RECOMMENDATIONS FOR ARPA-E ACTIVITY ON ADVANCED POWERPLANTS FOR VEHICLES AND SOLAR THERMAL POWER

Personal, unofficial opinions only; Dr. Paul J. Werbos (http://www.werbos.com/CV_2009.pdf)

There are unmet opportunities for substantial breakthroughs in how we convert heat energy to torque or electricity, which in my view are our best hope for really important (factor of 2) impacts on advanced vehicles technology and dispatchable renewable power, different from the incremental work already being done elsewhere. First I will discuss these opportunities, and then comment on other issues/opportunities in those areas. The other opportunities are worthwhile, but I would not put scarce new money into them until and unless the powerplant work is moving at full speed.

1. PRIMARY RECOMMENDATIONS

1a. Context and Rationale

The two most important missions to ARPA-E from Congress are to seek breakthroughs which could eliminate our dependence on oil imports as soon as possible, and to seek breakthroughs which allow deep reduction in the emissions of greenhouse gasses. In allocating funds and defining programs**, it is crucial to base all decisions on these larger goals.**

With oil dependence (the more time-sensitive issue, agreed to by a larger percentage of Congress), our nearest-term hope of eliminating oil dependency is through a **combination** of light plug-in hybrid cars with fuel flexibility, plus a large increase in the production of alternate liquid fuels in order to replace the remaining demand for liquid fuels. (See http://www.ieeeusa.org/policy/positions/PHEV0607.pdf, www.werbos.com/E/500mpg.df and http://www.werbos.com/E/China_IV_Break_Oil.pdf.) According to the EPA analysis of the Waxman climate and energy act of 2009, the biggest remaining source of CO_2 emissions by 2050 if the Act passes will be in transportation; thus transportation is the area where new technology breakthroughs have the greatest chance to improve things beyond the limited (44%) reduction in CO_2 now projected.

But even the best hybrid cars (like the Prius) use a small gasoline engine which is only 30% efficient in converting gasoline to electricity (in part because of its size). If we can develop usable fuel flexible engines which are 50-60% efficient instead, we can cut the remaining need for liquid fuel in half. (Lack of flexibility would hinder us in doing our best with alternative liquid fuels.) Because there is no other serious hope for that much reduction in the remaining fuel use, this is the most promising direction for R&D to meet the charge of ARPA-E, after the R&D on alternate fuels and plug-in hybrids themselves.

Likewise – for dispatchable renewable power, "solar farms" represent the largest untapped source of renewable electricity in the US. From basic physics, it is easy to calculate that the desert lands of the US could supply many times our total national energy need -- **IF** the cost could be reduced. Also, solar power in sunny desert areas is **not** intermittent, but is a predictable source of power at peak daytimes when electricity is most valuable. The cheapest "solar farms" on the horizon today are the "dish" systems pioneered by Sandia and by Stirling Energy Systems. Sandia has estimated that solar power from these systems should be 6 cents per kwh when they are fully deployed – far less than any other source power, and good enough to beat small gas turbines for the entire daytime peak market all across the US. (For example, PJM reports that they pay 8 cents or more per kwh for electricity at all hours between noon and 8PM, and that it would only take a little less than 2 cents per kwh to carry gigawatts from the desert part of Texas to their area. The sun in Texas lines up very nicely with demand in Virginia, Maryland and Pennsylvania.)

A key factor driving the cost of this dish type of solar power is the efficiency of the small Stirling engine they use to convert heat differences to torque and to AC electricity. (Unlike photovoltaics, they don't require expensive DC-to-AC conversion.) As in the Prius – Sandia says that the best recorded efficiency is only 31% (and I'm not entirely sure whether that's a true whole-systems efficiency measure). If this could be almost doubled, the electricity output per dish would be almost doubled. Since the cost is mainly due to the cost of making and building the dish, this would cut the cost per kwh IN HALF. **If Sandia's 6 cents estimate is right, this would give us a**

power source so cheap it could be used to displace even the baseload market (assuming parallel advances in batteries and in smart grid – though it might pay even with compressed air storage). If Sandia underestimated its costs, this breakthrough would be crucial to actually reaching their very important objectives.

Slightly larger Stirling engines of the same design have also been used to convert municipal waste and biomass to electricity.

1b. Specific Opportunities

This topic should be open to any effort which has a decent chance of getting to 50% efficiency or more in a mass-producible affordable car-sized powerplant. But it should also be very explicit in including examples of the two or three big unmet opportunities which are now staring us in the face. It has been very painful to see these kinds of opportunities get lost at places like NSF, because of review criteria which really did not focus on the national needs and opportunities here. On the other hand, these opportunities do involve very real risks (about 50% from start to finish, in my view, even if we do everything right).

The two most clear-cut important unmet opportunities in my view are:

(a). Higher efficiency manufacturable Stirling engines. Several years ago I obtained permission to post a sanitized version of a proposal co-authored by the lead inventor of the existing best Stirling engine for solar dishes and cars, at http://www.werbos.com/E/NextGenStirlingV2.htm. Given that dozens of larger stakeholders tried and failed to replicate Johansson's earlier success (except by buying access to his earlier work through Boeing or Sweden), it is really crucial to work with and include this team somehow, even though it takes special social skills to do so. A key requirement is to combine what Johansson knows (and has patented) about coatings to prevent hydrogen embrittlement, what Oak Ridge knows about new high-temperature materials and heat pipes, and what the automotive partner knows about mass-production in existing engine plants. Note – since existing engine plants are large and underutilized, it should be possible to scale up production of these engines very quickly **after** the first mass-producible prototype is tested and properly documented. But of course, new players and even universities are also needed, in order produce a decent sized workforce in this area.

(b) The Johnson Thermoelectric Energy Converter (JTEC), a fundamentally new way to convert temperature differences to electricity directly, without any moving solid parts, governed by the Ericson thermodynamic cycle. (See http://www.popularmechanics.com/science/earth/4243793.html and http://www.spectrum.ieee.org/mar08/6079 .) While NSF was able to fund some small initial work validating the basic concept, considerably more R&D is needed to bring this breakthrough through to readiness for more risk-averse government funding programs. Because this is an entire new family of technology, work in this area is inherently less incremental than any work on the well-established family of standard heat engines such as Otto, diesel and Stirling; it will take many decades to reach the same level of maturity, even though it may be only a few years before it could be market-worthy if all goes well. Credible modeling with some hardware validation suggests that 65% efficiency may be available, using JTEC either as the primary source of electricity or as a "bottoming" cycle to exploit waste heat from a heat engine.

Risk and novelty have been the main problems in getting funding to these areas.

To be a bit balanced, a program announcement in this area might **perhaps** also mention:

(c) solid oxide fuel cells or other truly fuel-flexible fuel cells… to the extent that breakthrough research is possible which does not replicate other work already ongoing; and

(d) breakthrough all-electronic technologies. Present thermoelectric chips are only about 20% efficient at best, but Dr. Robert Trew (the new Division Director for Electrical, Communications and Cyber Systems at NSF, with a distinguished prior career as a director of DDR&E and as editor-in-chief of the flagship journal of the IEEE) does see serious if high-risk possibilities for really major breakthroughs getting to 50% or more, taking new approaches.

It currently seems unlikely that the usual Otto or Diesel engines or others, **at the small size used in the Prius**, could achieve the combination of 50% efficiency and true fuel flexibility (at least good enough to use petroleum or E85 or M85 or any mix thereof, perhaps with water vapor leading to corrosion as great that of M85) required here. But a "subtopic" could be included which lays this out, urging those with ideas along those lines to try to show a credible plan for getting that far.

1c. Implementation

As one part of this effort -- I would recommend that ARPA-E negotiate a memorandum of understanding (MOU) with the National Science Foundation (NSF), structured like the one which led to the earlier joint program with NASA, http://www.nsf.gov/pubs/2002/nsf02098/nsf02098.pdf. In this case, ARPA-E should commit at least $20 million per year to an open solicitation to all US universities, small businesses and other eligible parties. While the NSF machinery for processing proposals and awards and setting up review should be used, a joint working group of ARPAE and NSF program Directors should manage the effort, and the ARPA-E program officers should have direct selection authority through the NSF machinery in allocating the ARPA-E funds. DOD may also be interested in joining and kicking in. This kind of system gives the advantage of more access to a wider pool of ideas, and a faster and easier machinery for getting money out the door. For efforts on this scale, it is important to bypass the traditional requirement for industrial cost-sharing, which is very burdensome to arrange for small groups at this stage of research.

In another context, some of us have been discussing an unofficial draft idea on these lines, as part of a larger effort to break our dependence on oil imports as soon as possible:

The Secretary of Energy, the Director of ARPA-E and the Director of NSF are directed to sign a Memorandum of Understanding (MOU) by the end of 2010, which provides for a series of continuing joint programs to be funded out of the ARPA-E budget, with an option to receive and use additional funds from other sources if available and to include other interested government agencies. All such programs shall be managed within the NSF electronic proposal submission and review process, and shall be open to all universities, small businesses and nonprofit corporations in the United States, as provided for in the NSF Grant Proposal Guide with no additional eligibility rules. Notwithstanding this provision, the MOU should allow the use of mechanisms such as exclusion of proposals for which the preproposal did not pass merit review. While funds shall be transferred to NSF, actual selection authority shall go to teams of technical experts at ARPA-E and NSF, under terms to be specified in the MOU. Each joint program shall be widely announced through the NSF system, and shall be open to new competitive proposals at least once per year. There shall also be some provision for small seedling grants. Reasonable strategic thinking about future technology costs must be discussed in all proposals and review. No awards shall be for more than $2 million total.

(a) Joint Programs

While ARPA-E and NSF (and their other partners, if applicable) may agree to other joint programs under this MOU, there shall be at least three new continuing programs which receive at least $20 million per year in 2009 dollars from the ARPA-E budget:

(1) **Breakthrough battery research** – …

(2) **Breakthrough research in powerplants for cars** – Reviewers will be asked to address the following question for each proposal in qualitative terms: "If this proposal is funded, how much will it increase the probability that in five to ten years we will have a prototype of a new fuel-flexible system for use in converting fuels to electricity, suitable for use on-board a plug-in hybrid vehicle after it goes to mass production, with a whole system efficiency of 50% or more?" Fuel flexibility should include at least the ability to use a standard hydrocarbon fuel, as well as an alternate fuel as defined in section 4 of this title. Panelists and selection committee members shall be directed to choose those proposals which are best, in toto, based on this criterion and this criterion only, when allocating the base $20 million from ARPA-E. This program shall continue at least until such mass-producible prototypes have been developed or proven near-impossible for the four currently promising possibilities (solid oxide fuel cells suitable for cars, advanced Stirling engines, the JTEC system and JTEC-enhanced system) and for other such new possibilities identified by the Director of ARPA-E.

(3) . **Breakthrough in renewable fuels** – …

Back in 1994 at NSF, with a go-ahead from OSTP, I wrote and managed a new small business research topic on "enabling technologies for fuel cell and electric vehicles." I learned many, many lessons from that experience, and from similar experiences at NSF. For example – it's very good that we opened a door to electric cars (properly worded with a statement that incremental work on low-range cars would not qualify), and that we did not restrict it the expected winner of the day. I also learned that sometimes "a broader scope" is actually very narrow in reality; for example, if we had broadened it to "automotive engineering ' or "automotive efficiency,' we would have been overwhelmed by incremental "gasket research" – and we would have gotten **fewer** proposals from the crossdisciplinary and innovative kinds of people we really needed. Fuzzwords do not attract those folks.

2. OTHER OPPORTUNITIES FOR ARPA-E IN MOTOR VEHICLE TECHNOLOGIES

Certainly there are other opportunities for breakthrough research in vehicles technologies which are relevant to ARPA-E's mission and worthy of funding in principle – but **only if the possibilities for powerplant breakthroughs are being explored at about 90% of the potential rate of progress** (which, of course, is not the same thing as the potential level of funding, which is not even bounded). Many of these worthy other areas are being funded elsewhere to some degree, so they pose special challenges in avoiding redundancy. I have funded some worthwhile work in a number of these areas at NSF. ARPA-E needs to take special care to make sure that short-term pressure from people working on near-term or mature projects in stakeholder companies do not bias it towards incremental work which is not what Congress had in mind here.

2a. Structural Materials

With regard to lightweight materials and components – people have been working on this and talking about it for decades and decades. Last I heard, USABC had a vision of possible things for the future… for which the most advanced, "holy grail" was affordable usable C-C composite materials. In fact, C-C materials have been a major part of the high-performance aircraft industry for years, because the performance benefits outweigh the high costs. In 1990, I organized a joint workshop with McDonnell-Douglas in St. Louis, where they showed me the best available technology in the world for making quality C-C parts for vehicles. Costs were then very high, because the C-C parts were mainly produced by a "batch process, just like using PhDs to bake cookies, and burning 90% of them." McDonnell was very proud of a new continuous process they had developed to make the needed breakthrough in costs – but even after lots of investment in classical AI and nonlinear control, they were unable to control it well enough to make it pay. As part of our cooperation, they began to try out new adaptive programming methods (ADP), which were able to solve the problem. The early success was documented in a chapter by David White and Donald Sofge in the Handbook of Intelligent Control, Van Nostrand, 1992. (I have not posted that chapter on the web, because of copyright issues.)

More recently, Boeing has inherited, assimilated and matured the technology from McDonell-Douglas. Affordable C-C parts are the main technology enabling their new breakthrough aircraft, the Boeing 787. Because weight is more important to aviation than to cars and trucks, the fuel savings are greater there (about 30%?) than I would expect with cars and trucks. But is the manufacturing process control affordable enough even now to be suitable for automotive use? Certainly the details are a matter of great proprietary importance to Boeing, and its competitors have found it impossible as yet to come even close. Intelligent process control is the key. Work on less advanced structural materials would not be appropriate for ARPA-E. Even the 20% savings possible through C-C materials (assuming honest accounting of potential savings) is far less than what powerplant improvements could offer. Expensive high-quality parts may be worthwhile for small but crucial parts, like clutches in universal drive trains for hybrid or electric cars.

Of course, many of us at the frontiers are looking even beyond this kind of C-C. For example, the aerospace company ATK sometimes talks about more advanced (and expensive) carbon-based materials. The most advanced hope is the use of new nanopatterned materials. It is hoped that existing government initiatives on nanotechnology might be adjusted to make more room for nanopatterned materials (as opposed to nanoparticle catalysts and traditional nanoelectronics, which are useful but do not serve the same needs). Unfortunately, the design and manufacture of nanopatterned materials are not ready yet for use for structural materials even in high-performance aerospace vehicles; it is a worthwhile area for research, but tends to stretch the ARPA-E mandate.

2b. Recapture, flexibility and control technologies

Work on energy recapture (not counting things like JTEC) is already routine in companies making hybrids. It's easy enough to recapture braking energy – the most important recapture opportunity – in a hybrid. Since we need to switch to hybrids and plug-ins anyway, as part of the larger effort, it would be grossly wasteful and inappropriate to spend ARPA-E money on how to achieve such recapture in the kinds of cars we will be phasing out.

On the other hand, there is opportunity and need for more advanced work on improving efficiency and air quality across a wide range of fuels even in Otto engines, or in plug-in hybrid cars as whole systems. There has been important initial work, which demonstrates big possible improvements, which have not been fully taken advantage of because of limited funding availability at NSF and recent financial problems in the automobile industry. See http://www.ieee-cis.org/technical/isatc/alternative/ and http://www.werbos.com/E/China_IV_Break_Oil.pdf for some examples and connections. As an example, Toyota has recently developed a "software patch" to the control of the Prius which reduces fuel per mile by 15% or so, with no change in the car or the engine, when running on gasoline. Order of magnitude reductions in air pollution have been reported for Otto and diesel engines running on gasoline or diesel fuel, form a number of sources. Furthermore, most fuel-flexible cars in the world today are run by a "nominal controller" tuned for maximum efficiency on one fuel; the new adaptive technology allows automatic optimization across a range of fuels. There are huge unmet opportunities here – but not a prospect of 50% reduction in fuel requirements. It is crucial to overcome the powerful inertia of older technology, and to use the new breakthrough technologies, in order to achieve these kinds of results.

3. OTHER OPPORTUNITIES FOR ARPA-E IN DISPATCHABLE RENEWABLE POWER

3a. Central Station Solar Power ("Solar Farms")

APRA-E should certainly give first-tier priority to certain other research opportunities which may offer breakthroughs in the cost of "solar farms" by dish technology, which may possibly even be relevant to other types of solar farm.

For example, I have heard that 90% of the cost of dish-style solar farms in the US will be the cost of making and assembling the reflecting dishes themselves. (Presumably this would be true even if "sandwich" style photovoltaics should someday become economically feasible as an alternative to Stirling engines). Al Sobey, a former Division Director at GM, says he has new ideas for how to use **existing automotive body factories** (with only minor retooling) to mass produce the required reflector panels. This would not only lower cost, but allow a very rapid scale-up in solar generating capacity, if all aspects of the system are adequately addressed. (It would be crucial of course to develop a synergistic relation with Sandia and even SES in managing this part of the effort, both to avoid duplication and to get a better idea of what is really needed that is missing.)

But for assembly cost – if I had had time and mandate, I would long ago have set up a workshop bringing together key people from Caterpillar (like Anya Getman, who has unfortunately left but still knows the company) and key people from the Japanese construction automation/robotics community (like Fukuda) to try to work up a research blueprint to see whether we could get to deep cuts in assembly/construction costs by developing new automated systems. (I would envision new vehicles to be manufactured by Caterpillar or one of its US competitors.)
There is real hope that such a breakthrough in construction costs could cut the cost of dish-style solar farms (whether Stirling or PV or JTEC) by a factor of two… **in addition** to any factor of two we get from replacing the old Stirlings. And, again, it might allow a much faster scale-up of generating capacity than we could get from the old way. Note that solar dishes are large enough they would require Caterpillar-sized vehicles to assemble, but not so large that it becomes very risky.

Many of the other areas in the RFI for this topic seem inspired by existing incremental efforts. It is understandable that the folks investing in those efforts would like to see more money come their way, the opportunities to change the larger game are not so clear. For example, a lot of the numbers I have seen for new types of larger-scale storage have not seemed so exciting, compared to what already exists or is in prospect. High efficiency concentrator cells (with "boosting" from a thermoelectric layer) are of interest to NSF and NASA, particularly for use in space, and should probably receive **some** consideration from ARPA-E – but only after the first-tier opportunities are being fully pursued, and only in full cooperation with NSF and NASA.

It will be really crucial throughout all ARPA-E activities to make sure that the people running external review panels know how to focus on the right questions, and not let vested interests (especially, reviewers who are well-funded by other funding sources) tilt the selection towards providing augmentation for topics already funded elsewhere.

3b. Other Central Station Renewables

As discussed above – new powerplants for cars and solar thermal could also be used to process the heat from furnaces using municipal waste, wood waste and so on.

Wind power is the other renewable electricity source (besides solar farms) such that the total US capacity is probably large enough to meet all our needs. But where are the breakthroughs that ARPA-E might hope for, aside from what ARPA-E might fund through the topics on batteries and on the smart grid? Certainly industry and other existing efforts are far along in the usual mechanical engineering and manufacturing here.

I have funded **some** work in this area from NSF. (See some of the things cited at http://www.werbos.com/E/Intelligent_Grid.pdf. That paper is partly based on input from IEEE members at Siemens, and needs to be rephrased very slightly for the US case – but that doesn't change the basic research challenges and opportunities.) There are some unmet opportunities to improve the management of power electronics and of wind turbines themselves through intelligent control. This is another very worthwhile second tier research opportunity – though the smart grid area itself is of first tier importance, because of its importance to wind energy.

New larger-scale forms of water power are sometimes said to be "at the same stage that wind was at 10-20 years ago." That's a reasonable analogy. Because cost of traditional mechanical systems is such a central issue here (e.g. it was what killed the once-promising OTEC), Scotland's big testing plant plays a central role in what is really needed for the wave power part of this, which claims the largest potential. EPRI assessments of the potential electricity we can get from all these sources combined is not so encouraging right now, but that was also true of wind 10-20 years ago, and maybe partly a reflection of costs (in the case of wave power). There is room for new wave power ideas as a second tier here, maybe even a heavy second tier, but the rest (including demo-scale work on existing wave power ideas) is of questionable relevance to ARPA-E.

There was a highly touted report on the "new deep geothermal technology" from MIT last year. It showed an interesting ladder of possible geothermal power sources – all finite, nonrenewable resources – from easiest to hardest. The total undiscovered potential of the hardest sounds like a big number, on the scale of shale gas or coal, but the cost and potential of even the easiest is not so encouraging. Also, while it is strongly asserted that these are environmentally benign technologies, our presented understanding of the ecology and adaptation of the archaea deep under the earth is only at a very primitive stage, and we do not yet understand the full risks here (or for geologic carbon sequestration or clathrates, for that matter, which may pose greater risks). We have recently learned that 90% of the biomass of the ocean is a mix of archaea and other prokaryotes, and that archaea become more dominant at greater depths. We know that archaea produce methane and H_2S (which gram for gram is as toxic as hydrogen cyanide), and can be stimulated by some human activities. Given very limited funding, I would not dilute it with the kind of demo projects they have been calling for.

There is also a growing community of people interested in energy from space – mainly talking about space solar power (SSP). I know something about this technology, from having run a research partnership on it in 2002 and having followed up to some degree. (It does take some effort here to get past propaganda both from enthusiasts and from those who are not quite up to date.) Certainly SSP – like wind and solar farms – could be made to work, and to supply enough electricity to meet all our energy needs many times over. Also, it would provide base-load power. In principle, if we used solar farms for daytime power and SSP for baseload power, we would need far less storage and generation margins to meet the total demand profile than we would in an y other renewable way. Perhaps, like wave power, this is a heavy second-tier technology. Some of the crucial but solvable cost barriers (like transportation cost) belong with other agencies. But there is room for parallel work on some of the other issues. For example -- because of the great recent progress in lasers and in nanopatterned materials, I could envision new breakthroughs in high powered optically pumped lasers for use ON EARTH OR IN SPACE to fuse D-D pellets, as designed by John Perkins et al in Lawrence Livermore. A competition to model and design a suitable laser, for either or both locations, might be an excellent if heavy second-tier ARPA-E project, worthy of yet another joint program, perhaps more like $10 million per year. Japan has announced a $20 billion program in this area, but is missing some of the necessary enabling technologies available in the US. The cost of laser construction on earth, as Livermore has found, might well be greater than the cost of laser assembly in space, if other agencies do what is needed on transportation. Some relevant material is posted at www.werbos.com/space.htm.