Chapter 3
Issues and Findings

LACK OF POLICY COMMITMENT

A major difference between the National Aeronautics and Space Administration's (NASA's) space science program and its manned program is that the latter has developed in response to national commitments to particular goals such as landing an astronaut on the Moon before 1970 or developing a reusable space shuttle; space science has not been charged with meeting particular national goals, but has proceeded in accordance with its own priorities, at a much lower level of commitment.

Because of the central importance of this issue, it is desirable to reach a clear understanding of the different levels on which the United States can (or cannot) be said to have a space policy. At one end of the scale, the 1958 National Aeronautics and Space (NAS) Act is the foundation on which the U.S. space program now rests.[1] As a kind of constitution for the conduct of space activities, the NAS act articulates the principles in accordance with which particular national policy commitments are to be framed. These policy principles include that:[2]

- peaceful uses of space are to be developed;
- U.S. preeminence in space science and applications be maintained;
- knowledge be increased;
- economic and social benefits be derived;
- civilian and military activities be separated (though they are to be coordinated so as not to duplicate one another unnecessarily;
- NASA, the civilian agency, be limited largely to research and development (R&D); and
- international cooperation be fostered.

The NAS act neither specifies national policy goals nor provides guidelines for implementing particular programs.

At the other end of the scale are particular internal policies which NASA or any other agency may institute in order to carry out its mandated duties in an orderly and successful fashion. One example of such particular policies is that NASA maintains lead-agency responsibility for space-based astronomy, and the National Science Foundation (NSF) that for ground-based astronomy.

Between these two levels is that of national policy goals. Such goals are set at the highest levels of Government; they provide a direction, define the scope, or name targets for the Nation's space activities. In addition, a policy goal defines by its wording the relative importance of the activity. The most significant aspects of these goals are that they often command bipartisan support, that they outlast any given administration, and therefore, that they serve as pledges that the Nation will support the long-term conduct of the specified space activities. At present, no national policy goals for space science exist.

Without national commitments to particular science goals, waste and instability in the space science program have been unavoidable: 1) projects being planned have been substantially changed or reordered in priority (e.g., Galileo), 2) projects under development have been deferred or canceled (e.g., the Mars Voyager mission), and 3) projects in operation have been scheduled for early termination (e.g., the Apollo Lunar Service Experiment Package, ALSEP). * In addition, the absence of policy goals has meant that several important questions, the answers to which determine the characteristics of the space science program the Nation actually undertakes, are decided ad hoc. These questions include:

[1] National Aeronautics and Space Act of 1958 (42 U.S. C. 2451 et seq.), as amended.

[2] For a fuller discussion of these policy principles, see: *Civilian Space Policy and Applications*, OTA-STI-177 (Washington, D. C.: Office of Technology Assessment, U.S. Congress, June 1982), pp. 35-44.

*Part of the reason for these difficulties is that a commitment to science is usually understood to be a commitment of funds to a "new start." More conducive to the accomplishment of good science would be a view that a policy commitment entails commitment to a *program* of investigation (i.e., a series of related missions extending beyond the time when the instruments are successfully operating to include funds for data analysis and design of future experiments). For further discussion of a program approach to science, see "New Starts" v. "Programs."

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- How may the goals of space science research be maintained vis-a-vis the objectives of a much larger manned space program?
- What is the proper mix of expensive, complex science projects and those that are simpler and less costly?
- How may international missions be undertaken effectively?
- How can the management of space science be improved?

The issues raised by these questions will be examined in the subsequent sections of this technical memorandum.

The key factor to be noted here is that in the absence of a national policy goal for space science, program decisions are determined by the size of the available budget. The reverse situation, in which policy determines budget, would probably be the more desirable, but neither Congress nor any recent administration has made a policy commitment strong enough to do so. If such a policy commitment were made, it would then be possible to set a base budget adequate to sustain the activities deemed essential to meeting the goals of that policy.

An advantage of adopting one or more national policy goals for space science would be resolution of the question of balance in the space science program. Traditionally, it has been thought that science as a whole progresses best when effort is rather evenly balanced among its parts. Maintenance of a balanced effort by means of an even spread of funding has, for example, been the cornerstone of NSF's support of science. Space science, however, forces a reconsideration of what balance in the program should mean, because some disciplinary areas (and the planetary sciences in particular) seem to have a higher threshold value for worthwhile missions than do others.

If a national policy commitment to a clear set of goals for U.S. space science were in place, space scientists would have a more realistic framework within which to set their priorities. Even without such a commitment, it is necessary, given NASA's limited budget, that scientists make a more thoroughgoing attempt to weigh scientific priorities against considerations of cost.

CONTINUITY, PLANNING, APPROACHES, COSTS, AND LIAISON

There are several problems in the space science program that result from causes other than the absence of policy goals. These causes include: modification of long-term planning in response to near-term crises, NASA's emphasis on "new starts, " the accumulation of costs that may be unnecessary, and the need for more effective contact between the scientific community and the appropriate congressional committees and their staff. These problems are summarized in this section.

Continuity and Planning

Continuity is essential to the accomplishment of good scientific work; no scientist can respond efficiently to frequent, major perturbations. Continuity is particularly important when the Nation undertakes cooperative programs with other countries. The development of hardware and software and the training of personnel cannot be accomplished overnight. In addition, the cancellations and/or deferrals of programs (International Solar Polar Mission, Venus Orbiting Imaging Radar, Gamma Ray Observatory, Galileo, Origin of Plasmas in the Earth's Neighborhood, Upper Atmosphere Research Satellite, etc.) suggest to many young scientists, engineers, and technicians that the future of U.S. space science programs is now sufficiently uncertain that they should direct their careers elsewhere.

One step in addressing the discontinuities in the space science program would be to establish policies that would permit effective long-range planning. If such policies were in place, the program would be more stable because discontinuities would be minimized, if not eliminated. According to the workshop participants, achieving stability in the space science program, through effective implementation of NASA's long-range plans, is at least as important as raising the current level of funding. The key term here is "effective;" NASA's long-range planning seems formally to be quite good, requiring few modifications. However, if there were a stronger national policy commitment to space science, * NASA's long-range planning would be less affected by instabilities deriving from extreme budget fluctuations** and by delays in portions of the manned program on which the science program is dependent.

Because space science projects require a decade of work from serious inception to significant funding, a good long-range plan should span 10 to 15 years. Within the general guidelines of any plan, however, flexibility must be maintained, first, because the priorities of science may change, and second, because budgets tend to fluctuate. Given that a base budget has been established as a matter of national policy, the plan could make provision for periodic adjustments resulting from those budget fluctuations. In general, the plan could be elaborated within two separate budgets, one for science per se, the other for missions, including hardware, launch, and operations. The first would establish a base level of continuing activities, all of which would be supported even in times when overall funds are strictly limited. The second would establish a set of initiatives, subject to revision in the light of scientific advances or because of changes in scientific priorities, to take advantage of unique opportunities as budget allocations allow. In particular, provision should be made that:

- a detailed subset of the plan extend over 3 to 4 budget years,
- all interested parties contribute to the formulation of the plan,"*** and
- there be developed alternatives in the schedule and scope of the program that correspond to realistic alternatives in the final budgets. *

Finally, subject to full discussion and periodic review, the plan could incorporate decisions reflecting the balance to be struck among subdiscipline.

"New Starts" v. "Programs"

The current practice of designing most flight missions as independent new starts tends to emphasize the space spectaculars and to distort priorities of space science, thus reducing the efficiency with which its objectives are pursued. In many cases a more cost-effective method would be to support continuing programs, which might include small- to moderate-sized missions as part of the baseline, and to obtain new-start status only for the more expensive missions.

NASA's conduct of space science by means of spectacular, independent missions has achieved notable successes in opening new scientific domains to investigation. In addition to accomplishing much good space science, such missions are attractive to Congress and to administrations because they appeal to the public and because they add to the international prestige of the Nation.

Another characteristic of stand-alone new starts, however, is that they tend to be expensive. There is every temptation to make these missions as sophisticated as possible, rather than to design them to accomplish, say, 90 percent of the scientific objectives at 50 percent of the cost.** In times when the budget for space science is large enough to meet the major objectives of science, this method of conducting research presents few problems, but in the present era of more limited available funding, another method may be appropriate.

***Throughout this document, the notion of stronger policy commitment is to be distinguished from that of a greater funding commitment (i.e., larger appropriations); neither necessarily implies the other.**

.● To see that there have been extreme budget fluctuations, it suffices to consider the figures in app. A.

..● It is important for all components of the space science establishment, Federal and non-Federal alike, to participate in the planning process. If all groups are invited to contribute to these discussions, those which may be adversely affected by the final outcome will have a better chance to adapt to their new constraints and will have less cause to complain. In addition, particular attention should be paid to interagency coordination. The importance of this topic warrants separate discussion, in see *International Concerns.*

"Given that budgets might vary from 5 to 10 percent above and below the planned levels, three alternative plans might be devised: one, in which the current level of effort is maintained, and one each for a 5 to 10 percent increase or decrease in the current level. If it is necessary for the budget to be cut, or possible for it to be raised, the existence of these alternative plans will facilitate making the corresponding changes in the program, with minimal wasted effort.

● *To a first approximation, it is just as difficult to obtain commitment to a small mission as to a large one.

The major alternative is to conduct space science research programmatically—i.e., to emphasize the development of disciplines and continuity of operations rather than to emphasize new starts (see app. A). This approach might be realized by sets of several closely connected space missions or experiments, planned as integrated series, directed toward well-defined goals of the several subdiscipline, and supported by programmatic and budgetary continuity from inception through postmission reduction and analysis of data.

Funding Delays and Effects of Cost Uncertainties

Funding delays, wherever they arise, generate increased costs, and these, in turn, cause instabilities in program planning, leading finally to an instability in the program and the planning process.

One important aspect of program stability is sound implementation. If projects turn out to have been initially underbudgeted, or if they must be stretched out in order to accommodate budget cuts, their cost effectiveness decreases. The Galileo mission is an example where costs have increased substantially because of delays; the result is fewer missions or higher overall costs. Although current budget totals appear to compare favorably with those of the past, a larger portion of current funding in fact tends to go toward supporting unproductive work, as is the case with Galileo. *

● The labor expended in redesigning a mission for other than scientific reasons, particularly when it is done more than once, can hardly be considered productive.

The appearance of current budget totals (see app. A) is deceptive in another way: just as the Space Telescope is taking a larger portion of NASA's physics and astronomy budget, so Galileo is doing the same for the planetary science budget-a factor which makes the decrease in the overall total of the latter even more serious for other parts of the planetary program.

The Need for Adequate Discussion of Program Priorities Among Interested Participants

If they wish to take a more active role in setting national policy goals for space science, appropriate congressional committees need to have closer contacts with the space science community. Because of its scientific status, its representative character, and its relative independence, the Space Science Board (SSB) of the National Academy of Sciences could be the appropriate vehicle for improving the liaison between Congress and the community of space scientists. Especially if, as suggested in the section entitled *Management Issues,* its advisory responsibility is widened to include related activities within other agencies, SSB can present formally to NASA and informally to Congress an integrated program of space science priorities based on the process of peer review. Through annual discussions with SSB, or more frequently, if appropriate, congressional staff could, in turn, assist scientists in understanding congressional priorities and funding considerations. *

● A formal relationship could also be established between Congress and SSB. In this mode, Congress through its committees could ask SSB to carry out special studies, in which case Congress could be expected to contribute to SSB'S costs. If this were done, SSB would be related to Congress as it now is to NASA: reports would not be delivered to Congress until they had been carefully reviewed and approved by the National Academy of Sciences through the mechanisms of the National Research Council. It would be desirable, however, for congressional staff to attend the nonexecutive sessions relevant to the studies, so that nothing in the final report would come as a surprise,

IMPACT OF MANNED SPACEFLIGHT–PRESENT AND FUTURE

Manned space projects arise as national political imperatives. Their total costs dwarf the science components that accompany them, and their impact on the space science program is substantial. Although scientists will learn to make good use of the shuttle, that vehicle is inappropriate and inadequate for certain kinds of research. Similarly, if a single permanent, manned space station is built, its architecture and orbital characteristics will to a large extent determine the kinds of space science research that can be done. For example, X-ray research needs a low-inclination orbit; solar terrestrial research and weather monitoring need high-inclination orbits. In general, a space station could be useful to science if: 1) a large enough research and analysis budget is set aside; 2) requirements for cleanliness and capabilities for pointing are met; and 3) access to orbits incompatible with the shuttle is assured (i.e., a commitment to an orbital transfer vehicle is made).

Many scientists believe that the United States should ultimately develop a permanent presence in space, centered on some type of manned space station, and that the need for such an effort will become more evident as the intentions and capabilities of other nations become clearer. In its planning for a possible space station, NASA has initiated extensive discussions with the Space Applications Board and SSB of the National Academy of Sciences to define the science and applications needs that a space station could meet. Scientists remain concerned that capabilities promised for a space station be in fact achieved, in order that situations may be avoided in which these capabilities —on which scientists have counted in planning and designing their experiments-remain merely virtual. Scientists are also concerned that cost overruns for a space station might, at least indirectly, reduce the funding available for the space science that cannot be done on a space station, or might result in a less ambitious project from which science capabilities might be excluded. *

Although most space scientists prefer to conduct experiments on unmanned flights, they will now have to tailor many of their projects to manned flights. To date, most space science experiments do not require the assistance of astronauts, whose presence adds greatly to the expense of missions. In the future, however, this situation may change. Already, the manned program has been very valuable to solar physics (e.g., in X-ray and ultraviolet photography of solar flares from Skylab) and to planetary science (e.g., in the selection of lunar samples), and it promises to be important for the life sciences. Eventually, astronauts may retrieve, repair, or refurbish scientific experiments, just as they may construct large space structures which hold promise for space research.

● **Space scientists remain convinced that cost overruns on the shuttle have been a major cause of the problems now facing the space science program. See, for example, app. F.**

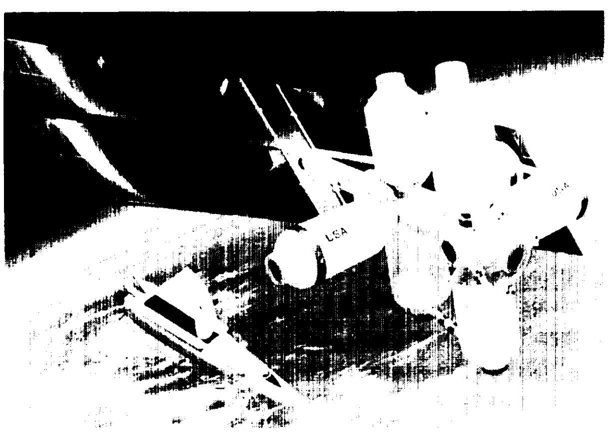

Photo credit: National Aeronautics and Space Administration

Artist's conception of future space station tended by advanced space shuttle

General Problem of Big Projects

For purposes of this discussion, a dividing line between big and small projects within NASA might be set somewhere between $100 million to $200 million. * Small projects include unmanned,

***More accurately, there are four major mission categories: 1) small (< $100 million to $200 million); 2) large ($200 million to $750 million); 3) very large ($750 million to $1,500 million); and 4) manned missions. Explorers fit into the first category. The second contains missions such as Galileo, Voyager, HEAO, Landsat, and GRO. The third contains the Viking missions, the Space Telescope, and the proposed OPEN and AXAF missions.**

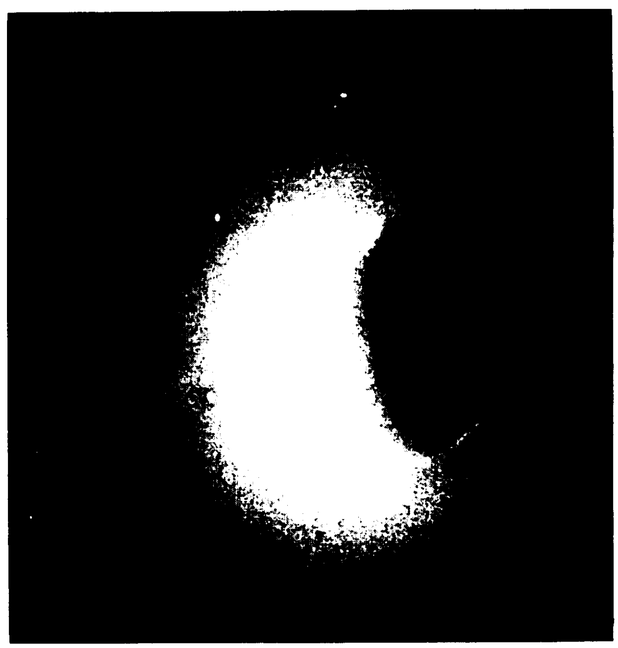

Photo credit: National Aeronautics and Space Administration

A 15-second far-ultraviolet exposure of the Earth, showing the extended hydrogen geocorona. This picture was taken by Apollo astronauts on the Moon

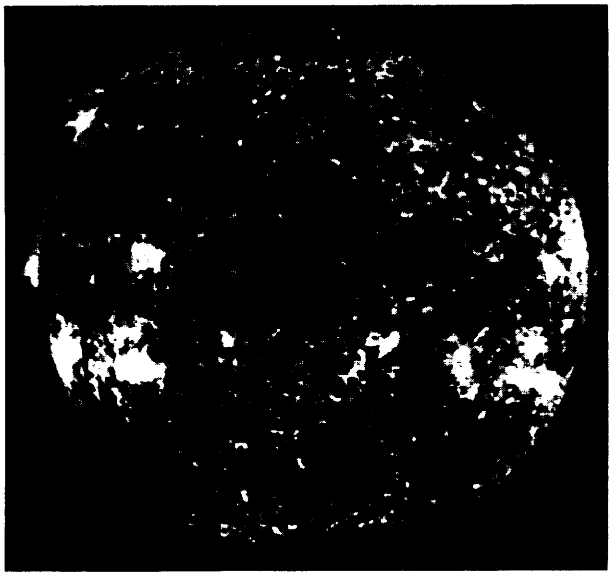

Photo credit: National Aeronautics and Space Administration

This photograph of the Sun taken Dec. 19, 1973, by astronauts aboard Skylab, shows one of the most spectacular solar flares ever recorded, spanning more than 588,000 km across the solar surface

typically scientific spacecraft in the Explorer class; big projects include all the manned efforts as well as several important science and applications missions, including Viking, Voyager, and Landsat. In practice, the large projects are divided into two categories: those, like Apollo or Space Shuttle, which respond to a national policy commitment and those, like Galileo or Voyager, which respond to the priorities of particular communities of end users. To date, no unmanned missions have fallen into the first category.

Within NASA, missions of the first type are given priority over those of the second, and those of the second tend to receive priority over small projects. The scientific community is apprehensive, on the one hand, that large reamed projects will increasingly call the tune for the entire space research program, specifically that science projects will be required to use the shuttle or a new space station, and, on the other hand, that large space science projects will draw funds away from smaller ones.

There are different points of view concerning the role of big projects within NASA. One point of view, reportedly shared by all previous NASA Administrators, holds that the agency's raison d'etre is its large manned programs, and that much of what the agency has been able to accomplish in space research has been based on the existence of those programs. A contrary point of view, shared by a number of scientists, holds that the Nation would have recognized the importance of basic research in space and that the space science program would have been successful regardless of the presence of large programs like Apollo or the Space Shuttle. The rationale given for this second point of view is that space science and the techniques for accomplishing it have provided and may be expected to continue to provide the basis for most of the utilitarian applications of space technology.

The Nation's past and largely successful space science effort has been conducted with both large and small projects in progress at the same time. Just as there are fundamental questions in high energy physics that cannot be answered without large particle accelerators, and questions at the frontier of astronomy that cannot be addressed without large telescopes, so there are important areas in space science that can be opened for investigation only by large, sophisticated missions.

On the other hand, small space science projects (e.g., those conducted on balloons or rockets, laboratory investigations, data analysis, and instrumentation development) are important in at least two respects: first, it is uneconomical to employ large instruments to do what small instruments can do; second, the existence of relatively many small projects provides the overall scientific context of the field, from which new ideas and concepts originate. A space science program consisting only of small projects would, over the long term, produce results of decreasing interest; one consisting only of large projects would soon be unsupportable.

NASA is aware of these arguments and, by conducting large and small projects simultaneously as much as possible, has strived to maintain a balanced space research program. With the maturation of each of the disciplines in space science, with level or declining budgets, and with other large, costly projects within NASA, it has become increasingly difficult to conduct a well-balanced space research program. Since autumn of 1981, the large-scale science projects have been particularly under attack, for the elimination of one or more of them would free substantial funds for other programs.

Another consideration is that, in a situation of more limited funding, large space science missions can be flown less often, partly because the competing needs of the several space science disciplines call for higher percentages of the space science budget, and partly because the growing sophistication of the instrumentation on these missions tends to make them more costly. Indeed, the frequency with which they are flown has decreased to the point where major groups within industry and in U.S. universities that are necessary for a successful, long-term space science program are being disbanded.

In summary, large science projects are necessary to sustain scientific progress, but tend to crowd out smaller scale projects, and, given current budget constraints, they have been mounted less often than required to maintain space science teams. In the present situation of level overall funding divided among fewer, but generally more expensive activities, an increasingly heterogeneous space science community has been forced into a mode of divisive competition for available resources. The diversity of the community, set in the context of constrained funding, makes consensus on priorities set by means of broad-based peer review especially difficult to achieve. Thus, officials at NASA, whose responsibility it is to make these decisions, face growing difficulties. A good solution to this complex set of problems is not yet evident.

Costs of Shuttle Payloads

During the past decade, the United States has spent some $20 billion for development of the space shuttle, which is the fundamental component of the Space Transportation System (STS). Advocates of this development have maintained that STS will: 1) advance the Nation's technological competence in space flight, 2) make it feasible to continue manned flight, and 3) reduce the cost of launching scientific and applications payloads (below the cost of launches on expendable unmanned launching systems), and thereby greatly expand the practical uses of space technology. The first and second of these objectives seem well served by the shuttle; the verdict on the third is not yet clear.[3] The whole question of how shuttle costs ought to be calculated is vexed. NASA's current policy is to recover from users only the marginal operating costs of shuttle flights, not total operating costs. (See app. D for a comparison of shuttle and expendable launch vehicle (ELV) costs.)

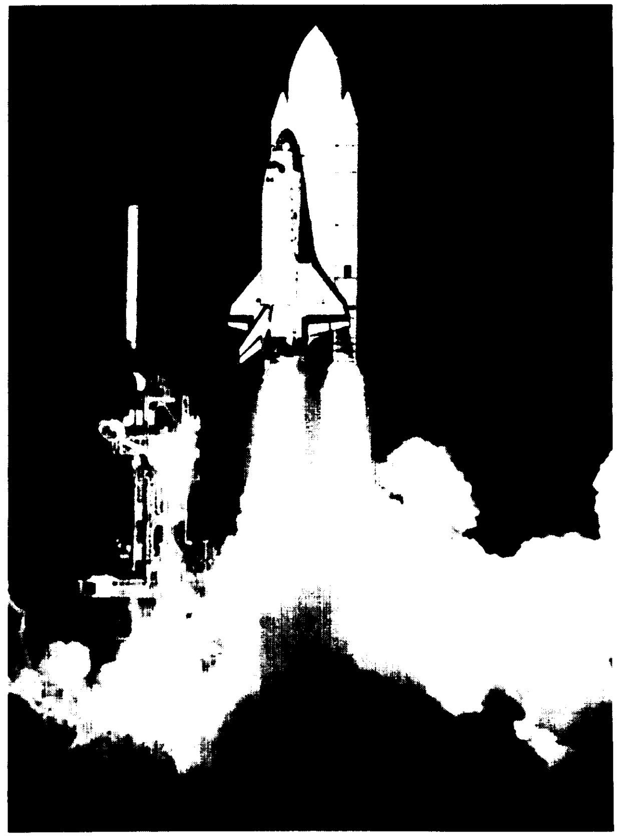

Photo credit: National Aeronautics and Space Administration

Launch of the space shuttle Columbia

Constraints Imposed by the Shuttle

Use of the shuttle, both because of what it requires and because of what it can and cannot ac-

[3]See, for example, Daniel Deudney, "Space: The High Frontier in Perspective, " Worldwatch Paper 50, The Worldwatch Institute, August 1982: 'The reusable space shuttle, first tested in 1981, was expected to accelerate the exploitation of space by reducing the cost of putting an object into orbit and allowing the repair or retrieval of orbiting satellites. However, [because of] funding delays and cost overruns it now appears that the shuttle will be only marginally cheaper than the new generation of expendable rockets. " See also app. E for an analysis of shuttle economics by James A. Van Allen.

complish, has a significant impact on space science. Experiments to be conducted on the shuttle require extensive documentation because they must all be man-rated, (i.e., certified not to endanger the crew). This requirement presents an unwelcome and inconvenient barrier to scientists, and raises costs. As more experience is gained with the shuttle, however, documentation requirements for man-rating may be eased.

The brief time of a shuttle flight presents another problem for scientific experiments designed to be conducted on the shuttle. In many investigations, a shuttle flight is scarcely long enough to provide a check-out of the equipment and provides modest results when compared with really long-term temporal and spatial coverage—often for more than 2 years—with free-flying satellites. In addition, if space science is not to be limited to those orbits accessible to the shuttle, one or more high energy upper stages or perhaps one type of ground-launched, ELV will be needed to supplement the shuttle's capabilities.

The shuttle is especially appropriate for space science missions that do not require long durations in orbit, that require a heavy payload to be placed in low Earth orbit, or that can fly in shuttle orbital inclinations. Observational astronomy and Earth observational experiments can benefit from the shuttle more readily than can other disciplines. On the other hand, the present capability of the shuttle is incompatible with the needs of some scientists (e.g., atmospheric scientists who require a polar orbit, or planetary scientists who require high-energy stages for interplanetary probes). However, with greater frequency of shuttle flights, launches into polar orbits from Vandenberg Air Force Base, and the development of an appropriate upper stage, this particular problem may diminish over time.

Concerns Regarding Military Use of the Shuttle

There is a general concern throughout the civilian community that military requirements may begin to dominate the shuttle's budgets, flights, and schedules. There is a similar concern that if a space platform is built, the military would preempt it, crowding out possible uses for science.

Recently, the U.S. General Accounting Office (GAO) has reported' that of the 234 shuttle flights scheduled through 1994, 114 of them (i.e., 48 percent) are dedicated to the Department of Defense (DOD); in the nearer term, 13 of the initial 44 flights through 1986 will be exclusively military. Not only will DOD be NASA's single largest customer, but also, at least through 1986, when user fees for the shuttle are to be renegotiated, NASA may be charging DOD substantially less per flight than it charges civilian users-$12.2 million as opposed to $18 million, a discount of 32 percent. In addition, it seems likely that NASA will be doing substantially more work for the military, and that the Jet Propulsion Laboratory, which has been heavily involved with NASA's planetary science program, may return, for the most part, to its earlier support role for the military.[5]

[4]"NASA Must Reconsider Operations Pricing Policy to Compensate for Cost Growth on the Space Transportation System," a Report to the Congress by the Comptroller General, MASAD-82-15 (Washington, D.C.: U.S. General Accounting Office, Feb. 23, 1982).
[5]*The Washington Post,* **June 5, 1982.**

MIX OF SCIENCE EFFORTS

Mission Complexity

Over the years in which the United States has been conducting space science, a broad range of mission types has been flown-from relatively simple experiment packages carried by sounding rockets and high-altitude balloons, to the highly complex Voyager flybys of Jupiter and Saturn. Now that funds to support space science have, overall, been on the decline since 1974 (see app. C), it has become necessary to make certain difficult choices as to which future missions, and even which types of future missions, are to be supported.

As space science matures, missions tend to grow more complex and expensive. As missions grow more costly, fewer can be flown, given a constant or decreasing level of funding; but as flight opportunities are cut back, there is a tendency to make the remaining missions more complex (and, usually, more expensive). Some program officers at NASA believe that the era of small-scale missions is mostly passed-that to obtain scientifically useful results now requires sophisticated (and

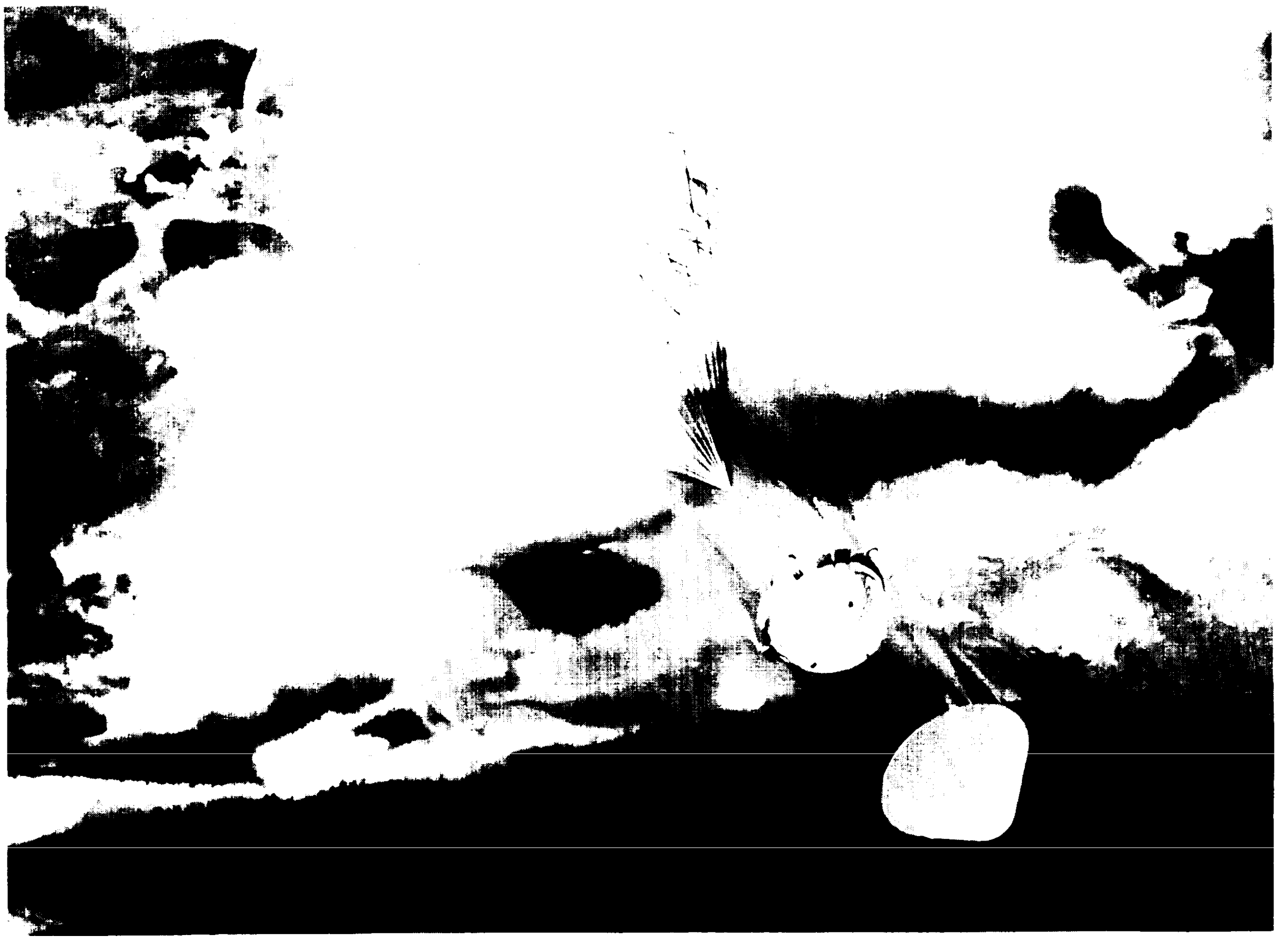

Photo credit: National Aeronautl/es and Space Administration

Artist’s conception of the descent of the Galileo probe into the atmosphere of Jupiter

costly) missions. It has become more and more difficult to mount small-scale missions, with the consequence that fewer sustaining activities are available between major flight opportunities. *

In view of the difficulty of planning and executing a balance of large- and small-scale missions in planetary science, NASA has established the Solar System Exploration Committee (SSEC). The charge of SSEC is to recommend to NASA an ordered, affordable program of exploration of the solar system.

There is disagreement about whether large or small missions are the more cost effective, and a detailed analysis of this question is beyond the scope of this study. * Similarly, the question of whether the Nation’s broad technological base would be strengthened by a policy of funding more small missions is difficult to resolve. Basic agreement, however, seems to exist on the position that a range of missions makes the most scientific sense. Not only will the small missions pro-

● Two committees of SSB—the Committee on Space Astronomy and Astrophysics, and the Committee on Solar and Space Physics—are currently examining these issues.

*Often a debate about cost effectiveness degenerates into a dispute about costs. It is tautological to assert that large missions cost more than small ones; the real question involves the amount of science returned per dollar of outlay. (The probability that quality of science differs from quantity of science is another complicating factor.) The absolute increase in launch costs, which translates into an increase as a proportion of total mission cost, has become a dominant factor in such calculations.

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vide a continuous level-of-effort when budgets are low, but these types of small expenditures, including the funding for laboratory work, will lead to interesting and promising new laboratory techniques and other new instrumental approaches.

If NASA were to revive small-scale scientific missions, some sort of safeguard would be necessary to allow at least some of them to remain small. One possibility would be to adopt some version of the strategy employed by the Department of Defense (DOD). The U.S. Air Force has been designated as the service responsible for coordinating much of the unclassified space research for DOD, and it does so through the Space Test Program (STP). A scientist who is interested in flying an experiment on an STP mission submits a proposal without knowing to which mission it will be assigned. Both university-based scientists and those at Government labs may compete for space on a mission.

Experiments are selected for utility and relevance to the DOD mission. Once the experiments have been ranked within each DOD lab, STP selects a compatible set of lab and university-based experiments for flight on a mission for which one or more DOD experiments have the top priority. There is an average of six or seven experiments per flight, together with the experiment(s) for which the flight was chosen. As of spring 1982 there have been 32 STP flights. Currently, the average is one flight per year. The cost for an STP flight is comparable to that of an early-day Explorer mission.

Scientists who have experience with STP cite several advantages that it provides:

- minimal documentation;
- minimal oversight and review procedures;
- emphasis on low-cost missions;
- willingness to stand by decisions, with no rescoping;
- willingness to accept experiments with a relatively high risk of failure; and
- possibility of short turnaround times for some types of experiments.

There are, however, some generally perceived disadvantages. These include:

- orientation toward narrowly conceived missions;
- inadequate support for postflight data analysis;
- uncertainty of launch time and possibility of long waits in the queue;
- poor cross-checking of experiments for payload compatibility; and
- a failure rate higher than that of NASA.

For the needs of space science, the general negative feature of STP is that it supports isolated missions rather than basic research programs.

Some features of STP appear to be attractive for the support of small-scale missions; some of its procedures might be adopted by NASA in cases where they would be appropriate. Another possibility would be for NASA actively to promote, through wider publication and support, the flight of experiments in which it is interested on STP missions. STP offers some segments of the scientific community an alternate way to fly an experiment at low cost and with few restrictions.

Funding Allocation for Data Analysis

Compared with the costs of the actual mission (including launch vehicle, instrumentation, mission operations, and support facilities and personnel), the cost of postmission data reduction and analysis is minor. Even so, according to recent independent reports of SSB and GAO, the funding set aside for data analysis is inadequate. [6]

NASA's emphasis on supporting new starts has tended to concentrate attention on hardware and operations, rather than on the total scientific project. Consequently, proper attention to the problem of data analysis has not been given beyond that required for the major, relatively easily achieved initial results of the experiment. There is, however, more science to be gained by allocating an additional small percentage of the total cost of the mission to further data analysis. Continued examination of the data can still yield im-

[6] According to GAO, "the Congress should examine the adequacy of NASA's allocation of resources between gathering space science data and analyzing it. Greater emphasis is needed during the data analysis phase of a program to obtain the maximum scientific benefit from the data obtained." ("More Emphasis Needed on Data Analysis Phase of Space Science Programs," a Report to the Congress by the Comptroller General; PSAD-77-114; June 27, 1977). See also the CODMAC report of SSB ("Data Management and Computation, Volume 1: Issues and Recommendations," National Adademy Press, 1982).

portant scientific advances as new techniques for analysis are developed, existing techniques are refined, and increased understanding of the underlying physical processes is acquired.

Postmission data analysis is often the lifeblood of university space research groups. If NASA places more emphasis on this activity, these groups will be better protected in times of more limited budgets; data analysis is a type of activity that can be conducted with success, regardless of the level of the overall budget. At present, however, follow-on data analysis for the lunar and Martian missions, as well as for those of several subdiscipline (e.g., X-ray astronomy), is funded below the level at which the activity can remain viable. If all scientific activity is not to cease in those areas of research which will have no missions for the foreseeable future, then a base level of funding for data analysis must be maintained.

In addition, there is a need for data from past missions to be correlated over a long time sequence in order to corroborate the findings of independent missions and/or experiments, as well as to facilitate syntheses unifying results from related fields of science. Past results are often the key to understanding in the fields of solar physics, solar-terrestrial relations, and atmospheric physics, all of which require that data obtained *over* many years be compared in order to understand the basic processes in the Sun and in the Earth's weather and, in particular, to discover cyclic phenomena.

Archiving of Data

Whether or not data returned from space missions should be archived depends on several factors, including that of costs v. benefits. Because space missions are very costly, and the data returned from them correspondingly valuable, data should be archived if they are likely to be lost, if the cost to repeat the experiment is sufficiently large, if a long time base is crucial for the success of the project, or if the data might be unavailable when required. Too often, however, data may have been archived as an alternative to timely analysis.

One technological development that may go far toward solving the problems of data archiving is the new laser-read video disk. All the data returned from the Voyager missions to Jupiter and Saturn could be stored on disk and made widely available to scientists for a few hundred dollars. An investment in technology will still be needed to realize this possibility, and it will still be necessary for a calibration program to be included on each disk in order to make the data most usable to other researchers.

INTERNATIONAL CONCERNS

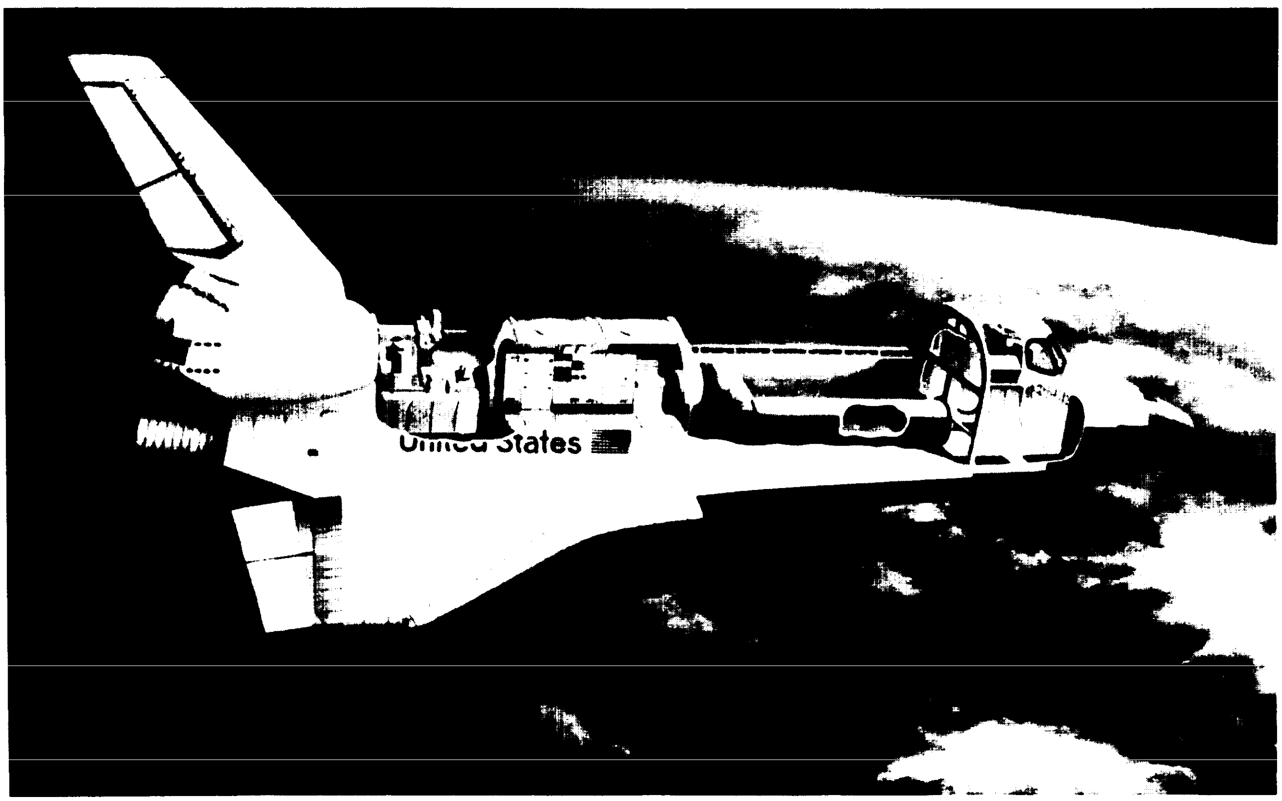
International cooperation promises scientific, cultural, economic, and social benefits to all participants. Over the long term, the prospects for international cooperation in space science ventures look very good. In general, it seems clear that missions that would be too expensive for the United States to mount alone could be undertaken with international support. The space programs of the Europeans and the Japanese have now sufficiently matured to permit them to become substantial partners in joint missions. While the current international situation tends to minimize East-West interactions in high-technology fields, the Soviet Union is quite active in space research activities, and it would be to the Nation's advantage to cooperate in some areas where the U.S.S.R. is clearly the leader—e.g., in the life sciences. In the past, international science projects have been one of the most effective means of making contacts across cultural and political barriers.

In the short term, however, prospects for major cooperative efforts are not so bright. Cancellation of the U.S. spacecraft in the International Solar Polar Mission argues that the United States and the U.S.S.R. are unreliable partners. If Spacelab-6 (to which Canada has already committed substantial funds) is also disapproved, foreign governments will be even less likely to agree to international ventures with the United

States. In any case, there seems to be little possibility for cost sharing on the missions in the immediate future, for most of them are already planned and do not include foreign partners. A more feasible form of cooperation for the near term is that of coordinated spacecraft launches, such as the International Sun-Earth Explorer missions, where each spacecraft is fully prepared by a single nation; in such cases, the scientific benefits derive from the sharing of data. However, the decision not to fly an appropriately designed mission of this sort to Halley's Comet, in coordination with the Giotto spacecraft of the European Space Agency (ESA), represents another lost opportunity for international cooperation.

In general, a more effective mode of cooperation must be devised that will allow two or more nations jointly to plan and execute space missions with minimal difficulties. There are two particular concerns that should be addressed. One is that commitments which the United States makes to international ventures should, if at all possible, be kept. Nothing will more effectively prevent future joint missions than a U.S. record of broken agreements. Cancellations of U.S. commitments to international space ventures maybe necessary for budgetary reasons, but such cancellations indicate uncoordinated budget planning and the lack of concerted, bipartisan support for space exploration. It would seem fruitless to urge that, in order to avoid such cancellations in the future, commitments to international missions should be made at higher levels of authority, for some of the international commitments on which the United States has reneged were made by Congress. Nevertheless, it would be desirable to devise some means of assuring that U.S. commitments are honored.

The second concern is that, whereas NASA has allowed foreign experiments to be proposed for inclusion in U.S. missions (e.g., 20 percent of the instruments on the Space Telescope are Euro-



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pean), foreign organizations have generally been opposed to including U.S. experiments on their spacecraft. Now that foreign programs have in important respects become competitive with those of the United States, there is less need for them to protect their missions from possible dominance by U.S. teams of experimenters. In response to U.S. pressure for an appropriate quid pro quo in order to redress this imbalance, ESA, at the urging of the Space Science Committee of the European Science Foundation, has recently begun to formulate a new policy to address this problem.

From the U.S. point of view, increased participation in foreign missions is desirable. First, it obviously costs less for a U.S. team of scientists to pay the incremental costs of participating in a mission whose major costs (for design of the mission, for the launch vehicle, and for various support services) are borne by a foreign agency, than for the United States to develop and fly a comparable mission. Second, U.S. scientists find the cross-fertilization of ideas which results from participating in foreign missions to be very valuable. Nevertheless, if the level of U.S. participation in foreign missions does increase, the Nation should be prepared to provide the travel and other mission support costs that will be necessary to assure the success of that participation.

One difficulty for international cooperation in science surfaces when foreign experimenters participate in U.S. missions, for acceptance of a foreign proposal in a given subdiscipline necessarily reduces the support for U.S. teams competing in the same subdiscipline. Another problem is that NASA does not have well-understood and uniform procedures for funding U.S. co-investigators on European missions.

In summary, the prospects for international cooperation on large-scale projects, which the United States would find it difficult to fund alone, appear to be promising. Before an international venture is begun, however, it should be assured that all partners will gain by the cooperation, that it can be carried out with the backing of all participants throughout the lifetime of the project, and that the additional administrative costs that will be incurred are acceptable.

MANAGEMENT ISSUES

Management Alternatives

To date, most of the support for space science research has come from NASA, though DOD has funded much classified activity as well as the relatively small STP effort, and NSF supports work in ground-based astronomy and some data analysis and theoretical work in solar-terrestrial space physics. As space science has developed into well-defined disciplines and subdiscipline, requirements for flights to further their research programs have multiplied. Because of the high cost of its scientific missions relative to the cost of related science projects within DOD or NSF, NASA has a greater problem as it considers whether its missions will be supported by Congress and be appealing to the public.

It has been suggested that NASA should place more responsibility for space science experiments in the hands of principal investigators and to assign responsibility for future space science operations to organizations like the Space Telescope Science Institute (STSCI). Another more radical suggestion has been to establish a separate agency, with a structure parallel to that of NSF, whose responsibility would be to support large, more costly, scientific enterprises such as space science research and high-energy physics. The general rationale for these suggestions is that NASA is chartered as an R&D agency, not an operations agency, and that the orderly progress of science requires commitment to continuing operation of scientific facilities. Both of these suggestions require comment.

Although there are good individual scientist-managers, not all scientists are good managers. As long as the science is not costly, and risks can be taken, there is considerable advantage in diversifying managerial responsibility. However, when costly, large projects are undertaken, the Government will generally institute procedures to make success more certain. These procedures lead to more oversight and tend to separate the scientist from his experiment.

The growth of "big science" has led to the establishment of national centers in astronomy, of which STSCI is one example. Patterned after its successful ground-based counterparts, STSCI is an example where managerial responsibility has been placed in the hands of the space science community. NASA set up STSCI at the urging of the scientific community, although NASA's success in managing the International Ultraviolet Explorer indicated that it could have managed the Space Telescope quite effectively. In any case, STSCI promises to satisfy the desires of the space scientists to have a more active management role in space research than they have had in the past.

At first sight, the establishment of a separate agency to support large scientific enterprises appears to have merit. As noted above, large projects require a different type of organizational management and more personnel than do small projects. Although concentration in a single agency would group large science projects, it might later destroy much of the internal balance and coordination between large and small science that currently exists within NASA and the Department of Energy (DOE). The new agency could easily become a target for budget cuts on the premise that a small percentage cut would still permit the science to be done, but would be the source of significant amounts of funds.

Distribution of Talent and Resources

So far, the space research programs conducted within NASA, at universities, and in industry have been rather evenly supported. The U.S. space science effort is truly a national program, and the distribution of resources and capabilities throughout the country has resulted in a reasonable balance. If, however, budgets for space science research remain constrained, then the possibility of an imbalance in the distribution of talent becomes greater because university and industry teams increasingly will have to be disbanded. It is important that a reasonable balance be maintained: healthy competition among researchers within NASA, at universities, and in industry has produced and will continue to produce the best science. Each leg of this triad has particular strengths, requirements, and/or responsibilities:

- Many of the new ideas for research originate within universities; in addition, the independence of university teams is vital to the process of correcting imbalances in the program if they should occur.
- NASA teams need sufficient work to maintain a reasonable level of competence because they manage the projects and provide valuable firsthand experience necessary for successful missions.
- As builders of most of the complex spacecraft systems, industry teams are crucial to the survival of an experimental space science. It is already the case that much of the complicated hardware for sophisticated missions cannot be built except with the expertise to be found in industry. There are too few civilian space missions to provide substantial profits for any company, but industry desires to participate in the civilian program because of the positive influence of space research activity on recruiting and retention of staff and on corporate image.

A further problem resulting from recent budget cuts is that, in order to retain key technical personnel, NASA maybe assigning them to relatively unproductive oversight roles. This may lead not only to further loss of key personnel, but also to an overassignment of oversight tasks to the people who remain, even though there are fewer programs to administer. However understandable this response to a difficult budgetary situation may be, it is wasteful of resources, and in the long run, it maybe detrimental to the morale of good scientists and engineers who would prefer to work in a more strictly scientific capacity.

Toward a More Effective Program

With time, the boundaries between space- and ground-based astronomy are becoming less well defined. The division of Federal funding wherein NASA has the lead agency responsibility for space-based astronomy and NSF that for ground-based astronomy is becoming arbitrary, for scientists increasingly need to conduct both types of research in the ordinary course of their studies. As a result, it has been suggested that NSF should assume a larger share of postmission analysis of space-derived data and that NASA should fund a larger share of ground-based research that is of interest and importance to its overall mission. If this course is to be taken, the Office of Management and Budget (OMB) could assume the responsibility for assuring that the balance of funding available to these two agencies would allow a more effective partnership to develop in the Federal funding for space- and ground-based astronomy than that which currently exists.

A similar problem has been indicated by the Astronomy Survey Committee report of the National Academy of Sciences,[7] namely that the support for ground-based astronomy provided by NSF is not keeping pace with the support for space-based astronomy provided by NASA. In order to correct this imbalance, $40 million (in 1982 dollars) would have to be added to the NSF astronomy budget. The Solar-Terrestrial Physics report of the National Academy of Sciences also shows a relative lack of support for ground-based activities. In general, whereas at least the physics and astronomy portion of NASA's space science budget has continued to increase in recent years, NSF has been unable to make any new major capital expenditure decisions in astronomy for over 12 years.

Scientists generally do not believe that existing coordinating mechanisms are very effective. They do not see evidence that coordination is occurring at the higher levels of the agencies; some of them doubt that it occurs sufficiently at lower levels. One widespread opinion holds that OMB or the Office of Science and Technology Policy should take a more active role in interagency coordination, if only to set up a formal cross-agency advisory mechanism.

Some type of cross-agency advisory mechanism might be useful, for example, in resolving potential jurisdictional disputes and coordinating situations where one agency may wish to assume new responsibilities, to relinquish previous ones, or to exchange some of its current ones for others belonging to another agency. If an effective mechanism of this kind had been in place, the recent budgetary uncertainties involving the responsibility for funding the Infrared Telescope Facility in Hawaii might have been averted.

Another means of addressing the problem of interagency coordination would be to broaden the responsibilities of SSB to include oversight of NSF's activities in solar-terrestrial physics, astronomy, and some atmospheric research, as well as space-related activities of other agencies. As the major existing scientific advisory body for space research, SSB presents recommendations only to NASA. If, however, the purview of SSB were broadened, then its recommendations to NASA would be more likely to be based on considerations of disciplinary continuity across agency boundaries. Such continuity would help to ensure a more balanced Federal program in space science. *

In addition, it maybe desirable to broaden the charter of SSB in another direction, namely, to give it the responsibility for combining priorities of space science with considerations of cost. If this is done, several considerations must be kept in mind. First, SSB, by virtue of its role within the National Academy of Sciences, operates in conjunction with an extensive process of peer review, and, hence, cannot do short-term problem-solving. Second, the space science community is quite heterogeneous (as is SSB itself); space science, properly speaking, covers a broad range of disciplines, each of which has its own set of

[7] George B. Field, report of the Astronomy Survey Committee, *Astronomy and Astrophysics for the 1980's* (Washington, D. C.: National Academy of Sciences, 1982). It should be noted, however, that this report does not treat the entire range of space science disciplines.

*On the other side it must be said that NSF's Astronomy Activity Committee already provides guidance in the area of astronomy and takes the space science activities into account. The National Academy of Sciences has found it necessary to maintain separate committees for space physics and solar terrestrial research, even though they address the same subject matter, because the modes of operation for space- and ground-based research are rather different.

In addition, such broadening of SSB'S responsibilities could make it into an unwieldy bureaucracy. In each subject area in which this were done, the relevant committee of SSB would have to assume additional responsibilities, and SSB itself would have to assume more of an oversight role and less of a coordinating role. All of this would be possible only if quite a number of different offices in several different agencies consented to be advised in this manner.

priorities. Third, as contrasted, for example, with the organization of high energy physics into three or four cost centers, space science has many cost centers; it is big science, but it is not so heavily concentrated. For these reasons, SSB cannot be expected to function in the same way as does the High Energy Physics Advisory Panel (HEPAP), a scientific advisory panel of the Department of Energy. *

The heterogeneity of space science complicates the choice of priorities. Given NASA's division of space science into:

1. physics and astronomy,
2. planetary sciences, and
3. life sciences,

and assuming, further, that in each of these divisions subsets of SSB can meaningfully rank potential projects, it is nevertheless true that the assignment of a single absolute priority for all of space science from among the top ranked projects in each division is *not* a scientific decision. Such a decision is essentially political, based on considerations of what Congress is likely to support or what is needed to maintain balance among the disciplines. Space scientists are not notably more qualified to make such a political decision than is any other community.

An essential element in making this situation more tractable is to make a clear separation between the activities that are purely scientific and those that are, more strictly, engineering. In the past, the funds for the latter have far exceeded the funds for the former, and each division of science within NASA has had to pay for its missions out of its own budget. For reasons detailed earlier, big science missions—i.e., the engineering activities (hardware development, launch costs, and mission operations)—tend to consume the resources for small science-i. e., the continuing activities that are scientifically significant (including data analysis, theory, experiment design, and perhaps small to moderate missions for which hardware costs do not entirely dominate). If these budgets were separated, then SSB could very well make recommendations for setting the level of the nonmission budget, which would support the continuing science efforts of each of the space science divisions.

The level of the hardware budget, because it would have a strong political component, would be much more complicated to set. In general, because there would be a series of missions, each dedicated to one or more disciplines or subdiscipline, the level of this budget would vary rather widely, depending on the point of the mission cycle. SSB could function with respect to this budget much as it does now; i.e., it could continue to make recommendations for major missions corresponding to projected increases in available funding.

The current crisis in space science might well be an opportunity for SSB to take stock of the details of the problems indicated in this report. There is, for example, no clear accounting of the numbers of people engaged in space science, or of their distribution; data concerning the precise effects of reduced or level funding on research groups are still anecdotal. Without this information, the present health of space science research in the United States cannot be precisely assessed, nor can its future needs be predicted. This information would be especially useful for determining what an optimal base budget for space science would be.

'Overall, the HEPAP model is not a good one for SSB. High-energy physics is a rather narrowly defined scientific area with a small range of potential initiatives that have been well studied and costed. SSB and its committees, on the other hand, deal with areas in which costs are usually vaguely defined, although they make use of whatever cost information is available.