Towards a Rational Strategy for the Human Settlement of Space

Paul J. Werbos, Room 675
Directorate for Engineering
National Science Foundation[1]
Arlington, Virginia

Abstract

This paper revisits the core issues of space policy from the viewpoint of optimal decision theory. First it argues for a metric: maximizing the probability that humans and their technology in space someday reach what Rostow called the "economic takeoff" point where autonomous growth becomes possible, not bound by the rate of growth on earth. Next it discusses three concrete requirements to reach that point: benefits to earth which exceed costs to earth, large and diverse enough "exports" from space to earth, and advancements in technology and infrastructure. Energy from space (ES) is now one of the most promising export possibilities, based on what was learned in the last open US government effort on that topic, "JIETSSP," led jointly by NSF and NASA. I review several options for ES, and propose a new one which, while slightly riskier, offers real hope of electricity at a price that could compete with coal and fission-plus-enrichment.

Defining the Metric to Be Maximized

For most of us, space is a means to an end. Thus no metric for performance in space can perfectly represent exactly what we all would want, in the larger course of history. However, we do need a metric – a quantitative sense of where we want to go – in order to be focused and efficient in setting policy and making decisions. This section will propose a specific metric, based on the larger tradeoffs we humans face as a species.

This paper will address the challenge of space to *the human species as a whole*, not the specific role of particular agencies like NASA, NSF, DOD, UN, Chinese Academy of Sciences, etc. The Bush vision for NASA [1] is an important part of the larger whole, but here I will discuss it only as part of the larger picture. Even in recent workshops on developing the moon, NASA has been very clear that it views itself as only *one* of many players in space, and recognizes the importance of other players to making the overall activities in space truly viable.

In the short term, the future possibilities for human activities in space offer a myriad of possible scenarios. But in the long term, these various possible trajectories flow into three very distinct streams of possibility:

(1) If human technology and society do not reach a sufficient level of sustainability, the economic and political base for activity in space may gradually erode, and the

---

[1] The views herein are the personal views of the author, and totally unofficial; however, they do constitute work by a federal employee under government time.

entire enterprise – including even GPS satellites and communication satellites – may terminate, gradually but permanently, as society reaches a certain kind of static or stagnant equilibrium at a lower level of technology. Because our economic rise over the past three millennia depended so heavily on low cost, easily accessed natural resources, and on serendipitous disequilibrium in society, it may be difficult to rise again as we have in the past. (Again, this is a conceivable scenario, not a prediction. It does not specify whether humans as such continue to exist on earth or not.)

(2) Human society may reach a kind of dynamic equilibrium at a level of technology and prosperity similar to what we have today, in gross qualitative terms. In this family of scenarios, GNP might be much higher, because of information technology and entertainment, but space would still be used in a manner similar to what we see today. Space would be a site for communication satellites, GPS, and some highly expensive efforts at exploration and tourism which never reach an economic takeoff point, and remain forever as a kind of side show. In economic terms, space would be a kind of secondary sector without autonomous economic growth, exactly as in the classical dependent "banana republic"[2].

(3) Humans and their technology in space may someday reach what Rostow has called the "economic takeoff" point [2], where autonomous growth becomes possible, not bounded by the rate of growth on earth.

From the viewpoint of Bayesian utilitarianism [3,4,5,6], it is rational for each one of us to think about the question: what are the probabilities p_1, p_2, and p_3 of these sets of eventual outcomes, based on every thing that we know? But it is more rational to ask: what we can do to make p_3 larger and p_1 smaller? This paper will focus on the question of what the human race as a whole could do to maximize p_3. It will discuss the possible roles of different actors – but success in this kind of activity requires a great deal of flexibility in finding people to fill the many roles and holes that need to be filled.

This paper will not elaborate on the various reasons *why* we should try to maximize p_3. In the end, that is a subjective matter, and I have described my own reasons for it elsewhere [7,8].

As a seeker of rationality, I would not advocate that we focus *exclusively* on the goal of maximizing p_3. In this paper, I will discuss how opportunities in space link synergetically with equally fundamental long-term goals, such as the achievement of sustainability here on earth, in terms of energy and population and important streams of basic science [9] ; however, complete strategies for those sectors are beyond the scope of this paper.

Likewise, the streams or "ergodic sinks" enumerated above lead to further long-term branching. For example, even if we achieve stream (3), we are not assured that the human race could continue to survive in space if humans made the earth itself uninhabitable. We are not assured that humans will be able to reach beyond this solar system – if, in fact, that should be possible, which is not at all certain for us. This paper will say very little about these important issues, in part because we need to move into stream (3) in any case before we can actually reach its best substreams. These issues also require complex strategic thinking about technical details of physics well beyond the scope of this paper [8,9].

This paper assumes that the situation we find ourselves in today, as a species, is a kind of "crossroads situation." This is a specific kind of situation which can arise in stochastic, nonlinear dynamical systems. The mathematics behind it were summarized in [10], which described some mathematical tools which are widely regarded now as new and promising tools within the field of engineering; Dual Heuristic Programming (DHP) has recently shown that it can manage many electric power systems far better than previous methods [11]. In [10], I did not correctly translate the implications of that mathematics into policy terms because I overstressed the role of the government as a center for decision-making. Decades of experience within government have since improved my understanding of the realities of political systems; however, they have also reinforced the conclusion that we are indeed in a crossroads situation, because of the way in which historical trends are playing out on earth.

The mathematics of crossroads phenomena can be very complicated, but a simple example explains the basic idea. Consider a dynamical system made up of n possible states. At any time t, the system will be in one state i(t), where i is an integer between 1 and n. There is a "transition matrix," P_{ij}, which defines the probability that the system will be in state i at time t+1 if it was in state j at time t. Suppose that the possible states i fall into three sets – A, B and C. For every state in set A, there is some probability greater than zero that the next state will be in B or in C. But for every state in B, the only possible transition is to another state in B. For every state in C, the only possible transition is to another state in C. If you are in a state in set A, your long-term future happiness depends only on whether you finally get to B or to C.

In reality, things may be more complicated. For example, consider what happens if the probability of escaping B becomes smaller and smaller with time, comparable to the probability of a table floating off a floor due to atoms in the floor all pushing "up" together, by coincidence at the right moment. For all practical purposes, this is still a crossroads situation.

In a crossroads situation, short-term goals which seem very exciting and worthy are often like "castles in the sand" – irrelevant achievements which get totally washed away by the flow of events. The only thing which really matters in the long term, for space policy, is the choice of which stream we get washed away *to*. Performance metrics other than p_3 are a dangerous distraction, except when they represent tentative, changeable subgoals well-calculated to serve the larger goal of maximizing p_3. For example, the metric of trying to minimize the time delay between now and the time when US footprints next appear on the moon does not maximize p_3; use of that metric has recently put NASA on a course which would make p_3 indistinguishable from zero, if there were not other actors in the game and hope for mid-course corrections.

Summary of Conclusions

To reach the economic takeoff point in space, we need a more tangible picture of what scenario (3) entails. It does not require total self-sufficiency in space. Based on economic theory, it requires three major ingredients:

(1) Exports from space to earth must be as large or larger than imports to space from earth. Space must deliver more value to earth than it costs to earth; and
(2) Exports and other production in space must be large and diverse enough to rationally justify further investment in space;
(3) Technology and infrastructure in space must be advanced enough that the “input-output coefficients” for production in space, *combined with (2)*, lead to multiplier effects large enough to generate takeoff.

Humanity does not possess any assured or guaranteed path to meet all these necessary conditions. Thus rational space policy is very similar to wildcat drilling [4], in requiring a stochastic approach, a drive to “buy the most valuable information” and an ability to shift gears very quickly as that information comes in. Many of the more extreme errors in policy are based on the tendency of humans untrained in stochastic thinking to try to form “an opinion” (a deterministic prediction or rigid plan), to identify their entire personality with that opinion, and to defend it past the point of absurdity.

Requirement one: This requirement is a *long-term goal*, part of a set of goals which have not yet been achieved. There is no need to demand that we achieve it immediately, before the other requirements are met. If we consider the long-term fate of humanity, it is rational that we make a net investment now, as part of a strategy for satisfying *all three* requirements as soon as possible. The goal of increasing exports from space (requirement two) will naturally tend to satisfying requirement one in any case.

Requirement two: It is possible but unlikely that communications, GPS, media events and million-dollar tourists would be large and rich enough to satisfy requirement two. To substantially increase the probability of meeting this requirement, we need to drive to develop much larger sources of possible revenue as soon as possible, with the highest possible probability of meeting the real market requirements they entail. In my view, the most promising single option today is the sale of energy beamed down from space to earth. Five years ago, I would have considered this conclusion counterintuitive, but new results have come in from the NASA-NSF-EPRI funding effort in energy from space (JIETSSP), which I co-chaired, and new trends in world energy have underlined the great need for such a new energy source.

Other major possible sources of massive revenue include the tourism and scientific efforts which might be possible if costs were much lower, earth defense, space manufacturing, the supply of scarce elements like platinum from space, and efforts to massively improve Internet access and education for the rural (and poor) half of humanity. The rational policy for now is to probe each of these opportunities at a level of about $20-100 million per year, aggressively exploring the highest potential technologies needed to make them fully attainable. It will be crucial to keep these specific investments out of the hands of those kinds of advocates or careerist opportunists who argue that “all the problems have been solved,” who cannot understand the problems, or who overemphasize “low-risk” technologies which have little hope of ever reaching the tough requirements of the free marketplace. We also need to create institutions with the ability to scale up very quickly as soon as we are ready to do so, in an economically rational way.

This paper discusses energy from space in more detail below. Some sources of information on other possible exports from space to earth are cited on the web page of the National Space Society, www.nss.org.

Requirement three. There are a variety of important technologies and metrics for technology, related to requirement three. Many areas require improvement, from the biological side of supporting humans in space through to in situ resource utilization (ISRU), deep space transportation, telerobotics and communications [1]. But for the present, the most overwhelming threat to p_3 is the lack of effective action to reduce the long-term marginal cost per pound of lifting up material from earth to low earth orbit (LEO) – "access to space." Many observers believe the NASA's current plans for a permanent human presence on the moon will be doomed to failure, if third parties do not offer NASA less expensive launch services soon enough to allow low-cost resupply.

In the long term, many serious researchers believe that the lowest cost access to LEO will come from hypersonic vehicles exploiting plasma effects ("Ajax"), space elevators, magnetic levitation like Gerard O'Neill's "mass drivers" but on earth, lasers pushing mass up from earth, and so on [12-14]. For several years, I managed about $1 million in NSF awards aimed at studying the "Ajax" option, involving three universities and two corporations with strong connections to other agencies. But in the end, our most advanced effort in plasma hypersonics, managed by Ray Chase of Analytical Services Inc. (ANSER) concluded that: (1) none of this can become real unless we resurrect and upgrade formerly "black" technology for stable hull structures which can withstand the heat of re-entry; and (2) if we make full use of that technology, within the context of true aviation-style design, we could build a reusable rocketplane within 5-10 years [15,16] with an initial long-range marginal cost of $200 per pound – an order of magnitude less than anything in prospect either form NASA or from private sector launch services in the near future. This also turns out to be what we need in order to achieve 5-10¢/kwh electricity from space. The near-term vehicle would only have a 10 ton payload, but that does appear to be good enough for the major sources of revenue I have mentioned, if we use Chase's vision of flights from airports and assembly-line production of reusable vehicles.

Naturally, when evaluating such claims as an NSF Program Director, I have checked with many, many authoritative sources, some of which must remain confidential by law. The most startling outcome of those checks is the conclusion that *humanity is very close to losing this option forever*. It may even be too late already, but rationality demands that we do our best to fill in this *necessary* hole regardless of the difficulty. The problem is that the necessary technology for tough hull structures, though declassified, was developed under "black" programs overseen by the CIA. This technology was developed at an extremely high cost at a time when the US spent a huge amount of money in this area (for reasons related to the Cold War, made obsolete in part because of observations satellites in space), and – more important – when the US had a great abundance of the world's best engineers, highly motivated and led by patriotism to get past the petty obstacles which tend to limit our bold technological achievements today.

We may hope that the worst of those circumstances never arise again. In the meantime, essential structural test articles have been lost or destroyed, engineers with the key know-how have retired without training replacements, and essential papers and reports are buried in garages of such engineers or in the trash can. One of the key technologies, superplastic diffusion bonding for honeycomb structures, has stayed alive in Rockwell and in what remains of McDonnell-Douglas, but they need to be integrated with the actual structural and materials technologies. It has been estimated that it would cost $30 million just to re-invent the most relevant and promising structural test article which Boeing once developed, and $150 million to fully upgrade it to incorporate new versions of the relevant materials. In terms of sheer logic, it is grossly irrational for the human species to spend billions of dollars on anything else in space before this urgent investment is made.

Fortunately, Chase's concept for a near-term vehicle[15,16] would have dual use, both for space transportation and for national security. Present mission models to LEO in the US security community are enough to justify Chase's proposed investment, even without considering its other benefits. The international need for new sources of energy, and the investment capital available for advanced energy technology, might be enough to allow a solution to this essential crisis, even without flexibility at NASA or at the small space entrepreneurs. Of course, some US government role is essential, if only as part of an international consortium, because of the export-controlled nature of this technology. Many of us believe that a "new Intelsat for energy and launch services," based on a new international treaty, would be the best hope at present for getting to $200/pound-LEO, if the US government cannot afford to do so itself.

There are two main obstacles at present to building this kind of RLV with direct funds from the US government. First, the U.S. Air Force is facing liquidity problems, due to budget pressures related to the Iraq War and the general budget deficit, as well as an indication that the Senate would not be sympathetic to lease/purchase kinds of contracts like the one recently attempted to supply air tanker services from Boeing. More important, there are historic rivalries between the communities which support rockets and space, and the communities which support airplanes and hypersonics; the challenge here is to subordinate contributions from both of these groups to a larger vision which draws heavily on both areas of expertise. Nevertheless, US government funding is still our second best hope for now, after a new Intelsat. The third best hope is a private sector investment in space far larger than anything in prospect at present.

Note that our short-term "hot structures" crisis could also be solved by projects which do not immediately get us to the $200/pound-LEO which we need in the long-term. For example, development of an *upgraded*, safer and less expensive version of "Shuttle C" could use these kinds of structures. In its recent design study under Michael Griffith, NASA rejected the Shuttle C option because of turn-around costs and the danger of foam hitting delicate seams between tiles, as it did in the recent shuttle explosion; use of hot structure materials instead of those tiles would solve both problems. Likewise, research programs in hypersonics (aimed at speeds beyond Mach 10) should rationally pay for this work, if no one else does, as soon as possible – because they will need it, in order to have any hope of delivering real vehicles.

<u>**Energy From Space: Highlights**</u>

New options have arisen for generating energy in space and beaming it to earth which totally change the policy tradeoffs. The situation is very different from what I would have thought in the year 2000. The global energy situation itself has also been in flux.

Near the start of this decade, the UN-affiliated project in futures research called the Millennium Project (www.stateofthefuture.org) did a survey of science policy makers and decision makers all over the earth. They were asked: "Of all the many things that science and technology might do to improve the human condition, which would be most important and valuable?" The number one answer was: "Develop a new non-fossil non-fission source of baseload (24/7) electricity, large enough to meet all the world's needs." This report was decisive in persuading James Mink and myself of NSF to approach NASA, and join forces in a small new funding initiative in 2002 called "Joint Investigation of Enabling Technologies for Space Solar Power" (JIETSSP). At this writing, the JIETSSP solicitation (including citations to prior work) can still be found by use of the search engine at www.nsf.gov. The workshop report which helped pave the way for JIETSSP has been reposted at www.werbos.com/space.htm. John Mankins of NASA and I served as co-chairs of this effort, the last explicit funding of energy from space in the US government.

The *reason why* the international policy makers answered as they did is that they were aware of very frightening trends involving carbon dioxide emissions and also involving future nuclear proliferation (related to a growing need for enrichment and advanced reactors, and diffusion of the technology on a larger and larger scale to areas where access is not perfectly controlled). The details of climate change may be debatable, but at this writing there is near certainty that human emissions of CO_2 lead to a massive increase of acidification of the oceans – a phenomenon which killed more than 90% of the life in the oceans in the handful of times when it occurred before, in geological time. Reflective particles in the atmosphere and other easy "quick fixes" would not solve this aspect. Personally, I believe that it is grossly naïve to pretend that we are certain humans will continue to exist on earth if these trends continue. China is perhaps the only major nation on earth which has already embarked on the "coal solution;" however, they now estimate that the world supply is really good for only another 70 years or so, on that path, and they are already facing difficulties in producing and importing as much coal as they need. (Source: China Daily, June 2007.)

To achieve a global sustainable energy system [10], we need to meet *all three* of certain very challenging requirements: (1) to find enough sustainable affordable car fuel – most likely by an accelerated use of plug-in hybrid cars, which draw at least half of their energy needs from electricity, or by future cars which store energy in other forms (like heat or hydrogen) produced locally from electricity from the power grid; (2) to replace natural gas being used to generate daytime electricity, most likely by using new low-cost solar thermal energy farms linked to electric utilities; (3) to replace both coal and fission for the bulk of baseload (24/7) electricity generation, which is likely to grow as we use electricity in more and more applications.

Energy from space – unproven as it still is – is our best hope by far of meeting the third requirement, at a cost similar to what coal and fission cost today, on a scale large enough to displace them. Some say that methane gas hydrates are another major hope – but for now, they seem to involve greenhouse gas problems even worse than coal, and a supply of fuel as large as coal but far form unlimited. Wind is growing in efficiency, but few authoritative sources claim that it could meet more than about 20% of our present electricity needs. There is some hope of using low-cost solar farms on earth, and then using long-distance transmission, intelligent grid control and overnight storage to meet nighttime needs; however, it now seems unlikely that this could get as low as 10 cents per kwh, twice what coal and nuclear cost. A very large carbon+fission tax could level the playing field here, but it would be more sensible to wait until solar farms have fully penetrated the daytime electricity market, and hope that we can do better with energy from space. A *small* carbon+fission tax recycled to the sustainable energy sector might be a good mechanism to speed up technological progress, and close the small gap (if any) between the future cost of energy from space and the cost of electricity from coal or fission.

But can we really better with energy from space, compared with storing earth-based solar power?

In the late 1970's, NASA published two "reference system designs" for space solar power which were claimed to offer 5.5 cents per kwh (in 1970's dollars). At the time, I was lead analyst for long-term energy futures at the Energy Information Administration (EIA) of DOE; in that capacity, I assisted the team of Fred Koomanoff at DOE Germantown, which issued DOE's evaluation of these reports. We were not optimistic either about NASA's cost claims (which involved common pitfalls in cost estimation that we knew in great depth) or about the readiness and reliability of the technology. We did *not* argue for cancellation of research into space solar power, but certainly advocacy groups used it to promulgate Opinions which cancelled the program. Of course, advocates *favoring* space solar power have also constructed wishful stories at times.

Much later, John Mankins at NASA worked very hard to fill in the vacuum here. Through his "fresh look" and "SERT" studies, he first verified that the old reference designs *would not work*. Then he funded the development of new designs which were far more reliable and validated. He also funded far more credible life-cycle cost analyses, such as the work of Molly Macauley of Resources For the Future and work at SAIC.

In our joint technical interchange meeting of October 2002, the SAIC people reported that: (1) certain new designs developed up to that point had a very high degree of reliability on the whole, and could be costed out to a reasonable degree; (2) the best of them would still cost 17 cents per kwh; (3) 4 cents of that is the cost of microwave power beaming, including antenna, rectenna and power hookups; (4) most of the cost is proportional to the cost of transportation – with these estimates requiring $200/pound-LEO and $200 more to GEO.

Fortunately, three new design options emerged from that meeting and from discussions following up on that meeting:

(1) Richard Fork and I proposed a new "backbone" or "spinal cord laser" design, which would convert solar light directly to coherent light (no electricity steps!), and beam it down to earth as continuous radiation, at a cost probably in the 10-20 cents ballpark, without the need for a rectenna.
(2) Mankins and Marzwell proposed a new "sandwich cell" design, using high-efficiency "sandwiches" of concentrator solar cells and a thermo-electric layer, drastically improving the previous designs based on solar cells.
(3) I proposed [10] a new design, using *pulsed* light-driver lasers to ignite fusion in a new type of fuel pellet developed by John Perkins of Lawrence Livemore, producing electricity to be beamed to earth by microwave.

The Mankins/Marzwell design is close enough to previous designs that we can have reasonable confidence that John's estimate of 10 cents per kwh can be achieved, *if* we don't lose our option to get to LEO at $200/pound. James Mink – former editor of the IEEE Microwave Theory and Techniques (MTT) journal – convinced us that power beaming by microwave will be at least as safe as cell phones (and far safer than fission or coal!), even though it needs some demonstration work, and some assurance that the international community will allow some reasonable narrow frequency bands for this use. (The frequency issue would be easy to solve at present, but some regard it as a serious crisis.) The microwave community also has stated that we have a good chance to cut the power beaming cost in half or more, if we support aggressive advanced research, drawing on new technologies such as superresolution, smart antennas, lightweight materials, integral design and higher frequencies. A key part of the Mankins/Marzwell design is the use of lightweight mirrors or lenses, such as those developed and validated by Entech, which offer far more concentration than the mirrors available for solar power systems on earth. (On earth, gravity requires bulky, heavy structural elements and designs which fight gravity, unlike inflatable sorts of mirror systems which work fine in space.) Another key part is the use of new "heat pipe" technology developed only recently.

Some very respected critics have argued at times that earth solar power "must" always be cheaper than space solar power, because the ultrasafe receiving antenna on earth used in the Mankins/Marzwell design captures energy less than half of the solar light hitting the earth. This is a classic example of bad arithmetic, driven by strong emotions blinding human rationality. The energy received per acre of desert land is not a major cost driver, because the cost of desert land is a small factor both for energy from space and for rational solar farms on earth.

My design concept is riskier, but it offers a greater hope of truly deep cost reduction. The primary source of risk is the design (and assembly) of the laser. Leading laser designers have assured me that they know how to design this kind of laser, using new materials such as photonic bandgap materials. Lawrence Livermore Laboratories (LLL) have not yet finished the earth-based laser they need to actually test their pellet design, but they do operate the world's largest (Blue Gene) supercomputer at present, and their careful simulations do have a rich empirical basis behind them. I propose the use of their new D-D pellet design, primarily made of deuterium, an element present in vast quantities in the seawater of earth. When D-D pellets are used *in space*, the energy emerging from the fusion reaction is 80-90% composed of *electrical currents*. (Fusion on earth may always be more expensive than fusion, because it requires large expensive heat reaction systems, extracting energy which comes out as heat; however, vast amounts of vacuum are available for free in space, and allow us to use simple transformers instead of heat reaction systems). Crudely speaking, my design would require a laser twice as big and expensive as the Fork/Werbos design, but the D-D "afterburner" would yield a hundred times as much electricity. That multiplies the cost by per kwh by about (2/100) – implying that we have an excellent cost of reducing the cost of generation to under 1 cent per kwh. (At $200 per pound-LEO, we have to add 0.1 cents for the cost of lifting up the pellets from earth.) At these low generation costs, it is conceivable that laser transmission of this energy to earth might make economic sense, even if it is less efficient than microwaves – but we don't really know as yet.

In JIETSSP, we only had $3 million to spend, which we distributed over 12 projects, mainly based in universities and small businesses like Entech, based on an open competition for new ideas and experiments. But the review panels recommended that we fund $21 million worth of the excellent creative new ideas which we received. This is one reason why I believe this would be a reasonable minimum level of funding here. A rational approach would begin by swiftly developing powerful new simulation models (as suggested by Jon Dowling) capable of effectively evaluating new concepts for a space-based high powered laser, and then would support a wide-open competition funding many teams to try to win the competition for best competition *in simulation* (with modest laboratory-based experiments to back them up). Then, when we have a better understanding of the possible costs and feasibility, we can proceed to scale up to higher levels of technology readiness. The particular Technology Readiness Level (TRL) strategy developed by Mankins at NASA comes closer to a rational decision-theoretic approach than anything else I have ever seen in government procurement.

Conclusions

No matter what policy we adopt, we cannot *guarantee* that humans will ever be able to settle space in a sustainable, cost-effective way which makes a net contribution to earth. However, the *possibility* may be there; a rational global space policy would maximize the probability that we achieve that hope, sooner or later. Our probability of success will be greater if we try to reach sustainability as soon as possible, by focusing heavily on developing larger "exports" from space to earth, and developing the technologies and infrastructure which can reduce costs.

No matter what kind of exports we seek, we will need cheaper access to space to make it possible. We have a very good chance of getting to $200/pound-LEO in 5 to 10 years, if we act soon. But we also face a very real risk of losing that option forever if we do not give it greater priority, and learn to overcome the conflicts and rivalries which have prevented progress in the past.

Earth-launched energy from space (ES) is the leading hope for now for providing the necessary level of benefits from space to earth. I would like to see a major international commitment (starting from a few core partners) to try to have gigawatts of electricity beamed down to earth, in ten years, at a marginal cost of 10 cents per kwh or less. This would be approximately as risky as trying to go to the moon in ten years, starting form John F. Kennedy's speech. It calls out for a commitment, like Kennedy's, to take the *efficient* road – holding down costs by developing new technology and infrastructure, even though it may add a risk of a 5-year delay. Risky as it would be, it would *reduce* the risks that really matter to the humans species – risks related to nuclear proliferation as enrichment technology starts to spread, and risks related to pollution and the less-than-infinite world supply of coal.

In the past, great visionaries like Gerard O'Neill and David Criswell claimed that ES would be much cheaper (and human settlement of space more assured) if we could somehow use materials from the moon to build the kind of systems I have discussed here. I still agree with that claim. NASA's goal of developing the moon [1] is a very important part of the human space program. However, the success of that longer-term effort will depend on developing a more direct *market* for lunar products and materials, and on developing crucial infrastructures and technology. Human development of ES and other activities in earth orbit, in the mid-term future, will be an essential part of making that longer-term vision successful.

References

1. NASA, *The Vision for Space Exploration*, February 2004, www.nasa.gov/mission_pages/pdf/55583main_vision_space_exploration2.pdf
2. W.W. Rostow, *The Stages of Economic Growth*. Cambridge University Press, 1960 (paperback, 2004)
3. J.Von Neumann and O.Morgenstern, *The Theory of Games and Economic Behavior*,Princeton NJ: Princeton U. Press, 1953.
4. Howard Raiffa and Robert Schlaifer *Applied Statistical Decision Theory*, Wiley-Interscience; New Ed edition (May 15, 2000)
5. P. Werbos, Using ADP to Understand and Replicate Brain Intelligence: the Next Level Design. In R. Kozma, ed., *Neurodynamics of Higher-Level Cognition and Consciousness*, Springer 2007. (see arXiv.org, q-bio.NC/0612045.)
6. P.Werbos, Rational approaches to identifying policy objectives *Energy: The International Journal*, 15 (3/4), 1990, p.171-185
7. P. Werbos, Space, Ideology and the Soul: A Personal Journey. In R. Krone, ed., *Beyond Earth*, Apogee Books, 2006
8. www.werbos.com/space.htm
9. www.werbos.com
10. P.Werbos, Changes in global policy analysis procedures suggested by new methods of optimization, *Policy Analysis and Information Systems*,Vol.3, No.1, June 1979.
11. Jennie Si, Andrew G. Barto, Warren Buckler Powell and Don Wunsch (Editors), *Handbook of Learning and Approximate Dynamic Programming* (IEEE Press Series on Computational Intelligence), Wiley-IEEE Press, 2004
12. National Research Council, *Evaluation of the National Aerospace Initiative*, 2004, http://books.nap.edu/catalog.php?record_id=10980#toc
13. Ming Tang and R. Chase, *Hypersonics – A Periodic Quest*. In AIAA/CIRA 13th International Space Planes and Hypersonics Systems and Technologies, Capua, Italy, May 2005, pages 1-12. AIAA: AIAA-2005-3258, 2005.
14. P. Werbos, A new approach to hypersonic flight. *Actual Problems of Aviation and Aerospace Systems* (RASJ), Vol. 7, No. 1, 1999. http://www.kcn.ru/tat_en/science/ans/journals/rasj.html
15. R.Chase, Designing a responsive space launch vehicle,*Aerospace America*,May 2006.
16. R. Chase, *A Least Cost Design strategy for a Near Term Earth-to-Orbit Transportation System Based on Legacy technology*. AIAA: AIAA-2004-3731, Revision #81b.
17. P. Werbos, Reassessing The Global Energy Future: How Can We Achieve Sustainability? In *State of the Future 2004*, Millennium Project (www.stateofthefuture.org), CD ROM. See also www.werbos.com/energy.htm.