysis of spatial temporal predictions.

Has broad impact in reducing many barriers.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Excellent progress toward using satellite data for solar irradiance forecasting. Beginning to collaborate with utilities for data collection that will in turn help with forecasting is a good step.

4 KM spatial modeling and the 0-6 hour/1-3 day forecasting show excellent results and progress.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Great path toward collaboration with industry to obtain additional solar data to help develop better solar irradiance forecasting methodologies for large scale solar systems. It may be time to do outreach to the utility industry on the programs capabilities.

It seems that the technical collaborations are all in place, but user collaborations are weak.

Could collaborate more with end users of the data.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
The proposed research fits into EERE goal such as the CSP Handbook due for publication in 2010. Also proposed plan to hold workshops is an important outreach program to educate and gain feedback from industry groups.

In addition to the comment on Criteria 4 it looks like an industry workshop is not planned until 2011. I would also highly recommend a workaround to put the satellite data sets back in the public domain by either purchasing back the rights from Clean Power Research or developing new capabilities as noted in the presentation

Project Strengths:

Evaluating satellite data for short and long term solar forecasting is a strength. Also being able to adjust R&D to develop insolation data set for modeling of large solar resource systems that is geared to utilities is a strength.

The resource work has continued to expand to include forecasting and uncertainty analysis essential to the developing market.

Resource data is critical for all aspects of solar.

An excellent presentation format, easy to follow and evaluate since it included all points evaluators needed to look at. Good list of publications. Should be used as an example or template for presentation for the next Peer Review.

Project Weaknesses:

No particular weakness is evident in presentation.

User group feedback is weak. High resolution recent data - even real time should be possible.

Recommendations for changes to the Project Scope:

Consider holding a solar resource workshop in 2010 to obtain industry feedback on direction for 2011.

Consider ways to incorporate the large number of agricultural GH sensors into the data base, and determine the errors involved with using GH-only for PV harvest predictions.

Ground truth (SOLRMAP) could be expanded, now while funding is available.

Look at doing a workshop with end users of this data.

Review: EERE 2010 Solar Program Review
Presentation Number: SI011
Presentation Title: Southwest Region Experiment Station
Investigator: Rosenthal, Andrew

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Provides support to EERE solar program. The 7 activities are in line with goals of the SETP program objectives.

Long-time researchers in PV who understand the issues.

The SWRES has an excellent array of work all directly related towards SETP goals.

This project is a combination of a number of relatively small tasks that all fit very well with the goals of the EERE solar program.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
The approach identified in the 7 activities for 2010 work is well defined. Data collection on the large solar projects is an important activity.

The activities are well defined and laid out for understanding and good technical outreach. However a list of publications was not included.

All tasks address barriers. Especially important are the transient cloudiness measurements, inverter temperature study, and long term inverter test facility operation.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The support to be provided by SWRES for 2010 will assist other programs such as much needed performance data of thin film PV and irradiance transients on large utility scale systems. Little

accomplishment defined prior to 2010.

The information presented indicate excellent progress towards system reliability knowledge, accurate date monitoring, performance model development - and much more.

These tasks will help overcome the identified barriers.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)
Score:

Comments:
SWRES collaborates primarily with SANDIA and SERES. On some of the activities will also collaborate with industry on monitoring projects.

Unclear how this university affiliates is working with NMSU students. Some significant involvement should be required.

Collaborations are great. It is not evident how the standard field test protocol will be disseminated, but as larger systems emerge in the US market this work will be incredibly useful. The fact that they are collaborating with AZ and FL utilities with large systems is indicative of good collaboration.

Important to publish results.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
The 7 activities are well defined for 2010 monitoring work.

Future research was not specifically addressed, but the projects underway certainly warrant continuation or expansion

Continue to leverage from extensive experience base.

Project Strengths:
Instrumentation and data monitoring.

The RES's have a legacy as the DOE's in the field problem solvers. The high risk component analysis has long been needed.

Many small tasks that are individually valuable combine to a solid program.

Leverages from SWRES experience and location.

Impressive contributions to solar at a low cost!

This project has just started in April 2010 due to delays in contract signing. Although difficult to evaluate since the project is just beginning, the project is relevant to DOE goals and objectives, and the approach presented is feasible.

Project Weaknesses:
Possibly the current market and industry is unaware of the engineering capabilities and should be. In fact I notice a similar comment from last year.

Recommendations for changes to the Project Scope:
No changes. They are working toward DOE goals.

Review: EERE 2010 Solar Program Review
Presentation Number: SI012_
Presentation Title: Test and Evaluation Activities
Investigator: Reedy, Bob

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Provides support to EERE solar program.

The proposed work on measurements and reliability can be useful, but there was little in the document or presentation that the organization knows how to analyze or interpret the data. Perhaps NREL is supposed to do that for them?

Reliability and performance hands-on engineering is essential

Leverages FSEC experience and location on a number of smaller scale projects that fit well with the program objectives.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Supports Sandia and NREL with data collection that supports program activities.
Appears to be a repeat of work on-going in many places.

Module testing and certification are very necessary. The long term exposure seems to be a difficult approach due to changes in manufacturing.

Good small projects.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
This rating is based on the cooperative relationship between the RES's. It might be helpful to have a combined presentation or at least a consistently formatted presentation

Not a lot of detail of accomplishments was presented.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration with FSCE and industry partners. Also an important relationship with FPL on the utility scale PV system.

Even though this is a university project, there appears to be little or no student or faculty involvement. Rather, it supports full-time researchers. A university project must have student involvement; else it simply competes with faculty projects.

There is industry collaboration and FSEC tries to make their facilities open to utility meetings such that the work is evident.

Could involve more outside stakeholders (beyond labs).

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Project year 2010 well defined.

There was no plan given, except to collect data.

The RES's are essential and should continue the coordinated field and reliability work

Most of work described is in future and should be planned in detail.

<u>Project Strengths:</u>
Data acquisition for Sandia and NREL. The relationship with FPL on the utility scale system. High voltage testing of modules is important start to the path of incorporating > 600 V in NPFA.

Ability to do field engineering.

Leverages FSEC location and experience.

Module testing is important service.

Project Weaknesses:
Not likely to produce anything of use.

A northern climate RES would possibly balance hot/dry and hot/humid locations.

Seems to need more detailed planning (details did not come out during presentation).

This project just started in May 2010, which makes it difficult to evaluate since the project is just beginning. However, the presentation did not properly explain alignment of the project to DOE's goals and objectives, nor relevance beyond what other projects are already doing. Plans for next year seem vague, without details to predict potential impacts on EERE's mission and objectives.

Recommendations for changes to the Project Scope:
System certification - inverter certification is still a strong industry need especially if the big box stores get into the business.

Ensure good communications of activities and results.

Project personnel must work hard to demonstrate the value added in investing taxpayers' money on this project and to show the unique contributions of this project to the SETP. Neither the presentation nor the presenter delivered confidence to the reviewer on the capabilities of the team especially phrases such as "we were barely hanging on" (before the project was awarded). The honesty is appreciated, but this year should not be just to catch up, real progress must be shown in next year's review.

U.S. DEPARTMENT OF ENERGY | Energy Efficiency & Renewable Energy

Review: EERE 2010 Solar Program Review
Presentation Number: SI013
Presentation Title: Systems Analysis
Investigator: Margolis, Robert

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
The system analysis is a very strategic part of the EERE solar program in that it provides an analytical basis for the future direction of the solar program. This program provides guidance to the industry while corporations, regulators and policy makers develop their solar strategies.

An expensive project that does a job that the free market will do.

This body of work combines the technical aspects of SETP with policy and market drivers to fully understand how the goals can be reached

Very important and helpful analysis and communications activities for EERE.
Solar Vision document should be a substantial resource.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Engaging industry is an important component in developing strategy as was done in development of the Solar Vision Study to be published in 2010. The PV penetration, cost and financial models are important tools for the solar industry.

The approach to setting priorities was not evident and I wonder if industry/user input might focus the work. That said this project seems to accomplish all that is needed.

Reports help with some important non-technical (such as legislative and financial) barriers.

Analysis and modeling of costs is important for LCOE optimization.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The Solar Vision Study will be an important addition to the EERE Solar Program. There are many accomplishments that have been completed such as the PV Financing Analysis.
The accomplishments speak for themselves.

Good reports published.

RE Project Finance website/blog is very interesting and could be a good model for DOE communications going forward.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration with industry, regulators and policy makers. Transfer of information via the web is an important method of communication. Outreach to industry by holding workshops would be good to have as part of this program.

The collaborative approach towards accomplishing the Solar Vision Study was stellar.

Could involve more industry partners.

Good publications.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Work beyond 2010 is not well defined. With the large funding increase in 2010 I would have expected to see more detail on what would be proposed in 2011 and beyond.

As noted in an MT session, getting interval analysis on success of policy is needed.

Further details could be provided.

Project Strengths:
Great analytical analysis is being performed by this project set. This is important work that is helping to shape future direction of the Solar Energy Technology Program.

The team seems to have the right background and produce more than the "sum of the parts"

Very impressive work. Comprehensive and very relevant. Very good list of publications for one year.

Project Weaknesses:
Future work is not well defined. Not sure how much outreach this project set performs?

Not technical, more of a market study

Even though the Solar vision Study was fully collaborative, the program as a whole could utilize collaborative approaches to set priorities.

Recommendations for changes to the Project Scope:
The researchers are well qualified. But put this effort into developing equipment ratings for the taxpayer.

Possibly a more formal collaboration towards setting priorities. It seems the intellectual strengths could also continue "tiger Team work for a both utilities and SACTO.

Look at opportunities for additional workshops.

Continue improving the integration of financial tools that better reflect the social and environmental benefits, as well as negative impact if any, of solar systems.

Review: EERE 2010 Solar Program Review
Presentation Number: SI014
Presentation Title: Reliability R&D - NREL
Investigator: Kurtz, Sarah

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
NREL is performing a highly valuable service to the solar industry by providing analysis and results of PV module reliability. This is important as larger PV systems are being built that require extensive financing. I am looking forward to seeing more degradation data on thin film solar.

Solid technical work. Impressive study of delta-temperature damage mechanism.

Understanding long term performance and reliability will eventually get to market. Identifying accelerated or testing that simulates field exposure shortens the product improvement time.

Good focus on LCOE and reducing market barriers to high penetration levels.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Excellent approach by the researchers and a good path toward dissemination of data to the solar industry. Could grow the outreach of this program with more workshops.

It was good to understand both the strategic and technical approach. Additionally, utilizing both results from systems in the field and lab induced stress is needed for validation.

Tasks being done under this project are important - especially work on characterizing degradation rates and improving reliability testing.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Great results on module degradation analysis. This program is helping SETP reach its goal by providing data to the industry they can use to make project decision and obtain financing.

The graphics both in the review presentation as well as the supplemental slides are indicative of the results and accomplishments. Please encourage the supplemental slides including additional results for future reviews. The information on post 2000 degradation rates is an incredible contribution.

Accomplishments are impressive.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Outstanding work by the researchers in reaching out to collaborators. Outreach to industry through workshops and presentations is important to disseminate reliability information on PV module degradation.

Industry collaboration seems very good the concept of everyone bringing something to the table keeping the forum open so that everyone gains a great deal is an ideal approach to collaboration. User outreach workshops should not cost $40K as noted in the budget enhancement slide and should happen sooner rather than later.

Broad list of partners.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Looks like the program proposed future plan is in line with accomplishments. Good to see that there will be an increase in workshops.

Possibly more timely testing of new products is necessary as the market growth accelerates.

Important ongoing work.

Project Strengths:
Analysis and presentation of reliability data. Team shows leadership in the industry.

Incredible technical and collaboration with industry and the technical community.

Work includes addressing key industry and market concerns.

This project's leadership is essential in sharing information that supports companies' reliability efforts. Very good emphasis on "big picture" for field PV performance. Excellent interaction with companies last year (around 100 different contacts) ensures practical relevance of the work.

Project Weaknesses:
I assume reliability analysis is flowing to SNL who I believe is performing system reliability analysis.

Investor and user awareness and outreach

Recommendations for changes to the Project Scope:
Continue with outreach and collaboration with industry.

Don't be afraid to put all this good work into some type of DOE suggested performance standard for the US taxpayer. Why not rate equipment, also? Take the lead on this. Taxpayers would look favorably on this.

Continue to review tasks to ensure that highest priority items are being addressed.

Should continue efforts to coordinate with other projects that are collecting solar data to ensure unnecessary repetition (beyond just following standards for data collection).

U.S. DEPARTMENT OF ENERGY | Energy Efficiency & Renewable Energy

Review: EERE 2010 Solar Program Review
Presentation Number: SI015
Presentation Title: Reliability
Investigator: Granata, Jennifer

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
This program is vital for the EERE solar program. System reliability is a very important aspect of creating financially viable large scale solar projects. I would like to see more refined O&M cost analysis.

Great technical work, necessary to insure quality equipment on the market.

Very relevant and seems complimentary to RES's and NREL work

System reliability is key to achieving reduced LCOE and has historically been a barrier - this program is very focused on this. Leverages extensive Sandia capabilities and experience in this area.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Most aspects of system reliability are being addressed by this project team. Course corrections on more outreach accomplished since last review.

Moving predictability into models and then validating is always accepted in technology development.

Topics are solid and cover both technical and non-technical barriers.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Very good and important work on system reliability such as the predictive PV system reliability model. Continue with the work you are performing in this space. Inverter reliability is also important as that appears to have the highest failure rate. Northern climate with a lot of fluctuation in temperature and moisture is something this program could tackle.

PVRAM results to compared to actual an excellent example.

System integrator workshop is especially valuable.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
This project has developed good collaboration with industry. Could focus on developing additional relationships with the utility industry that is playing a larger role in the solar industry.

Why no university involvement?

Integrator collaboration workshop looked to have good results.

Excellent list of partners and participants.

Lots of publications - good.

Workshop approach is good.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Great slide on future work. Looking forward to key results from this project in the future.

For the inverter work, it was not evident how this would get back to and improve industry.

All items focused on addressing important barriers.

Project Strengths:

Good solid analysis on accelerated testing, FMEA and predictive modeling. Seen as an unbiased source of reliability information. Good to see reliability data collection website.

strong technical and collaborative approach.

Good focus. Builds on long history and experience.

Excellent focus on integrator side, and on "growing an integrator's community". Encouraging that many utilities are acting as integrators. Very good milestone table on slide 8.

Project Weaknesses:

Could use analysis on CPV in this project.

Not enough industry and user input or awareness.

Recommendations for changes to the Project Scope:

Consider adding inverter reliability analysis for northern climate with wide temperature and humidity variations.

Use all this good work to suggest DOE-approved testing methods, and then test equipment on the market. This would be a great service to the US taxpayer.

More user collaboration and publications in utility journals might initiate this.

Look at adding more workshops as this seems to be an excellent mechanism for information exchange.

Should continue integrator's workshops, placing strong emphasis in participation from utilities.

Review: EERE 2010 Solar Program Review
Presentation Number: SI016
Presentation Title: NREL PV Test and Evaluation
Investigator: Marion, Bill

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
T&E is a critical foundation for everything upstream so accurate models and reliability analysis can be performed.

An example of high-quality technical work that should be continued.

T and E very relevant

Test data and capabilities by an independent laboratory such as NREL is critical in moving the solar industry forward.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Appears to be a well-managed and creative T&E project for EERE solar program.

The approach seems to be cost efficient and accepted by industry.

All tasks described support overcoming important barriers.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
I see that this program is evolving into testing CPV and new devices coming to market. This is in line with EERE solar program goal. Good direction.

The flexibility to test new market devices such as the Solar Magic device is incredible.

The topics that were presented (CPV testing, CdTe and CIGS Module stabilization, shade mitigation device testing, and flat plate module spectral correction) are important.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Very good collaboration with industry. Good to see the program going after data from diverse region of the country.

It seems the program is highly collaborative

Lots of good partners.

Good publication list.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Great future research that includes more diversity of sites from other regions of the country. Data is needed from regions outside of the west and southwest.

Future plans right on target

Prioritization of tasks taken on is critical due to limited resources.

Project Strengths:
This is a solid test team with well-defined T&E process to support other programs. Methodical approach to manage workload. Good to see that there will be data collection from meteorologically diverse regions.

Engineering and technical expertise. It also appears that the program is highly accomplished relative to budget.

Harnesses excellent capabilities of NREL.

Important support for industry in addressing barriers holding PV back.

Comprehensive work, good list of collaborations and number of publications. Very good milestone table on slide 6. One of the few presenters that explicitly showed decision points.

Project Weaknesses:

Not sure there is enough data being collected on thin film PV.

Limited resources. Support for PV industry could be increased.

Presenter mentioned that lack of capital equipment money prevents the replacement of aged equipment. This causes work to progress slower. They get by with cheaper equipment to meet immediate needs. In the long run that is an efficient way of spending taxpayers' money. If equipment needs to be replaced, it should be replaced. Milestones are attained, but the work could be faster if appropriate capital investments in Testing and Evaluation are made. This area is critical to the success of the SETP.

Recommendations for changes to the Project Scope:

Continual review of tasks that this project will address to ensure that they are the highest priority and will have biggest positive impact.

Look at increasing funding.

Review: EERE 2010 Solar Program Review
Presentation Number: SI017
Presentation Title: Test & Evaluation
Investigator: Granata, Jennifer

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
T&E is a critical foundation for developing accurate models and to perform reliability analysis. I know SNL & NREL collaborate, but It's important for the two to be aware of each other's module testing to ensure that there is no duplicate testing.

I found it very difficult to distinguish this from the reliability work. Possibly these two programs should be combined?
Sandia has always had strength in BOS testing and evaluation

All tasks in this project support DOE objectives.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Well managed T&E and good approach to keeping up with new technology. There is an increase in the use of microinverters that do need to be evaluated for reliability.

Input from industry, develop results and then provide feedback

Broad range of products including cells/modules and BOS.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
I see the move to increase reliability testing of many manufactures in this program to be good path that is providing more data to SAM and other modeling efforts. I think this is the right direction for the solar program. Also DC-DC testing is important as that is new technology that's come to the market. BOS is important evaluation as that is the cause of most O&M and

downtime.

The ancillary service evaluation is timely.

Good list of projects. This reviewer is especially impressed by the work at DETL.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration. It's important for this group to keep a close eye on the SEGIS program to make sure DOE maximizes its value.

Need user collaboration - utilities - especially for inverter service enhancements.

Lots of good partners and participants. The technology transfer of the module testing from Sandia to TUV is interesting and could be a good model for other such transfers.

Could improve number of companies in validation, verification & modeling.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Any acceleration of Arc Fault testing to provide guidelines, standards development, etc. would be good.

Inverter communication integration with other smart devices as well as load management is missing.

Project Strengths:
Great strength in T&E capability and leadership in providing independent analysis.

Excellent capabilities.

Excellent engineering and approach.

Harnesses Sandia's capabilities toward meeting DOE goals.
BOS and inverter testing is particularly important.

This area is critical to the success of the SETP.

Project Weaknesses:
Could try to be a little closer to pre-market product to start testing. Based on the industry collaboration, that may already take place.

Could use more priority setting relative to the uptake from utilities.

Limited number of activities due to limited resources.

Recommendations for changes to the Project Scope:
Increase focus on arc fault detection and mitigation techniques.

Suggest this project focus on specifying "DOE Testing Procedures" for PV panels and equipment. And then test actual equipment and publish the results on the web. Then, Consumer Reports could do the *, **, ***, **** ratings. This would be a great contribution to US taxpayers and would help prevent junk equipment from proliferating.

Inverter communications and specifically in the testing and evaluations.

Continuing evaluation of tasks being taken on to focus on those with biggest impact potential.

Additional funding should be considered.

Must continue ensuring close collaboration with NREL's T&E to avoid repetition. Although certain level of overlap is healthy and beneficial. Close collaboration ensures that the overlap is not a waste of resources.

Review: EERE 2010 Solar Program Review
Presentation Number: SI018
Presentation Title: Development, Validation and Commercialization of Grid Smart Inverters for wider photovoltaic technology utilization
Investigator: Reedy, Bob

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
The SEGIS is important project for the industry to gain acceptance by utilities for ride through during disturbances and disconnection from the electric system when required. This learning can then be used by the SDO to accelerate standards changes.

An expensive project, but it's not clear what is new about this inverter.

The work seems very relevant, but the relevance was not well communicated.

Addresses a number of inverter related technologies.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Good logical work progression. Good to see that witness testing of power management and ride through went well in the lab. The next phase with real world application is very important. It would have been good if Lakeland Utilities could test the functionality in their control center.

Other inverter functions and architectures could also be evaluated and are important to utilities

Shared inverter approach with DC-DC converters is innovative.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
I am assuming that the project is on track with work scope. I do not see a project schedule presented.

Prototype testing results mentioned sound good although details were not presented.

Work is of importance, but presentation was very general. Not many details to support the claims of progress during last year.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Many collaborators listed which is good. Important to technical utility partners. How often do they meet, hold conference calls or web meetings? Who are the utility contacts?

It seems like a very good team, but could seek an advisory group?

Good list of partners (leading solar developer and utility).

It will be important to publish results.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
The stage 3 work plan could be better defined. Are any of the UDAC planning to do demonstrations on their electrical system with smart inverters?

Future plans restated original plans

Field testing will be very important.

Future work seems vague on the hardware side

Project Strengths:
Close to becoming a real world demonstration with industry and utility involvement. Will help support 1547 & P2030 standards development.

Real world test of VAR control.

Lab testing and plans for field testing are really good.

The presenter mentioned that “one utility operator will believe another utility operator before he believes a solar person” This is a truth we must accept and work with in the SETP. Thus, one of the main strengths of this project is its interactions with utilities, getting “utility people” to help in attaining higher education and awareness of solar technologies and the ways to effectively integrate them in the power grid.

Project Weaknesses:
No project schedule. Not sure if Lakeland Utilities was involved in any of the witness testing (a must)?

Architecture may be unique and add cost.

Concern about additional complexity and component count (DC-DC converters) not supporting LCOE and high system reliability.

The hardware discussion was disappointing. Also, this presentation was posted late to the PeerNet system, giving no time for reviewers to properly prepare for it.

Recommendations for changes to the Project Scope:
Would be good to have Lakeland and possibly other utilities such as IOUs involved and take a more active role in the next stage.

Should be made clear what they are doing as opposed to inverter manufacturers.

Clarify how Low Voltage Ride-Through requirements are being addressed.

Check reliability and LCOE levels for determining status and how to proceed.

Review: EERE 2010 Solar Program Review
Presentation Number: SI019
Presentation Title: Development of Economically Viable Highly Integrated, Highly Modular SEGIS Architecture
Investigator: Mensah, Adje

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Project concept supports the goals of the EERE solar program. There is no data being represented to indicate if this accomplished the challenging goals identified in the projects. For the money ($3M)I don't see the results. There should have been a demonstration part of their project for $3M. I hope SNL has some data to back up their accomplishments.

Very expensive, but not sure what is new here.

Inverter communication and functionality is important to open new markets.

Microinverters are worth investigating to address some of the key objectives.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Approach is OK but where are the results? No project schedule to evaluate.

Not much information provided, possibly because of proprietary nature, if so these projects should go through a more private peer review.

Presentation did not give enough details to be confident that these barriers have been successfully addressed. These barriers are challenging for micro inverters (LCOE and reliability).

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
I may be missing something, but I don't see how this project answered the 4 challenges identified in the presentation. This is a novel concept of connecting many PV panels to the grid. It's important for SNL to take a critical look at the accomplishments.

This rating is given based on information provided by PSEG at a recent webinar

This is very difficult to judge as presentation gave very little detailed information. No cost or reliability data was presented.

Work is of importance, but presentation was very general. Not many details to support the claims of progress during last year

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Collaboration with the utilities besides PSE&G, who is already committed to purchasing this product, is important.

This would be outstanding, but are First Energy and PEPCO holdings are not full collaborators?

Difficult to judge from limited information in presentation.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Would have been good to see a little more detail on future plans.

I can't evaluate, I do not remember what an ASIC building block was.

Proposed future plans seem to be important items.
Not enough information to evaluate better

Project Strengths:
Could be a novel way to manage many small PV panels on the grid, but don't know how that will be accomplished from the presentation.

Cooperation with PSE&G

PV is the ultimate DG and the on a pole with a micro inverter has many applications

Looking at microinverters is reasonable to see how they could contribute to meeting the DOE goals.

The idea presented would have a positive impact on system integration of PV.

Project Weaknesses:
I don't know how this system architecture is supposed to function. Is it autonomous or centrally controlled? Project presentation could have used more detail.

What is new? Proprietary?

Level of data presented at the program review was very incomplete - qualitative and vague.

Unclear what LCOE and reliability targets have been achieved.

This was probably the most disappointing presentation. The project is 95% complete, yet basically there is not enough information to evaluate the technical merits of the work. If there are proprietary issues involved, then a closed session would have been better for the project. If that were not possible, then eliminate the presentation from the panel review.

Recommendations for changes to the Project Scope:
Needs well defined project scope, schedule and budget.

Integrate with other inverter work as well as data collection work

Suggest a detailed review of LCOE numbers and product reliability. This should be used to determine next steps.

Review: EERE 2010 Solar Program Review
Presentation Number: SI020
Presentation Title: SEGIS Smart Grid Inverter Systems Integration
Investigator: Pfeifer, John

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
As presented this project is strongly supporting the SEGIS solar program goals. Solid accomplishments through stage 2. Good there is involvement with the utility industry through EPRI.

Has the potential to use batteries to effectively shift solar peak to grid peak.

This project combines storage, optimizing system w/storage operating efficiency and inverter communication. If this added functionality is done at a minimal cost then the relevance is very high to DOE and the emerging utility market

Project goals align reasonably well with those of the DOE/EERE.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Good approach to stage 1 & 2. Transformerless design is good. How is UL dealing with this in their testing? If at all possible, the UL certification should be in place prior to stage 3.

Having 3 separate components does not seem optimal or a good approach to reducing costs.

Focus on LCOE through transformerless design (however this may be a problem for market acceptance). Modular approach to add storage may have advantages.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The accomplishments to date are excellent. The metric show good improvements and path to commercialization defined.

This was the only SEGIS project that provided data via slide 8 and NPV with cash flow. Seeing a graphic on efficiency would have been helpful

Initial lab testing reported to be successful (little data provided).

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Shows good collaboration with other organizations. Good they are participating with EPRI. Needs to identify utility partners who are committing to be part of stage 3 testing.

With the inverter and utility focus, possibly multiple utility input and collaboration could have been a SEGIS requirement

Coordination seems reasonable. Could consider adding Utility and a local code inspector to gather input.

No plans for publication or outreach to the community or the public.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
For the most part a well-defined plan for stage 3. May want to make sure that UL certification is achieved in time for demonstration. Also, cold use additional utility collaborators.

Combining market evaluation with technology milestones is an optimal approach towards commercialization. LCOE improvement and reliability should be quantitatively determined to be improved.

Project Strengths:
Good design using transformerless inverter. This is not a unique design, but getting it accepted could be a challenge.

The presentation clearly shows a strong background in the equipment and has depth in the technical facts and figures. High likelihood for success.

Good collaboration with financial evaluation. Modular approach to adding storage.

If the claims are correct, this work has a great potential in increasing PV grid integration. Costs were reduced (for battery-less option), and the flexibility of using (or not) batteries is a big milestone in inverters.

There was enough description to understand the merits of the project, and the presenter answered openly questions from the panel; however, technical merit cannot be definitely ensured since technical details were not presented.

Project Weaknesses:
Would be good to get utility protection engineers involved in evaluating the inverter.

Not enough utility collaboration.

Transformerless design may not be accepted in marketplace. Local inspectors may not allow systems that have a non-isolated inverter.

Recommendations for changes to the Project Scope:
Enlist additional electric utilities in stage 3 evaluation.

During the presentation it seemed that the presenter was impressed that utilities are ignorant of electronic interconnections and this is not true. Stronger utility involvement would be beneficial to the utilities and the project lead.

The project could include optimal battery sizing for dispatchability, as well as battery management for increasing battery life.Gather market and inspector feedback to gain confidence that product will be accepted. This should be a gate for proceeding to phase 3.

Review: EERE 2010 Solar Program Review
Presentation Number: SI021
Presentation Title: 100kW Demand Response Inverter (DRI)
Investigator: Hammell, Darren

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Impressive accomplishment through stage 2. Certainly is on track to meet SEGIS goals. The DRI is a device that will appeal to utilities because it's large enough to make a difference in utility applications.

The idea is to incorporate a battery into the PV-inverter system to achieve a UPS-like capability with energy storage. It is unclear what is new here.

This is an innovative design that has most currently known potential requirements to operate in an IT controlled grid.

Overall goals fit well.

Limited potential to generally reduce LCOE due to relatively small system size (100kW) and extensive feature list (which add cost and complexity). Perhaps could help reduce the LCOE if a system uses all four ports.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Strong solid approach with electric utility input to requirements. Would be good to see the financial metric for this system.

The organization is trying to create another inverter. How does it differ from existing inverters? Any claim such as "the inverter transformer is 99.75% efficient" immediately raises questions in my mind.

In addition to the functions noted it also does demand control acting as an ASD for motor loads

Market size for highly integrated product unclear. Unsure of usefulness of variable speed drive port for general applications.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
For the most part the project has met or exceeded its goals. It's possible for them to improve on the tracking efficiency.

The claims need to be confirmed by a 3rd party.

As one of the few SEIG projects that provided results, in both single lines and energy graphics, it seems the accomplishments are outstanding.

Focus is on highly integrated system. MPPT algorithm needs improvement.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration with utilities. Is there an electric utility system protection engineer involved in evaluating this product? Also good to see NJ investing in PPS manufacturing capabilities.

Good utility collaboration.

Good list of partners.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Solid plan to complete the project. UL certification is important part of this project.

Possibly obvious, but market assessment may enhance the project, especially demand response and energy management

Project Strengths:
PPS has solid technical expertise to create the DIR. Has demonstrated its successful operation such as var support and grid isolation with signal from a utility operator source.

A PV-UPS would be nice, but is not a breakthrough.

Obviously the project has listened to utility needs

Technical work for a highly integrated system seems solid.

Project Weaknesses:
Needs to firm up the demonstration sites and gain commitment from utilities to participate in project.

The claims seem impossible. How can this inverter be so far superior to what's on the market now? What inverters has this team built before?

Optimal battery sizing.

Probably only cost effective for a system that uses all or almost all of the features as focus of approach is for a highly integrated system.

Unclear whether manufacturer has capabilities to launch and support product in high volumes.

Recommendations for changes to the Project Scope:
None identified.

Detailed market analysis should be done to determine potential for product.

Detailed cost analysis should be done to quantitatively assess real reduction in LCOE.

Manufacturing and support plan should be reviewed to determine if product can be successfully deployed in significant volumes.

Review: EERE 2010 Solar Program Review
Presentation Number: SI022_
Presentation Title: PV Inverter Meets Smart Grid
Investigator: Scharf, Mesa

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
This SEGIS project is important to the utility industry in that will demonstrate a method of HP PV management using PMUs for system protection. This learning can also funneled to IEEE SDO to accelerate changes in standards development.

The proposal had two very interesting ideas: (1.) using synchrophasors for islanding detection, and (2.) a dynamic MPPT plan

This SEGIS project incorporates many additional user functions which will accelerate grid integration including customer energy management integration which could ultimately increase cost effectiveness as demand charges may soon increase.

Proposed synchrophasor anti-islanding approach has performance advantages for high penetration scenarios.

This anti-islanding will probably add to system cost - going against LCOE.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
The program approach is well defined to address all steps to commercialization.

Slide 5 graphic is excellent and could optimize DOE or Lab FOA's that are targeted at product improvement moving forward.

Anti-islanding approach proven to be feasible which is good progress.

Unclear if proposal for MPPT test protocol by individual private company will be accepted.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The project has successfully accomplished what it set out to do under budget. I would like to see a little more data on how quickly the PMU detected an islanding situation. There is no indication that there was any witness testing by Sandia. What size inverter is being deployed?

Other than MPPT, the metrics of results were not well presented. There were only notations that they were accomplished.

Progress to date seems good on anti-islanding.
Little progress on MPPT protocol.

Too many general remarks in the description of this project

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Very good collaboration partners on the project. SEL is a well-respected system protection company and PGE is a progressive utility in the DER integration space.

Seems to have no university involvement

PGE is a leading utility in this space, though additional utility input in the future phases may enhance the project as indicated in the future plans. They did not list industry as collaborators, such as the developer or component supplier for the ODOT system and building energy management companies.

Good list of partners which will facilitate needed testing and review.

Very few collaborators. Little outreach to industry or public.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Looking forward to seeing results from the utility field deployment testing and results from MPPT.

Budget seems quite high for future work.

Project Strengths:
Good teaming of solid companies. Comprehensive plan that will provide good learning to the industry on managing HP PV and inverter grid interaction.

Beyond the other SEGIS projects this brings in an integrated approach to on-site energy management.

Proof of feasibility of innovative anti-islanding approach.

Project Weaknesses:
Could have displayed data from the test results such as MPPT energy gains.

Limited progress on MPPT testing proposal. Unclear whether this MPPT test protocol is best championed by an individual private company (would seem to be more ideal for DOE).

This presentation was posted late to the PeerNet website, giving little time to reviewers to prepare for it.

Recommendations for changes to the Project Scope:
If possible gain another utility partner on the project for additional utility experience.

No changes.

None noted.

Examine proposed budget closely.
Perform a detailed review of increase in LCOE with new anti-islanding approach.

Review: EERE 2010 Solar Program Review
Presentation Number: SI023_
Presentation Title: Smart Grid Photovoltaic Pilot
Investigator: Freestone, Maryl

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Project is supporting PV integration with storage. It's good to see the four different study groups to make comparisons. Good high level definition of project.

Will be an excellent utility field test.

Understanding customer response is the next phase towards grid efficiency enhancements. More advanced communication and controls which take advantage of the electronic interconnection would enhance the project.

This will likely be one of the seminal studies of residential PV in the US.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
It's good to see the four different study groups to have comparisons. Looks like everything is in place to start selecting customers for this pilot. Engage local code people early in this process to minimize installation delays.

Stepped up control groups is a great approach, as well as multiple periodic surveys. No indication that base line measurements will be taken

Plan seems to be well thought out.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
No technical results as this is very early in the project.

NA - just started - ranked same as plan.

Still lots of work to do.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Should consider collaborating with NREL or Sandia on instrumentation and data monitoring methods.

Utility led, possibly PJM input would be beneficial.

Could get input/support from NREL and Sandia.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Overall a well-defined plan to accomplish by end of 2012. DO collect historical data on these customers. Consider installing AMI on homes as soon as they are selected for additional baseline data.

NA - just started, so same rating as relevance and approach

Most of work is in the future.

Project Strengths:
Project will gain a good perspective of customer behavior.

Valuable field test by Com Ed, Chicago area, to investigate high-penetration issues.

Utility lead and integrating customer perceptions.

Interesting to get data on solar in the Midwest.

Results are likely to be very influential.

It was a pleasant surprise to hear about this project where residential customers were effectively engaged. PV integration is not only about technology, it is about empowering people with tools and information so that they can be active participants in a new energy future. Very important to include social sciences aspects, not only marketing, in gauging customer attitudes and perceptions towards PV and other technologies essential to grid integration.

Project Weaknesses:

Could have included a project and budget schedule for this project.

Limited to residential size systems.

Recommendations for changes to the Project Scope:

Consider enlisting Sandia or NREL on data collection methodologies.

Include input from PJM

Very important to widely publish as many details as possible.

Consider adding Sandia or NREL to project (formally or informally) - in support of Argonne and to ensure coordination with other complementary projects.
If the installations result in an interestingly high penetration level, then consider metering the feeders to gain additional information on grid response. This seems like a good opportunity to get additional real impact data.

Consider interacting with other projects that are also engaging customers, so that experiences are shared and best practices identified that benefit similar projects throughout the Nation.

Review: EERE 2010 Solar Program Review
Presentation Number: SI024_
Presentation Title: SMUD PV and Smart Grid Pilot at Anatolia
Investigator: Rawson, Mark

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
This is a very relevant project to EERE solar program that will address how to manage PV production to meet GHG goals. This will be an excellent test case that will help drive future PV integration strategies.

Combines AMI, storage, PV, and customer response. It does not include demand side management or load control.

Continuation of important PV contributions by SMUD.

Real world data from California is very important.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
The project design approach as outlined should provide desired results. How to manage the customers in this project could be better defined.

The approach is excellent, especially the leveraging of the Anatolia Solar community.

Inclusion of storage is important.

Plan to include "Community Energy Storage" is innovative and will provide valuable data on that configuration.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Too early to make any comments. Project has not started.

NA - has not yet started - - ranked same as plan

Project is just starting.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Excellent team put together by SMUD with good tie into the national lab network.

A big project like this one should have the involvement of a California university.

Great collaboration and hopefully the CEC is more than just a cash contributor.

Solid list of partners.

A social science perspective would help better gauge customer acceptance, attitudes and perceptions about PV.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This whole project as described is proposed future research. The project plan is well defined. Having Navigant on board for project management support enhances this project.

NA - same as relevance and approach since it just started

Project is just starting.

Consider using social science-based surveys, not only marketing or price-responsiveness.

Project Strengths:
Good team and project plan. Good to see a project schedule.

A clearly defined plan, with people who are capable of doing it.

Well planned.

Should provide valuable data.
"Community Energy Storage" inclusion is innovative.

Important to engage residential customers. PV integration is not only about technology, it is about empowering people with tools and information so that they can be active participants in a new energy future.

Project Weaknesses:
Customer interaction is not well defined.

Just starting - so difficult to evaluate.

It is very important to include social sciences aspects, not only marketing, in gauging customer attitudes and perceptions towards PV and other technologies essential to grid integration.

Recommendations for changes to the Project Scope:
Develop a plan for communicating to and enlisting customer for this project.

Consider approaching energy storage sizing as the minimum required to firm the intermittency.
Align customer response to get demographic diversity - though this may be difficult in a single neighborhood.

Consider adding higher resolution data measurements to give details of dynamic impacts.

Ensure results are widely published.

Consider interacting with other projects that are also engaging customers, so that experiences are shared and best practices identified that benefit similar projects throughout the Nation.

Review: EERE 2010 Solar Program Review
Presentation Number: SI025
Presentation Title: Analysis of High-Penetration Levels of PV into the Distribution Grid in California
Investigator: Kroposki, Benjamin

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
This project is one of the most relevant to the HP solar program. We should be able to gain results on HP of PV on distribution fairly quickly as the project already has HP of large PV systems incorporated on the SCE grid.

This is directly relevant to DOE goals and with a lab lead there is potential for leveraging other lab work.

Leverages from unique opportunity to get data as SCE deploys significant amounts of PV.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
The approach is well structured to deliver quantifiable results to the industry of how to study and manage HP of PV.

It is important to integrate with existing distribution system models wherever possible for repeatability.

Also, the high penetration VP handbook would be even more beneficial if it were targeted at both design and planning. Additionally this would be beneficial prior to phase 5.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
No results as project just started.

NA - just started.

While this project is just starting, the proposed work is set up to make significant contributions to the DOE goals.

Ideal situation to get high penetration data for commercial sized installations.
Clarify expected methods to measure success.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Great team that encompasses industry leaders with NREL as the project lead.

A project of this size should have one or two university partners, even if their funding is small (e.g., one student). The students could be required to have US citizenship. Give engineers in the education pipeline the opportunity to take part.

Great team, it could include ISOs and additional distribution models developers.

Coordination with SCE will be critical.
Other inverter manufacturers could be included.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
I am looking forward to early results from this project. It is well planned out with clear yearly milestones.

NA - just started - - ranked same as plan

Most of this project is in the future. Lots of the definitions take place in phase one. Good definitions will be critical to the success of this project.

Project Strengths:
Having ready-made large PV projects with HP PV scenarios to study is a real strength.

This is a model applied research project and exactly the type that DOE should fund - it has a cooperating electric utility and an excellent experimental test bed in the form of utility-controlled large PV rooftop arrays. It is ideal for studying high-penetration PV.

Takes a great opportunity to gather information as SCE does large PV deployment - this is an opportunity that should not be missed.

Plan to publish High Penetration PV Handbook will be valuable.

Extremely important work. Great idea to develop a Handbook for High Penetration of PV Systems.

Project Weaknesses:
None seen. Just starting.

Recommendations for changes to the Project Scope:
It would be good to create and publish chapters of the HP PV handbook as the project progresses.

It seems also that in the 5 year period inverter communications will be more common place such that results will be less relevant if this is not included.

Define objectives and deliverables more clearly.

Consider adding other inverter manufacturers.

The work on year 2 seems ambitious. Part of that work could start in the latter part of year 1. On a similar note, the work on year 5 is very important and might require more time. It could begin during the latter part of year 4.

Review: EERE 2010 Solar Program Review
Presentation Number: SI026_
Presentation Title: Sunshine State Solar Grid Initiative
Investigator: Meeker, Rick

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Project as defined supports the solar program goals with many partners and 202 MW of PV to be studied. Understanding the PV output variability and grid interaction at the transmission and distribution level is important.

Big money, but little plan.

The project is highly relevant, and the team includes multiple utilities with multiple business models. However, not all of these projects will be representative of high penetration. Especially if connected to the transmission system.

Important to get PV performance and impact data from Florida.

Goals seem good in general but need to be more clearly and quantitatively defined.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Good high level view of the program. Including a project schedule would have enhanced the presentation. A data collection plan for all these sites should be provided. Data consistency may be a problem?

Little focus on the technical issues. Most of the discussion was about the team that has been formed.

With the project just getting started, utility partner evaluation may be beneficial to project as it progress to prevent from staying in the purely academic space. Additional supplemental slides of previous related research were very helpful

It will be important which sites are selected.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Excellent start to the project with many strong partners. Management and team approach is well defined.

NA - just started. I do not think a kiosk approach to outreach is valid at this point of market development.

Project has just started. This is difficult to assess at this time.

The presentation included many generalities and few specific ways through which the team expects to accomplish its objectives.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Many great collaborators in the state of FL. I would like to see the national labs play a role in this project as they could gain more data points.

A university-run consortium, but apparently little (in terms of $) student or faculty involvement.

Multiple utilities, but it will take creative approached to keep them engaged.

Should ensure that this program is coordinated with other projects in Florida. For example, the Desoto project is already planned to be monitored in another program.

More input from Sandia/NREL would seem appropriate.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
Future plan is well defined.

NA - just started - ranked same as plan.

Selection of sites will be very important.

Project Strengths:
Large data set of PV sites to be studied. It will be good to be able to study large PV systems connected to the transmission system.

Data from Florida will be valuable.

This is an excellent opportunity to gather data in situations with variable resources.

Very good list of collaborators.

Project Weaknesses:
No data collection plan discussed. Poorly defined plan.

Just starting and exact sites not yet determined.

Although project is just beginning, plans for accomplishing project objectives are vague. This presentation was posted to the PeerNet website Late.

Recommendations for changes to the Project Scope:
The funding level far exceeds what is likely to be accomplished.

Careful review of exact sites that will be included in this project should occur as soon as possible.

Ensure that data taken is high frequency to show dynamic impacts. Define goals more clearly and measurably.

Review: EERE 2010 Solar Program Review
Presentation Number: SI027_
Presentation Title: Improved Modeling Tools Development for High Penetration Solar
Investigator: Washom, Byron

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
PV grid integration, high penetration of PV, forecasting and storage relate to the objective of the solar program.

The relevance is extremely high because of the well rounded approach and taking advantage of the University microgrid for controlled testing. Additionally, the climate monitoring and cloud tracking seem to be unique to this project

Leverages unique position of UCSD as a large "microgrid"

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
The approach to accomplish the project objective is not clearly defined in this project. There are pieces of information that indicate this could be good project with interesting data collection and analysis but certainly unclear and not well defined.

The climate monitoring and cloud tracking seem to be unique to this project, as well as the bidirectional communications

Opportunity to address many key issues.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Not much progress defined as this project just started.

Even though recently started, this project seems to have a running start with monitoring in place.

NA - early in project.

Although this is a new projects, general plans for accomplishing objectives are reasonable and feasible.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
How this project will collaborate with others is not well defined.

This project specifically addresses awareness to RTOs and ISOs

Good list of partners.

Intent to put models and data in public domain is excellent.

Could improve plans for dissemination of information and outreach

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
This project has interesting future work in cloud tracking and insolation forecasting.

NA - none proposed - - ranked same as plan.

Good path outlined but additional details could be provided.

Project Strengths:
Data availability in this project is a strength. I t will be interesting to see how EDSA is incorporated (could expand on this in the project description).

A model project - the team is creative and also performing field tests.

Incredible team and seems to be well leveraged with some existing capabilities at UCSD

Unique situation with UCSD being on the utility customer side of the meter.

Self-permitting authority will facilitate inverters with new features to be deployed.

Intent to gather significant high resolution data is really good.

Open nature of data and models is important. It should ensure that the results from this project are published in a highly visible way.

Very important practical work correlating cloud coverage and availability of solar energy.

Project Weaknesses:
Project is not well defined and organized. Could use a clearer project management plan and certainly a project schedule.

Recommendations for changes to the Project Scope:
A more defined project structure and clearer plan that includes a project schedule. Maybe enlist a project manager.

No changes - keep them doing what they are doing!

None noted.

Review: EERE 2010 Solar Program Review
Presentation Number: SI028_
Presentation Title: High Penetration of Photovoltaic Generation Study – Flagstaff Community Power
Investigator: Narang, David

Criterion 1. Relevance to overall DOE objectives – the degree to which the project supports the goals and objectives of the EERE Solar Program. (Weight = 20%)

Comments:
Contains some of the objectives of the solar program. Good to see an electric utility lead activity with distributed PV.

An extensive utility-conducted field test to uncover any problems, expected or unexpected, with high-penetration PV.

One of the only projects that addresses the possibility of Solar enhancing resilience of distribution feeder.

Important to get data from AZ installations.

Study items planned for 2010-2012 are somewhat duplicative of other projects.

Criterion 2. Approach – the degree to which technical and non-technical barriers are addressed, the project is well-designed and feasible. (Weight = 20%)

Comments:
Phase 1 design for FY 2010 follows logical steps. The partner's role could be defined a little better in the presentation.

Seems sound, but information provided was minimal.

Good technical design.

Weakness is that it is too slow.

Criterion 3. Technical accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The project is just starting and does not have any results.

NA - just started - but it will be somewhat integrated with an extensive smart grid project

Just starting.

Criterion 4. Collaborations and Technology Transfer with other institutions – the degree to which the project interacts with industry partners, universities and laboratories. Technology transfer is occurring as indicated by patent applications and licenses. (Weight = 10%)

Comments:
Good collaboration partners. What is NREL's role in this project? Is it the data acquisition? Engaging internal utility resources early is desirable to get buy in and commitment to the project.

It seems like a great team. They may want to include AZ Corporation Commission on progress as well as other AZ utilities

Good partners.

It will be important to publish results.

Criterion 5. Proposed future research – the degree to which the project has effectively planned future work in a logical manner by incorporating appropriate decision points, considering barriers to the realization of the proposed technology and when sensible, mitigating risk by providing alternate development pathways. (Weight = 10%)

Comments:
The project could use a project schedule to define milestones. More detailed plan could have been presented.

NA- Just started - - ranked same as plan.

Good plan but could be faster.

Project Strengths:
It's a utility lead team with high penetration of PV.

Good plan. Qualified researchers. Strong enthusiasm.

Good to get data from Arizona.

Project Weaknesses:
Could have more project details in the project plan such as project schedule.

Long schedule. Lots of study before deployment - should be accelerated.

Recommendations for changes to the Project Scope:
Start to collect detailed electric base line data on the feeder prior to installing more PV systems.

Look at ways to accelerate deployment of demonstration project! This information would be beneficial long before it could be published in 2014 (and that will be only for a part of a year from 2013).

In order to ensure the anonymity of reviewer feedback, reviewer comments are listed in random order for each question

Review: EERE 2010 Solar Program Review
Presentation Number: MT001
Presentation Title: Solar America Cities – Solar Boston
Investigator: Belden, Andy

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
This project appears to depend too much on simply procuring additional resources to be constructed within the city. That activity does little to further the overall RD&D objectives.

Program did a great job on a limited breadth of market barriers. The focus on solar in emergency response at first seemed a questionable priority, but upon further discussion, am wholly persuaded by the merits and see the value of getting non-traditional stakeholders to understand and embrace the value of DG solar. Kudos on that. Workforce development and municipal procurement are squarely in the wheelhouse. However, other cities did address additional mission-critical market barriers such as permitting and financing that can assist the long-term market success of independent solar industry, and would have been nice to see Boston take it on as well.

The city's activities (solar map, procurement templates, installer and city manager education initiatives) serve as good efforts towards meeting DOE's objectives.

In Boston's case, municipal procurement might be the most direct and controllable way a city government can expand market penetration. Significance for long-term market transformation depends on replicability. What I missed in Boston's case was a proactive strategic plan to extend their success to other public agencies.

The use of PV in emergency situations demonstrates PV's unique properties but is it a significant contribution to market transformation?

Great work on the integration of solar into emergency response
Recommend thinking through resource, capacity issues as market penetration increases

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
This project does not seem effective at exporting the city's experience to other venues.

For selected projects, grantee seems to be working capably and effectively. Again, the only concern is with the breadth of activity.

Boston has adequately addressed the barriers they outlined. These efforts seem to be focused on addressing market barriers unique to the city of Boston rather than developing broader market potential outside of municipal procurement. More could be done in addressing the barriers that impact the residential, commercial and industrial communities across the city. I'd like to have heard more about the C&I buying pool mentioned in the presentation and how this effort makes procurement of solar more accessible to these stakeholder groups.

Developing a standard PV procurement process in a major city is a significant though not ground-breaking achievement. Replicability will depend on whether the barriers surmounted for one set of public agencies will resemble those presented by others.

Well thought-out linkages between projects, goals, budget, and objectives
Broader thinking about permitting best practices rather than just fee reduction would be important

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide little assurance that these goals are being met effectively. Absent further detailed information, this objective does not appear to have been achieved.

The workforce training will leave a lasting legacy (one hopes), providing the nucleus of education and standards for a high-quality workforce. And the focus on leadership by example by putting solar on municipal buildings should help jumpstart market development. Again, additional barriers remain.

The city seems to have made some good progress in addressing the barriers it outlined as its primary objectives.

The program was apparently effective in modifying an entrenched bureaucratic procurement process and is working toward a standard template that will streamline the process for other agencies. This is a solid achievement but only the first step in developing a strong market on municipal buildings. City ownership of the systems

Well on the path to market transformation in an area with relatively low insolation and challenging, older building stock

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
This project does not seem effective at exporting the city's experience to other venues.

Program seems to have partnered effectively with full range of stakeholders. Extra credit for pulling in non-traditional solar users with the emergency response pilot.

Most of the collaboration was on a cost sharing, in-kind staff and facility-type basis. What is Boston doing to support non-governmental institutions in facilitating greater penetration of PV by other stakeholder groups (residential, commercial, industrial etc.)?

There were few new partnerships formed other than with the incentive programs that were already in place to support the solar market.

Very broad, highly complementary partnerships.
Budget leverage ratio of ~16:1 is impressive

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The city does not provide a significant funding match for the proposed future activity. Further, simply purchasing additional PV installations should not be considered helping to realize the overall proposed goals of the RD&D program.

The proposed solar advisor (to navigate complex incentives) and the marketing efforts seem particularly helpful.

Boston mentioned a preponderance of residential properties that are rented in the city. What can be done to work with the owners of these properties to further the penetration of solar PV in the

residential sector?
Attention to streamlined permitting and solar curriculum for schools seemed like arbitrary add-ons unrelated to the primary strategic aim of mining the municipal building market.
Didn't present as tight a focus on projects going forward
Possibility of capacity issues, resource model linked to growth
This will be an issue in general for all cities, not just Boston
Addressing through shared funding, collaborations, etc.

Project Strengths:
The project helps develop city-level institutional knowledge for managing the zoning, siting and local use issues that arise with distributed renewable deployment.

A big strength is the ground-breaking effort to effectively engage non-traditional solar users in the planning for evacuation routes. Would like to see some evangelism on that effort--make it a national model.

Tenacity and focus in accomplishing an unquestionably useful objective that could expand penetration of an important market sector.

Strong foundation, well-integrated set of projects, goals, objectives and high leverage of federal dollars

Project Weaknesses:
The project appears to do little to facilitate knowledge transfer or systems standardization outside of the city proper.

A solar market is only as strong as the weakest link--and there are still significant barriers. Completing work on permitting and developing a financing program would be good.

Absence of a coherent long-term strategy with mutually reinforcing parts.

Need to look at complexity and challenges associated with growth

Consistency, predictability of permitting processes, not just fees
Uniqueness of buildings, age, insolation, structure, installation, materials

Recommendations for changes to the Project Scope:
The project scope should increase city share of matching funds. The project scope should expand

city external engagement through state regulatory reporting, industry standards forums, engagement, and federal energy regulatory comments. The scope should expand evaluation of regional wholesale energy market impacts of the distributed generation resources.
A solar market is only as strong as the weakest link--and there are still significant barriers. Completing work on permitting and developing a financing program would be good. Would also like to see an effort to take the solar-in-evacuation-route concept statewide/national...

What can Boston do to address barriers associated with residential, commercial and industrial stakeholders in the market? Can the city leverage its leadership to encourage broader market penetration?

Drop schools and permitting and focus on multi-year plan for the municipal building market, including bundling with energy efficiency retrofits.

No need to change the project scope per se
Focus on achieving the same level of focus, goals, metrics, and coordination going forward vs. work to date

Review: EERE 2010 Solar Program Review
Presentation Number: MT002_
Presentation Title: "CITY OF SAN DIEGO Sustainable Energy 2050 Plan"
Investigator: Giannelli Pratt, Linda

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Adequate performance in this area.

Certainly a broad range of activity, and a lot of solar happening in the city. (One concern: it was not completely clear how much of the work was directly done under the grant and how much was leveraged from other stakeholders). Getting solar on municipal facilities at such scale is a tremendous achievement. The 'Fire Safe Communities' effort is certainly worthwhile, but not a key market barrier (though with all the state policy tailwinds, nice to see the city expand beyond traditional players). The marketing efforts (mapping, etc.) are also cool.

A good array of activities that lay a solid groundwork for next phase efforts. Good recognition of projects that can support information transfer and leverage private investment and further market penetration.

Performance analysis, market research, outreach--these are all activities that are, or should be, carried on by the local program administrator for the California Solar Initiative. What is objective of MASH-related activity? How will you "demonstrate benefits" of PV on MF affordable?

The 2050 plan is the flagship. But it's weak. Not breaking new ground in a focused way by defining the most crucial questions to answer. What they think are key learnings (e.g., solar is best promoted through word-of-mouth and happy customers) are commonplace. Permitting has been "streamlined"--but San Diego had an exemplary permitting process prior to the SAC program.

They have identified the barriers but little evidence that they have done things to overcome them.

Significant activity in all sectors, broad approach across many areas
Focused on cost, performance, installed base, measuring results

#1 installed base in US, congrats

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
In a region with a workable wholesale energy market, focus on barriers should also address the ability for delivery of aggregated distributed resources to the broader energy market.

Projects selected were largely within control of grantee. Given the robust local solar industry, question whether the marketing efforts are redundant to private efforts of the same; proof will be in the pudding. Would like to see a good plan for marketing the PACE program once it is available.

Overall a well balanced approach to address significant market barriers and to support information transfer to a broad array of stakeholders.

The objectives are hazy and unrelated to stated barriers.

Good combination of technical performance, outreach (real estate), permitting, education

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
As the current #1 city for solar resource, it would be impolite to argue against the level of success. However, the transformative nature of the project goals will require a higher set of goals. Reaching broader national goals of renewable supply will probably entail the situation of local/distributed resources putting output back to the regional electric grid.

Great progress on getting solar on municipal buildings. Project has been a long time in coming; look forward to seeing more of the same. Really looking forward to seeing the outcome of the marketing work. It did not appear that any of the selected projects were beyond the scope of the grantee.

San Diego has some good ideas, however, it wasn't clear how much progress has been made to date on a few of the key activities and what the pay-off has been as a result. Overall, San Diego seems to be headed in the right direction.

It appears that a solar mapping system was the sole product added by this program.

Significant activity in all sectors, broad approach across many areas
Focused on cost, performance, installed base, measuring results
#1 installed base in US, congrats

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Additional information might be useful here, but the appearance that the outreach at the educational/discussion level does not result in long-run institutional development.

Seems there is a great partnership with CCSE and internal city staff in particular; beyond those, not sure with whom else they partnered.

San Diego seems to be using marketing and communications strategies effectively to raise the awareness about solar PV and its use within the city. They could probably expand on this by working with a broader array of community organizations to further the adoption of the technology by end-use markets.

Little evidence of collaboration beyond the prescribed project partners. Weak on developing new business relationships. There has been little progress in forging a stronger partnership with San Diego Gas & Electric.

Unclear on the extent of utility and workforce collaboration.
City of San Diego stated "it's not us" but that runs contrary to collaborative models. With the high degree of installations, MORE collaboration is necessary, don't be so quick to point out "that belongs to someone else"

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Keep up the efforts.

A lot of the marketing activity (Solar Implementation Plan, outreach to real estate community, etc.) are to take place in the future. I'm not sure I have enough information on what those activities are exactly comprised of to make good recommendations.

The bread and butter of this effort are in the real estate valuation analysis and the permitting process improvements.

Future goals are unclear beyond the development of a long-range plan, the actionable objectives of which are also nebulous.

Leverage the existing foundation to better define goals that extend current initiatives further with clear end-game results

Project Strengths:
Efforts in a solar-rich area are inherently more valuable than in a solar-poor area.

The effort to solarize city facilities is truly inspiring.

Impressed with the real estate valuation analysis. I feel this is an important area to address and should be instructive outside of San Diego.
The rate tariff analysis is also an interesting project where the results could be leveraged to further enhance the value proposition for certain stakeholders.

Harmonizing the permitting process is also an area of strength.

Solid foundation

Broad activities (outreach, education, technical, performance, processes)

Project Weaknesses:
Could use better metrics and advance statement of measurable goals.

It's hard to tell how much of the activities described are done by grantee and how much are done by allies in the city and CCSE. Not a criticism per se, just a comment. A lot of the project deliverables and outcomes are deferred to the future; hard to judge at this point.

Wasn't clear how much progress has been made on some of the activities outlined.

Very unclear why Center for Sustainable Energy, the program administrator for the California Solar Initiative, isn't doing what the city has used this funding for (e.g., preparing for MASH program).

Unclear on the extent of utility and workforce collaboration.

City of San Diego stated "it's not us" but that runs contrary to collaborative models. With the high degree of installations, MORE collaboration is necessary, don't be so quick to point out "that belongs to someone else."

Justification for meter data gathering strategy is unclear

Recommendations for changes to the Project Scope:
Need to address interaction with wholesale energy markets for distributed resources.

Would like to see additional effort made to market the PACE program once (if) it is developed. For PACE programs, the larger the participant pool, the lower the rate. As PACE is a new effort, it needs some specially-focused outreach.

Choose a single "SMART" goal that adds to what is already being done and focus on it.

Take a very hard look at the real-time meter data collection system. Answer was "This is additional, above and beyond, felt like an additional level of confidence above and beyond CSI 3rd party meters"

This raises a number of questions at the local, state, and federal levels vs. existing third party monitoring put in place as required by state programs.

Need to really understand what the data is being used for, resources it takes to manage, why other data is not used, what the utility SDG&E perspective is, CPUC/CSI, etc.

This has the potential to snowball and be a significant resource drain if it is an essential element to the City of San Diego's rollout

Review: EERE 2010 Solar Program Review
Presentation Number: MT003
Presentation Title: Solar Market Transformation in Portland, Oregon
Investigator: Jacob, Andria

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Presentation materials highlight in particular the benefits from reduced costs and timeline on the permit/zoning issues.

Project appears to have done a good job of identifying barriers, and addressing elements that are within the city's control. I like the combination of barrier-busting in creating market demand. It seems apparent that quality thought went into upfront planning.

On target with their objectives.

"Solar Now!" and "Solarize Portland" exemplify another SAC effort that has singled out one high-priority market barrier and developed a model for attacking that barrier by improving the marketing and delivery of residential systems, a model that can have a significant impact on programs around the country. Community aggregation and volume buying spearheaded by the city is one of the next best practices for cost reduction and market transformation.

Barriers addressed -
Lack of consumer awareness
Consumer misperceptions
High upfront costs
Low energy prices
Small installer base
Inconsistent regulation - non-existent permitting/inspection processes

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project presentation materials provide some assurances that goals are being met. Additional detailed information would be useful.

Relevant activities, ambitious but achievable goals. Most importantly, addressed issues within grantee's control.

What qualifies as a "reach" in terms of increasing market demand among Portlanders, businesses, opinion leaders etc...?

Committing to a stretch goal of a four-fold increase in installations above 2006, provided the motivational launchpad for a well-planned strategy to attack the first-cost and consumer skepticism issues at the same time. "Unified market presence" originating from the city itself is a very powerful concept and breaches the traditional gap between public and private sectors. Marketing blitz that made simultaneous use of many effective marketing elements in a focused, intensive way.

-Well thought out, nice mix of local, state, city, utility, and industry partners
-Proactive approach to developing and implementing Solar Now!
-Leadership with Climate Action Plan
-Financing options
-Predictability AND consistency in permitting. This is one of the only cities that have distilled permitting down to the key issues OTHER than just calling their efforts a 'streamlined permitting'

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide assurance that there is good progress to achieving project goals.

Results speak for themselves. Seems to be good progress--if not homerun success--in all listed project areas.

Good presentation demonstrating their effectiveness in meeting goals while highlighting the "take-aways" from their experience.

High goals backed by great progress (even if they didn't meet them all). A 240% increase is nothing to sneeze at.

Huge uptick in residential market due to neighborhood approach and direct marketing by community champions and customers themselves demonstrates a best practice for the residential market. Establishment of high proportion of e-permitting is also a significant accomplishment.

Great job at specific and measurable results in 4 key areas, each with a linkage to barriers and results, that roll up to DOE/EERE
Slide #7 is exactly the way all cities should think about presenting results, progress, links to objectives. The closing take-aways show the ability to scale

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
The outreach initiative to other Oregon seed cities seems potentially effective at exporting the city's experience to other venues.

Love the engagement with citizen action. And with pubs. Key stakeholders...

Spawned new levels of collaboration with grassroots groups that can become effective, trusted ambassadors for solar.

Solar NOW! organizational chart is a brilliant way to communicate roles, relative contributions. Nice work.

Great result on $120 permit fees (residential) and 80% of activity online.

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The Portland project presentation seems well focused on next phase goals and measures.

Current grant is ending, though work continues through new special projects award.

Like the scalability of these efforts to other cities across the state.

Building the neighborhood approach as a standard practice is the next best strategic move.

Strong leverage of:
- Portland Neighborhood Solar Initiative
- Expanding volume purchasing
- Others per slide #14

Project Strengths:
The project helps develop institutional knowledge for managing zoning, site, and local use issues that arise with distributed renewable deployment.

I like the mix of barrier busting, consumer education, and new purchasing models. The permitting effort is well-targeted, well-executed. Really excited about the SolarNow campaign, and the efforts to take it on the road. Andria is a passionate, articulate, and seeming extraordinarily effective solar advocate--kudos on a job well done in a state with super cheap power.

Building programs that have scalability for the activities across Oregon

Innovation and demonstrated effectiveness in cracking a major market barrier.

-Leadership with Climate Action Plan
-Financing options
-Predictability AND consistency in permitting. This is one of the only cities that have distilled permitting down to the key issues OTHER than just call their process a 'streamlined permitting'
-Leading by example by putting solar on city facilities

Project Weaknesses:
The project seems very effective based on the presentation and discussion; no specific criticisms

come to mind.

The biggest weakness is no fault of the grantee--power is cheap in Oregon and state policy is limited.

Perhaps how to make the transition from neighborhood guidance to a more pluralistic approach that can include a growing number of contractors.

What about workforce development? Community colleges, training, vocational schools etc.? Any need yet?

Recommendations for changes to the Project Scope:

The project scope should expand city external engagement through state regulatory reporting, industry standards forums, engagement, and federal energy regulatory comments. The scope should expand evaluation of regional wholesale energy market impacts of the distributed generation resources.

Current grant is ending, though work continues through new special projects award. Looking forward to seeing increased exposure for SolarNow purchasing model. If not already planned, suggest that model be written up as a “DIY kit” so that advocates in other areas of the country can replicate.

Produce how-to guide to neighborhood approach and quantification of reduced cost of PV as a function of volume buying versus the costs of administering the program.

Begin to look at workforce development, community colleges, training, and/or vocational schools, etc.

There are some opportunities to collaborate or leverage work going on with other industry organizations working closely with CA WIB (California Workforce Investment Board)

Review: EERE 2010 Solar Program Review
Presentation Number: MT004_
Presentation Title: Solar Salt Lake Project
Investigator: Baldwin, Sara

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Nice efforts apparent on this project, particularly with regard to market penetration. The transition from a net metering "F" to "A" in two years indicates good progress in removing obstacles.

The project went further than most in addressing critical state policy barriers (net metering, interconnection, 3rd party PPA) and should be commended for it. Good effort on creating demand through municipal projects and consumer demand.

They have done a tremendous amount in a short period. Good alignment with broader DOE Solar Program goals.

This program started with virtually none of the pro-solar policies and regulatory structures that were available to the other SAC programs. Through effective collaboration and considerable strategic acumen, the Utah program has successfully undertaken activities for putting these basic elements in place: interconnection, net metering, third party ownership, and partnerships with potential private sector champions. In Utah's case, what solar first needs is an effective institutional champion, and this SAC program filled that need.

Program was able to generate momentum in all key technical, process, policy, awareness and incumbent mindset.

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Interesting engagement at the level of the residential/commercial construction sector.

One of the big challenges is that so many of the barriers are under state, not city, jurisdiction. Given that, program has done an excellent job of wielding what leverage it has to address barriers.

Utah is making great progress in a variety of important areas identified as barriers. Their approach is producing results that have been recognized by other organizations (e.g. IREC - net metering grade).

The program has correctly prioritized the most important barriers and is moving quickly into later phases of program evolution (e.g., community solar).

Well thought out, broad policy, process, technical, education, qualitative, and quantitative goals. Model can be scaled extremely quickly.

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
In general, it would be nice to see better goal metrics established up-front with tangible evaluation of progress.

Excellent progress so far, with key state policy barriers addressed. Still waiting for final results for solar on municipal buildings, but progress seems good to date.

Net metering grade increase from F to A is laudable.

The program has not only laid the groundwork for future market expansion but has also begun to enlist the participation of key stakeholders in longer-range strategic planning that promises to lead to a healthy solar business environment.

Well thought out, broad policy, process, technical, education, qualitative, and quantitative goals. Model can be scaled extremely quickly.

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Appears to have numerous participants and points of engagement. Future efforts should ensure that the operating points of engagement entail their own funding/supporting mechanisms.

Grantee has done an excellent job of leveraging stakeholders throughout the spectrum--utility, city government, and non-profit policy groups. Was key to building public support and getting good policy.

Great mix of organizations and application of those organizations towards meeting their objectives.

PI states, "Most work has centered on bringing stakeholders to the table," and the program has established ties among the utility, city and county governments, and a major land developer. Surrounding communities are likewise becoming interested.

The right stakeholders were brought together: policy, utility, education, industry, city, etc. with a solid framework combined with leadership, goals, and vision.

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
It would be nice for the project to address integration of the energy resources into the broader electric marketplace.

Focus on financing options, utility incentives, and city-owned solar systems seems on target. RFP for solar on city buildings is out--looking forward to seeing results.

Again, evidence of clear strategy, with next focus on PACE financing. Extremely good sense of how major strategic elements of the future program must work together (e.g. "we have to make the utility whole."

Project Strengths:
Good apparent engagement with state-level constituents.

Program focused more than most on key state policy issues--kudos for taking on that daunting task, and so successfully. Program covered all the bases and delivered results.

Focus on building community to integrate solar into their products.
Addressing net metering, interconnection and 3rd party financing issues.
Analysis of PV and storage on deferment cost value
Education focus on real estate and appraisal community

Great choice of strategic moves. Organized all the main elements of a progressive program very quickly into a unified strategy.

Work with community development corporations and local banks to explore non-PACE financing. Solar leasing/PPAs as alternatives to capitalization.

Has started a comprehensive program from scratch, including workforce training.

Given the challenging cost of electricity from existing sources (coal) vs. the sunshine/market opportunity, this program is a model for what can be done in ways to address other comparable market challenges to drive paradigm shifts.

Project Weaknesses:

Could begin to address regional energy supply interactions.

None apparent.

Recommendations for changes to the Project Scope:

Suggest adding metric-based goals and providing progress measurements against these goals. Also suggest adding engagement with wholesale energy market functions and reliability standards organizations.

If PACE legislation passes in the state, look forward to the development of a financing program. If no PACE, perhaps another model? What learning points can be documented from the builder activities to incorporate solar into new homes? How might other cities work with the building community to offer similar projects?

Could Salt Lake look at and document the different models for aggregate purchasing within organizations such as LDS? How can an organization like LDS (or other faith-based group) leverage its vast network of facilities and resources to develop projects? What financing mechanisms work best for organizations like this? How can members of these organizations be included in the process? What messaging and marketing approaches work to reach these types of stakeholders? What is the market potential among the LDS, Catholic, Jewish, Muslim and other faith-based organizations nationwide and globally?

None--the program is making all the right moves. Take a look at workforce development, skills readiness in conjunction with scaling as fast as you can or as funding will allow

Review: EERE 2010 Solar Program Review
Presentation Number: MT005
Presentation Title: Santa Rosa & Solar Sonoma County
Investigator: Wright, Tasha

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
This project appears to depend too much on simply procuring additional resources to be constructed within the city. That activity does little to further the overall RD&D objectives.

California has a well-established regulatory/legal infrastructure for solar; Solar Sonoma County appropriately focused on addressing gaps (financing) and getting steel in the ground. This is one of the most (if not the most) ambitious project amongst them all and I salute the gumption.

The bulk of the project's budget and effort went to the establishment of a new organization that can serve as a local policy advocate, organizer, and customer education point of contact. The value of this strategy depends on what the new institution can do that couldn't have been accomplished within the existing organizational framework. The accomplishments of the organization are thus far difficult to quantify and leave an open question as to whether the SAC funds would have been better spent on new processes than on a new structure.

Complex county model is highly relevant to penetration of solar in areas OUTSIDE of major cities where consensus building across smaller jurisdictions is required.

Excellent work across financial, regulatory, educational areas

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
This project does not seem effective at exporting the City's experience to other cities. In addition the project presentation materials provide little assurance that goals will be met. Additional detailed information would be useful.

It's a hugely ambitious project, and partners have brought necessary real commitment to the table in support of achieving success (i.e. PACE program, local non-profit, etc.).

The new organization funded by SAC seems to have joined existing efforts to remove major market barriers--e.g., establishment of PACE financing—rather than to have staked out its own barrier buster to work on independently. The project needs stronger leadership in defining its unique mission.

Well thought-out approach to lay the appropriate initial groundwork to build the program. A non-profit consisting of many stakeholders was an important first step - tested, proven, and can be scaled

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide little assurance that these goals are being met effectively. Absent further detailed information this objective does not appear to have been achieved.

Success with the ambitious pilot PACE program speaks for itself. It's a difficult undertaking and the [Sonoma] program is the largest/most successful in the nation to date.

The creation of SSC is a good basis from which to build county-wide efforts.

The project managers believe that Solar Sonoma's existence had a significant impact both on the amount of new solar installed through the California Solar Initiative and on the development of California's most active PACE program. This may be true, but there is no way to verify this claim without better quantification of actual results attributable to the program. If having a neutral third-party customer advisor is making an appreciable improvement in customer uptake--and it is reasonable to assume that it could--then more work needs to be done to quantify the impact of this strategy.

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
This project does not seem effective at exporting the City's experience to other cities.

Program worked almost exclusively with a broad range of local partners (public and private). It did not leverage national labs--but that's not necessarily a criticism. Grantee took care of business pretty well without outside help...

Good mix of organization types (utility, educational, municipal, trade). Where does the private sector factor in?

The program describes its impact mostly with verbs like "facilitated," "supported," and "coordinated." It is reasonable to assume that the program did succeed in increasing awareness of the value of solar with a variety of local stakeholders.

City of Sebastopol
- IBEW
- Pacific Gas & Electric(PG&E)
- Sonoma County

County of Sonoma
- All nine cities in Sonoma County
- Climate Protection Campaign
- Sonoma State University
- Santa Rosa Junior College

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Simply purchasing additional PV installations should not be considered helping to realize the overall proposed goals of the RD&D program.

The future activity seems to consist primarily of the Clean Energy Advocate program, and the continuation of the AB 811 and SSC programs. I don't feel like I have enough information to evaluate the necessity of the CEA, but I look forward to seeing the results of the effort.

Seems like there could be some additional efforts going forward to build on or address additional market barriers.

Creating a "clean energy advocate." Will this role increase customer uptake?
Effectiveness of clean energy advocate will depend entirely on the strategic marketing plan.
Will such a plan be an integral part of grant proposal?

Project Strengths:

The project helps develop city-level institutional knowledge for managing the zoning, siting, and local use issues that arise with distributed renewable deployment.

Hugely ambitious--and largely delivered on the ambition. Kudos for that.

Organization of SSC is a great platform from which to deploy additional market transformation efforts.

PACE program - good success out of the gate.

Mapping and property assessment tool.
Outreach and education

Solar Sonoma seems to have been a useful policy advocate.

Excellent results with a complicated mix of stakeholders. Overcame a very unique challenge requiring the need to establish an entity prior to actually doing market transformation work.

Project Weaknesses:

The project appears to do little to facilitate knowledge transfer or systems standardization outside of the city proper.

SIP - can the recommendations of the SIP be translated into actions. Can SSC get a commitment from the cities county-wide to do some tangible actions towards addressing local barriers in each community?

Created Solar Sonoma County. Was this type of institution needed in a locale that had such an advanced solar program being pushed by other sources? Why was a "neutral party" needed? What did it add? Additional installations would come from CSI and PACE anyway, yes? Increasing collaboration seems to have been the key intent of this project. What did collaboration accomplish that wouldn't have been accomplished otherwise? Project involves a lot of "supported, contributed to, and facilitated" activities, but what was the unique product? Did program play an essential role in SCEIP?

Did SAC effort need to create and fund a new institution to achieve increased collaboration, or could they have saved time and money by adding a new job within the Santa Rosa city government structure?

Inclusion of industry seemed to be small, didn't stand out.

Recommendations for changes to the Project Scope:
The project scope should expand city external engagement through state regulatory reporting, industry standards forums engagement and federal energy regulatory comments. The scope should expand evaluation of regional wholesale energy market impacts of the distributed generation resources.

Would like to see a good way of measuring the effectiveness/necessity of the CEA. Also, perhaps herein lays an opportunity for national labs to provide assistance in creating tools for the CEA to evaluate different options?

Because of the unique scope of including cities countywide, they should investigate procurement aggregation strategies between multiple municipalities, similar to what is happening down in Silicon Valley/Santa Clara County. The thought here is to build on the SIP goals by developing mechanisms that address municipal purchasing barriers and transaction costs.

Have they considered an online permitting approach?

Has SSC considered some sort of competitive solar program between the 9 cities? Within the green power market, some communities have produced some amazing results by developing challenges between cities and neighborhoods within cities to procure green power on a competitive basis. Utilizing the natural competitive nature of people to drive program growth is proven using this approach. EPA has some experience in this area through their Green Power Communities efforts.

Set quantifiable goals for customer education and municipal strategic planning activities.

Perhaps the industry visibility isn't a weakness but would recommend this become more prevalent as SSC market accelerates
Look at ways to incorporate more specific tools for standards, best practices, etc. across the stakeholders as the market transforms (vis-a-vis IREC, NABCEP etc.)

Review: EERE 2010 Solar Program Review
Presentation Number: MT006_
Presentation Title: Solar San Francisco
Investigator: Broomhead, Cal

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Not evident that cost reduction objectives have been achieved.

Project has brought a lot of resources to bear in directly putting PV on roofs in SF. Solar map, solar incentives, solar on city buildings, workforce training, PACE financing, etc. While CA has a lot of existing incentive and regulatory infrastructure, program did a good job of addressing gaps and accelerating adoption.

Their objectives are in line with EERE solar program objectives.

This program has taken all the tools already available from a rich state program and used them to build an innovative and effective machine customized to the needs of its unique market. Focus on multi-tenant buildings and schools is an essential strategy for achieving significant future installations. It has used SAC funding to maximum advantage by linking every new capability--e.g., mapping--to a clear marketing objective.

Primarily focused on multi-family, high density community challenges, key relevance
Assisted neighborhood groups with selection and purchasing criteria
Did a good job of education and awareness

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Uncertain if the resources contributed thus far have resulted in transformations beyond certain social rather than specific technical aspects.

Project did a great job of identifying the high-ticket items. There are a lot of others, but given the resources available, did a great job.

One of the better presentations in terms of identifying and addressing barriers unique and not-so-unique to the SF marketplace. There is some question in my mind as to how effective some of these approaches have been in delivering on the objectives (Solar Founders Circle has not resulted in a single installation)

The project has been notably clever in choosing strategies that will have the highest impact in the unique San Francisco market.

Multi-tenant buildings - well designed, commercially promising model for scaling

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Could use better proposed metrics as part of the goal statement as well as measurements of performance achieved in-progress.

Hard to argue with the outcomes. There's a whole lot more solar coming online as a part of this project. The program established big goals, and put real resources behind it. Good job.

Lukewarm success with Solar Founders' Circle (no solar projects out of 93 assessments).

The project has focused on three clearly defined market segments—multi-family buildings, schools, and neighborhoods--and made progress in all three in an orderly way.

Program should consider demonstrating a tighter linkage between goals, projects, budget, and metrics.

A lot of activities going on but appeared very broad without clear integration across all the activities and directions.

However, upon questioning, direction and activities are clear. Recommended better overall metrics to track progress.

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
The presentation cites numerous collaborations.

Program has worked with other city agencies and outside stakeholders where appropriate.

Sounds as if the city is its own worst enemy. Dueling agencies result in conflicting results. (Moscone Center is an example)

The program has been extremely clever in using collaboration as a means, not an end, in the service of clearly defined strategic objectives. Using the power of the mayor to approach property owners was adept--93 of 1,500 rooftops is a great start.
Reasonable coordination in place with PG&E, 1 or 2 other organizations. Would like to see additional partners, financing, industry, NGOs called out.

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Review of the future activity could benefit from enhanced detail provided in the goals and objectives.

Focus on multi-unit tenant housing is appropriate given SF demographics; hope success is forthcoming. Target of financing for commercial solar PPAs is undefined--need more details.:

Continuing to build on progress in three distinct market segments should produce high-value contributions to the overall market transformation effort.

Future plans - too brief, no goals, unclear roadmap beyond "trying to provide more financing options"

Project Strengths:
Good local level engagement.

Success with solar on city-owned facilities is a strength. GoSolarSf and the PACE programs are big, bold and beneficial programs.

Lots of interesting, innovative ideas. (Some appear to result in little benefit, but sometimes you learn more through your failures. Would have liked to hear more about some of the ideas for fixes to these initiatives.)

San Francisco is at the opposite end of the pole from Salt Lake. The latter has made huge progress in laying the basic groundwork in a new market and the former demonstrates best moves in a highly developed market where the PV support infrastructure is firmly embedded.

Mayor's founders circle to create visibility, outreach, buy-in
SF Solar Map

GoSolarSF - excellent stimulation to local market
Multi-tenant solutions

Project Weaknesses:
Should address broader regional energy market and associated electric reliability standards issues.

Marketing plan for GreenFinanceSF is a bit inchoate. Recommend perhaps bringing in some professional assistance, as the program has a huge amount of potential to scale.

The solar access issues seem hugely problematic. The indemnification approach mentioned by the speaker is something they should pursue.

The presenter skipped through the slides too fast.

Presenter jumped around quite a lot so it was hard to get an objective read on goals, objectives, results. Random presenting style took a lot away from clarity of program results. Unfortunate....

Program didn't appear to have a tight linkage between goals, projects, budget, and metrics. A lot of activity going on but appeared very broad without a lot of consistent direction.

Unclear if SF was leading market transformation or following/responding to market developments as they might occur, such as 1BOG. Mayor Solar Founders Circle was a nice idea but it is still waiting for financing which possible could have been avoided with closer understanding of industry trends

Hard to see real results coming from a very vague "Future Plans" slide 23

Recommendations for changes to the Project Scope:

Address interaction of distributed resources with regional wholesale energy markets.

Given the new SHW incentive program, would like to see a more structured approach to supporting this promising new market (perhaps it exists, wasn't fully evident)

The community aggregation initiative needs to better identify what transaction costs are being addresses to result in savings for customers and determine how to best translate that information in the market transformation on a broader perspective.
Hope to see solid white papers on how to penetrate the multifamily market.

Take a hard look at what else can be done above and beyond multi-family, multi-tenant buildings. This is a clear priority for SF but what else?

Look at closer ties to market, industry conditions.

Highlight the metering/interconnection issues more, especially given the lack of clear solutions discussed at the end of the presentation. Recommend adding it to the future plans and developing solutions around it.

Review: EERE 2010 Solar Program Review
Presentation Number: MT007
Presentation Title: Program Title: Milwaukee Shines for a Sustainable Solar Economy
Investigator: Luecke, Andrea

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Presentation materials indicate advances in penetration based on procurement assistance, less clear on actual benefits for cost reduction other than through subsidy.

This project does not seem effective at exporting the City's experience to other venues.

WI is starting from a lower baseline than other states, and Milwaukee is challenged by a lack of supportive state policy (i.e. incentives). That said, program featured a robust number of activities addressing areas within its control.

They haven't set the bar very high in terms of goals, but their focus is good based on what they can do without adequate state policy support.

Is manufacturing a solar program objective? If so, I might increase this to a "Good" rating.

Tackling a relatively large-scale economic development initiative makes this project unique but perhaps less replicable, given the many elements that must be in place if a city is to contemplate a serious play to develop solar-related manufacturing. In addition to this venture, Milwaukee is doing a creditable job of laying the foundation for a core program where little solar infrastructure was in place.

Informational - Smart proactive approach to lay the groundwork
Economic - Address key cost questions in a region full of cost-conscious consumers
Well defined, clear program targets
Clear summary of status by activity (i.e. x% complete by y# activity)

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project presentation materials provide some assurances that goals are being met. Additional detailed information would be useful.

Focus on inspector training, installer training, and financing is appropriate as these are things under the direct control of award recipient.

They seem to be taking a very measured approach to the activities they are undertaking. I still feel as though they are not getting as much out of the grant compared to other cities, particularly given the amount of in-kind contributions from other organizations.

Adequate progress in laying the groundwork but more intense work required on basic customer awareness, given the current low level of development.

Basic customer education ("Solar Works"). Solar coach.
Increase by 100 PV, 50 SHW. "Small goals"--why?
Increase solar components mfrs; new product lines & firms. SHW Biz Council.
Increase SHW install techniques & designs
Increase decision-maker awareness.

Reasonable, balanced, focused goals, covers a combination of installations, technologies, PV/SHW, processes, and policies

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Score:
4 - Outstanding. Excellent progress toward objectives; suggests that barrier(s) will be overcome.
3 - Good. Significant progress toward objectives and overcoming one or more barriers.
2 - Fair. Modest progress in overcoming barriers; rate of progress has been slow.
1 - Poor. Little or no demonstrated progress towards objectives or any barriers.

Comments:
The presentation materials provide little assurance that these goals are being met effectively. Absent further detailed information this objective does not appear to have been achieved.

Many milestones have been achieved. Good work.

This program demonstrates the challenges of starting to move a seriously backward solar market, and it has made a good start at constructing a core program that can evolve over time.

Reasonable, balanced, focused goals, covers a combination of installations, technologies, PV/SHW, processes, and policies

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
This project seems marginally effective at exporting the City's experience to other venues. However, although boys'/girls' clubs may be non-profit organizations, their inclusion in the outreach population does not seem consistent with the RD&D goals of increasing industry and marketplace outreach.

Appears that partners across the community are engaged.
Real good mix of organizations.

It appears that considerably more work is required to build a strong partnership with institutions whose cooperation is essential to building a long-term program.

Well thought out mix of stakeholders, involved across technologies, cost issues, policy, utility, city/building

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Caution: simply purchasing additional PV installations should not be considered helping to realize the overall proposed goals of the RD&D program.

Future activities appear to be mostly bringing solar install projects to close, preparing the best practices SHW manual, and running the PACE program. I question whether the awardee is necessarily the best author for the SHW manual, but awardee is in best position to be the judge of that. Look forward to seeing future enhancement, development, and promotion of the PACE program.

As a project starting from scratch, this project should invest up-front time in creating a strategic plan that will yield a prioritized list of tasks.

Well thought out approach, ready for market activity

Project Strengths:

The project helps develop city-level institutional knowledge for managing the zoning, siting and local use issues that arise with distributed renewable deployment.

Achievable yet ambitious goals, great execution. Taxpayers got excellent value out of this award.

The organization of the PACE program is good.

The development of the Solar Hot H2O Biz Council committed to creating a viable manufacturing venture.

In-school competition that is unique because it goes beyond science-class concern with the basic technology to allow students to appreciate the rudiments of solar business, including assessing, financing, and installing on schools.

NREL financing options leveraged to increase community education for financing
Installer workshops, leveraging a "solar coach"
Inspector Workshops
Solar Manufacturing feasibility study
Number of installers 4->9, SHW 3->11
Demonstration projects
Collaboration stakeholders
Solar Schools swap

Project Weaknesses:

The project could do more to facilitate knowledge transfer or systems standardization outside of the city proper. The goals for the project seem too easily achieved, recommend more aggressive commitment.

The major weaknesses are in state policy, which can’t really be blamed on grantee.

The goals don't seem as aggressive as other presentations. Particularly in light of the amount of in-kind contributions added to the original grant, I didn't see the same amount of productive outcomes as in other cities.

There seems to be a big focus on jobs development (installer training/manufacturing). Is there any concern that the workforce training elements may not be deployable in the market if the city doesn't address some of the basic barriers to project development?

Apparent absence of a multiyear strategic plan for establishing and growing a core program.

For challenging climate/insolation market they've done a great job.

Recommendations for changes to the Project Scope:

The project scope should expand city external engagement through state regulatory reporting, industry standards forums, engagement, and federal energy regulatory comments. The scope should expand evaluation of regional wholesale energy market impacts of the distributed generation resources.

It's a judgment call, but perhaps grantee might get more involved in state policy, as that's where the primary barriers seem to be? Just raising the issue, not necessarily a recommendation. Overall, great job.

I'm wondering if there are some other barriers that they can address such as permitting, building codes, market analysis, aggregation procurement models across municipal governments etc...

More attention to basics as well as to economic development project.

Might want to think about closer ties to policy to leverage foundation in place

Go faster in 2010 and 2011 (spend allocated funds, move forward) to drive the demand side of policy

Milwaukee could be a shining example of managing both sides of the supply/demand equation

Review: EERE 2010 Solar Program Review
Presentation Number: MT008
Presentation Title: Seattle: The Emerald City Solar Initiative
Investigator: Irvine, Linda

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Presentation materials indicate advances in penetration based on procurement assistance, less clear on actual benefits for cost reduction other than through subsidy.

Nice breadth of project focus, from interconnection to education, to market opportunities. Love the run at community solar. I'm not sure, however, that the community solar model as developed has a trajectory/vision that grows a local solar industry and leads to a long-term, subsidy-free solar market. Maybe it does--hard to understand all the pieces of the program given the shortness of time and the dynamics of state tax policy--but in my opinion that should be a declared goal, and it was hard to tell from the presentation.

Focusing on community solar is a smart strategy for surmounting the first cost barrier for individually owned systems. Program has thought through the necessary steps in creating a self-perpetuating program with a revolving fund. If the program continues to refine this model, it will make a significant contribution to the national effort.

Well thought out relevance to other low insolation areas across the US

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project report did not make clear how the city's Interconnection Requirements (IR) will help ensure market consistency. The city is not the market. What steps is the city taking to provide outreach and network so its IR could be adopted elsewhere?

The commitment to research was apparent and welcome. Grantee demonstrated a very systematic approach to identifying problems and devising solutions. There are still state-wide policy gaps, but that's not necessarily something that is appropriate for grantee to address.

The research undertaken to identify the optimal model for community solar was good (by including customers).

Good move to establish a solar plan, address a basic technical problem (interconnection), public skepticism, and financing.
Removing bureaucratic barriers.
Move message out to potential customers
Immediate emphasis on community solar, which is a good strategy for a high renter population.

Excellent creativity in designing the community solar program financial mechanisms

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation could provide additional detailed information on the measurements for the stated goals. However, this project is commendable compared to the other solar cities for its explicit statement of what goals have been set and why they have been identified.

Grantee has made good progress in execution.

I think the innovative community solar approaches are great.

The program's strategic objective has been turned into a real project already.

Clear progress in technical, process, education, and community areas

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
This project seems somewhat effective at exporting the city's experience to other venues. The focus on outreach should include an evaluation of why or how the communication will increase industry penetration, standardization and utilization of renewables.

Collaboration with city gov’t, non-profit advocates, and the local utility is admirable.
Community solar project may have benefited by collaboration with outside entities such as IREC.

Good mix of organizations. Is there a university opportunity being missed?

Established, or at least leveraged, relationship with local utility. Understands potential impact of utility as an ally.

Excellent combination of stakeholders

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The city provides an acceptable co-investment of funds for the proposed future activity. Caution, however, that simply purchasing additional PV installations should not be considered helping to realize the overall proposed goals of the RD&D program.

Much of the work to date was to build a foundation--the real payoff will come with future implementation of community solar project. Ultimate success can't be judged until then.

Community solar program (design, agreements, marketing)

If this program proceeds along the current lines of action, it has the potential to be a national showcase for the community solar project.

Moving into the logical next step of financing options to provide to community outreach and accelerate

Project Strengths:
The project helps develop institutional knowledge for managing zoning, site, and local use issues that arise with distributed renewable deployment.

Grantee clearly put a lot of work into a systematic approach to identifying problems and devising solutions. I like the run at a novel community solar program--brave and ambitious. I also like the partnership with such an iconic venue as Pike's Place--slinging salmon sell solar...

Incorporate solar energy evaluation and deployment into city planning efforts
Interconnection barriers (action plan completed, Seattle code assessment, tech assistance)
Education and outreach (guide to installing systems, customer awareness workshops)
Ownership Model research

Clearly defined SMART goals and a coherent action plan. The program took a relatively new and potentially high-impact business model, community solar, and ran with it. This is a well-chosen priority.

Broad base of initial phase of market transformation work across key education, awareness, infrastructure, and financing option development

Project Weaknesses:

The project could do more to facilitate knowledge transfer or systems standardization outside of the city proper.

Would have liked to see more outside collaboration on the community solar program design (maybe that occurred, was not evident from presentation). It's a novel concept that many other entities are trying to pilot; legal and policy guidance from other venues might have been helpful.

None apparent.

Little mention of workforce needs, code/building official training

Recommendations for changes to the Project Scope:

The project scope should expand city external engagement through state regulatory reporting, industry standards forums engagement and federal energy regulatory comments. The scope should expand evaluation of regional wholesale energy market impacts of the distributed generation resources.

As much of the community solar implementation takes place in the future, imagine we'll see some changes and shifts as obstacles rear their head...

Think through the resource side of the equation to handle the possible increase in demand on building officials, inspectors, planning departments

Review: EERE 2010 Solar Program Review
Presentation Number: MT009
Presentation Title: Solar America Showcases, Government Solar Installation Program (GSIP)
Investigator: Stoltenberg, Blaise

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Helping a city negotiate a PPA is a poor general approach to increased market penetration.

The project contained such a wide range of different efforts that it is difficult to make specific and accurate assessments.

The federal government is the largest energy user in the country, and as such, it really makes sense to focus on changing its energy-buying practices. The activities performed seem to be a good initial step. Without seeing the how-to manual, difficult to assess. Trailblazing with targeted GSA buildings is similarly helpful, but until there's steel in the ground, it’s hard to judge.

For the SAC showcases, particularly liked the efforts that either trail blazed replicable projects or resulted in large solar acquisitions (Sequoia, Philly).

Financing
Local Regulations and Permitting
Solar Project Technical and Process Understanding

My only concern in this area is that the lessons learned from the very specialized project support activities will not be translated to others who can learn from these experiences.

This effort complements SAC by singling out replicable projects that break barriers by helping customer through first risk.

Essential program due to U.S. government being largest user of electricity in the United States

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project presentation seems more like a hodge-podge of disparate efforts than a coherently designed approach to achieve the RD&D goals.

It's hard to know without seeing the work product and seeing if the pilots are in fact replicated elsewhere. But the description seems to meet the criteria.

This is a very tailored approach, providing technical assistance to individual organizations.

The value of the Tiger teams to their clients is legendary and clearly is an important element to the success of the Solar Cities program.

Are the templates developed from each project transferable to other markets, sectors or organizations that have slightly different needs and challenges? Can a school in California use the same template as that developed for a school in D.C.?

Focused on the right education, site specific, regional variations and financing barriers resulting in a very broad and deep set of technical, process solutions

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Too much of the work reported in the presentation seems like one-off activity that is not part of a coherent overall strategy.

Again, hard to judge without seeing work products or long-term results. But from listening to the cities' presentations, it's clear that recipients of Tiger Teams' help were grateful.

On time, on budget with a high level of quality.

Challenging program in which to determine measurable goals, recommend taking a harder looking at developing MW / time roadmap

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Need to develop institutions to provide general knowledge transfer to any party on demand and spend less time and money on efforts that impart knowledge or information only on a case-by-case basis.

Projects worked with federal agencies and contractors. Not clear the extent to which the solar industry was involved (i.e., were solar developers consulted as to whether RFPs or contracting docs are good from a developer's perspective); same for other potential stakeholders such as builder associations, etc.

Probably the nature of their work, but didn't see the same type of coordination with secondary level partners outside of the client organization. I'm sure it exists, but I didn't get that from the presentation.

Program execution could not be possible without broad group of stakeholders: cities, utilities, national labs, regional variations, etc.

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Caution, however, that simply purchasing additional PV installations should not be considered helping to realize the overall proposed goals of the RD&D program. Some of the planned actions, such as generalized rooftop designs for institutional use, seem promising.

Seems that future plans focus on bringing current projects to close. Not clear what plans are in place for disseminating/promoting best practices guide, for example. Would really like to see an effort made to transfer lessons and work products in order to replicate successes in other venues.

Consider scalability of the program - where does it go from here? What are the new barriers at scale?

<u>Project Strengths:</u>
The project participant helps establish an infrastructure and technical capability which can

facilitate and assist renewable resource development.

Project responded to needs identified by the recipients of assistance. That's a good thing.

The financing and technical project support options are the most compelling.

Broad cross-section of showcase projects, representative samples of installations, technologies and applications

Project Weaknesses:
The project could do more to facilitate knowledge transfer or systems standardization outside of the participant's direct provision of assistance.

Hard to judge if project addressed biggest-bang-for-buck needs (not suggesting it doesn't, but that it is hard to know based on info available).

Workforce development vis-a-vis skills migration of existing facilities managers, site managers that would need a new set of skills

Recommendations for changes to the Project Scope:
The project scope should expand generalized methods of engagement to promote the expanded use of renewable resources. The participant should be mindful of potential avenues of engagement with state regulatory agencies, industry standards forums, and federal energy regulatory comments regarding wholesale energy rules.

Having gotten this far, would be good to see some effort expended putting final work products to active use.

Focus on encapsulating the many learning points from each project and make them available in order to reduce the learning curve for other projects.

Add workforce development
Add a MW / time roadmap with clear linkages to funding milestones, workforce readiness by the adoption site, location, facility mgmt. etc.
Policies or incentives to "pull" solar adoption forward to accelerate adoption

Review: EERE 2010 Solar Program Review
Presentation Number: MT010
Presentation Title: Market Transformation Analysis
Investigator: Friedman, Barry

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Nice generalized approach that covers a broad spectrum for applicability and outreach.

Wow. Spot on focus. Projects under discussion are all helpful tools for policymakers and advocates. Really critical stuff.

In general, the areas of focus for this presenter cover very important issues. To the extent that they can collect and transfer the information, or present it through tools and user resources this initiative seems well-aligned with DOE's objectives of cost reduction and market penetration.

This project compiles data that are essential to constructing an ever more persuasive case for large-scale transition to solar.

Inadequate information and analysis on jobs and economic impact
Current & future workforce needs
Rates/Policy gap analysis

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project presentation indicates this effort is engaged at the proper levels to promote widespread standardization and acceptance, as well as reduce market barriers.

Program seeks to provide answers to critical questions. The tools and data developed will help policymakers and advocates make the case for solar. That said, many of the tools could probably be sharpened a bit more. For example, rate design is a key solar market barrier (or enabler). Developing a database is helpful. But putting that data to work via analysts that intervene in rate cases is what's necessary to make real, positive change.

The approach wasn't explicitly detailed in the slides. Based on the presentation alone, I have no real concerns with the approach as it seems very measured in its focus on very specific market barriers.

Two major barriers are workforce and rate issues.

Workforce. How might solar help the overall economy through jobs and other economic impacts? What skill sets are you looking for, and how difficult is it to find them? Compare renewables and fossils on jobs creation and economic impact.

Open PV Mapping Project.

Utility Rates. Objectives of rate design analysis? Where are we now with utilities (e.g., decoupling, TOU, load mgmt)?

Very analytical approach to non-technical MT issues. This is challenging to do and do well. Good work!

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide a good description of the project goals. Further detailed information on measurements of achieving the objectives would be welcome.

So far, an enormous amount of progress towards stated goals and identified projects.

No specific comment. They seem to be making exceptional progress on all fronts.

Open PV project >70,000 is excellent, extensions to the application are timely and relevant

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Nice selection of external constituents. The list could be expanded however.

They've done a tremendous job soliciting input and engaging with knowledgeable stakeholders. This is a key strength.

The role of the utility in a variety of their projects seems particularly critical to their success. The speaker suggested that this relationship is particularly hard to cultivate. Given that the inputs for many of these projects rely on good utility rate data, etc., this would be an area to focus on collaboration and information transfer.
All key partners to drive requirements, tools, application, and improvements were clearly represented

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Nice solid plan, could include proposal of additional evaluative measures or performance metrics.

Much work remains to be done to complete identified projects and I'd like to see some of the project scope expanded. For example, not enough to just develop a rate database--there's another step necessary before positive change happens. Same with proposed 'value of dg to grid' effort. The value is really site-specific--would like to see many more individual efforts like the RW Beck study in APS territory.

The behavioral economics and value of DG to the grid projects are interesting and potentially very valuable.

Very intriguing future directions of potentially very high value in the marketing of PV to both customers and utilities. Integrating behavioral economics into customer segmentation and quantifying the value of PV to the grid are two essential tools for the future.

<u>Project Strengths:</u>
The project participant helps establish an infrastructure and technical capability which can facilitate and assist renewable resource development.

Project has done an excellent job of identifying key areas of analysis, and beginning to provide the necessary transparency and data.

Data collection in areas where gaps are significant (rate data, PV project costing etc.)

Utility rate database
Rate design roadmap
Clarity of aim and very sensible definition of what sorts of data will be most useful in the work of advancing the solar market.

Breadth of non-technical MT analysis: workforce, policy gap analysis, installations, utility rates, link between DESIRE/NREL/SAM/IMBY
Rate design roadmap is a great idea
Labor market analysis over 5 year SITN
PV JEDI1.0, JEDI 2.0
Solar Gateway / openei.org

Project Weaknesses:
This project could establish additional performance metrics to evaluate the success of its own initiatives and could expand the list of outreach participants to other industry institutions.

Unfortunately, as discussed elsewhere in this review, some of the efforts do not go quite far enough.

Self acknowledged need to integrate various tools (Jedi v.1-8)

Not a clear enough picture yet of how the data will be turned into useful information products and for what users.

Look upstream to additional parts of the value chain to broaden the workforce needs/readiness planning strategies. Specifically materials, R&D, test/certification technicians, software/systems/electrical engineers

Recommendations for changes to the Project Scope:
The project scope should expand the list of focused outreach parties and increase suggested performance metrics. For example, the participant should be mindful of potential avenues of engagement with state regulatory agencies, industry standards forums and federal energy regulatory commission regarding wholesale energy rules. The rate design activity could also incorporate avoided cost forecast information for QF puts to host utilities in areas that do not have PURPA exemption.

Barry should be given more money and more resources to take what he is doing and do much more of it. He's going down the right path--it's just that it's a really long road.

One additional item to add: policymakers need tools and assistance understanding the ratepayer impacts of policies (or lack of policies). Would very much like to see future efforts in this area. Emphasize modes of communicating these data in a way that has maximum impact on policy-makers, politicians, and utility executives. See Edward Tufte's *The Visual Presentation of Statistical Data* and his other works.

Scenario planning tool
What-if supply/demand capacity analysis for workforce resources
Supply chain costs are changing faster than the tool is updating
Allowances for new technologies, not just existing ones
Innovation will drive cost reduction, stimulate economic development and hiring

Review: EERE 2010 Solar Program Review
Presentation Number: MT011_
Presentation Title: Austin Solar City Partnership
Investigator: Libby, Leslie

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
This project seems limited in its fundamental technical advancements. The K-12 educational component has limited value to achieve market transformation.

Some great work. However, I would have like to have seen more of a focus on steel in the ground--the case for why the focus on schools/curriculum constitutes a priority market barrier still needs to be made, in my opinion.

Program did not address the more important and difficult barriers to greater market penetration.

Good focus on lack of understanding of distributed and large scale solar - generated momentum to increase goals to 100MW by 2020
Strong focus on lack of solar education - installed demo projects along with curriculum at schools

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Unclear from the presentation how potentially limiting issues such as permitting, zoning are mitigated. Also does not appear to address much from a fundamental technology level.

Project is really focused on education. That's important, but could also be improved by a larger commitment to deployment.

Of the resource studies that were funded under this grant, unclear how much impact it had on utility operations.
There is a lot more that AE could easily do to remove barriers and facilitate solar market transformation. AE caps its net metering program to 20 kW, for example--this precludes solar

on commercial customers' roofs. Incentive programs have recently grown, but still not a plan to lead the local market to grid parity. Financing? TX has PACE-authorizing language. Permitting? Not sure how much of an issue.

Why was lack of understanding as a market barrier addressed at the school level? To what extent will more public understanding of the fundamentals of solar translate into more systems installed on the grid? The same question could be asked about rooftop assessment--it's a necessary first step but if not turned into a market tool, it's academic only. Objective seems to have been to make people more aware that solar works, but was that fundamental understanding really necessary in a progressive city like Austin? Focusing on school curricula is a long-term payoff--was it the most strategic thing to do now?
It also seems that the program took on tasks that would be more properly done through the regular utility budget--i.e., transmission study. Is it the function of the SAC program to contribute to utilities?

Rooftop project due diligence was sound, correct technical resources leveraged to size kW, kWh production targets
Good work taking a combined solar, wind approach
6 projects totaling ___ kW?

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
It appears the project would benefit from more precisely stated goals with better metrics to measure success. The CREZ projects mentioned in the presentation were not developed or deployed in response to this DOE program and the presenter should use caution in creating the implication that the CREZ activity was due to (DOE) market transformation efforts.

Again, would like to have seen more of a focus on building a local solar market via the policies under utility control, and less education of schoolchildren. While there's nothing wrong with doing outreach to schools, effort did not address highest priority barriers.

It's hard to see how focusing on school curricula, as valuable as it is, is the best activity to support near-term market transformation and cost reduction.

How will rooftop assessment be used as a sales tool and as a policy instrument?

School goals were accomplished, progress clearly tracked towards goals

Austin City plan 100MW or 200MW by 2020?? Which one, how do you decide?

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
The project does include some engagement with educational areas.

Program worked with local solar orgs on delivering educational message--not clear the extent to which other stakeholders were offered a role in determining project activities.

Program did not establish new inter-organizational relationships that will accelerate market transformation.

Regular integration with community orgs like Kiwanis, Rotary etc. along with solar energy, EE, climate protection programs, school districts, consultants

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
It appears the project would benefit from more precisely defined and more ambitious goals, with a particular improved focus on technical and political barriers.

Future efforts seem to consist primarily of continuing educational process. Would like to see a greater effort on getting steel in the ground.

Scaling the solar curriculum statewide is great.
Is there more than a school focus? Additional barriers that the city could address?
It is not clear how to calculate how expanding general public education on solar will contribute to market transformation.

K-12 curriculum statewide, make it available nationwide possibly
outdoor learning center concepts

Project Strengths:
The project attempts to address training issues related to solar energy supply.

Grantee appears to have competently delivered on selected work products.

Solar Schools -> educational value is important.
Solar/Wind assessment
Community Outreach
Rooftop assessment

Increased interest within school establishment in integrating solar energy into the general curriculum. This result could have salubrious effect on the Austin solar market over time. In the marketing funnel, awareness precedes consideration and decision, so any activity designed to increase awareness can't hurt.

Leveraged existing solar school program to build partnerships
Interesting approach to do an upfront bulk purchase of modules, then contract for Installation -

Project Weaknesses:
The project presentation does not indicate much activity to remove barriers from the point of view of an entity wishing to install wind. In fact the education activity described appears to help create a demand, but does not appear to eliminate significant barriers to widespread adoption. This project should elevate its goals to attempt more ambitious results.

Project did not address the full suite of local market barriers: net metering, financing, and permitting. These are all things that other grantees took on; not clear why AE didn't, especially as it has more direct control than other grantees.

Lack of a clear strategic plan for connecting current activities with increased market penetration.

Leveraged TXSES 3rd party to reach out--- might be relying too much on others to do the work. Using TXSES as a distribution platform is good but be careful of relying on them too much to carry the DOE/SAC message. Some things can be lost in translation, ownership is a good thing.

Recommendations for changes to the Project Scope:
Add project activity to address permitting and zoning. Add a level of facilitation of power purchase agreement assistance for potential project participants.

Should be more focused on local market barriers.

Investigate metrics to assess the success and impact of the solar on schools program.

Are there other areas to address, such as permitting and integration of solar through local building codes? The future plans seem only focused on school activities.
How about support of a solar home/business/schools tour?

Some cities have had success in challenging specific neighborhoods to see which could install the most solar or meet some sort of goal. The winning neighborhood would receive a free solar PV array on a library or other public facility located in their neighborhood. Leveraging the competitive spirit within an activist community like Austin might be an interesting way to increase awareness and penetration.

More emphasis on measuring the effect of in-school and general public education on local perception of and beliefs about solar.

[A] Accelerate spending on marketing, outreach, ensuring the solar wedge into the wind mindset is very important from a transformation standpoint. Perhaps increase planned spending 20-30% above and beyond current goals
[B] As the program potential might increase from kW--MW need to be careful about hedging against price changes, risk, options. This can and has been done elsewhere, so it’s important to think through for future purchases.

Review: EERE 2010 Solar Program Review
Presentation Number: MT012
Presentation Title: The City of New York Solar City Strategic Partnership
Investigator: Case, Tria

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Nice approach that covers a broad applicability and outreach.

Focus on addressing Con-Ed concerns with network grid was key to opening up market. Permitting clearly a priority. Program appropriately focused on efforts they had control over.

Technical network grid issues
Financial
Administrative

A major accomplishment of this project was the recruitment of ConEd as an ally in the next phase of solar roll-out, such that placement of privately-owned systems will be coordinated with ConEd's resource acquisition plan. This is replicable, as are the solutions to PV on the networked grid problem.

Technical interconnection to network grid
Financial - addressed high relative costs in NYC via property tax abatement
Administrative/difficult permitting process

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The written materials give the impression that this level of engagement is at the retail marketing program level rather than the technical engineering or operations level. But the verbal presentation of the utility-level engagement indicates good progress was made between the city and the local utility.

Project was sharply focused on unique local market barriers, and has delivered some results that will have national impact (e.g. on networked grid). Some activities need a little more explaining before one can judge their benefits (empowerment zones, solar potential study).

Very methodical approach and diverse set of goals ...barriers, planning, education, financing, specialty applications, workforce, realistic goals.

There was clear identification of major market barriers, in part because the city had created a solar roadmap in 2005, in time for Bloomberg's NYC 2030 plan. SAC project was therefore embedded in long-term sustainability plan. The program ambitiously addressed a broad range of barriers--technical, financial, administrative--the combined solutions which will dramatically increase market penetration in a major urban center.

Technical interconnection to network grid - correct primary focus on the critical technical limitations to adoption. Strong industry relationships along with utilities.
Financial - high relative costs in NYC
Administrative/difficult permitting process

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide a fair description of the project goals. Further detailed information on measurements of achieving the objectives would be welcome.

Clearly a lot of progress has been made by Con Ed in implementing solutions to interconnection - kudos on a major job well done. It's not clear from the presentation exactly how much progress had been made on some of the other issues--is the permitting process actually fully streamlined, or just identified by parties as a problem?

Impressive progress given the complexity that NYC has as a very diverse and complicated landscape in deploying solar.

Impressive progress on each of the major barriers identified at the outset, with a strong emphasis on turning strategies into action.

Streamlined interconnection through interaction between SAC players.
Increased education of customers and installers.
Report on solar as emergency management application.
ConEd developed interest in its own solar program as result of collaboration.

Creation of solar advisory board and NYC Solar Summit (300 attendees).
Creation of solar empowerment zones.
ConEd proposed a 100-day interconnection and permitting process; ConEd had been a major barrier.
Green energy training at CUNY. 150 new installers.
Increased limit on net metering.
Property tax abatement shortened payback to 3-4 years.
Gigantic increase in solar goals (from 8 to 45 MW).

ConEdison has now filed to take ownership of NYC solar program
NYC Solar Summit has seen 50% increase YoY

Regular workshops, collaborations around the table to streamline permitting in conjunction with fire codes

Workforce development metrics are very sound

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
This project seems marginally effective at exporting the City's experience to other venues that would result in an expansion of the renewable market.

Program leveraged NREL's technical capacity to fullest extent. Bringing NREL to table for discussions with ConEd on networked grid was clearly what the doctor ordered.

What role could the NYC investment community, real estate management companies, corporate community, transit agencies play in this effort?

The project's choice of major players resulted in rapid and significant decisions and actions. SAC's organizing impact caused major stakeholders to sign on. Collaboration among critically important participants was key to success.

Strong common theme of collaboration to bring the right stakeholders around the table for any given technical or process issue
CUNY, Economic Development, Mayor's office + Con Edison
DCAS? - leveraging this organization to gain access to city buildings, MTA etc.

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The plans are not presented clearly in the presentation to indicate which are reliant on the RD&D program and which would proceed in any event. Further the future plans referenced in the presentation include a lot of topical discussion but little in terms of established goals and associated measurements.

Still need some clarity on the content/relevance of future work--for example, what are the 'empowerment zones' and what exactly will they do? Is there money attached to them, or is it an (useful) exercise in identifying and publicizing congested areas?

Will assess amount of solar that can be leveraged in each solar zone. Integrate with ConEd's peak reduction and grid vulnerability reduction programs.

NYC's FY2011 plans are the most well-defined, milestone-driven example across SACs. All other cities should have this same rigor of thought

Project Strengths:
The project helps develop city-level institutional knowledge for managing the zoning, siting, and local use issues that arise with distributed renewable deployment.

Top-notch work of national import on the collaboration with ConEd on the networked grid. A lot of excellent effort in a very challenging environment.

Great support by the Mayor's office and city commitment to solar installations.
Solar Empowerment Zone
Stakeholder involvement from the start
Online application/interconnection tracker systems
Permitting, permitting, permitting, permitting...
The quantification of the impacts of the net metering and property tax abatement to reduce payback by 4-5 years is very compelling.

Very impressive progress on several of the key barriers to increased solar deployment. This project made a distinctive and replicable contribution to achieving SETP goals.

ConEd is driving '100days' of solar, single application to be 100 days vs. 6-12 months!!! Still long by some standards but for NYC this is an excellent development

Focus of frustration has now shifted to Dept of Buildings, moving in the right direction of next key issue

Very strong technical interconnection risk/concern was overcome

ConEd online application tracker

Project Weaknesses:

The project could do more to facilitate knowledge transfer or systems standardization outside of the city proper.

Unclear how much of the accomplishments listed were achieved by project award vs. others. Would have liked to see more effort put into getting NY city agencies to buy solar for own facilities--they've got the land/roof and the load. While the Empowerment Zones seem like a good step forward in terms of identifying areas where DG is especially valuable, the missing piece is monetization of this benefit.

None to speak of.

Could have highlighted some of the technical challenges present for high density area, limitations, shading, roof challenges, technology solution needs. Building stock is old, therefore it represents a unique set of challenges both in NYC and the boroughs. Recognize the opportunity to leverage to other higher density cities across the US

Recommendations for changes to the Project Scope:

The scope document could be revised from a list of topical concepts to some specific planned achievements and measurements for associated success. In addition the scope document could be clearer on the details of requested funding and cost sharing offers.

Effort to get solar on city-owned facilities, perhaps some more market education.

Given NYC unique position as home to the nation/world's financial center, could the city take a look at programs that engage the investment community to better understand the benefits of solar as an investment. What role could the NYC real estate management and corporate communities, or transit agencies play in this effort? Perhaps think about broadening the effort to facilitate stakeholder involvement from these groups.

Can the online application tracker system be open source for other cities to use? Also, would NYC consider analyzing the cost benefit savings generated from an e-permitting platform? Is there a business case for permitting departments to invest in this type of system as a way to make their shrinking budgets go farther? What is the upfront investment requirement for this? Learning points, hurdles and challenges?

If funding is available, begin working on how to reach the multifamily market.

Increase the thinking of activities around characterization of the building stock because of the unique urban environment.

Review: EERE 2010 Solar Program Review
Presentation Number: MT013
Presentation Title: Linking San Jose's Green Vision and Solar Cities
Investigator: Tucker, Mary

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
The mapping initiative is good. The effort to reduce permit/inspection time is also commendable.

California has a well-developed regulatory infrastructure; project does a good job of addressing the elements that are under the city's control (i.e. PACE, permitting, putting solar on municipal facilities, group purchase for city employees).

Cost barriers
Permitting barriers
Regulatory and legal barriers
Workforce barriers

Can/should DOE funds be used to influence state legislative process?

This is a highly evolved market due to city's long history of solar advocacy (15 MW within city is major). From SAC point of view, this is a curse as well as a blessing because it is difficult to distinguish what happened as a result of the MT project and what would have happened anyway. The SAC funding paid for staff, so that ongoing work could accelerate. However, any contribution to San Jose's program cannot help but further national market transformation goals.

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
This project does not seem effective at exporting the city's experience to other venues.

The project presentation materials provide some assurances that goals are being met. Additional detailed information would be useful.

Project focus has a good combination of ambition and reality. The goals are challenging, and they have made real progress towards them.

The work on the City RFP and PPA issues are great. Should be very useful to other cities. We receive requests for these types of examples all the time.

Barriers were identified ahead of time through stakeholder meetings and the city's Green Vision plan. Program has chosen to focus on financial barriers, permitting, and link to city's Green Vision.

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Based on the presentation the project has some good, specific initiatives to help address specific cases. Could use more evidence that market expansion is supported through specific deliverables. Metrics like "% increase in awareness" are not strong measures of market transformation.

Many of the projects are still in process, and final outcome not yet determined. That said, much progress seems to have been made. Really looking forward to seeing outcome of municipal PPA.

Some great accomplishments in areas that are clear barriers. Permitting, fire safety, community outreach etc.

The funding enabled community activities that would otherwise not have happened, including streamlined permitting. The permitting model alone, if adopted by other municipalities, would be worth the price of admission.

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Nice level of engagement with external organizations for outreach, could be somewhat expanded.

Project has great collaboration with partners across the spectrum, from internal city stakeholders to industry participants.

Excellent group.
The city employee group purchasing option is an interesting approach and collaborative angle.

This city has a long history of effective collaboration.

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Does not seem applicable based on the project presentation materials.

Future projects are all about following projects that are currently underway all the way through to completion. It's a full plate, and very much supports continued funding.

Online permitting - excellent idea.

The city has a well-developed strategy for advancing the market.

Project Strengths:
The project helps develop institutional knowledge for managing zoning, site and local use issues that arise with distributed renewable deployment. Kudos to the city for its demonstrated high level of existing solar resource.

Project very much focused on getting steel in the ground: municipal PPA, permitting effort, PACE, etc.--these are all big ticket items. Project has high ambitions.

Permitting one-stop center -> future online permitting is brilliant!
PACE program
Fire safety training DVD and training
Solar School workshops / Train the trainer
Group purchasing

Augmented funding for an already developed and highly effective program with clear directives.

Project Weaknesses:
The project could do more to facilitate broad general knowledge transfer (rather than specific initiative details). The project could expand on efforts at systems standardization outside of the

city proper.
None really come to mind.

Difficult to distinguish effect of SAC funding. Use mainly for staff support to extend initiatives already underway.

Recommendations for changes to the Project Scope:
The scope document could be revised from a list of topical concepts to some specific planned achievements and measurements for associated success. In addition the scope document could be clearer on the details of requested future funding and cost sharing offers.

I don't know if this is a change or if it is already planned for, but there will need to be a massive education/outreach effort post-PACE launch. Resources/plans should be prepared.

Measure and document the most effective neighborhood and business association outreach efforts. What is the effectiveness of these programs and how can they be replicated in other cities?

Online Permitting: The city should make an effort to document this process (lessons learned, challenges) so other municipalities can follow suit. In particular, identify the cost savings implications of this approach.

None.

Review: EERE 2010 Solar Program Review
Presentation Number: MT015
Presentation Title: Project Title: Tucson Solar Initiative
Investigator: Plenk, Bruce

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
This project has developed numerous fairly unique applications and expansions of renewable deployments. Nice work.

Arizona is a key solar state; Tucson's efforts are well targeted to address gaps. Putting solar on public buildings is a high-benefit activity. Work on permitting processes and 'solar ready' solar ordinance are appropriate activities for enhancing local solar industry.

Overall a very strong approach to addressing barriers.

This is a solid forward-looking project in a rich solar resource area that has used SAC funding to intensify outreach. Leading an effort to attain CREBs money is an important demonstration of how available financing can enable projects. Project made reasonable progress on a number of fronts.

Good focus on cost reduction, education/examples
Numerous elements underway to reduce financial hurdles, PPAs, bonds, tax credits, private parties

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Nice credible performance demonstrated.

There are many advocates working on state-level pro-solar policy; program has appropriate focus on issues over which it has control. I really like the focus on getting solar installed on city facilities; less convinced that the publicizing/outreach activities are high priority or high impact.

They seem to be hitting on many of the major issues that would be also relevant to other cities. Awareness, permitting, demonstration, trainings, public land reuse, solar developer map, solar ready homes etc...

Solar Tourist Map....how effective is it? Number distributed each year?
Like the solar bus stops, which are paid for by advertising. How much of the ad space is used for increasing awareness about solar?

The project was clear on which market barriers were most significant. However, insofar as residential financing was identified as one of the key barriers, it is unclear why there was not more intense focus on this issue.

Integrated City plan, information, combined with driving tours & outreach
Financing - CREBS, PPAs, other sources such as Bonds, tax credit swaps, very broad and creative

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Could have provided more specific measurements in the project presentation materials, but the list of accomplishments appears quite credible.

CREBS approach is appropriate as state does not currently explicitly allow PPAs. Looking forward to seeing those projects brought to completion. Permit cost reductions are also quite helpful. Project has really set ambitious goals and made great progress.

Doing a great job in the areas of awareness, permitting, demonstration, trainings, public land reuse, solar developer map, solar ready homes etc...

Although the project has clearly framed plans, it seems to have produced fewer concrete results to date than many of the other SAC programs.

5% requirement in all new city buildings is a strong statement
1MW new PV installed last year, now moving into parking shade structures
Information/Education - multi-faceted approach across 7 elements

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Could expand coordination and outreach to additional institutions.

Working well with NREL and AZRISE and city agencies. Good job.

Good mix of organizations.

The program seems heavy on government collaborators (state, county, federal) with no clear strategic objectives as to what these partnerships will produce.

Nice work

Governmental - County, State, Congressional Rep
Non-Profits - 3 different NGOs
Others - Utility, Community College

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The city provides an acceptable co-investment of funds for the proposed future activity. Caution, however, that simply purchasing additional PV installations should not be considered helping to realize the overall proposed goals of the RD&D program.

Phase 2 is really about taking the momentum to its logical end--completing the mission, as it were.

During Q&A, program identified two most important next steps:

1. On the private side, more residential financing
2. On public side, more money to buy down CREBs.

Future tasks included continued work on these fronts but there was little detail on just what next steps and expected outcomes would be.

Open Solar One Stop - location & website, on-the-spot information
Integrate into green building planning, learning from 5% requirement guidelines
Standard PPA for Arizona cities

Project Strengths:
The project participant helps establish an infrastructure and technical capability which can facilitate and assist renewable resource development.

Grantee has done some creative work getting solar on municipal buildings in a state that does not allow PPAs. Kudos. Grantee also did great work enhancing the local environment for solar.

Good mix of collaborators from government, non-profits, education, and utility.

Good use of municipal policy requirements (5%) to drive installations.
Shade structures and their benefit towards urban heat islands.
Permitting resources.

Clear commitment to advancing the market.

Collaborations are broad and deep. Nice work
High visibility installations combined with solar tourist map - Brilliant!
Linked solar into community marketing, tourism, outreach, showcase it
CREBS building project completed
Solar Ready homes ordinances

Project Weaknesses:
This project could establish additional performance metrics to evaluate the success of its own initiatives and could expand the list of outreach participants to other industry institutions.

There are still additional barriers to knock down--financing for one. Understand that resources are limited and choices have to be made.

Insufficiently detailed task schedule.

No mention of workforce?

Recommendations for changes to the Project Scope:
The project scope should expand the list of focused outreach parties and increase suggested performance metrics. For example, the participant should be mindful of potential avenues of engagement with state regulatory agencies, industry standards forums and federal energy regulatory comments regarding wholesale energy rules.

Think about the metrics you will use to measure the success or effectiveness of your Solar One Stop. Think about documenting your success, failures, costs and impacts so that other cities can replicate this approach, if it proves useful.

Develop a concrete strategic plan.

Get industry NGOs involved in the standard PPA, training strategies for designers/engineers

Get industry NGOs involved in the project performance reporting and side-by-side comparisons

Program is accelerating so might want to start looking into performance of systems installed to date? Possible effects of high heat, high temperature, reliability/performance

Review: EERE 2010 Solar Program Review
Presentation Number: MT016_
Presentation Title: "Minneapolis Saint Paul Solar America Cities Program"
Investigator: Hunt, Anne

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
A lot of excellent focus on policy--key solutions to market transformation. Still some key holes to fill--net metering, for example. Project focused more than most on establishing the right policy foundation at the state level.

Pro investment solar policy development
Public awareness
Workforce

This is another project--like Utah's--that exemplifies the power of the SAC program to enable rapid establishment of a core solar incentive program where none existed before. Project demonstrates that foundation-building process can be dramatically accelerated.

Appropriate focus on relevant areas
-Policies that impede investment in Solar Technologies
-High cost of solar
-Lack of public awareness
-Low visibility
-Lack of trained solar professionals

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Very much so. Project covers an impressive swath of market-enhancing activities, from solarizing public buildings to workforce training to state policy barriers to upcoming financing programs.

The program identified the key elements of a basic program and successfully developed them.

Sound program overall. Well-aligned in all areas

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Excellent progress to date. Real steel in ground.

The program made impressive progress in creating the bulwark for a long-term solar development effort.

Collaborated in establishment of state incentive program.
Laid groundwork for expansion of net metering
Improved local codes and standards
Organized successful effort to establish a PACE program
Brought leased system to city
Convened marketing initiative aimed at general public
Got a muni system up, did site assessments.
Developed installer training with IBEW.

$30M solar incentive program is a significant accomplishment
Xcel Energy Solar Rewards Program
Communication/Marketing Workgroup --> Branding Call to Action is creative
600kW install, 30 potential public sites
Even moving into EV charging stations, good work!
Training addressed well

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Outstanding work leveraging NREL to prepare supportive analysis. Worked well with wide range of stakeholders: utilities, city agencies, etc.

Great line-up of collaborators.

Recruited the right institutional players to create a robust solar industry.

Nice start. Look being the obvious utility and business relationships to increase the program potential for leverage.

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The district solar project is quite ambitious and to my knowledge unique in the country. That said, there was not enough information presented on it to give a full evaluation.

Net metering policy
Increase capacity installations across sectors

Program is taking an orderly process of establishing the basic platform for a customer-sited program and then turning those achievements into action by increasing installations and refining net metering policy.

Project Strengths:
Project focused on a wide range of key barriers--the scope is its greatest strength.

Developed report to lay ground work for good solar policy
Solar Rewards Program
PACE financing legislation
Education - workgroups, train the trainer, solar installers, college programs

Constructed the basic groundwork necessary to expand installations, including incentives, improved net metering, and financing, as well as establishing ties with the relevant utilities.

Sound integrated approach to education, workforce, policy, utilities

Project Weaknesses:
Nothing outstanding.

Think bigger, bolder, faster integration of PV and Thermal programs given the type of combined solutions (electricity, hot water) needed in M-SP area.

Recommendations for changes to the Project Scope:
Really looking forward to hearing more about the district heating project. Didn't get enough

information from the presentation to judge.

Workforce development to support PV and thermal project pipeline.

Increase focus on effective marketing by drawing on what has worked elsewhere.

Look into how to balance the planning ahead for downstream challenges than can arise from running parallel PV and SHW programs, such as: technical skills, building officials, inspectors, workforce, etc.

Review: EERE 2010 Solar Program Review
Presentation Number: MT017
Presentation Title: SmartSolar Program: A Partnership to Serve the East Bay
Investigator: DeSnoo, Neal

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
This project appears to depend too much on simply procuring additional resources and co-marketing non-RD&D activity to be constructed within the City. That activity does little to further the overall RD&D objectives.

Berkeley's baseline includes robust state policy and an engaged citizenry. Really interesting how program addressed a problem that other cities don't have: too many choices and too many options. Appreciated Berkeley's upfront engagement of customers in choosing to invest in a Solar Advisor.

Cost
Education/Confidence
RE vs. EE

– Improves financial performance by integrating with energy efficiency
– Leverages energy efficiency rebates and program marketing for deep savings
– Integrates with PACE financing

Berkeley project demonstrates a winning and replicable strategy for getting beyond the early adopter market and expanding the available market through blending with EE, offering third party consultation, and steering consumer toward PACE financing, all of which will make a dramatic improvement in each home's energy performance. This could also help spawn a home performance improvement industry that will move solar ahead much faster than a stand-alone model.

Model for City Climate action plans
Leaders for innovative policy/processes
Clear leader with PACE model

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
This project does not seem effective at exporting the city's experience to other venues. The standardized bid form described during the verbal part of the presentation seemed like a good candidate for sharing "best practices."
Based on the project presentation, could increase the strength of engagement with the local utility. The materials give the impression this level of engagement is at the retail marketing program level rather than the technical engineering or operations level.

Yes. With a well-developed local industry, and ambitious city goals, trying to develop synergies between solar and EE really makes sense.

The city uses its partnerships (UCB) well to identify and work on very specific barriers through innovative approaches.

Very clear on barriers that need to be addressed in an already developed market: first cost, consumer confidence, interplay with energy efficiency in order to produce a more affordable blended package. The project undertook a variety of consumer-friendly activities based on a clear vision of what the next phase of residential marketing and delivery might look like.

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide little assurance that these goals are being met effectively. Absent further detailed information, this objective does not appear to have been achieved.

Project is still being implemented and results are still coming in, but staff has been hired and is performing as asked.

Clear policy leadership development
Applaud them for looking at mechanisms to integrate EE and RE into a doable model

The program has successfully produced a neutral third party solar ombudsman and discovered the ideal communication channel for a city like Berkeley--i.e., the street. Helping consumers choose the right mix of solar PV and thermal and energy efficiency will require face-to-face interaction if the intent is to move more people through the marketing funnel that leads from awareness to action. The Berkeley model could be highly replicable in urban areas.

3% of installed base being covered
10 solar vendors
Founders of PACE model program
Leader for permitting fees
Transparent reporting visibility

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
This project does not seem effective at exporting the city's experience to other venues.

Engagement with utility has been lucrative and productive. Key partnerships with university. Love the expansion to neighboring communities.

Good list of collaborative partners and effort to leverage their expertise to develop quality approaches.

Many essential partnerships had already been achieved, but especially notable in this project was the emphasis on grassroots groups as neutral advisors on the solar value proposition and on working with the industry itself to improve customer protection, as in developing standard bid forms.

Integrated Community Services, Business Plan partners, solar map providers, outreach & marketing, City, PG&E, UCBerkeley

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The future plans reported in the presentation are vague and do not convey a sense that continued future funding would result in efficient achievement of the program goals.

Love the regional expansion of the SmartSolar program.

Program has defined a very clear and actionable path toward integrating solar into an comprehensive cost-effective home performance improvement product that could have a dramatic effect on reducing GHG if implemented widely.

SmartSolar opportunities to scale along with other programs

Project Strengths:

The project helps develop city-level institutional knowledge for managing the zoning, siting and local use issues that arise with distributed renewable deployment.

Project direction was formed by community input--really liked the upfront survey.

Permitting fees
Commercial permit fee hourly basis payment schedule
Solar map
Consumer outreach and education
PACE model

This project is a sterling example of how game-changing leadership can result from federal funding support.

Forward thinking, integration with EE to make the package more cost effective

Project Weaknesses:

The project could do more to facilitate knowledge transfer or systems standardization outside of the city proper.

Much effort was put into building local solar market. Other cities have also included an effort to solarize city buildings--perhaps this could be included in Berkeley as well.

Project needs to put increased attention on quantifying the impact of its marketing model compared to alternative marketing strategies.

Unclear how much industry involvement and workforce training/readiness is going on to keep "PACE" with the leadership at the city/policy level

Recommendations for changes to the Project Scope:
The project scope should expand the list of focused outreach parties and increase suggested performance metrics. For example, the participant should be mindful of potential avenues of engagement with state regulatory agencies, industry standards forums, and federal energy regulatory comments regarding wholesale energy rules.

I believe that it will be a challenge to have the local solar industry embrace the push to combine solar with EE. Perhaps more can be done to understand and address their concerns (as well as make the case, e.g., point out that solar incentives are diminishing to zero and EE will help make their future solar installs seem more economic).

This might seem like an odd comment (and please excuse the micromanaging), but the logo for the public banner for the smart solar program could be improved. In the pictures in the PPT, it shows a large acronym of CESC. That might be relevant to the person behind the booth, but is not very good advertising to bring people in.

The project needs to produce an affordable and trusted consumer advisory service that can steer each consumer toward the most cost-effective mix of investments.

Start to take a broader approach to including various aspects of industry leadership and workforce training/readiness elements to keep "PACE" with the leadership at the city/policy level

Review: EERE 2010 Solar Program Review
Presentation Number: MT018
Presentation Title: Midwest Solar City Model (MadiSUN)
Investigator: Hoffman, Jeanne

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Presentation materials indicate advances in penetration based on procurement assistance, less clear on actual benefits for cost reduction other than through subsidy.

Project focused on city policy (permitting) not state policy--like the approach to dealing with things under their control. There are other barriers that remain unaddressed (i.e. financing)--other cities took things like these on (understanding that there are limited bandwidth and funds, perhaps this was not feasible).

Consumer knowledge
Institutional knowledge
Lack of supporting policy

Doubled market penetration

Passable job of marshaling and focusing available resources and incentives, leading by example, and addressing a major market barrier - consumers' inability to assess the opportunity and work through the decision process. The result is an appreciable uptick in solar installations.

Lack of knowledge and time on part of prospective system owners
Institutional gatekeepers
Inadequate city policies & processes for solar energy
City staff awareness

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project presentation materials provide some assurances that goals are being met. Additional detailed information would be useful.

Project focus was more on providing information and education and changing consumer behavior rather than breaking down institutional barriers (though there is some of the latter).

Good foundation in addressing barriers.

Focus on consumer ignorance and weak city policies. Took a next logical step in improving marketing: the creation of a "trustworthy, unbiased solar agent." Leveraged its role as a provider of services to create greater credibility for solar.

Training focus with city, residents, on the policies, economics, etc.
Show leadership through City installations
Combined approach to businesses and residences with respect to Marketing & Outreach

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide a good description of the project goals. Further detailed information on measurements of achieving the objectives would be welcome.

Execution appears to be good.

Great job.

Annual installation rate has multiplied due no doubt to the impact of SAC-funded activities.
Created independent solar advisor for Madison residents.
20 solar systems installed on city facilities.
Improved zoning and ordinances. One-stop permitting, including better information to installers on what's expected.
Marketed financing opportunities.

Solid work in all areas
- 12 fire station installations
- launched education portal website
- PV and SHW installations combined are 16x on annually adjusted basis!

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
This project seems marginally effective at exporting the City's experience to other venues that would result in an expansion of the renewable market.

Interesting collaboration with Frieburg on the business center. Appears that some city agencies are still somewhat resistant.

Good mix of collaborators.

Project did not focus on barrier busters that required new forms of collaboration. Additional partnerships might give rise to new ideas and process improvements.
Appropriate level of city, utility M&O outreach, community stakeholders, businesses

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The future plans reported in the presentation are vague and do not convey a sense that continued future funding would result in efficient achievement of the program goals.

future plans mostly build on current activities. Not clear that all barriers have been addressed. Really like the new effort on community solar.

Community solar model
Marketing to businesses
Solar business center

Future activities are, sensibly, doing more of what has been shown to work in phase 1, especially the solar agent model, which will also be extended to businesses. Adding volume buys through community aggregation should further stimulate the residential market.

Project Strengths:

The project helps develop institutional knowledge for managing zoning, site and local use issues that arise with distributed renewable deployment.

Project took on a limited number of activities and saw them through.

Solar agent to assist businesses and residents overcome major barriers
Updating zoning code - expanded solar market
Training for city staff (institutional gatekeepers)
Solar mapping
Solar homes tour - there is no better way to sell solar than have people who own and live with it talk about it. All solar cities should be utilizing this as an inexpensive way to increase awareness. Great way to engage with the installer community as they can offer their best projects to be part of the tour.

Focus on leveraging the full potential of one key barrier buster, the provision of trustworthy consulting to the receptive consumer.

Sound approach to education, awareness, marketing/outreach, evolution of city processes
Establish the City in a leadership position with respect to solar in the region
Plans to establish a solar business center modeled after Frieburg Germany sister city relationship
Solar Agents made a big impact

Project Weaknesses:

The project could do more to facilitate knowledge transfer or systems standardization outside of the city proper. Additional nit: The dollar funding amounts reported on slide 13 seem to be in error.

Most of the activities concerned education, rather than structural barriers.

Slow in exploring additional cost reducing models for making solar available to residential customers.

Workforce development, or maybe too early since just getting off the ground?
What about more aggressive policy work with Madison Gas & Electric?

Recommendations for changes to the Project Scope:
The project scope should expand the list of focused outreach parties and increase the detail of suggested performance metrics. For example, the participant should be mindful of potential avenues of engagement with state regulatory agencies, industry standards forums and the federal energy regulatory commission regarding wholesale energy rules.

Some future changes were discussed during the back-and-forth: future PACE program, bulk purchase for communities, etc. Look forward to seeing the community solar program.

Great list of future projects.

Set some timetables for instituting community aggregation and community-based systems.

Explore broad collaboration as the State/SEIA/SEPA level to increase the policy levers in order to take advantage of the work to date
Expand with integrated EE and SHW efforts. Share notes with other Solar America Cities like M-SP that share similar regional characteristics, utility costs, etc.
Shift away from some of the softer aspects of market transformation (education, awareness, processes, etc.) and more to the finance/economics/value proposition and how to make the case to influence policy changes favorable to solar

Review: EERE 2010 Solar Program Review
Presentation Number: MT019
Presentation Title: Solar America Cities – NREL Support
Investigator: Coughlin, Jason

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Nice generalized approach that covers a broad spectrum for applicability and outreach.

This may be the most important piece of the whole program. Real-time response to situation/location-specific problems seems to have been critical. NREL's help was often cited by cities as providing key support at key turning points.

It is clear that without the technical support offered here, many of the cities wouldn't know how to proceed. The cities' presentations actually provide the real validation of the worth of this work.

The cross-pollination function performed by this program is essential to reducing the information cost of designing and implementing programs.

Competitiveness with retail electricity prices
Procedural issues with codes, permits, and solar access
Lack of qualified installers
Solar energy awareness is low
Utility relationships

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project presentation indicates this effort is engaged at the proper levels to promote widespread standardization and acceptance, as well as reduce market barriers.

A strength of the approach is that they did not choose the issues to work on--each SAC brought local issues to them. Kudos for recognizing the strength of flexibility--this work is not cookiecutter.

Very useful technical support. Tiger Teams have great reputations among Solar Cities. They clearly offer critical support in addressing barriers.

The choice of topics for technical analysis signifies a deep understanding of market barriers encountered by SAC programs and the relevance of successful accomplishments to other jurisdictions. Extremely good job of filling the knowledge gaps all along the value chain.

Incredible breadth of work spanning technical, process, training, education, and tools development.

Practical in all communities with which they are engaged while maintaining overall strategic progress to move solar market transformation forward

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide a good description of the project goals. Further detailed information on measurements of achieving the objectives would be welcome.

The projects that were presented were uniformly successful--if there were any failures (or less-successful projects), we didn't hear about them. The work with ConEd on the network grid was precedential and path-breaking. The net metering paper for MN appears to have been quite helpful. Clients seemed happy with the support they received--but you should ask them.

Technical reports: Interconnection, solar ready buildings planning guide, net metering policy (MN), rate structure analysis (San Diego)
Financial Advisory Work: Webinars (tax-credit bonds, municipal contracting issues, PPAs, securities compliance issues), CREBs symposium, 3rd party finance analysis (Boston, San Jose), RFP/PPA selection committee participation
Training and installer development: NEC, assessor, structural load calculations
Policy Impacts: ConEd NYC plan, PACE program and legislation analysis
Project work: structure site assessments

Each report and webinar has proved useful.

Timely, relevant, in-depth work on
- interconnection PV to network grid in NYC
- net metering policy development & distributed gen in Minnesota
- Solar San Diego

Leadership on addressing financing challenges nationwide
- webinars for tax credit bonds
- PPAs
- securities compliance issues
- symposium on CREBs
- buy vs. PPA tradeoffs, RFP/PPA processes

Examples of Training, local installer development for PV, SHW, NEC code, structural
Additional work on policy

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Nice selection of external constituents. The list could be expanded however.

Appears that local capacity-building was a key goal and was incorporated.

Utilization of local contract support from many more organizations, of all types, than what was listed in the presentation.

The Tiger Teams are pure collaboration in that they have high value for client input in developing their lines of research.

Almost by program definition this should score well, but the results are clearly above expectations and aligned with the overall MT program

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The presentation does not provide sufficient information to indicate that future activity is anticipated.

Really like the assessment of the economic impact of Boulder County's PACE program. Not clear what all the future efforts will be.

Keep up the good work.

Looking at historic facilities
Economic impacts study of PACE in Boulder
Solar Optimization Tool Development - both PV and SHW at the same time
Increase TA to 40 special projects
Solar on brownfields

Project Strengths:

The project participant helps establish an infrastructure and technical capability which can facilitate and assist renewable resource development.

Flexibility to respond to local issues on a real-time basis. Liked the diversity of efforts undertaken--a lot of interesting and non-intuitive efforts.

Too many to list.
Cross cutting tasks.
Automate the Tiger Team -> Looking forward to seeing how this turns out.

Combination of willingness to tailor projects to specific needs while producing cross-cutting work of general relevance to many cities.

Timely, relevant, in-depth work on
- interconnection PV to network grid in NYC
- Net metering policy development & distributed generation in Minnesota
- Solar San Diego

Leadership on addressing financing challenges nationwide
- webinars for tax credit bonds
- PPAs
- securities compliance issues
- symposium on CREBs
- buy vs. PPA tradeoffs, RFP/PPA processes

Examples of training, local installer development for PV, SHW, NEC code, structural
Additional work on policy

Project Weaknesses:

This project could establish additional performance metrics to evaluate the success of its own initiatives. This project could expand the list of outreach participants to other industry institutions.

Not a weakness per se, but only a few of the projects provided transferable lessons.

None. How do they do so much with so little? I'd give them twice the $$$.

Few weaknesses, but there could be a need for more funding & resources to start increasing the cross-pollination for 25 cities.

No initial mention of how the TA support will be pushed or stretched thin as a result of the SACTO award, but then this concern was adequately developed.

Lots of tools development - how do you actually measure adoption?

Recommendations for changes to the Project Scope:

The NREL role as a facilitator of Solar Cities efforts should be generalized to be a process incubator/technical assistance source for any entity seeking to develop or deploy renewable resources. This scope change should generalize the current role provided for solar cities and should wean specifically-funded cities from the program.

Wondering if this program could be expanded beyond just SACs? There are a lot of other solar advocates out there. Perhaps NREL could conduct periodic solicitations for partners/issues/ideas, and select the best ones to support?

Consider the usability aspects of the automation of the Tiger Teams. Know your user audience.

Love the scalability focus of the future effort to include more than just the Solar America Cities.

[1] Ensure resources to start increasing the cross-pollination for 25 cities remain at the forefront of FY11 planning to minimize any risks to focus, bandwidth, projects as a result of the SACTO award [2] Clear definition of TA support if or as it might scale along with city growth as their markets ramp up

Review: EERE 2010 Solar Program Review
Presentation Number: MT020_
Presentation Title: Technical Integration
Investigator: Orr, Frank

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
The project participant has provided resources to other project participants. This helps make the methods and analysis more consistent and can help establish a more uniform market environment.

As they provide support to individual cities, the relevance of their work is dependent upon the priorities of the clients they serve. In general, projects look good. Seems that the mapping application was quite popular...

Good alignment with DOE objectives in the core areas of:
Consumer education
Metrics
Coordination
Permitting
Site assessment
Economic benefit
Expanding roles

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Some tools and process developments by this project have been useful in "cross-pollinating" the participating cites.

Again, hard to judge, as they seemed to work at the behest of client interests. Projects all seemed to fit into the general framework of helpful barrier-busters. I really like the idea of making this kind of resource available to cities; it's just hard to judge the effectiveness from the short presentation.

Tightly integrated working model with Sandia, DOE, and all SACs. This has led to excellent performance

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
It is good for this project provider to be variously engaged with the Solar America Cities participants. It will be important to ensure that the tools developed and lessons learned become available throughout the Solar America Cities activity, rather than have limited release to requesting cities.

Hard to know--can't tell from the presentations how much they did, and how much city staff did in many cases, and would have to see the work product to assess quality. The solar maps appear to be popular and are well done.

Solar metrics.....very important...glad to see them working on this aspect.

Moved in lock-step with Cities

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
The project presentation indicates there has been engagement with industry partners.

Hard to know. They serve their clients; hard to say from a distance how well they integrated with other stakeholders in developing their work products.

Good collaboration within the context of their role.

Appropriate mix of labs, outreach, technical, solutions providers. CH2M Hill is so large that they are able to bring other stakeholders to the table from within their own ranks from time to time.

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
It is not clear that there are explicit future plans referenced in the presentation. The verbal discussion made mention about potential "bridge" activity at a regional or national level. Additional details would be helpful here, as well as clear and measureable goals for continuation of future activity.

Looks like efforts are mostly about wrapping up current projects that are underway, with some adaptation to new resources (i.e. integrating IMBY into maps).

More of the same. All good.

Good foundation. Would like to see a bit more specific goals, milestones for FY11 plans

Project Strengths:
This project helps ensure a good technical resource "center" for the SAC participants.

Having the flexibility to deploy technical resources to projects as needed clearly is helpful.

Measuring successes...solar metrics
Consumer Education – solar portals
Financing – Santa Rosa CREBS
Permitting/licensing processes – Philadelphia, Portland
Economic development – Milwaukee, Sacramento
Site assessments – San Jose, Twin Cities, Forest City
Expanding the role of solar – Boston, New York
Media relations & outreach

As an extension of the work being done by the labs, this program shares the credit for producing consistently useful technical assistance.

Very broad effort, wide mix of tools and solutions across many technical, process, and other areas

Project Weaknesses:
In the case where the emergency preparedness recommendations were implemented by the Solar

America Cities, it appears from this presentation (as well as those of the SA Cities) that this was developed through input from this project participant. It is not clear that this idea for market transformation was responsive to stakeholder concerns or desires original to the SA Cities.

They were pricey. Hard to know how mission-critical their efforts were, versus using the $6 million to fund another 35 Solar America Cities...

Would have liked to see a bit more evidence of cross-pollination, cross-cutting. Given CH's perspective to see across all cities simultaneously, there could have been more potential for cross-cutting coordination than stood out.

When pressed on this issue, the response was "somewhat ad hoc, passive x-collaboration"

Didn't see a lot of involvement in financing, economics, value proposition?

Recommendations for changes to the Project Scope:

Establish (or confirm) the guiding principle that the tools and methods developed for one or more of the Solar Cities by this project participant will be made available to the additional cities (and the public) in the future.

Would probably be best to have client cities provide this review.

Consider evaluating the effectiveness of the various marketing, media outreach, and educational approaches that cities use in order to determine the most effective approach. An example provided by the speaker was conducting a focus group before and after a specific outreach activity to increase awareness among a stakeholder group.

Keep it going until the transformation is complete.

Try to develop a comprehensive set of success metrics faster to measure success across all cities, in a more comprehensive integrated way.

Having a process built around the need to measure success is important, but be careful about spending too much time on the process engineering. With the number of cities now accelerating solar activity, the need to start measuring actual results is very important. Closed-loop processes could come later to a certain extent

Review: EERE 2010 Solar Program Review
Presentation Number: MT021
Presentation Title: Solar State Technical Outreach Partnership Project
Investigator: Sinclair, Mark

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Nice generalized approach that covers a broad spectrum for applicability and outreach.

State policy is *the* most important place for policy driving solar markets. Focused attention on the key state policies that drive solar markets, delivered to state officials, is very important.

CESA plays a unique role in turning program data and results into usable white papers for utilities and public agencies trying to start or improve their solar programs. Extremely good choice of topics and identification of the most important questions to be answered.

Barriers
- High upfront costs for solar installations: Significant amount of activity
- Lack of strong solar value proposition: Highly relevant but only appeared to do moderate work in this area relative to the others listed
- Lack of comprehensive state policy support to advance solar markets and system deployment
- Excellent work, on point

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project presentation indicates this effort is engaged at the proper levels to promote widespread standardization and acceptance, particularly in the local regulatory aspects of the industry. It is less clear how this project will reduce other types of market barriers.

Peer-to-peer connection is an important and effective tool. That said, there are some limitations to that approach. Developing and implementing policy can be a long and challenging process, and often needs strong advocacy to push all the way through to the end. Perhaps this is beyond the scope of the available resources.

CESA has provides one of the essential ingredients for continuously improving solar programs by providing solid guidance on the big questions that every program needs to address. Their work is one of the resources that need expanding in order to avoid the reinvent-the-wheel predicament. Sharing of best practices is essential to accelerating the necessary transformation.

Excellent focus on target audience, using high impact tools, in the most cost effective way across multiple topics that are continually changing

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide a good description of the project goals. Further detailed information on measurements of achieving the objectives would be welcome. The greater detail should help identify how this project delivered results that would not otherwise have developed (or results that developed more quickly than absent the program). It may be that some of the progress reported through this project would have occurred in any event, rather than due to this project.

With the limited funding, program had good, positive, relevant output. Solar markets depend on a suite of policies to work, and a market is only as strong as the weakest link--not clear if program's focus on a single issue at a time (i.e. financing) make that point clear.

The preparatory work of gathering and digesting information and turning into a form useful to others is well underway. Probably the next focus ought to be on how to reach the intended audience in a way that motivates action and collaboration.

Objectives
- assist states to establish/expand solar programs
- convince all 50 states to support
- provide capacity building
- foster strategic partnerships between states and stakeholders

Results - 300hrs / 25 states

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Nice selection of external constituents. The list could be expanded however.

Getting LBNL's support is a key and welcome partnership. They've put out some great papers. Beyond that, it's not clear that program collaborated with industry, industry associations, or other organizations with experience on solar policy in helping direct work and review work products. That said, given the amount of differing opinions, perhaps that was a good decision in terms of efficiency.

Expand focus to develop dialogues among utilities, government, and the solar industry.

Very strong peer-to-peer network built over 12 months, covering states, labs, CESA members

Quarterly webinars are timely and relevant - Financing, Costs, Marketing Strategies, State incentives

CESA RE monthly webinar series is very strong to take timely topics and translate them to the states, e.g. PACE, Financing DG, DOE Loan Guarantee, Utility Roles

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The future plans reported in the presentation are vague and do not convey a sense that continued future funding would result in efficient achievement of the program goals.

Grant ends soon; no future funded work.

Would like to see a strategic plan for getting input from users on how to play the most meaningful role.

<u>Project Strengths:</u>
The project helps develop institutional knowledge for managing zoning, site, and local use issues that arise with distributed renewable deployment.

They are in the right venue--states are the key arena in which decisions affecting solar market development are made.

Has carved out a much needed role in the overall movement and has performed that role well.
State/Federal RPS collaborative across 28 states/250 officials
Comprehensive approach Targeting messaging/outreach

Project Weaknesses:
This project could establish additional performance metrics to evaluate the success of its own initiatives and could expand the list of outreach participants to other industry institutions.

It is often not enough just to make information available--it often needs to be tailored to specific locales, and, to make change, there needs to be an advocate to push policy through. A second stage of place-specific effort could be very helpful in achieving goals.

Need to convene more powwows on major topic issues--get beyond the webinars.

Recommendations for changes to the Project Scope:
The scope document could be revised from a list of topical concepts to some specific planned achievements and measurements for associated success. In addition, the scope document could be clearer on the details of requested funding and cost sharing offers.

Presentation made some suggestions for future work if funding were available. The ideas presented are solid and worthy of funding. In particular, states can benefit from hand-holding throughout the process of developing new programs (i.e. more targeted assistance than issuing blueprints or whitepapers).

One additional item to add: policymakers need tools and assistance understanding the ratepayer impacts of policies (or lack of policies). Would very much like to see future efforts in this area.

Conferences and interactive think sessions on specific roles. Explore how this program can, should, or might be extended to dovetail with the SACTO award to scale the city/local level MT work

Suggest some thinking going forward into how adoption of new technologies might be addressed, given state and utilities are so slow to accept change. Innovation is happening very quickly; thus, some of the work to date might now or soon be obsolete from a technology or solutions perspective

Review: EERE 2010 Solar Program Review
Presentation Number: MT022
Presentation Title: Project Title: Stakeholder Outreach - Workforce Development
Investigator: Weissman, Jane

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Nice generalized approach that covers a broad spectrum for applicability and outreach.

IREC plays a central role in building the solar future in the United States. Each task they take on is important, and they do it with a high level of competency. Their regulatory work is very important, and their role in training and credentialing is key.

IREC plays a unique role by focusing on an in-depth way on a few complicated regulatory issues as well as codes and permitting.

Rule-setting (technical, qualitative, quantitative) is essential

Unfair and uneven state policies for interconnection, net metering
Need for information and technical assistance
Unqualified and untrained installers, substandard and unsafe installations

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Nice outreach and education efforts, but based on presentation materials, the project appears to be missing or weak on some vital sector engagements (e.g. utility, producer, regulatory bodies, reliability standards etc.)

Very much so.

One important consideration in federally funded programs is that each activity makes a unique, distinctive, and non-duplicative contribution. I consider IREC's work to be definitive on the topics of interconnection, net metering, and permitting.

Both breadth and depth with the state-by-state reach across 4 key areas

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide a good description of the project goals. Further detailed information on measurements of achieving the objectives would be welcome.

I have direct knowledge of their regulatory and model practice work and can vouch for their excellence in this field (full disclosure: I serve on IREC's net metering and interconnection advisory board). Having the model standards gives policymakers across the country an understanding of the target. IREC's attorneys are literally game-changers. Their work before regulatory bodies has made all the difference in establishing key barrier-busting policies (net metering, interconnection, 3rd party solar services).

IREC has produced the basics reference works on the topics it has staked out for attention.

Accreditation, Credentials, and Certificates provide an outstanding way to close the loop between higher level strategies, the practical programs, and tangible measurable effectiveness

“Freeing the Grid” scoring effective transformational changes.

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Nice selection of external constituents as well as delivery of credible training program assistance and news to sectors of interest. The list could potentially be expanded, however, to reflect utility and other wholesale sectors.

IREC plays well with others. They partner with local stakeholders (installers, advocates, etc.), and develop excellent relationships with regulators.

IREC is completely embedded in the community of organizations addressing the many aspects of spawning a national solar industry and is conscientious about networking and respecting the unique roles of other institutions.

IREC is one of the best at reaching out to the right mix of stakeholders at the right time, in a comprehensive way to add value to key issues

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The future plans reported in the presentation are vague and do not convey a sense that continued future funding would result in efficient achievement of the program goals.

Their current funding shuts down at the end of the year. Bummer.

A little weak on where IREC wants to be and by when.

Not critical of the program - difficult to propose future plans w/out certainty in budget

Project Strengths:
The project participant helps establish an infrastructure and technical capability which can facilitate and assist renewable resource development. In particular, the attention to the credentialing issue seems to be a strong capability provided by the participant.

Two words: incredibly competent. IREC simply delivers, at a very high level of excellence.

Unique contribution, solid research.

Certifications, accreditation to link program activities to measureable results
State-by-state scorecard to drive change
Rich, regular newsletter, papers and articles

Project Weaknesses:
This project could establish additional performance metrics to evaluate the success of its own initiatives and could expand the list of outreach participants to other industry institutions. This project appears to be at risk of becoming a bureau instead of a center of achievement.

The only thing wrong with IREC is that there are not enough of them.

IREC needs to strengthen its identity and make program people more aware of the substantial help they can provide.

Recommendations for changes to the Project Scope:
The project is wrapping-up. But the scope document for future potential activity could be revised from a list of topical concepts to some specific planned achievements and measurements for associated success. In addition, the scope document could be clearer on the details of future requested funding and cost sharing offers (if applicable).

As the U.S. solar market grows and costs come down, new market opportunities and business models have developed. It would be helpful to have the experienced hands at IREC evolve some of their best-practice model development and regulatory work continue to address these new challenges (as is happening, just want to encourage more of it). Also, they need more resources.

The scope seems about right.

Would like to see a continuation of the program in conjunction with ways to address any lessons learned, observations etc. Significant lessons came out of Q&A discussion.

Find ways to incrementally raise the bar, respond to technology/innovation changes while maintaining the foundation that's been laid

Review: EERE 2010 Solar Program Review
Presentation Number: MT023
Presentation Title: Strategic Growth Plan
Investigator: Auerbach, Ezra

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Nice generalized approach that covers a broad spectrum for applicability and outreach.

This is part of the professionalization of the industry--a key part of getting to scale.

The only game in town for installer skill standards.

Essential to build behind-the-scenes skills programs to deliver solar across the country by ensuring availability of credentials, confirming that needs assessments are aligned, and establishing consumer confidence.

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project presentation indicates this effort is engaged at the proper levels to promote widespread standardization and acceptance, as well as reduce market barriers.

Program is well-thought through, identifying rationale for support from all stakeholders, good plan for future growth, and sustainability.

Staked out an essential market barrier and has addressed it.

Consistent approach to the core mission - credentialing, certification of end market stakeholders

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The presentation materials provide a good description of the project goals. Further detailed information on measurements of achieving the objectives would be welcome.

The results speak for themselves--project is on-time, on-budget, and on a glide-path to self-sustainability.

More work needed on establishing itself with consumers as the standard of quality and on more aggressively organizing local contractors around a set performance standard.

Slide #9 clearly shows the ultimate test - growth in enrollment

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
Nice selection of external constituents.

As far as I can tell, NABCEP collaborated with all of the necessary players.

NABCEP needs to become more active in promoting quality performance within all active solar markets.

100+ contributors to develop program on an ongoing basis
15 states

Solid cross-section industry represented on the board, but appears to be more historical

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Nice statement of future goals and good job of building on past success to establish the credibility of these proposed goals.

The grantee has made great plans for the future--makes a compelling case of value and of future growth.

I would like to see more attention put into how to ensure that high skill sets translate into consistently high-performing, high-quality systems.

Nine certifications to be stacked and mixed for a variety of career pathways
Logical extension of historical work
Need to think about being more inclusive of additional parts of the value chain

Project Strengths:
The project participant helps establish an infrastructure and technical capability which can facilitate and assist renewable resource development.

The whole program is a key part of the professionalization of the industry, which is in turn a key part of coming to scale. I really like the future growth plans--not just new job categories, but ones that are complementary and show a clear path to more and more responsibility.

Only game in town, and it is played well.

Structure
Initial strategy and design
Metrics
Foundation
Ability to leverage, extend, expand

Project Weaknesses:
This project seems to be functioning very well. The participant could focus on increasing penetration of potential certifications with the intent of being able to characterize the proportion of certified entities in the overall work environment.

I wonder if this couldn't be rolled out quicker. Understand the need for buy-in every step of the way, and grantee is in best position to judge, but products planned for the future would be helpful in the market right now.

NABCEP's mission is too self-limited.

Think about how to evolve the makeup of the board or program contributors to be more reflective of the industry today, where it is headed, and how credentialing may need to evolve

Recommendations for changes to the Project Scope:
Other than establishing targeted performance measurement goals for the future initiatives, none come to mind.

Create strategic plan for more active intercession in markets to ensure consistently high-quality systems and contractors.

Would like to see a continuation of the program in conjunction with ways to address any lessons learned, observations etc. Significant lessons came out of Q&A discussion.

Find ways to incrementally raise the bar and respond to technology/innovation changes while maintaining the foundation that's been laid

Review: EERE 2010 Solar Program Review
Presentation Number: MT024_
Presentation Title: State Legislative Outreach on Solar Technology and Policy Options
Investigator: Savage, Melissa

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
It is unclear from the presentation materials that the legislative education program is effective in the cost reduction area. However, it may be possible that penetration is increased through policy initiatives facilitated by NCSL.

NCSL is a key stakeholder with unique access to a critical audience. That said, it is hard to assess their activities without seeing the work products.

The quasi-lobbying role this organization is trying to play would seem more effectively accomplished by other types of institutions and interest groups.

Barriers
-State policies that discourage adoption of solar energy
-Lack of knowledge about solar energy and its applications among state policymakers
-Lack of awareness about effective state policies to promote solar energy

Unclear on the value of this program relative to .org solar industry trade associations doing projects in support of DOE EERE goals

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Without objection to communications efforts, it is difficult for the scope to improve on some of the fundamental aspects necessary for market transformations. That said states with limited market access due to policy or legal impediments could be swayed by the efforts of this project participant.

Again, hard to assess without seeing the work products. In general, feel that state policy is best affected by a tailored approach, understanding state-level issues, and crafting appropriate responses.

The program has used the standard communication methods for reaching its intended audience, but there is a sea of information about renewables going to lawmakers at the moment and it is hard to imagine how NCSL's methods would make them more effective in influencing opinion.

Sound approach to integrate bi-directionally with legislators in all states

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
It would be nice to see additional measurement details for how deliverables were achieved directly due to the efforts of the project participant.

Again, hard to know without seeing work products.

Measured impact is minor compared to the enormity of the task of getting a basic solar incentive program established in all 50 states.

Presenter stated total possible target audience ~7800 policymakers
Slide 7 data reported 16 / 7800 target audience for a 2-day conference in DC.
This is <1%. It won't move the needle fast enough

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
This project participant is inherently the collaboration provider among state legislatures. However, using passive efforts such as the magazine and Web communications, it may be more difficult to capture measurement of results. It would be helpful to measure the extent of tailored communications or an active effort for targeted communications to achieve specific goals related to renewable market transformation.

Grantee served clients directly, provided information sharing, etc. I believe that there is the opportunity for more collaboration with other stakeholders and policy experts.

Little evidence of how NCSL ensures that it is thoroughly covering the waterfront. What channels does it use to make sure its database is comprehensive?

Reasonable outreach, partnering

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Project is coming to a close in June.

Much work needed on clarifying and condensing a precise and unique mission.

Project Strengths:
The established role of the project participant is the greatest strength, a "bully pulpit" for the chance to expand interstate collaboration on the project issues.

Grantee has unique access to key policymakers.

It is difficult to identify a unique contribution this organization is making to the general energy policy discussion.

Good framework using established organization

Project Weaknesses:
The project seems passive in its communications. For example, the participant makes info available but is not measuring the results of the communication and seems not to be making directed efforts to areas which lag in market transformation.

Many of the webinars/papers/etc. have been prepared and delivered by numerous other entities in similar timeframes (i.e., I can think of at least five different organizations that have recently held PACE webinars). Is this unnecessary duplication of effort? Or does the fact that so many stakeholders are presenting information speak to the need for a neutral party like NCSL to be involved?

Part of the project connects legislators with solar bills (introduced or passed) in other states. Two issues: 1) there's a lot of bad policy out there, and there's a need for some qualitative judgment; 2) the legal and regulatory foundation of energy policy varies greatly from state to state, and it is often difficult to cut-and-paste legislation/programs in whole cloth from state to state.

The organization's objectives seem vague. There is an emphasis on communication vehicles but little mention of the message. Is a clearinghouse an essential role to fund at this point? Do legislative staffs routinely review legislation in other state's prior to drafting their own bills? To my knowledge, NCSL has not been a significant presence in California's legislative staff policy discussions. I'm unsure of the need for this organization, as solar lobbying is most effectively done by state-based interest groups who have well-developed ties with legislators. They produce the same types of materials as NCSL.

Presenter stated total possible target audience ~7800 policymakers
Slide 7 data reported 16 / 7800 target audience for a 2 day conference in DC.
<1% penetration after building for 3 years towards a conference for collaboration. Even after adding in webinar count (21) accounting for travel restrictions, this won't move the needle fast enough.

<u>Recommendations for changes to the Project Scope:</u>
For more direct relevance to US DOE RD&D goals, it would help for the NCSL to adopt some form of specific mission and tasks with associated measures. Would there be benefits if NCSL were to identify specific common technical resource references (e.g. NABCEP for installer certification) that could be used as a standardizing influence when states reach out for guidance and information?

Grant is closing...but for future efforts, would emphasize putting resources into policy assistance custom-tailored for individual state efforts, as goals/players/legal foundation vary so much from state to state.

Restrict scope to being a library of renewable legislation for policy types who might need quick access, i.e., strictly a web-based tool where all renewable energy laws are in one place.

Evaluate effectiveness vs. other organizations doing similar work
Fund on condition of ensuring alignment and coordinated focus with other industry or advocacy organizations

Review: EERE 2010 Solar Program Review
Presentation Number: MT025
Presentation Title: Large Scale Integration
Investigator: Parsons, Brian

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
The problem is well-described. I'd recommend putting a lot more money into this effort.

Utilities are in dire need of a comprehensive treatment from a technically expert neutral non-vendor of large-scale solar feasibility as a competitive wholesale resource. This NREL program can fill that gap.

Barriers addressed
Outstanding - Grid Integration Opportunities and Policy Analysis
Good - Institution Engagement and Outreach
Outstanding - Regulatory, planning, and operational standards not incorporating solar
Good - National Renewable Coordinating Collaborative

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The efforts described are all helpful, but not sure that they are commensurate to the problem. Would like to see a lot more resources deployed in this arena.

Strong evidence that this program has a thorough and comprehensive understanding of the multifold barriers confronting this technology.

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
The effort has just begun. There's a long road before progress will become evident.

Very high level of understanding of the technical barriers, now to work on the institutional barriers.

Too early to tell. Seventeen percent average progress (slide 1 task %) in five months since funding started. Need to accelerate or determine why progress is challenging.

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
It seems to me that this is an area that could use some improvement. It's not clear the extent to which grantee has solicited input from stakeholders on priority activities.
Great work so far at interfacing with regional power planning organizations. Needs much stronger emphasis on establishing ties with the institutions that ultimately make resource acquisitions - the individual utilities.

Collaboration is good relative to the challenge or task at hand. Possibility exists to do more even with a small budget by increasing network of stakeholders, if applicable

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
One of the biggest difficulties with dealing with this problem is the numerous competing venues, and lack of clarity as to where the problem will be solved--state level? FERC? Regional or sub-regional planning effort? And if so, which one?

Would like to see a stronger outreach program that features interactive sessions in small group settings with utility executives and boards (for munis) who are empowered to make long-range resource plan decisions.

Early to build for the future, but FY11 plans are sound, with goals and milestones

<u>Project Strengths:</u>
It's clearly a crucial and under-addressed subject.

Very high degree of technical expertise applied to a crucial issue.

Sound strategic investment in emerging area that will take years to solve the essential technical, process, policy, and planning issues to transform this market segment

Project Weaknesses:

Honestly, I think DOE should put together a separate ad-hoc oversight committee to help evaluate and guide this important effort. Not only is the landscape ever-changing, but it is probably out of my competency to evaluate.

Inadequate utility outreach and communication plan.

Seventeen percent average progress (slide 1 task %) in five months since funding started. Need to accelerate or determine why progress is challenging

Recommendations for changes to the Project Scope:

This isn't just a transmission issue. I'd also add interconnection at the distribution level. There is a ton of wholesale DG coming on-line in the near future in CA and other states--and managing the rush is going to be a challenge.

Formal strategic plan for outreach.

Possibility exists to do more within existing/incremental budget by increasing network of stakeholders, if applicable. No change in scope needed. Recommend trying to find ways to accelerate progress

Review: EERE 2010 Solar Program Review
Presentation Number: MT026
Presentation Title: Facilitating Utility Use and Integration of Solar Electric Power
Investigator: Hamm, Julia

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
The methods of education and outreach may assist increased penetration through policy development, but the impacts to cost reduction are probably low.

Utilities are certainly key players in energy markets, to put it mildly. Making inroads with this stakeholder group is clearly important.

A singularly valuable program for integrating distributed PV into the utility's grid, cost recovery processes, and traditional business models. No other institution is providing this nexus between the key player in the rapid expansion of solar--the utility--and the emerging, dynamic industry.

Barriers identified form 3 critical pillars of affecting MT inside the utilities
- Varied utility knowledge about solar energy
- Price differential between solar and traditional sources of generation
- Disconnect between the utility and solar industries

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
The project design does not lead well to specific, directly measurable results.

Utilities have, frankly, long been a barrier to solar market growth. From lobbying against solar incentives or renewable programs, to implementing unnecessarily onerous interconnection standards and procedures, many utilities have historically been more of a problem than a solution. This effort is a welcome collaborative effort to change that.

That said, there's also a danger of utilities using their monopoly status and unique access to customers in order to crowd out other solar market participants, to the detriment of the larger solar industry. Would need to see and evaluate work products (such as upcoming 'Utility Solar Business Model Report') to properly assess.

With the new utility business models study, the program has defined the landscape and clarified the conversation about the choices utilities have regarding where to fit into the inevitable expansion of solar.

Developed a number of different educational mechanisms for utility staff, market research, analysis of issues, along with being closely integrated with utility strategic planning processes

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
Based on the imprecise measures for performance, it is difficult to evaluate performance under this criterion. Additional information would be welcome.

Selected project categories are on target. And utility interest in solar has changed dramatically in the past few years. The regional directors are top-notch, highly respected professionals. The webinars are highly relevant, and often feature top experts. Grantee doing a great job on many levels.

The program has made a lot happen in a short amount of time due to clearly defined objectives and effective resources.

Great progress in all areas, somewhat unclear though on the cost differential side of things. Suspect it is buried in all the materials, work, outreach, education, tools, etc.

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
The selection of project collaborators is good. Additional efforts for measurable performance or more formal arrangements would help indicate progress.

Would highly recommend that future efforts seek collaboration with non-industry stakeholders. Great existing collaboration with other utility groups and some solar trade associations; what's missing is the advocacy element.

SEPA is excelling at constructing an information matrix that includes all leading organizations' work on the surmounting the key barriers to market transformation.

Very strong approach with key established organizations, reference slide #8

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
The project could potentially devise additional efforts to overcome barriers.

We now have enough disparate experiences/experiments that it might be useful to have a backwards look: what are the results of programs? Did rhetoric match reality?

The program continues to evolve an effective model for expanding utility thinking on how to most effectively support and adopt solar.

Excellent foundation. Future plans should try to be more specific, actionable, linked to specific milestones as opposed to a broad list of potential areas of effort.

Project Strengths:
The project participant has an engaged, large venue of stakeholders. And many of these stakeholders have a great deal more leverage on the volume of solar resource purchases than those tied in to other projects under this program.

Really key target community, top-notch staff, great effort.

Clear sense of mission and highly effective strategy for getting the most results from available resources.
Breadth and diversity of approach
Nice utility stakeholder penetration trends
- 143 in 2010
- 92 in 2008
- 53 in 2007

Provide personalized technical assistance to over 500 utility contacts
Summary online toolkit describing diversity of business models across the US

Project Weaknesses:
The project presentation seems lacking on technical details.

There may be more that grantee can do to encourage growth of both utility and IPP business models (with the caveat that without seeing work product, hard to make accurate assessment). I note that training has been given to Duke Energy, which has mightily resisted broader non-utility solar participation at the expense of the best interests of the broader solar industry.

Greater need to take their message on the road rather than rely upon their conferences to reach utility decision-makers.

Unclear on how Barrier #2 was addressed (price differential between solar and traditional sources of generation)

Recommendations for changes to the Project Scope:
For activities funded by DOE, develop specific project goals with precise measurements of performance.

Incorporate more collaboration with non-industry stakeholders. Suggest that the grantee also address more technical concerns as well, such as managing the interconnection of increasing wholesale projects at the distribution level.

Allocate additional resources for strategic planning intensives with individual utilities.

Look at developing a series of reports that can start to show the convergence trends (or gap analysis) towards the eventual grid-parity crossover
- on a utility-by-utility basis
- state-by-state basis
- correlate with DSIRE database?
- work with other industry associations that can bring the consumer into the discussion to accelerate adoption around cost differential, although this requires a different approach for wholesale vs. retail market segments

Review: EERE 2010 Solar Program Review
Presentation Number: MT027
Presentation Title: State Labs
Investigator: Friedman, Barry

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
Good, relevant goals stated.

Frankly, this is a project that could be enormously helpful or do a disservice, and I think inclusion of a broader group of stakeholders into the oversight process is important. Take the focus on FITs, for example. While the German feed-in tariff program built the solar industry into what it is today and did the world a great service in terms of building enormous manufacturing scale, it also drove prices of modules higher for 4 years running (to the great harm of programs elsewhere that depended on prices coming down in order to scale), and severely distorted costs through the value chain (to the great harm of many industry players). In my opinion, the singular goal of market transformation is to bring down costs while building sustainable long-term markets, and a more critical analysis of the actual impacts of market design as well as the range of policy options is needed.

This project builds the fundamental datasets needed to make policy, investment, and program design decisions.

Barriers addressed
Good - Provide technical assistance to state policy-makers, NGOs, and PUCs on instate solar resources,
Good - PV cost benchmarks and economic values, and policy questions
Good - CSP awareness

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Based on the presentation, the approach details are much less clear than the goals. This project may not be commercially or technically feasible if it is not delivered to the proper recipients. It risks being a light bulb on a sunny day or a candle in an empty room.

Context is everything. While it is helpful to have more transparency and information about current costs, I'd more like to see analysis on why costs are so high and how they can be brought down through market design--the status quo is not where we want to be. And while a tool to calculate FIT rates might also be helpful, it would be even more helpful to show how market policy can be used to bring down costs beyond current levels--silicon prices, for example, are still several times higher than the cost of production due to legacy contracts from overpriced German FITs. Are fixed-price markets based on current prices the best way of addressing this? And while some U.S. FITs have been very successful, others have been much less so: one-day markets at prices double what could have been achieved by competitive solicitation, with no long-term sustainability. Would like to see NREL's efforts help contextualize the range of policy options into sustainable, long-term market design that delivers low cost solar power.

Data is being turned into information needed to directly answer most common questions asked by solar stakeholders.

NREL TA approach was briefly laid out, could have been articulated more in depth
CSP approach was reasonable

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
It is difficult to measure from the presentation materials just how much this activity has contributed to actual adoption or penetration of the resources.

The report on jurisdictional issues was excellent. I don't have enough information on the subsequent efforts to assess.

Numerous products in the marketplace, with potentially large number of users.

NREL TA - June Boot Camp was a good milestone to achieve, then build on with respect to the projects listed

CSP - Primarily education, awareness results laying the groundwork for future activity.
Good start in both areas, too early to be "Outstanding"

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)

Comments:
The project participant is well-situated to provide broad outreach and industry support.

This is an area where additional collaboration/input/oversight with a broader range of stakeholders could be of benefit.

Careful tailoring of products to the often arcane needs of a variety of clients.

NREL TA - Reasonable initial collaborations. Would like to see more broad industry involvement
CSP - Very nice start (ref slide #5)
Hard to gauge how much or how well partners were engaged

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Also need to focus on how activity will result is measurable progress toward program goals.

I've made my points in the sections above.

I'm not entirely clear on the overall strategic goals that are informing the choice of data projects.

Too early to tell and get into detailed recommendations

Project Strengths:
The project participant helps establish an infrastructure and technical capability which can facilitate and assist renewable resource development.

An area of increasing interest, and much disinformation.

Formative construction of usable information from a plethora of data that could otherwise induce paralysis.

NREL TA - Technical depth of the labs being brought to bear on end market needs such as rate characterization, state/county solar value proposition economics, industry tools

CSP - Like the comprehensive technical review of 9 projects for BLM. Nice work on the World Bank presentations, 230 tours

Project Weaknesses:
This project could establish additional performance metrics to evaluate the success of its own initiatives and could expand the list of outreach participants to other industry institutions.

Made suggestions above.

No clearly defined pathway to market transformation objectives.

Both efforts seem more supportive and responsive as opposed to proactive, forward-thinking.

Recommendations for changes to the Project Scope:
The scope document could be revised from a list of topical concepts to some specific planned achievements and measurements for associated success.

In addition to the recommendations above, I believe that tools for ratepayer impacts would be enormously helpful. The three biggest barriers to getting policymaker support for new solar programs are cost, cost, and cost. Providing tools or individual support for assessing ratepayer impacts would go a long way towards increasing program adoption.

More refined communications plan.

Keep the scope the same
Accelerate tool deployment, adoption, dissemination
Try to be more involved strategically to get ahead of what cities, counties, and industry needs are; challenging but important
Look ahead to newer technologies and how that might impact the work
Be open to what's going on in Europe or around the world to inform activities

Review: EERE 2010 Solar Program Review
Presentation Number: MT028
Presentation Title: NREL Environmental Impact
Investigator: Turchi, Craig

Criterion 1. Relevance to overall DOE objectives of cost reduction and market penetration of solar technologies. (Weight = 20%)

Comments:
It is not clear from the presentation materials if the project is intended to reduce environmental barriers to renewable penetration. This seems more like basic research than research addressing expansion of a market segment. It is not evident how much of the research is addressing existing barriers or anticipating future barriers.

This is a really important area of focus, and I am very glad this work is being done. Will prove to be of enormous assistance. Beyond this general statement of support and a suggestion for future inquiry at the end, it's hard to make additional recommendations/evaluations without a deep dive into the work product.

It is important to establish lifecycle assessment for the varieties of CSP, but it is hard to believe that the product will have substantial influence on the politics of siting.

Evaluate environmental impacts of utility-scale projects
• Identify effective impact mitigation measures and opportunities for
technology based solutions
• Develop resources and tools to support solar energy development
• Environmental compliance and permitting uncertainty
increase costs and can halt projects

Criterion 2. Approach to performing the project – the degree to which market barriers are addressed, the project is well-designed, technically feasible, and commercially promising. (Weight = 20%)

Comments:
Need further information to evaluate the extent to which this basic research demonstrates commercial promise.

As far as I know, this project is hitting on all the high notes and key points.

Clear definition of need and intended outcome of study.

Very well thought out approach towards addressing a highly volatile issue that is a critical barrier for EERE

Criterion 3. Accomplishments and progress toward overall project and DOE goals – the degree to which progress is measured against performance indicators and to which the project elicits improved performance. (Weight = 40%)

Comments:
There are insufficient performance indicators represented in the project presentation to draw any conclusions on this evaluation point.

Hard to know until work product is released...

Orderly progress towards goal.
Solid results, referencing slides #6-10 (Turchi), slides #7 (Gasper)

Criterion 4. Collaborations and information transfer with other institutions – the degree to which the project interacts with industry partners, universities, non-profit organizations, and other key stakeholders. (Weight = 10%)
Comments:
It is unclear from the presentation that substantive measurable interactions with key stakeholders have resulted from this effort.

Seems to have done a good job of connecting with key stakeholders, especially in the conservation community.

Adequate communication with all parties to the CSP siting issue has been a necessary input to ensuring the objectivity of the project.

Short but appropriate list

Criterion 5. Proposed future activity – the degree to which the project has effectively planned future work in a logical manner, considering barriers to the realization of the proposed goals. (Weight = 10%)

Comments:
Need to be more clear on goals and deliverables if future activity continues on this project.

FY 10 and 11 activities right on track...

No clear pathway to applying the results of the study to resolution of the debate.
Reasonably thought through but too early to assess

Project Strengths:
The project participant helps establish technical capability which may potentially facilitate and assist renewable resource development.

This kind of data, from a trusted government source, is key to the cause.

Provides a necessary ingredient to a multifaceted policy problem.

Broad approach to a complex issues, good initial progress & results

Project Weaknesses:
This project could establish additional performance metrics to evaluate the success of its own initiatives and could expand the list of outreach participants to other industry institutions.

Doesn't take the next step of integrating study results into the larger terms of the debate.

Recommendations for changes to the Project Scope:
The project scope should expand the list of focused outreach parties and increase suggested performance metrics.

One additional potential target of inquiry could be demonstrating the need for central station generation and new transmission to achieve our climate goals. There's a large and vocal community (with varying interests) that seek to block the development of new transmission by arguing that EE and DG is sufficient. While this might be the case at low levels of grid penetration, trying to run a grid and enjoying current resource adequacy and power quality at high levels of dependency on intermittent and non-dispatchable resources will require tapping into a vastly larger scope of generating resources...and that will require new transmission. It would be helpful to the cause if NREL would help demonstrate, from a resource adequacy point of view, what the grid needs to look like under a 50-80% renewables scenario.

Development of a broader policy paper comparing costs and benefits of remotely-sited PV with competing options.

Not as familiar with the issue as I'd like to be

EERE Information Center
1-877-EERE-INFO (1-877-337-3463)
www.eere.energy.gov/informationcenter

U.S. DEPARTMENT OF ENERGY | Energy Efficiency & Renewable Energy

December 2010 DOE/GO-102010-3119

Printed with a renewable-source ink on paper containing at least 50% wastepaper, including 10% post consumer waste.