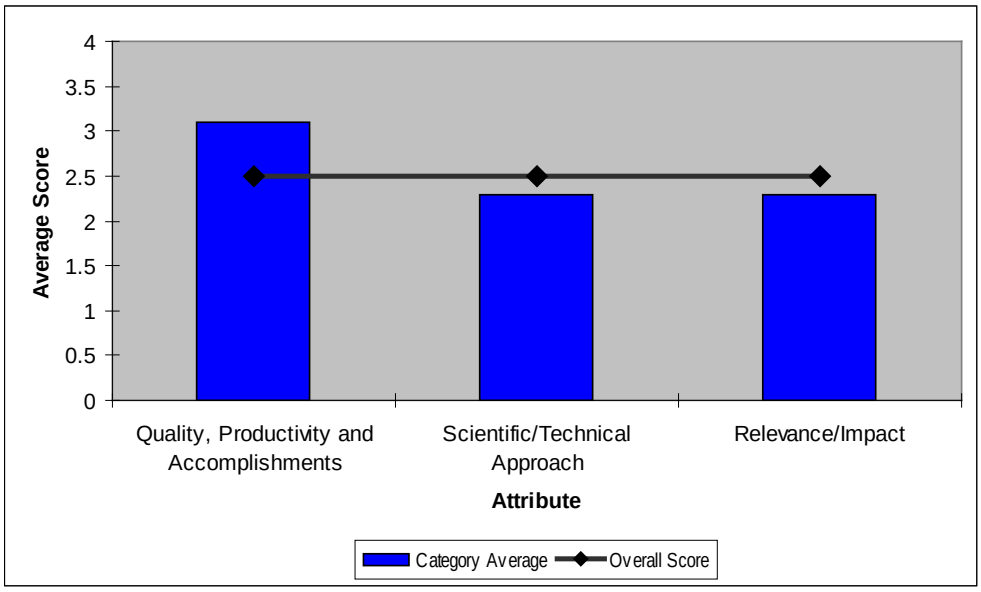
Third-Generation Organic PV Devices

Principal Investigator: *David Ginley and Garry Rumbles, National Renewable Energy Laboratory*



Quality, Productivity and Accomplishments (Average Rating 3.1)

Rating	Comments
3.0	Given that the combination of polymers and nanoparticles can be used to determine if nanoparticles themselves can be a viable solar absorber, the PI/researchers are in a good position to answer this fundamental question. They have all the tools and are fairly far along in the formation of conclusions. The work should also draw upon rough theoretical modeling (from literature) and measurements of their material's transport properties in order to see if their I-V and EQE measurement are predicted. The PI is in a position to do this but did not present such results.
3.2	The objectives of this program are entirely consistent with the mission of the Exploratory Research/SETP program. As many of the Exploratory Research program address particular aspects that limit the development of solar cell technology, a separate integration task is needed to take advantage of their research results and be a technical resources for future solar cell development. Accomplishments: Nine projects were funded in 2008. These efforts support NREL in-house R&D, universities, and existing industry and/or new companies and may last for multiple years. It is useful for the SETP to act in a seed funding capacity, providing technical resource, and foster temporary R&D efforts to explore new ideas beyond the abilities of single, smaller capital entities. Qualifications of Research Team and Resources: The researcher has assembled a high quality and diverse research team. The resources available to the research team are appropriate to task.
3.0	Outstanding team but I am not convinced that results to date validate the approach as much as claimed in the presentation. The ability to synthesize certain quantum dots is shown from optical data, but initial device efficiency results are exactly in the range previously demonstrated by other groups using this sort of device architecture.

na	THIS IS A PROGRAM FOR WHICH I HAVE A DECLARED CONFLICT OF INTEREST. Ginley is a CO-P.I. on an EFRC proposal pending with DOE right now, on a related topic area.
na	Very comparable in ambition to the Florida project, led by Xue. But this team has superior synthesis.

Scientific/Technical Approach (Average Rating 2.3)

Rating	Comments
2.0	This work does not reach very far beyond what has already been published and is being worked on elsewhere. Also, the materials will not be able to be deployed on a large scale at the concentrations needed, as they would be the major solar absorber. In addition, the project does not even touch upon the stability issues for the organic and inorganic portions. The materials selected are either toxic or unstable. A cost model (simple/rough) needs to be provided by DOE to guide research and material selection. Surface quality needs to be assessed via time resolved spectroscopy and/or other methods. It would be best to fold transport and surface info into a predictive model for device performance. Even a rough model is better than what the PI has now.
3.0	The program supporting the exploration of 3rd generation mechanisms for OPV provides an invaluable incubator for early concept examination. During the last period of performance, this program reviewed thirteen pre-proposals and selected seven for submission as full proposals (5/08), reviewed nine of these original projects and the seven new proposals by a team of seven scientists and ranked them for seed funding in FY09. (10/08), and engaged in a stage-gate decision process to select which Seed Fund projects are terminated and which are moved into core program. To provide a case example of how this process works as an idea incubator, Dr. Ginley described the time course history of his own seed program. There were two notable results from this program at this early stage: 1) the projected efficiencies of the integrated system were focused towards the higher 2015 goals of the SETP program, and 2) while this program utilized concepts from several PV programs, and it identified several issues particular to the integration of different technologies. Thus, new research efforts returned to address incomplete integration tasks of prior work, but were also essential to maintain the technology investment of the SETP program. The purpose of this program is clearly beyond the missions of the individual research programs, but invaluable in its own right to capture and disseminate the technical resources generated under the SETP auspices.

2.0	Studies an area that has already been pretty well worked over by the Alivisatos group and is also supported at University of Florida under this program. The results here (1 – 2% efficiency) are similar to results I have seen from other groups and the proposed routes to higher efficiencies seem likely to generate the usual small improvements but nowhere near the order of magnitude increase needed to make this technology attractive. There also does not seem to be a clear understanding of what limits the efficiency of the devices and the future plans seem more of a scatter-gun approach than hypothesis-driven science. Admittedly, I am no expert in inorganic semiconductor nanostructures, but I have not seen a clear argument as to why these things have the potential to achieve > 10% efficiency but are currently limited to the well-trodden 2% - 4% range. If someone could determine that and show a path to overcoming the obstacles, then it would be easier to get excited about the research. The ultimate project goal, by the way, is stated as > 30% (although outside of the funding period of this research). But how are we to have confidence that this is achievable?
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na	This ground has been trod by many others previously. What is different about this approach relative to Alivasatos, for example?

Relevance/Impact (Average Rating 2.3)

Rating	Comments
2.0	This is a good piece of work, but nothing so far suggests that it can lead to a viable and cost effective device. Many of the materials are unsuited to large scale PV. This is basic research best suited to BES. A question arises as to what DOE BES is doing in this field study and if this meshes with the SETP projects. Here below, I copy the details from the BES web site – 1 Solar Energy Utilization Research is sought in two major areas: solar-to-electric and solar-to-fuel conversions. Many of the proposed research directions identified in the BES workshop report Basic Research Needs for Solar Energy Utilization (http://www.sc.doe.gov/bes/reports/files/SEU_rpt.pdf) concern important cross-cutting issues, including: (1) coaxing cheap materials to perform as well as expensive materials in terms of their electrical, optical, chemical, and physical properties; (2) developing new paradigms for solar cell design that surpass traditional efficiency limits; (3) finding catalysts that enable inexpensive, efficient conversion of solar energy into chemical fuels; (4) identifying novel methods for self-assembly of molecular components into functionally integrated systems; and (5) developing materials for solar energy conversion infrastructure. BES contacts: Mark Spitler, 301-903-4568, mark.spitler@science.doe.gov, James Horwitz, 301-903-4894; james.horwitz@science.doe.gov URL:http://www.sc.doe.gov/bes/SISGR.html, http://www.sc.doe.gov/bes/reports/files/SEU_rpt.pdf
3.0	This program provides significant impact and relevance to SETP program and solar cell development. Under the auspices of this program, the success of an individual project is not necessarily defined by continued funding or transition into the core program. Success of an individual project could be an unequivocal answer on the viability of a new concept or technology. The goal of the project aggregation is to develop new programs for the core PV conversion

technology effort with the best approaches to resolving technical and financial barriers.

2.0 It is not clear where this effort will add value in a fairly well-trodden field.

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na See comments to #4.

Overall (Average Rating 2.5)

Rating	Comments
2.0	PI stated: "Cost projections for OPV-based devices are already attractive compared to conventional Si or thin film – if for little added cost absorption and charge transfer can be enhanced this could be important for adding efficiency at little cost." This links this work to DOE's OPV projects. If they fail (or are dead ends) then this project is less relevant. One needs to add the cost of the QD nanomaterials. If the project is successful, as targeted, will this lead to reasonable LCOE in the time frame specified by the SAI?
3.0	Restating, this is a strong project deserving priority attention as there is no other program within the SETP portfolio with the mission to capture and integrate the most efficient solutions to technical problems developed within individual programs, into strong, core research programs for the SETP portfolio. In other domains, this program has the responsibility for knowledge capture. Evidenced by the number of proposals examined, reviewed, and eventually sponsored, this program has had an important and critical role in solar cell development.
2.0	See previous comments.
na	THIS IS A PROGRAM FOR WHICH I HAVE A DECLARED CONFLICT OF INTEREST. Ginley is a CO-P.I. on an EFRC proposal pending with DOE right now, on a related topic area.
3.0	I am not convinced that solution processing will bring cost advantages. Why not just use bulk films? Let's be a little more rigorous about the justification of this project.. Can we really expect this process to be much cheaper (materials and fabrication costs)? Some discussion in terms of the published CdTe cost model (Zweibel, et al.) would be instructive.