POLICY CHALLENGES FACING THE US SPACE RESEARCH PROGRAM

The realization of the US scientific community's aspirations for research in space will likely depend on how several critical policy issues are resolved.

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In a companion article in this issue (page 56), Joseph K. Alexander and Frank B. McDonald discuss the current state of space research in the United States, some of the changes that are taking place in the US space science program and the contributions that the space sciences have made to addressing a wide variety of scientific questions. Based on these accomplishments, the space research community has developed an ambitious menu of possible new initiatives for the end of this century and beyond. But describing scientific dreams, identifying trends and carrying out elaborate planning studies to define and choose new missions will constitute purely intellectual exercises unless both NASA management and the scientific and engineering communities take specific actions to improve program implementation. Also, the need for a stable policy that leads to orderly planning and execution of such programs must be recognized by the executive and legislative bodies that formulate and approve both the plans for space research and their budgets.

In this article we will examine some of the policy, program and management issues that must be addressed if the promise of significant scientific advances in space is to be realized. While some of the issues are unique to space research, many are shared with much of contemporary American science:

▷ the balance between large-scale and small-scale programs

▷ the impact of commitments to the construction and long-term operation of large facilities

▷ the need for innovative approaches to control costs and to optimize the use of available funds

▷ the steps that must be taken if new talent is to continue to be attracted to research

▷ the need for budgetary stability so that projects begun are completed and effectively realized.

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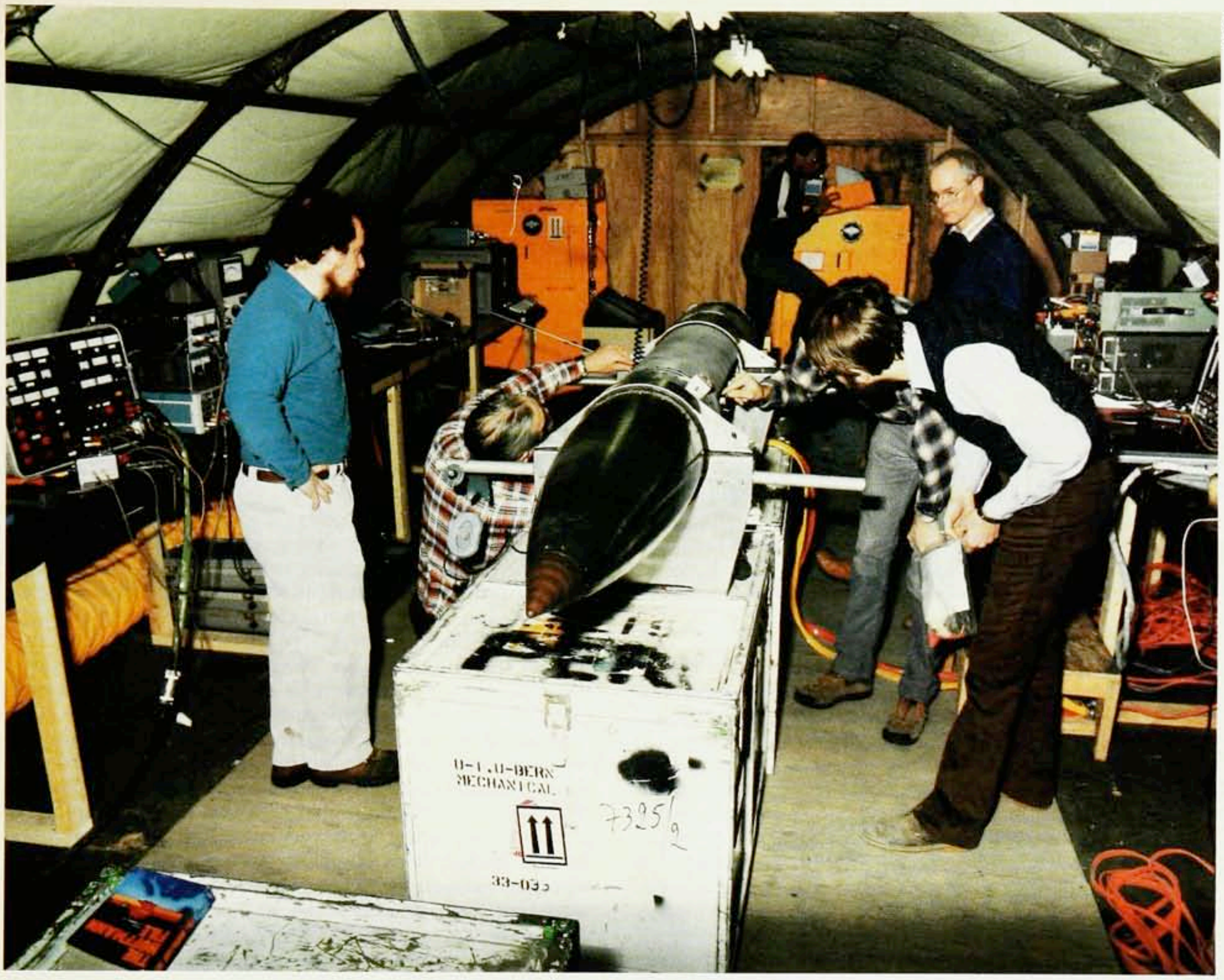
Consideration of these issues has spilled out of the scientific community into a more national arena of debate. The recent lively discussions in the pages of PHYSICS TODAY and elsewhere concerning the Superconducting Super Collider eloquently attest to their currency. Thus how—or even whether—such issues are addressed is likely to have implications that go well beyond the specific concerns of space research.

It is just as likely that the resolution of program and policy issues will determine the future course of scientific space research in the United States as that the scientific imperatives themselves will do so. In particular, at the outset we must recognize that advances at the frontiers of space research do not come cheaply. For three decades Americans have accepted the costs associated with scientific discovery and progress; every indication suggests that their support is ongoing. Nevertheless, the nation quite rightly demands that it receive the best possible return from the dollars spent. But neither NASA nor the scientific community can claim that all that is needed to take advantage of newly emerging opportunities is an ever increasing amount of money. Other steps must also be taken. At present NASA's space science and applications program is receiving approximately \$1.6 billion per year, and the request for fiscal year 1989 is nearly \$1.9 billion. NASA's first priority must be to ensure that those resources are used as effectively as possible. The people, funds and facilities needed to convert important initiatives into scientific reality are always going to be limited. Optimal use must be made of these precious resources if NASA and the nation are to obtain the best return from their investment in space research.

Assured access to space

At present, without an assured access to space, NASA does not have a space science program. As Sally Ride has noted, "A space program that can't get to orbit has all the effectiveness of a navy that can't get to sea."[1] The need for a diversification of NASA's launch capability has also been forcefully articulated by the Space Science Board, the NASA Advisory Council and other groups.[2] Even when the shuttle does resume operation, there may still be a significant mismatch between the shuttle launch capacity and US launch requirements. There is an



Suborbital rockets are essential for carrying instruments to study auroral plasma physics—for example, to measure ion acceleration processes. Here a payload is undergoing checkout at the launch site prior to launch on a three-stage Black Brant 10 rocket from Søndre Strømfjord, Greenland. The instruments were built by scientists from the University of New Hampshire, Cornell University, Marshall Space Flight Center and the University of Bergen, Norway.

overwhelming consensus that NASA must once again use expendable launch vehicles. Much of the current program planning in space science is now based on the use of a mixed launch fleet.

The US Department of Defense had already recognized the need for alternatives when it began its Complementary Expendable Launch Vehicle program. And the development of Titan IV and Delta II launch vehicles is well under way.[3] Another important step toward diversification of the US launch capability was a Presidential policy decision—made in summer 1986—that the shuttle should no longer launch commercial satellites. A varied US commercial launch service capability appears to be rapidly emerging.[4] NASA's scientific programs must be able to take advantage of this diversity. In particular, while many types of scientific programs do require astronauts, routine launches of satellites or of planetary probes do not.

In the future, the most appropriate launch vehicle must be adopted for each program. A mixed launch fleet will allow human beings on a mission when they are actually needed, but will let unmanned vehicles be the carriers of choice for other missions. Diversity will allow better matching of a mission's scientific requirements with the available launch capability, rather than forcing each mission to meet the constraints of a single launch system. The current trend is clearly in the right direction; however, care must be taken to ensure that budget (or other) pressures that may develop do not force a return to the previous—and obviously failed—policy of reliance upon a single system.

A spectrum of flight opportunities

In planning and implementing the US space research program, NASA and the scientific community have usually focused their attention on large missions, which require the most substantial engineering and management attention. These also make the largest demands on the space research budget. However, the long-term vitality of the program requires the availability of a range of activities and facilities to provide both a continuing flow of observations and experiments and the theory and models needed to consolidate and reconcile the empirical evidence gathered from measurements and observations.

Both small and large missions are essential components of the space science program. For example, low-cost suborbital missions are essential for investigating new scientific questions with relatively short lead times, for developing instruments and technology required to set the stage for major spaceflight missions, and for graduate education. Low costs and relatively fast turnaround times make acceptable the risks associated with innovative experiments, which may not be fully successful on a first try. Missions of modest complexity and moderate cost, such as the Explorer satellites, can study more focused scientific questions that can be addressed with specialized instruments. Large facilities—such as the Hubble Space Telescope—provide multipurpose instruments of sufficient sensitivity, spectral resolution, spatial resolution and general versatility to make the long-duration measurements needed for the most dramatic advances.

Planning for the future must take into account that the goals of large and small space missions are different, as indeed are their contributions. Attention must be paid to the balance between small-scale experimental missions with more limited goals and larger, high-visibility projects. All parties involved in the program and budget approval process must recognize that the more focused projects and the ongoing operation of existing satellites—along with basic research, data interpretation and associated theory—are as indispensable to the conduct of a successful science program as are those major projects that more readily catch the public's attention.

Controlling costs

A growing burden for space research is the increasing cost of missions—both for development and for operations. Innovative approaches are needed to decrease spacecraft costs, particularly for missions that are medium to large in scale. Savings may be obtained in the Explorers and Planetary Observers by introducing more continuity and standardization into these programs. This strategy is being implemented for the Planetary Observers by adapting existing near-Earth orbital spacecraft designs to satisfy the science requirements of diverse planetary missions. Expenditures also might be reduced on Explorer missions by introducing multiple-mission spacecraft buses as standard platforms on which experiments are exchanged in orbit. Some science disciplines (astrophysics, for example) are, in fact, beginning to implement this approach. Care must be taken, however, to assess critically the virtues of using standardized spacecraft on the basis of current experience, to ensure that the apparent benefits are not illusory.

Consideration should also be given to developing instruments that could be used on several missions. In such an approach, engineers would need to be unusually creative, or scientific goals might have to be compromised somewhat; nevertheless, by reducing the costs of individual missions, such strategies might provide the additional flight opportunities that are so badly needed to ensure the future vigor of the space research program. But the scientific community may have to be more willing to accept compromises than was necessary in the past. A careful distinction must be made between the spacecraft and instrument capabilities required to meet the scientific objectives of a mission and the capabilities that might be technically realizable. It is important to avoid situations where a significant fraction of the mission cost goes toward achieving a relatively modest improvement in performance that could possibly be done without. Both instruments and missions must be sized for the expected return.

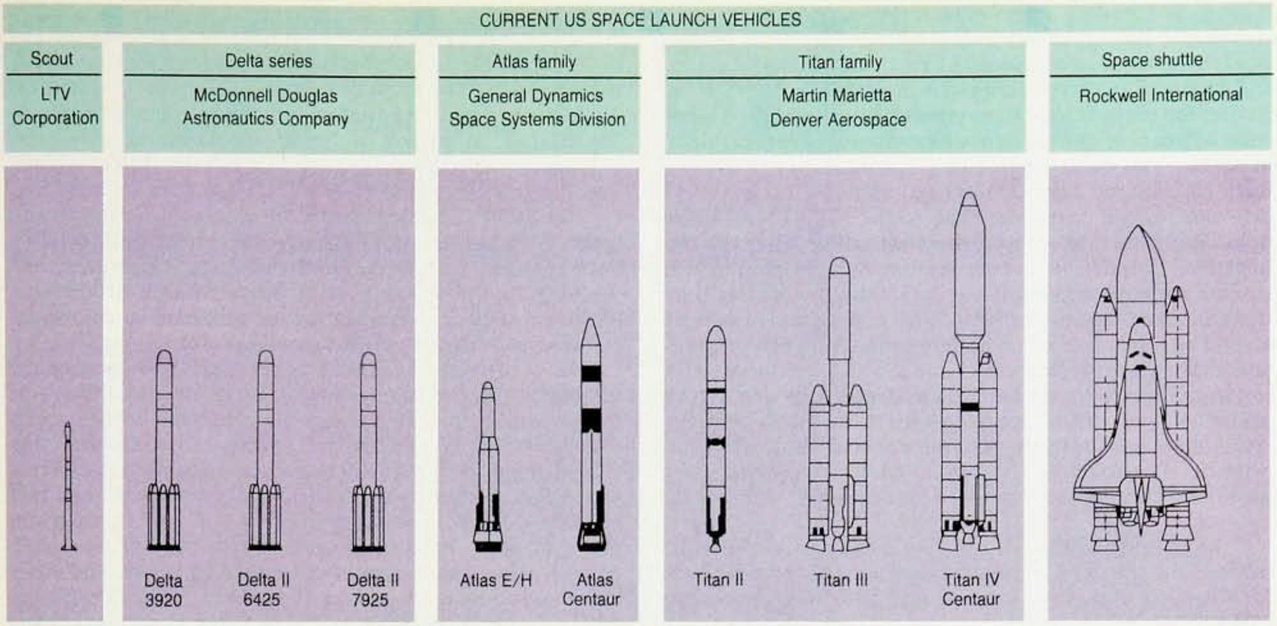
Similarly, overall expenditures may be reduced by decreasing the amount of documentation and inspection required for scientific experiments carried aboard NASA spacecraft. So long as it is consistent with the needs to protect the spacecraft and other instruments, the responsibility for the successful operation of an experiment should be borne largely by the principal investigator, who of course will be highly motivated to have a productive flight.

In the long run it may actually prove more economical to decrease reliability for some types of science missions. At present it typically costs more per kilogram to develop a scientific payload than it does to place that payload in a low Earth orbit; however, the ratio can vary from 1:1 to 50:1. Since a significant fraction of the development cost can be associated with the requirement for high technical reliability and quality assurance, a major reanalysis of the cost–benefit ratio of reliability requirements on low-Earth-orbit missions is in order. The experiences of other US agencies with responsibilities for spaceflight development or of other spacefaring nations may prove instructive, and a detailed examination is needed of various approaches to program implementation. The possibility of decreasing reliability on certain science missions is especially appropriate as the era of the space station approaches, with its potential for the repair and refurbishment of satellites. Reductions in the development and construction costs of major missions that do not have to be designed for reliable long life could have a dramatic effect on the amount of science produced per dollar by the NASA program. However, we must also recognize that the cost reductions that may result from easing reliability requirements for some science missions might well be reintroduced in the form of the extra expense of making those missions repairable. Such trade-offs need to be examined explicitly.

As the Hubble Space Telescope program has shown, substantial extra costs can be associated with the design and development of a repairable-in-orbit spacecraft. One may ask, might it not have been less expensive to have proceeded with a program involving several copies of a simpler spacecraft, and to achieve long life (as well as the periodic upgrading of focal-plane instruments) through the use of multiple spacecraft launched one after the other? The answer is far from obvious. Also far from obvious is whether technical, financial and political considerations would necessarily lead to the same conclusion. We need more careful looks at the advantages and disadvantages, the economics and the politics of the possible approaches to implementing such programs.[5]

Optimizing use of available funding

Developing new approaches for reducing mission costs is only one of several steps necessary to manage resources more effectively. Once a program has been started, its development must proceed on a timely, stable course. Over the past several years, a significant fraction of NASA's science budget and of scientists' time has been wasted by delays and "stretchouts" of flight projects. Three recent notable examples, which illustrate the problems introduced by such stretchouts even prior to the delays imposed by the Challenger accident, are Spacelab 2, Galileo and the canceled Solar Optical Telescope. Spacelab 2's budget escalated from an initial sum of $27 million to a final cost of $70 million at launch, which took place five years later than originally planned. And as the launch date of the Galileo mission to Jupiter slipped from 1982 to 1986, and the baseline launch system also kept changing, NASA's cost for the mission—as reflected in the budget of its Office of Space Science and Applications—rose from $379 million to $843 million. This cost has increased even further due to the additional delays resulting from the Challenger accident and from the changes in the mission that had to be made following the cancellation of the shuttle's Centaur rocket upper stage. Three years of delays in SOT led to an estimated cost increase of $73 million, a significant element in the decision to cancel the program. Increasing the cost of a program by delaying it, then canceling it because of those increases, does not appear to be a particularly effective way to manage a program. Had SOT proceeded as planned, the



Expendable launch vehicles with a wide range of capabilities and the manned space shuttle will be available for carrying space science and applications payloads into Earth orbit and beyond.

delays on these three projects alone would have accounted for $580 million in increased costs to OSSA over five years.

The effective "loss of funds" from these three missions amounts to nearly 10 percent of the total yearly OSSA budget and is equivalent to one-third of the OSSA annual research and analysis budget. Were it not for these slips, another new mission could have been developed during this same period without an increase in OSSA's level of funding. Such delays produce no useful science and represent a diversion of resources that might otherwise allow additional projects to be carried out.

In the future, once a project has been started, we stress that it must be completed on the most cost-effective schedule. Not only NASA but also the Office of Management and Budget, and Congress as well, need to recognize that such an optimum schedule exists. At times program schedules have had to be altered significantly because Congress has changed funding requests or imposed funding limitations, or because Congressional committee staffers have made program changes. Such actions have been a significant contributor to the current situation.

Margins in NASA's program planning

Perpetually tight budgets have driven NASA to a situation in which a single mishap can devastate its entire program. Thus the failure of an O-ring has grounded NASA's total launch fleet and brought most of experimental space research to an abrupt halt. Much of the damage could have been avoided by maintaining a mixed fleet of manned and unmanned launch vehicles. The space research community has expressed concern that there may be other single-point failures in the NASA system that also have the potential for severely damaging the US space program and space research. Failure of the Tracking and Data Relay Satellites or their ground station, for example, could lead to simultaneous loss of all the data from the Hubble Space Telescope, the Gamma Ray Observatory, the Upper Atmospheric Research Satellite and other missions as well. All such possibilities for single-point failures must be examined, and plans for contingencies and alternatives must be developed. Current plans for the development of a second TDRS ground station suggest that NASA is, in fact, becoming increasingly sensitive to this situation.

The evolution of the space research program toward the use of large, facility-class missions not only focuses the major direction of each scientific discipline involved for a decade or more around such facilities, but also increases the chances that devastation will be wreaked upon an entire discipline by the loss of a single mission. At the same time, the potential for disaster has been increased by decisions to build and launch only a single spacecraft for each mission—even missions involving irretrievable and unrepairable spacecraft, such as Galileo. Even though such large projects may have strong reliability and quality control programs and the spacecraft may have many redundant subsystems, we think it is unrealistic to expect space missions to be 100 percent successful. Therefore NASA planning must not be so success oriented that unforeseen mishaps can decimate large elements of its scientific program. While some substantive steps have been taken toward dealing with this issue (most notably the recent decision to provide spares for the Mars Observer), for the most part NASA seems to be continuing to plan its programs with little if any margin for error. The wisdom of such tight planning needs to be carefully reconsidered.

Toward more realistic planning

Over the past decade the American scientific community has completed a number of major science planning studies that have laid a firm scientific foundation for an ambitious program of space research going to the year 2000 and beyond.[6] In addition to identifying possibilities for scientific advances, many of these studies have also laid out possible program plans and specified the funding needed to implement them. It is, however, apparent from comparing these ambitious funding requirements with current budget levels for space research in NASA that the aspirations of the space research community substantially exceed the level of funds that is likely to be available.

Unfortunately these studies are just the most recent of numerous detailed planning exercises and subsequent definition studies of individual missions that, for a variety of reasons, have resulted in the actual start-up of only a small number of new missions. Much time, energy, effort and money have been dissipated in carrying out mission studies for projects that have either been canceled or have entered years of definition studies without an actual start in sight. The wasted effort and unfulfilled expectations have squandered scientific energy and enthusiasm, and diverted limited resources that might have been better used. It is time to adopt a more conservative policy toward initiating detailed definition studies and promoting the start of major new missions—a policy that recognizes that the limited resources available (both human and financial) should be concentrated on defining relatively few projects and that future opportunities for the start of large-scale programs may be restricted. Ideas for new missions should continue to be examined; however, as missions progress from very preliminary assessments of the scientific promise of concepts to more detailed stages of study, the number of candidates under consideration should be severely reduced.

An overabundance of detailed definition studies can mislead the scientific community concerning the prospects for the start of a given mission and become a hindrance rather than a help to orderly and realistic planning. In recognition of the space capabilities that have developed in countries other than the US, it is also important that NASA foster well-coordinated planning with our non-US colleagues.[7] The strategic planning now being undertaken by OSSA seems to be an excellent first step toward achieving more realism in planning.

How broad a program?

The budgetary pressures of the last several years have raised the question of whether NASA and the nation can continue to support a wide range of scientific programs or whether the agency will be forced to reduce the scope of its program to fit available and projected resources, with the result of a much more specialized program. At the same time there has been an increasing interplay among results from the various scientific subdisciplines. For example, the magnetospheres of the planets (including the Earth), the more diffuse plasma that permeates the Solar System and has its origin in the solar wind, and the ion tails of comets are the only large-scale cosmic plasmas likely to be accessible to *in situ* measurements for the foreseeable future. It is increasingly apparent that many astrophysical phenomena (for example, pulsar magnetospheres or the tails of radio galaxies) can be properly understood only by applying the results of plasma physics. Understanding such phenomena requires a vast extrapolation of ideas in basic plasma physics that have only been tested in detail through laboratory-scale experiments. Through appropriate planetary and interplanetary measurements, the Solar System itself thus becomes a large-scale laboratory for checking the validity of such extrapolations and thereby rendering more secure the applicability of the results of plasma physics to truly astronomical settings. Restricting the scientific scope of the program would foreclose the opportunity to apply the results from one scientific field to important problems in other areas.

Beyond the purely scientific considerations that argue for a program of broad scientific scope, care must also be taken not to distort the nation's space research program by forcing it to emphasize that science that supports NASA's current or planned large space initiatives. It is clear, for example, that new types of long-duration scientific experiments that require human operation, as well as ones that need periodic human inspection, will become possible with the space station.[8] However, the space station should not be considered an end in itself but rather only one member of a set of tools to be used for addressing a wide range of scientific problems. Payloads and research activities must be selected according to scientific need. It is imperative that the scientific community have the ability to attack a broad spectrum of scientific questions using the full range of available tools, including—but not restricted to—the space station.

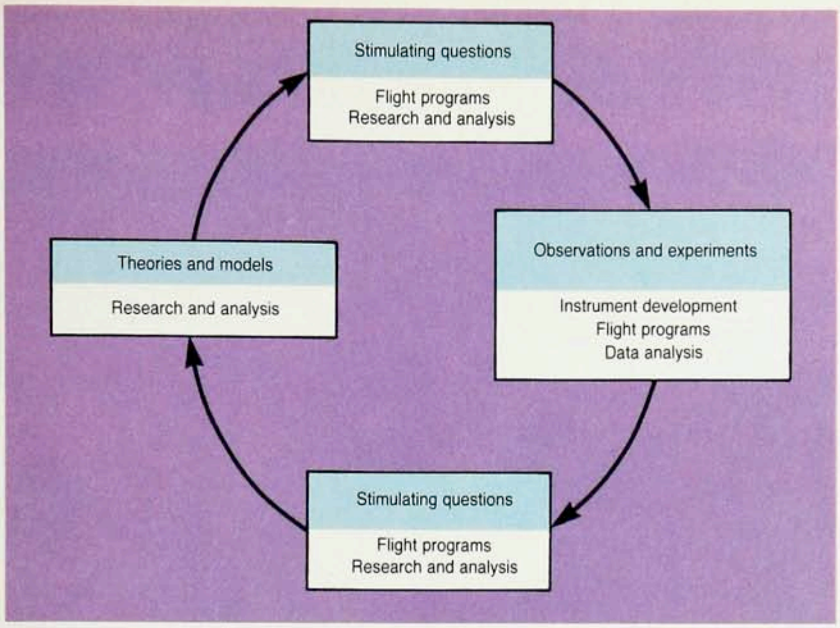
Similarly, important scientific questions can be attacked with an intensive Mars science program involving both manned and unmanned elements.[9] However, addressing many key aspects of Solar System origin and evolution will also require future missions to asteroids, comets and other planets and satellites of the Solar System.

It is encouraging that there are now promising indications of increased attention and support for NASA's space research program that will permit the continuation of a broad range of scientific studies. Nonetheless, the volatility of the current economic situation and the attempts to balance the Federal budget suggest that the funding increases necessary to carry out a broad program may not be so easily achieved. Some very critical and painful choices may very well have to be made, and such choices cannot and should not be made by NASA (or the scientific community) alone. Also, they should not happen by accident. They should be conscious decisions concerning the structure and scope of a national space program, reached by a consensus of the American people through their representatives in the executive and legislative branches of government.

Steps toward a promising future

A scientific discipline is kept alive and vigorous when stimulating questions can be posed and means are at hand for providing clear answers. The space sciences are replete with provocative questions; imaginative theorists in the space community have developed innovative ways of constructing predictive models based on experimental and observational evidence; and a talented community of instrumentalists knows how a next generation of space missions capable of testing these models should be designed. But the opportunity to develop such missions to sustain a continued data flow has seriously declined over the past few years, and the base support for innovative research and analysis has also suffered. Particularly in the wake of the Challenger accident, the future has looked bleak for established space researchers and uninviting to the talented young scientists whom a productive field must continue to attract to remain vigorous and enterprising. If the most gifted researchers are to remain in the field and if space research projects are to stay alive and healthy, then the various disciplines must display a promising future. That future can be assured by establishing a number of favorable conditions for fostering excellence.

First, the range of space research activities that the nation undertakes must be kept broad. Each subdiscipline of space research learns from advances in related areas. Small and large undertakings should be interwoven in ways best suited to progress in each field. Human interaction with equipment should complement automated instrumentation; launch vehicles should be chosen to meet technical demands. Steps to restore the vitality of the flight program will not suffice unless healthy support for basic research and data analysis complements space missions; research and analysis meld isolated observations into coherent models that form the basis of new scientific understanding. The expense of space missions can only be justified if they are part of a coherent scientific program and produce major new insights into nature. Large programs will often yield the greatest gains only if complemented by smaller missions. This necessi-



Vitality of scientific research depends upon the interplay of several important elements: stimulating questions, observations and experiments, and theories and models. In the US space research program, achieving the maximum scientific return requires a comprehensive approach involving the careful nurture and balancing of support for theory and data analysis, instrument development and flight opportunities.

tates a clear-headed appraisal of the most effective mix of mission types.

Second, a promising future can be assured only through of a sound research infrastructure. The overall value and uniqueness of each of the contributing classes of institutions, whether university, government or industry establishments, must be recognized if a diverse, productive space research effort is to be sustained.

Third, the means by which scientists and the nation decide on the direction of research for years to come must be based on a systematic framework for evaluation of competing proposals. Major missions that have the potential to provide vast leaps in understanding or that promise substantial benefits for society will continue to remain at the center of attention only as long as the perceived advances warrant appropriation of the required funds.

The need for a steadfast course

High funding levels alone are not the sole answer to budgetary problems. Equally significant is budget reliability. Orderly conduct and cost-effective execution of a space research project can only be planned if future budgets can be predicted. Stability of support may be even more important than the level of support.

Keeping talented researchers dedicated to NASA's programs, particularly when these programs may require many years for their successful implementation, requires some assurance that their projects can be carried through to completion. The directions and the support of their research cannot be erratic. Steadiness in financial planning is absolutely essential. This becomes doubly true when projects are carried out collaboratively with other countries. And though technological difficulties can lead to unanticipated delays and expenditures, effective means can often be found to reallocate manpower and resources to minimize the financial impact of setbacks. That is what effective management is all about. However, without steadfast planning there can be no clearly perceived future; and with an uncertain future, the talent (which is the key to ultimate vitality of any scientific undertaking) will not be attracted into space research.

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