QUESTIONS FROM THE CHAIR

The Collected Columns from John H. McElroy
Space Studies Board Chair

July 2000 – June 2003

Space Studies Board
Division on Engineering and Physical Sciences
NATIONAL RESEARCH COUNCIL
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Preface

In late 1999 when the search began for the next chair of the Space Studies Board, we hoped, as always, to find the perfect candidate. And we did. John McElroy brought enormous experience and insight to the job. As a former practicing engineer in the US Army and NASA, a senior R&D manager and leader at NASA, NOAA, and Hughes Communications, Inc., and a professor and dean in academia, he brought an unsurpassed breadth and depth of perspectives to the Board. In addition, John's sense of history, analytical inclinations, and unflappability made him especially well equipped to lead the SSB during a period when the space program had a full plate of issues to challenge the Board.

In a wonderful illustration of the adage that "timing is everything," I was able to catch John at the perfect time. He was retiring from the University of Texas in May 2000, and he agreed to take on the SSB chair for a three-year tour beginning in July.

For more than a decade, one of the responsibilities of the chair has been to author a column—*From the Chair*—in the Board's quarterly newsletter. The topic is always up to the chair, and the views expressed are always entirely those of the author. John rose to the opportunity with relish, and his thoughtful and provocative pieces frequently elicited spontaneous compliments from readers.

In the pages that follow I have collected all of John's newsletter columns, covering his three-year tenure as chair. They are presented in approximate chronological order, except that I have grouped columns on related topics into a set of five thematic chapters. The only editing has been to smooth over (irrelevant) references to other items in the particular newsletter in which the column appeared.

John's columns are especially valuable both in terms of the analytical perspectives that they offer and in terms of the questions that they raise. Collectively, those questions would present a full agenda for the SSB all by themselves. John often ends a column with a statement along the lines of "Obviously, I have raised many questions and I have given no answers. I really don't believe that there are universal answers. Instead, I believe that once the issues are raised in a given context, wise people on the advisory and agency sides can reach solutions, perhaps imperfect ones, which best serve the public need." Hence the title for this collection: *Questions from the Chair.*

The questions that he has posed demand our careful attention.

Joseph K. Alexander
Space Studies Board
June 2003

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1 PROLOG

(September 2000)

Assuming the chair of the Space Studies Board is a daunting experience. You need only to review the list of past chairs to become concerned about your own adequacy. My immediate predecessor, Claude Canizares, created a formidable record that is one to which a successor may aspire, but certainly feel quite successful by simply approaching it.

My own experience with the Board and its committees extends back to the mid-1970s, although more as a briefer in the earlier years than as a member. From the outset the Board has been a respected institution, and any new chair recognizes his or her role as the standard bearer for the institution. For those who are interested in the early history of the Board (which has a remarkable, continuing relevance to events occurring today), I recommend most enthusiastically the books by two individuals who were pivotal in the history of the Board, Homer E. Newell and John E. Naugle. Newell's *Beyond the Atmosphere*[1] and Naugle's *First Among Equals*[2] are priceless memoirs of the history of the space program and the role that the Board has played.

Work with the Board brings a person into contact with some of the most notable figures of the space program—figures who provide perspectives on the events (successes and tragedies alike) that form the history of humankind's ventures into space. I have always found great pleasure in meeting people whom I have known only by their distinguished reputations.

The origins of the Board lie with the Space Science Board, and draw in further threads from the Space Applications Board, an important board that was disbanded some years ago. The first chair of the then Space Science Board was Lloyd V. Berkner, founder of the International Geophysical Year and President of the International Council of Scientific Unions. The SSB held its first meeting on June 27, 1958. I had joined the U.S. Army's guided missile program in 1957, so my own career has paralleled the Board's evolution, although it was nearly twenty years before I was senior enough to have any direct involvement with the Board's activities.

In the years since 1958, the Board's work has related to and influenced every major aspect of the U.S. space program, and many international efforts as well. Global space programs have grown in quantity, but even more importantly in complexity and sophistication. Space efforts that were once dominated by government-funded research programs now include research, defense, operational, and commercial satellites and systems. As a result, the Board's work will continue to grow in complexity, and even political sensitivity as questions are raised regarding the proper roles of government *vis-à-vis* the private sector and of research agencies *vis-à-vis* operational agencies.

The United States and its partners are in the midst of accumulating the greatest store of knowledge of the space and Earth sciences in history. The United States is also beginning the most intensive campaign of Space Shuttle launches in the nearly twenty-year life of the Shuttle, as NASA and its partners deploy the International Space Station. These activities will inevitably spawn a similarly large number of studies to by conducted by the Board.

It is our charge that the future studies we undertake uphold the standards of the Board's past and give credit to the National Research Council from whom we draw our association with the prestige of the National Academies. While apprehensive about my own capacities, I have no concerns about the Board's members and staff and their ability to meet the challenges we will face.

1. Homer E. Newell, *Beyond the Atmosphere: Early Years of Space Science*, NASA SP-4211, The NASA History Series, Washington, DC: 1980.

2. John E. Naugle, *First Among Equals: The Selection of NASA Space Science Experiments*, NASA SP-4215, The NASA History Series, Washington, DC: 1991.

2 THE SPACE STUDIES BOARD AGENDA

Diverse responsibilities and complex challenges (December 2000)

The last quarter of 2000 showed a continuation of the rapid pace of activities of the Space Studies Board. The diversity of those activities remains extraordinary, and that diversity will continue for the foreseeable future. The start of a new presidential administration, continuing and revolutionary changes in information exchange and use, advances in industrial and commercial capabilities, an increasing flood of new data from the space and Earth sciences, and new mission concepts and technologies are all driving the Board to ever more complex responsibilities.

Looking toward 2001, the Board will be involved in crucial decisions regarding the future of the U.S. space program. The areas of the Board's involvement in 2001 will represent a mixture between evolutionary changes in topics with which the Board has a considerable history and new topics stemming from enhanced understanding or changes in agency or administration policies. An incomplete listing of the areas that will demand special attention from the Board in 2001 are: (1) the use of the International Space Station for basic and applied research, (2) the implications for science stemming from the development of an eventual successor to the Space Shuttle, (3) an examination of the role that commercial suppliers of space and Earth sciences data may play in NASA and NOAA programs, (4) the blending and balance of large and small spacecraft approaches in all of the areas of the Board's interests, (5) the infusion of new technologies into both large and small missions of NASA and other federal agencies, (6) the conduct of the continued exploration of Mars and the other objects in the solar system, and (7) continued enhancement of the space observational capabilities and supporting research to expand the understanding of the universe and its origin and the connections between the Sun and the Earth. These are, of course, only some of the *foreseeable* areas of work, with other unforeseen areas certain to emerge during the course of the year, just as they did in 2000.

In keeping with the past experience of the Board, many activities will require collaboration with other groups within the National Research Council structure. Notably, those collaborations will involve the Aeronautics and Space Engineering Board, Board on Atmospheric Sciences and Climate, Board on Biology, Board on Chemical Sciences and Technology, Board on Earth Sciences and Resources, Board on Physics and Astronomy, and the Ocean Studies Board. The Space Studies Board will play both lead and supporting roles in its collaborations with these bodies. The collaborations are a welcome part of the Board's work, enhancing the quality of studies released by the National Research Council through the application of wider talents than are possessed by any one board or committee. The collaborations also demonstrate quite vividly the integration of space activities within the wider framework of science and engineering. At the beginning of the space program, space activities often stood apart from related disciplines using terrestrially-based observations; now space observations are recognized as another essential tool in an integrated approach to the advancement of science and its applications.

The last quarter of 2000 marks the end of the administrative structure within which the Space Studies Board has operated successfully for many years. The year 2001 will begin with the implementation of a long and carefully considered reorganization of the National Research Council. While the Board worked effectively within the old structure, the reorganization offers enhancements to the efficiency of the Council's operation and will assist all of the units of the Council in carrying out their work more effectively. The report describing the objectives of the reorganization and the new structure is available on the web site of the National Academies.* The task force preparing the report was given a seven-part charge that would allow the Council to:

1. Be more flexible, efficient, cost-effective, and timely;
2. Communicate more effectively with clients, peers, and the public and offer a broader spectrum of products;
3. Utilize Academy members and NRC volunteers more effectively;
4. Attract and retain the very best staff and maximize time spent on productive work;
5. Strengthen relations with executive and legislative branches of the federal government;
6. Develop activities within a broader range of sponsors and audiences; and
7. Monitor successes and failures to create a continuously learning and improving organization.

From my personal perspective the changes that are being put in place are well crafted to achieve the objectives of the charge.

*Report to the Governing Board of the National Research Council, *The NRC in the 21st Century: Report of the Task Force on NRC Goals and Operations,* August 4, 2000, http://www.nationalacademies.org/about/pdfs/taskforce.pdf.

International Space Station under stress, an agenda for NOAA-NESDIS, and international cooperation (March 2001)

For the most part, the details regarding the new administration's budget priorities are yet to be released, but in some cases policy recommendations are stated that have clear implications relating to the Board's interests. This is notably true for the International Space Station.

On the one hand, a policy is stated that the Station, which is experiencing a sizable cost overrun, will not be permitted to drain resources from other NASA programs – notably the space and Earth sciences. Because many in the science community have long expressed concern regarding such an eventuality, the recommended policy is a welcome one. On the other hand, the proposed absorption of the overrun within the Station budget will require delays in the availability of research apparatus for laboratory work, and cancellation of the development of some apparatus, where delays are an inadequate means to reduce the budget. In addition, under this guideline, the overrun will also require a reduction in the size of the permanent crew from seven to three, and the elimination of some crew accommodations.

Prior to the above recommended changes, concerns had already been expressed questioning the proportion of the crew's time that would be needed to maintain and upgrade the station versus the time available to conduct research (for a discussion of presently planned workarounds for some known Station problems, see, e.g., James Oberg, "NASA's big push for the space station," *IEEE Spectrum*, Vol. 37, No. 11, November 2000, pp. 49-54).

The combination of reductions and delays in research apparatus, the move to a smaller crew in more Spartan accommodations, and a sizable number of workarounds with which the Station's crew will have to cope inevitably raises questions regarding the scientific productivity attainable from the Station. It is premature to suggest answers to such evident questions; NASA needs the opportunity to prepare its plans, and the Congress can be expected to review those plans in considerable detail in its response to the President's proposed budget. Congressional responses often differ considerably from administration proposals. Nevertheless, the questions are legitimate ones, and one of the Board's committees has already begun working on them.

Also ambiguous in the early budget documents is the future of the National Oceanic and Atmospheric Administration's satellite-based earth observation and research programs. The National Polar-Orbiting Environmental Satellite System, a joint effort of NOAA and the Department of Defense, is reaching its critical funding period, and the proposed budget shows a sizable reduction in the overall Department of Commerce budget. The reduction may or may not affect the schedule or implementation of NPOESS. However, because NOAA is such a large part of the Department's budget, it is reasonable to be wary about possible ripple effects.

NOAA has asked the Board to host a workshop on planning for a study of technology infusion in the agency's satellite programs. The workshop will be held in April and will provide a means to develop ideas for a future, more comprehensive study that may begin in the fall. The problems of technology infusion, transition from research to operations, calibration of operational satellite sensors, and other aspects of the NOAA satellite system have been of longstanding interest to the Board and have resulted in a long series of formal reports. That body of work will provide a substantive foundation for the future effort.

Also in April, the Board will be represented at the biennial meeting of the European Space Science Committee in Florence, Italy. All of the disciplines represented on the Space Studies Board have a strong international connection. The science and applications missions conducted by NASA almost invariably have international partners, and often involve the joint or separate flight of complementary instruments and spacecraft. A hallmark of NOAA's programs has always been recognition of their inherent international character. Shared data collected from surface, aircraft, balloon, and satellite-based measurements of many nations contribute to operational forecasting needs and research on the Earth and its environment.

The space-power duopoly of the United States and the Soviet Union that once existed has long vanished. The duopoly ceased to exist because of the growth of the number of nations participating in space systems. While Europe and Japan come immediately to mind because of their strong, highly competent space programs, other nations are making excellent and growing contributions as well. China's meteorological satellite program is illustrative of rapidly expanding capabilities. Keeping track of the many programs is a challenging task. While the international ties of the nation's space program complicate the Board's work, the complications are welcome ones, because they accompany major contributions to the understanding of the universe, and especially of the planet Earth.

3 R&D MANAGEMENT

Impacts of International Space Station budget cuts: Lessons from Apollo and Shuttle (June 2001)

In our preceding issue of the *Space Studies Bulletin* (January-March 2001), reference was made to budget cuts for the International Space Station that are being proposed by the Administration. At that time, the details were not available, but more have now been released. I have chosen to devote my column to a personal view of the historical context within which the reductions can be considered.

The space program can be parsed in many ways. For this column, I chose to divide the space program into three generations. My choice of the first generation extends from fuzzy beginnings prior to World War II through the Apollo Moon landings and Skylab. The second generation is chosen to span the period from the end of Apollo through the development of the Space Shuttle. The second generation also saw the maturation of space science and applications. The third generation can be defined as the development and use of the International Space Station and the missions that will come later. In other words, my parsing places us near the start of the third generation.

The first generation of the space program had the advantage of having straightforward visions of what was to be done, and the profound disadvantages of limited technology and great gaps of knowledge. The clarity of the visions of the first generation (combined with geopolitics and a ringing declaration of purpose by a young, attractive, but doomed, American President) provided a rationale for the space program that the public found convincing and worthy of support. There were of course many subtleties, but the public did not need to digest them to lend support to the program. The public was willing to leave the subtleties to the "rocket scientists."

From the outset of the space program, the unmanned versus manned controversy nearly always simmered and occasionally flared. The science community often allied itself with the unmanned side—along with some presidential science advisors. The proponents on either side of the controversy sometimes painted their views in broad strokes of pure black and white, but in sober moments the two sides added more realistic shades of gray. Most people supported a role for both the manned and unmanned programs, and a number of scientists sought to shape the manned program to increase the scientific return. Our late colleague, Gene Shoemaker, comes to mind in this regard. The efforts produced striking successes. Nevertheless, the science community was disappointed when only a single scientist flew to the Moon, and the planned high-science-content missions of Apollo 18-20 were cancelled to reduce NASA's budget. NASA had attempted (notably in Apollo 14-17) to gain greater scientific return from its Apollo missions, but scientists believed that more could have been done. Skylab closed out the era of the first generation using the remaining Apollo hardware to create an initial space laboratory that was a tantalizing portent of what might be done with a more permanent facility. Skylab prematurely fell out of orbit when the development of another manned space flight program, the Space Shuttle—that could have reboosted Skylab to a safe orbit, was delayed.

Toward the end of the first generation the visions began to blur as conceptions were supplanted by reality. Gifted paintings were replaced by even more striking photographs. Nevertheless, the Apollo missions began to appear redundant to the public. At the same time, unmanned missions continued to be conducted, and they were more directly influenced by scientists and were consequently more favored by the scientific community. For the scientists, the legacy of Apollo was a mixed one. Naturally the scientists appreciated the technological achievement represented by the Saturn-V. One must view with wonder a vehicle larger than a Navy destroyer that lifts from Earth to deliver its payload safely a quarter of a million miles away. The scientists shared the pride of achievement, but saw unrealized promise and felt disappointment, too. Of course, Apollo is remembered by the general public for its manifold achievements, rather than for any scientific shortcomings.

The second generation of the space program profited from the achievements of the first, but it was to be confounded by the complexities flowing from the both the early achievements and the changing environment (scientific, technological, military, and political) in which they occurred. What was to follow Apollo? What was the government to do with the rapidly growing space applications of telecommunication and remote sensing satellites? Some people had reservations about extending the responsibilities of the federal government, and budget directors instinctively mistrusted the increased funding demanded for new capabilities (e.g., weather satellites). Telecommunication satellites quickly became self-sufficient in the private sector save for some specialized ventures within the government. Weather satellites found a home in NOAA, but issues with the transfer remain unresolved forty years later. Other remote sensing capabilities have remained in a multi-decadal limbo torn between differing visions of the roles of the government and private sector. The monolithic vision of Apollo was replaced by a jumbled multitude of visions that changed like the images in a kaleidoscope depending on the background and beliefs of the observer.

The scientific missions of the second generation produced a long string of scientific advances in Earth orbit and beyond, and produced their own unique images and data extending humankind's understanding of the universe. Initially the results remained easily within the grasp of the public (a never-before-seen close-up of a distant planet easily captures attention), but here too complexity was soon spawned, and the challenge of keeping the results accessible to a reasonable fraction of the public and its representatives tested NASA's and the scientific community's ingenuity. The public has often been left behind as the space program reached new intellectual depths, and much remains to be done to bring the public along.

At the end of the first generation, an expedient answer was given to the question, "What was to follow Apollo?" The major system development capabilities of NASA were preserved by the approval of the development of the reusable launch vehicle called the Space Shuttle. The broader, longer-term question was postponed, and utilitarian matters were stressed above support to some future manned mission. The underlying rationale that was offered for the Shuttle proved to be flawed, NASA's attempt to mandate unrealistic operating principles was a failure (e.g., the Shuttle-only policy), the capabilities of the Shuttle that were actually attained did not live up to the advance billing, and the costs proved to be barely short of prohibitive. But nevertheless the flying vehicle that was created has proven to be a great, even extraordinary, engineering achievement. While its on-orbit time is limited, the flexible research capabilities provided by the Shuttle have served many disciplines, and have done so with distinction. For example, the interferometric synthetic aperture radar measurements made from the

Shuttle have produced unique data for the Earth sciences that could have been obtained in no other way. A desire for continuing use of those capabilities remains.

The relatively limited use that was made of certain specialized scientific facilities developed for the Shuttle is a cause for regret. Nevertheless, as with Apollo, it is the current capabilities of the Shuttle by which we judge it, rather than any scientific shortcomings. The excitement of a Shuttle launch or landing is easy for both scientists and the general public to appreciate, although the frequency of Shuttle flights precludes a mass public interest. Once again, however, the scientific community—and especially the segment that had been specifically cultivated by NASA to employ the Shuttle—sees unrealized promise and feels disappointment. A bill is being advanced in Congress to secure more research-oriented flights by the Shuttle in the next several years, but the prognosis is uncertain.

As we now enter the third generation, it appears that the Space Shuttle will be the vehicle of choice for human space flight for the foreseeable future, despite numerous efforts to spawn a successor. This will be true because of budget realities, but also because it is not evident that a remarkably better vehicle can be developed from scratch. In a view shared by many, Tom Young noted at the last meeting of the NASA Advisory Council, "The successor to the Shuttle will be the Shuttle." Some call the Shuttle the "B-52" of the space program and say that it will ultimately carry astronauts who are younger than the vehicle in which they fly. That is not intended to be a critical comment; the B-52 and the Shuttle are both remarkable flying vehicles. The cost of sustaining the Shuttle will be high, but familiarity has made the costs somewhat less troubling. The cost of extending the Shuttle's lifetime and capability will also be great, but perhaps competitive with the other alternatives.

With the completion of the Shuttle program, NASA's system development capabilities were again preserved by a decision to build enabling infrastructure, rather than being directed toward a new human exploration mission. The International Space Station proposals stressed utilitarian objectives (as had the Shuttle proposals), but was also offered to be the enabling infrastructure for whatever new venture was to come. At a minimum, the Station is to provide the scientific underpinnings for the undefined new venture in terms of understanding human survivability in space during long-duration missions. As with the Shuttle, the initial concept and rationale (circa 1984) has proved to be flawed, the capabilities being deployed (even prior to recent proposals for reductions) will not live up to the initial advertisements, and the costs are proving to be nearly unsupportable. But despite these shortcomings a permanent Station is now in orbit with a three-person crew, and the Station and its placement in orbit are again great engineering achievements. A hugely complex spacecraft as large as a football field is being deployed by a multinational crew.

The Station is a superb example of international cooperation. The bulk of the population of the Earth can look upward to see the Station pass overhead, and a NASA web page tells them when and where to look. Some of that large population can also draw pride from their citizens flying in the Station and from the sophisticated equipment their nations have provided. A question that can now be posed is whether the creation of the infrastructure and the things that will certainly flow from it, even without further augmentation, are sufficient outcomes from the Station's investment. It seems unlikely that people within NASA or the scientific community could respond in the affirmative.

NASA and its international partners are at the point where decisions are being made regarding the extent of Station capabilities and when those capabilities will be available. The Administration's proposed budget calls for accommodating Station funding overruns by deleting approximately 80% of the planned scientific capability. Furthermore, the crew size and associated hardware have been scaled back from six or seven to three, precluding any significant astronaut involvement in research even in the modest remaining capability. The deleted research and crew capability, it is proposed by the Administration, will be restored piecemeal, with each future restoration being subject to the always-uncertain NASA new-start process and to further reviews of scientific priorities. To say that a previously approved hardware development project is assured only of a possible place in a queue is obviously of little interest to the investigator attempting to advance a research program or educate graduate students.

The decisions made to terminate the later Apollo missions, restrict the scientific uses of the Shuttle, and now to reduce the scientific capability of the Station have all been done for understandable reasons by decent people attempting to do the best under the circumstances that were presented to them. However hard we may seek them, there are no villains here. Nevertheless, from a scientific research perspective the history is not encouraging. To be fair, some great achievements have been produced in spite of the decisions that were made to reduce scientific capabilities. In certain cases, there was likely no practical alternative. However, at least in hindsight, more could have been achieved in Apollo and with the Shuttle, and the question becomes whether we can do better in the future on Space Station.

No lengthy study is needed to conclude that a laboratory without researchers or equipment produces no world-class science, and yet the latter is a declared purpose of the Station. Certainly the subtle questions related to the ability of humans to endure long-duration space flight cannot be unraveled under such circumstances, and our NASA colleagues understand that just as well as does the scientific community. NASA has worked hard to build a scientific community ready to capitalize on planned-for research opportunities in the biological and physical sciences on the Station. Maintaining that community will require that they be given some reasonable opportunity to carry out their planned in-orbit experiments. Researchers in the micro-gravity biological and physical sciences often lack the alternatives of their colleagues in other disciplines, whose work is largely carried out via unmanned spacecraft. Scientific teams soon experience organizational entropy in the absence of clear opportunities and direction, and the disbursal of the carefully crafted teams who staked their work on the availability of the planned facilities on the Station will be difficult to restore.

Our NASA colleagues are working on the above issues, and doing so within the commendable guideline that the other parts of the NASA science and applications program not be gutted to fund the overruns within the Station program. We wish them success in their efforts to find an accommodation we can all support.

Making program transitions effective (September 2001)

Much of the work of the Space Studies Board relates to *transitions*, and I have chosen this topic as the subject for this quarter's column. I am using the term in a broad way that encompasses both the movement of a program from one stage to the next and the nature of the interface between the stages. I am also using the term to include changes in program direction and reductions in scope, as well as the movement between the stages in a project's development.

One of our colleagues, Eb Rechtin, has written several insightful books addressing—among other things—the importance of interfaces in the development of complex systems (see, e.g., *Systems Architecting*, Prentice-Hall, 1991), and I have been influenced by his insights in writing this brief column. Among the heuristics that he offers are:

- The greatest **leverage** in system architecting is at the **interfaces**.
- In architecting a new software program, all the serious **mistakes** are made in the **first day**.
- In architecting a new aerospace system, by the time of the first design review, performance, cost, and schedule will have been predetermined. One might not know what they are yet, but, to first order, all the critical assumptions and choices will have been made that determine those parameters.

Rechtin provides many other heuristics, but the ones above have particular relevance to the Board's work.

If I broaden *interfaces* to *transitions*, we can note that the best opportunity to influence a program occurs at the approvals associated with transitions between major phases of the program, including the transition that begins the first phase of a program. Furthermore, we can note that a program may go awry by failing to have satisfied the interface requirements needed to permit a program to move from one stage to the next. The technology may be untested or inadequate, and the overall effort becomes bogged down while a "marching army" awaits completion of an R&D task that should have been finished in the earlier phase. The science base may be insufficiently developed, with similar consequences. The budgetary requirements may not yet have been developed with adequate accuracy. These are all valid areas of inquiry for the Board to follow. Concurrent engineering remains a current fad, with much to recommend it in certain settings, but paralleling activities in a development program can be risky.

In development projects, when two pieces of hardware or software are to be joined, it is common practice to create an interface control document that stipulates the assumptions to be made and the requirements to be satisfied on both sides of the interface. The interface control document, or *ICD*, may be fairly straightforward or very complex, and it is especially important when different groups are responsible for the items on either side of the interface. The ICD may stipulate heat transfer, electrical power inputs (and assumed degree of filtering), grounding points, data connections and their characteristics, mechanical tie points, etc.

A programmatic analog can be imagined to serve a similar purpose, and NASA and DOD have long had in place various review processes to control transitions from, e.g., Phase A (conceptual design) to Phase B (detailed design), or Phase B to Phase C/D (production and test). Periodically, and usually in response to some project's development failures, these processes are reexamined and tweaked to improve matters. Matters usually improve – at least for a while. It is reasonable to ask why matters continue to go awry in spite of the existing processes.

The Space Studies Board reviews programs and projects, and also program and project changes, and assembles prospectuses for future research programs and flight projects, sometimes in the form of decadal surveys. As would be inferred from the above, our work usually focuses on the points in the efforts where key transitions are being made. In recent months, for example, the Board has been asked—among other things—to review the transfer of technology from NASA's research satellites to operational use by NOAA, evaluate the effect on research of reducing the diameter of the primary mirror in the proposed Next Generation Space Telescope, and assess the readiness of the research community to begin use of the facilities on the International Space Station. The Board has examined the NASA policy of *faster, better, cheaper*, and has studied the readiness of a number of scientific missions to be advanced in their development cycles. The latter activity has been a continuing responsibility of the Board over its entire history.

The work of assembling the various elements of research programs has many similarities to the processes of system development. For example, optimizing the programmatic return within the bounds imposed by science and budget is a classic systems engineering task – at least in concept. Beyond this connection, there are other similarities as well. When a program is ready to advance from one stage to the next, one can raise obvious questions about the readiness for the transition to occur. Are the initial programmatic assumptions still valid? If not, have the implications of all changes been assessed and corrective actions taken when necessary? What is the nature of the interface between the two phases of work? Are adequate total funds available to move responsibly to the next stage? Are the funds available in the appropriate budget years, or must schedules and work be adjusted to account for lower-than-optimum funding in one or more years? Has the technology been advanced far enough to permit the costs and schedules for the next stage to be known within manageable and acceptable error bars? Can the knowledge gained in earlier stages be adequately transmitted to those responsible for later stages, if different groups are involved? Are the people who performed the earlier work still available? Are the laboratory setups and test articles used in the earlier phase still available in the event that new results do not conform to prior measurements or their extrapolation? Programmatic transition control has the same importance as its counterpart in interface control, but it is often conducted with considerably less rigor. Often absent is a rigorous, disciplined process that seeks out, exposes, and addresses faulty assumptions – and this lack has consequences.

Returning to the issue of why things go awry, there are always more ideas than can be accommodated in NASA's or NOAA's new start processes. Hence ideas are put in competition with one another, with the measures of competitive success not necessarily being related to the quality of the science to be pursued, but consistent with the questions I raised earlier) total budget, budget profile (the per-year budget spread), readiness of technology, shortness of development schedule, spacecraft or instrument mass, consistency with management's views, etc. We may then find that a mission's position in queue is based on other considerations than the value of the results to be obtained.

But as we drift away from measures related to the results proposed to be obtained, the measures may have elements that are increasingly subjective, and it would not be realistic to expect that human beings, in the midst of competition, would tip toward assessments of subjective elements that are likely to lead to competitive disadvantage. And if the total budget establishes the position in the queue, for example, the total budget will be estimated with favorable assumptions regarding cost, risk, and schedule. If the budget profile must fit a particular distribution, the budget profile will be assumed be more easily adjustable than later proves to be possible. Similarly, the technology that seemed so promising on the laboratory bench will be found to be unyieldingly resistant to space qualification. The development schedule will have little pad for the unexpected delay in technology development. Likewise, the device's mass or power or data requirements will prove to be much greater than originally conceived. I am not suggesting that information is being provided that is known to be false – only that there is a long series of assumptions that prove to be excessively optimistic. And this is the *rational* part of the mission development process.

When the above process begins or is complete, there often comes the implementation of "edictatorial" management and oversight. Edicts may flow from within the agency, from elsewhere in the Executive Branch, or from the Congress. The, at least arguably, rational process that leads to a mission proposal is overlaid with what I will call the t*hou shalt* rules. *Thou shalt* propose no budget larger than, e.g., $100 million. *Thou shalt* propose only the use of a particular launch vehicle or class of launch vehicles. *Thou shalt* provide a minimum societal benefit-to-cost ratio of, e.g., 100:1 to secure project approval. *Thou shalt* launch within 18 months of project approval. Lessons learned in elementary mathematics courses on over-constrained problems seem long forgotten.

Thou shalt rules are rarely, if ever, grounded in science or engineering or related to the direct objectives of the project, but are instead based on convictions (as in the belief that, if a project team is coerced to try hard enough, the desired result will be obtained despite initial opposition), budget (as in "take this budget" or none), policy (the stimulation of the use of small satellites in lieu of mid-sized satellites is "good" for future projects), or other considerations. Not surprisingly, when an agency places a $100 million *thou shalt* ceiling on a class of projects, and especially when that class may represent the only flight opportunities for some time to come, all projects in the class are found to require $99.9 million. Of course, the ceiling may also be imposed from outside the agency (but usually with agency participation), as in the approval of major initiatives such as the Space Shuttle or the International Space Station, where initial proposals submitted by the Administration had cost estimates that were strikingly short-lived. Likewise, when the permitted launch vehicle has a payload capacity of 100 kg, the competing projects conclude they will need no more than 99.9 kg, and so forth. Yet, even these processes are at least partially rational. There is an objective being sought that, while likely unconnected to the given project, nevertheless is intended to serve some larger purpose.

Occasionally less rational are the programmatic transitions that are experienced in the federal efforts to which the Board serves as an advisor. In the Board's context, programmatic transitions may occur within programs (e.g., changes in direction or funding), between programs within an agency (as between a research entity and organizations providing operational launch or communications support), between agencies (e.g., between NASA and DOD or NOAA), and between sectors (e.g., between a research organization and private for-profit or non-profit entities).

Within an agency, separate budget lines may be involved, and abrupt shifts in operational costs (as in implementing so-called "full cost accounting") may profoundly affect a research program's financial performance. Between agencies, the program budgets are likely addressed by different examiners in the President's Office of Management and Budget, and acted upon by different committees within the Congress. The interests of the various parties may have little to do with the efficient conduct of a research program, but very much to do with limiting the size of a particular cost account. Such factors create powerful incentives dissuading federal managers from engaging in interdepartmental programs; they are simply too complicated to secure approval for and to retain annual budget allotments.

Between agencies and the private sector to which federal activities may be transitioned, reliance may be placed upon assertions of what *will* – more *thou shalt r*ules – be feasible, rather than what has actually been achieved to date, and upon the implementation of untested philosophies or ill-designed rules aimed at addressing the latest concern of one of the parties in the process. Projects are told that, for example, if they will just leave alone the private firm building the spacecraft or instrument, a minimum savings of, e.g., 15 percent will be obtained simply due to the lack of interfering oversight – and the purported savings are built into the budget. When the magic of the marketplace lays an egg (pardon the mixed metaphor) and the private firm breaks out of the triple constraint (cost, schedule, performance) despite a lack of government meddling, the government's lack of oversight becomes suspect.

Still further, in recent years missions have increasingly been characterized as technology demonstration missions, with only incidental science. Disassociating a project from tangible objectives, and leaving only nebulous "demonstration" objectives leaves few "teeth" in any agency oversight process. In the early years of the space program, large gaps in both knowledge and ground testing capability led to a need for some technology demonstration missions. However, most development took place within the framework of purposeful missions, where technology was developed because it was specifically needed. In a demonstration mission it is easy to take the position that the budget is sacred, but the science objectives are secondary and mutable. In other words, the ruling guidance for the project is, "How much science can you afford?" Of course, when the mission development costs grow, the fixed overall budget squeezes the science budget that was initially set and reduces the research objectives, as in the presently proposed solution to the major cost overrun on the development of the International Space Station.

The Space Studies Board faces all of the above considerations as it goes about its work. The question then becomes, "What constructive use can be made of the observations on program and project reviews?" While there are no easy solutions for the Board and its committees, we should also not forget that the steps in responsible program and project management are scarcely unexplored territory. It is possible to scrutinize programs and projects to detect whether they contain the seeds of past difficulties. A few, non-dogmatic suggestions for consideration during project and program reviews of space missions may serve as starting points for discussion. I will leave the companion suggestions for broader research programs to the committees to sort out.

For space missions:

- Is the mission results-driven, or technology-driven? How were the mission costs derived? What are the fallback positions? In either mission class, what are the potential retrenchments to accommodate cost growth? Does each potential retrenchment lead to a configuration that offers the potential for results that continue to warrant the mission proceeding?
- Does the mission contribute to the mainline needs of the program, or is it merely an opportunity to fly *something*—perhaps a technology test article—and only incidentally contribute to a discipline? Does the mission at least offer the potential to make a lasting contribution?
- Is the mission being required to employ a particular technology or technological approach? Does the use of that technology or approach contribute to or detract from the mission objectives? If the mission is required to employ technology to serve other needs, is the cost increment for the dictated technology accounted for separately in the approval documentation?
- What is the pedigree of each element of critical technology? Are key components "one-of-a-kind" or have several versions been shown to meet the requirements? Does a breadboard, brassboard, or engineering model exist of key subsystems? If not, does a plan exist to create such a model and do its results "gate" the continuation of the program?
- What is the history of the proposal under examination? Is this proposal an iteration of a previously unaffordable effort? When mission planners produce unaffordable proposals, the planning process must begin again at the outset, with a reexamination of objectives, means, and schedules. The process cannot simply re-price the unaffordable proposal to make it more acceptable.
- Does the proposal evidence management by edicts that are not based in realistic practice? Can we identify cases where arbitrary assumptions have been made regarding savings and efficiencies (cost or schedule) that have yet to be demonstrated? Are the efficiencies due to *sloganeering* or *engineering*?
- Does the proposal rely upon management by exhortation? People will sometimes volunteer, and even allow themselves to be pushed, to achieve extraordinary results. How extraordinary must the results be to achieve a "triple-constraint C"? Does the project require simply an *extraordinary* effort or a *superhuman* effort? How sustained must the effort be?
- Are the requirements to be satisfied to move from each stage to the succeeding one well defined?
- Does a funded plan exist for the exploitation of the results that are to be achieved by the mission?

I would welcome suggestions for additions, deletions, and clarifications. Board chairs don't issue edicts to committee chairs and their members, but I might able to slip—figuratively speaking—something quietly under their office doors. If members of the SSB community haven't read the book I cited above, I would encourage you to take a look at it. As it is a fairly sober book, you may want to balance it with a book by still another colleague, Norm Augustine (*Augustine's Laws*, 16th Edition, AIAA, 1997). The latter book is familiar to many of you I am sure, and tempers great wisdom on system and program management with telling humor.

Organizational models and roles for Federal in-house R&D capability (December 2001)

It isn't possible to write this column at the end of 2001 without acknowledging the terrorist attack of September 11th. The matters about which I will write are small compared to the grief of those who lost relatives and friends. When I was a boy in Marion, Ohio, I vividly remember our paperboy riding his bicycle down Silver St. in the dark yelling, "Extra, extra! Pearl Harbor attacked!" Our family clustered around the radio to get more news of the shattering event. Sixty years later, I was attending a meeting at the NRC's Georgetown facility, when a person came in to tell our chair, Bill Wulf, that the World Trade Center had been struck by an airplane. The tragedy quickly evolved and we were soon clustered around a television set to get more news of another shattering event. We adjourned and I returned to my sixth-floor hotel room from which I could see the smoke rising from the Pentagon, mostly gray but sometimes mixed with the white of steam. We won't forget the images of September 11th, just as we remember the burning ships of Pearl Harbor.

The aftermath of September 11th will likely increase the calls for reviews, and increase the emphasis on ranking activities and securing savings wherever possible. As you all recognize, the Space Studies Board is often called upon to participate in such reviews, either solely or in partnership with other entities in the National Research Council. Some of the reviews are likely to be prompted by continuing pressures to reduce, eliminate, or "contract-out" federal activities. These pressures were present prior to September 11th, but will surely increase in its aftermath, as the federal budget must stretch to include new priorities. These pressures will add to the earlier NASA problems caused by the budget overrun in the International Space Station, which has left our international partners charging abrogation of their agreement with the U.S. and segments of the research community seeing their plans evaporate.

I chose the term "contract-out" above, rather than "out-source," because I wanted to encompass government-industry partnerships, government-university partnerships, privatization, commercialization, and any of the other arrangements that have been or are being considered. A particularly thorny problem to be addressed is the in-house R&D capability an agency must retain to carry out its responsibilities and I will focus this quarter's column on that one issue. I won't offer any general prescriptions below, but I will note some issues I believe to be important for the SSB and its committees to consider in conducting reviews.

In-house capability is, of course, only one aspect of federal research management. For a broader perspective, I recommend a book co-authored by one of our colleagues, Hans Mark, on the management of research institutions in the context of the mission-oriented federal laboratory (H. Mark and A. Levine, *The Management of Research Institutions: A Look at Government Laboratories*, NASA SP-481, U.S. GPO, 1984). The book is available at http://www.sti.nasa.gov. While published by NASA, the book's coverage extends to some Departments of Defense and Energy laboratories as well, and it reviews a wide variety of issues associated with research management, including in-house capability.

Models for federal R&D programs are varied, and agencies have employed in-house federal laboratories, federal contract research centers, wholly and partially sponsored industrial and academic laboratories, and many forms of contracts and grants. Obviously, the degree of in-house capability retained by an agency is strongly modulated by the choice of organizational model. Within the context of the SSB, nearly all of the models mentioned above may be encountered. Understanding which of the models is involved in the program (both presently in-place and projected for the future) is an important aspect of beginning a review.

Other than recognizing the organizational model that is in place, none of our reviews start from a "clean slate", but they begin against a backdrop of R&D activities and agency policies that are usually of longstanding. That backdrop is important to understand. Beyond the customary statistical data regarding demographic trends, the organizational perspective includes assumptions regarding the use of civil servants, support service contractors, the appropriate balance between the two, the role of advisory groups, functions to be contracted out, specialized facilities to be maintained, etc. Whether those data and assumptions, in the opinions of the program's reviewers, are compelling or well founded is not important at the outset of a review. We may disagree with logic or structures, but the assumptions must be understood, if the context of the review is to be established. The reexamination of assumptions can follow after the initial context is in place.

Moving now to the NASA structure, mixtures of federal employees and support service contractors are common in NASA's field centers. The numbers of all on-site and near on-site personnel (federal and contract) are often a particular focus of internal government reviews and proposed management changes. The use of in-house laboratories staffed by civil servants is deeply embedded in NASA's culture. NASA's roots lie within the National Advisory Committee on Aeronautics, the Army Ballistic Missile Agency, and the Naval Research Laboratory. All of these entities had a strong in-house capability and provided NASA's initial staffing. While those roots now extend back more than forty years, they continue to shape center organizational structures, facilities, and value systems. Another former colleague of the SSB and former Marshall Space Flight Center director, Wernher von Braun, testified before Congress (quoted from A. Levine, *Managing NASA in the Apollo Era*, NASA SP-4102, GPO, 1982, p. 4):

> A good engineer gets stale very fast if he doesn't keep his hands dirty . . . . it is for this reason that we are spending about 10 percent of our money in-house; it enables us to really talk competently about what we are doing. This is the only way known to us to retain professional respect on the part of our contractors.

Marshall's founding engineers brought with them some of the deep-seated values and philosophies of the Army's arsenal approach. The degree of contractor oversight and technical penetration on the part of Marshall's engineers was a contentious matter throughout the development of the Apollo launch vehicle (see, e.g., R. E. Bilstein, *Stages to Saturn: A Technological History of the Apollo/Saturn Launch Vehicles*, The NASA History Series, NASA SP-4206, U.S. GPO, 1980, p. 81, pp. 266-267; H. E. McCurdy, *Inside NASA: High Technology and Organizational Change in the U.S. Space Program*, Johns Hopkins University Press, 1993, p. 41).

Similar sentiments to von Braun's have probably been expressed by all of NASA's field center managers at one time or another, and by other federal agency leaders as well. The in-house work was regarded as important in its own right and as a means to keep center staff professionally skilled. Maintaining state-of-the-art skills of the in-house staff is not achieved by people simply looking over the shoulders of contractors, but by carrying out personal work (McCurdy, *op. cit***.**, provides a good discussion of the debate over in-house work).

However, even if one concurs in the need for at least some personal involvement in technical activities, maintaining that involvement within the federal system is not straightforward. For example, hiring freezes and reduced personnel ceilings have been common within NASA since the Apollo era. They have driven the average age of the staff upward and fostered a growing use of support service contractors. Contrary to popular belief, civil servants are in the minority in all centers. The normal personnel flows of a stable organization have been blocked. On the other hand, salary caps – a common phenomena occurring across the federal government because of the connection between civil service and Congressional salaries – have also militated toward the use of contractors to secure high-demand skills. At the same time, outside individuals and organizations, for both valid and partisan reasons, resist NASA's internal expenditure of resources. Naturally, the pressures to reduce simultaneously both civil service and support service contractors are inconsistent and in conflict unless there are reductions or new efficiencies in the work to be done. As a result of all of these factors, the maintenance of a competent internal staff has been a dilemma for NASA, and one that plagues any technical organizations whose principal role is overseeing contracts.

Beyond its civil service staff and support service contractors, NASA also has used a number of joint ventures and other arrangements to support R&D in particular areas or to support particular functions, e.g., the present-day Space Telescope Science Institute (STScI) or the Apollo-era Bellcomm. Both are or were successful ventures, and other examples could be cited as well. The evolution of the STScI and the roles played by the SSB in its origins are important case studies in university-government partnerships (Robert W. Smith, *The Space Telescope, A Study of NASA, Science, Technology, and Politics*, Cambridge University Press, 1989). Likewise, Bellcomm demonstrates the integration of private capabilities into the Apollo decision-making process through the provision of systems engineering skills (A. S. Levine, *Managing NASA in the Apollo Era*, The NASA History Series, NASA SP-4102, U.S. GPO, 1982). The Bellcomm example is interesting also because the organization quietly closed its doors and disappeared when the Apollo needs for which it was created to serve no longer existed.

In both the STScI and Bellcomm examples, the agency could have advanced arguments for increasing civil service staff to carry out the science management or systems engineering roles but chose not to (although some NASA individuals certainly disagreed with that view in internal discussions). Similarly, and from the opposite perspective, arguments were sometimes, but unsuccessfully, advanced that the roles of these entities extended too far into government prerogatives. A tension will always exist between the two views.

The lesson of these examples is that federal regulations do not necessarily preclude innovative uses of academic and industrial talents in the conduct of NASA missions. Many more examples (Caltech JPL, Aerospace Corporation, JHU Applied Physics Laboratory, MIT Lincoln Laboratories, etc.) could be mined for lessons to be learned, but the basic message would remain. Care and good sense must be exercised, but we should not reject out of hand the possibility for effective partnerships based on new models, while recognizing at the same time that the skills of the in-house staff need to be sustained for the overall good of the program. Restrictions on in-house work by civil servants and hardware exclusion clauses in contracts for supporting contractors are understandable and perhaps a well justified means to satisfy some public goals, but they may have adverse and unintended consequences requiring compensatory actions.

To this point, I have offered two considerations regarding in-house capability: the choice of organizational model and the need to maintain the skills of the in-house staff. Other considerations affecting the needed in-house R&D capability are the fundamental government functions that must be retained by federal managers. Mark and Levine (p. 152) list the following:

- Responsibility for general management (e.g., supervision of prime contractors)
- Conduct of external affairs (e.g., negotiating international and interagency agreements)
- Securing and allocating resources (e.g., negotiating with the Office of Management and Budget and the Congress for budgets)
- Procurement (e.g., issuing requests for proposals and judging responses)
- Determining what work is to be done (e.g., planning the program to be carried out, ranking its various elements, and evaluating outcomes)

These five core functions can be regarded as the minimum overhead or stewardship activities that must be supported to carry out a federally funded program, and we can relate the threshold in-house capabilities to these functions. For example, the need for NASA, or NOAA, to be an "intelligent buyer" connects in-house skills to government responsibilities. Likewise, the conduct and oversight of a complex program of scientific research requires in-house skills that are and remain of high order. The SSB can expect to be asked, both directly and implicitly, to assess the minimum in-house capability needed to meet federal responsibilities and to examine justifications for capabilities exceeding the minimum.

In its examination of programs, the SSB may be asked to review two quite different situations: the establishment of a new R&D program and the reorganization of an existing one. The greatest complexity occurs in changes in existing arrangements, rather than in the establishment of a new R&D institution. When a new institution is created, all of the diversity of approach cited before is available to the responsible officials.

On the other hand, changing an R&D activity from one form to another is rife with opportunities for conflict or disruption.

Situations are rarely simple or straightforward to solve. The SSB and the Aeronautics and Space Engineering Board (ASEB) have examined the establishment of a non-governmental organization to oversee research on the International Space Station (*Institutional Arrangements for Space Station Research*, National Academy Press, 1999; http://www.nas.edu/ssb/iassrmenu.htm). This could fit the model of a new function being created, from the perspective that the principal research activity on the Station is some distance in the future, but the situation is complicated by the work already underway (involving both in-house and external researchers), the very large cost overrun now projected for the Station, and the massive reductions in the planned research activities. The time constant of program change may be incompatible with the thoughtful establishment of a complex new interface between NASA and the scientific community, and the reductions may modify the activities that made the non-governmental organization attractive.

Likewise, the NRC may become involved with NASA's recent studies on the privatization of the Space Shuttle, which could fit the latter case of modifying a current activity, but which could also require major innovations to reconsider existing private sector and civil service roles in the launch, operation, and upgrading of the Shuttle fleet (for background on this item, see *Concept of Privatization of the Space Shuttle Program*, Space Shuttle Program Office, Sept. 28, 2001; it is available from the NASA web site). In all changes, both simple and complex, the issue of program disruption and adding risk will have to be assessed. The potential gain must provide the justification for making the change.

We now have three considerations influencing the amount of in-house work: the original organizational structure, the need to maintain in-house skills, and the preservation of federal responsibilities. Naturally, although I have treated the considerations separately for this column, they are overlapping and interrelated. Moving to the next consideration, what leads a federal agency to establish or continue activities that extend beyond the five minimum responsibilities cited by Mark and Levine? I will outline several possible answers to this question, but there are likely to be others as well. There may also be criticisms or different perspectives on the answers I offer, but I will leave that to the future review committees to sort out.

An agency's legislative charter may have been established more expansively than the minimal responsibilities. The National Weather Service, for example, has a variety of explicitly enacted responsibilities, and other responsibilities have been derived from them. Furthermore, the provision of weather services is accepted as a permanent need that serves the safety, health, and economic well-being of the nation. The Weather Service could, in principle, be privatized, but that is not a step that policy officials have chosen to take. Federal subscriber fees, citizen subscriber fees, pay-per-forecast services, free services, climate projections, liability, potential anticompetitive actions, and such matters all make privatization at the very least exceedingly complex and likely infeasible. For example, the nation does not seem ready for 1-900-TORNADO and 1-901-HURICAN ventures. Instead, less sweeping initiatives have been taken to allow the private sector niche opportunities in weather forecasting.

In common with the weather example, an agency may be chartered to carry out a program that has no easily discernible end-point. Space exploration is a series of increasingly sophisticated ventures, but those ventures do not lead to a culminating mission. The study of the space and Earth sciences is one of ever-advancing knowledge and understanding. Such activities better fit the mold of federal activities than private ventures. The give and take of the budget process moderates the efforts to a politically acceptable level, and opportunities are available for the private sector through federal contracts. As with the National Weather Service, one can imagine a non-profit, federally chartered corporation to conduct these efforts, but the gain to the public is not evident, and policy officials have preferred current practices.

Agencies have also cited advanced technology development as a justified activity beyond the minimum. The SSB and ASEB examined that issue in an extensive study published in 1998 (*Assessment of Technology Development in NASA's Office of Space*

Science, National Academy Press, 1998; http://www.nas.edu/ssb/tossmenu.htm) and updated the study in 2000 (*On Continuing Assessment of Technology Development in NASA's Office of Space Science*, http://www.nas.edu/ssb/tgtossletter.htm). In the two studies, a critique is made principally of the NASA centers' declarations of needs for sweeping world-class core competencies and of the degree of reliance on outside entities in achieving world-class status. The review concluded that the stated core competencies were too extensive and that the allocation of federal funds for technology development could be greatly improved.

In examining an agency's need for in-house talents, however, it may not be sufficient to examine only the process of advanced technology development. In the past, NASA has sometimes used its advanced technology development program to serve some of the needs for skill maintenance cited earlier in this column. The SSB may need to assess the balance between advancing technology and maintaining the skills of the staff. Activities serving the latter may not be world-class, but nevertheless serve a valuable need. Easy prescriptions for resolving the balance don't come to mind. Similarly, what about maintaining mundane skills? Solar arrays are mundane until they fail. High-voltage power supplies are mundane until they arc over in the partial vacuum of an out-gassing spacecraft. Antennas are mundane until they fail to deploy. Securing a balance between world-class R&D and ensuring that devices and systems within the current state-of-the-art perform properly is not a straightforward task. What degree of knowledge do we expect from our federal employees?

Finally, how is incremental improvement to be carried out? The Delta launch vehicle began decades ago with a lift capacity to geostationary transfer orbit of about 100 pounds. Today, the much-improved Delta can raise more than 8000 pounds to the same orbit. Major technological improvements can occur in incremental advances that continue year after year. Should such improvements be solely the responsibility of private industry, or do they fit into the in-house efforts of NASA as well?

Obviously, I have raised many questions and I have given no answers. I really don't believe that there are universal answers. Instead, I believe that once the issues are raised in a given context, wise people on the advisory and agency sides can reach solutions, perhaps imperfect ones, that best serve the public need.

What do we do about success: Lessons learned from space applications (March 2002)

Homer Newell succinctly defined space applications as, "… the use of space knowledge and techniques to attain practical objectives."[1] What could be more reasonable and appealing than devoting some of the nation's investment in space activities to attaining practical objectives? Despite the nation's more than forty years of experience with space applications, a quick review of the SSB's recent reports and on-going studies suggests that questions surrounding space applications are anything but resolved.

Over recent months, the Board has published reports addressing the integration of research and operational satellite systems,[2] small spaceborne synthetic aperture radars,[3] space weather,[4] and other applications of space technology. Were I to go back more than just the recent months, the list of reports would be far longer than this column can reasonably be. I would also have to include the work of the Space Applications Board (SAB), whose responsibilities our Board later subsumed. In addition, the Board's Steering Committee on Space Applications and Commercialization (SAPPSC) has conducted three workshops, and the first of its planned three reports has been published,[5] The latter report was published jointly with the Ocean Studies Board, which is only one of the NRC bodies with which the SSB has collaborated on various aspects of space applications. Furthermore, the Board is just beginning a study entitled *The NASA-NOAA Transition from Research to Operations* and is near completion of *Solar and Space Physics: A Community Assessment and Strategy for the Future*; both studies have strong space applications elements.

Space applications are usually considered to encompass satellite communications, navigation and position location, Earth observations, launch vehicles and upper stages, space weather, and extensions of the microgravity and biological sciences to practical uses. For this column, I'll begin with what I consider to be the most clear-cut success in federal participation in civil space applications: satellite-aided navigation and position location. I believe that this example offers some general principles that can be applied to other space applications. After briefly reviewing the history of satellite-aided navigation and position location, I'll try to extract a few plausible lessons learned, and then offer some comments tying those lessons to two areas of current Board interest: weather satellites and space weather. I have ignored the other applications listed above (including the other categories of Earth observations), but believe that the lessons taken from navigation and position location do have relevance with only modest adjustment. My discussion of lessons learned profited from the paper given by our friend and colleague John E. Estes at the first of the workshops arranged by the SAPPSC.[6] Jack died too young and too suddenly less than a year after the workshop.

On one occasion the path from idea to operational application has been direct and swift. The use of a satellite as a navigation aid, with the satellite being an all-weather, day-or-night substitute for the stars was an early success. One chapter of that evolution was described in a prescient Applied Physics Laboratory (APL) internal memorandum of March 18, 1958.[7] Dr. Frank McClure, Chairman of the APL Research Center, recorded the discussions of a meeting he had held the preceding day with two of his researchers in which they discussed the surprising accuracy of orbital parameters being obtained by single, short-arc passes of Sputniks and Explorers. Dr. McClure went on to note, "… it occurred to me that the inverse problem, namely that of locating the observing station by analysis of the Doppler signal of a well-established satellite, would be much simpler and precision would be more easily obtained." Dr. McClure was well aware of the possible defense applications for such a system, and went on, "I believe this could turn out to be one of the most important jobs APL could undertake."

From this projection, and others as well, came the Transit system, Timation satellites, the Air Force's 621B Program, Global Positioning System (GPS), the Russian *Global'naya Navigatsionnay Sputnikovaya Sistema* (GLONASS), Inmarsat's Satellite-Based Augmentation System (SBAS), and the planned European system, Galileo. The Transit system passed from concept to experimentation to full operational status in just six years (1958-1964). The Timation and 621B Programs were merged in 1973 and the first developmental constellation of four GPS satellites was launched in 1978. In the case of GPS, sophisticated science was involved (i.e., calling upon both special and general relativity in the system development because of accelerating reference frames and space-time curvature of the satellite signal). Sophisticated technological development was also required for GPS [e.g., in the development of the space-qualified precision clocks (quartz, rubidium, and cesium) that were first tested in the Timation satellites]. I have skipped still other similar applications such as data collection platforms and the COSPAS-Sarsat search and rescue system.

Why was navigation and position location so quickly accepted? Why did it not experience the difficulties that other space applications have encountered? We can see, perhaps, several reasons. The problems of navigation and position location were of obvious importance in defense systems, and key officials in the Department of Defense, Office of Management and Budget, and the Congress needed little convincing of the value of improvements. Also, every student of the mathematics of management, whether in business school or industrial engineering, learns the basic prescriptions for calculating the minimum cost and optimum timing of equipment replacement. Space-based navigation and position location systems fit both of the two general categories that students must master: (1) where new equipment offers a superior way to carry out present tasks and (2) where new equipment enables entirely new tasks to be performed. Therefore, with a clear, accepted user need and the applicability of familiar cost analysis methods, the navigation and position location systems required no new conceptual framework in their approval and adoption.

Similarly, because the need for the navigation and position location systems could be justified within the confines of the Department of Defense's plans and budget, and because an authoritative assignment of responsibility could be made for the system's development and deployment, there was small need to sort out government-private sector or agency-agency roles. The defense budget provided sufficient latitude to permit the new technology to be developed and deployed without having to offer speculative projections of future civil uses to make the system cost-effective and affordable, although such uses were to appear in abundance. Civilian uses of the new systems were welcome, but unnecessary to support or sustain the deployment of either the Transit or GPS systems. At the same time, because all of the civil users employed the same military satellites, there was a *de facto* establishment of worldwide user equipment standards to serve both military and civil needs.

Furthermore, the federal commitment to the GPS, combined with the compelling value of the new technologies and their complete integration into modern defense systems, gave users an assurance of continuity of service. Users could then make long-term investments with confidence. Likewise, the absence of a fee for the service allowed the user's investment to be confined to the purchase of a device no larger or complex than a hand-held computer and the devotion of sufficient time to learning to apply the new technology. Even more importantly, while the new navigation and position location technology was revolutionary in its impact, the technology was also incremental in concept from a user's perspective.

Finally, enlightened public policy decisions made the navigation and position location systems available for civil use from the very outset, and then enhanced that availability when the Selective Availability (SA) feature, whose effectiveness had already been reduced by the introduction of Differential GPS techniques, was turned off on May 2, 2000 permitting civil users to have full use of the GPS system. As a result of all of the above, and beyond the military uses for which the systems were intended, a strong worldwide civil industry has developed yielding annual revenues of nominally $10 billion.[8]

If we convert the above characteristics of navigation and position location to a generic form for any civil space application fostered by the federal government, the following list might result:

Well-defined user community needs and interfaces:

1. The application addresses an important, accepted agency need, as opposed to serving the needs of another government sector, the public at large, or combinations of both.
2. The application offers accepted, measurable improvement over existing methods.
3. The application creates new capabilities unattainable with existing methods.
4. The application requires only incremental change in users' relationship to tasks.
5. The application provides an assured continuity of service that allows users inside and outside the government to make long-term investments in equipment and training with confidence.

Minimum external dependencies:

6. An authoritative, focused responsibility exists for implementation and operation (no dispute over agency-agency or government-private sector roles).
7. No requirement exists to justify system cost on speculative market projections outside the agency's purview or expertise.
8. Funding is available for system deployment and operations.
9. Standardization of user equipment specifications is under agency control.
10. System enhancements can be planned and implemented under the same entities responsible for development and operations.

Favorable policy environment:

11. Enlightened public policy decisions have been made.
12. No national security issues restrict or block the application's use.

I don't mean to suggest that all of these characteristics are necessarily required by every civil space application advanced by the federal government, but that each should be considered to evaluate the seriousness of the problems that will be created by its absence. In some cases, strategies will have to be developed to overcome the obstacles. Other people may array the characteristics differently, of course, but I suspect that the issues will remain essentially the same.

The contributions to military and civil uses and the creation of business opportunities for equipment suppliers were not the only contributions of the navigation and position location systems; they stimulated other applications as well. A key phrase in the previous quotation from Frank McClure is "well-established satellite," and it deserves amplification. The solution to the inverse problem alluded to by McClure requires a better and better knowledge of the satellite orbit as the position-location error is reduced to meet user needs. Solving this problem required the production of a unified world geodetic datum within which to place the satellite and user. As a part of that effort, an applied science of precision orbit determination and prediction emerged and found application to both military and civil research and applications.

A succession of technologies was employed to provide the supporting data. Experimental satellites were flown to test key concepts, e.g., the drag-free satellite.[9] While the initial objective of the work was the refinement of the knowledge and prediction of the orbit, the orbital parameters also proved to be sensitive measures for determining and refining the parameters of geodetic models in support of the Earth sciences and applications. Satellite laser ranging, lunar laser ranging, very long baseline interferometry, satellite microwave and laser altimetric measurements, GPS measurements, satellite-to-satellite tracking data, and measurements from the French DORIS (*Détermination d'Orbite et Radiopositionnement Integré par Satellite*) system have all made their contributions. These data are used directly and also incorporated in the NASA-National Imagery and Mapping Agency (NIMA)-Ohio State University (OSU) Joint Geopotential Model EGM96, a spherical harmonic model of the Earth's gravitational potential to degree 360.

Among the many uses of these data, variations in the mean sea level are used to monitor the Pacific Ocean's El Niño - Southern Oscillation (ENSO). The ENSO cycle reflects interactions between the surface of the ocean and the atmosphere, with oscillations between warm (El Niño) to neutral or cold (La Niña) conditions on intervals averaging 3 to 4 years. The ENSO cycle has profound implications to the Earth system and produces storms, droughts, loss of fisheries, and other phenomena.

These applications stand in considerable contrast to the navigation and position location applications described above. While the applications often coalesce around particular technologies and standard models, the user communities are diverse and disaggregated. In some cases, practical applications have emerged and national agencies, such as NASA and NOAA, have assumed responsibility for them. In most cases, however, the applications remain in an evolving research phase, and the responsible agencies or user communities have yet to coalesce in the same manner as those in navigation and position location. I invite you to map the characteristics of any of these applications (e.g., variations of mean sea level, monitoring three-dimensional deformations of the solid Earth, or Earth rotation and polar motion) against the qualities I listed above that eased the transition of navigation and position location from R&D to full operations.

An original member of the Space Science Board was Harry Wexler, director of research for what was then the Weather Bureau. He served on the SSB from 1958-1960. He was a staunch advocate for the capabilities of weather satellites well before the first weather satellite was launched, and even commissioned a painting to show the view a weather satellite would have.[10] The magazine *Weatherwise* published the mid-1950s painting in its April 1984 issue celebrating the 24th anniversary of the first weather satellite. As the magazine noted, the painting showed " . . . what the weather might look like at noon on June 21 (summer solstice) from a satellite positioned 4,000 miles above Texas." The painting was based on a cloud photograph taken from a V-2 rocket launched from White Sands in the late 1940s. Wexler's projection was as farsighted as McClure's, but the path has been quite different.

With the advent of the space program, an R&D program to develop a weather satellite was begun under the Department of Defense. A contract was issued in 1956 (a year before Sputnik) to what became RCA Astroelectronics. Upon the establishment of NASA in 1958, the weather satellite program was transferred to NASA's new Goddard Space Flight Center, and in 1960 the polar-orbiting Tiros I was launched. As the technology matured, NOAA assumed responsibility for operational use, NASA continued to develop the first-of-a-kind generations of instruments and satellites, and NASA formed a new research satellite program, the Nimbus series. A classified, polar-orbiting weather satellite program, the Defense Meteorological Satellite Program (DMSP), also continued out of public view to support special defense needs. As a result, the United States was supporting three polar-orbiting satellite systems under the direction of three agencies, and I would assert it was a very good deal indeed for the nation.

The operational civil and defense systems provided mutual backup when one or the other experienced outages and augmented each others data stream under normal circumstances. Somewhat differing user requirements allowed experimentation with a variety of sensors, with each agency creating notable advances that would not have occurred had only one existed. The Nimbus research satellites were superb vehicles, and the achievements of Nimbus-7 rank well with any other research or applications satellite. Any marginal savings that might have been obtained by an early coalescing of the programs would have made little difference to the total United States investment in space technology, but would have profoundly weakened the strength of the applications element of the program.

The polar-orbiting satellites were soon joined by geostationary satellites. A spin-scan camera conceived by Verner Suomi, a colleague who served on the SAB and participated in many SSB workshops, was flown on an experimental NASA communications satellite, the first of the Applications Technology Satellites, ATS-1. This destined-to-be-controversial satellite went on to a twenty-year career providing free public service communication to the islands of the South Pacific. The satellite's camera became the basis for the design of the Geostationary Operational Environmental Satellites (GOES) that were developed by NASA and operated by NOAA.

Paralleling the U.S. activities, Russia, Europe, Japan, India, and China operate weather satellites, and every country in the world receives data directly or indirectly. Strong and very effective international ties have been developed in data sharing and through the Canadian and European instruments provided for use on the polar-orbiting operational satellites. Later, Europe provided the loan of a geostationary satellite to the United States when the U.S. geostationary program was experiencing problems.

Ultimately, NASA withdrew from its commitment to develop first-of-a-kind instruments and spacecraft for use by NOAA in favor of developing research instruments that could serve as precursor, but not necessarily identical, instruments for the operational spacecraft. While the NASA decision caused early difficulties,[11] a new accommodation has been reached between NASA and NOAA that promises to be of equal benefit. Eventually the U.S. military program was declassified, and in 1994 the two programs were merged and placed under NOAA, with joint military, NOAA and NASA staffing. Within NOAA the two polar-orbiting programs were managed with the civil geostationary weather satellite program, and will be further combined with the launch of the joint National Polar-orbiting Environmental Satellite System (NPOESS).

In addition to military and civil governmental needs, the weather satellites began to serve commercial applications as well. Private firms emerged that tailored the data products for television news broadcasts and specialized weather forecasts. Beyond value-added products, the private sector builds all of the satellites and equipment upon which the government and users rely. In most cases, the private sector also operates the equipment and processes the data. The market is not nearly as robust as that for navigation and position location equipment, but meteorological forecasting and the generation of storm warnings have always been more closely allied with federal responsibilities than with the activities of private firms or the demands of the consumer marketplace. For most of the history of weather satellites, the data have been provided free, but now a number of countries are beginning to try to recoup some system costs by the imposition of user fees.

Obviously, the path taken by the world's weather satellites is far different from the nearly linear path of navigation and position location. Whether a more orderly path would have benefited the public in the past, or would do so in the future, is an interesting subject for cocktail hour speculation. Nevertheless, agencies and nations have banded together to keep the weather satellite system operating and advancing in capability. Probably every precept implied or stated in the table of characteristics given above has been violated in this space application in one way or another, but somehow the application survives. Am I arguing that rational management would have harmed the program and squeezed out innovation? I don't know; maybe I am.

That brings me to the last of the topics for this column, space weather. Space weather, the operational side of solar and space physics, is underpinned by one of the great intellectual achievements of the space program, the advances in understanding of the solar terrestrial environment made possible by spacecraft and the associated research programs (see ref. 12 for a particularly cogent discussion and a historical perspective). The National Space Weather Program defines space weather as "conditions on the sun and in the solar wind, magnetosphere, ionosphere, and thermosphere that can influence the performance and reliability of space-borne and ground-based technological systems and can endanger human life or health."

As noted at the outset of this column, the Board – via its Solar and Space Physics Survey Committee – is completing the study *Solar and Space Physics: A Community Assessment and Strategy for the Future*. The Survey Committee reports to the Board through the Committee on Solar and Space Physics, which is also completing a related study entitled *Solar Connections: A New Emphasis for Solar and Space Physics*. Furthermore, studies of changes in the Earth's climate have reemphasized the need to carefully monitor the Solar Constant.

These studies and the body of knowledge upon which they build demonstrate the value in the operational monitoring of the Sun, its emissions, and their interactions with the Earth and its magnetosphere. The importance of such monitoring has been long recognized by the community, and some steps have been taken beyond the very successful research missions that have produced the initial understanding. The operational satellites of NOAA and the Department of Defense have routinely carried fields and particles instrumentation in both low-altitude and geostationary orbit. A solar X-ray imager was recently added to NOAA's geostationary weather satellites. As welcome as that addition was, I should acknowledge that the community had been requesting this capability for more than two decades, including requests made to me to which I was unable to respond.

In addition, operational warning systems have been established to issue alerts of potentially harmful or dangerous events; you can now go to the web and get the current space weather, and that is certainly a significant advance over what existed not too many years ago. Unfortunately, despite the many practical applications of solar and space physics, it has been extraordinarily difficult to move to the sort of operational satellite constellation and ground system that would best serve those applications. Even a casual perusal of the characteristics that led to the success of satellite navigation and position location shows that space weather forecasts and alerts will have difficulty achieving the same operational status as meteorology, but as our technological systems become increasingly complex and sensitive the arguments for advancing the status of space weather may become quite compelling.

Our system of government handles sophisticated successes awkwardly, and often finds epigrammatic goals more comfortable. Unfortunately our space program has created capabilities that cannot be captured in easy phrases, and capabilities that do not always mesh well with sweeping political declarations. Our Board must address these complexities with sophistication and insight, and these complexities will exist for as long as I can see into the future. It was for that reason I chose to devote this quarter's column to the topic of space applications. Another way to state the topic of this column would have been, "What do we do about success?"

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Learning from the past or reliving it: Lessons from the Hubble Space Telescope
(June 2002)

This past spring we saw two events that reminded us why the heavens have always fascinated people. The first was the conjunction of the visible planets with the Moon that appeared low in the western sky just after dusk. By the middle of May, the seldom-seen conjunction showed Jupiter, Venus, Mars, Saturn, Mercury, and the Moon aligned in nearly a straight line that was canted southward, a little off vertical, as viewed from my backyard. Well-crafted descriptions appeared in many newspapers, and often on the front page.

The second event was the release of the first images from the new Advanced Camera for Surveys (ACS) that was installed on the Hubble Space Telescope (HST) during a very challenging and successful servicing and repair mission carried out from the Space Shuttle. The striking images showed once again the artistic quality of many views of natural objects and events. Even without the insights of a trained eye, the images are marvelous. Again, excellent press coverage (especially in news magazines) was provided. The HST results have long overshadowed the difficulties of its earlier years. Nevertheless, I want to make a few comments about those early years.

It isn't my purpose simply to review the early history of the HST, as that history is readily available elsewhere in both scholarly and more subjective versions.[1-4] Rather than reviewing HST history, my purpose here is instead to draw a few threads from the project's history and see whether they comprise some cautionary lessons that we might employ in the Board's work. For those who are interested in the HST itself, it would be better to consult the references or an expert, such as our colleague, Roger Angel (a current member of the Board who served on the HST failure review board).[5]

While I was not personally involved in the HST, its problems, or its several remarkable fixes and improvements, I do recall quite vividly the atmosphere that surrounded projects when the HST Project began. It is that atmosphere that I believe has continuing relevance to the SSB. I would characterize the atmosphere as a mixture of hubris and deep concern. Hubris stemmed from the admirable and undeniable successes of the Apollo program that made anything seem possible. Deep and entirely understandable concern stemmed from diminishing budget allocations in the post-Apollo era and the consequent difficulty in gaining approval for major new initiatives.

In the aftermath of Apollo, it was hard to argue that some things remained very difficult to do. Historical examples of overly conservative or incorrect predictions were used against those suggesting caution, such as the oft-quoted and very tired example of the limited market for computers originally projected by the head of IBM. Counter examples of overly optimistic projections (e.g., a helicopter in every garage and electricity produced so cheaply by nuclear plants that it wouldn't be cost-effective to meter it) that were at least as numerous and equally flawed were never offered in the environment of unchecked optimism. An especially pernicious view that was propounded, and – even worse – acted upon through incorporation as an assumption in budget estimates, was that space system engineering was now routine practice, and that a variety of longstanding precautions could be eliminated wholesale – notably the parallel development of alternative technologies or approaches, mock-ups, close oversight, and extensive testing. For example, consider the blanket elimination of prototype or back-up spacecraft for all projects no matter how large or small, complex or straightforward, or costly or cheap. It became acceptable to develop billion-dollar missions without a back-up – as if to say that, while we were willing to spend the money on one unit, we didn't *really* need the scientific results so badly as to take precautions. The single proto-flight model became the only permitted approach. Another ramification of the cost ceiling on the HST Project was the severe restriction on the number of in-house personnel that could be employed by NASA's Marshall Space Flight Center. The ceiling was also a reaction to Marshall's reputation for deeply penetrating the projects it supervised, often to the chagrin of contractors. Obviously, this proved to be a false economy.

Naturally these assumptions resonated favorably with NASA's desire to continue to undertake ambitious missions despite severe budget restrictions. Regarding cost and schedule estimates, I recall a quotation, but whose source I no longer remember, to the effect that, "We believe what we want to be true." The assumptions also resonated with a dominant political philosophy often propounded by both political parties that the government was placing needless burdens "on the back" of industry and that less oversight was needed. It takes a strong personality to withstand criticism for being too cautious when placed in the position of having "to prove a negative," as in not being able to show incontrovertibly that some aspect of a project is too aggressive. Likewise, it takes an even stronger personality to hold tenaciously to a cost estimate when told that the size of the estimate will doom the continuation of the project and possibly put your colleagues' jobs at risk.

It was in this environment that the Shuttle and payloads such as the HST emerged, with both seeking passage through the budgetary *New Start* gate near the same time early in the 1970s. As Tatarewicz has written[3]:

> … With austere times ahead after Apollo, the Space Telescope and the Space Shuttle soon found common cause. An orbital telescope of that size and cost could not be justified unless it could operate for years or decades, and to that end the Space Shuttle promised routine access for repair and upgrade. … Lacking the Space Station … the shuttle needed a place to go and useful work to do that could not be accomplished by expendable boosters. To that end, the Space Telescope and Space Shuttle pair became an exemplar of a new and cost-effective way of doing Earth orbital science.

Of course, the roots of both the Space Telescope and the Space Shuttle extend much farther back than the 1970s, but it is the more recent history that is germane here.

The cost effectiveness of the Shuttle was extensively debated at its birth and the debate led to the now well-known 1970 *Mathematica, Inc.* studies led by Klaus Heiss. As one especially articulate Shuttle supporter wrote[6]:

> …Heiss's bottom-line conclusions were as follows:
>
> - Based on the best estimates and projections available, the fully reusable shuttle would be *marginally* cost-effective. The dominant factor would be the total number of flights, which depended on the further demand for payload launchings; whether or not the payloads were manned or unmanned made very little difference.
> - The principal savings would come, surprisingly enough, not from reducing the cost of transportation itself, but from reducing the stringent *payload* specifications required by expendable launchers. That is, the big shuttle payload bay and low-acceleration launches … allowed much cheaper payload construction; further, the ability to repair payloads in orbit, replace worn-out or defective parts, or to return them to earth for modernization or salvage offered considerable overall cost reductions. …

> Because the *Mathematica* report revealed such barely marginal benefits (the "most probable" estimate calculated by Heiss showed scarcely $100 million savings from a $12.8 billion investment), the highly likely event of even a minimal cost overrun could wipe out the entire economic rationale for the shuttle …

From the perspective of our Board's work, the second bullet is the crucial one, not only for what is said directly in the above quotation, but also because of the implicit assumption that the cost of repair missions would be cheap – at least to the user. These assumptions directly affected the development of payloads in general and the HST in particular.

Throughout the history of the human space flight program up to the Space Shuttle, justification had been built on the asserted human imperative to explore, international competition, and the benefits of aggressive technological development. Because the benefits of human space flight were considered to be unquantifiable, conventional cost-benefit analysis was not employed. Furthermore, the costs of the human space flight program were not levied on the budgets of the other program offices within NASA. As a result, and in keeping with past practice, the cost of the Space Shuttle's development and operations was separately carried in the NASA budget. Thus, the use of the Shuttle was nearly "free" to the program offices from a programmatic perspective. Of course, the Shuttle was itself a powerful competitor for funds within the agency's overall budget.

However, the Heiss report postulated a cost-effectiveness goal for the Space Shuttle element of the human space flight program based on cost reductions for other parts of the national space program. A policy framework was then established to protect the forecasted cost-effectiveness, and indeed to protect the human space flight mission of NASA: (a) a very high flight rate was predicted (in the vicinity of one flight per week during early discussions), (b) a Shuttle-only requirement was established for government payloads, (c) a subsidized and aggressively competitive launch fee structure was created: no-cost shuttle flights for NASA customers and low-cost for other government and private customers, and (d) a requirement that those seeking to use expendable launch vehicles rather than the Shuttle pay program dollars and full cost. All of these policies would be revisited after the Challenger accident.

In light of the above considerations, the HST was developed with misestimated and highly constrained costs, a requirement to use the Space Shuttle, optimism over the feasibility and cost of on-orbit repairs, and a conviction that space system engineering was routine. For each of these there were consequences. The HST Project never achieved schedule and funding stability, and as a result the management at all levels of all participating organizations remained consumed by cost overruns and schedule slips, when their time would have been better spent addressing the technical issues of the project. Constrained costs also led to reductions in development tests.

The use of the Shuttle limited the choice of orbits, and the orbits the Shuttle could reach limited the time for continuous observations and subjected the spacecraft to repeated thermal cycling as it passed in and out of the Earth's shadow. Optimism over on-orbit servicing and repairs led to a perception that higher risk approaches were acceptable in development, because matters could be rectified later. The expectation that repair flights would not be fully charged to the HST Project, despite being planned to occur every two-and-one-half years, further encouraged reliance on on-orbit repairs. As mentioned above, the conviction that space system engineering was routine, and as a result costs could be reduced, militated toward minimizing the in-house staff overseeing the contractor and subcontractors.

The HST Project experienced still further complications stemming from having an international partner and employing U.S. technology developed for highly classified intelligence missions. The assignment of the solar array development task to an international partner, for example, was done under pressure on NASA to reduce project costs, but the assignment raised concerns about technology transfer that were related to both industrial competitiveness and national security. Arrays meeting similar requirements had been built for classified programs, but the arrays and their performance were sequestered from the HST Project as part of the effort to protect reconnaissance satellite technology. Interestingly, British Aerospace, which ultimately manufactured the HST array, is a firm that is well acquainted with classified work and joint ventures with U.S. firms. Unfortunately, the arrays were to become a major problem due to their vigorous flexures during the frequent thermal cycling of the HST orbit. Ultimately, the solar arrays were replaced by more rigid panels made possible by the higher efficiency of gallium arsenide (GaAs) cells. The new arrays were developed for the Iridium Project.

Of course, the use of sensitive technology went well beyond the solar arrays, and especially involved the primary mirror being built and tested by the then Perkin-Elmer, a key subcontractor to Lockheed, the prime contractor. It was naturally believed that Lockheed's skills, honed in building a long series of spacecraft using similar technology, would be applied to the HST and would reduce risk. Perkin-Elmer had been a subcontractor to Lockheed on the KH-9 reconnaissance satellite[2, 9], and it was expected that Perkin-Elmer's skills and the past relationship would reduce the risk for the HST. Furthermore, Perkin-Elmer had developed a sophisticated, computer-controlled device for more rapidly and precisely grinding large telescope mirrors, the reflective null corrector.

The use of reconnaissance satellite technology restricted the number of people available to review designs and tests, and unfortunately also gave a false comfort regarding the ease with which HST could be built. It was assumed that the HST mirror was simply another in a series of mirrors with considerable flight experience. As the HST Project proceeded, and even beyond the fact that relatively few NASA employees carried the clearances needed to access reconnaissance satellite technology at the outset of the HST Project, a major government effort was begun contemporaneously to reduce the total number of cleared personnel in both the government and private industry. This latter step was taken in the aftermath of several infamous espionage cases. However, the reductions in the numbers of clearances had its own troubling effects, and made it even more difficult to deploy people to assist the development of HST when difficulties were experienced. The effects of more closely holding technology exacerbated the previously described pressures in the HST Project. Thus, for a variety of reasons, the U.S. Government did not allow the application of its best available information to the development of the HST. The reasons were understandable, but the consequences proved to be serious.

The HST Project saw the joining of the techniques of human space flight, the objectives of space science, and the technology of reconnaissance satellites. Of course, the nation's full capabilities should be brought to bear upon such a major national program. Moving to the present, the new NASA Administrator has called for closer collaboration between human space flight and robotic space flight, as in a recent statement[7]:

> For example, there is a necessary link and connection between our human space flight program and our work in robotics. NASA must eliminate the stovepipes and build an integrated strategy that links human space flight and robotic space flight in a stepping stone approach to exploration and discovery.

The Administrator has also on a number of occasions called for closer collaboration between national security agencies and NASA. Certainly no one can dissent from collaborations that advance all of the nation's interests. However, the HST showed both the benefits and, in some cases, the difficulties that can occur in such collaborations, and the SSB must be wary as we review programs and projects involving them.

International collaboration in the HST complicated the use of defense technology. Security restrictions and a flawed vision of how much oversight was needed interfered with the proper scrutiny of work on the HST's primary mirror, and also shielded what proved to be a recalcitrant contractor. Budget restrictions prevented the full testing that would have detected the flaw built into the mirror, but it must also be acknowledged that the contractor ignored vital clues in the simple tests that were performed and used bad practice in the principal test that was conducted. The bad practice was the lack of a procedure to detect an error in assembling the test fixture and failing to have an independent means to verify the measurement approach. The human space flight program saw the Space Shuttle as a means to continue its work until NASA was called upon to again tackle a new goal, and fought successfully to gain the Shuttle's dominance among the launch vehicles that NASA's program offices could employ. The science community associated with the HST had no choice but to employ the Shuttle. Some in the science community saw the potential for useful capabilities and lower costs, but accessing the capabilities made possible by human space flight technology carried with it compromises that were not trivial. Thus, in the HST example, the three communities (science, human space flight, and national security) were motivated by quite different and sometimes conflicting objectives.

Looking back, the servicing and repair capabilities made possible by the human space flight program allowed corrections to be made for the error in the figure of the mirror that originated in the secrecy of the defense program. The servicing and repair capabilities also allowed the correction, repair, and updating of other elements of the HST. The ability of astronauts to carry out repairs and upgrades was again demonstrated to great advantage with the HST, as they had been demonstrated earlier with the simpler Solar Maximum Mission (SMM). Certainly the experience gained on HST has contributed greatly to the assembly of the International Space Station (ISS).

On the other hand, the savings projected for development costs in the expectation of being able to service and repair the HST on orbit failed to materialize (just as it failed in other missions), and the application of principles of full cost accounting may inhibit further servicing missions. Because of the skills and extraordinary dedication of the astronauts, servicing can indeed be carried out as well or even better than originally foreseen, but the cost effectiveness of servicing remains unproven. The direct cost of the first Hubble servicing mission was about $500 million, not including the cost of the Shuttle flight and mission and data operations support. That places the full cost accounting figure at nearly half the cost of the observatory.

The Space Shuttle and ISS were both projected to have major roles in spacecraft servicing and repair. Those projections have now gone away. The orbits of the Shuttle and the ISS are simply too distant from the orbits where the bulk of spacecraft need to be. The most numerous satellites and those having the strongest economic base are those for communications, navigation, and earth observations and can neither be reached by piloted spacecraft nor brought to the Shuttle or ISS orbits via their own propulsion systems. Thus, the aerospace community has by default reached its own conclusion regarding spacecraft servicing and repair, and the conclusion is not favorable.

The servicing and repair missions carried out by the Shuttle have been great technological successes, but had greater care been taken during development could we not have achieved equal or greater scientific success by placing a well-crafted telescope in a more desirable orbit to which astronauts could not travel? I don't know the answer to that question, although I have my suspicions or perhaps my biases. Also, in a full cost accounting environment, the trade-offs are even less clear. Furthermore, my temptation is to judge the HST's use of a primary mirror with roots in the most sensitive of classified technology to have been a failure. I don't see how any full accounting of the repair and service costs can fail to produce a number that swamps the savings that resulted from using this technology. The HST has taught us that, when we bring together disparate interests (human space flight, national security, and space science) for the most commendable of reasons, we must make certain that the inevitable trade-offs that will be required are justified and not excessively harmful to a mission's purpose.

As noted above, the ISS has been a beneficiary of the HST experience. Another beneficiary has been the Next-Generation Space Telescope (NGST). In recent briefings to the Board, NASA's NGST Project has shown that it has studied the HST experience carefully. While the NGST will not be a spacecraft serviceable by astronauts, nevertheless the HST experience in system development and test will be directly relevant.

As we approach the scientific uses of the International ISS, we are again merging the goals of human exploration of space with those of scientific research. During the early planning, the expectation was that astronauts would play the role of laboratory researchers. As a result of very large overruns in the ISS project, that concept has had a considerable setback with called-for reductions in research funding, research apparatus, and crew size. Indeed, researchers are being advised by NASA to reduce their reliance upon astronauts and to increase the automation of their experiments. In the absence of having a capability for human intervention and interaction with scientific equipment is the ISS the appropriate platform upon which to carry out research in the biological and physical sciences?

In the HST Project the international connection inhibited some possible actions, but was benign in other regards. In the ISS, the changes being sought by NASA have profound impacts on the International Partners (IP) that NASA so carefully assembled in creating the ISS program. During the early phase of the most recent changes in the ISS plans and funding, the Partners were left out of the decisions that led to a new definition termed the "U.S. Core Complete" that was to bound the U.S. participation in the ISS – perhaps temporarily, perhaps permanently. As this new configuration was not envisioned in the Intergovernmental Agreements (IGA) that had been negotiated among the Partners, considerable consternation resulted. That consternation was not eased when NASA announced its position that it would, "Respect the provisions for IP design interfaces and operational accommodations, as stipulated in the IGA/MOUs, *and as negotiated and interpreted by the U.S. Govt*." (emphasis added to the final phrase, which has been taken by some to modify the agreed-to IGA). NASA is now at work repairing the ties to the Partners, and I assume NASA will be successful ultimately.

In the midst of those efforts, however, the U.S. agencies, private sector, and their current and future international partners are mired in the processes and procedures associated with the International Traffic in Arms Regulations (ITAR). While separate from the ISS discussions to some extent, the problems that have been created by recent changes and interpretations of ITAR are affecting administrators, scientists, and engineers in all U.S. agencies involved in space activities and in all of the IP nations. Many of the same people are involved in both the ISS discussions and the effects of the ITAR, and the conjunction is not a happy one. It is not clear to me how international partnering in space research can be discussed, what practices and procedures may be allowed, or what schedule may be feasible to gain governmental permission for normal project activities. NASA, the Department of State, and Department of Defense are attempting to address some of the difficulties created by ITAR, but most discussions leave me baffled, and the personal liability contained in the regulations chills possible international collaborations.

The NASA Advisory Council has a Research Maximization and Prioritization (ReMaP) Task Force to explore the issues associated with the major redirection of the ISS, and its report is now scheduled to be released in mid-July. NASA has assembled an exceptional panel of individuals to carry out the study. The NASA Administrator has committed to a utilization of the ISS that will be driven by science priorities, and in a broader context has said[8]:

> We are going where the fundamental questions that we seek to answer take us. … NASA's mission … must be driven by the science, not by destination. And while policy and politics and economics are inevitable factors, science must be the preeminent factor.

That is a statement from which the SSB could scarcely dissent. Later this summer we will receive copies of the ReMaP Task Force report, and we will certainly read it with interest. This has been a difficult period for our NASA and international colleagues, and for our science colleagues who have been relying upon the ISS to conduct their research. I am sure that we all look forward to a path for the productive utilization of this enormous investment in space technology. The HST experience offers many lessons for us to ponder as we move forward.

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4 ROBOTIC VS. PILOTED SPACE FLIGHT

Connections to the family tree of post-war rocketry (September 2002)

Rocketry has a complex family tree, but we are fortunate because many currently relevant parts of the tree can still be drawn using the insights of key contributors. Our work as members of the Space Studies Board (SSB) and its committees connects us to the family tree of post-war rocketry and to the scientific and technological forces that have shaped it.

One of the marvels of the Internet is the ready access provided to histories and original documents, e.g., the files available through web sites of the NASA History Office,[1] our own National Academies archives,[2] and the records of the IEEE History Center.[3] In the first two cases, discussions of the SSB's early history are to be found, and we can also find references to many of our friends and colleagues. In the IEEE site, the MIT's Radiation Laboratory (Rad Lab) series of oral histories connect readers to many famous individuals who were or became major figures in the National Academies, National Research Council (NRC), and the NRC's various bodies. The Rad Lab history is also preserved in the web site of MIT's Research Laboratory of Electronics (RLE), the successor to the Rad Lab.[4] I'll connect the Rad Lab's contributions to the SSB's work a bit later in this column.

The familial roots of NASA and the SSB grow from – among others – the National Advisory Committee for Aeronautics (NACA), Muroc Air Force Base (later to become Edwards Air Force Base and Dryden Flight Research Center), Patuxent Naval Air Station, Langley's Missile Test Station (to become Wallops Flight Center), the Naval Research Laboratory, MIT's Radiation Laboratory, Johns Hopkins' Applied Physics Laboratory, Cal Tech's Jet Propulsion Laboratory, Army's Redstone Arsenal, Bell Telephone Laboratories, the Air Force and its intercontinental ballistic missile (ICBM) and space programs, and the southwestern deserts with White Sands Proving Grounds (later to become White Sands Missile Range) and the New Mexico firing ranges used by Fort Bliss, Texas.

However, it wasn't only the rockets that were significant; other enabling technology was equally important. From MIT's Radiation Laboratory came the magnificent 28-volume Rad Lab library of books that tutored so many of us working in numerous areas of electronics (e.g., Samuel Silver's landmark and still-valuable book on microwave antennas[5]) and the SCR-584 gun-laying radar of World War II, which among other accomplishments shot down 89 of 91 V-1 rockets that the system engaged over London and Antwerp in August, 1944. The Rad Lab books were illustrative of the profound changes that were to occur in engineering education due to the technologies emerging from the war, as vastly greater theoretical foundations were found to be necessary to accommodate new design tasks demanding sophisticated electromagnetic theory and a much deeper understanding of solid-state physics. It is not accidental that many of the key participants recruited for the Rad Lab's radar work came from the nuclear physics community. I would acknowledge only a slight oversimplification in saying that the era saw engineering change from a handbook and procedures oriented profession to one solidly based upon research and modern science.

Ernest Pollard's brief book gives a wonderful insight into that era and the many deservedly famous contributors of the Rad Lab (seven of whom received the Nobel Prize in Physics and one the Nobel Prize in Chemistry).[6] The success of the gun-laying radar was dependent not only upon the radar itself, of course, but also upon the newly developed variable time or VT fuse that prompted detonation of the antiaircraft artillery shell by sensing the presence of the target within the shell's destructive range. The connection between a VT fuse and the SSB may not be obvious. The VT fuse was one of the projects upon which our SSB colleague, James Van Allen, worked at APL during the war, and the difficult environment and miniaturization requirements provided valuable experience for his later work on sounding rocket and satellite instrumentation. Likewise, another long-term SSB colleague, Herb Friedman, had worked on the VT fuse as well before joining NRL, where he was destined to make so many contributions.

The SCR-584, however, has had an even longer role in the post-war era than it did during the war, as it became a key device in tracking experimental aircraft and missiles. Even today, the Internet steers people to sources of parts and information as the radar continues in service. Walt Williams and Jerry Truszynski, two people well known to the SSB, used the SCR-584 radar to track Chuck Yeager's historic supersonic flight at Muroc. The radar even played a small role in the movie *The Right Stuff*, although it was noted by knowledgeable viewers that the radar had been filmed in the incorrect operating mode. Similarly, the rockets launched from both Wallops and White Sands were tracked by the workhorse SCR-584 radar.

Paralleling the MIT work, Bell Labs made many contributions to national defense, and worked on radar systems since 1937 with Navy funding.[7] Among the other contributions that are especially germane here were pulse-code modulation, information theory, and the transistor. The Bell contributions included overseeing the development of large, complex systems, such as the successors to the SCR-584, the M-33 and especially the Nike Ajax antiaircraft missile, but more about the last a bit later in this column. As had Rad Lab, Bell and its staff also contributed more than its share of tutorial books that, while designed principally for Bell employees, taught modern communications to several generations of engineers. What communications engineer did not have a copy of Bell's *Transmission Systems for Communications* (or its predecessors) on his or her bookshelf? Not many, I think.

White Sands, not far from the Roswell testing grounds for Robert Goddard's pioneering liquid-fueled rockets of only a decade before, became the launch site for captured V-2 rockets and many later rocket generations, as our colleagues and past board members learned to conduct upper atmosphere and solar research from converted weapons. Richard Porter of General Electric was in charge of the V-2 launches. Wernher von Braun and the Operation Paperclip scientists, engineers, and technicians passed through Fort Bliss and White Sands on their way to Redstone Arsenal. Van Allen, Friedman, Jack Townsend, Bruno Rossi, John Simpson, Harry Wexler, Milt Rosen, S. Fred Singer, Dan Mazur, Al Jones, Jack Mengel, Herman LaGow, and many others taught us the potential of what was then new technology as they launched the remaining V-2s and began the development of new generations of sounding rockets, and eventually satellites. As had occurred with the SCR-584, the wealth of technology and surplus military equipment led to ingenious and unpredicted applications. For example, the booster of the Honest John battlefield nuclear missile became the first stage of a sounding rocket whose second stage was the booster from a Nike Ajax antiaircraft missile and upon which was placed a variety of further upper stages.

Like the SCR-584, the operational military life of the Nike Ajax was brief, only a decade, but its boosters and tracking equipment were incorporated in many research projects, and led to still further generations of antiaircraft and antimissile missiles. Sadly, although the Ajax was intended to defend against hostile aircraft, the only injuries and deaths the Ajax caused were to its own crew members, as it was a most temperamental and unforgiving rocket that taught us all too well the dangers of rocket fuels, explosives, and static electricity. Ironically, the *Bomber Gap* leapt into the public mind when (on the Soviet Aviation Day in July 1955) 10 Bison strategic bombers circled past the reviewing stand repeatedly to give the impression of a force of 60 jet bombers, which analysts projected as growing to 600 bombers. As a result, the United States soon had nearly 80,000 regular and National Guard soldiers manning guided missile sites (initially Nike Ajax and then the nuclear-armed Nike Hercules) around U.S. cities and critical installations. About a quarter of the soldiers were skilled officers and highly trained technicians in short supply throughout the military services. The Soviets had already made their choice to emphasize missiles rather than bombers, but welcomed the diversion of U.S. resources.[8]

As an example of the research projects the Ajax hardware would ultimately support, the tracking mounts of the Ajax and its immediate successor, the Nike Hercules, were ideal to carry the laser transmitters and receivers for the satellite laser tracking systems of NASA's solid Earth and precision orbit determination program. Many other uses were found as well.

Of course, by the late 1940s and early 1950s it was obvious that a replacement for the V-2 and the *ad hoc* assembly of surplus military rockets was necessary. With Navy funding, Van Allen and his colleagues at APL led the development of the Aerobee, which became immensely popular. The "Aero" part of the name came from the prime contractor Aerojet-General. The Navy also funded, through NRL, the development of Milt Rosen's elegant Viking rocket.[9] The prime contractor for the Viking was Maryland's Glenn L. Martin Company. The Viking rocket was intended to succeed the V-2, and it had many improvements. Among them was the replacement of the V-2's attitude control system that had been based on steerable graphite vanes immersed in a fixed rocket motor's burning exhaust gases by the Viking's servo-controlled, gimbaled rocket motor.

Despite having greater capability than the Aerobee, however, the Viking failed to gain wider acceptance because of its much greater cost (the Aerobee was less than 10 percent of the cost of a Viking, and the available research grants simply could not support the higher cost). Nevertheless, the Viking was a considerable engineering achievement, and was upgraded to become the first stage of Vanguard, just as the Army's Redstone was upgraded to be the first stage of the Jupiter-C. The research and development carried out in the Viking project was to contribute to numerous other space and missile projects, notably the intermediate-range ballistic missiles. Homer Newell noted that the competition between Aerobee and Viking was emblematic of a larger issue. He wrote[10]:

> The contrast between Aerobee and Viking typified a situation that has recurred in the space science program. One group of scientists would favor developing large new rockets, spacecraft, or other equipment that would greatly extend research capability. Another group would prefer to keep things as small and

simple as possible, devoting its funds to scientific experiments that could be done with available rockets and equipment. The former group could always point to research not possible with existing tools, thus justifying the proposed development. In rebuttal the latter group could always point to an ample collection of important problems that could be attacked with existing means.

In the quarter of a century since Newell wrote those words, the cogency of his observation has been demonstrated again and again. Two examples from X-ray astronomy illustrate the power of fairly modest sounding rocket payloads. NRL had a longstanding interest in the upper atmosphere, and the source of its ionization. Herb Friedman pursued that interest and flew instruments on V-2s and various sounding rockets, as well as the ingenious Rockoon (a rocket suspended under a balloon capable of loitering at 100,000 feet); his instruments detected solar X-rays,[11] and his research connected solar X-rays to the existence of the ionosphere. Friedman carried out complex experiments in which a Rockoon would be launched over the Pacific Ocean early in the morning that (after reaching its loiter altitude) then awaited word from New Mexico's Sacramento Peak Observatory that a solar disturbance was underway, leading to the command to launch the rocket and its payload. During a solar eclipse, and in another examination of X-rays from the Sun, a series of six Nike-Asp rockets were launched from the deck of the U.S.S. *Point Defiance*, while Friedman worried, "we had some nightmares of the good ship *Point Defiance* going to the bottom when we exploded the first Nike." [11] Later, past SSB members Riccardo Giacconi and Bruno Rossi led the development of an X-ray detector that was flown on an Aerobee on June 18, 1962 and discovered the first X-ray source outside the solar system, Scorpius X-1.[12, 13] Flexible, affordable technology firmly under the control of the researchers led to striking results.

In the immediate post-war era, scientists who had often been pressed into service as engineers were anxious to resume their research careers, and were also anxious to employ the many new technologies that had emerged from military developments. We are the inheritors of those scientists' work. Thus, the roots of the SSB's family tree began in wartime with people and technologies that would be crucial to the space program and to the efforts of our Board. The trunk of the tree was firmly rooted in and nourished by military technology, but was soon to divide into two great branches, and it is the division of the trunk to which I will now move. The branching is superbly described in DeVorkin's *Science with a Vengeance*,[14] and I commend the book to anyone interested in the history of space science.

Throughout the buildup of science and technology that would lead to the International Geophysical Year (IGY), the United States relied upon a series of very effective committees. Among them was the V-2 Upper Atmosphere Panel, which became through several name changes the Rocket and Satellite Research Panel (RSRP). The Panel involved many familiar names: E. Krause, F. L. Whipple, W. G. Dow, M. J. E. Golay, J. Van Allen, H. Newell, W. H. Pickering, W. Stroud, J. W. Townsend, K. Ehricke, E. Stuhlinger, W. von Braun, W. W. Kellogg, J. Kaplan, R. Tousey, H. Friedman, W. Nordberg, to name only a few.[10]

The potential of satellites was well recognized in the mid-1940s and early 1950s. On the one hand were studies funded by the military[15, 16] and on the other hand space enthusiasts published their visions far more openly.[17, 18] Beyond the studies, secret efforts were begun in 1954 to develop an intercontinental ballistic missile (ICBM). This was actually a restart, after an earlier effort was terminated in a Pentagon budget reduction. In 1956 the recognition of the satellite launching capability of the emerging rocket technology led to the establishment of two programs: the Vanguard civilian program to support the IGY and the highly classified WS-117L program under contract to Lockheed. The latter program was to spawn by 1959 the Discoverer, Satellite and Missile Observation System (SAMOS), and Missile Detection Alarm System (MIDAS) satellites.[19] The budget for the military satellite program was more than twenty times that for the civilian program in the time leading up to the formation of NASA.

The separation of civil and military satellite programs was a conscious policy decision of the Eisenhower administration. The first classified satellites were scheduled to be launched in 1958, and interestingly they were to be approximately the weight of Sputnik III that was launched in 1958 and that was to raise so much national concern. Of course, the public remained unaware of the classified satellites and their successors until many years later. It is interesting to speculate whether the concern and even hysteria that was stimulated by Sputnik would have been as great had the public known that the *Satellite Gap* was the close sibling of the earlier *Bomber* and *Missile Gaps*. Naturally, the industrial base that supported the classified program also served, in most instances, the unclassified program, so the full national capability was rarely on view. Nevertheless, the United States lost an important public relations war when Sputnik-I was launched, despite being in the midst of efforts that would quickly give the country a clear lead in space capability.

Returning to the main story leading to the SSB, the most important next step took place on April 5, 1950 at a dinner hosted by James Van Allen and his wife, Abbie, at their Silver Spring, Maryland house to honor Sydney Chapman, Sedleian Professor of Natural Philosophy at Queens College, Oxford.[20] Chapman was in the process of relocating from Oxford to the United States at the time. In attendance was Lloyd V. Berkner, a veteran of the First Byrd Antarctic Expedition and distinguished research administrator who was later to be President of the International Council of Scientific Unions (ICSU). Also in attendance were geophysicists J. Wallace Joyce, S. Fred Singer, and Ernest H. Vestine. Predicated on the availability of so much technology flowing from the war, Berkner proposed a third International Polar Year (IPY) twenty-five years after the second IPY, rather than the fifty-year separation between the first and second. I find it most important that the model of the IPY so imprinted the efforts that were to follow. The second IPY had involved hundreds of researchers from 44 countries involving principally meteorology, magnetism, and the aurora, but other disciplines as well. The original resolutions were issued in eight languages.[21] It would have been inconceivable to carry out either the first or second IPY as a national venture, and that recognition conditioned the thinking regarding a third IPY.

The tireless enthusiasm and advocacy of the small dinner-party group (and especially Chapman and Berkner) ultimately led to a renaming and the adoption of an IGY in 1952 by ICSU, and the designation of a *Comité Spécial de l'Année Géophysique Internationale* (CSAGI) headed by a bureau. The CSAGI bureau was made up initially of Sydney Chapman (President), Lloyd Berkner (Vice-President), Marcel Nicolet (Secretary-General), with Vladimir Beloussov and Jean Coulomb added as members later. ICSU also established the Committee on Space Research (COSPAR) to which CSAGI would report. Today, the SSB remains the U.S. National Committee to the COSPAR. While the IGY could not be and was not entirely free of issues of nationalism, science was dominant and the modest nongovernmental organization that was put in place was largely apolitical. Expanding upon the disciplines involved in the second IPY, the IGY had panels on aurora and airglow, cosmic rays, geomagnetism, glaciology, gravity, ionospheric physics, longitude and latitude determination, meteorology, oceanography, rocketry, seismology, and solar activity.[22] Thus, from the very outset, the IGY was inherently international and scientific in character; ironically the IGY would soon prompt a most nationalistic space race, and that race would be driven by large-scale engineering demonstrations of national prowess rather than scientific research or international cooperation.

The advent of the IGY then led to the establishment of a U.S. National Committee for the IGY (USNC) under the National Academy of Sciences, and familiar names reappear in the committee's membership list. Joseph Kaplan (the distinguished University of California-Berkeley geophysicist) served as the chair, and the committee included Lloyd Berkner as an *ex officio* member (for his role in initiating the IGY, and as President of the Associated Universities, the supervising body for Brookhaven National Laboratories), and Hugh Odishaw (past Director of the National Bureau of Standards) as the Executive Secretary. In addition to having led the National Bureau of Standards, Odishaw was well known as the co-editor (with E. U. Condon) of the massive *Handbook of Physics*. Odishaw hired Arnold Frutkin to be his deputy for international affairs and director of the USNC's Office of Information.

A proposal to include an earth satellite as a part of the IGY was soon prepared:[23]

> Following an October 1954 meeting of the International Council of Scientific Unions (ICSU) in Rome, the US National Committee for the IGY, working under NAS sponsorship, recommended that the US institute a scientific satellite program as part of its overall IGY program. Shortly thereafter, NAS President Detlev Bronk, along with National Science Foundation Director Alan Waterman, took the satellite proposal to the US government. It was approved, and on 29 July 1955 the Eisenhower Administration announced the US goal of orbiting an artificial earth satellite during the IGY.
>
> Shortly after the announcement, the Academy put together the Technical Panel on the Earth Satellite Program (TPESP). TPESP's mission was to oversee the scientific aspects of the satellite project, as well as those aspects concerning public information and institutional relations. Chaired by Richard Porter, the Technical Panel's original membership included US National Committee Chairman Joseph Kaplan; USNC Secretary Hugh Odishaw; the Naval Research Laboratory's Homer Newell, Jr.; William Pickering of the California Institute of Technology's Jet Propulsion Laboratory; Athelstan Spilhaus of the University of Minnesota; Princeton University's Lyman Spitzer, Jr.; James Van Allen of the State University of Iowa; and Fred Whipple of the Smithsonian Institution's Astrophysical Lab.

TPESP was extraordinarily well staffed to carry out its major task, the selection of the experiments that would be flown on Vanguard and Explorer. In the aftermath of the U.S.S.R.'s unexpected contribution to the IGY on October 4, 1957 – the launch of Sputnik I – the nation's attention shifted from a low-key interest in the science of the IGY to the previously cited concerns of national defense and geopolitics. Of course, behind the scenes in classified programs, national defense and geopolitics always carried primacy.

Soon after Sputnik-I was launched, the SSB was formed and shortly after NASA as well. The RSRP and TPESP provided many of the initial members of the SSB, and the SSB was built upon the attitudes, beliefs, and mores of the scientists and engineers who had adopted the technologies emerging from World War II and modified them to create the space and Earth sciences that the SSB now serves. The SSB was and remains closely allied with the branch of space activities that is involving scientific research, support of investigators, a purposeful and deliberative selection of goals, the use of appropriate technology, peer review, intellectual competitiveness, and international cooperation. The key players have been scientists and engineers. Lloyd Berkner became the first chair of the SSB, Hugh Odishaw the first Executive Director, and Arnold Frutkin served briefly as secretary to the International Relations Committee of the newly formed Board. Among the prominent members appointed over the first few years of the Board's existence were Leo Goldberg, Richard Porter, Bruno Rossi, John Simpson, Harold Urey, James Van Allen, Harry Wexler, Joshua Lederberg, Herb Friedman, Luis Alvarez, Charles Townes, and too many others to list. Unlike many advisory committees the roots of the Board were very deep, and the associations of many of its members were of longstanding. Naturally, the scientific credentials of these early members cannot be challenged.

Arnold Frutkin soon left the Board to move to NASA first as the director of NASA's Office of International Programs, and later as Associate Administrator for External Relations. Frutkin's NASA service extended from 1959 to 1979, and he brought with him his experiences with the IGY and SSB. He became pivotal in establishing the early policies and practices of NASA, and his heritage remains strong. Ironically, among the experiences that evidently shaped him profoundly were the reluctance of the Soviet Union to live up to the agreements made under the IGY and a profound skepticism that international scientific cooperation can spill over and contribute to broader political and diplomatic understanding. Whether one subscribes to Frutkin's views or not, his book remains a valuable discussion of international cooperation, and provides important insights into NASA's institutional thinking.[24] The NASA that Frutkin helped to create is most closely aligned with the branch of space activities that are directed at national prestige, engineering, national security, industrial competitiveness, classification and export control, serendipitous discovery, and focused on human exploration of space. The segments of this branch include Mercury, Gemini, Apollo, Apollo-Soyuz, Skylab, Space Shuttle, International Space Station, and aspirations to go farther. The key players have been aerospace engineers and executives, military officers and pilots, business executives, and politicians. Relations between the two branches of space activities have often been strained, and it isn't always clear that the two speak the same language. But despite contrasting frames of reference, both branches can enunciate objectives worthy of national support.

When the space race was eventually won and the accompanying research tallied, scientists could not claim that it was the public's allegiance to space science that carried the day and likewise those seeking the extension of humankind beyond the Earth could not claim that it was the public's allegiance to the goals of human exploration of space that carried the day. Many, likely most, of the public had a modest sustained interest in space science or the travels of astronauts (short-term spikes of interest from early moon landings and planetary encounters not to the contrary), but a very great deal of interest in whether achievements in space reflected threatening imbalances of technological and military power. Consequently, the sporadic efforts to regain the urgency of the space race and secure another imperative to achieve a new goal "within a decade" have been futile – despite several such presidential decadal declarations having been successfully secured by NASA. Is this because the public's support for space activities has reached an equilibrium level that permits the present level of effort, but precludes for the time being a major venture such as the human exploration of Mars? If so, can that be changed, or should we accept the present equilibrium? If we accept the present equilibrium, what is the balance of activities to be pursued? How do we reconcile the differing perspectives of the two branches of space activities described above? The conflicts over the research funding for the International Space Station are not encouraging. As usual, I have closed with questions for the Board and its committees to ponder as they go about their important work.

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What the Space Science Board said in 1958 (March 2003)

The death of each crewmember on Columbia was tragic and certainly cause for our deep sorrow. At the personal level the loss of someone you know has a special poignancy. Several faculty and staff members of the College of Engineering at the University of Texas at Arlington went outside near 8:00 AM on Saturday, February 1, 2003 to observe the reentry over north-central Texas of Kalpana Chawla, an alumna of their program and a veteran astronaut with more than 400 hours in space. One of the professors noted later that the expected sonic boom was somehow different from what he had heard on other reentries, “too long, too loud.” Not yet troubled, although some said they saw worrisome multiple trails across the sky, they went about their business only to hear very shortly later that the unusual sonic boom and multiple trails were produced by the destruction of the shuttle and marked the death of the second of the university's graduates to fly in space. In her visits back to the university, Dr. Chawla had long been a source of pride to the campus and a marvelous role model for the community. Her articulate, enthusiastic presentations, exuberant demeanor, and willingness to linger well afterward to talk with students are well remembered by all of us who attended.

Of course, the Columbia accident has now reopened the questions regarding the role of piloted versus automated space flight that have never been too far below the surface in the half-century history of the space program. The accident also exacerbates problems that were only beginning to be addressed prior to the accident regarding support to research on the International Space Station (ISS). Congressional hearings, trade journals, and commentators are raising questions regarding the cost-to-benefit ratio of piloted space flight and even whether the ISS might have to be abandoned. While discounting the tenor of some of the questions as reflecting more the immediate aftermath of the accident than a sober judgment of likely consequences, nevertheless the questions themselves remain.

The histories of the many efforts that have been made to map a direction for NASA are lengthy. They are certainly too lengthy to describe even in the tersest form in a brief column.[1] Indeed, the studies and recommendations prepared by NASA, the bodies of the National Academies, and other organizations bring to mind Odysseus' admonition. As Homer wrote, when Odysseus neared the end of his much-delayed return to Ithaka from the Trojan Wars, Odysseus was feted by Alkinoös and Arete, the King and Queen of the Phaiakians, on the island of Kalypso. Odysseus entertained the banquet guests by narrating the adventures that led to his rescue by Arete, but he stopped short of retelling how the Queen had brought him to the palace. Instead, Odysseus said to Alkinoös:[2]

> *Why tell the rest of this story again, since yesterday*
> *in your house I told it to you and your majestic wife?*
> *It is hateful to me to tell a story over again, when it has been well told.*

As I sit in my den and stare at bookshelves of reports of such studies, I too find it hateful to try to paraphrase “stories” that have “been well told.” Nevertheless, the loss of Columbia is forcing the nation to revisit the aims and means of our national space program, and that is a matter I cannot avoid here. Rather than trying to summarize so many reports, I have chosen to employ a single report around which to begin my column – the very first report prepared by the then Space Science Board (SSB).[3] The report was sent from the founding chair of the SSB, Lloyd Berkner, to his long-time friend and fellow pilot, James Webb, NASA's second administrator. That friendship was to prove somewhat controversial when the SSB issued its first report, and one account of the writing of that report shows how difficult it was for the Board to reach consensus.[4] The account also illustrates quite well the legitimacy of the concern for the appearance of conflicts of interest when controversial matters are under study. I am not suggesting that in this instance there was an actual conflict of interest, but only that there were strongly held views (especially by the chair), and opponents of the study's conclusions could use the potential conflict of interest to object to its conclusions.

Rather than undertake the task of paraphrasing the "well-told story," let's consider the opening language to the first attachment to the letter from Berkner to Webb:

> Man's Role in The National Space Program
>
> At its meeting on February 10 and 11, 1961, the Space Science Board gave particular consideration to the role of man in space in the national space science program. As a result of these deliberations the Board concluded that scientific exploration of the Moon and planets should be clearly stated as the ultimate objective of the U.S. space program for the foreseeable future. This objective should be promptly adopted as the official goal of the United States space program and clearly announced, discussed and supported. In addition, it should be stressed that the United States will continue to press toward a thorough scientific understanding of space, of solving problems of manned space exploration, and of development of applications of /space science for man's welfare.
>
> The Board concluded that it is not now possible to decide whether man will be able to accompany early expeditions to the Moon and planets. Many intermediate problems remain to be solved. However, the Board strongly emphasized that planning for scientific exploration of the Moon and planets must at once be developed on the premise that man will be included. Failure to adopt and develop our national program upon this premise will inevitably prevent man's inclusion, and every effort should be made to establish the feasibility of manned space flight at the earliest opportunity.
>
> From a scientific standpoint, there seems little room for dissent that man's participation in the exploration of the Moon and planets will be essential, if and when it becomes technologically feasible to include him. Man can contribute critical elements of scientific judgment and discrimination in conducting the scientific exploration of these bodies which can never be fully supplied by his instruments, however complex and sophisticated they may become. Thus, carefully planned and executed manned scientific expeditions will inevitably be the more fruitful. Moreover, the very technical problems of control at very great distances, involving substantial time delays in command signal reception, may make perfection of planetary experiments impossible without manned controls on the vehicles.

At the close of the report, the Board wrote:

> The Board strongly urges official adoption and public announcement of the foregoing policy and concepts by the U.S. government. Furthermore, while the Board has here stressed the importance of this policy as a scientific goal, it is not unaware of the great importance of other factors associated with a United States man in space program. One of these factors is, of course, the sense of national leadership emergent from bold and imaginative U.S. space activity. Second, the members of the Board as individuals regard man's exploration of the Moon and planets as potentially the greatest inspirational venture of this century and one in which the entire world can share; inherent here are great and fundamental philosophical and spiritual values which find a response in man's questing spirit and his intellectual self-realization. Elaboration of these factors is not the purpose of this document. Nevertheless, the members of the Board fully recognize their parallel importance with the scientific goals and believe that they should not be neglected in seeking public appreciation and acceptance of the program.

Forty-two years later, the arguments on the side of piloted space flight enunciated by the Board seem familiar and little changed. Some would note the familiarity as a sign of validity and the lack of change as one of durability, while others might suggest that the lack of change may indicate ossification. The Board's view on piloted space flight expressed in this first report is more enthusiastic than that held by the contemporary science community, and more enthusiastic than a fair amount of the Board's subsequent views. Indeed, this report likely represents a high point in the Board's views on the merits of piloted space flight.

McDougall was later to write:[5]

> … the Space Science Board altered the terms of debate. Beforehand, the main conflict had been one of politicians and engineers pushing manned spaceflight for prestige, security, or big budgets, versus scientists and treasurers favoring unmanned flight because of greater scientific returns and much lower costs. But now a body of scientists had come out for a manned moon program, asserted its scientific value, and appealed to something more than 'knowledge gained per dollar spent.' Manned spaceflight could now be viewed as something over which 'good scientists disagree'; the weight of purely political judgments was accordingly enhanced.

There is a further paragraph of the SSB report that McDougall ignores, and that would be quite extraordinary, if it were written today:

> There is also another aspect of planning this country's program for scientific exploration of the Moon and planets which is not widely appreciated. In the Board's view, the scale of effort and the spacecraft size and complexity required for manned scientific exploration of these bodies is unlikely to be greatly

> different from that required to carry out the program by instruments alone. In broad terms, the primary scientific goals of this program are immense: a better understanding of the origins of the solar system and the universe, the investigation of the existence of life on other planets and, potentially, an understanding of the origin of life itself. In terms of conducting this program a great variety of very intricate instruments (including large amounts of auxiliary equipment, such as high-powered transmitters, long-lived power supplies, electronics for remote control of instruments and, at least, partial data processing) will be required. It seems obvious that the ultimate investigations will involve spacecraft whether manned or unmanned, ranging to the order of hundreds of tons so that the scale of the vehicle program in either case will differ little in its magnitude.

I cite this paragraph not just to chasten us regarding our ability to predict the future, but to provide a springboard into comparing the technology available in 1961 versus that available today and how current technology may affect space policy.

For better or worse, a number of us on the Board were already well into our careers in 1961, and likely all of us who were have clear memories of the state of technology at the time. I was still writing computer programs in machine language to be run on an IBM 650 computer with punched card input and using a memory based on a rotating magnetic drum with a 2000-byte capacity, carefully husbanded with preprinted charts on which could be noted in handwriting the assignment of memory locations. Optimization was sought by arranging the use of the memory so as to minimize the time between successive uses of locations. I also remember spending an inordinate amount of time setting the various dials on an analog computer used to simulate the performance of a guided missile's control system. No higher order languages were available to me (a limited version of FORTRAN existed for the 650, but wasn't on our machine), and simulation and test results were preserved on circular and strip charts with ink or electric pens. I was still teaching future guided missile system operators the differences among triodes, tetrodes, and pentodes and how to design circuits using load lines. Vacuum tubes were certainly prosaic, but not irrelevant.

My log-log duplex decitrig slide rule was my constant companion, as no pocket calculator or personal computer existed, and the IBM 650 to which we had access was far too awkward and inconvenient to use for most routine matters (the unit only supported integer arithmetic, so floating point calculations required the writing of special routines). Of course, the mechanical adding machine was in constant use for the computations unsuited to the slide rule. We attended various training programs, and were usually subjected to exhortations on the need to do our jobs well that were accompanied by spectacular movies of tumbling, exploding ballistic missiles. Of course these were the same missiles soon to carry astronauts into space. The bullpen in which we worked had a single telephone on the supervisor's desk (at the head of 50 to 75 desks and drafting tables), and it was unthinkable that anyone except the supervisor should use it – and he only rarely. Long distance calls were nearly unheard of, and information from suppliers was obtained by an engineer or technician writing a letter in longhand to be typed (and corrected) by a secretary with carbon copies on onionskin paper, and initialed by the supervisor to show his approval before being collected and carried to the mailroom to be dispatched. Ditto and mimeograph machines were in heavy use. Naturally, there were only the bare beginnings of satellite communications, and the ARPA-net that was to give birth to the Internet was years in the future. Cellular phones were something in the Dick Tracy comic strip. Black and white television sets were still being sold, and color television and remote controls were regarded as frivolous by many. The advances in technology since 1961 are legion, and probably all Board members can add far more than the brief recitation above.

Clearly, the advice we gave regarding the comparable sizes of automated and piloted missions was incorrect, and swayed the recommendations toward a more favorable view of piloted flight than would otherwise have been the case. Would our advice have been the same if we could have anticipated advances in sensors, computation, communications, and the myriad enhancements in the various dimensions of spacecraft capability per unit mass and per watt of electrical power? I have avoided mentioning in this litany more recent advances in robotics, spacecraft autonomy, visualization, virtual reality, and the profound changes in the commercial applications of space (notably communications, position location, and remote sensing). Do the old arguments regarding the indispensability of the human crewmember still hold sway? I can imagine that they may for some laboratory sciences – but not necessarily all, and certainly they apply where the astronaut is the research subject. However, has advancing technology shrunk the domain of applicability of the human operator or researcher in space? What is the direction and scope of future change? Will advancing technology make humans more capable or more dispensable? Of course, how do we weigh the actual human role in carrying out research versus the more ethereal issues the Board cited in its original report: national leadership, inspiration, spiritual values, and humankind's questing spirit? Do the latter issues dominate whether or not the human offers cost-effectiveness in the conduct of the particular research that may be done?

During approximately the past two years, the Board's committees and those of the NASA Advisory Council have addressed some of the above questions in great detail. Our Committee on Microgravity Research, chaired by Peter Vorhees, released its *Assessment of Directions in Microgravity and Physical Sciences Research at NASA* in September 2002. The committee concluded that good results had been obtained in a number of areas by the program administered by NASA's Office of Biological and Physical Research, and that more were in the offing if adequate support was available to conduct work on the International Space Station.[6] At approximately the same time, the Board's Task Group on Research on the International Space Station, chaired by James Bagian, released the second of its reports assessing the readiness of the community and the space station operators to carry out the research favorably noted by the Committee on Microgravity Research.[7] The committee concluded that, while the community was well prepared, the reductions in NASA's planned facilities for the space station and uncertainty regarding future schedules and budgets would make support of a viable research program at least unlikely and probably impossible.

NASA’s Advisory Council formed a NASA Biological and Physical Research Maximization and Prioritization Task Force, chaired by Rae Silver, that also reported near the same time as the above two groups.[8] The task force cited heavily past National Research Council studies, many of which were prepared by or contributed to by the Space Studies Board. The task force concluded that serious problems existed regarding upmass delivery capability and crew research time, and that "If enhancements to ISS beyond US Core Complete are not anticipated, NASA should cease to characterize the ISS as a science driven program." All three reports are available at the web sites of the Space Studies Board and NASA. There is also a long backdrop of other studies of these and related issues.

Near the start of this column I noted the wide public concern about NASA's future directions that has arisen following the Columbia accident. I also mentioned that it would be impossible here to reiterate the conclusions of all the studies that have related to the human space flight program and its role and scope in the nation's space program. While we cannot reiterate the individual conclusions, we can formulate one overriding conclusion that consistently appears in all of our studies, and it has to do with our ability to justify human space flight on the basis of likely research achievements. Our consistent conclusion remains: There is no compelling scientific reason to justify in a cost-benefit sense or benefit-risk sense the investment in human space flight. But, if a decision is made to carry out such missions, based on other arguments or national priorities, there are good scientific uses to which facilities such as piloted space vehicles and inhabited stations can be put.

The accident investigation on the destruction of Columbia is proceeding, and I am confident that the space shuttles will return to flight at the appropriate time. However, their ability to provide robust support to the International Space Station in particular and to human space flight in general must be carefully examined. I don't know what such an examination will yield. Furthermore, it seems evident to me that such examinations should be firmly based in a review of the role of the human space flight program in our nation's space program and the relative emphasis that is applied to it versus that to automated space missions. I raised some of those questions above, but the talented committee members of the bodies of the National Research Council, the advisory bodies of the federal agencies, and the professional societies can do a far better job than I have in this column. As in my other columns, I will leave the hard work to them.

1. See, e.g., John M. Logsdon, et al., *Exploring the Unknown, Selected Documents in the History of the U.S. Civil Space Program, Volume I: Organizing for Exploration*, The NASA History Series, NASA: Washington, DC, 1995. Less accessible, but valuable, is Office of Technology Assessment, *Civilian Space Policy and Applications*, STI-177, GPO: Washington, DC, June 1982.
2. Quoted from Richmond Lattimore's translation of *The Odyssey of Homer*, Harper Perennial: New York, 1991, closing lines to Book XII, p. 197. Note that other translators render these lines quite differently.
3. L. V. Berkner, Letter to James E. Webb, Administrator, NASA, March 31, 1961, with two enclosures: "Man's Role in the National Space Program" and "Support of Basic Research for Space Science"
4. Walter A. McDougall, *The Heavens and the Earth: A Political History of the Space Age*, Basic Books: New York, 1985, pp. 315-316.
5. Walter A. McDougall, op. cit., p. 315
6. Committee on Microgravity Research, Space Studies Board, *Assessment of Directions in Microgravity and Physical Sciences Research at NASA*, National Academy Press: Washington, DC, 2002
7. Task Group on Research on the International Space Station, *Factors Affecting the Utilization of the International Space Station for Research in the Biological and Physical Sciences*, National Academy Press: Washington, DC, 2002
8. NASA Advisory Council, *Report by the NASA Biological and Physical Research Maximization and Prioritization (ReMAP) Task Force*, NASA: Washington, DC, 2002

5 INTERNATIONAL COOPERATION
(December 2002)

We associate the birth of the space program with Cold War competition between the Soviet Union and the United States. But it was not only international competition that was bred into the infant space program, international cooperation was as well, and that is the subject I have chosen for this column.

International cooperation in space began with the International Geophysical Year (IGY) and was incorporated in the founding legislation for the U.S. space program, the National Aeronautics and Space Act of 1958, which states:[1]

> The space activities of the United States shall be conducted so as to contribute materially to … cooperation by the United States with other nations and groups of nations in work done pursuant to this Act and the peaceful application of the results thereof.

President Kennedy elaborated upon this theme in his inaugural address,[2] and in his first State of the Union message, when he stated:[3]

> … this Administration intends to explore promptly all possible areas of cooperation with the Soviet Union and other nations "to invoke the wonders of science instead of its terrors." Specifically, I now invite all nations--including the Soviet Union--to join with us in developing a weather prediction program, in a new communications satellite program and in preparation for probing the distant planets of Mars and Venus, probes which may someday unlock the deepest secrets of the universe.

He repeated these proposals in his address to the General Assembly of the United Nations later in the year:[4]

> All of us salute the brave cosmonauts of the Soviet Union. The new horizons of outer space must not be driven by the old bitter concepts of imperialism and sovereign claims. The cold reaches of the universe must not become the new arena of an even colder war.
>
> To this end, we shall urge proposals extending the United Nations Charter to the limits of man's exploration of the universe, reserving outer space for peaceful use, prohibiting weapons of mass destruction in space or on celestial bodies, and opening the mysteries and benefits of space to every nation. We shall propose further cooperative efforts between all nations in weather prediction and eventually in weather control. We shall propose, finally, a global system of communications satellites linking the whole world in telegraph and telephone and radio and television. The day need not be far away when such a system will televise the proceedings of this body to every corner of the world for the benefit of peace.

In 1963, President Kennedy returned to the General Assembly of the United Nations only two months before his assassination to say:[5]

> Why, therefore, should man's first flight to the moon be a matter of national competition? Why should the U.S. and Soviet Union, in preparing for such expeditions, become involved in immense duplications of research, construction, and expenditure? Surely we should explore whether the scientists and astronauts of the two countries – indeed of all the world – cannot work together in the conquest of space, sending some day in this decade to the moon not the representatives of a single nation, but the representative of all our countries.

President Johnson reiterated Kennedy's offer in his first State of the Union message to Congress after the assassination, noting that "we must assure our pre-eminence in the peaceful exploration of outer space, focusing on an expedition to the moon in this decade-in cooperation with other powers if possible, alone if necessary."[6] While in the short run the above efforts did not bear fruit, the spirit of possible cooperation did survive and later years saw numerous joint ventures, including notable collaborations between the Cold War adversaries.

Of course, this favorable view has its dissenters. One historian, Walter A. McDougall, in praising NASA's first Director of International Programs, Arnold Frutkin, declares that the efforts of the Kennedy and Johnson administration, despite Frutkin's "hard-headed business-like approach," were guilty of "raising expectations that could not be fulfilled."[7] McDougall writes:

> In substantive ways, U.S. diplomacy was quite successful … But the image of American space programs as open and altruistic and able to spread brotherhood and prosperity to a world tempted by communism amounted at best to a benign hypocrisy. In later decades … U.S. diplomats would silently rue the image making of the nation's first decade in space.

Going further, McDougall favorably notes that Frutkin "advised the Kennedy administration, as he had Eisenhower's, to build an extensive record of overtures while scolding the Soviets for not responding." Frutkin's role was pivotal in shaping NASA's international cooperation policies and practices, and his book remains a key source on the evolution of NASA's cooperative efforts.[8] For a more textured presentation of Frutkin's views than that provided by McDougall, I recommend reading the transcription of his recent interview for the NASA oral history series.[9] In it, among other interesting comments, he narrates how he publicly criticized a Soviet representative in an astronautical society meeting in Cloudcroft, New Mexico using such a dossier (page 46 of the transcription).

What NASA policies and practices emerged? In view graph form, NASA recently distributed the current version of the guidelines for international cooperation:[10]

- Cooperation is mutually beneficial; must meet NASA programmatic objectives
- Project has scientific and technical merit
- Partners are generally government agencies due to level of investment and legal requirements
- Seek clearly defined and distinct managerial and technical interfaces to minimize complexity to the extent possible
- Protect against technology transfer and take into account industrial competitiveness
- No exchange of funds
- No joint technology development

In his book, Frutkin describes the early development of these principles, and observes that President Johnson, declaring U.S. determination to move forward unilaterally if necessary, said in a broader context:[11]

> The United States shall welcome any who wish to join with us in seeking to serve the common good of mankind. But if others are not willing – or if they are not able – to join with us, our own endeavors will not slacken.

NASA has enunciated its policies for international cooperation in slightly varying form over the years, but the core principles – and undercurrents – have remained largely unchanged. Undercurrents have included an assumption of U.S. dominance, a willingness to "go it alone," a bent toward autonomous decision-making, and Frutkin's "hard-headed" approach to negotiations. Recent years have seen added emphasis on restricting technology transfer due to the ITAR processes, but tensions existed in earlier years as well. The Frutkin transcript (page 52) describes the considerable efforts that were required on the Soviet side during the Apollo-Soyuz Test Project (ASTP) due to the difference in the cabin atmosphere in the two systems. The Soviets relied upon an atmospheric pressure mixture of nitrogen and oxygen, while the United States used a 5-psi pure oxygen atmosphere, which necessitated the use of a decompression procedure before the door between the two spacecraft could be opened. The Soviets designed the docking mechanism and provided it to the United States, and accepted the dangers (fire hazard and the possibility of the "bends") in bleeding off nitrogen to bring their cabin atmosphere to that of the American craft after docking. During the preparations for the mission, the Soviets requested a sample of the fire-retardant fabric used in American space suits so that they might use it in their own space suits. Of course, it was the American's choice of spacecraft atmosphere that necessitated the change. The request was denied on technology transfer grounds. Frutkin notes that the Soviets went on to develop their own fire-retardant fabric, which NASA later found to be superior to the U.S. fabric. Technology transfer restrictions may delay a competitor's capability, but not necessarily deny it. Other examples exist.

Despite Cold War tensions NASA built a successful and what can justifiably be characterized as an outstanding record of international collaboration based upon the principles whose development Frutkin led. The early NASA years are well documented in a limited-distribution internal publication.[12] Perhaps at some point NASA will find it useful to add the document to the others on its web site.

However, all of the United States' and NASA's space cooperative efforts did not go smoothly. In citing examples where cooperation was less successful, it is not my intent to detract from the overall record, but to point out past difficulties that may foretell future tensions. Satellite communications technology was an early arena for conflict. The United States adopted a very restrictive policy in this area that caused considerable friction, and stimulated foreign technology development:[13]

> The controls were enforced by the U.S. Office of Munitions Control, an office of the State Department. Although it delayed nearly ninety-five percent of foreign requests for technical information, the Office … ultimately refused only two to three percent of these requests. Nevertheless, the imposition of these trade restrictions created some problems between the United States and its allies across the Atlantic, many of which questioned the national security justifications for the restrictions. In a number of cases European industry chose to develop the relevant electronic and aerospace technologies on its own, rather than waiting for the State Department to release the restricted information.

Furthermore, in attempting to support the Intelsat system, and the U.S. representative, Comsat, the United States adopted a policy of opposing regional and domestic satellite communication systems, and using the withholding of launch services as a means to enforce the policy. The ill-fated policy is described in these excerpts from a 1965 National Security Action Memorandum:[14]

> … It is the policy of the United States to support the development of a single global commercial communications satellite system to provide common carrier and public service communications. …

> 3. The United States should not consider requests for launch services or other assistance in the development of communications satellites for commercial purposes except for use in connection with the single global system established under the 1964 [Intelsat] agreements.

The policy would have relegated all civil satellite communications (including proposed U.S. domestic satellite communications) to Intelsat. Going still further, the policy recognized that some nations had their own national security needs that would not be satisfied by Intelsat, and the policy fancifully recommended:

> 5. The United States aim is to encourage selected allied nations to use the U.S. national defense communications satellite system rather than to develop independent systems and to accommodate allied needs within the U.S. system (with additional costs normally to be borne by the participants). …

Enforcing the policy, the United States declined to provide a Thor-Delta rocket to the Europeans for the launch of the Symphonie Franco-German communications satellite, but later provided the launcher to Japan with scarcely a murmur. Ultimately, the United States launched Symphonie, but stipulated that it could only be used for experimental purposes and not as an operational communications satellite. The U.S. policy was – at best – regarded as heavy handed. While NASA was only one contributor to these national policies, it did play a key role and was usually the interface with the proposed international partner. The reluctance to provide launch assurance, and to impose a U.S. policy of a single global satellite communications system, lent support to Europe's ultimately successful efforts to secure European autonomy in space technology through the establishment of the European Space Agency (ESA) and development of the Ariane launch vehicle. Now, of course, technology transfer in launch vehicles is further muddied as Russian engines replace American engines on the Atlas launch vehicle.

In some cases, U.S. hubris over its ability to proceed autonomously led to missed opportunities for both collaboration and cost savings. France approached the United States to create a joint SPOT-Landsat Program that could have been mutually advantageous, but was rebuffed because NASA believed that ill-advised technology transfer would occur and that U.S. commercial interests would not be served. Both programs continue, but the U.S. Landsat has remained on the precipice of cancellation for more than twenty years, dissuading both commercial and governmental commitments to its use.

The vehicle that NASA has chosen for its cooperative projects is the Memorandum of Understanding (MOU), which is negotiated between technical agencies and concurred in by diplomatic and national security bodies. The agreement does not impose the obligation of a treaty, and therefore lacks the protections of a declared national commitment. Only on the rarest of occasions has NASA participated in more formal international agreements.

The International Solar Polar Mission (ISPM) illustrated how problematic the use of MOUs could be. The risk that results from proceeding on a joint mission involving large investments on the part of both parties based on only an MOU remains an unresolved issue today. For years the example of ISPM has haunted NASA-ESA negotiations. An MOU had been negotiated between NASA and ESA for each to supply a spacecraft, with one to be placed into orbit that would take it initially above the Sun's north pole and the other above the Sun's south pole. Experiments from the two parties were mixed on the two spacecraft, which complicated matters later. The mission is well described in Joan

Johnson-Freese's book, as are many aspects of international cooperation in space.[15] NASA found itself in budget and schedule difficulties due to the Space Shuttle's development problems in general, and because of the ISPM's reliance upon the Shuttle in particular. There were further problems with the upper stage that was required to propel the two spacecraft from the Shuttle orbit to Jupiter, where they would separate and use gravity assist to travel on to their planned orbits over the Sun. Consequently, under pressure from both the Office of Management and Budget and the Congress, NASA chose to cancel its participation in the ISPM. Complicating matters, and in keeping with the usual budget secrecy, ESA's Director General Erik Quistgaard was told of the cancellation only a few hours before it was announced as part of President Reagan's budget cuts. The repercussions are well described in an excerpt from an aide memoire written by ESA's Washington representative:[16]

> 2. I am to say that:
>   a) The cancellation of the NASA satellite, which was effected without consultation, is a unilateral breach of the ISPM MOU; this cancellation is totally unacceptable and ESA requests full restoration of the programme to its original level.
>   b) If the cancellation were permitted to stand, there would be serious damage to European/United States cooperation in space.
>   c) Naturally, there has been a very unfavorable reaction in Europe. No less than seventeen European scientific institutes are involved in the United States spacecraft and would consequently be unable to fly.

In more moderate retrospect, two players on the European scene have written:[17]

> While, in general, the cooperation with NASA has been an essential element in the successful development of European space science, and extremely beneficial to Europe, it has also involved some difficulties because of the unequal weight of the two partners, and the quality of the relationship has varied from case to case. For example, in the case of ISEE [International Sun Earth Explorer] and IUE [International Ultraviolet Explorer] it was flawless, while in the case of ISPM … it accumulated difficulties and revealed how little binding on NASA is the Memorandum of Understanding (MOU) … and how vulnerable are the U.S. space projects to the process of annual budget approval.

There were many special circumstances accom-panying the cancellation of the U.S. contribution to ISPM that must be acknowledged. Certainly all science and applications missions associated with the Space Shuttle were to suffer from its early growing pains, and OMB and the Congress must share the blame for the cavalier attitude displayed in the cancellation.

Technology transfer issues entered other aspects of Europe's participation in the Space Shuttle program. Europe initially sought a full partnership role and offered the development of a Shuttle-carried upper stage, a so-called Space Tug, but U.S. defense interests objected. Many of Europe's ambitions for the Space Tug have since been met in the Automated Transfer Vehicle (ATV) now being developed for the unmanned transport of equipment and consumables to the International Space Station (ISS) and for the reboosting of the ISS to counteract the effects of atmospheric drag. At the time, however, Europe was diverted to the development of Spacelab, a flexible Shuttle-carried laboratory that could be configured in a variety of ways using a shirtsleeve environment laboratory module and a number of reconfigurable external pallets. The lengths of both the module and pallets could be adjusted (in large increments) to meet particular mission requirements.

Spacelab was Europe's entry into manned space flight, and can be said to have been the price of admission to the ISS program. The laboratory was a very complex undertaking, involving as it did nearly every aspect of manned spaceflight.[18] In Spacelab's fifteen-year life, there were 23 Spacelab missions, and eight European astronauts flew on the missions. Numerous experiments were conducted, and the results published in refereed journals. But, was Spacelab a success? Johnson-Freese has a cogent analysis of this question (*op. cit*., pp. 25-30), and quotes a NASA director of the program:[19]

> Why are former participants in the Spacelab program such difficult negotiators today? Is NASA unable to accept the concept of ESA as an equal partner in the next venture? Were both NASA and ESA so overbearing and inconsiderate in their demands of each other during the course of the Spacelab program as to create a lack of trust for future joint programs? Why do so many people consider the Spacelab program as a negative example, as the way "not to do it?"

I leave these questions unanswered. It can be noted, however, that the technical objects resulting from the Spacelab program were outstanding engineering achievements. From the European side, their development and the experience gained in working with NASA to incorporate the objects into the Space Shuttle program were vital steps in the evolution of a manned spaceflight capability in Europe. From the NASA side, valuable hardware was obtained at minimal cost, and the utility of the Space Shuttle was enhanced. The cost-benefit question remains. Was this the most cost-effective way to continue the development of European space industry? Was this the best means to advance U.S. interests? Were both parties satisfied with the outcome? Perhaps, but some commentators would dissent. I'll leave that for others to sort out.

That brings us to the ISS. Based upon the European Spacelab experience, Russia's lengthy experience in space station development and operations, Canada's robotic arm experience on the Space Shuttle, Japan's overall capability, and other considerations (principally a need to reduce U.S. budget requirements), the U.S. invited other nations to join in the development of an ISS. Europe, Japan, and Canada became partners in 1988, and Russia joined in 1993. The history of the ISS is a fascinating one, and one that evolves at this writing. Early descriptions, notably Hans Mark's,[20] deserve attention, but the recent history is carried only in reports and more transitory media. Within the SSB, the plans for the research on the ISS have been a major study topic for a long time, but especially during the past eighteen months.[21-23] (The cited reports are available on the SSB web site.) The initial announcements of major reductions in research, crew size, and limitations on upmass availability stimulated considerable concern among possible users of the ISS. In recent months, NASA has begun to offer assurances that future budgets will restore at least some of those reductions, although the details are still emerging and always subject to future OMB and Congressional actions.

Nevertheless, the process by which we have reached the current state seems destined to raise concerns that will adversely affect future international cooperation. U.S. budget problems with ISS development led to the definition of a new contribution of lesser capability, U.S. Core Complete, that was not contemplated in the Intergovernmental Agreement (IGA) signed by the partners. NASA raised the possibility, unilaterally and without negotiation, that the United States would not proceed with its original plans, but stop well short of them, but still meeting the minimal IGA requirement to deploy the partners' contributions. The international partners had been invited to participate in the ISS by first President Reagan and then President Clinton. Within their own procedures they went through a process committing themselves to participation that was more extensive than a simple agency-to-agency MOU. With a backdrop of Presidential invitation and their own level of commitment, the partners were understandably surprised and chagrined to find that the United States and its representative, NASA, felt free to make unilateral changes with neither advance warning nor negotiation. All of us hope that the resulting frictions are in the process of being ameliorated. The December 6, 2002 meeting of ISS heads of agency lends encouragement that such is the case.[24]

NASA has enjoyed great success in its international programs. However, at least to some degree, that success has been built upon NASA being the dominant, and very competent, partner. How will international cooperation evolve as the difference in relative capabilities shrinks? Opportunities abound for collaborative ventures that range from space science and applications to the human exploration of space. Those opportunities will always enhance mission results, and may reduce national costs. The price of accepting some opportunities may require NASA's willingness to play a subordinate role. They may require a more determined approach to overcoming bureaucratic impediments associated with ITAR regulations. NASA may need the authority to enter into agreements that are more binding than simple MOUs, or at least the ability to convincingly express its commitment to honor agreements when more binding agreements are not feasible. No agreement can be so carefully drawn as to eliminate all ambiguity or be so complete as to remove the importance of trust between the parties. Some of the past history – not most or even a large part – does not foster trust. The principle that one Congress cannot bind a subsequent Congress may be a truism, but that should not preclude the nation acting in its own best interests, or assuring that reviews consider not only the immediate budget issues but the longer-term repercussions as well. If we move beyond international cooperation to *mutual reliance*, are the longstanding NASA principles for international projects still valid, or are they too rooted in an irrelevant past? In mutual reliance, partners can undertake the exciting, productive ventures that surpass the cooperative projects assembled from discretionary elements of desirable, but not necessarily essential, character.

As our committees go about their reviews of future international programs, the nature of the international agreements involved must be included in the review, and theirs risks and benefits carefully analyzed and noted. While we should not look pessimistically at such agreements, we must nevertheless be realistic in our assessments. As is my custom in these columns, I have provided no answers and left the hard work for the talented committees who must examine not just fuzzy principles but complex and challenging realities.

1. National Aeronautics and Space Act, July 29, 1958 (Public Law 85-568, 85th Congress, H.R. 12575 [72 Stat. 426], Section 201)
2. John F. Kennedy, The Inaugural Address, January 20, 1961
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4. John F. Kennedy, Address to the U.N. General Assembly, September 25, 1961
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6 EPILOG

(June 2003)

This newsletter completes my service as Space Studies Board (SSB) chair, and with more than a little relief I now hand the post over to a distinguished scientist and administrator, Lennard A. Fisk. I have known and admired Len for nearly 16 years, and I could not think of anyone better suited to chair the SSB during the difficult years that lie immediately ahead. Len is an active researcher and his work led to his election to the National Academy of Sciences this spring. He has served as a principal investigator on successful space experiments, academic department chair, university vice president, and both NASA's Associate Administrator for Space Science and Applications and NASA's Chief Scientist. He has served as Chairman of the Board of Trustees for the University Corporation for Atmospheric Research, and has served on the SSB and its committees in the past. Len has also worked with the private sector. His career has touched upon every aspect of the Board's responsibilities.

My tenure as SSB chair has been marked by more tumult than I could ever have predicted when I accepted the post. My term has included the September 11^{th} terrorist attack, two wars, and the Columbia space shuttle accident. At a less traumatic, but nevertheless serious, level we have attempted to cope with massive cost overruns on the International Space Station and the ramifications those overruns have produced. In addition the Board has maintained a very full plate of regular work including decadal surveys, impact studies, user interaction, and mission assessments. I was indeed fortunate to have truly exceptional SSB members and a talented professional and administrative staff. Certainly, Joe Alexander is the linchpin for the Board's activities and the source of unfailingly sound advice. Naturally, Betty Guyot was simply indispensable.

Over the years I spent more time as an academic dean than in any other position, and I had the responsibility to advise students on their careers, and I always offered a simple recipe. That recipe was to join organizations where you had talented colleagues from whom you could learn and superior leaders who could foster your development by example and action. I was lucky enough to be able to follow my own advice, and serving on the SSB was a capstone to my career. I was consistently amazed at the scope of the knowledge and interests of the Board members and staff, and most appreciative of the leaders of the NRC who counseled me. I close this column and my service to the SSB by offering very heartfelt thanks to all.