Reminiscences on the End Of the Hope for Primates in Space

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Abstract

This paper addresses the question of whether humans will ever be able to settle space in an economically sustainable way and, if there is still hope of this, where the greatest hope may lie. It reviews key developments of the last 40 years relevant to this issue, such the space shuttle, the National Aerospace Plane, the Russian Ajax effort, nonlinear control challenges, challenges of developing a "skin" to withstand re-entry, current space programs around the world, and key markets for using such vehicles, such as energy from space and space manufacturing.

1. The Beginning of the Hope

Very powerful new mathematical tools have been developed, and are still being developed, which could play a crucial role in allowing humans to achieve economically sustainable settlement of outer space. [1-5]. Unfortunately, the global dynamics of science and technology as a cultural system are coupled with larger economic and political dynamics all over the earth, in such a way that the probability of humans moving out in a permanent, serious way into space now seems only slightly larger than the probability of monkeys learning to fly on their own. There is still some hope, logically. It is not rational to give up hope when it is a matter of survival of an important endangered species (humans), but all the remaining hope requires some understanding of the larger situation. I would like to express my thanks to Dr. Kuzmina for asking me to give some reminiscences on the progress and challenges which I have been part of here.

Of course, the hope for humans to settle space in a sustainable, permanent way goes back to people long before I was born. People who read this journal probably all know about Tsiolkovsky more than I do. Even in the USA, many people remember the words: 'The earth is the cradle of mankind, but we cannot live forever in the cradle." In the US, many people remember the vision of Kraft Ehricke [22], who said that the movement of humans into space would be a great moment in the history of life in this solar system, as important of the movement of the first ocean vertebrates up onto the land.

Still, in considering this vision of Ehricke, a cartoon story has sometimes appeared in my mind. I see a beat-up talking fish in the ocean (a bit like myself in some ways) who is talking to the other fish. He says: "I have succeeded in the great ambition to visit that other world above us, to explore, and to learn what our true

* The views herein are just my personal views. They do not represent the views of any of the organizations I have worked with or for.

role in the universe really is. In that greater world, they call us sashimi…" Not exactly, but in order to be "rational" as defined by von Neumann and Morgenstern [20,21], it is important that we consider a wide range of alternative possible scenarios.

This paper will discuss a lot of earlier history, precisely because of the pressing needs we have today. Most of the space policy makers today are on track to repeat the same kinds of mistakes they have made in the past, both at the political level and at the technical level. Only by learning the lessons of past mistakes could we have much hope at all for success in the future. I will discuss this from my personal viewpoint, because that is the original database I have to work with here.

2. The Early Shuttle Era

My own journeys into the human space program began in graduate school, in the early 1970's. Actually, I saw the first moon landing on TV in 1969, on a television in Ann Arbor, Michigan. In 1971, when I worked at the Bendix Office of National Security Studies, in Ann Arbor, data from the latest moon landings came right back from space to the computer I was using for my summer job. I remember this well, because the computer operators gave priority to the program I was developing myself. At that time, I had developed a concept later called "Granger causality," and was using it to disentangle the web of causation for variables describing the state of the war in Vietnam. My early papers on this concept were not published, because they were classified; however, on my 21st birthday, in September 1971, they flew me to present the findings to the number 6 and number 3 people in the Pentagon (Thayer and Einthoven), and they had a major impact on the changes which extracted the US from that war.

Back at Harvard that year, I learned of the then recent seminal NASA reports by George Mueller of the Goddard Space Flight Center (GSFC) in Greenbelt, Maryland [6]. Muller's reports demonstrated a level of careful analysis, vision and detail which was already more advanced than most of what I see in the space area today. He understood, intuitively, that the hopes for humans in space depend very directly on a key number: the cost of access to space, which I think of as "$/kg-LEO." Dollars per kilogram, for mass conveyed to Low Earth Orbit.

Mueller showed that the actual fuel cost of getting to earth orbit is much, much lower than the cost we were paying for the Apollo program. He performed an in-depth analysis of the many sources of cost in traditional 1960's launch technology. Above all, he developed a comprehensive strategy for how to minimize that cost. He performed tradeoff studies for airplane-like reusable launch vehicles, two stage to orbit (TSTO) and single stage to orbit (SSTO). He also provided a concept of operations at airports, to minimize the cost. These NASA reports were widely available, and I read them in the Harvard library.

Mueller's reports were exciting and electrifying to me, because of my own previous efforts to understand the dynamics of historical processes and the role of key technologies. I recognized immediately why this breakthrough would be crucial in opening up a viable new economic frontier in space [4], and in creating a new means of production with profound implications for the social and political level of human life. I wrote and distributed a one-page advertisement on this, and created a new recognized student organization at Harvard, the Harvard Committee for a Space Economy (HCSE).

HCSE was a remarkably entertaining group of people. All of the dozen or so active members had degrees (earned or being earned) BOTH in natural science or technology, and in social sciences. One of the members was famous for having built a workable intercontinental class missile in his backyard when at high school, and was then working for GE while at Harvard. Our discussions for future options in space were highly creative, well anchored in the political and economic context, and free from the usual inhibitions caused by narrow intellectual backgrounds and anxieties about next week's deliverables. We mainly discussed three pillars of future economic settlement of space: (1) Mueller's space shuttle proposal; (2) space manufacturing, with some discussion of a few other possible markets; and (3) materials to be extracted from asteroids, where it is not necessary to waste so much money and energy to fight a gravity well.

In 1972, my closest friend in Harvard at the time invited me to ride with him in his car to the Republican national convention, where it was planned to nominate Richard Nixon, who promised to get past the now obsolete Apollo era of "flags and footprints" but also promised to create a new renaissance in space (similar to what Brezhnev had promised). But something very strange happened along the way.

My friend asked me to stop along the way at Dupont Circle in Washington DC, where his girl friend had an apartment. Because of intense and unexpected issues between him and his girl friend, I found myself locked alone in her apartment for many, many hours. There was nothing to do there, except perhaps to read a book – but she only had about ten books on the shelves – a Bible, a dictionary, and some other things of no interest at all.

A Christian Fundamentalist friend in the Harvard mathematics department had been urging me for years to read the Book of Revelations. Based on how he talked about the book, I never had much interest. But I did like to read science fiction, and this was the closest I could find in that apartment. So I spent a few hours reading it very closely.. and once again I was very surprised. In some cases, I remembered the Latin words close to some of the English text, and could see some immediate associations my friend was clearly unaware of. I was amazed to see the close relation between parts of this book and a paper I had written the week before. There was a city floating the heavens, like what we wanted to build. There was a kind of "ark" in the heavens, which reminded me of Mueller's idea for a space shuttle. There were a number of parallels I would not want to discuss here. And there was discussion of about twelve "crystals," which sounded a lot like some of the proposals I had seen I visited the GE Valley Forge space manufacturing center. I checked the dictionary, and found a one-to-one match, with just one exception.

We never made it to the Republican convention. After the interaction with his girl friend, my friend just drove us back to Harvard. At Harvard, I asked the GE guy about the one exception: "Is there any use for THIS crystal?" He suddenly looked worried, looked around for people listening, and asked: "How did you find out about that one? It is not supposed to be possible. If you can tell us where you read about this, maybe we can do something to limit its distribution." I tried hard to contain my laughter, and calmly said, "No, I do not think that that would be so realistic in this case."

We did elect Nixon in 1972. Nixon did keep his promise to replace Apollo with a new thrust based on the concept of space shuttle, originated by Mueller. But Nixon had "friends." He had friends in Utah who believed in solid fuel rockets, and who wanted to make money selling them. He did not think it would do any real harm to twist Mueller's plan a little to make room for them.

After Nixon's election, half of the press reported that a momentous decision about the future of NASA was made by the Office of Management and Budgets (OMB). In comparing Mueller's design with Nixon's tweak, they agreed that Mueller's design would lead to much less cost per flight, but they argued that it would entail more money and risk to develop. Comparing operations cost with development cost, and assuming a very limited "mission model" for the future of NASA, they argued that we would save more money by funding the Nixon tweak instead of Mueller's original proposal.

Half the press was much more cynical, and suggested that the OMB study was basically just a whitewash for a decision made by Nixon himself, based more on sources of campaign funds than on long-term needs. Nevertheless, the OMB study set a precedent and established a methodology. In actuality, the only justification for building a new launch vehicle was to lower costs, in order to increase the probability that humans really could expand their economic activities in space, which is simply not plausible when it costs $2,000-$20,000 per kg LEO. But myopic approaches to decision making essentially do not consider such hopes, and are effective means of preventing them from ever becoming real.

The myopic approach to decision making creates a severe "chicken and egg" problem. In the US, "chicken and egg' refers to the old question: "which comes first, the chicken or the egg?" If an egg must be present before a chicken can be born, but a chicken must be present to lay an egg, which could be first? If we need a big mission model, with intensive applications, before we can begin to build low cost transportation to LEO, but we need low cost transportation before large-scale applications are economically justified, which can come first? How can we ever escape the false dilemma created by myopic thinking?

The press reported that OMB also considered another factor, which did not sound so clear to me at the time. They reported that OMB was also worried about risk. Mueller's approach, they said, depended very heavily on new technology for hot structures, which was new and therefore risky, which had been developed in classified research. History has shown that the "safe" path of relying instead on tiles and foam and o-rings in solid rocket motors was a great source of risk itself. I have seen many cases in other domains where people used older technology because they thought that "old" means "safe," when in fact it is the opposite, simply because the older technologies have less ability to withstand the challenges of the application. (For example, some at Ford have blamed the Firestone tire debacle on a decision by Ford lawyers to insist on a "conservative" control technology at an earlier time.)

The Richard Nixon tweak became the space shuttle. Nixon also cut back on the overall NASA budget.   In 1972, I also decided to divert more of my personal energy to solving some important problems in applied mathematics, for my PhD thesis (reprinted in [7]). I turned over control of HCSE to Mark Hopkins, who was vice-president of it before then. HCSE did not last long in its initial form after that, but Mark Hopkins then worked with others like Keith Henson to establish the L-5 Society, which played an important role in the next chapter.

3. The L-5 Era and Space Solar Power (SSP)

The movement for human settlement of space was energized to a huge degree by seminal books and papers (such as [8,9]) by Professor Gerard K. O'Neill of Princeton. He proposed a plan to build huge comfortable, green cities in the L-5 orbit around the earth, to be paid for by building huge solar power stations to beam electricity to earth by microwave. He proposed a plan to extract material from the moon, to build these cities and power stations at a cost far less than would be needed if the same material were to come from earth. In broad terms, this is still a reasonable vision, which still merits great attention, and still attracts great support from many in the aerospace community. O'Neill also argued that large-scale use of the space shuttle would give costs low enough to meet the needs of his overall plan to generate electricity at a cost which competes with our present sources of electricity from fossil fuel; that was his way to address the chicken and egg problem.

In 1975, I joined with my student Gary Barnhart and a friend of his to start a new space organization, the Maryland Alliance for Space Colonization (MASC), based in the University of Maryland, which at its peak had more members than either the Young Republicans or the Young Democrats. The most effective draw was to arrange a major talk by Gerard O'Neill at the Smithsonian Air and Space Museum, and car pools to get people to the talk.

Support for this general vision grew stronger and stronger in many sectors, especially as boycotts of oil in the Middle East made more and more people worry about energy. The futurist author Barbara Hubbard bought a mansion on Porter Street in Washington DC, and ran a salon which reminded me of stories of politics in older days. She brought together key representatives from many space societies, including me representing L-5 and MASC, Jerome Glenn representing a group called SYNCON, Carol Rosin representing Werner Von Braun (and, I think, the National Space Institute), and others. She arranged for a huge briefing in a huge room in the Capitol Building proper, where O'Neill presented his vision and Carol Rosin was also very visible. Not so long after that, Congress did agree to fund a $25 million study on space solar power (SSP), to be divided between NASA and the Department of Energy (DOE).

Soon after this effort began, I was contacted by a key manager at DOE, who had previously hired one of my students (Jim Titus) who told him about my mathematical methods for political and economic forecasting. After seeing how effectively I had accidentally uncovered major gaps in global conflict models used by the Department of Defense [10], he offered me a job in the tiny group at DOE responsible for evaluating all energy models across DOE and its competitors, and responsible for informing the Secretary of Energy about what we really know about the energy future. I gave up academia because I felt that this was too important a call to ignore. As a result, I ended up providing technical support and liaison to Fred Koomanoff of the DOE Office of Energy Sciences, which was responsible for the DOE part of the SSP effort. At about the same time, I was elected as a Regional Director of L-5, which Mark Hopkins was effectively running throughout; he appointed me to be the L-5 representative to the US government. This gave me multiple sources of information about the SSP effort. However, both activities together used far less of my time than the main part of the DOE job, which spanned the entire spectrum from decoding the dense FORTRAN code of many major energy models, through to the mathematical economics of energy markets, through to large-scale policy questions.

The NASA part of the SSP effort funded three design efforts for SSP. By NASA's rules, two of these efforts (Lockheed-centered and Boeing-centered, as best I recall) were restricted to SSP designs to be launched totally from earth. These were later called the "NASA Reference Study Designs," and are widely available from NASA still. The total price tag they proposed was huge, on the order of $1 trillion over many years to produce a huge amount of power. However, they estimated that the ultimate cost of the electricity would be only 5.5 cents per kilowatt-hour (kwh), in 1970's prices. There was also a third major study, deliberately publicized less, which evaluated the original vision of O'Neill, and concluded that it would indeed allow a huge saving in cost compared to the Reference designs.

The L-5 community was quite upset that this third study did not receive more attention. Why lose the option to save money, and finally overcome the barriers to human settlement of space, both at the same time? Why suppress this option? There were slides by Stan Sadin of NASA Headquarters conveying this decision, which stimulated some people to suggest that "Sadin" sounds like "Satan." As I think back on this, I cannot help noticing that the two reference system designs would have meant more money for Lockheed or Boeing… IF they had been funded. Incentives to minimize costs are not always as strong as they should be in government contracting. But as I look back, I also remember how I have learned to be somewhat skeptical of O'Neill's initial cost estimates.

The DOE study, led by Koomanoff, was much less optimistic than the NASA study. It pointed to many technical questions and unproven assumptions in the Reference System designs. We did not argue that funding for SSP should be terminated; rather, we argued that key uncertainties needed to be addressed head-on, and new R&D conducted, before people rush ahead to deployment. Unfortunately, irrational emotions opposed to SSP and space in general were even stronger and even crazier than the emotions of those who simply Believed the Reference system study or O'Neill's book. Groups such as Ralph Nader's basically quoted us out of context, and introduced red herrings of their own, and the program was killed.

It was very disturbing to see allegedly mainstream evaluations in major scientific journals, which would compare SSP with earth-based solar power based on cost estimates which assumed 1990 predicted costs for photovoltaics on earth, and 1960 costs for photovolatics to be used in space. It was also somewhat shocking to see some energy "experts" proposing that we should make a choice between these two options based on the amount of desert land they would require per kilowatt of peak power; this is a grossly irrational approach to analysis for many reasons, such as the fact that the cost of desert land is a very minor cost factor for both technologies.

The greatest logical error by opponents and advocates of SSP was the assumption that the world must choose just one best source of electricity, for the time when fossil fuels run out or become too expensive. The best plausible hope for the future at present is that earth-based solar farms and energy from space (including SSP) may both someday provide steady, reliable electricity at a cost of 10 cents per kwh, enough to meet both the baseload 24-hour demand and peak daytime demand without major additional costs to the electric power grid. SSP is a complex and risky technology which may take some time to perfect, even if we begin aggressively now. However, without SSP, we will need to make huge changes in the operation of electric power grids, which are also complex and difficult [11]. Because our lives now depend on maintaining a safe and reliable source of electricity, the safest policy is to work hard on both possibilities. It is also interesting to consider how the new computer tools and approaches used to upgrade electric power markets might be used to achieve more stability and optimality in some other markets.

At about 1981, as my work load at DOE grew, I turned over the job of L-5 representative to a colleague of mine at DOE, Gary Oleson. Gary Oleson worked closely with Jim Muncy, who worked with Republican Congressman Rohrabacher, who remains a key force in space policy and a strong advocate of US-Russian collaboration. Oleson and Hopkins were key people in organizing a merger of L-5 with Von Braun's National Space Institute, to form the National Space Society (NSS), a major force even today. Likewise, as NASA remained unresponsive to O'Neill's vision, some of our friends turned to DOD, and put a spin on the High Frontier concept to try to get money from people interested in missile defense (but more interested in expanding federal funding). The people interested in missile defense were quite sincere about being open to partnership with the Soviet Union, so long as they would still get the money.

4. The creation of NASP

While I was still L-5 representative to the federal government, and studied the details of the various SSP proposals, I realized more and more how important it was to return to George Mueller's original agenda, to try to minimize $/kg-LEO. I discussed this with Dana Andrews of Boeing [19], who was part of the L-5 movement but also a leading expert on space transportation, aware of Boeing's work on the Trans Atmospheric Vehicle program. (By the way, my old security clearance lapsed before I started work at the Department of Energy; thus I have never had access to any classified material through my entire career in the federal government, except for one brief conversation about neural networks unrelated to anything in this paper.) I was also excited about the possibility of using airbreathing vehicles instead of rockets to lower costs even more than Mueller's original proposal.

A friend of mine from MASC days, Bruce Friedman of B'Nai Brith and the Naval Academy, played a crucial role in arranging discussions with a few key offices in Washington, to follow up on this interest.

First we had a discussion with a local Republican office in Congress, to create at least one local supporter, to call for serious attention to the issue. Then we had a decisive meeting with Jonis, the number two guy at Reagan's Strategic Defense Initiative. We noted that if we really could reduce $/kg-LEO by a factor of ten, then for the same dollars we could afford to orbit ten times the payload. After checking the math, Jonis took over (with only a bit part for us), and things went very well. It was a moment of really enormous excitement when President Reagan announced his new "Orient Express" or "National Aerospace Program (NASP)" priority, in his State of the Union speech. It seemed as if the political problem was finally solved, and we could finally move on to working out the technical details and making it real.

NASP was set up as an interagency effort, run by a Joint Projects Office (JPO) based at the Wright Patterson Air Force Base (WPAFB) in Dayton, Ohio. NASA and the Air Force both had especially prominent roles.

5. NASP: From start to finish

From 1988 to 1989, I moved from DOE to the Engineering Directorate (ENG) of the National Science Foundation (NSF), to take over new programs in neural networks and in Emerging Technology Initiation (ETI). One of my first actions was to set up a workshop in New Hampshire on the use of artificial neural networks to solve difficult problems in control, which led to a book [12] intended to focus my funding and create a new field of "neurocontrol" combining neural networks and control theory.

At that time, I also attended the IEEE Intelligent Control Conference in Virginia, where I met Bob Pap, who had just set up a new company on paper, called Accurate Automation Corporation (AAC), based in Chattanooga, Tennessee. I remember vividly eating Chinese chicken takeout at the food court in the Rosslyn metro station, for my first conversation with Pap and one of his people at that place. I mentioned how one of my new colleagues at NSF, Dr. George Lea, had told me that he was very skeptical about the success of NASP, because of the challenging control problems. Lea had said: "This kind of vehicle requires keeping a scramjet engine operating in a very narrow sweet spot of about 1%, which keeps moving, as the coupled system of engine and body is buffeted through a stochastic atmosphere. It is an incredibly difficult dynamic stochastic nonlinear control problem. We simply do not have control methods which can handle such problems."

But I did have such methods. I asked Pap whether he would be willing and able to prove this, by making the connection between new neural network control methods and NASP. He said yes. I soon signed off on the small grant which was the first funding of AAC, and which started that company on a course to tremendous growth for many years after that. (At one point, AAC made it to the INC 500 list of fastest growing companies in America.)

Pap and I both understood that the insertion of new methods demanded that we build a partnership with the NASP JPO, which led that effort. Thus we both had some important visits to WPAFB. (I still remember my great excitement one time when I saw a big plaque "WPAFB INNS" where I stayed one time. I thought "they are very serious about collaboration with the International Neural Network Society." But no, it was just one of many inns.)

On the first visit, with Pap, I asked them what kind of benchmark control challenge they could offer, which Pap could use to prove himself worthy of their funding, which he would work on under NSF funding. They mentioned a difficult nonlinear trajectory optimization problem, for which they had spent millions of dollars and several years at a major aerospace company, to find a solution which was still not quite perfect. The equations were normal airbreathing vehicle equations, but the parameters, the speed, and the required maneuvers were all beyond what humans could handle and beyond more conventional controllers.

Pap found a very quick and easy way to do well in that application. He translated the control problem and tricky maneuvers into a video game, played at a speed slowed down enough that a human could cope with it. Because humans enjoyed this game, many of them worked hard to get to the highest level, and meet the tough NASP requirements at slow speed. Only two of them succeeded, but the computer recorded how those humans behaved. He trained a simple recurrent neural network to copy the human behavior, and then showed that he could easily run that neural network at the speed of the actual system. For a small fraction of the cost and time, he met the specifications. In essence, he used the strategy now called "behavioral cloning" which I first proposed in [12]. After that, his company built a stronger partnership with the University of Tennessee. On his own, and through partnerships, Pap obtained a series of grants and contracts from NSF, from NASP, from Joel Davis of the Office of Naval Research, from NASA and from other sources. A more complete account of these methods may be found in [1].

For the very next stage of the AAC work, I was hoping to get a mathematical representation of the original challenge suggested to us by George Lea. This required coupling the body, the engine and the environment of the vehicle. I was somewhat disappointed when I learned that these aspects were being handled by separate activities, which were planned to be integrated some time in future years. I was also quite worried about what this division of effort would do to the ultimate success of the program, if the pieces were not designed to allow a solution of the combined control challenge. Therefore I pushed Pap and JPO to try to do more integrated modeling, to set the stage for addressing Lea's challenge.

In parallel with this, I met two energetic and creative engineers from McDonell-Douglas of St. Louis (McAir), David White and Don Sofge, at the International Joint Conference on Neural Networks (IJCNN). We discussed how neural networks could be applied to a variety of challenges at McAir, including challenges with their work on NASP. In addition to those discussions, we agreed to hold a new joint workshop on neural networks for control at McAir in Missouri, which led up to a new book [1] which is still the best source in existence for parts of the new technology. I have posted my own chapters of the book on the web, as US law allows me to do, but one must buy the book to see chapters by White and Sofge and others, such as the benchmark challenge in hypersonics control based on NASP, and the breakthrough in low-cost manufacturing of carbon-carbon thermoplastic parts (which enables new vehicles such as the Boeing Dreamliner).

One year, when I heard from White that continued funding for NASP was in doubt, I volunteered to join the NSS group visiting the office of Senator Mikulski for their annual space policy discussion. Kevin Kelley represented Senator Mikulski. Kelly said he had grave doubts about NASP, because of the way the procurement people had somehow cut out Boeing, the company whose contributions would be most essential to the success of the program. There was also some concern that NASP was falling into a common set of overspecialized activities, which some called "paper airplanes," without enough systems-level reality testing. I remembered what Dana Andrews had said about TAV, and wondered. In retrospect, I wish I had done more than just wonder. I wish we had worked harder with Kelley to solve the problem back when it would have been easier.

Back in St. Louis, I heard how all the contractors were being asked to contribute to a new kind of combined systems analysis. And then the devastating news went out: "the vehicle will not close on weight." The official NASP design from the start had relied on a "Thermal Protection System" (TPS) using active cooling by slush hydrogen to keep the vehicle from melting on re-entry. This was part of the "Copper Canyon" vision which key aerospace lobbyists had been advocating, whom Reagan had turned to when real funds became available. Even now, active TPS based on slush hydrogen remains a kind of article of (convenient) religion for many people trying to get more money for their research in hypersonics. But a working TPS based on this principle was simply too heavy. The resulting payload in going to LEO would be negative. Oops. This was one of the many pieces of experience which remind me of the ancient poem.. "For want of a nail … the Kingdom was lost."

White was able to imagine one possible way out. With a complex system of smaller scale valves (such as nanotechnology valves discussed by Ken Gabriel at the time), and a very advanced form of neural network control, it seemed possible to reduce the TPS weight dramatically. Tests were just starting of this option at about the time when NASP was cancelled. Perhaps it would have succeeded, but it involved new technologies far more aggressive and risky than others we have been discussing, and it would have introduced many unnecessary risks.

Just this past year, a colleague in NSS pointed me to the official NASA history of the NASP era [13]. It was a great shock to learn that the people who implemented NASP began, as a first stage, to cancel the TAV program. If I had expected that, with what I know now, I would probably have skipped NASP altogether. Valuable as it was, NASP was not worth the cancellation of TAV.

6. Traditional Hypersonics After NASP

The cancellation of NASP, after the cancellation of TAV, marked a low point in US activities in hypersonics.

In the last stages of NASP, when there was pressure to demonstrate some kind of holistic vehicle, people looked around to see who had been working on NASP, who had been trying to integrate the pieces. Ironically, AAC suddenly occupied a central role for a short time, because that was the direction I had been pushing them into.

At one point, Pap put together a kind of bottom-up hypersonics program, by combining small grants, mainly from Small Business Innovation Research (SBIR) from multiple agencies, to add up to an entire vehicle. His LoFlyte vehicle was briefly the flagship of active, operational hypersonics in the US. Since I was one of the funding agents, I had a number of visits to Chattanooga and to two major Hypersonics conferences led by the American Institute for Aeronautics and Astronautics (AIAA). These conferences were crucial for me in learning who really understands what, in general, in this field; that in turn was crucial to making full use of the NSF peer review system, in deciding what to fund in these areas.

LoFlyte was a scaled down (1/3 size?) flying version of a "waverider" airframe, based on work and guidance from Isaiah Blankson of NASA headquarters and his grantee, Prof. Mark Lewis of the University of Maryland. Pap could only manage a small engine for this airframe, but he argued that the main control challenge here is to overcome the instabilities which a Mach 5.5 airframe would have at low speeds. He demonstrated that he could overcome those instabilities by using neural network control, both in simulation and in a whole suite of test flights out of Edwards Air Force Base and elsewhere. Reporting on his NSF grant, he sent success story videos to NSF which were presented at the highest level of the agency.

Some of the further developments at this stage would be somewhat embarrassing to key people. As a result, I have posted the most important (and amusing) details on the web [14], with important names shifted or deleted.

Hypersonics research has certainly continued after that time, especially in relation to research on scramjet engines all over the world. There are some efforts whose primary objective is simply to keep the money flowing, without so much serious attention to the coupling between funding and ultimate outcomes. Scramjet engines and waverider design may be necessary parts of air-breathing RLVs, but they are not sufficient for overall success. Overall success is now very much in doubt, for many reasons, some of which I will discuss.

7. The Ajax Period

Circa 2000, there was growing interest in the US regarding Russia's "Ajax" concept for hypersonics. The idea was to upgrade airbreathing hypersonics (like NASP) by using electromagnetic fields to ionize the air fields near the vehicle, to reduce drag and to allow operation of scramjet or ramjet engines at speeds which would normally shut them down, and to use reformation of complex fuels instead of slush TPS to cool the leading edges of a vehicle.

Three government agencies sent people both to AIAA conferences and then to Russia itself, to learn more about the technology, and to build partnerships with Russian engineers. For example, NSF sent Prof. Murthy of Purdue to make a report. After that, they funded programs to try to test the more controversial aspects of the "Ajax" theory of what could be done. The large efforts, in traditional large stakeholder organizations, generally reported that the claims could not be replicated, and cast doubt on the entire enterprise. However, given the size of vested interests and pride, and the social "heresy effect," it is not clear whether they tried quite as hard as they might have. After extensive peer review, and rejection of some proposals by people widely respected in the DOD funding community, the program I run funded two important and successful efforts: (1) a small effort to Stevens University, with a subcontract to AAC; and (2) a large effort to ANSER corporation, under Ramon Chase, with a major subcontract to Richard Miles of Princeton and a number of smaller subcontracts, including one to Mark Lewis. The latter program hosted quarterly technical review workshops, with presentations from other centers.

I have previously published a paper in this journal [2] summarizing the main technical lessons learned, and new opportunities, towards the late stages of these projects. Chase himself had acquired incredibly important unique knowledge of hypersonics through the years, in part from his role in successful on-budget earlier efforts to develop the highest speed airbreathing vehicles in human history.

The work by Stevens and by ANSER was both important and successful, but the two groups had very competing visions and personalities, which made cooperation difficult. Ganguly of WPAFB achieved the greatest success that I know of in core Ajax issues; he was able to combine what was learned from both of these efforts, and many others, and develop a more unifying model as needed for any really solid engineering. AAC's success was enough on its own to provide a basis for further practical work, aimed at speeds far less than what RLVs required, funded by other agencies. AAC made enough progress to believe it was ready to make a breakthrough in a huge DOD procurement; however, after they "bet the company" and achieved remarkable technical specifications, politics intruded. They felt they might have won a lawsuit in court, but not in a way that would really be good for their long-term survival. Since then, I have heard relatively little from that company.

The ANSER work ended in a less graphic way. The award received a small follow-on from WPAFB, which took over management. Some of the technical issues which we had been probing in our term no longer received the same attention. In October 2003, funding rates in the division I work at in NSF dropped very abruptly to only 10%, forcing abandonment of a number of key technologies, from plasma hypersonics to quantum learning. But technical factors also entered the story, as I will now explain.

8. From JIETSSP to the Challenges of the Present

While all this work on hypersonics was going on, there was also renewed interest in space solar power (SSP). In the 1990s, perhaps in part due to the efforts of

Congressman Rohrabacher, NASA was funded for a "fresh look" study of SSP, led by John Mankins of NASA Headquarters. Mankins' program, SERT, checked some of the issues raised in the earlier DOE evaluation, and verified that the earlier Reference Designs would have failed if they had been naively funded (just as NASP had failed). However, SERT also developed an entire suite of more viable designs, which passed extensive testing.

In the late 1990's, I contacted Mankins on behalf of NSF, and proposed that we hold a joint workshop on the use of advanced learning technologies like neural networks as a possible enabler for low-cost SSP. The NSF-NASA workshop on that topic was held in April 2000, in Crystal City, Virginia. The workshop report is posted at www.werbos.com/space.htm, along with some related materials. Among the key materials was a table by Ivan Bekey of NASA giving aggressive but probably achievable technology parameters, needed to make SSP inexpensive enough to be really useful in world energy markets. About half the workshop focused on earth-launched options, and about half on options based on lunar resources, in spirit of Gerard O'Neill or in the spirit of David Criswell, who attended the workshop and proposed generating power on the moon and beaming it from there to relay points to the earth. Professor George Bekey of USC, a well-known expert on robotics, chaired the workshop.

The key need for machine learning in earth-launched SSP is to allow greater use of robots in assembling large space structures, to hold down cost. It is too early to make certain key technical decisions on the details. In general, we now envision human operators in warm space stations, each controlling about ten semiautonomous robots, through teleautonomy, to perform assembly and maintenance, building on what has been learned from real-world mining and construction robotics.

By 2002, more information had become available [15] supporting hope for SSP. Jerome Glenn, then the director of the UN University Millennium Project (www.stateofthefuture.org, not to be confused with the later UN Millennium Project), had been commissioned to do a study of science and technology policy world wide. When asked to name the most important new contribution that science and technology might make in the coming century, the most votes went to the topic: "affordable large-scale nonfossil, nonfission source of baseload electricity." SSP was by far the most serious candidate for that particular requirement. Also, the National Academy of Sciences conducted an extensive review of the new SSP concepts from SERT, and published a report (publicly available on the web) supporting the area.

Building on that support in 2002, the Engineering Directorate of NSF authorized me and a colleague (Dr. James Mink, a leading expert in microwave technology) to visit NASA and propose a new joint funding program, to carry on past the recently cancelled SERT program. NSF did not endorse SSP as such, but agreed that basic research aimed at developed the required enabling technologies would be of great value in any case. I then led this activity [15], in a 50-50 partnership with NASA, with me taking on the actual operational leadership.
The great funding collapse of 2003 prevented a continuation of this activity, but many important things were learned from it. Above all, I learned that the chances of success in getting to affordable energy from SSP would be much greater than I had

guessed before I looked deeper into it, IF we do the right things. One of the most important of these things would be truly competent work on reusable launch vehicles (RLV).

In this period, Ramon Chase provided me with a kind of final report on the activity we had funded through ANSER on plasma hypersonics. In order to show how much Ajax would really be worth, he developed a kind of final assessment of cost to orbit under proper Ajax varieties, under conventional airbreathing hypersonics, and under a "near term" straw man representing the lowest costs we could achieve with rockets with almost no risk.

The main conclusions at the end of this work were electrifying to me:

(1) With a best practice use of rockets (using design principles from high speed aircraft), it would have been possible to build "near term vehicles" able to reach $400/kg-LEO, within about 5 years, with at least 90% chance of success, relying entirely on proven off-the-shelf technology for which a substantial database already exists. That figure, $400/kg-LEO, is the same as Ivan Bekey's target from our 2000 workshop for affordable SSP.

(2) One key requirement for the near term vehicle would be the use of a specific "hot structures" technology developed by Boeing under the TAV and RASV programs. That was the only technology ever developed on earth (that we knew about) which passed the stringent three-way tests of ability to withstand re-entry conducted by the only test facility in the US capable of such tests, located at WPAFB. This eliminates the need for active TPS. Furthermore, the serious CFD evaluations of air-breathing hypersonics at Princeton showed that such re-entry capabilities are essential, even when electromagnetic fields and waverider design are used to reduce the stresses.

(3) The Boeing hot structures technology would soon become seriously endangered. The key test article was already lost. Many key people had retired, and many key designs were probably now just technical papers stored in people's garages at risk of being lost forever.

(4) The systems level design developed by Chase himself also relied on unique skills and background and concepts, which I later checked in many ways. In the ANSER/Princeton project, it was essential that I be there to ask key questions, but I have often worried about the implications of Chase aging and possibly retiring before the world receives the full benefit of his understanding of this design.

Given these facts, and given the funding situation at NSF, I decided not to fund a penny more on hypersonics until and unless a way is found to rescue the hot structures technology. It is probably best that I not describe all the follow-on discussions here. Chase published a brief summary of his design in Aerospace America [17], but also wrote many AIAA papers and technical reports with extensive details.

Perhaps one of those later discussions really should be mentioned. On a visit to WPAFB, to hand off the final stages of the ANSER project, I had an opportunity to visit the testing lab there, run by a prominent member of the IEEE Control Society. There were hundreds and hundreds of test articles in view, including the latest advanced materials under development by and for NASA. There was also a big "black room" of materials I couldn't see. The lab director was quite proud that this is the test facility in the US capable of testing for all three types of stressed experienced under re-entry – thermal, mechanical and (acoustic?).

I asked him: "Ray Chase tells me that out of all the many thousands of articles you have tested here, there was only one which passed all the tests for ability for repeated re-entry to earth – the Boeing RASV hot structures article. Is that true?" I knew that Chase had documented his analysis quite diligently, and gone well past the executive summary of the newly declassified testing effort, but it was still quite overwhelming to see how many groups had worked so hard to address this challenge. The lab director replied: "Yes, it is true. Only one has passed the test. And any really serious candidate in the US would have go through this lab." This was before the recent X37B tests, so that now I would expect the number has risen to two. Nevertheless, one should remember that all the makers of those other test articles have a strong incentive to try to sell their products. This visit had a strong impact on how I see these challenges.

In September 2002, the Ohio Aerospace Institute hosted a joint technical interchange meeting for JIETSSP. Among other things, SAIC presented a full life-cycle cost analysis for various SSP designs previously developed under Mankins' SERT program. The lowest cost design was projected to cost 17 cents per kwh, if Ivan Bekey's technology parameters were achieved. This highlights the importance of getting to $400/kg-LEO. But it also highlights the importance of continued work to improve the designs, such as what we funded through JIETSSP. At that time, it seemed that the best design involved the use of lightweight lenses developed (and well tested) by Entech to concentrate light up to 500X in space, and to focus it onto "sandwiches" of solar cell and heat-to-electricity layers of new chips. Neville Marzwell of NASA JPL had played a key role in developing that concept. Mankins felt that the cost could be reduced to 10 cents per kwh by taking advantage of new heat pipe technology; however, the funding ran out before the new design concept could be worked out, tested and evaluated. About half the weight of those designs was for power management and distribution; there have been huge breakthroughs in power electronics in recent years, based on new chips and higher frequency power transmission [11], but there has been no funding to go back and see how that could reduce the weight and cost of the earlier designs. At the time of JIETSSP, I also proposed a version of Space Fusion Systems (SFS) as an alternative to develop in parallel with SSP; this would carry higher risk, but would offer hope of much lower costs per kwh, if we meet the Ivan Bekey technology specifications such as $400/kg-LEO. Kaya of Japan has developed another interesting option, using well-tested phased array power beaming, which is receiving serious funding in Japan but has not yet been costed out, so far as I know[17].

In 2009 and 2010, more and more people in Congress began to understand that the "Apollo on steroids" program for NASA was leading to vast cost overruns for expendable rockets, which would be too expensive to allow us to afford a vigorous space program. The Obama Administration cancelled these rockets. Lori Garver, a former Executive Director of NSS, played a central role in representing the Administration on these issues. Unfortunately, many Washington lobbyists had been looking forward to the huge additional revenue which those cost overruns would have produced, and the resulting jobs suitable for low-skilled labor in key powerful states like Alabama and Georgia. Instead of moving on, they have begun to wage a kind of last-ditch war, attacking lower-cost suppliers of expendable rockets, and making it difficult to address the more urgent problems of a real advance to $400/kg-LEO.

Of course, the US military did not want to pay excessive costs for every ton it lifts into space. From the limited press coverage of the X37B program, it is clear that there were people in the US military smart enough to understand the need for reusable winged vehicles. The press coverage appears to say that the X37B has now become the second system in US history to pass all those tests at WPAFB. As I write this, in late 2011, talk has begun to appear in the press about X37B or an expanded X37B being used to carry astronauts to the International Space Station [18]. It seems likely that the extreme secrecy about X37B has been mainly due to a desire to avoid conflict with very powerful Washington lobbyists, who would be afraid of undermining their last-ditch effort to get money for a new expendable heavy lift vehicle.

The initial press coverage of the X37B seemed very exciting to me at first, but certain realities then intruded. The vehicle is covered with a combination of "second generation shuttle tiles" and of very hard carbon-carbon parts developed by ATK of Utah (perhaps essentially the same company that Richard Nixon worked with). For the leading edges, they are relying on a MATERIAL rather than a STRUCTURE, which means that the material must do more work, and must be more expensive. I do not know how expensive it is to produce these materials in quantity,
but I do remember how even the easier thermoplastic C-C parts were extremely expensive to fabricate before our breakthrough using technologies which ATK probably does not understand [1]. The ideal path would perhaps be to combine the best passive STRUCTURES – as in the Boeing TAV technology – with the best materials, and even insert some Ajax-like fuel reformation in later stages of development. But for now, cost per vehicle is just as important as reusability. It appears that once again the dynamics of powerful lobby groups has intruded to eliminate what had been the last best hope of humans in space.

There is something very strange and ironic about the role of powerful forces from Utah blocking our hopes that a brilliant and sincere Mormon like Chase could build us an ark.

Naturally, I have tried at times to discuss this with people I know in Boeing, but Boeing is a very large corporation. To a first approximation, it seems to be a loosely coupled combination of two companies, one of which sells airplanes to the private sector and leads the world in efficient low-cost mass production of advanced vehicles, and another company, Federal Systems, which is essentially a cost-plus contracting firm grounded in Washington. Chase has estimated that it would cost only about $30 million to reconstruct the successful hot structures test article, and to train new people and archive the technology such that it would not be endangered again soon. In informal discussion, Boeing has said that it would cost more like $150 million to do it right, by exploring how it could best be upgraded by inserting new materials (and of course infusing it all with modern mass manufacturing technology). But that kind of support simply has not been forthcoming from anywhere. People I know in other parts of Boeing have said that the key people remaining in the TAV and RASV groups were scattered to the four winds just this year; we are very close to losing the capabilities.

A few months ago, I was asked a question: "If Chinese investors and Boeing were to agree to establish two really well-designed new consortia, one for space solar power and one for launch services, what is the probability that the launch services group could get to $400/kg-LEO within 5 to 10 years?" (Of course, this assumes Chinese money, tight control of the launch services group to respect US ITAR laws and Boeing IP, and benefits to all parties.) My estimate was "70% now, and a lower probability for every year we wait. It WAS 90% in 2002, but key people and documents have been lost or moved."

I was also asked: "What about the new China National Aerospace Plane project, CNASP, with design leadership from Yong Yang of the Chinese Academy for Launch Technology (CALT)? What is the probability than in ten years China will be able to orbit more than ten times as much mass per dollar as any other nation on earth, due to success in that program?" My guess was: "Most likely they will fail, since it seems to be a copy of our NASP program, also depending on slush hydrogen. However, some technology has advanced, and they have shown a remarkable ability to learn from the experience of others. The current effort seems weak on the kind of bold, creative and rigorous 'yang thinking" needed for success in this venture on their own, but there is some possibility that help from India or even Russia might overcome their weakness. I assess the overall probability of success as about 30%."

In addressing the issue of international partnerships (like Boeing/China), I have said: "I hope we will not be like those proud people who beat their chests, and say they will insist on having 100% of nothing rather than 50% of something. That is a good way to become bankrupt."

But at present, unless something new happens, humanity does seem to be on a path to bankruptcy in this sector. There have been many times in the past when great civilizations slowed down, could not keep up with new challenges, and then went downhill. This time, if we do not develop this and other key technologies soon enough, I see a substantial likelihood of a global downward course which, because of new interdependencies and nuclear effects, might well result in extinction of the entire species. Slow and well-considered paper studies through the usual deliberative processes are on course to providing nothing but an amusing inscription for our tombs.

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