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 wide-

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ly) 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.