Space Science Research in the United States

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SPACE SCIENCE RESEARCH IN THE UNITED STATES

A TECHNICAL MEMORANDUM

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Preface

Selective budget cuts and reallocation of funds within the National Aeronautics and Space Administration's space science program have left planetary science, solar and heliospheric physics, gamma ray astronomy, and X-ray astronomy with uncertain futures. Indeed, many in the space science community think that the program has lost its sense of direction and that it is time for national goals for space science to be articulated. Responding to this concern, Rep. Cooper Evans of Iowa requested the Office of Technology Assessment (OTA) to examine the direction and purpose of the U.S. space science program. In particular, Rep. Evans asked OTA to "assist Congress in evaluating the effectiveness of our space science program and in discovering what its future needs are."

In undertaking the study, OTA sought the contributions of a wide spectrum of knowledgeable and interested parties. To this end, a questionnaire, designed to elucidate the issues and problems in the current conduct of the U.S. space research program, was sent to representatives of several Federal agencies, space scientists, and staff members of congressional committees and individual Congressmen. Over 80 formal replies were received. Concurrently, OTA arranged a number of separate meetings and discussions with these three groups for additional detailed evaluation of the issues. These various contributions were then organized into background reports which served as the basis of a workshop held at OTA on May 5, 1982.

The following technical memorandum summarizes and critiques the views of many people interested in space science, several of whom attended the workshop. In preparing this document, OTA did not attempt to obtain the views of those who are opposed to or uninterested in space science, nor did OTA attempt to rank space science against other national priorities.

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Chapter 1
Summary

The information presented in this document derives primarily from consultation with a wide spectrum of people involved in space science. In undertaking this study, OTA did not obtain the views of those outside the space science community, nor did OTA attempt to rank space science against other national priorities.

CURRENT SITUATION–DESCRIPTIVE

Despite its many noteworthy achievements over the past 20 years, the space science program of the United States has, as a whole, been placed in a holding pattern and, in significant parts, been forced to retrench. Many now see space science as in a state of crisis. The National Aeronautics and Space Administration's (NASA's) overall budget for space science has, in general, been on the decline since 1974 (see app. C), and that of one category, planetary science, has declined precipitously. For the latter part of this period, the space science budget for physics and astronomy has been on the rise, but this increase is largely being spent on a single major project, the Space Telescope.

As a result, few missions are in prospect: in planetary science, only the major Galileo mission to Jupiter is planned for the 1980's; in solar and heliospheric physics and X-ray and gamma ray astronomy, all major missions have been postponed. Not only have the numbers of missions decreased, but there is insufficient funding for important interim activities such as data analysis. Thus, there is an uncertain future, not only for planetary science, but also for several subdisciplines which fail under the rubric of physics and astronomy.

Two additional factors make the situation particularly acute. First, most research activities follow a cycle in which new subdiscipline are born, grow to maturity, and then taper off into a continued, but reduced level of activity. But space science, as a relatively new field of research, finds itself with all of its subdiscipline still ripe for further growth. No space science subdiscipline have yet reached a point of naturally reduced activity. Given this situation, the general truth that, with a relatively constant level of overall funding, growth of some subdiscipline can occur only at the expense of others, becomes of particular concern. Second, as the number of missions has declined, those that remain have tended to be more complex, sophisticated, and expensive, and have tended to squeeze out the smaller and less expensive missions which have in the past supported a broad research base. These two factors, taken together, make it difficult for a number of productive teams of researchers in universities and in industry to remain viable.

CURRENT SITUATION–ANALYTIC

In all U.S. pronouncements on space policy, from the 1958 National Aeronautics and Space Act to the White House Fact Sheet on National Space Policy, released on July 4, 1982, continuing progress in space science is cited as a national goal. Because of this general policy, space science receives some portion of the Federal budget for space activities. Unlike the manned program, however, space science has never been directed toward a particular goal of unequivocal priority. Without the kind of commitment that arises from acceptance of a challenge to meet a particular goal, space science research has been conducted in a mode where the programs undertaken are determined primarily by whatever budget is made available (the levels of which fluctuate widely) and only secondarily by scientific goals. Furthermore, even within the budget made available for all of space science, the importance of certain scientifically critical activities has not always been recognized. As a result, no base budget has ever been set to ensure that these activities are sustained.

The impacts on space science from the manned program are often substantial. Large development programs, like Apollo or the Space Transportation System (space shuttle), are undertaken as national political commitments, and therefore have the highest priority within NASA. When these programs experience cost overruns, they tend to draw funds away from NASA's other programs, including that of space science. When there is pressure to move funds out of space science into other parts of NASA, the purely scientific activities—data analysis, theory, and mission design—are the least protected.

An analogous problem exists within the space science program in that large science projects, like Viking or Voyager, tend to draw funds away from the smaller ones. A significant measure of the past success of NASA's space science program has resulted from a balance of large and small projects undertaken at the same time; a certain concern, therefore, must attend the possibility that, with limited total funding, the few remaining projects may be large, leaving no support for small ones. There is an additional concern that, especially with approval of new-start status for small missions at least as difficult to achieve as for large ones, missions originally designed to be small tend to grow into relatively large ones. The Space Test Program, operated by the U.S. Air Force for the Department of Defense, could provide a model for a renewed NASA effort to design a series of small-scale, productive missions that would remain small.

At the present time, funding allocation for post-mission data analysis is generally insufficient to permit optimal use of data returned from spacecraft. Relative to the total cost of a given mission, the cost of analyzing the data returned is small. Given the current practice of combining the budget for science per se with the budget for hardware, some have suggested a minor reallocation of mission funding, in which a modest reduction in overall mission capability would free additional funds for data analysis, as a way to ensure an improved scientific return. Alternatively, a base budget for science, including support for data analysis, might also solve the problem.

The budgetary situation of space science has been characterized by major fluctuations caused, in large part, by pressures originating in other areas of the space program. Planning for space science, therefore, even if it is extensive, is not necessarily effective over the long run. Every effort should be made to avoid situations in which plans are made on scientific grounds and then modified later on nonscientific grounds. At a minimum, the science will suffer; to the extent that expenditures have been made, there will also be monetary loss. In addition, because many efforts are interrelated, changing one may affect others, sometimes substantially. Finally, failure to see a project through to completion can adversely affect the careers of the scientists involved, often requiring them to reorient their research programs, and can damage the prestige of the Nation, particularly when international agreements are broken.

International cooperation in scientific activities has been fruitful in the past and, for possible major missions in the future, may be highly desirable in order to share costs. International partners, however, perceive a problem of the United States reneging on commitments to international missions, including commitments made by Congress. As a result, they are reluctant to enter future agreements with the United States. This situation is likely to continue until better assurance can be given that U.S. commitments made to international space science missions are honored.

POSSIBLE NEW DIRECTIONS

The current practice of budgeting most flight missions as independent new starts emphasizes spectacular accomplishments, and is not necessarily optimal for scientific progress. This practice has perhaps been perceived as necessary in the absence of a national commitment to particular space science goals. The alternative most often discussed—what might be termed the programmatic approach—differs in at least two respects.

First, budgets for important continuing activities (including instrument design, data analysis, theory, and perhaps small- to moderate-sized missions) would be separated from (and thereby protected from cost overruns in) the budgets for major missions (including hardware, launch, and mission operations). With this separation, scientifically valuable, but unspectacular activities could be sustained even in times when overall funds are strictly limited; missions to take advantage of unique opportunities could be supported as the overall budget allowed.

Second, missions for each discipline would be designed primarily in accordance with long-term scientific needs. Especially with a commitment to particular scientific goals, the programmatic approach might make the entire space science effort—planning, execution, and data analysis—more effective.

In the opinion of many space scientists it would be advisable to place the responsibility for scientific projects as nearly as possible in the hands of the principal investigators. The current management scheme for the Space Telescope Science Institute provides an interesting example (and test case) of how scientists themselves may undertake the long-term operation of a major research facility.

The boundaries that formerly justified NASA's being the lead agency for the space-based efforts in space science and the National Science Foundation's playing a similar role for those that are ground-based are becoming increasingly arbitrary. There is some indication that a cross-agency advisory mechanism would be useful in adjudicating jurisdictional questions that occasionally arise among these and other agencies responsible for various space science activities and in ensuring a balanced, nonduplicative space science effort. One possibility for addressing problems of coordination would be to broaden the charter of the Space Science Board (SSB) of the National Academy of Sciences to include determination of priorities of all activities in space science, not just those proposed for NASA.

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 technical memorandum. An initial task would be to give a clear accounting of the numbers of people engaged in space science, and of their distribution; analyses of the effects of reduced or level funding on research groups could be undertaken. Information relating to these matters is still largely 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.

Overall, it seems desirable for SSB or some other duly constituted body to begin a more thoroughgoing effort to set scientific priorities for space science within a framework of possible budgetary alternatives. If this were done in a context in which: 1) one budget would be set for continuing activities that are scientific per se, and 2) another budget for missions would be separately negotiated, scientific expertise might be brought to bear on the choice of space science activities more effectively.

Chapter 2

Current Situation in Space Science

Over the past quarter century the United States has developed a scientifically productive space science program. The largest number of missions have been dedicated to solar and solar-terrestrial physics; as a result of this sustained work, scientists are developing a good understanding of Earth's magnetosphere and its interactions with many solar phenomena. The Mariner, Pioneer Venus, and Viking missions have been flown to our neighboring terrestrial planets, Mercury, Venus, and Mars; Pioneer and Voyager spacecraft have returned a vast array of important and exciting data from Jupiter and Saturn in the outer solar system. The planned launch of the Space Telescope will increase our knowledge of the farthest reaches of the universe with imaging capabilities never before achieved in optical astronomy.

Over the past few years, however, there has been a significant downward trend of budgetary support for one subdiscipline* of space science, planetary exploration. (See app. A.) Recent budget cuts have now called into question the continuation, survival, and future viability of the U.S. planetary science program. In the view of the planetary scientists, the program is in danger of complete collapse. If the present trend in funding were to continue, the planetary science program would be extinct by the end of the decade.

In spite of the apparently greater resources at their disposal, problems confront solar and heliospheric physics, and X-ray and gamma ray astronomy, all of which have had their major missions deferred. Overall, only the launch of the Space Telescope and a possible repair of the Solar Maximum Mission remain in prospect. Indeed, there is no assurance that the United States will fly a major space science mission after the Space Telescope.

It was widely held in the OTA workshop that current funding for space science makes it impossible to maintain all the subdiscipline at viable levels. There is some possibility, however, that a relatively minor reapportionment in funding from hardware development to postmission data analysis could be a useful interim measure. Despite its best efforts to provide a balanced space science effort, the National Aeronautics and Space Administration (NASA) has found it necessary to reduce support in planetary research in an attempt to maintain previous commitments within the agency and to maintain the viability of the remaining projects.

Space scientists are especially concerned by four immediate problems:

1. lack of flight opportunity;
2. uncertain commitment to the range of disciplines;
3. lack of support for postmission data analysis; and
4. threatened loss of data from existing spacecraft.

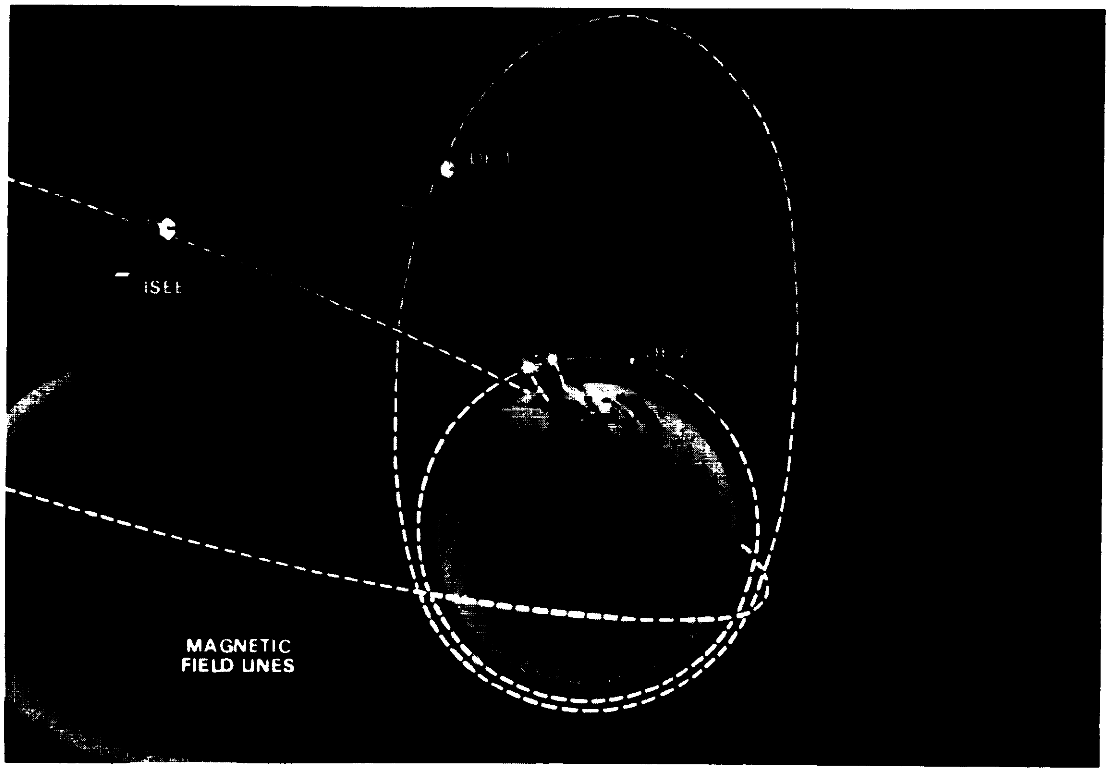
None of the disciplines of space science escapes the threat posed by these elements.

The area of most pressing concern to scientists, however, has been planetary exploration, where effected or proposed funding cuts of so percent in mission operations and data analysis and 30 percent in research and analysis, between fiscal year 1981 and fiscal year 1983, are resulting in significant reductions in basic research in planetary science programs at universities and NASA centers; if inflation is taken into account, these cuts are significantly larger. Some research laboratories and institutes such as the Jet Propulsion Laboratory, which have been operating for many years, may have to direct their efforts into other fields.

These cuts have seriously threatened to terminate productive, active spacecraft (Pioneer Venus Orbiter and Pioneers *10* and 11). Because of the current situation in planetary science funding, no experimenter can with any reasonable degree of assurance foresee the time when his experiment, if selected, will fly; no laboratory scientist can feel confident that a new research project will be funded long enough to come to fruition.

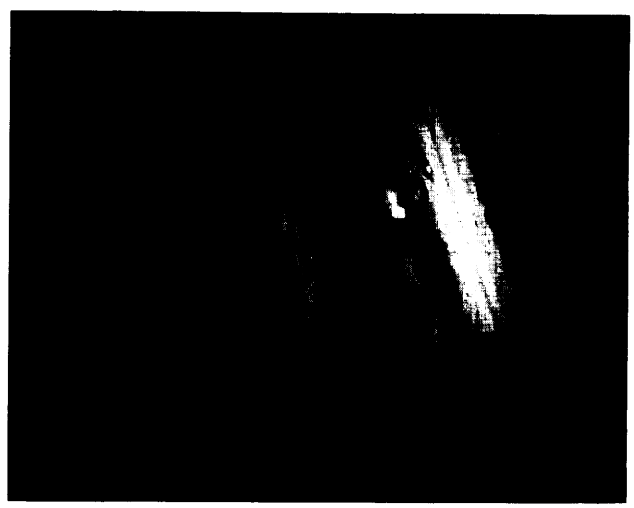
* In this document the word "subdiscipline" is taken to mean any branch of recognized disciplines; thus, X-ray astronomy is a subdiscipline of astronomy.

The Diversity of Space Science

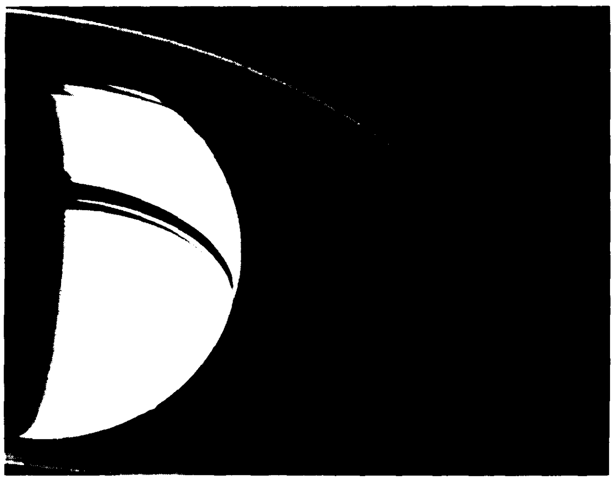


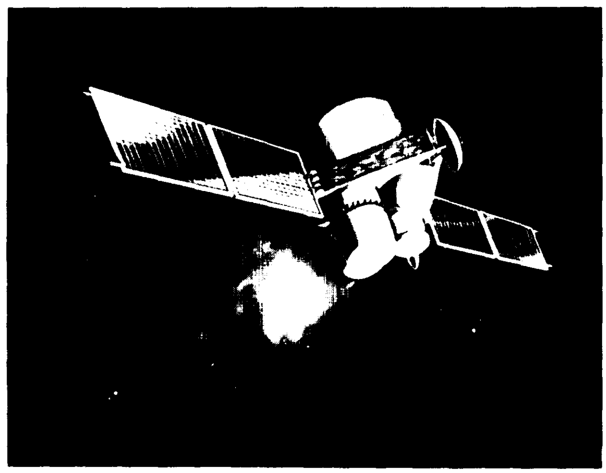
By releasing visible metal vapors at altitudes of about 1,450 km, NASA scientists were able to measure the electric fields that cause auroras in the underlying ionosphere by accelerating charged particles along the Earth's magnetic field lines. Measurements were made Apr. 12 and 28,1982, by particle detectors aboard two Dynamics Explorer satellites

Region of volcanic eruption on the Jovian satellite 10 is pictured from data returned from Voyager I's encounter with Jupiter, Mar. 4, 1981



Jupiter, its Great Red Spot, and three of its four largest satellites are visible in this photo taken Feb. 5, 1979, by Voyage 1, when the spacecraft was 28.4 km from the planet. The innermost satellite, 10, can be seen against Jupiter's disk





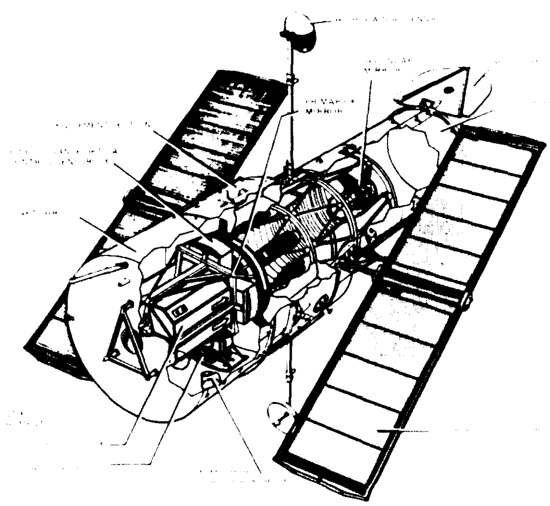


Photo credits: National Aeronautics Space Administration

Top: **Litter on Mars: the shroud which protected the surface sampler instrument on Viking II during the spacecraft's year-long journey from Earth lies shining on the Martian surface after its ejection**

Middle left: Image of the far side of Saturn and its rings, returned by Voyager II on Aug. 29, 1981

Middle right: **Artist's conception of the Gamma Ray Observatory**

Lower left: **The Space Telescope, to be launched into orbit from the Space Shuttle, will allow scientists to gaze seven times farther into space than ever before**

Throughout space science this problem is compounded by the maturation of the several space science disciplines. At present funding levels, the frequency of new missions for each major discipline has already decreased to about one per decade per discipline. The disciplinary category of planetary exploration and the subdiscipline of X-ray astronomy are perhaps affected the most by this situation; solar-terrestrial research and the atmospheric sciences also are approaching a critical level of support. Increasingly, universities and industry are assessing the opportunities to be so minimal that they will no longer pursue them; as a result, experienced scientists are leaving these fields, and students and other new researchers are not entering them.

This situation has been caused in large part by a trend toward fewer, more expensive missions. During the earlier stages of the national space program, there was, at least in the astronomy and physics disciplines, a broad mix of relatively inexpensive science opportunities (including experiments on sounding rockets and balloons) and facilities-class spacecraft (Orbiting Geophysical Observatories, Orbiting Solar Observatories, Orbiting Astronomical Observatories, Interplanetary Monitoring Platforms, Pioneers, etc.). This mix allowed for an active, growing community to do important, productive work. If one group failed to qualify for flight on a major observatory spacecraft, it might still qualify for a small project, in which the group could still maintain productive activity and could develop plans for the next major opportunity. Today, however, failure to qualify for a major mission is extremely detrimental to the long-term continuity of the proposing group, for there are insufficient funds set aside for the smaller projects. (These include such important interim activities as analysis of data from previous missions.)

A number of problems have attended the development of large, sophisticated missions. For example, because the Galileo mission to Jupiter is dependent on the shuttle, delays in the latter, and its associated upper stage, have caused the former to be delayed as well. The delay in Galileo caused its funding to be stretched out, and ultimately increased its costs.[1] For another example, maintaining balance within a discipline becomes a problem when, given a relatively constant space science budget, increasing outlays for one project, particularly a major one like the Space Telescope, reduces the funding for others.[2] Space science has been affected also in that the number of flight opportunities on the shuttle has been decreased and in that those missions which require a high-energy upper stage have been indefinitely postponed.

Another important aspect of the current situation is that the United States currently enjoys scientific and technological leadership in most of space science, but in certain areas, e.g., cometary research and ocean remote sensing, leadership could soon pass to the Soviets, the Europeans, and the Japanese. Failure of the United States to mount a mission to Halley's Comet puts U.S. scientists at a clear disadvantage with respect to the Europeans and the Soviets, both of whom will be flying such missions. A similar situation prevails in oceanography:

> Japan presently has an ocean satellite under development, MOS-1, scheduled for launch in 1985; it will carry all passive sensors. The European Space Agency is in the final stages of authorizing funds for ERS-1, scheduled for launch in 1986 or 1987–it will carry active and passive sensors. Canada is interested in flying a Synthetic Aperture Radar (SAR) with another country in the late 1980's.[3]

[1] See app. B.
[2] See app. C.
[3] W. Stanley Wilson, "Oceanography From Satellites?" *(Oceanus,* XXIV, 3.

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.**

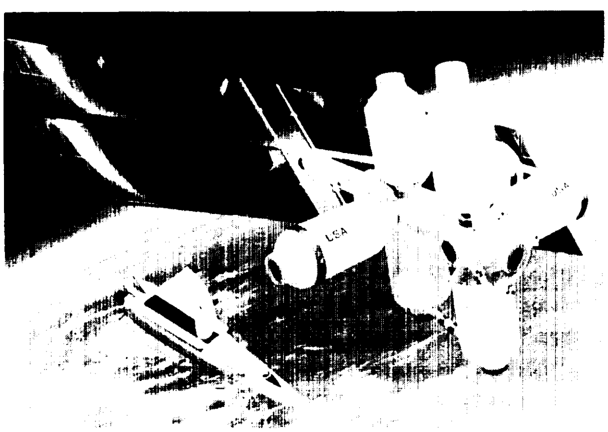

Photo credit: National Aeronautics and Space Administration

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.**

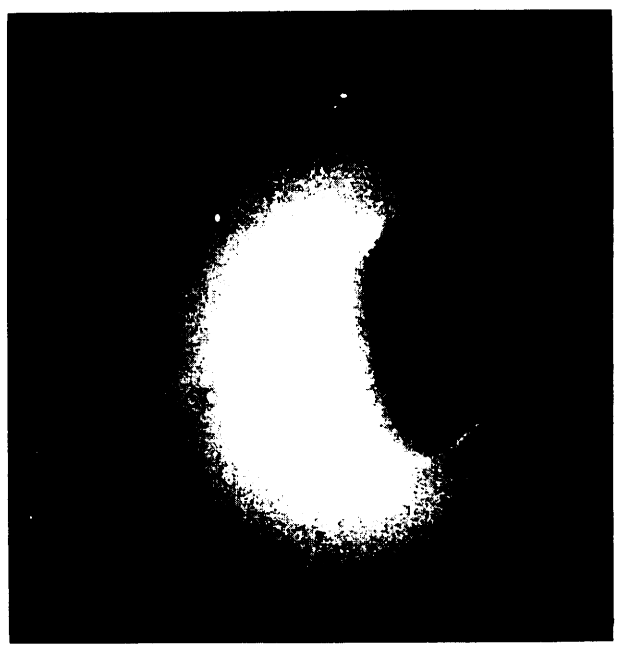

Photo credit: National Aeronautics and Space Administration

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

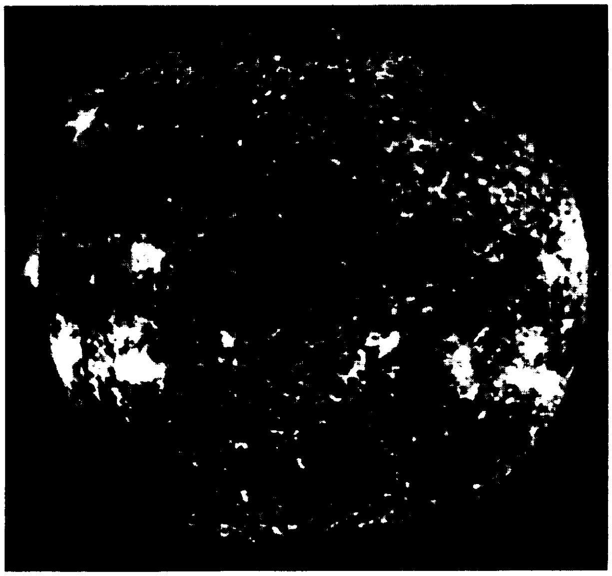

Photo credit: National Aeronautics and Space Administration

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

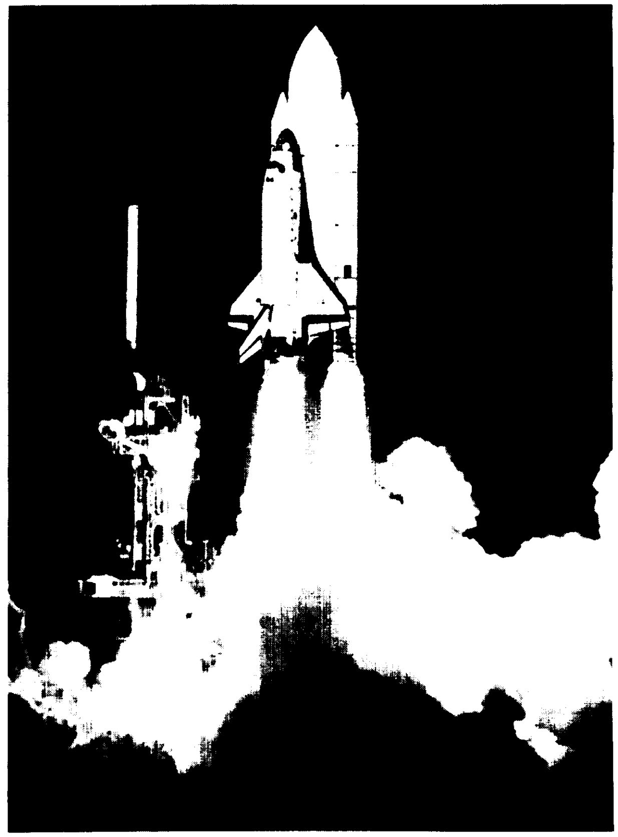

Photo credit: National Aeronautics and Space Administration

Launch of the space shuttle Columbia

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.)

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

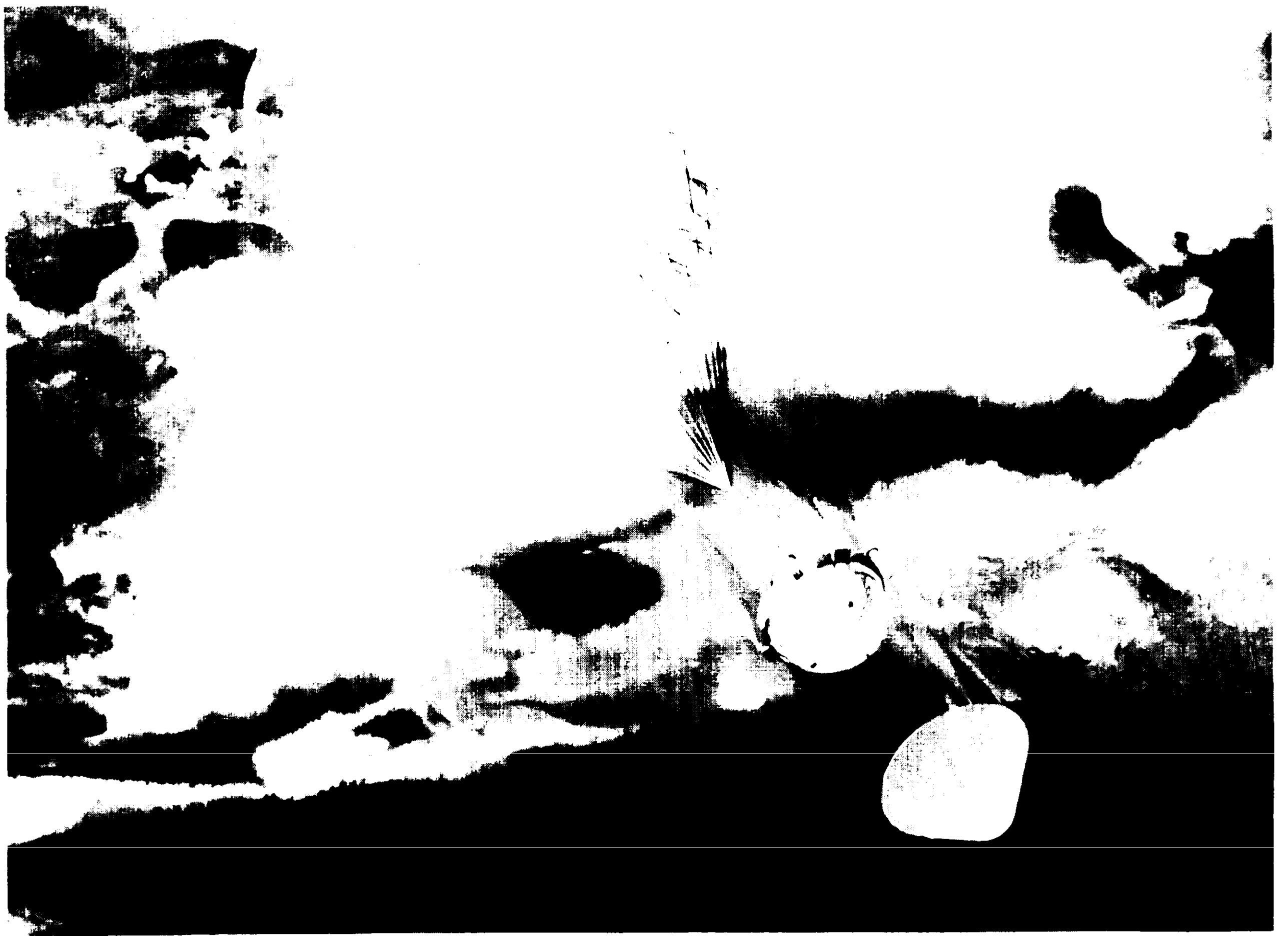

Photo credit: National Aeronaut/es and Space Administration

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

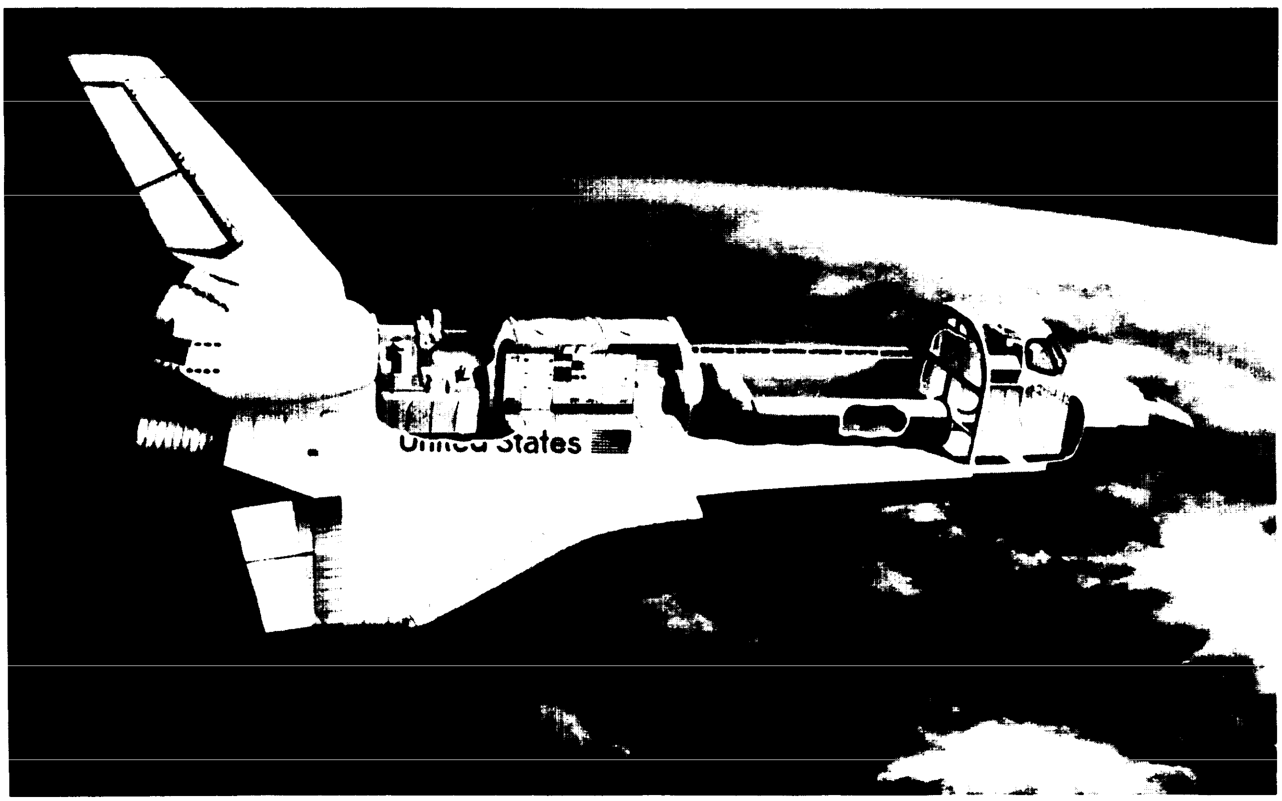
International cooperation promises scientific, cultural, economic, and social benefits to all participants. Over the long term, the prospects for international cooperation in space science ventures look very good. In general, it seems clear that missions that would be too expensive for the United States 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.

Chapter 4

The Importance of Doing Space Science

INTRODUCTION

Over the past 300 years, accelerating advances in scientific theory and practice have aided man in remaking the world. As scientific descriptions of our surroundings become more detailed, the practical consequences of applying scientific results and techniques to the problems of everyday life become more far-reaching. As the effects of science move out of the theoretician's study and the experimenter's laboratory, it is important to reflect on the reasons for undertaking science at all. It is not in the province of this report to justify the national effort in science, but it is *never*theless appropriate to discuss the importance of the space science program as a component of all Federal expenditures. Indeed, much of the rationale for doing space science is a corollary for doing science in general.

Space science is an undertaking that satisfies the visionary and exploratory needs of the human race. "In the future, as in the past, our freedom, independence and national well-being will be tied to new achievements, new discoveries and pushing back frontiers."[1] It is a cultural as well as a scientific activity that seeks to understand the Earth's place in the solar system, the solar system's place in the Milky Way Galaxy, and our Galaxy's place in the Universe. In assisting man to gain a better understanding of his place in his surroundings, space science also explores the fine structure of the universe in the form of samples, either examined in situ or returned for study on Earth.

At the bottom of the Earth's atmosphere, our ability to sense the universe is restricted to the visible and radio portions of the electromagnetic spectrum, but our extended ability to observe from above the atmosphere by means of instruments aboard spacecraft has widened our scientific vista enormously, and has permitted observations to be made of celestial objects that could not have been made in any other way. We are now truly viewing the universe through a set of multispectral eyes.

[1]President Ronald Reagan, "State of Space" speech, July 4, 1982.

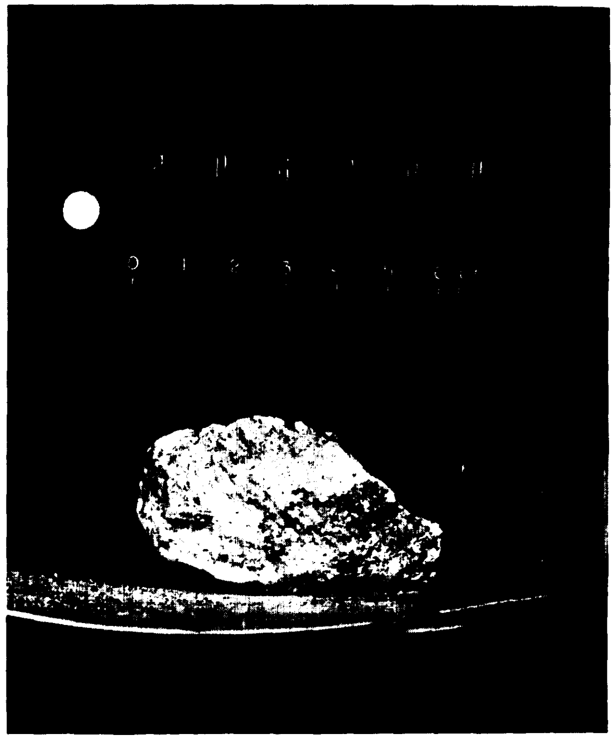
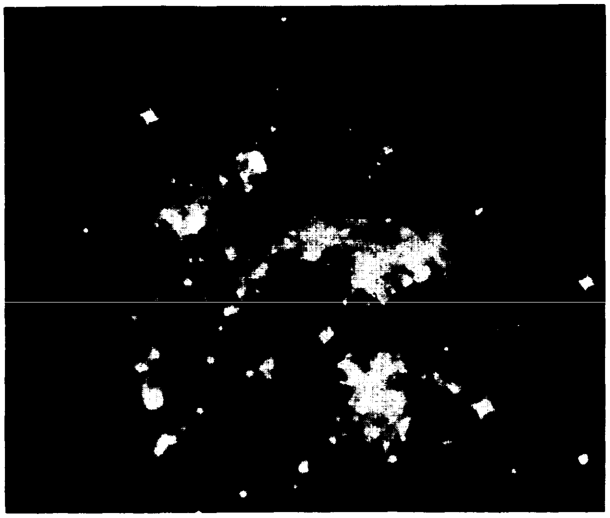


Photo credit: National Aeronautics and Space Administration

A lunar sample from Apollo 14 pictured in the Lunar Receiving Laboratory at the Manned Spaceflight Center, Houston, Tex.

Astronomy is only one of the fields of science of which instruments aboard spacecraft have *rev*olutionized our understanding. Others include the physics and chemistry of the Sun; energetic particles; the interplanetary medium; and the planetary sciences, whose purview properly includes Earth, as well as the Moon, the other planets and their satellites, and comets and asteroids. These fields have immediate importance for life on Earth. In fact, solar terrestrial physics and the planetary sciences as conducted from space have provided the basis for many of the important utilitarian applications for space technology--communications, navigation, meteorology, atmospheric physics.



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EXPENSE OF SPACE SCIENCE AND THE ROLE OF PUBLIC SUPPORT

Increasingly, there are those who believe that the pursuit of expensive science solely for the sake of understanding, particularly in the face of other urgent problems facing mankind, may not be defeasible. Because an expensive science project is a social product that depends on the common labor of many scientists, and because tax money must be allocated to support such an activity, the decision to pursue an expensive project and to allocate resources to it among competing alternatives necessarily entails political oversight.

More than ever before, a successful scientific career now depends on the support of public and private institutions. The days when individuals of independent means could make fundamental advances in science have mostly passed. The expense of pursuing fundamental research, particularly in the areas of so-called big science (e.g., high-energy physics, astronomy conducted with large telescopes, or space science), places these activities beyond the financial means of individuals. The costs of adequate scientific instrumentation are, for the most part, not borne by those who are to use them, but by Federal, State, and private laboratories—and ultimately by society as a whole. Thus, there is a kind of social contract between scientists and society, in which the pursuit of knowledge is exchanged for economic support.

Although the results of science have become part of our common heritage, the practice of science is becoming more and more a cooperative enterprise. Even though the individual genius will always be important in the process of scientific discovery, especially in purely theoretical work and in the practice of small-scale science, teams of scientists engaged in large-scale research projects are now quite common.

If society agrees to support science, the problem of just how that support should be apportioned remains complex. First, the very progress of science often leads to the need for more powerful instrumentation, especially in space science. As our understanding becomes more detailed, additional subdiscipline are founded, and each of them requires continued public support if it is to advance further. At the same time, other subdiscipline may be terminated, either because they reach a natural close or because they become too expensive to pursue further. In general, however, a situation where overall funding does not increase requires that some projects be delayed, stretched-out, or dropped, if others are to be supported.

Second, it is not possible to predict which scientific research programs will lead to improvement in the quality of human life. Applied science and engineering are undertaken with a view to producing relatively near-term benefits, but their productivity will soon become exhausted if a broad-scale program of basic research is not sustained.

It is important in this context to distinguish between further or continued research at a more or less constant level of funding and *expanded* research at a higher level of funding. In this report, OTA examines what value space science research has had in the past and is likely to have in the future, and what difficulties have arisen in maintaining a research effort at more or less constant overall funding levels; OTA has not considered the desirability of increasing funding levels for space science.

EARTH AND THE PLANETARY SCIENCES

The pursuit of planetary science has been of substantial importance to many of the geosciences, including geology, geochemistry, geophysics, geodesy, cartography, and photogrammetry. Exploration of other planets has returned results fundamental to understanding the evolution of Earth. These results derive, in large part, from the study of the crustal features and inferences about the interior of terrestrial (i.e., Earth-like) planetary bodies, including Mercury, Venus, Mars, and the satellites of Jupiter and Saturn.

The two principal drivers of planetary evolution are tectonism and vulcanism. Tectonism, the

Photo credit: National Aeronautics and Space Administration

Earth as a planet: the weather satellite GOES EAST shows two hurricanes bracketing Mexico, on Aug. 8, 1980

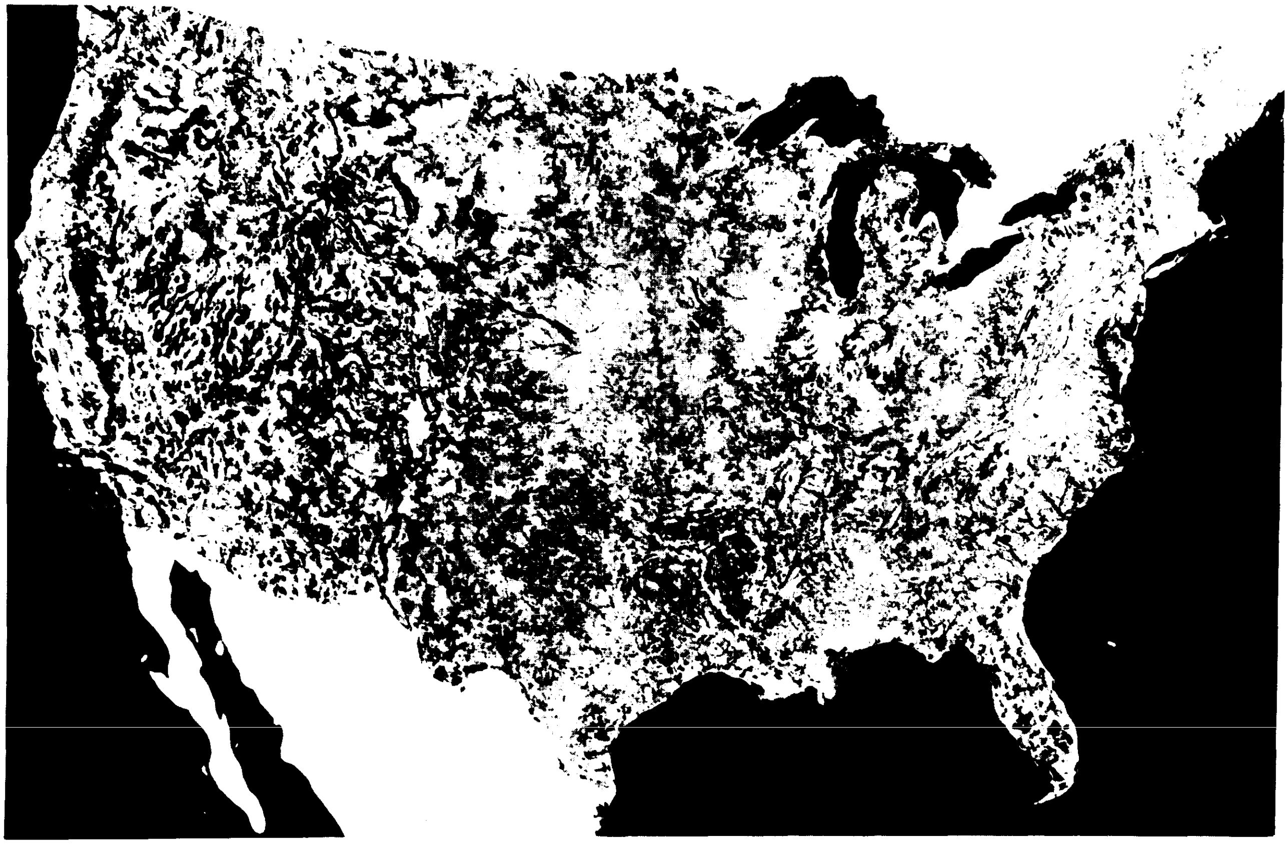

Photo credit: National Aeronautics and Space Administration

A giant photo map of the contiguous 48 States of the United States, the first ever assembled from satellite images. The images were produced by the Multispectral Scanner aboard Landsat I (formerly ERTS-1) between July 25 and Oct. 31, 1972

processes that determine how the crusts of planets deform and buckle, is important to understand for purposes of predicting and giving early warning of earthquakes; research into vulcanism, the processes whereby molten portions of a planet's interior emerge onto its surface (either on the seabed or on land, in the case of Earth), is a subject of practical importance because further understanding may eventually lead to prediction of volcanic eruptions.

Both earthquakes and volcanic eruptions remain major hazards in many parts of the world. The thorough devastation resulting from the recent eruption of Mt. St. Helen's in Washington State and the great loss of life in recent earthquakes in Italy and Central America attest to the power of these processes and their consequences for human life. Planetary science is providing revolutionary insight into these processes. The hyperactive vulcanism on the Jovian satellite Io, for example, follows an entirely different pattern from that of the Earth; study of these differences may be the key to understanding how volcanic processes work.

Planetary science, by furthering our understanding of the processes whereby mineral deposits are formed, may provide unexpected assistance in evaluating, seeking, and discovering these resources on Earth. Extensive research on Earth has revealed that mineral deposits are unevenly distributed; a fundamental problem in plate tectonics—the theory of how the continental land masses slowly move over the Earth's surface—is to explain the peculiar distribution of these deposits. Many of them are very ancient, formed when Earth was more like the Moon, Mars, or Venus;

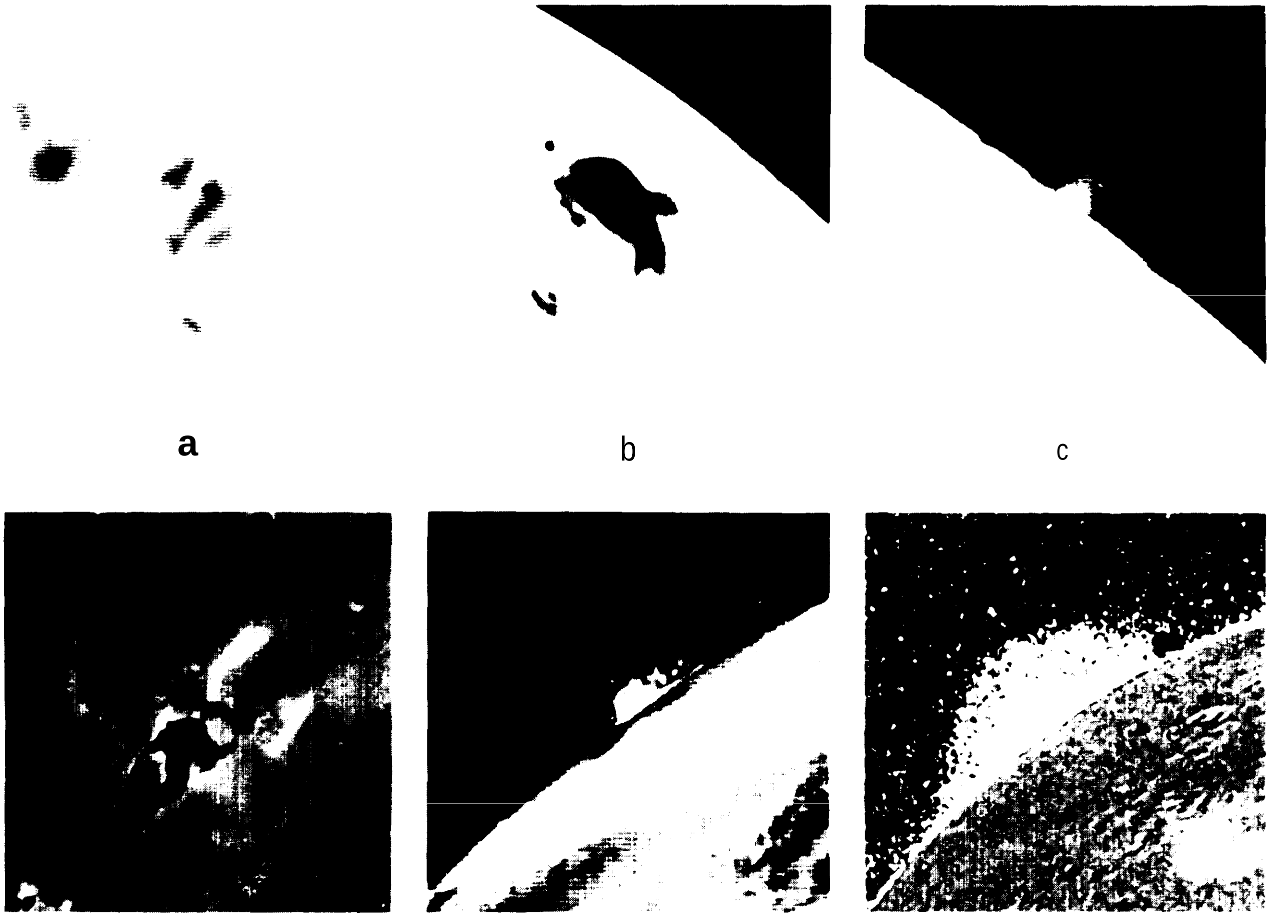

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Photo credit: National Aeronautics and Space Administration

Successive images of volcanic plumes on 10, taken from the Voyager I spacecraft

some of these are no longer being formed; others are still being produced, by a combination of crustal movements and volcanic activity. Comparative studies of other terrestrial bodies are assisting in the resolution of these issues.

In many respects, Venus and Earth are twins, but they have taken radically different evolutionary paths. Scientists now think that Earth owes its particular course of development to the early formation of life. Life forms in the early ocean are thought to have pulled carbon dioxide out of the atmosphere and laid it down in limestone. Decreasing the level of carbon dioxide prevented the runaway greenhouse effect that characterizes the atmosphere of Venus, and this in turn preserved Earth's oceans. Recent evidence from Venus indicates that it once had oceans, too. Presumably, however, life did not appear at all there, or it did not become sufficiently widespread to remove much carbon dioxide.

The oceans are not a mere secondary feature of the Earth's surface; they permit continued evolution of its crust. Ocean water cools molten basalt emerging from the midocean ridges, thus making the basalt dense enough so that the continental plates can "float" above it. Without this oceanic cooling of basalt, the continents would freeze in place, as they have on Venus. In this view, sea floor spreading and plate tectonic motion, made possible by the presence of the oceans, provide the present dynamic control of Earth's evolution. Thus, the presence of life and the presence of the oceans make Earth unique, at least in our star system. Life and the oceans each owes their preservation to the other, and the oceans control the way Earth's crust forms and therefore, indirectly, the course of evolution of living things. Without a study of Venus—an opposite case, where some particular differences have made all the difference—it is unlikely that our understanding of Earth's evolution would have progressed as far as it has.

Planetary science, therefore, has enlarged and deepened our understanding of the fundamental processes molding the Earth. In addition, it has given evidence both of what kinds of results might be expected if the balance of Earth's system were disturbed, and of how a relatively small change could drive the whole system into a dry, dead end.

FORMATION OF THE OZONE LAYER

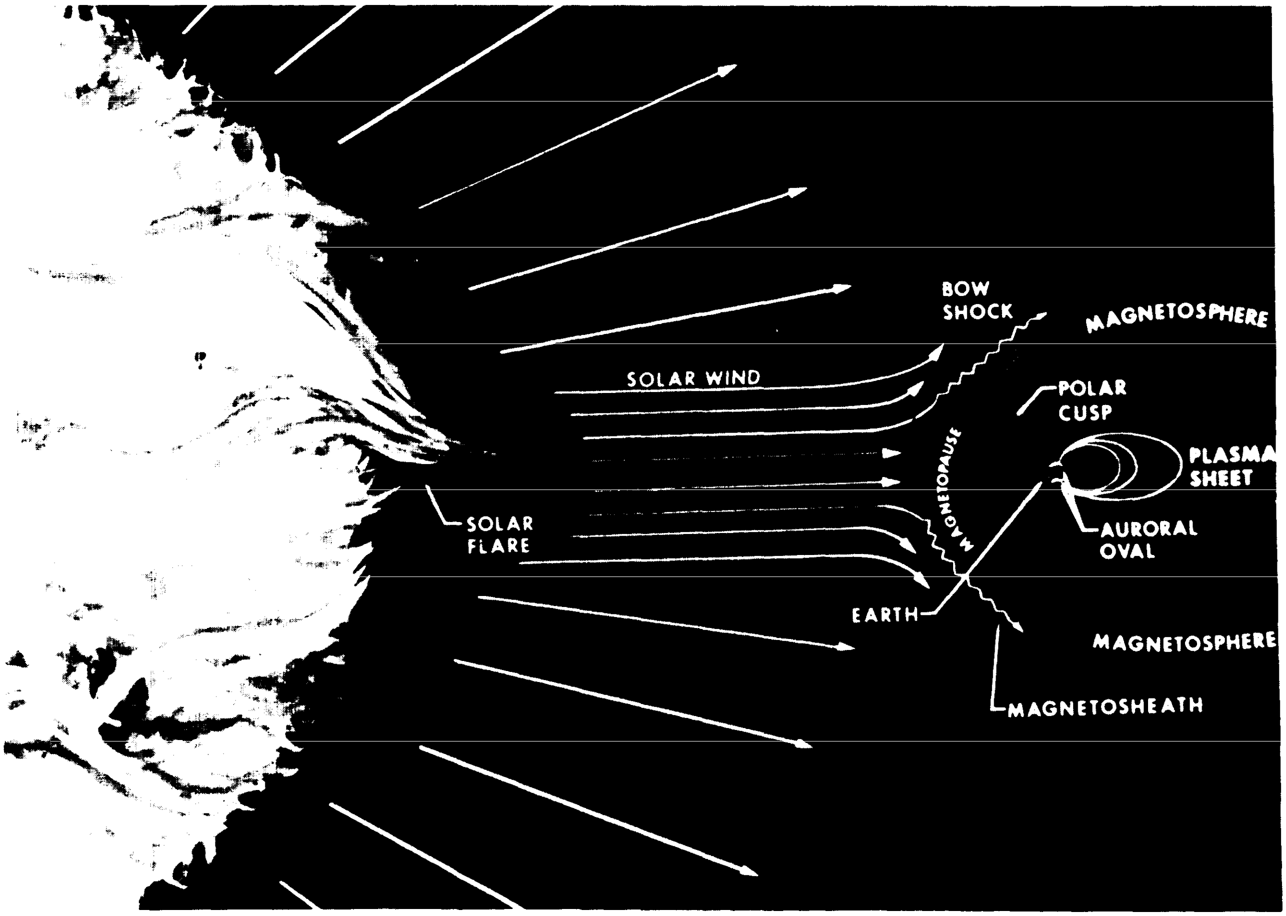
The monitoring of ozone is one example of a practical activity growing out of space science. Ozone is a small, but important constituent of the Earth's atmosphere: too little of it in the stratosphere allows dangerous levels of ultraviolet radiation to reach the Earth's surface; too much of it near the Earth's surface has more immediate deleterious effects on human health. The level of ozone in the stratosphere can be altered both through increased technological activity, which causes relatively slow changes, and through alterations in the level of solar ultraviolet radiation, which causes more rapid fluctuations. Whereas increased technological activity tends to add compounds to the atmosphere which decrease the stratospheric ozone, solar ultraviolet radiation produces additional ozone. The mechanisms by which stratospheric ozone is formed and maintained are still not thoroughly understood, and space research systems are being used not only to monitor ozone but to measure related parameters which are critical to increasing our knowledge.

SOLAR PARTICLE EMISSIONS

Space research systems are also useful for measuring the level at which high-energy protons, emitted from solar flares, bombard the Earth's atmosphere, particularly at high latitudes. Instruments aboard spacecraft can detect the emission of these particles early enough so that the routes of aircraft flying over the polar caps may be changed or manned satellites may take appropriate precautions. To perform well, these warning systems must be sufficiently accurate to differentiate between flares that have a major effect and those that have only minor effects. The flare signatures that will provide this separation are not well defined, and only further basic research from space vehicles will provide the answers.

WEATHER AND CLIMATE

Another example of the practical effects of space science is the observation of the Earth's climate and weather. The Earth's weather and secular or cyclic changes in its climatic conditions have a significant impact on commercial activity. Accurate weather predictions are very useful for a variety of purposes, and accurate climatic forecasts could be of great strategic value—in predicting energy consumption requirements, for exampie. Accurate predictions depend on a substantial monitoring system in which satellites play a major role. If these satellites are to succeed in their missions, they must monitor the most predictive sets of parameters, and their downward-looking



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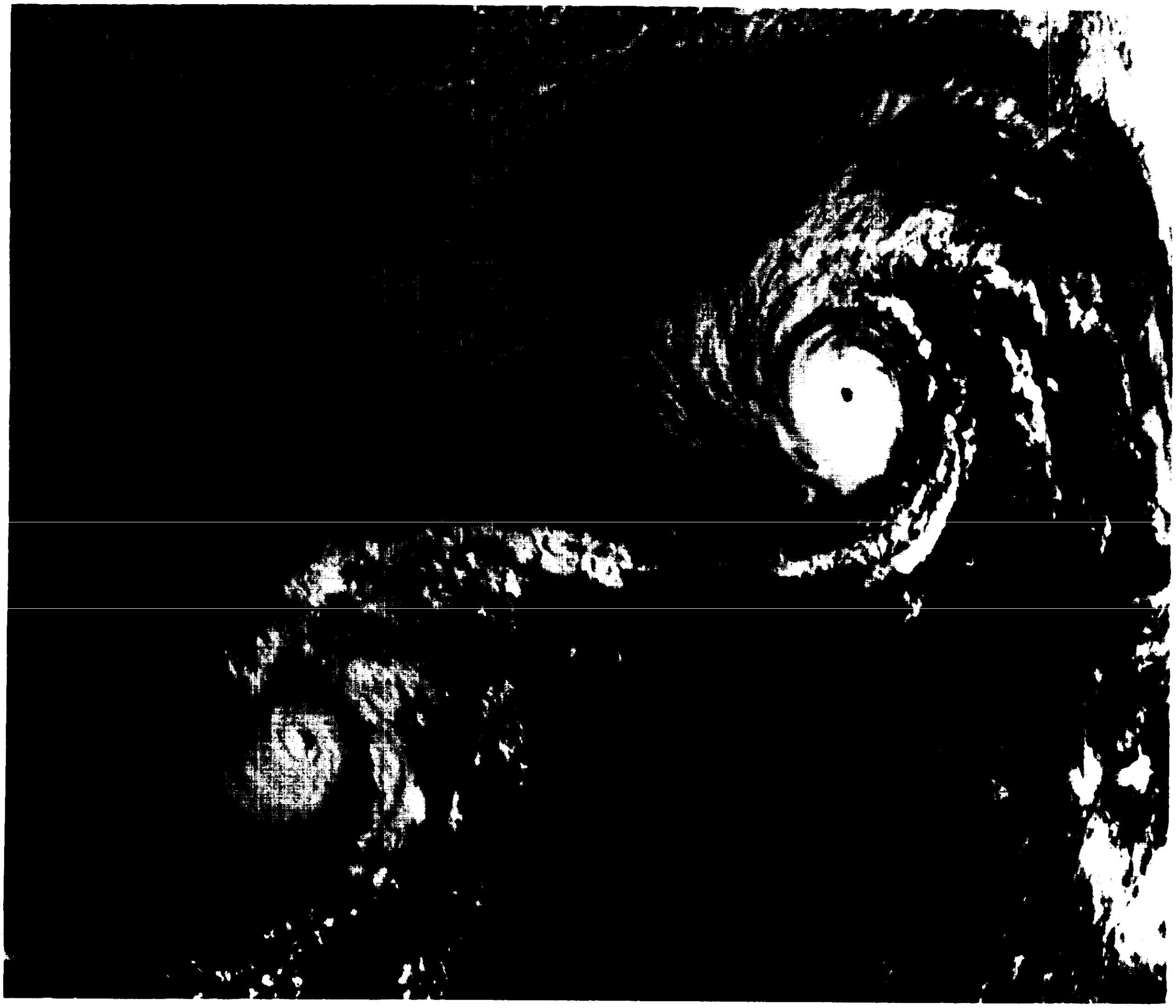
systems must measure cloud cover, infrared and visible radiation, temperature, and the changes in these parameters. Because the relative reliability of these parameters is still under investigation, there is need for a continuing basic research partnership in which observations made from space and studies made from the ground are correlated, the parameters most critical over long time sequences are identified, and the largest available computers are employed—if there is a determination that the national interest is well served by more accurate weather predictions and climate monitoring.

Prediction of the Earth's weather presents an extremely complicated problem. Weather prediction is still in its infancy, and progress to date has been dependent on an increasingly sophisticated sensing system, combined with elaborate computer analysis. Space scientists are gaining further insight into how planetary atmospheres originate, circulate and evolve, principally because of recent space research on the atmospheres of other planets (Venus, Mars, Jupiter, and Saturn). This insight promises to remove some of the uncertainty in our knowledge of the circulation patterns of the Earth's atmosphere.



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PROCESS INTERCHANGE IN THE EARTH'S ATMOSPHERE

Our life system on Earth is crucially dependent on a delicate balance and interchange of processes that occur at the interface of the Earth, the oceans, and the atmosphere. The presence of man and, in particular, the population explosion, compounded with the growth of industry, have begun to affect that balance in ways that are not yet understood. One primary means of monitoring and understanding these processes is through satellite remote sensing, an activity that is only a decade old.

Sampling the troposphere, the stratosphere, and much of the ionosphere can be done by Earth-based methods; the magnetosphere can be investigated only by space vehicles. The parameters of the "middle atmosphere, " the region from 30 or 40 to 100 km above the Earth's surface, are still uncertain, especially the composition and the electric fields of the region. If the effects that solar variations and the Van Allen Belt will have on spacecraft operating near the Earth are to be understood, the magnetosphere must be fully mapped throughout the solar cycle.

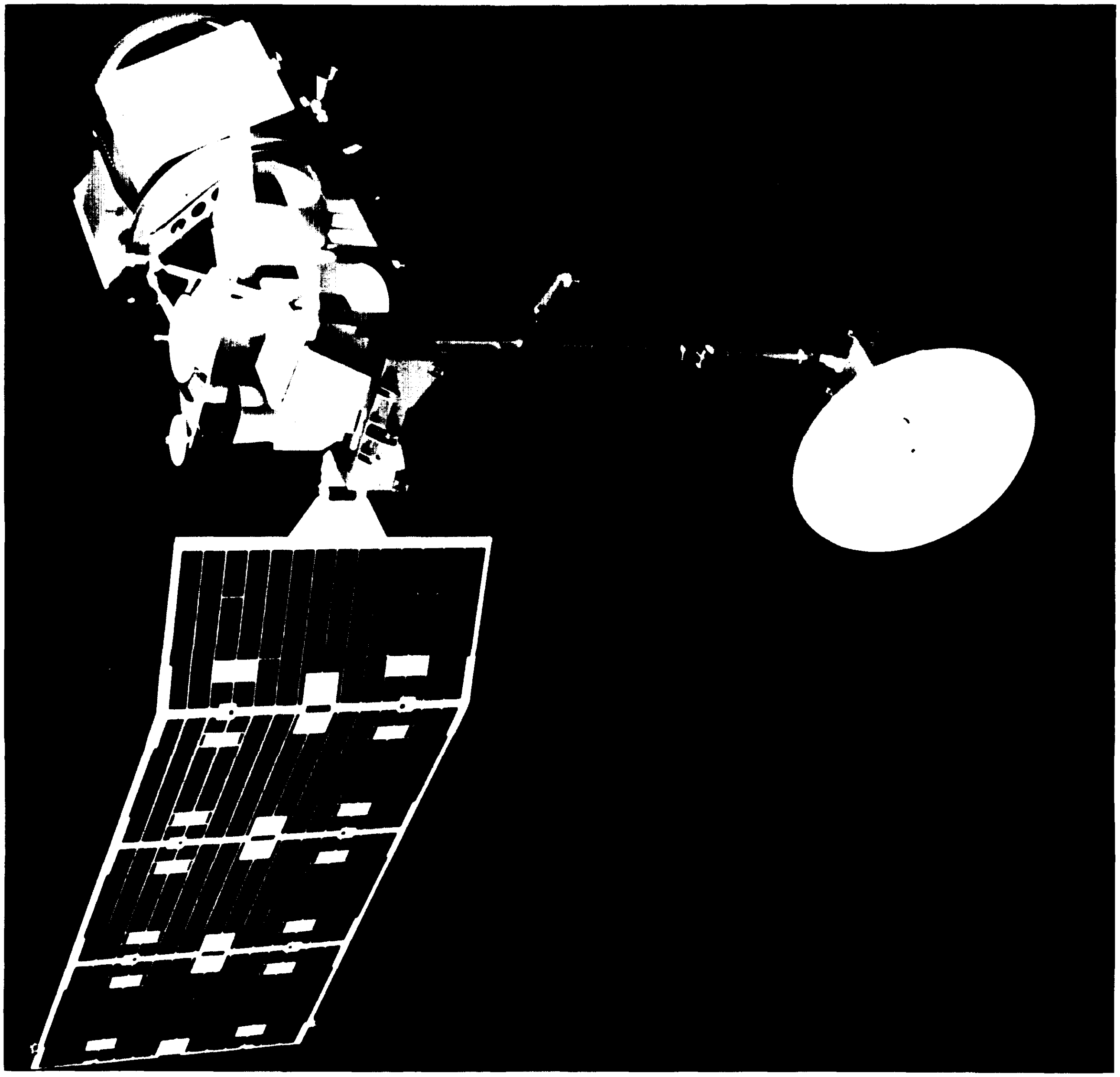


Photo credit: National Aeronautics and Space Administration

Landsat D—launched July 1982

EFFECTS OF SOLAR VARIATIONS ON COMMUNICATIONS

The Sun is the primary driving power underlying most processes on Earth, and its presence is an absolute necessity for our own existence. But the Sun is variable, and most of the variations that can occur have important effects on communications systems. Many of the important variations, however, can be seen only in regions of the spectrum accessible from above the Earth's atmosphere.

It had been known for decades that geomagnetic storms on the Earth seriously affect communications on the Earth, particularly at high latitudes. Geomagnetic storms result from streams of hot ionized gas that originate in solar storms, then are shot into interplanetary space, and finally impinge on the Earth's magnetosphere and disrupt communications. It is now known that the geomagnetic storms coincide with the arrival of streams of matter that travel much faster than the normal solar wind, and that these high-speed streams originate from regions on the Sun that do not emit X-rays, regions where there are "holes" in the low-energy X-ray emission, seen on photographs of the Sun taken in X-ray light. Thus, the presence of solar coronal holes, dark regions on an X-ray photograph of the solar disk, correlates significantly with geomagnetic storm activity on Earth.

Thus, there is a direct connection between the reliability of radio communications at high altitudes on Earth and our ability to monitor and study the Sun by X-ray satellites, by means of techniques not possible from the ground. The manifestations of this connection are not yet highly predictable, and the benefits of making them so will accrue only if basic research is continued.

SATELLITE RELIABILITY

Spacecraft operate in an environment that is largely foreign to us and virtually impossible to duplicate for study on the ground. In addition to the primary hazard of energetic particles in Earth's radiation belts, they are subject to differential charging, to emissions of electrons, protons, and other energetic particles from the Sun, to cosmic rays, and to high-energy solar and stellar ultraviolet, and X-ray and gamma ray radiation, any of which, if encountered in sufficient strength, can degrade the performance of the spacecraft. Damage may occur through irradiation of its detectors and electronics, electrostatic discharges, and the physical effects of collisions, including particle sputtering on mirror surfaces. As longer-lived satellites are orbited, degradation in performance because of environmental factors will be a more likely source of failure than will exhaustion of onboard energy sources.

The environment in which the now more than 4,000 manmade objects are orbiting the Earth is still not understood in detail. We do not yet know the time, frequency, or amplitude ranges over which variations in particulate bombardment and radiation take place. In the beginning of June 1980, for example, an unexpected increase in the flux of high-energy electrons at synchronous altitude particularly affected the performance of geostationary satellites. This result showed that the near-Earth space environment still held surprises and that instruments more immune to the effects of radiation had to be developed.

The requirement of the Department of Defense that the electronic components of its spacecraft be protected both against the natural radiation environment and, especially, against radiation from the explosion of nuclear devices has been the primary driver in the development of advanced "hardening" techniques. (The deep space probe Galileo, which must be able to withstand an environment of very high radiation around Jupiter, will be making use of some of these developmerits.) After these techniques are perfected, further study of the radiation environment around the Earth, particularly of the triggering mechanisms by which particles are dumped from the Earth's geomagnetic tail into the Earth's atmosphere, will be needed, if the national interest requires more assured satellite operations.

Common to civilian and military applications is the requirement to minimize payload weight while maximizing payload performance. This requirement has been the principal driving force behind the miniaturization of components, of which the development of tiny electronic circuits on silicon chips has been a major technological breakthrough. When these chips first found application on satellites, they were relatively large and not too densely packed. These features, combined with space hardening techniques, made the chips relatively reliable in the spacecraft environment. However, the technological state of the art in making chips has now progressed to the point where the chips are smaller and more densely packed, and have, size for size, orders of magnitude more capability than the previous generation of chips they will replace.

It is known that the new, high-density chips will be more susceptible to damage from radiation bombardment than were their predecessors, but it is not known how much more susceptible they will be. Here *is* another instance in which, if the answer is to be achieved, basic and applied research will both be needed: basic research to investigate the radiation environment, and applied research to investigate the effects of that environment on the new series of chips in order to predict how long they will last under various environmental conditions and under various degrees of radiation hardening.

COMMERCIAL IMPORTANCE OF NEAR= EARTH SPACE

The commercial importance of the space environment near the Earth has not yet been fully evaluated because, apart from the communications industry, there has been little involvement of the private sector. In the future, the investment of the private sector in space activities will almost certainly increase, particularly in satellite communications, remote sensing, and materials processing.

Materials processing in space (MPS) may be singled out as a new and interesting area for commercially oriented space research. In order to ease the way for industry to exploit the possibilities of MPS, NASA has developed the Joint Endeavor Agreement, in which the agency and industry share in the costs and the risks of the project: NASA provides technical advice and assumes the costs of the launch vehicle, including flight time, and industry provides the development funds.

One promising example of this Government-industry symbiosis is in drug manufacturing, where McDonnell Douglas Astronautics and the Ortho Pharmaceuticals Corp. of Johnson& Johnson are making a substantial investment in order to determine whether certain drugs can be manufactured in space more profitably than on the ground. Studies have shown that, by means of a process known as electrophoresis (a technique whereby a solution flows through an electric field in which molecules of different charges are separated from each other as a result of their migration in different directions at different speeds), it should be possible for cells to be separated from proteins about 100 to 400 times more quickly and with five times the product purity that can be obtained from the ground.

There are potential applications in the manufacture of interferon (a treatment for cancer), beta cells (a possible single-injection cure for diabetes), epidermal growth factor products (for treating burn patients), growth hormone products (for juvenile bone growth stimulation and the healing of ulcers), antitryspin products (for limiting the progress of emphysema), and antihemophilic products (for eliminating immunological reactions for hemophilia). In all these cases, there is promise that commercially viable quantities of these drugs can be made in the zero-gravity environment of space.

Materials processing is only one example of the possibilities of industrial use of near-Earth space. If these possibilities are to be exploited, scientists will require a better knowledge of the space parameters that may modify processes whose ground-based instances are well understood. If these developments are to be successful, continued interaction between pure space science and applied space science will be necessary.

Appendixes

Appendix A
Trends in the Space Science Budget

It is instructive to view the total NASA budget in the context of recommendations that were made within the administration in 1969, the year of the first Apollo landings on the Moon. Recognizing that the Nation needed to take a close look at the space program in the post-Apollo era, the President in February 1969 formed a Space Task Group (STG) to study the future course of the U.S. space program. STG was chaired by the Vice President, with membership from the Department of Defense (DOD), the National Aeronautics and Space Administration (NASA), and the office of President's Science Advisor.

The report, entitled "The Post-Apollo Space Program: Directions for the Future," was received in September 1969. It presented a comprehensive plan which presented three possible program levels to achieve five program objectives: 1) increased emphasis on activities that have service to man (an expanded space applications program); 2) an enhanced defense posture for the Nation; 3) continuation of a strong program of space research to increase man's knowledge of the universe; 4) development of new systems and technology for space operations, emphasizing certain critical factors as: a) commonality, b) reusability, and c) economy, through the development of a new space transportation capability and space station modules; and 5) promotion of programs that provide broad international participation and cooperation.

The three program levels all contained a space shuttle, a 12-man space station, a 100-man space base, and lunar orbiting and surface stations. Two of the three options also included a manned mission to Mars. The time frames of these options differed, depending on the annual budgets projected for the future, which varied from $6 billion to $10 billion per year (1969 dollars). Figure A-1 shows a comparison of those NASA funding options (I, II and III, with a low-level bound having no manned flight). The upper curve marked "maximum pace bound" presents the funding required for a program limited not by funds, but by technology; the low-level program was constructed with an increased unmanned science and applications effort without a manned flight program. Figure A-1 also shows in 1969 dollars what level of funding for NASA actually was achieved.

In the post-Apollo era there was opposition to the levels of the space program proposed in the STG report which was expressed by critics in Congress, in the media, and in the American public generally and which led to a stretched-out time schedule shown in figure A-1. The actual program included the development of the space shuttle, but the rest of the recommendations of STG were not implemented. At the moment, no effort to develop a space station has been approved by the administration, despite the fact that the Soviet Union has been very active in developing a strong capability in this field for a number of years and will soon be able to sustain a permanent presence of man in orbit, either around the Earth or around the Moon.

Figures A-2 and A-3 show the space science budget of the NASA Office of Space Science and Applications (OSSA) from fiscal year 1964 to fiscal year 1983, expressed in millions of (1983) dollars, corrected for inflation.[1]

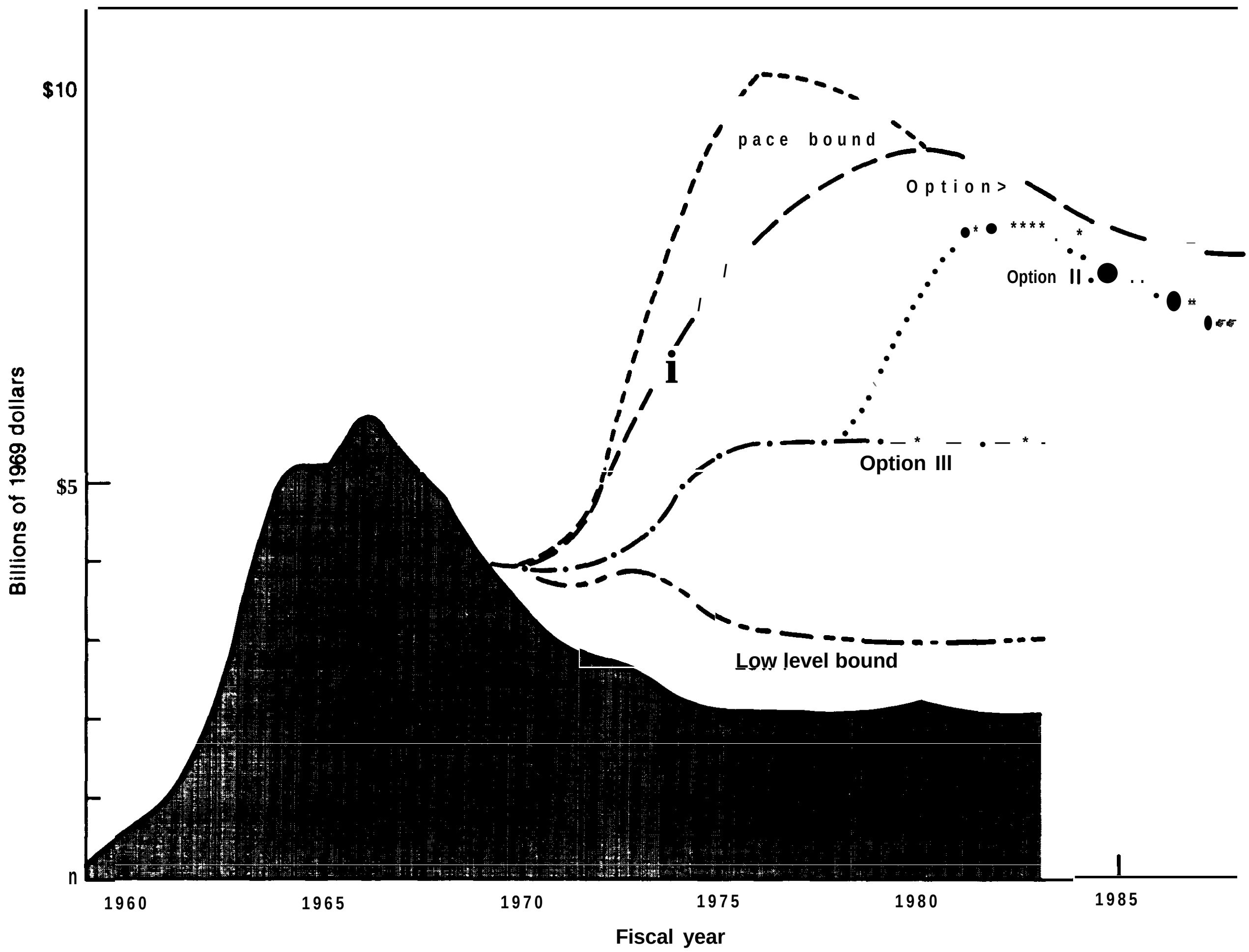
They are expressed in numerical form in tables A-1 and A-2. Three major components of the space science budget are shown: 1) the planetary sciences program, 2) the physics and astronomy program, and 3) the life sciences program. The factors by which actual budget figures have been converted to constant dollar figures are given in the middle (escalation) columns in table A-1. Table A-2 shows the budgets for individual flight programs during the same time interval, expressed in current-year dollars.

Several features of figures A-2 and A-3 deserve mention. First, in terms of purchasing power, the *total* budget for space science from the mid-1960's to the present time has fallen from about $1,450 million to about $650 million, or to approximately 45 percent of its former size. Most of this decline took place precipitously between 1966 and 1969. The budget increased again, but by only 50 percent of its decline, from 1972 to 1975, but fell again to its current value of about $650 million by 1977.

Reference to the other components of figure A-2 shows the trends of those components during the same time interval. The physics and astronomy program decreased by approximately 5o percent in the decade from 1964 to 1974, but has regained about two-thirds of its former value since 1974, principally because of increased funding for Space Telescope. The planetary science program was cut to one-third of its 1965 level by 1969, but grew to slightly more than its 1965 level by 1972-74, principally because of funding support for the Mars Viking program and the beginning of the

*The budget figures for the NASA Office of Space Science and Applications were supplied to OTA by 1. Duke Stanford (NASA/OSSA).

Figure A-l .—Annual NASA Expenditure and Options

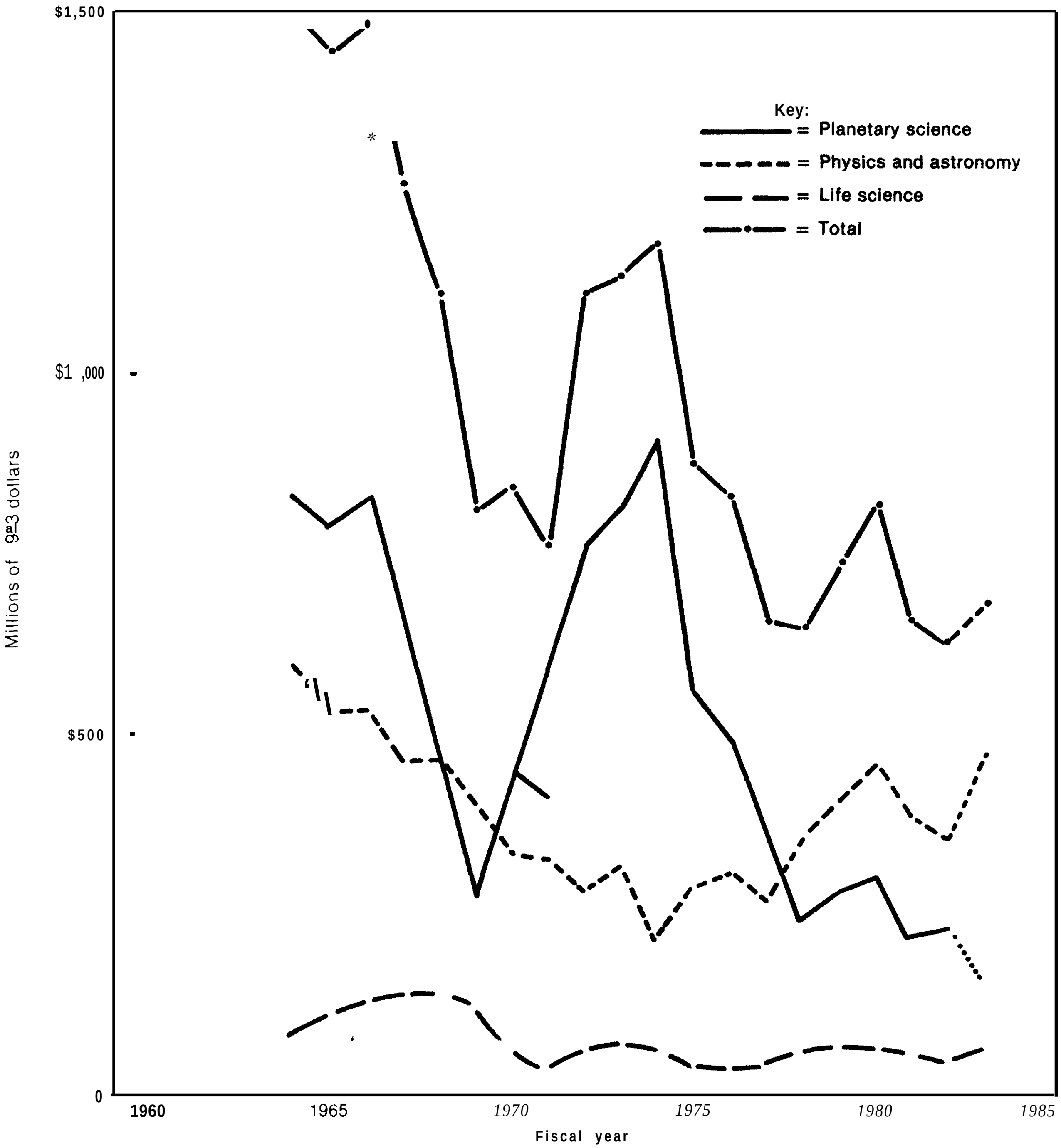


SOURCE: Space Task Group Report to the President, "The Post-Apollo Space Program: Directions for the Future," September 1969.

Voyager programs. However, as these programs tapered off, starting in 1974 and 1976, respectively, the budget for planetary sciences underwent a sharp decrease from its peak of $900 million (1983 dollars) to its current level of about $200 million, a drop to 22 percent of its value in 1974. The life sciences component of the NASA space science budget has remained relatively constant for the past decade.

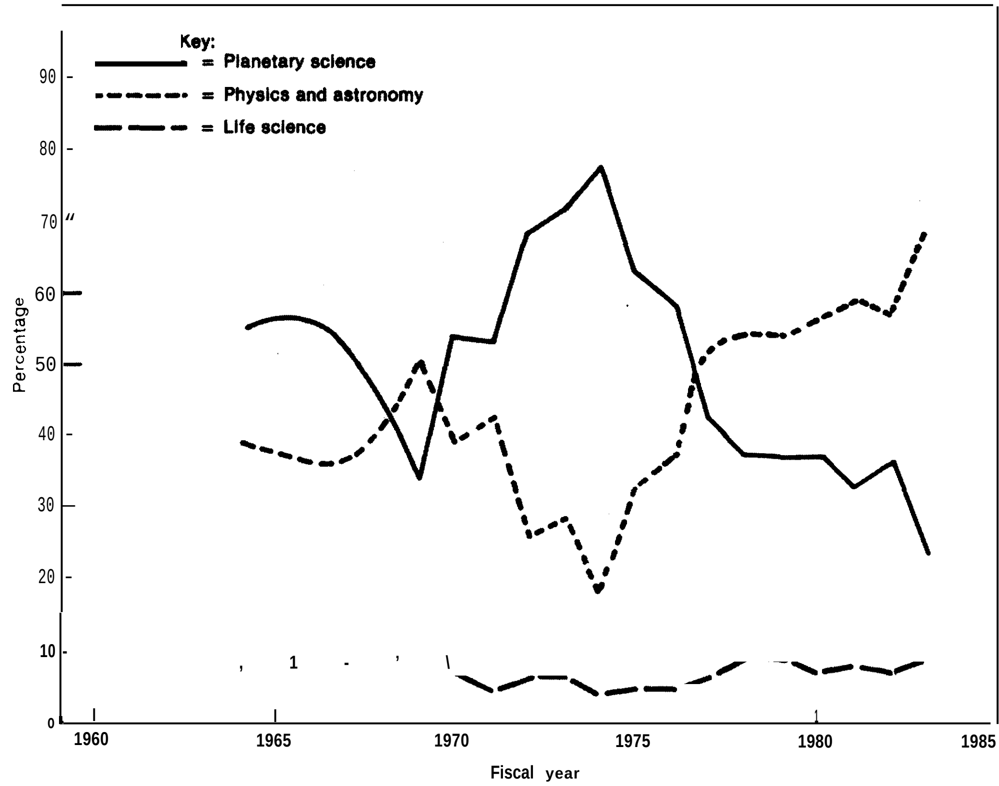
Reference to figure A-3 shows that the life sciences have typically taken about 510 percent of the space sciences budget for the past two decades. The budget for planetary science exceeded the budget for physics and astronomy from 1964 to 1968 and from 1970 to 1976, whereas the budget for physics and astronomy exceeded the budget for planetary science for 1969 and for the period from 1977 to the present. The budget for planetary science has been significantly lower in 1981-1983. It was noted that the fiscal year 1983 budget is still under discussion at the time of this writing (August 1982).

Figure A-2.— Budgets for NASA's Space Science Program



SOURCE Off Ice of Technology Assessment from NASA's supplied data

Figure A-3.— Breakdown of NASA's Space Science Budget



SOURCE: National Aeronautics and Space Administration

Table A-1 .—Office of Space Science Funding Summary (dollars in millions)

	Actuals				Escalation[a]		In constant 1983 dollars			
Actual fiscal year	Physics and astronomy	Planetary	Life science	Total	Rate	Factor	Physics and astronomy	Planetary	Life science	Total
1964	148.6	205.8	21.5	375.9	3.7	3.983	591.8	819.7	85.6	1,497.1
1965	139.1	206.0	28.5	373.6	3.0	3.867	537.8	796.6	110.2	1,444.6
1966	142.8	221.4	34.4	398.6	4.1	3.714	530.3	822.2	127.7	1,480.2
1967	129,8	184.2	42.0	356.0	4.9	3.541	459.6	652.2	148,7	1,260.5
1968	139.5	147.5	41.8	328.8	5.4	3.359	468.5	495.4	140,4	1,104.3
1969	128.9	87.9	37.9	254.7	5.7	3.178	409.6	279.3	120.4	809.3
1970	112.8	150.9	19.7	283.4	6.9	2.973	335.3	448,6	58.5	842.4
1971	116.0	144.9	12.9	273.8	6.3	2.797	324.4	405.2	36.1	765.7
1972	110.1	285.5	22.8	418.4	5.7	2.645	291.3	755.4	60.3	1,107.0
1973	126.2	325.9	26.6	478.7	5.7	2.503	315.8	815.7	66.5	1,135.0
1974	94.0	387.7	22.8	504.5	7.2	2.335	219.4	905.2	53.2	1,177.8
1975	136.3	261.2	19.8	417.3	10.8	2.108	287.3	550.6	41.7	879.6
1976	159.3	254.2	20.6	434.1	9.0	1.934	308.0	491.6	39.8	839.4
1977	154.7	191.9	22.1	368.7	8.5	1.782	275.7	341.9	39.3	656.9
1978	212.6	147.2	33.3	393.1	7.8	1.653	351.4	243.3	55.0	649.7
1979	270.0	182.4	40.1	492.5	9.5	1.510	407,7	275.4	60.5	743.6
1980	336.8	219.9	43.8	600.5	10,7	1.364	459.3	299.9	59.7	818.9
1981	323.7	175.6	42.2	541.5	12.0	1.218	394.2	213.8	51.4	659.4
1982	323.5	205.0	39.5	568.0	10.7	1.100	355.8	225.5	43.4	624.7
1983 [b]	(471.7)	(154.6)	(55.7)	(682.0)			(471.7)	(154.6)	(55.7)	(682.0)

[a]Based On NASA R&D Index dated September 1981
[b]Proposed.

SOURCE National Aeronautics and Space Administration.

Table A-2.—Space Science Budget From Congressional Submission

	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983
Flight program	***$297.8***	***$283.0***	***$299.3***	***$249.5***	***$208.8***	***$146.0***	***$192.6***	***$184.5***	***$330.2***	***$368.7***	***$381.6***	***$277.7***	***$275.6***	***$196.0***	***$191.6***	***$279.6***	***$371.4***	***$292.0***	***$341.8***	***$425.3***
OSO's	20.0	16.6	19.1	10.1	11.3	13.8	14.5	16.9	18.6	20.4	12.8	4.3	3.6	1.0	1.3	—	—	—	—	—
OAO's	35.6	32.6	22.3	27.7	44.8	36.4	33.3	23.2	13.4	5.7	2.3	2.2	2.3	2.6	2.0	—	—	—	—	—
OGO's	42.9	30.3	28.2	24.8	20.1	13.1	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Explorers	15.5	21.6	18.6	18.2	17.5	19.4	18.3	25.9	22.6	33.2	32.8	34.0	29.9	30.2	24.3	31.3	32.3	33.3	33.3	34.3
HEAO	—	—	—	—	—	—	—	—	13.4	21.8	4.8	42.9	59.2	39.4	19.8	10.6	2.1	—	—	—
SL P/L	—	—	—	—	—	—	—	—	—	—	—	3.3	2.1	8.4	37.6	50.6	57.2	40.1	54.0	105.4
SMM	—	—	—	—	—	—	—	—	—	—	—	—	—	21.3	29.6	16.7	3.1	—	—	—
S.T.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	36.0	79.2	112.7	119.3	121.5	137.5
S. Polar	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	12.5	47.9	28.0	5.0	21.0
GRO	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8.2	8.0	34.5
Ranger	30.3	11.0	1.0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Surveyor	70.0	81.8	104.6	79.9	33.0	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Lunar Orbit	20.0	49.5	58.1	26.0	9.5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Mariners	49.2	17.4	17.6	43.2	66.2	46.2	63.9	41.8	61.6	37.7	11.1	5.3	—	—	—	—	—	—	—	—
Voyager '73	—	7.2	17.1	12.7	0.4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Pioneer	13.6	15.0	12.7	6.9	6.0	4.7	22.6	41.7	15.2	11.6	—	—	—	—	—	—	—	—	—	—
Viking	—	—	—	—	—	12.4	40.0	35.0	176.2	232.2	290.4	89.0	39.5	—	—	—	—	—	—	—
Pioneer-Venus ...	—	—	—	—	—				—	—	—	26.9	56.6	42.8	20.0	—	—	—	—	—
Voyager '77	—	—	—	—	—	—	—	—	9.2	6.1	27.4	69.2	82.4	50.3	—	—	—	—	—	—
Galileo	—	—	—	—	—	—	—	—	—	—	—	—	—	—	21.0	78.7	116.1	63.1	120.0	92.6
Res. Base	***78.1***	***90.6***	***99.3***	***106.5***	***120.0***	***108.7***	***90.8***	***89.3***	***88.2***	***110.0***	***122.9***	***139.6***	***158.5***	***172.7***	***201.5***	***212.9***	***229.1***	***249.5***	***226.2***	***112.1***
MO&DA	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	84.8	95.9	100.7	88.3	112.1
R&A	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	128.1	133.2	148.8	137.9	144.6
Total OSS	***$375.9***	***$373.6***	***$398.6***	***$356.0***	***$328.8***	***$254.7***	***$283.4***	***$273.8***	***$418.4***	***$478.7***	***$504.5***	***$417.3***	***$434.1***	***$368.7***	***$393.1***	***$492.5***	***$600.5***	***$541.5***	***$568.0***	***682.0***

Flight programs	*Number of missions*
OSO's	8
OAO's	4
OGO's	6
Pioneers	15
HEAO's	3
Rangers	9
Surveyors	7
Mariners	10

SOURCE: National Aeronautics and Space Administration.

Appendix B

Successive Galileo Cost Estimates

Table B-1 .—Chart of Galiieo Cost Estimates (In 1982 dollars)

Date of cost estimate	Cost estimate	Launch date
1977 (for 1978 start)	$455 million	January 1982
January 1980	$650 million[a]	February 1984 (orbiter) March 1984 (probe)
February 1981	$695 million[b]	April 1985
February 1982	$865 million[c]	May 1985
July 1982	$(pending)[d]	May 1985

with three-stage IUS.
%With Centaur.
cwith two-stage IUS with kick stage.
dwith Centaur.

SOURCE: Figures provided by Klein Spencer, U.S. General Accounting Office.

Appendix C

Budget for Space Telescope

Table C-1.—The President's Fiscal Year 1983 Budget for Space Telescope (doliars in millions)

	Fiscal year 1978	1979	1980	1981	1982	1983	Budget to completion	Total
Space telescope development	$36.0	$79.2	$112.7	$119.3	$121.5	$137.5	$117-167	$725-775
Space telescope operations[a]	—	—	1.8	6.7	13.2	32,8	continues	

aFiscal year 1982 and prior funding to the space Telescope Science Institute is $5.6 milllon and fiscal year 1983 funding is $7.0 million. The estimated cost of the Space Telescope Science Institute facility, located at Johns Hopkins Universlty, Is $10 million.

SOURCE: Figures provided by Lynne Murphy, Congressional Liaison Office, National Aeronautic and Space Administration Headquarters.

Appendix D
Shuttle/ELV Price Comparison

The estimated price to place a 2,750 lb spacecraft into geosynchronous transfer orbit if launched in 1982 is as follows:

	Shuttle (in millions)	Delta (in millions)
Transportation	$ 8	$ 30
Upper stage	4-5	3-4
Total estimate*	$12-13	$33-34

The above shuttle price is derived using the shuttle introductory period price of $18 million (in fiscal year 1975 economics) for a dedicated shuttle launch during the 1982-85 timeframe. If the recently established $38 million price (in fiscal year 1975 economics) for a 1986-87 launch is used, the equivalent price for a 1982 launch would be:

	Shuttle (in millions)	Delta (in millions)
Transportation	$ 17	$ 30
Upper stage	4-5	3-4
Total estimate*	$21-22	$33-34

● Costs include payload integration.

SOLJRCE: Estimates courtesy of Robert V. Lottman, Director, Congressional Liaison Office, National Aeronautics and Space Administration Headquarters.

Appendix E
Economics of the Space Shuttle
by James A. Van Allen

In April 1982, James A. Van Allen of the University of Iowa presented to a meeting of Tau Beta Pi a paper entitled "Applications of Space Research to Modern Day Society." In preparation for the May 5 OTA workshop in which he participated, Van Allen sent a copy of that paper to OTA. Included here is the section of that paper in which he treats the economics of the space shuttle. Van Allen's analysis, which differs markedly from NASA's, is included in order to show how the space shuttle may have, more or less directly, affected the funding for space science. It is included with only minor explanatory comments. The reader should note that Van Allen includes overhead costs* in his calculations; these costs are not included when calculating marginal costs.[1]

The opinions expressed by Van Allen are his own and do not necessarily reflect those of OTA.

> There were many . . . weaknesses in the famous forecast of $100 per pound into orbit . . . I have prepared several (charts) analyzing the economics of the shuttle. The summary of this analysis is that there is no prospect whatever of bringing shuttle launch costs below some $5,000 per pound (1982 dollars). Even this figure is optimistic because my assumed payload of 60,000 lb per flight includes the mass of upper stages and other equipment that is not properly classified as useful payload.
>
> Hence, for realistic missions during the next 20 years or so, the shuttle system is actually much more expensive than are conventional, expendable boosters as exemplified by Delta, the Atlas-Centaur, the Titan-Centaur, and the French-German Ariane, all of which are in the advanced state of development and available for frequent use.

*Overhead costs are defined to be the fixed costs of the Federal establishment and associated contractors for maintaining the full operational capability of conducting a program of space shuttle flights, whether or not such flights actually occur.

[1]See, for example, Engel, Rolf, 1982, *Interavia* vol. 2, No. 177.

Table E-1 A.—Shuttle Economics (Federal Government)

N = Number of flights per year
O = Annual overhead
A = Out-of-pocket costs per flight
P = Cost per payload
M = Mass of payload
C = Cost per flight
L = Launch cost per pound of payload

$$C = \frac{O + N(A + P)}{N}$$

$$L = \frac{O + NA}{NM}$$

OTA'S comments: Table E-1A gives Van Allen's basic equation in which the quantity C, the cost per flight, Is expressed as a function of overhead costs, the number of flights per year, the out-of-pocket costs per flight, and the cost per payload.

Table E-1 B.—Shuttle Economics (Federal Government)

Examples:
O = $2,000,000,000 per year
A = $40,000,000 per flight
P = $100,000,000 per payload
M = 60,000 lb

N	Annual total	C	Launch cost per lb of payload
1	$2,140,000,000	$2,140,000,000	$34,000
	2,420,000,000	806,700,000	11,780
10	3,400,000,000	340,000,000	4,000
50	9,000,000,000	180,000,000	1,300

OTA'S comments: Table E-l B uses Van Allen's basic formulas of table E-1A to compute values of launch cost per pound of payload as a function of the number of flights per year N for assumed values of O, A, P, and M as listed at the head of table E-18.

Van Allen's comments: This analysis Ignores overhead during the lo-year *developmental* period; amortization of the investment for development of the vehicle and for facilities; and Interest on the Investment during the developmental and amortization periods.

Note that often quoted "marginal" or Incremental launch cost per flight is the quantity A, a grossly unrealistlc representation of the true cost. For the examples given here the "marginal" launch cost per pound of payload Is $670, irrespective of the number of flights per year.

Note: "Payload" Includes upper stages, if they are necessary, plus other equipment not properly considered useful payload.

Table E4A.-Shuttle Economics
(Private enterprise/Federal Government)

Van Allen's comments:

Assumed: That the Space Transportation System including facilities had been developed by private enterprise and then had been taken over by the Federal Government.

I = direct investment costs accrued linearly as a loan over a period of 10 years

The direct investment costs plus interest over the developmental period are then amortized linearly over an operational period of Y years by the Federal Government.

X = annual interest rate over (10 + Y) years

In this case the *additional* annual cost averaged over the Y years of operational use is given by

$$z = (1 + 51X)(1/y + x/2)$$

Numerical examples are given in table E-2B for N = 1,3, 10, and 50 launches per year.

Table E-2B.—Shuttle Economics
(Private enterprise/Federal Government)

Examples:

I = $15,000,000,000

Y = 15 years

X = 0.1 (10%)

z = $2,625000000

N	Annual total	C	Launch cost per lb of payload
	$4,765,000,000	$4,765,000,000	$77,750
	[illegible]	1,681,700,000	26,360
10	6,025,000,000	602,500,000	8,360
50	11,625,000,000	232,500,000	2,270

Van Allen's comments: "Payload" includes upper stages, If they are necessary, plus other equipment not property considered useful payload.

Assumed values of O, A, P, and M are the same as In table E-l B.

All estlmatsd cost flguras In tables E-1A, 1B, 2A, and 2B are given In 1982 dollars.

OTA'S comments: Table E-2B uses the formula of table E-2A to calculate four specific examples of the launch cost per pound of payload. Taking a developmental cost of $15 billion amortized over 15 years at an Interest rate of 10 percent, Van Allen calculate that for 1,3,10, or 50 flights per year, the total (not marginal) launch cost per pound of payload will be $77,750, $26,360, $8,380, or $2,270, respectively.

Van Allen's calculations are Intended to call Into question the assertion that the shuttle will be able to bring the launch cost par pound of payload down to $100 to $30/lb. It should be noted, however, that this figure is the *rnarginal* launch cost per pound of payload, and that Van Allen's calculations are baaed on the total launch cost per pound of payload. It Is, of course, the total cost of the shuttle which has had an Impact on the space program.

Appendix F

Space Science Workshop Participants, May 5, 1982

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