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Space Science Board

Preface

This report is concerned with strategy and priorities in the NASA space-science program. It makes recommendations about immediate new starts, medium-term strategy required for planning purposes, and long-term aims. It is our intention to review the study in October 1975 and as frequently as possible thereafter in order to keep the recommendations in step with new scientific discoveries and changing fiscal constraints.

The report involved a great deal of work by many different people. We are particularly indebted to the disciplinary committees of the Space Science Board, to their consultants, and above all to their chairmen: Robert Phinney, Robert Danielson, Frank Johnson, and John Shepherd. Without the sustained effort of this group our report would have been of small value.

We were assisted by written comments on preliminary drafts of committee reports by approximately 120 scientists other than members of the Space Science Board and its committees. This attempt to involve a wider scientific community was an experiment undertaken because of a lack of funds for a more concentrated summer study. Despite certain vicissitudes, the experiment was successful.

At its final meeting in October 1974 the Board was joined by a number of guests—Charles Townes, James Arnold, and Reimar Luest—whose presence added to the quality of the report and to our pleasure in preparing it.

As is usual, we received all the help that we requested from members of NASA; Rocco Petrone, John Naugle, and Noel Hinners were present at different times during our discussions. Meaningful deliberations about mission costs and capabilities could only have been undertaken with the complete cooperation of the space agency.

This report was prepared, except for the Mars study, as part of the Board's regular operation. Such an undertaking would not have been contemplated without the devoted support and assistance of the Space Science Board professional staff, Milton Rosen, Richard Berendzen, Ann Grahn, Dean Kastel, and Richard Hart. Lally Anne Anderson, Mildred McGuire, and Gray Mason typed the manuscript and performed many other helpful services. All rose to the challenge of a difficult task and could have given the Space Science Board no better service.

Richard M. Goody, *Chairman*
Space Science Board

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I
Report of the Board

1 Purpose of the Report

The Space Science Board last made an overall assessment of the National Aeronautics and Space Administration's space-science program in 1970.* Four years later, the major recommendations of that report are in the course of implementation (see, however, Chapter 3, Section I). Other findings and recommendations of the report are rendered obsolete either by program developments such as the Space Shuttle or by new scientific discoveries. The impetus of the 1970 study has therefore been spent, and in the absence of a new study the Board lacks a substantial basis for advice to NASA where this advice involves interdisciplinary priorities, long-term strategy, or other matters that require a level of consensus in the scientific community.

At the 48th meeting of the Space Science Board held on February 7-8, 1974, the Board recommended that a new study be undertaken. Funds were not available for an extensive "summer study" activity, and the Board decided to try an experimental approach based on work by its disciplinary committees and consultation with a segment of the profession by mail.

The study was carried out in the following stages:

Priorities for Space Research 1971-1980 (National Academy of Sciences, Washington, D.C., 1971).

1. The planning wedge* in current use by the Office of Space Sciences (OSS) was adopted as a constraint on the funding available for new projects.

2. By agreement with disciplinary committee chairmen, planning wedges for the individual disciplines were allocated in order to illuminate the necessary choices. For planning purposes, 50 percent and 100 percent of (1) were allocated for each of the Committees on Space Astronomy and on Planetary and Lunar Exploration (the Committee on Space Astronomy also opted to investigate a 67 percent planning wedge); and 10 percent of (1) was allocated for the Committee on Space Physics.

3. Each disciplinary committee wrote a preliminary report for circulation to other active scientists. One hundred and twenty written replies were received, about forty in each discipline. These replies constitute a unique documentary resource for the Board.

4. On the basis of these replies, the disciplinary committees identified controversial areas and invited to their deliberations scientists who could speak for these areas. The Committee on Planetary and Lunar Exploration also sponsored, at the request of NASA, a special study on the future exploration of Mars, within the framework of the overall planetary and lunar priorities.

The working papers from these activities are printed, without modification, as Part II of this report.

5. On October 10-12, 1974, the Space Science Board examined these reports with particular attention to (a) general strategy, (b) interdisciplinary funding conflicts, (c) rationale for the new starts proposed for fiscal years 1976 and 1977, and (d) special problems.

The results of our study constitute Part I of this report.

*The planning wedge is the annual funding available for new projects. It is derived by summing (for each succeeding fiscal year) the estimated costs for the completion of all existing projects and subtracting the totals so derived from the expected fiscal year funding for research and development. It is a wedge (with its sharp edge in the next fiscal year) because the R&D funding for space sicence is expected to remain constant during the near term, while the runout costs of existing projects decrease with time. Hence the funds available for new projects increase with time, as a wedge increases thickness when one moves away from the leading edge.

Our report deals only with the program of the Office of Space Science. The Board is also concerned with space biology and medicine, funded principally by NASA's Office of Manned Space Flight, and the scientific basis of applications programs, funded by NASA's Office of Applications. In future years, the Board hopes to put increased effort into both of these areas. The annual report of the Committee on Space Biology and Medicine, accepted by the Board, is included in Part II.

An important consequence of an intensive study is that the Board gains a depth of familiarity with both the problems of NASA and the views of the scientific community. We have already pointed out that such expertise rarely has a lifetime of more than three to four years and therefore must be renewed at regular intervals. We have, therefore, concluded that it will be economical of effort, and our advice will be of greater value, if this study is updated year by year.

2
Strategies for Space Science

I. STRATEGY FOR SPACE PHYSICS

Space physics concerns interactions in space among energetic charged particles, ionized plasmas, and magnetic and electric fields. Most of the investigations are conducted in the ionosphere and magnetosphere of the earth, because of its accessibility. The interplanetary medium and the atmospheres of the other planets are of strong ancillary interest. In spite of the tenuous nature of matter in these regions, the complex physical processes that they exhibit are of fundamental importance to many of man's activities, both practical (e.g., long-distance radio communication) and scientific (e.g., study of plasma physics and astrophysics).

Knowledge of space physics has advanced rapidly in the more than a decade of exploration using space vehicles. The fundamental remaining questions are now much better defined. Some examples are

1. How does the solar wind flow into the earth's magnetosphere?
2. What are the mechanisms of coupling between the ionosphere and magnetosphere?
3. What are the mechanisms for generating nonthermal radiation from the magnetosphere?

The solutions to such problems require coordinated attacks not possible in the past because of the lack of adequate information and suitable technology.

What is needed now are carefully planned missions in which the important parameters (e.g., dc magnetic and electric fields, waves, energetic particle flux, thermal plasma properties) are measured simultaneously in the ionosphere and magnetosphere at several points chosen to identify or isolate particular physical phenomena. Rapid data handling and exchange of data between investigators are needed to permit quick reaction to the results of each experiment. Individual experiments can then be turned on and off or have their parameters modified by command from the ground to maximize the scientific output during the next experiment opportunity. New propulsion systems permit changes in orbits that are needed to bring about favorable relative positions of satellites. Active experiments involving injection of waves and particles into the magnetosphere permit a degree of control approaching that achievable in laboratory experimentation.

Much of space-physics research is done near earth. The instrumentation needed is well developed and does not place difficult requirements on space vehicles. Hence, the cost of space-physics research in the terrestrial environs is low by comparison with space projects in general. Its relative simplicity and low cost make this type of space research particularly well suited for cooperative international efforts for which each nation supports the activities of its own scientists. Also, associated with the modest spacecraft and instrumentation costs, there are larger numbers of scientists involved than would be assumed on the basis of the total cost of the space-physics program and a correspondingly high scientific productivity per unit cost.

Continuity of effort in the proposed program in space physics is achieved by alternating in succeeding 3-year intervals the launching of Electrodynamic Satellites with Atmospheric Explorers. The purpose is to maximize the number of active spacecraft that can contribute to coordinated measurements while limiting total expenditures for space physics and maintaining them at a relatively even level.

II. STRATEGY FOR SPACE ASTRONOMY

The goal of space astronomy is to operate *permanent* observatories in space. The Space Shuttle provides the capability to deploy, maintain, and repair them while in orbit. The Space Shuttle is also required to return the observatories to earth for major refurbishment and updating. The strategy is similar to that which has proven successful for ground-based astronomy. Following the pattern set by the Kitt Peak

National Observatory (KPNO), the Cerro Tololo Inter-American Observatory (CTIO), and the National Radio Astronomy Observatory (NRAO), we envisaged analogous national or international observatories in orbit.

The first of these observatories is the Large Space Telescope (LST). Historically the LST has evolved as a 3-m diffraction-limited (0.03 sec of arc resolution) telescope. However, it appears that an instrument in the 2-m class (perhaps 2.4 m) having a resolution of 0.10 sec of arc can be built at substantially less cost than the full performance 3-m instrument. Although the faintest star observable is about three times brighter (about 1 magnitude) than the 3-m LST, the 2.4-m telescope would be a magnificent instrument, capable of making many critical observations relevant to extragalactic research and cosmology. It could observe stars ten times fainter than is possible using ideal detectors on the largest ground-based telescopes. Moreover, it would have ten times sharper resolution. As a result, the fraction of the universe that can be observed with precision would increase 30 times.

Another permanent observatory that receives high priority contains a 1.2-m imaging x-ray telescope. This orbiting observatory will have the versatility to observe a wide variety of objects ranging from the nearby x-ray stars to galaxies in the farthest reaches of the universe. Like the LST, this national x-ray observatory has been planned in detail.

Eventually, we envisage other permanent observatories operating at gamma-ray wavelengths, infrared wavelengths, and radio wavelengths. We also foresee permanent solar and cosmic-ray observatories in orbit. A series of smaller free-flying and Shuttleborne instruments must precede these permanent observatories, however, as the Orbiting Astronomical Observatory (OAO) and High Energy Astronomical Observatory (HEAO) satellites precede the LST and the large x-ray telescope.

III. STRATEGY FOR PLANETARY AND LUNAR EXPLORATION

The goals of planetary exploration for the next two decades are to conduct broad-based exploratory studies of the full range of bodies in the solar system, to extend our knowledge in a diverse way beyond the moon and Mars, and to gather new data obtainable from probes and orbiters. The logical sequence of investigation of a planetary body should proceed via reconnaissance, exploration, and detailed study, in which discoveries in one mission help to shape the scientific objectives of the next. The strategy for planetary exploration should involve a balanced approach to all accessible objects in the solar system and should include all pertinent disciplines. The attainment of balance is a goal that can only be achieved over a decade; from time to time it is opportune to emphasize the investigation of certain objects, depending on flight opportunities and technological developments.

During the past seven years there have been major accelerations in the investigations of the moon and Mars beyond the logical exploratory sequence, culminating in sample return from the moon and the imminent biological investigation of Mars. Unless there is a positive indication of life on Mars, the continuing investigation of these bodies should emphasize global geochemical and geophysical surveys as a medium-term policy. Such surveys are complementary to the investigations of returned lunar samples and will help in the selection of optimum landing sites for future Mars sample return.

The inner planets, Venus and Mercury, have been objects of substantial mystery until the recent Mariner Venus Mercury reconnaissance. This relative neglect should be rectified in the medium term by orbital surveys: Pioneer Venus '78 for atmospheric and ionospheric study, imaging radar for mapping the Venus surface, and a chemical and physical study of Mercury.

The richness of the phenomena seen in the Pioneer 10 mission highlights the importance of a period of continuing emphasis on the outer planets through the middle 1980's, beginning with the ongoing Pioneer and the scheduled Mariner Jupiter Saturn missions. This emphasis is appropriate because a period of favorable flight opportunity coincides with the advent of our ability to assemble spacecraft with the needed performance. It is possible to carry out the preliminary reconnaissance of Saturn and Uranus during this period and to commence the detailed study of the Jovian system. The Jupiter orbiting missions have the potential for an immensely rich return of valuable scientific data concerning particles and fields; the composition, structure, and meteorology of the Jovian atmosphere; and the detailed properties of the regular satellites. The timing of these missions is critically dependent on the state of propulsion technology.

Cometary investigation by spacecraft has yet to be undertaken, and one may contemplate a ballistic intercept mission in the 1980 time frame, to be followed a few years later by a rendezvous mission. The latter will require the development of solar-electric propulsion, a technology that will also be essential for missions to investigate planets beyond Uranus as well as asteroids.

The ultimate objective of planetary investigations should be to return samples for detailed study in terrestrial laboratories. This has been accomplished for the moon, and the Space Science Board recommends that sample return be considered the long-term goal for Martian exploration. These samples will be of high exobiological as well as geophysical and geochemical interest.

Existing fiscal constraints prevent an optimum pacing of these exploratory activities, and the mission model described in this report responds to these constraints through postponement of some of the missions to later years. If choices must be made among recommended missions, they should preserve the relative emphasis given in the mission model.

3
Findings and Recommendations

I. APPROVED PROGRAMS

Three major approved programs in space science represent the next opportunities for important advances in space astronomy and in planetary and lunar exploration, namely, High-Energy Astronomy Observatories A, B, and C; Pioneer Venus; and Mariner Jupiter Saturn. All three programs have been slipped in time and restructured with considerable effort, sometimes more than once, in order to achieve minimum cost and optimum science output. Each has been evolved as the result of lengthy efforts by a substantial scientific community largely external to NASA; the scientific justification for each has been refined and tested to the extent feasible. Finally, these programs are NASA's main response to the recommendations of the 1970 study of the Space Science Board (*Priorities for Space Research 1971–1980*), and their implementation will be the tangible result of several years of interaction between the Board and the space agency.

The Board wishes to reiterate its strong support for these missions. We are aware that in times of fiscal stringency even approved programs are at risk. These three programs, however, are of such standing that their implementation must be given highest priority on an agency-wide basis if there is to be a viable, quality program in space science.

II. RECOMMENDED MISSION MODEL

Table 1 combines the most carefully refined mission models submitted by the disciplinary committees of the Board. The estimated total cost of these missions is close to, but somewhat larger than, the

TABLE 1 Model of Proposed Missions for Space Science

	New Starts		Near-Term Strategy					Long-Term Goals							Mission Totals
Fiscal Year:	76	77	78	79	80	81	82	83	84	85	86	87	88	89	
LUNAR AND PLANETARY															
Mercury															
Mariner Mercury orbiter	–	–	–	–	–	6	28		(99)						133
Venus															
Orbiting imaging radar	–	–	–	–	–	26	86		(93)						205
Moon															
LUNAR POLAR ORBITER		***4***	***13***	***13***	***9***	***2***	***1***								***42***
Mars															
VIKING EXTENSION	–	***10***	***10***	–	–	–	–								***20***
Mariner orbiter	–	–	–	11	32	62	28		(36)						169
Pioneer penetrator	–	–	–	–	–	–	7				(71)				78
Surface Sample Return	–	–	–	–	–	–	–					(1000)			1000
Jupiter/Uranus															
PIONEER JUPITER ORBITER WITH PROBE[b]	–	***6***	***29***	***43***	***51***	***26***	***4***		***(17)***						***176***
Mariner Jupiter orbiter	–	–	–	–	–	–	–			(278)					278
MARINER JUPITER URANUS[b]		***16***	***64***	***41***	***16***	***5***	***7***		***(31)***						***180***
Saturn															
Mariner orbiter	–	–	–	–	–	–	–		(282)						282
Comets															
Mariner Encke	–	–	5	27	34	15	–								81
Mariner Tempel II	–	–	–	–	–	–	–		(177)						177

ASTRONOMY									
Optical/ultraviolet									
LARGE SPACE TELESCOPE (2-m class)	3	***24***	***69***	***98***	***45***	***30***	***35***		***304***
Solar									
SOLAR MAXIMUM[c]		***9***	***23***	***16***	***9***	***8***	***10***		***75***
Sortie Instruments	–	–	–	2	10	10	10		32
Infrared									
SURVEY SATELLITE[d]		***2***	***12***	***23***	***6***	***2***	–		***45***
Cryogenic Telescope (1 m)	–	–	–	5	15	25	22		67
High Energy									
X-ray telescope (1.2 m)	–	–	2	5	10	25	25		67
GAMMA-RAY SATELLITE—100 MeV		***4***	***8***	***11***	***2***	***1***	–		***26***
Cosmic-ray and gamma-ray free-flyer	–	–	–	2	6	16	16		40
LAMAR (large-area x-ray) telescope	–	–	–	1	5	10	20		36
Sortie instrumentation	–	–	2	5	10	10	10		37
SPACE PHYSICS									
ELECTRODYNAMIC SATELLITES		***3***	***22***	***23***	***8***	***5***	***3***	*(1)*	***65***
Solar-wind Explorer	–	–	2	7	9	2	1	(1)	22
Atmospheric Explorer	–	–	4	21	19	5	4	(3)	56
AMPS (no cost figures available)									
Magnetospheric multiprobes	–	–	–	–	6	21	25	(8)	60
INTERDISCIPLINARY									
Out-of-the ecliptic	–	–	–	2	33	31	16	(11)	93
FY TOTALS (in FY 74 dollars)	3	78	265	356	335	341	358		
FY TOTALS (inflated)[e]	3	91	324	455	449	480	530		
OSS PLANNING WEDGE	3	113	247	396	400	490	580		
DIFFERENCE	0	22	–77	–59	–49	10	50		

[a] Conventional launch vehicle costs are included in mission figures where appropriate. Shuttle transportation costs are *not* included.
[b] See text for discussion of Mariner Jupiter Uranus and Pioneer Jupiter orbiter new start.
[c] See text for discussion of strategy recommended for solar astronomy.
[d] See text for discussion of strategy recommended for infrared astronomy.
[e] Assuming 5% annual inflation rate.

planning wedge presently adopted by NASA's Office of Space Science.

The Board has examined in detail the new starts proposed for fiscal years 1976 and 1977 and finds that each is well conceived and of first-class scientific importance. Details of these missions and their scientific rationale are to be found in Part II of this report.

The Board has further examined the near-term strategy represented by missions proposed for new starts for 1978 through 1982 and the long-term goals represented by new starts proposed for 1983 and later. This strategy and these goals are fully endorsed by the Board, with those reservations expressed below and in subsequent findings and recommendations.

We *recommend* that, fiscal constraints permitting, the new starts indicated in italics in Table 1 be undertaken in fiscal years 1976 and 1977 and that NASA adopt the near-term strategy and long-term goals indicated as a basis for future planning.

The Working Papers in Part II of the report contain additional mission recommendations, which have not been examined in detail by the Board but which could form the basis for an expanded program were the present funding constraints on the Office of Space Science to be alleviated.

The fiscal year 1976-1977 recommended new starts have funding patterns stretching over several years that exceed the OSS planning wedge in subsequent fiscal years. While the precision with which funding requirements can be estimated and the confidence one has in budgetary ceilings are both uncertain, the excess is substantial—$185 million—and illuminates a conflict that will have to be resolved at an early stage.

From the many options for stretching, slipping, and eliminating various missions, we identified four ways in which the excess could be eliminated:

1. Eliminate or slip for four years Lunar Polar Orbiter, Out-of-the-Ecliptic, Solar Maximum Mission, and Electrodynamic Satellites.
2. Slip the Large Space Telescope three years.
3. Slip Pioneer Jupiter Orbiter four years.
4. Eliminate Mariner Jupiter Uranus (slippage is not possible because of limited launch opportunity).

We reject option (1) on the ground that too much harm is done to too many programs. We favor a structured space-science program involving a mixture of uniquely important large missions (e.g., LST), cost-effective major missions (e.g., Pioneer and Mariner), low-cost Explorers, ground-based observatories, and laboratory programs. We regard it as unwise to base a program on a few large missions at the expense of the foundations upon which they must be constructed. Our views are further articulated in Chapter 4. We are therefore unanimous in rejecting this solution to the funding problem.

We also reject option (2). The availability of the Space Shuttle has enhanced the Board's support for large permanent astronomical space observatories. In accordance with the views expressed in Section VI, below, we believe that the LST should be a 1976 new start.

The Board concludes, reluctantly, that two large new starts in the outer planets program are not consistent with the presently envisaged fiscal constraints placed on the agency. (See Section IX, below, for further findings.)

III. THE EXPLORER PROGRAM

Explorer satellites have a long record of achievement in space physics and astronomy. These relatively inexpensive spacecraft permit much faster reaction times than larger satellites, allow greater flexibility in planning, and provide more opportunities for international cooperation. Sometimes, somewhat larger spacecraft than those that can be launched on Scout rockets are included in the Explorer program; the Atmospheric Explorers constitute an example. The Explorer program is normally considered a level-of-effort program, currently running about $33 million per year, although increases above this level have at times been made to accommodate the occasionally larger program.

The Space Science Board has continually stated the importance of this program, which supports new astronomical initiatives and much of the terrestrial space-physics program.

We believe that, even within a level OSS budget, it is important to increase the Explorer budget to offset inflation and to build on the progress already achieved.

IV. OUT-OF-THE-ECLIPTIC MISSION

An out-of-the-ecliptic mission is supported by all three disciplines: space physics, space astronomy, and planetary and lunar exploration. The Board endorses the scientific rationale for this mission to expand our direct knowledge of the sun and interplanetary space from two to three dimensions. We recognize that the solar wind, the interplanetary magnetic field, and the density of gas and dust are likely to be quite different away from the ecliptic. We also see an out-of-the-ecliptic mission as possibly providing direct access to nearby unmodulated cosmic rays, thus looking into interstellar space.

Although out-of-the-ecliptic missions are particularly attractive and have endorsement both from space physicists and space astronomers, more information is needed in order to establish their proper priority. A study of methods of achieving highly inclined orbits will indicate the extent to which solar-electric propulsion (SEP) can achieve solar latitudes more than 65° and the degree to which Jupiter encounters can provide the desired orbits.

If it is practical to place large payloads in highly inclined orbits, several new types of observation become possible: a polar view of solar features (e.g., solar streamers) could be obtained, and observations of very faint objects in the universe (e.g., extended wings of galaxies) should be facilitated by virtue of the reduced sky background. Much of the sky background in earth orbits is due to zodiacal light, i.e., sunlight scattered off dust particles concentrated in the ecliptic plane. Finally, the plasma frequency may be much reduced, thereby allowing radio-astronomical observations of the universe at much lower frequencies than is possible in the ecliptic plane.

We conclude that more comparative studies of Jupiter encounters and SEP should be made with respect to this mission so that the Board can better assess scientific output in relation to cost for these two alternative means of achieving high-inclination orbits.

V. SOLAR ASTRONOMY

Solar astronomy is an active discipline in which many important problems are being attacked. A large quantity of exceedingly valuable data was obtained from the Apollo Telescope Mount (ATM) on Skylab. The Solar Maximum Mission (SMM) is designed to move forward from the base provided by Skylab, taking particular advantage of the increased opportunities to observe flare phenomena near solar maximum.

Skylab ATM imagery combined with high spectral resolution has produced fundamentally new information on the structure of the solar atmosphere and its dynamical processes and the characteristics of solar flares. SMM is an appropriate transition from Skylab to Shuttle-supported operations, since it has the desired compatibility with Shuttle refurbishment operations. Its instrumentation should exceed substantially the performance of ATM in temporal resolution of spectral and spatial variations. Instrumentation is available for high-speed spectroscopy, and TV imagery can be adapted from systems already developed for other space programs such as the Earth Resources Technology Satellite (ERTS). With these capabilities it seems assured that SMM will markedly improve our understanding of solar flares and other plasma-physics phenomena revealed in solar activity.

Coupled to laboratory studies of laser-beam-generated, high-temperature plasmas, the study of solar plasma behavior should contribute to progress in fundamental plasma physics.

Priorities for solar astronomy cannot be assigned from a single point of view. The sun is the only star that we can observe in detail, and it is the dominant source of thermal and chemical energy for the earth, with all that this implies with respect to problems of agriculture, energy, climate, and other areas of environmental concern.

We conclude that it is time to assemble a group of scientists to review progress being made in solving the major problems of the sun, both in relation to fundamentals of astronomy and to terrestrial interactions, and to recommend priorities for instrumentation and mission objectives. The study group should include not only solar astronomers but also workers in related areas such as particles and fields, interplanetary physics, the magnetosphere, the ionosphere, and meteorology. It should also contain some nonsolar astronomers, particularly stellar astronomers. This review should precede the final structuring of the SMM.

VI. THE LARGE SPACE TELESCOPE

The Space Science Board strongly endorses the Large Space Telescope (LST) as a new start in fiscal year 1976. A space telescope in the 2- or 3-m class will be an exciting major step in the history of observational astronomy, observational cosmology, and the understanding of the universe. While we recognize the desirability of a 3-m telescope, fiscal considerations appear to limit us to an instrument of the 2-m class. Such an instrument will represent an order-of-magnitude increase in capability over the best existing ground-based telescopes and can accomplish most of the science anticipated from the 3-m instrument, with only an increase of observing time.

The long lifetimes anticipated for LST and for other permanent observatories proposed for space astronomy draw attention to the institutions that will be needed to ensure their proper use. The LST will probably need ground-support facilities comparable with or larger than those of existing national observatories. Creating such facilities raises many problems, the most immediate of which will be competent staffing. While we intend to keep this matter under review, it should also be the concern of bodies representing the astronomical community.

VII. INFRARED AND MILLIMETER ASTRONOMY

The broad infrared spectral region between the few micrometer and 1-mm wavelengths is a realm rich for astronomy, which displays its own special phenomena and also sheds light on almost all aspects of astronomy. While there has been rapid and spectacular development of infrared astronomy in recent years, the absorption of much of this radiation by the earth's atmosphere necessitates work in space for its proper exploitation.

A survey satellite, which would detect infrared radiation from astronomical objects with adequate sensitivity and at a variety of wavelengths, is the logical first major step in infrared space astronomy. In order to clarify the appropriate strategy and timing for such a survey promptly enough for a firm decision on a satellite mission, a study is recommended during the coming year that would examine the general design of such a mission and the state of development of the required technology and would compare the effectiveness and cost of alternative approaches to infrared astronomy. Aspects that should be examined in connection with the study of a survey satellite mission include

1. The quantitative comparison of a survey by satellite with surveys from the ground, from balloons, from rockets, and from airplanes such as NASA's C-141 observatory.
2. Possible increases in efficiency of the above methods of obtaining infrared observations by improvement of far-infrared detectors and by decrease of telescope radiation.
3. The state of technology for long-lived cryogenic cooling in space.

4. The efficiency and reliability of detectors for the very-long-wavelength infrared region and their likely rate of improvement.

The Board proposes that a study of goals and priorities in space infrared and millimeter astronomy be undertaken in 1975.

The Board's recommendation for a new start in fiscal year 1977 for an infrared survey satellite (see Section II, above) is conditional upon endorsement by such a study.

VIII. CONTINUING EXPLORATION OF MARS

The Viking 1976 Mars landers are designed to place a complex laboratory on the surface of the planet to perform *in situ* measurements on the soil and the atmosphere. At present, Viking mission planning provides a nominal 90-day postlanding period for operation of the surface and orbital experimental payloads. To cover operational costs and additional data analysis beyond the 90-day period, additional funds, termed Viking Extension, have been proposed in NASA's planning effort. The Board endorses the Viking Extension, at cost levels shown in Table 1, as a means of exploiting success in the mission.

The Viking approach is appropriate at the present state; however, instrumentation transported to Mars can never equal that available in a terrestrial laboratory, particularly for the life sciences. The Space Science Board is of the opinion that the long-term objectives of exobiology and surface-chemistry investigation are best served by the return of an unsterilized surface sample to earth. Available information points to the possibility that a verifiable system for isolation of the sample from the terrestrial environment can be developed at reasonable cost.

We, therefore, *recommend* that Mars surface-sample return (MSSR) be adopted as a long-term goal and that an early start be made on research and development into a verifiable system of sample isolation.

Interesting results from Viking 1976 can be fully used in the design of MSSR. There should not, however, be a gap in scientific investigations between these two large missions. Many fundamental and preparatory investigations can be made with existing Pioneer and Mariner spacecraft, and a cost-effective medium-range program can be based on them.

We *recommend* that a Mariner polar orbiter mission and Pioneer survivable hard lander and probe mission be undertaken in the period 1978 through 1983.

The Space Science Board has been urged by its Committee on Planetary and Lunar Exploration to recommend that the option of a Viking 1981, based on modifying an existing spacecraft, be kept open until the Viking 1976 results are in. In itself this seems a prudent course of action; however, it is our impression that major costs may be involved, which might eliminate or seriously delay other proposed missions in this period. Given the strategy described in the above recommendations, we see no missions in the recommended mission model (Table 1) that should be displaced to meet such option costs.

IX. EXPLORATION OF THE OUTER SOLAR SYSTEM

The Space Science Board has reached the conclusion that fiscal constraints do not permit the early start of two large programs (Pioneer Jupiter Orbiter and Mariner Jupiter Uranus) for exploration of the outer solar system and believes that the strategy for outer-planets exploration needs to be reassessed.

We, therefore, *recommend* that NASA undertake an immediate re-examination of the strategy for exploration of the outer solar system during the next decade.

We further request the SSB Committee on Planetary and Lunar Exploration to *recommend* to the Board for its 1975 revision of this study a program consistent with the anticipated fiscal constraints.

4
Rationale for Assignment of Priorities

How does a group of disparate—if cooperating—specialists arrive at a judgment of relative priorities in scientific experiments? Their decisions involve the comparison not merely of substances as unlike as chalk and cheese but of topics much more distinct, say, photons and planets. We should like to set down some of our criteria, not explicitly as in any organizing document but as they in fact arose in discussion itself. This requires some analysis of the implications of our work; it seems therefore helpful not merely to extract the principles from discussion but also to cite an example or two, so that the reader may assess the fairness of the conclusion.

I. BALANCE AND CONTINUITY

The phrase "balance and continuity" may adequately describe the most used of all criteria. It serves to remind us that science is not a human activity without parallel, for in most human plans such a criterion must be employed. Momentum is a real property of social as well as physical systems. Plainly, this speaks to the danger of self-serving, but just as plainly it is inevitable that these considerations must play a role in any practical planning. For science, even in its search for novelty, is an ongoing enterprise. Results will never appear unless there are skilled and devoted people, adequate instruments, problems set by previous knowledge, and whole disciplines of study that are based on past results. It is no use to propose experiments if no one is interested in carrying them out or if no one is competent to make the measurements or use the results. The body of existing disciplines must play a role in plans for the future. Thus, the

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This chapter was written by SSB member Philip Morrison.

Solar Maximum Mission or the x-ray telescope gain a place in our plans because there are powerful groups of experimenters who can use and will use well such opportunities and who cannot work without them. A gap of many years in such a series of experiments would waste a resource even more precious than the NASA budget. So much we share with every group that plans for the future of persons and organizations.

At the same time, exactly because of this strong parallel, we cannot find in this argument alone any justification for the future of any mission. If no problem exists or no means for solution is present or no novelty seems within reach, mere continuity would not serve for justification. Thus in the cases cited above, we were convinced that the mechanism of solar flares or of x-ray emission in the clusters of galaxies were real problems, open to likely progress by the missions chosen. For example, only one moon mission is on our list, even though in the past a large scientific community has engaged in the study of the moon.

II. THE BATTLE AGAINST NATURE

The work of science is less a marketplace of competing advocates and purveyors than it is a battlefield, with nature as the tireless, fair, but sometime capricious adversary. It is natural to use metaphors of military origin (although we are happy in the realization that victory and defeat in science, while perhaps not bloodless, are not destructive). In this vein, then, we can outline a number of considerations that play a large part in the decisions we make. They relate first to the exploitation of gains.

1. Salients. Sometimes the state of knowledge and power has made a recent advance to form a salient in the front of our ignorance (here the more general form of the cliche is *breakthrough*). It is then clear that such a field will repay more attention; the objectives are known, the means in hand, and we go on to a deeper picture. X-ray astronomy of compact sources, say, is such an area. Thus, the HEAO experiments build upon the proved results of *Uhuru* and give a high priority to x-ray studies designed for faster time resolution and repeated passage across sources.

2. Additions to the Arsenal. Here we have not new results but new weapons for science–instruments or techniques that promise light. Indeed, space science itself is such a case overall. We might give as examples the cryogenic infrared telescope, opening a new wide channel in astronomy in which we have only slight knowledge of what we might see but the conviction that we can see well in a new channel. Here we may well try to use the instrument first in an economical context, say balloons, as an initial step in its exploitation. A much grander example is LST, for an optical telescope, free of the atmospheric blurring of every previous large telescope, is a new way of seeing. We can outline numerous problems that it should be able to advance.

3. New Intelligence. Sometimes a finding gives us a mere glimpse at some quite new and unexpected problem. It is not that we have made some clear progress but rather that we have opened some new door into a room, still dark, but inviting. Take the active magnetosphere of Jupiter, with its tantalizing suggestion of pulsarlike behavior. It is clear that we want to know more. Or take the brief and little known gamma-ray bursts. A satellite to accumulate more data about these events is a direct extension of the limited intelligence that we have about this surprising feature of the stars (if stars they are).

4. Consolidation of Newly Occupied Territory. The goal of science, following the stages of discovery and exploration, is to achieve a quantitative understanding of the physical processes governing the system under attack. Whereas much of the astronomical and planetary programs recommended in this report involves discovery and exploration, space physics, notably the physics of the earth's outer environment, is in a state where quantitative understanding is within our reach.

III. GENERALITY OF INTEREST

Sometimes we cannot say what is to be found. This is partly true in almost every case. But with the Uranus missions, for example, it is all too true. We could see a strange magnetospheric shock front when the solar wind hits the pole–if in fact the planet has an appreciable magnetic moment. LST, on the other hand, will answer all sorts of questions, over many wavebands and for many types of objects, from planets to forming galaxies. Here we use a criterion of generality; faced with ignorance, hoping for answers we do not have at hand, how many fields, how large a domain of knowledge will the results apply to? In a way this criterion is at war with the ones in the previous section. We invoke it when we are sure of the measurements but less sure of the objects of investigation. Here the idea is not the exploitation of gains but the width of interest and applicability—within science—of what the instruments might see.

IV. RIPENESS

Ripeness is not all, but it is something. A mission must be based in general on expectations, on previous knowledge (or lack of it). When the mission clearly would undergo a considerable change in plan or importance were a prior and easier to obtain result already at hand, it seems prudent to defer it until it is ripe. This argument might be used—not decisively—against the infrared survey satellite, on the grounds that much more might be learned about the channel from aircraft and balloon flights before the design of an effective orbital spacecraft could be carried out.

V. EXTRASCIENTIFIC CONSEQUENCE OF SCIENCE

Even for the SSB, science cannot be taken to exist outside of the wider context. Applications of science to direct practical concerns, and the impact of scientific results on such deep human interests as the origins and ends of the solar system and the universe, which are clearly not practical at all but philosophical, cannot be neglected. They do not always loom large in our discussions, for other agencies hold final responsibility, and our charge is with science itself. Nevertheless, it is clear to us, and it received much mention, that the sun is our star (or rather the earth is its planet) and solar changes mean more to us than similar phenomena on another star. This adds a long-term but intensely practical interest to all solar missions; will we foresee an ice age if we know the solar-terrestrial interaction in all its detail? Or take Viking. The chance of life on Mars is perhaps not so very great. But the impact on the minds of all human beings, not merely on biologists, of the demonstration of such life is too evident and too important to neglect. This has in fact fueled much of the strong push toward the Viking 1976 landings on Mars.

One more example: images. The well-defined numerical result of measurement is easier for the scientist to wax enthusiastic about than for the outsider. But imagery with its wealth of information, in that very form to which human eye and mind have evolved, makes a real impact on anyone. The pictures of the vulnerable earth from orbit are justly famous; an albedo measurement would never take their place. Even the scientist, who must recognize a wide exploration as part of his growing knowledge as surely as a good number in one instrumental channel, gains from images. But even if his narrow case were clear against the image, there would be a loss in always choosing the more austere alternative. Generalizing, this idea leads sometimes to the choice of chancy missions, where we know only that we do not know. Not exactly parallel, the out-of-the-ecliptic studies are of a similar kind. They must, in the end, be done, lest we grow narrow. True, they suffer often—and they did here—by deferment under severe constraints of cost.

This provides no full account of our work; it is hard to tease every thread of rationale out of these complex discussions, but it is a beginning picture. Clearly, against these broad ideas we try to argue away all the obvious conflicts of interest and concern that we feel, not generally of any narrow self-serving, fiscal, or even corporate nature but in a wider intellectual field. It is a source of satisfaction that in our discussions we gain wide consensus, and that, as individuals, we have an appreciation of disciplines other than our own. Cosmologists will respond to Jupiter probes, and radio astronomers to the study of gases on Mars. Here resides the strength of the agreement that we finally reach.

II
Working Papers

Introduction

The following documents are reports to the Space Science Board from its disciplinary committees:

- Committee on Space Astronomy
- Committee on Space Physics
- Committee on Space Biology and Medicine
- Committee on Planetary and Lunar Exploration

The Board has reviewed these documents and accepts them as working papers for the present study. Recommendations in these papers are recommendations *to the Space Science Board*. Where they have been adopted by the Board as recommendations to the National Aeronautics and Space Administration and other bodies it is so stated in Part I.

The Space Science Board is deeply indebted to its disciplinary committees and particularly to the chairmen, Robert Danielson, Francis Johnson, Robert Phinney, and John Shepherd, for the efforts that they have made to present their cases to the Board in a constructive framework. The Board, therefore, wishes to record their deliberations without modification.

In a more optimistic fiscal climate, the programs put forward by these Committees might well be endorsed in entirety by the Board. Because of the limited opportunities anticipated in the next few years, some difficult interdisciplinary comparisons must, however, be made, and Committee recommendations cannot be accepted without the modifications described in Part I of this report.

A special study of the "Future Exploration of Mars" was requested by NASA. It was possible to schedule this study so that it would be an input to the final deliberations of the Committee on Planetary and Lunar Exploration, and eventually to the Board's

October 1974 meeting. The report of this study is included here as a working paper, but it can stand alone as a separate report. Some conclusions of the Mars study have been revised in the report of the Committee on Planetary and Lunar Exploration and by the Space Science Board in the perspectives of space science in general.

Committee on Space Astronomy

Robert E. Danielson, *Chairman*
E. Margaret Burbidge
George R. Carruthers
George W. Clark
William A. Fowler
William F. Hoffmann
Frank J. Kerr
Robert M. MacQueen
Philip Morrison
J. B. Oke
P. Buford Price
Vera C. Rubin
Blair D. Savage

Ann Grahn, *Executive Secretary*
Richard Berendzen, *Staff Consultant*
Richard Hart, *Staff Consultant*

A
Space Astronomy

I. INTRODUCTION AND SUMMARY

This report presents the goals and priorities in space astronomy as recommended by the Committee on Space Astronomy (CSA) of the Space Science Board. The CSA was greatly assisted in its work by more than 40 thoughtful letters from the scientific community, which evaluated the first draft of this report.

As terms of reference, the CSA adopted the following fiscal planning wedge:

	Fiscal Year				
	1976	1977	1978	1979	1980
	($ millions)				
OSS predicted budget	597	571	570	665	665
Run-out and ongoing costs	594	458	323	269	265
Planning wedge	3	113	247	396	400

The planning wedge is the estimate of funds available for new missions in the Office of Space Science (OSS). The planning wedge does not include the funding for Explorer satellites, supporting research and technology (SR&T), or balloons, rockets, and aircraft; these are included in the ongoing costs.

Section II of this report presents funding models for new astronomy missions at levels of 50, 67, and 100 percent of the OSS planning wedge. The overall strategy and rationale underlying these models is summarized. All the costs of individual missions are in fiscal year 1974 dollars. An annual inflation of 5 percent is applied to the yearly totals to conform with the inflated dollars given in the OSS wedge. It should be noted that no new starts are indicated after fiscal year 1979. Hence expenditures in fiscal years 1980–1982 represent run-out costs only.

Foremost of the recommended missions is the Large Space Telescope (LST). Other highly recommended missions are the Solar Maximum Mission, Infrared Survey Satellite, 1.2-m X-Ray Telescope, and Hard Gamma-Ray Satellite. Together with instrumentation for the Shuttle sortie mode, particularly for cosmic-ray astronomy, these missions comprise the key components of the 50 percent planning wedge. Achieving this degree of balance in the 50 percent wedge seems possible only if the LST is in the 2-m class. The 67 percent mission model can accommodate a full-aperture LST (3-m) within a balanced program of studies in all branches of astronomy. The 67 percent model includes two additional high-priority missions, an Ultra-Long-Baseline Radio Interferometer and an Out-of-the Ecliptic Mission. It also contains increased funding for Shuttle sortie-mode instrumentation.

Section III consists of descriptions of all the missions proposed in this report. They are listed in the same order as in the 100 percent mission model.

Section IV briefly describes some extremely important baseline activities in space astronomy: Explorers, rockets, aircraft, and balloons. The continuing tendency to reduce the support of these activities is very unfortunate. We believe that they will continue to be highly cost-effective vehicles for space science for a long time to come, and we urge that their funding be increased at least to compensate for the effect of inflation over the past few years.

At the time of this writing, we see indications that many excellent Explorer proposals are being submitted in response to a recent NASA Announcement of Planning Opportunities (APO). To avoid biasing the Explorer selection, we do not recommend priorities among possible Explorers that may be proposed. Section IV lists a few candidates that have received considerable discussion. Of these, an Explorer to detect gamma-ray bursts seems to draw the most interest. (The Infrared Survey Satellite, which may be considered for an Explorer mission, is described separately in the mission models in Section III.C.1.)

An increase in the NASA budget for SR&T is needed to ensure the most efficient use of the capabilities of Shuttle and Shuttle-borne instrumentation. Especially needed is a higher level of support for development of new detectors.

In Section V, we suggest that the longer-range future of space astronomy may be well served by using the Space Shuttle to help assemble very large astronomical observatories in orbit. Other long-range directions deserving detailed consideration include telescopes in synchronous orbit to increase the fraction of time available for observing and large radio telescopes in lunar orbit to reduce terrestrial interference. In the next century it may be desirable to locate radio telescopes on the far side of the moon. It may be that through such efforts space astronomy will ultimately provide the means for detecting intelligent life outside the solar system, a profoundly significant discovery.

II. MISSION MODELS

This section presents the overall strategy and considerations that led to the mission models proposed by the Committee on Space Astronomy. These models are given in Tables A.1–A.5. Tables A.1 and A.2 describe the approved astronomy programs; Tables A.3–A.5 present the 50, 67, and 100 percent mission models, respectively. The scientific rationale for the individual missions is given in Section III.

Space astronomy tends to group itself into disciplines according to wavelength intervals: high-energy astronomy (cosmic rays, gamma rays, and x rays); optical and ultraviolet astronomy; and infrared and radio astronomy. Solar astronomy is regarded as a fourth discipline by virtue of the unique character of our sun.

A balanced program in space astronomy requires that each of these four disciplines continue to make good progress in under-

TABLE A.1 Flight Schedule for Approved Astronomy Programs

	Calendar Year					
	1974	1975	1976	1977	1978	1979
Small Astronomy Satellite		C				
ANS (Netherlands)	X					
United Kingdom #5	X					
Orbiting Solar Observatory		I				
HEAO				A	B	C
International UV Explorer (IUE)			X			

TABLE A.2 Estimated Funding Required to Support Approved Missions in Space Astronomy (in Millions of Dollars)

	Fiscal Year				
	1976	1977	1978	1979	1980
Supporting Research and Technology	9.8	9.8	9.8	9.8	9.8
Data analysis	1.9	1.9	1.9	1.9	1.9
Skylab data analysis	5.8	4.4			
Sounding rockets	12.9	12.9	12.9	12.9	12.9
Spacelab experiment definition	2.5	2.5	2.5	2.5	2.5
LST–definition and technology	8.0				
OSO	2.6	0.4			
OAO	2.9	1.1	0.2		
HEAO	45.5	32.0	17.1	10.1	2.8
Explorers[a]	16.5	16.5	16.5	16.5	16.5
Balloon support	0.8	0.8	0.8	0.8	0.8
Airborne research	3.4	3.4	3.4	3.4	3.4
TOTAL	112.6	85.7	65.1	57.9	50.6

[a]The total Explorer budget for both physics and astronomy is about $33 million/yr. The figures in this table are for astronomy only.

Explanatory notes for Tables A.3–A.5

Scope: Three different mission models are presented depending on the portion of the OSS budget allocated to space astronomy. These models represent 50%, 67%, and 100% of the OSS "planning wedge"–the estimated amount of the OSS budget that will be available for new initiatives in all disciplines, i.e., not committed to approved programs.

Timing: All mission models begin with new starts for fiscal year 1976 but contain no new starts after fiscal year 1979. Figures after fiscal year 1979 represent *run-out costs only.*

Inflation: The figures in the tables are expressed in terms of constant fiscal year 1974 dollars. To make some allowance for present economic realities, a constant rate of inflation of *5% per year* has been included in the totals of each fiscal year expenditure.

Source of figures: The mission costs have been provided by the Physics and Astronomy Programs Office of the NASA Office of Space Science; C.R. O'Dell, Project Scientist (LST), Marshall Space Flight Center; the NASA Ad Hoc Planning Group of the High Energy Astrophysics Management Operations Working Group.

Total costs: The figures in the total cost column represent the development costs; operating expenses (abbreviated ops) are added where appropriate. These operating expenses frequently include ground support, instrument updating, and refurbishment. Certain missions, which represent continuing experiments to be flown on the Space Shuttle, have no definite total cost. They are designated by a cost per year, e.g., the total cost for the Solar sortie instruments is given as $10/yr (in millions of dollars).

Launch vehicles: The cost of the launch vehicle is not included in the tables. For the three rocket-launched missions (Solar Maximum Mission, IR Survey Satellite, High-Energy Gamma-Ray Satellite) the cost is $5.3 million each. For the rest of the missions, which are Shuttle-launched, costs are not available.

TABLE A.3 Space Astronomy Mission Model (in Millions of Dollars) 50 Percent of Planning Wedge[a]

	Fiscal Year							
Projected (New Starts)	1976	1977	1978	1979	1980	1981	1982	Total Cost
Optical and Ultraviolet Astronomy								
Large Space Telescope (2-m)	3	24	69	98	45	30	35	264 + ops
Solar Astronomy								
Solar Maximum Mission	8	19	16	9	8	10	10	55 + ops
Solar sortie instruments				2	10	10	10	10/yr
Infrared Astronomy								
ir survey satellite		2	10	20	6	2	–	40
ir cryogenically cooled telescope (1-m)				5	15	25	22	60–80 + ops
High-Energy Astronomy								
X-ray telescope (1.2-m)			2	5	10	25	25	88 + ops
High-energy gamma-ray satellite		4	6	8	2	1	–	21
High-energy free-flyers				2	6	16	16	60–80
LAMAR, large-area x-ray telescope				1	5	10	20	83 + ops
High-energy sortie instruments	—	—	2	5	10	10	10	10/yr
TOTAL	11	49	105	155	117	139	148	
Total with 5% Inflation	12	57	128	198	157	196	219	
50% OSS Planning Wedge	2	56	124	198	200	200	200	

[a]See Explanatory Notes on p. 36.

TABLE A.4 Space Astronomy Mission Model (in Millions of Dollars) 67 Percent of Planning Wedge[a]

	Fiscal Years							
Projected (New Starts)	1976	1977	1978	1979	1980	1981	1982	Total Cost
Optical and Ultraviolet Astronomy								
Large Space Telescope (3-m)	3	29	84	98	67	45	33	349 + ops
Optical and uv sortie telescope (1-m)			4	10	20	12	7	50–60 + ops
Solar Astronomy								
Solar Maximum Mission	8	19	16	9	8	10	10	55 + ops
Solar sortie instruments			2	10	10	10	10	10/yr
Infrared and Radio Astronomy								
ir survey satellite		2	10	20	6	2	–	40
ir cryogenically cooled telescope (1-m)				5	15	25	22	60–80 + ops
Ultra-long-baseline interferometer			2	10	20	15	11	60
High-Energy Astronomy								
X-ray telescope (1.2-m)			2	5	10	25	25	88 + ops
High-energy gamma-ray satellite		4	6	8	2	1	–	21
High-energy free-flyers				2	6	16	16	60–80
LAMAR, large-area x-ray telescope				1	5	10	20	83 + ops
High-energy sortie instruments			5	10	15	15	15	10–15/yr
Cooperative Missions								
Out-of-the-ecliptic mission	—	—	4	7	8	4	1	24
TOTAL	11	54	135	195	192	190	170	
Total with 5% Inflation	12	63	165	250	257	268	252	
67% OSS Planning Wedge	2	75	165	264	267	267	267	

[a]See Explanatory Notes on p. 36.

TABLE A.5 Space Astronomy Mission Model (in Millions of Dollars) 100% of Planning Wedge[a]

Projected (New Starts)	Fiscal Year 1976	1977	1978	1979	1980	1981	1982	Total Cost
Optical and Ultraviolet Astronomy								
Large Space Telescope (3-m)	3	29	84	98	67	45	33	349 + ops
Optical and uv sortie telescope (1-m)		4	10	20	12	7	7	50–60 + ops
uv survey sortie telescope			3	9	15	10	7	30–50 + ops
Solar Astronomy								
Solar Maximum Mission	8	19	16	9	8	10	10	55 + ops
Solar sortie instruments			2	10	10	10	10	10/yr
Solar telescope cluster				8	22	40	58	150–200
Infrared and Radio Astronomy								
ir survey satellite		2	10	20	6	2	–	40
ir cryogenically cooled telescope (1-m)			5	15	25	22	13	60–80 + ops
ir ambient-temperature telescope (3-m)			4	10	20	15	7	40–60 + ops
Ultra-long-baseline interferometer		4	10	20	15	11	–	60
High-Energy Astronomy								
X-ray telescope (1.2-m)		5	10	25	25	15	5	88 + ops
High-energy gamma-ray satellite		4	6	8	2	1	–	21
Cosmic-ray free-flyer			8	16	16	10	3	54
Gamma-ray free-flyer			9	24	22	13	5	80
LAMAR, large-area x-ray telescope				5	10	20	20	83 + ops
High-energy sortie instruments		5	10	15	15	15	15	15/yr
Cooperative Missions								
Out-of-the-ecliptic mission			4	7	8	4	1	24
TOTAL	11	72	191	319	298	250	194	
Total with 5% Inflation	12	84	233	408	399	352	287	
100% OSS Planning Wedge	3	113	247	396	400	400	400	

[a]See Explanatory Notes on p. 36.

standing and solving the key scientific problems that motivate them. However, a balanced program does not necessarily mean that each discipline receives equal resources at a given time. The relative resources that are allocated to a discipline should depend on many factors including the following:

1. The availability of new instruments and techniques in the discipline;
2. The degree of theoretical understanding of existing data in the discipline;
3. The prospects of rich scientific yields emerging from further observations;
4. The possibility for unexpected discoveries emerging from further observations;
5. The availability of scientific and technical personnel to carry out the programs;
6. The degree to which progress in the discipline affects progress in other areas of science.

We discuss each of the disciplines in the following paragraphs.

A. Optical and Ultraviolet Astronomy

The Space Shuttle will permit large, *permanent* observatories in orbit. Foremost among these observatories is the Large Space Telescope (LST). In the few years since the publication of the Greenstein report,* the LST has moved to first place in priority among the large space astronomy projects under consideration by the astronomical community. This high priority is mainly due to three key developments. First, the Shuttle makes it possible to maintain observatories in space that would be too expensive if they could operate only for a very few years as smaller observatories such as OAO Copernicus do now. The second key development is the demonstration of technical feasibility of an LST as a result of the many studies that have been carried out by NASA, universities, and industry. The third key development is the prior initiation of the high-energy astronomy projects that were highly recommended in the Greenstein report, namely, the

**Astronomy and Astrophysics for the 1970's: Volume 1, Report of the Astronomy Survey Committee* (National Academy of Sciences, Washington, D.C., 1972).

High Energy Astronomical Observatory (HEAO)—although in substantially smaller design—and the ground-based Very Large Array (VLA).

Although there is a broad consensus that the LST merits the highest priority of all space astronomy missions, there is also a determination among astronomers that the LST should not jeopardize the achievement of a balanced program that exploits the many scientific opportunities that exist in all areas of space astronomy. In our best judgment, the 50 percent wedge represents about the minimum funding that can sustain a balanced program including an LST. In the 50 percent, however, the LST is in the 2-m class and has a development cost of $264 million in fiscal year 1974 dollars. Although such a reconfigured LST would be a magnificent observatory capable of obtaining many critical observations relevant to extragalactic research and cosmology, it would not provide the full range of observations possible with an LST in the 3-m class. The development cost of the latter observatory is about $349 million in fiscal year 1974 dollars; the 67 percent wedge is required to maintain a balanced program in this case.

We believe that a completely adequate scientific and technical justification exists to warrant a new start for an LST in fiscal year 1977.* However, a program of this magnitude requires an unusually thorough costing. Consequently, we *recommend* that at least $3 million be provided in fiscal year 1976 to gain an improved understanding of the scientific, fiscal, and technical tradeoffs. This will allow the most intelligent decisions to be made in fiscal year 1977.

As the first permanent orbiting observatory, the LST raises a new issue—the funding required to support ongoing operations. A ground-based laboratory generally requires about 10 percent of its capital costs to operate for a year; the same percentage appears to apply to the LST. Consequently, one must anticipate an annual operating budget of as much as $30 million for the LST program. A budget of this magnitude implies the employment of very substantial numbers of astronomers, engineers, technicians, and other supporting staff. Although the manpower is abundantly available, the best way to organize it is yet not clear (see the Greenstein report, Vol. 1, p. 106,

*The Space Science Board has recommended the LST as a new start in fiscal year 1976 in order to avoid any further delays of this important mission. It is expected that the definition of the exact size of the telescope will be completed during fiscal year 1975.

for one possible approach). Thus the optimal institutional framework for the LST program is an issue that should be widely discussed.

B. Solar Astronomy

Eloquently discussed in the Greenstein report, the solar flare is a spectacular phenomenon of great importance. The Solar Maximum Mission (SMM) will be specifically designed for solar flares studies, and we *recommend* this mission as a new start in fiscal year 1976* so that it can be launched during the next period of maximum solar activity when flares are best studied. Not only will the SMM possess a combination of temporal, spatial, and spectral resolution adequate to make substantial progress in understanding and defining the solar-flare phenomenon, but the basic spacecraft is refurbishable by the Shuttle. Consequently, the SMM ensures continuity of solar space astronomy into the Shuttle area.

The relative level at which solar astronomy should be supported is a controversial issue among astronomers. Some of the controversy stems from a degree of separation that exists between solar and nonsolar astronomers. Many nonsolar astronomers are unaware that good progress is being made in understanding one of the basic problems of astronomy, the mass and energy balance of the chromosphere and corona. On the other hand, solar astronomers have not clearly defined their priorities beyond the SMM. We believe it is urgent that the Space Science Board conduct a study to review progress being made in solving the major problems in solar astronomy and to recommend priorities for instrumentation and mission objectives. The participants in this study should include not only solar astronomers but also workers in related areas such as energetic particles and magnetic fields, the magnetosphere, the ionosphere, and meteorology, as well as some nonsolar astronomers. The results of this study of priorities and objectives in solar physics are required to make the best decisions on the direction of solar space astronomy after the SMM.

C. Infrared and Radio Astronomy

Infrared space astronomy has recently emerged as a vital and energetic discipline of space astronomy. That it has not emerged

*The Space Science Board decided that because of budget constraints the new start for this mission must be delayed until fiscal year 1977.

sooner is in great part due to the fact that outstanding progress was possible in this rapidly developing field by means of aircraft, balloon, and ground-based telescopes. Limitations due to atmospheric transmission and to telescope emissivity are now being reached, however, and they can only be overcome in space.

That the logical development of infrared space astronomy should begin with an unbiased survey is the overwhelming consensus of infrared astronomers. We share the consensus and *recommend* that an Infrared Survey Satellite be one of the highest priority missions in space astronomy. Our mission models include it as a new start in fiscal year 1977. We believe that the Infrared Survey Satellite may be as fruitful a mission as *Uhuru* was for x-ray astronomy.

The LST will be capable of following up the Infrared Survey Satellite, particularly when high spatial resolution is required. However, thermal emission from the telescope limits observations of extended sources. A moderate-sized (1-m) cryogenic telescope would overcome this limitation. It has strong scientific support from the scientific community, but there are serious reservations about its feasibility as a Shuttle sortie instrument in the presence of contamination from the Shuttle. This technical question is now being studied. Under the assumption that feasibility will be established, our mission models show the 1-m Shuttle sortie cryogenic telescope as a fiscal year 1979 start.

Rapid advances are now taking place in extending the techniques of millimeter-wave radio astronomy into the submillimeter region and improving infrared detectors in this same range. These developments, coupled with the rapidly expanding potential of infrared space astronomy, make it appropriate to review the status of these fields and to establish priorities for the future. Therefore, we *recommend* that the Space Science Board conduct a study to determine the goals and priorities in space infrared and submillimeter astronomy.

D. High-Energy Astronomy

High-energy astronomy is the study of extraterrestrial x rays, gamma rays, and cosmic rays. The spectacularly successful *Uhuru* Explorer has shown that x-ray astronomy is a richly rewarding field. Although HEAO-A and -B will greatly extend x-ray astronomy, its full potential will require permanent orbiting observatories. Therefore, we support the recommendation of the NASA high-energy astrophysics study* that the top priority mission in x-ray astronomy is a 1.2-m focusing x-ray telescope that can be maintained with the Shuttle. This observatory, being twice as large as HEAO-B, will have about ten times its sensitivity. Because it was previously planned for the *original* HEAO-C, its feasibility and costs are well established. As in the case of the LST, substantial funds (about $5 million/year) will be needed to operate the x-ray telescope in orbit. The institutional basis for this national facility also needs discussion. We *recommend* a new start in fiscal year 1978.

Gamma-ray astronomy has recently come into its own as an exciting subfield. The discovery by SAS-2 of unquestioned discrete gamma-ray sources and resolved emission from galactic spiral arms has provided the rationale for a high-energy gamma-ray experiment of much greater capabilities. We *recommend* such a satellite as a new start in fiscal year 1977. We also *recommend* that consideration be given to including a gamma-ray-burst detector as one of the next in the series of astronomy Explorers.

Improvements in detectors for cosmic-ray astronomy should make it possible to resolve individual isotopes of rare heavy elements and thus relate cosmic rays directly to astrophysical sources. Large instruments are needed to study extremely energetic nuclei, electrons, and positrons between 10^{10} and 10^{15} eV per nucleon and extremely heavy nuclei, possibly including superheavy elements. Because these types of measurement are well suited to the sortie mode, we *recommend* an early start on sortie payloads for cosmic-ray astronomy.

Several important missions could not be included in the 50 percent mission model. These include an out-of-the-ecliptic mission (as an adjunct to a Jupiter flyby) and the ultra-long-baseline interferometer (ULBI), which appear as fiscal year 1978 starts in the 67 percent mission model.

*Ad Hoc Planning Group of High Energy Astrophysics Management Operations Working Group, "A Program for High Energy Astrophysics, 1977–1988" (July 15–18, 1974). This draft, which discusses goals, priorities, and proposed missions, has been a valuable resource to the CSA for the present report.

III. INDIVIDUAL MISSIONS

A. Optical and Ultraviolet Astronomy

1. LARGE SPACE TELESCOPE

(a) MISSION DESCRIPTION The Large Space Telescope (LST) will combine for the first time both high angular resolution and large light-gathering power free of all atmospheric absorption and perturbation. The telescope will be nearly diffraction-limited so that a star image will have a diameter of 0.05 to 0.1 sec of arc; this can be achieved if the telescope diameter is 2 m or larger. At the same time, the largest possible diameter is desirable to maximize the rate at which astronomical observations can be made. Historically, the LST has always been conceived of as being a 3-m-diameter telescope. With recently developed very-low-background light detectors, however, a 2-m-class telescope can make the same observations as a 3-m telescope provided that background noise is produced only by the sky and the detector, but these observations will require twice as much time.

Several instruments will be mounted at the Cassegrain focus. Included may be the following: a high-resolution direct-imaging camera; a camera of somewhat lower resolution but larger field; a high-resolution spectrograph for use largely in the ultraviolet between 1000 and 3100 Å; a faint-object, low-resolution spectrograph covering the range from 1000 to 4000 Å or even possibly from 1000 to 10000 Å; a photoelectric photometer; a polarimeter; an infrared photometer; and a device for measuring accurate star positions for parallaxes and the like. In those instruments that require one- or two-dimensional imaging, either TV-type detectors or possibly solid-state arrays will be used.

The LST will be launched by the Shuttle and designed so that the telescope and auxiliary instruments can be refurbished from time to time to correct any breakdowns and to take advantage of new technology as it develops. The lifetime of the LST is expected to be at least 10 years.

(b) SCIENTIFIC RATIONALE The LST is comparable in light-gathering power with all but the very largest ground-based telescopes. It has the following capabilities not available from the ground: (1) observations can be made in the 1000 to 3000 Å vacuum ultraviolet; (2) it can be used in the infrared at all wavelengths, not just in the few restricted windows available from the ground; (3) because the telescope is above the earth's atmosphere, the image diameter can be controlled by the diffraction characteristics of the telescope; (4) because star images can be 0.1 sec of arc or less, the contrast against the sky background is a factor 40 to 100 times better than from the surface of the earth–this permits much fainter stellar objects to be observed than is possible from the ground.

The four properties of the LST listed above, coupled with the large collecting area of the telescope mirror, make it possible for the first time to study a host of new problems. The enormous scientific potential of the LST has been presented in detail in, for example, *Scientific Uses of the Large Space Telescope* (National Academy of Sciences, Washington, D.C., 1969); *Large Space Telescope–A New Tool for Science* (Proceedings of an American Institute of Aeronautics and Astronautics Symposium, Washington, D.C., January 1974); in the voluminous Phase B studies of the telescope and instrumentation; and in the reports of the various instrument definition teams.

With the high-resolution camera, which suppresses the sky background, it will be possible to obtain images of stars down to magnitude 27 or 28 and galaxies to about magnitude 24 or 25 depending on accuracy desired, exposure time, and telescope aperture. It will also be possible for the first time to resolve and count the stars at the center of a globular cluster to study the dynamics within the cluster; resolve individual stars, including variables, in many galaxies to derive their distances and help determine more accurately the scale of the universe; look for galaxies around quasars and quasarlike objects; study the detailed structure of planetary nebulae; examine the forms of galaxies in distant clusters in order to look for evolutionary effects as one looks back in time; and carry out far-ranging studies of planets and other solar-system objects.

The LST will provide the facility to obtain uv high-resolution spectra of faint stars such as faint white dwarfs, RR Lyrae variable stars, W Virginis stars, and evolved and unevolved stars in globular clusters. From these spectra it will be possible to obtain abundances of the common elements such as carbon, nitrogen, and oxygen. These are needed in order to understand how heavy elements were made in the early history of our galaxy and how elements are synthesized in stars.

The LST will be able to observe a wide range of normal galaxies and all known peculiar galaxies and quasars. In the case of peculiar galaxies, observations made with a resolution of 0.1 sec of arc will permit the detailed study of the dynamics and physics of their nuclei.

Spectroscopic studies may give clues about the nature of quasars, because direct comparison between the uv spectra of objects of small and large red shift will be possible. Also, for quasars, it will be possible to observe down to a rest wavelength of 250 Å for the objects with large red shifts. At such wavelengths, fundamental lines of neutral and ionized helium and other light atoms and ions can be observed and the chemical abundance derived. Such abundances may give clues to the early history of the universe.

Although the LST has not been designed to optimize infrared observations, its large light-gathering power and high resolution will almost certainly permit excellent infrared observations at those wavelengths that are not accessible from the ground and to broach the many infrared problems that require high spatial resolution. Relatively simple modifications have been made in several cases to ground-based optical telescopes to make them effective for observing in the infrared. Similar modifications could be introduced into the present design studies for the LST.

(c) TIMING AND CONSTRAINTS The ultraviolet observatories now in orbit are Copernicus and ANS, and they presumably have a lifetime of one or two more years. The small IUE telescope will be launched in late 1976 or early 1977 and is expected to operate for three years, i.e., until early 1980. These satellites can be used only to observe relatively bright objects. In view of the expected lifetime of IUE, the LST should be launched as soon as possible.

Probably the most difficult technological problems facing the mission are the detectors to be used. Existing detectors are usable, but they are by no means optimal and substantial funding should be made available to improve them as soon as possible.

(d) OTHER JUSTIFICATIONS Because of its size, diffraction-limited performance, and ability to work in the vacuum ultraviolet, the LST is a unique space instrument. It may well be unique even among astronomical facilities as a whole. Large ground-based telescopes are limited in their performance mainly by atmospheric scintillation, which degrades efficiency in many ways. For most problems in observational astronomy there appears to be no way to circumvent this difficulty; in particular, building telescopes larger than 4 to 5 m in diameter is not worthwhile. High efficiency can be achieved only by building more telescopes. The diffraction-limited LST gets around these problems and for many observations is more efficient by a factor of 10 to 20 than a 4- to 5-m ground-based telescope. It is thus a telescope that can solve traditional ground-based problems that cannot be solved from the ground.

2. GENERAL-PURPOSE ULTRAVIOLET AND OPTICAL SORTIE TELESCOPE

(a) MISSION DESCRIPTION The general-purpose ultraviolet and optical telescope is envisaged to be a diffraction-limited optical and uv telescope of at least 1-m aperture, intended for repeated Shuttle sortie flights. Characteristics of this instrument are outlined in the SSB study, *Scientific Uses of the Space Shuttle* (National Academy of Sciences, Washington, D.C., 1974, p. 112).

(b) SCIENTIFIC RATIONALE The Spacelab uv-optical facility telescope will be a versatile and flexible telescope, designed to accommodate a broad range of user-developed instrumentation. Spacelab astronomy will permit the use of observational techniques that demand the physical return of data and equipment. In particular, film recording techniques (such as electronography) can be used. These provide many more resolution elements per frame of data then do TV techniques, have much simpler and less costly equipment, and avoid the difficult problem of transmitting the information in real time to ground receiving stations. Research objectives requiring precise imaging over relatively wide fields in the sky (substantially wider than those observable with remote-readout detectors on the LST), with angular resolution exceeding ground-based capabilities, could be accomplished with a Spacelab uv-optical telescope. It would be especially useful for observing objects not requiring the full sensitivity or resolution capability of the LST, thereby reserving LST time for those objects that really need it. It would also be useful for exploratory observations of objects to be studied in more detail by LST and as an excellent photometric instrument for objects of intermediate brightness.

The flexibility of Spacelab missions allows a wide variety of instruments and optical configurations to be used with short lead times. This allows, for example, (a) test and evaluation of new detectors and optics for later use in the LST, such as the photon-counting television system, and (b) coverage of special wavelength ranges (e.g., 900–1150 Å) by use of optical coatings optimized for the range of interest, rather than for a very wide wavelength range as necessary in the multipurpose LST.

Extension of the wavelength range to include the 900–1150 Å region will permit the study of a number of important stellar and interstellar absorption lines such as H_2, HD, C III, N II, N III, O VI, Ar I, Ar II, and Fe III. Also, exploratory studies in the xuv range (500–900 Å) would be particularly important to determine the properties of the nearby interstellar medium, the interplanetary medium, and nearby hot stars, as well as solar-system objects.

(c) TIMING AND CONSTRAINTS In the event that the LST program is delayed, it may be highly desirable to fly smaller uv-optical telescopes as soon as possible on Spacelab to give continuity in the uv-optical astronomy program and to provide engineering and operational data that would aid the development of the LST.

(d) OTHER JUSTIFICATIONS The smaller Spacelab telescope would provide much new and important astronomical data beyond that obtained by the present OAO's, such as (with diffraction-limited optics) higher resolution in visual wavelengths than can be obtained with ground-based telescopes.

3. ULTRAVIOLET SURVEY SORTIE TELESCOPE

(a) MISSION DESCRIPTION The ultraviolet survey telescope is envisaged as an all-reflecting Schmidt telescope of 0.75-m aperture, having a 5° field of view. It is intended for use with electronographic film recording and repeated Shuttle sortie flights.

(b) SCIENTIFIC RATIONALE It is contemplated that the program will produce full-sky surveys and quantitative photometry in three uv bandpasses (tentatively, 1050–1650 Å, 1250–2000 Å, and 1800–2800 Å) to the faintest achievable limiting magnitudes and at a resolution equal to or better than ground-based (~1 sec of arc). It will also obtain an objective-grating uv spectral survey of a classification dispersion, with accurate relative (and absolute) spectral distributions. With a 5° × 5° field and a 10 percent allowance for field overlap and spacing, roughly 1800 fields will be required to cover the whole sky.

The uv sky survey will provide reference data on the positions of uv sources as well as uv fluxes and moderate-resolution uv spectra of large numbers of celestial objects. Such data are appropriate for exploratory and statistical studies, for finding objects for more detailed study by the LST, and for observing objects such as extended H II regions that are too large and diffuse to be studied adequately by the LST. It would also be useful for detailed mapping of the interstellar uv extinction in our region of our galaxy, as well as (in the objective-grating mode) interstellar hydrogen Lyman-alpha absorption.

The proposed instrument will be capable of reaching unreddened BO stars up to at least m_V = 20 (which is at least 10 magnitudes fainter than was achievable with the Celescope experiment on OAO-2) and will reach, for example, the faint blue objects at high galactic latitudes. Although, in principle, all these objects can be reached in the visible with the 48-inch Palomar Schmidt telescope, it is difficult to separate them completely from the far more numerous "ordinary" stars or to determine accurately their temperatures (for $T > 20{,}000$ K), especially in the presence of interstellar reddening. The wide field of the survey instrument allows more efficient photometry and spectrophotometry of large numbers of objects (particularly in crowded fields) than is possible with a narrower-field instrument.

B. Solar Astronomy

1. SOLAR MAXIMUM MISSION

(a) MISSION DESCRIPTION The Solar Maximum Mission (SMM) is envisaged to be a solar-dedicated payload for the study of flares and flare-related phenomena, utilizing a new platform to be developed from hardware of other programs. A modular approach will be employed for the platform subsystems, permitting replacement and refurbishment through Shuttle capture. The scientific payload is to be directed toward observation of flares and flare phenomena over a wide wavelength range with high temporal resolution and can consist of up to ten pointed instruments within an experiment section 0.9 m X 1.5 m X 1.5 m. The total launch weight of the scientific experiments for the mission cannot exceed 500 kg, but by Shuttle replacement the platform can accommodate up to 1200 kg of experiments. Pointing accuracy is ≤5 sec of arc. Orbital altitude will be approximately 450 km, with an inclination of 28°.

(b) SCIENTIFIC RATIONALE Solar flares exhibit a broad range of high-energy processes, including the generation of hard cosmic rays and associated radiation, extending over the spectrum from gamma rays to radio wavelengths. The sun provides an opportunity for detailed study of the interaction of high-energy particles and magnetic fields; such studies have clear and direct relevance to the study of other energetic objects in the universe and of the atmospheres of other stars.

The major problem of solar flare research is to answer these questions: What is the mechanism that triggers the energy release, and how is the energy transformed among the numerous modes involved? The thermal part of the flare plasma is best observed from experiments in the visible, xuv, and soft x-ray regions of the spectrum and from observations of the slowly varying emission at centimeter wavelengths; while the nonthermal phase of the flare can be studied in hard x rays, gamma rays, and impulsive centimeter-wavelength bursts. A comprehensive set of measurements, featuring high temporal resolution, is necessary to give insight into the triggering mechanism of a flare; the total energy content; and the acceleration, containment, and release of charged particles.

Observations of the state of an active region prior to the occurrence of a flare may serve to elucidate the period of rapid magnetic change and the period of escape of low-energy particles from the buildup region into interplanetary space.

Finally, the role of flare phenomena as a modifier of the magnetic structure of the solar atmosphere and the interplanetary medium–through shock waves and streams–is poorly understood, as are the dynamics of passage of the particles and fluid phenomena outward from the sun to the earth. Observations of the solar atmosphere and correlative measurements of the associated interplanetary plasma parameters are necessary to make progress in understanding the nature of fundamental solar-terrestrial phenomena.

The scientific objectives of the payload after Shuttle visit and refurbishment need not be the same as those of the initial instrument package. At present, many objectives of future solar-physics investigations cannot be defined until the data recently obtained from the Skylab ATM and those soon to be obtained from OSO-I have been thoroughly examined. Hence, the Shuttle-revisit option for the SMM spacecraft provides a high degree of flexibility for future solar astronomy investigations.

(c) TIMING AND CONSTRAINTS The SMM is under consideration for a Delta rocket launch in mid-1978 in order to operate at the predicted solar maximum in 1979. The payload will be pointed at the sun continuously during the daylight portion of the orbit. The current schedule for the final experiment selection is May 1, 1975.

(d) OTHER JUSTIFICATIONS The SMM permits observation of phenomena related to flares and activity associated with the maximum cycle of the sun; the next opportunity after 1979 occurs near 1990. Since the last solar maximum, solar satellite instrumentation has evolved to a degree that permits high spatial and spectral resolution on specific solar phenomena. Thus it is suitable for examining problems related to the flare kernel and ancillary problems.

The platform represents the initial attempt by NASA to construct a system suitable for refurbishment by capture with the Shuttle; the system has been designed to permit substantial upgrading of pointing and telemetry subsystems and to allow considerably heavier experiment packages to be relaunched with the Shuttle.

2. SOLAR SORTIE INSTRUMENTS

(a) MISSION DESCRIPTION Basic instrumentation for the Shuttle sortie Solar Observatory falls into several categories, as described in *Scientific Uses of the Space Shuttle.* Two payloads have been identified as basic multiuse facilities: the Solar Telescope Cluster (see next section) and the High-Energy Solar-Physics package. In addition, there are envisaged two different sized fine-pointed (stability of 1 sec of arc) platforms that can accommodate a variety of specialized instruments. The smaller fine-pointed platform should accommodate instruments up to 2 m in length and would be ideal for carrying the type of experiments now flown on rockets but providing greatly enhanced viewing times. Larger and heavier payloads than those currently flown also could be accommodated, thus permitting the evolution of current rocketborne experiments. A larger fine-pointed platform could carry either large special-purpose instruments not included in the Solar Telescope Cluster or problem-oriented packages of several experiments of a size intermediate between current rocket and OSO-type experiments.

(b) SCIENTIFIC RATIONALE The objectives of sortie instruments are extremely broad and have been described in detail in *Final Report of the Space Shuttle Payload Planning Working Group: Solar Physics* (NASA Goddard Space Flight Center, May 1973).

In order to understand the complex, active sun it is essential to make a concerted effort to obtain observations during the rise to the next solar maximum (i.e., 1990). In particular, since observations over a broad range of wavelengths are necessary to examine various depths in the solar atmosphere, high-resolution instrumentation of various types must be developed for possible later inclusion in free-flyers and the Solar Telescope Cluster and for various special-purpose objectives.

The success of the problem-oriented observational programs on the ATM indicates that the multi-instrument approach directed toward specific problems will be of great utility in solving principal solar problems. This approach will require the nearly simultaneous development of new-generation hardware for the Shuttle era.

(c) CONSTRAINTS The high spatial, spectral, and temporal resolution, with concomitant high data rates, of both the sortie and Solar Telescope Cluster require, in the interest of maximum scientific productivity, a data-relay system permitting nearly continuous contact with the Shuttle sortie.

3. SOLAR TELESCOPE CLUSTER

(a) MISSION DESCRIPTION The Solar Telescope Cluster represents the principal high-resolution facility for solar physics. It is capable of achieving 1 sec of arc or better pointing accuracy and can accommodate instruments of a size larger than previously flown. Sample characteristics of the Cluster are outlined in *Scientific Uses of the Space Shuttle*, p. 141.

(b) SCIENTIFIC RATIONALE The principal problems in solar physics can be divided roughly into two broad overlapping areas—solar activity and the mass and energy flow in the solar atmosphere. In the former, one confronts the formation, heating, and evolution of active regions as well as the cataclysmic release of energy through magnetic-field annihilation in solar flares. In the latter, one considers the mechanisms that produce large departures from radiative equilibrium in the chromosphere and corona, the flux of mechanical energy into the upper layers of the atmosphere, and the flow of mass into the corona and beyond.

Within these two areas recent results have demonstrated that (a) magnetic fields play a central role in both channeling the flow of energy and mass from the sun and in furnishing the energy for flares; (b) physical conditions characterized by temperatures ranging from a few thousand degrees to tens of millions of degrees result; (c) many of these phenomena have characteristic scales of a few hundred kilometers—for example, field strengths of several thousand gauss are concentrated within a sub second of arc domain; and (d) changes in the mass motions occur over short time scales. Further progress demands observations of the characteristics of the magnetic field and of the solar atmosphere at high spatial, spectral, and temporal resolution. A battery of high-performance telescopes capable of covering the wavelength range from below 1 Å to the millimeter range, equipped with special-purpose spectrometers, polarimeters, filters, etc., at the focal plane, is required in order to examine simultaneously a range of depths in the solar atmosphere. The detailed composition and definition of the Cluster must await the definition of the precise problem, which in turn will result from considerations of the results of the ATM, OSO-I, and the Solar Maximum Mission.

(c) CONSTRAINTS The development of the instruments of the Cluster should follow the logical evolution of the sortie Shuttle mode and become available for use at the time the Shuttle can provide longer orbital lifetime for optimum scientific return.

C. Infrared and Radio Astronomy

1. INFRARED SURVEY SATELLITE

(a) MISSION DESCRIPTION The Infrared Survey Satellite is an Explorer-class satellite containing a cryogenically cooled telescope of 25- to 50-cm aperture. It will have a variety of detectors covering some portions of the infrared spectrum from 57 μm to 1 mm. It will operate primarily or entirely as an unpointed survey satellite providing a high-sensitivity, redundant coverage of a large portion of the celestial sphere.

(b) SCIENTIFIC JUSTIFICATION Infrared astronomy has progressed rapidly in the past decade as a result of major instrumental advances in ground-based, stratospheric, and rocket instrumentation. The following are some of the astronomical discoveries and issues to which infrared astronomy has contributed: (a) composition and temperature of planetary atmospheres, planetary surfaces, asteroids, and comets; (b) molecular composition of atmospheres of cool stars; (c) unexpected excess ir radiation from stars; (d) cool dust shells around stars, sometimes sufficiently opaque to make the star undetectable by direct visible radiation; (e) dust clouds within ionized regions near hot stars; (f) identification of silicate grains as one of the constituents of circumstellar and interstellar matter; (g) excess ir radiation from the nuclei of Seyfert and other galaxies, which, in some cases is the dominant energy flux from the object; (h) intense compact ir objects, some of which may represent early states of star formation; (i) ir variability of stellar and possibly extragalactic sources.

Infrared observations are particularly important for the study of cool states of matter, which emit most of their radiation in the infrared.

Discovery in this field has been hindered by the lack of sensitive sky surveys in the 5-μm to 1-mm spectral range. Observations of galaxies have been entirely limited to objects known from optical and radio astronomy to be of interest. A sensitive satellite survey will yield an increased and unbiased sample of known kinds of extragalactic objects and will provide potential discovery of new kinds of ir objects unseen or unnoticed at other wavelengths.

Existing rocket sky surveys at 10 μm and partial balloon surveys of the galactic plane at 100 μm have demonstrated the feasibility and potential yield of a high-sensitivity ir sky survey.

(c) TIMING AND CONSTRAINTS The survey satellite is the first-priority infrared space experiment. It should precede any large infrared observatory.

2. 1-METER INFRARED CRYOGENICALLY COOLED TELESCOPE

(a) MISSION DESCRIPTION This is a Shuttle sortie instrument intended for repeated flights. It is to be a 1- to 2-m aperture telescope, diffraction-limited at 5 μm with optics cooled to approximately 30 K. It will be designed for operation for 7 to 30 days. The telescope will have a multiple instrument chamber with approximately six segments so that the focal plane instruments can be changed during flight to accommodate several observing programs within a single flight. Typical instruments would be a multiband photometer, multiplexing spectrometer (Michelson spectrophotometer), quantum imaging device, and polarimeter.

(b) SCIENTIFIC JUSTIFICATION Ground-based infrared observations are restricted to infrared windows below 30 μm and beyond 350 μm. Both ground-based and stratosphere telescopes utilizing the best ir detectors available are presently limited over much of the ir spectrum by background noise from the thermal emission of the telescope and atmosphere. Beam-switching techniques that provide discrimination between a faint discrete source and a bright uniform background cannot overcome this fundamental limitation. Full utilization of current and anticipated broadband detector technology requires the use of a cryogenically cooled telescope operating above the atmosphere. Such an instrument would carry out detailed ir studies of objects of known interest in the ir band and of objects newly discovered by the IR Survey Satellite.

A cryogenically cooled ir telescope will be optimal for the following kinds of observations: (a) broadband photometry of galactic and extragalactic objects, (b) infrared multiplex spectroscopy studies of stars and interstellar matter, (c) mapping of low-surface-brightness objects, (d) ir polarimetry, (e) observation of the ir structure of galactic and extragalactic objects by multiple detector arrays or by quantum imagery.

Millimeter-wavelength spectroscopy is producing exciting evidence about the existence of complex molecules in interstellar space and the conditions under which they are formed. Extension of these observations to the submillimeter region should allow the detection of low moment of inertia and a determination of the level of excitation of molecules in warmer regions. This will contribute to the investigation of interstellar molecular formation and destruction, grain formation, radiation transfer, and the interrelationship between interstellar molecules and interstellar dust.

The infrared continuum radiation is also closely related to dust, including circumstellar shells. Observations in this wavelength region can thus provide measurements on the composition and structure of the interstellar medium, circumstellar material, and stellar atmospheres, with applications to theories of nucleosynthesis, the evolution of the elements, and the production of molecules of varying degrees of complexity.

Maps of portions of the galaxy at these wavelengths with moderate spatial resolution can help to define the general distribution of gas and dust and its relation to the stellar content of the galaxy.

A number of galaxies have been shown to emit more radiation in the infrared out to 20 μm than at all other wavelengths. In a number of cases, the peak radiations appear to be beyond the region observable from the ground. The cryogenically cooled telescope will have sufficient sensitivity to study a number of extragalactic objects including Seyfert galaxies and QSO's and contribute to an understanding of how these enormous luminosities are generated and radiated in the infrared.

Observations in the ir region are of great importance for a full understanding of the cosmic background radiation. Existing data are consistent with the interpretation of this radiation as a remnant of the hot early phase of the universe, but the requirement now is a thorough and precise study of the shorter-wavelength portion of the spectrum. This can only be made from outside the atmosphere with narrowband instruments providing observations of both spatial anisotropies and spectral structure.

(c) TIMING AND CONSTRAINTS The cryogenically cooled ir telescope should be flown soon after the results of the Infrared Survey Satellite are known and digested.

The major constraint of the Shuttle sortie approach to the cryogenically cooled telescope is the severe limitations it places on the acceptable gaseous and particulate contamination from the Shuttle. The cold optical surfaces act as a cryopump for vapors and gases, resulting in a contaminant coating that can cause a gross reduction in the telescope's sensitivity. Particulate contamination is uniquely bad for ir astronomy because the thermal radiation from such particles is entirely in the ir spectrum. The radiation from a single small particle in the field of view of the telescope can dominate over celestial radiation. Studies* have shown that the Shuttle sortie cryogenically cooled telescope places the most severe constraint on the Shuttle relative to contamination and requires special precautions in Shuttle design and operation. If these constraints cannot be met, designing the telescope as a free-flyer should be considered.

**Scientific Uses of the Space Shuttle* (National Academy of Sciences, Washington, D.C., 1974) pp. 88–89, 96; Marshall Space Flight Center Report ED 202-1701 Rev. B (May 1974); Martin Marietta Corp. Report MCR 74-93 (May 1974).

3. 3-METER AMBIENT TEMPERATURE INFRARED TELESCOPE

(a) MISSION DESCRIPTION The ambient temperature ir telescope is envisaged to be the largest diameter telescope that is practical for operating as a Shuttle sortie instrument, i.e., about 3 m. The telescope will be diffraction-limited at 5 μm and will utilize both infrared and submillimeter techniques for observations up to wavelengths of 1000 μm (1 mm). It will have a cryogenically cooled instrument chamber and be specially designed for low emission in the 5-1000 μm range. Passive radiative cooling should be able to achieve temperatures lower than 200 K with appropriate baffling and shielding from the sun, earth, and moon. The telescope should be instrumented for both continuum and narrowband spectral observations. There is considerable room for improvement in detectors in this wavelength region, and effort should be directed in the early stages of the project toward detector development.

In addition, this telescope could be used for some measurements in the ultraviolet region of the spectrum, where large collecting area above the atmosphere is essential but the high spatial resolution of the LST is not required. Different mirror coatings would be required for uv missions.

(b) SCIENTIFIC RATIONALE A substantial number of the significant astronomical issues in infrared astronomy combine the need for sensitivity and for spatial resolution. Because of the long wavelengths involved, a very large aperture is needed to obtain resolution higher than is feasible for a cryogenically cooled telescope. Ground-based observations in the 5-1000 μm region suffer from heavy attenuation and noise generation in the atmosphere. The largest possible Shuttle telescope, optimized for low emissivity in the infrared, is required.

Such a telescope will be superior for three different classes of observations:

1. Observing the spectral structure of bright objects where a large aperture is essential to provide high-diffraction-limited resolution and the objects are sufficiently bright not to require the sensitivity provided by cooled optics;
2. Narrowband spectral studies where a large collecting area is needed to provide large flux gathering capability and where the telescope emission within the small spectral interval is not a limitation;

3. Broadband photometry at the longest ir wavelengths where the sensitivity of current and anticipated detectors is not high enough to be limited by telescope emission.

High-resolution spectroscopy of planets and comets over the broad spectral regions available in space can give molecular abundances and, hence, atomic abundances and isotopic ratios for the solar system. This information provides evidence about the history of the objects in the solar system.

Submillimeter spectral line receivers provide a method to investigate interstellar molecules in dense regions where both velocity structure learned from line shifts and spatial structure are required to understand the underlying physical phenomena.

Advances in the study of many galactic sources depend on discerning the extent and structure of the emitting region. For these studies, high resolution coupled with large aperture is crucially important.

(c) TIMING AND CONSTRAINTS The LST can provide many of the capabilities needed for ir observations, but it will be required for a multitude of observations in many spectral regions and for a great variety of projects. The LST will not be optimized for low emissivity in the infrared and hence will be substantially less sensitive than an optimized ir telescope.

Because of the potential opportunity of meeting some ir observing goals with the LST, potential productivity of the 1-m ir cryogenically cooled Shuttle telescope, and a European study of a large ambient-temperature ir Shuttle telescope, the ambient temperature mission is not placed high in priority for a start in the 1970's. However, if current studies indicate that contamination requirements for the cryogenic telescope cannot be met and an alternate free-flyer is not possible, then the ambient temperature telescope should replace it in priority.

4. ULTRA-LONG-BASELINE INTERFEROMETRY SATELLITE

(a) MISSION DESCRIPTION This mission requires a single satellite in a highly elliptical (300 km × 30,000 km) orbit, carrying a 10-m dish, and operating at a wavelength near 1.3 cm in the continuum and in the water line at 22.235 GHz. It will operate with a ground-based observatory as a variable-baseline interferometer.

(b) SCIENTIFIC RATIONALE The development of ground-based very-long-baseline interferometry (VLBI) has shown that both quasars and H_2O masers exhibit structure that cannot be resolved with existing ground-based telescopes (baselines < 10,000 km). In the case of quasars, higher-frequency observations (designed to obtain baselines that are longer in the number of wavelengths) do not resolve the sources because the volume over which the components are optically thick diminishes with increasing frequency. For the H_2O masers, of course, the wavelength is fixed. Space baselines are therefore necessary to increase the angular resolution. The orbital motion of the satellite gives a continuously varying baseline, providing most Fourier components over a wide range in two dimensions. This will permit mapping the structure of a source much more completely than can be done from the ground with VLBI techniques.

Unexpected physical processes are taking place in both quasars and H_2O masers, and information on their angular size and fine structure will be important in discovering more about their nature and origin. The precise measurement capability will also permit important contributions to geophysics and celestial mechanics.

(c) TIMING AND CONSTRAINTS Quasars and H_2O masers are both time-variable on a scale of weeks or months. It is therefore desirable to continue the observations for some years to study variations in the angular structure. In addition, over a sufficiently long period, orbital precession will continuously vary the orientation of the baselines relative to the line of sight to a source, enabling the full brightness distribution to be studied.

D. High-Energy Astronomy

1. 1.2-METER-APERTURE X-RAY TELESCOPE

(a) MISSION DESCRIPTION This mission will establish a permanent national observatory to carry out image analysis with 0.6 sec of arc resolution, low- and high-resolution spectroscopy, and polarimetry in the wavelength range from 3 to 60 Å on galactic and extragalactic x-ray sources with a photon-limited sensitivity 10 times better than HEAO-B. The improved performance will assure frontier research in x-ray astronomy through the early 1980's.

(b) SCIENTIFIC RATIONALE X-ray observations now provide essential data on the nature and evolution of many of the objects of greatest current astrophysical interest, such as pulsars, quasars, Seyfert galaxies, clusters of galaxies, and the intergalactic medium. Beginning with rocket and balloon observations in the early 1960's and culminating with the first dedicated x-ray satellite, *Uhuru*, the exploratory and survey phase of the development of x-ray astronomy revealed an x-ray sky rich in information about the behavior of matter and fields under extreme conditions of pressure, density, and magnetic fields. The discovery of x-ray emissions from compact objects in binary star systems opened a new and powerful approach to the study of condensed matter in neutron stars and provides the most compelling evidence for the existence of black holes. The discovery of x-ray emission from clusters of galaxies has provided a new way to investigate the previously unobserved intergalactic medium, which may constitute a large fraction of the total matter in the universe. The study of x-ray emission from clusters of galaxies at extreme distances ($z > 3$) may throw new light on the large-scale structure and evolution of the universe.

The culmination of the approved NASA flight program will be the HEAO-B x-ray observatory that will provide high-resolution (~2 sec of arc) x-ray imaging by means of a 0.6-m grazing incidence telescope and will achieve photon-limited performance in the analysis of discrete sources with intensities down to 10^{-7} that of Sco X-1. HEAO-B will provide, for the first time, the means to study the detailed distribution of x-ray emission from extended objects, such as supernova remnants, clusters of galaxies, the coronal x-ray emission of nearby main sequence and giant late-type stars, the emission and absorption features in x-ray spectra in the critical wavelength region longward of 10 Å, and the properties of discrete x-ray sources in nearby galaxies beyond the Clouds of Magellan. With a design life of one year, HEAO-B will have time only to begin these studies and point the way for future development, just as the ground-based optical telescopes early in the century were the essential precursors of the larger instruments to follow. The thrust of the development of space facilities for x-ray astronomy is toward the establishment of a permanent observatory that will provide high-quality observations with a long-term continuity that is essential not only to the solution of specific astrophysical problems but also to an orderly development of the field as a whole.

The 1.2-m x-ray telescope is the next logical step in the development of x-ray observing facilities beyond HEAO-B. Based on the detailed design studies carried out for the original larger HEAO-C mission, the 1.2-m telescope will be Shuttle-launched and maintained and will comprise a national x-ray observatory with a tenfold increase in sensitivity and a substantial improvement in angular resolution over the HEAO-B instrument that is presently under construction. It will provide critical new data on the position and structure of discrete sources in our own and nearby galaxies and of extragalactic sources to the great distances required for the detection of cosmological effects.

From a programmatic point of view, the 1.2-m telescope will serve as a testing ground for the technical innovations that must be accomplished in order to construct the future larger telescope that will be the x-ray equivalent of the LST in optical-uv astronomy. It can be accommodated on a standard spacecraft designed for large Shuttle-launched payloads in high-energy astronomy.

(c) TIMING AND CONSTRAINTS This mission should be initiated early enough to assure continuity in the development of x-ray astronomy after HEAO-B. Assuming that the latter is operational through 1979, the 1.2-m telescope should begin operation as soon as Shuttle facilities are available for launching. The orbit should be low-altitude and circular, preferably equatorial to minimize the interference from the radiation belts.

2. HIGH-ENERGY GAMMA-RAY SATELLITE

(a) MISSION DESCRIPTION A gamma-ray telescope based on the digitized spark-chamber technique developed in SAS-2, but with about 30 times the sensitivity of the SAS-2 instrument (or ten times that of COS-B), can be flown in a Delta-launched spacecraft. The payload will weigh about 1200 kg, and the satellite will have a circular orbit at ~400 km and a >1-year lifetime. Angular resolution will be about 1.5° at $E \sim 100$ MeV and rise with energy. Energy resolution may be around 10 percent from 70 MeV to 10 GeV. Time of arrival is measured to about 1 msec. The high-efficiency plastic scintillation anticoincidence shield—essential to the experiment and well tested—makes a sensitive single detector for fast, low-energy, gamma-ray bursts.

(b) SCIENTIFIC RATIONALE SAS-2 has demonstrated that the cosmic-ray induced π^0 gamma rays from the interstellar gas can show the spatial distribution of the product of the cosmic-ray intensity and the interstellar matter density ($j_{cr}\rho$). Detailed study of this parameter in the galactic center, spiral arms, and other features of the galaxy is a new and important clue to the dynamics of the gas and the sources and fate of cosmic rays. Moreover, since discrete sources have now been observed in supernova remnants—pulsed in the Crab, steady from the Vela region—it appears that a new class of sources, hoped for since 1960, has now been reliably identified. The making of such a source list—from energies of 100 MeV on up—is an exciting prospect.

The diffuse background spectrum is another objective; little is known about this flux beyond 1 MeV, and its interpretation is of obvious interest for extragalactic and even cosmological theory.

(c) TIMING AND CONSTRAINTS The results of SAS-2 suggest that a new channel in gamma rays has opened. The time is ripe to fly a device that can exploit the leads given by the first success.

3. HIGH-ENERGY ASTRONOMY FREE-FLYERS

(a) MISSION DESCRIPTION The development of cosmic-ray, gamma-ray, and x-ray free-flyers is envisaged. Instruments are presently under consideration (see *Scientific Uses of the Space Shuttle*, p. 57, instruments with prefix FF) and will be chosen on a competitive basis. Each spacecraft will contain either one or two large and heavy experiments, or as many as ten individual experiments, with a total weight of up to 6800 kg. The spacecraft will be standardized, with capabilities comparable with those of the original HEAO: 8000 kg, 300 W, 25 kbit/sec, 1 min of arc pointing, and 1 sec of arc/sec jitter. For the experiment support structure, standardized modules 3 m in diameter and 3 m long will facilitate the direct use of sortie-qualified experiments. These modules can be assembled to a total length of 18 m. After an operating time of one to three years, the free-flyer may be retrieved and refurbished.

(b) SCIENTIFIC RATIONALE The extraordinarily large collecting power (product of detector area and observation time) of instruments on free-flyers will usher in a new era of high-energy astronomy. In cosmic-ray astronomy it will be possible to determine abundances of even the rarest elements in the periodic table, to determine the relative abundances of their major isotopes (some of which will be radioactive), to study the changes of composition with energy, to look for directional effects at energies to $\sim 10^{15}$ eV/nucleon, to study the electron-positron component to $\sim 10^{14}$ eV, and to search with great sensitivity for antimatter and superheavy nuclei.

In gamma-ray astronomy it will be possible to survey the whole sky at various energies, to study a number of sources in detail, to search for bursts of gamma rays from supernovae, and to detect nuclear gamma-ray lines from such processes as radioactive decay of newly synthesized elements in supernova remnants.

In x-ray astronomy it will be possible to measure aperiodic line variations of candidate black holes like Cyg X-1 at energies above 3 keV with timing resolutions as fine as 1 μsec, to search for periodic and other regular variability on time scales from microseconds to years, to study hard x-ray sources in the energy range 3-300 keV to an intensity level of 5×10^{-5} of the Crab nebula, and to carry out high-resolution spectrometry and sensitive polarimetry in the energy range above 3 keV, which is not accessible to focusing telescopes.

(c) CONSTRAINTS In the 50 percent planning wedge, funding limitations make it necessary to delay development of high-energy astronomy experiments for sorties and free-flyers until 1978 and 1979 and prevent all but a small fraction of the experiments from being flown.

4. LARGE-AREA MODERATE-RESOLUTION X-RAY TELESCOPE

(a) MISSION DESCRIPTION The large-area moderate-resolution x-ray telescope (LAMAR) mission will establish a permanent national observatory to study very faint x-ray sources, to observe the structural features of extended sources, and to carry out the detailed measurements of rapid time variations in the x-ray intensity of compact sources at photon energies below 4 keV. These objectives require an instrument of very large sensitive area and of sufficient angular resolution to avoid degradation of data by source confusion. The LAMAR will have a grazing-incidence x-ray focusing system of the Baez type, constructed in modular sections each of a sensitive area of 1 or 2 m^2, and highly efficient image detectors that will achieve an overall resolution for image analysis of the order of 1 min of arc. The instrument will be tested in the sortie mode and ultimately launched as a national observatory in the mid-1980's.

(b) SCIENTIFIC RATIONALE The use of large-area proportional counters with mechanical collimators in surveys of faint sources is limited by source confusion. Already the large-area proportional-counter array on HEAO-A will have reached a level of sensitivity at which the expected number of sources per resolution element exceeds the criterion of 0.03 generally used in radio astronomy to define the practical limit of source confusion. Thus any further extension of x-ray surveys to sensitivities greater than that of HEAO-A must provide for appropriately improved angular resolution, which at present can only be practically achieved with grazing-incidence focusing systems.

Design criteria for LAMAR are provided by the goal of surveying the spatial distribution, time variations, and spectra of binary x-ray sources in the Andromeda nebula. Scaling from the known angular separation of the bright x-ray sources near the center of our galaxy, one finds a value of the order of 1 min of arc for the minimum resolution necessary for effective study of the x-ray sources in Andromeda. A resolution in this range, which is much less sharp than will be achieved by the 1.2-m telescope, can be attained with a Baez-type grazing incidence focusing system at a much lower cost per effective area. This large area, which is more than 20 times that of the high-resolution 1.2-m x-ray telescope, will make possible the study of transient phenomena that cannot be detected by the 1.2-m instrument. Thus, the LAMAR will complement the 1.2-m telescope and make it possible to attack many critical problems including the following:

1. To investigate the nature of the fainter extragalactic sources and extend the log N-log S curve in search of cosmological effects;
2. To extend the survey of the x-ray sky at wavelengths longward of 44 Å and explore the nature of the soft galactic x-ray background;
3. To measure the time variations of x-ray sources in nearby galaxies (LMC, SMC, M31, M33, M81, M82) in search of x-ray pulsars and black holes;
4. To determine the distribution of galactic matter by measuring the x-ray absorption edges of the elements in the spectra of galactic sources;
5. To extend the study of coronal x-ray emission from the brightest main-sequence stars to much fainter ones than will have been accessible to the 0.6- and 1.2-m telescopes;

6. To study the variation of the intensity and spectra of x-ray emission over the surface of supernova remnants in order to elucidate the dynamics of blast-wave phenomena in the early states of explosions in space.

(c) CONSTRAINTS The LAMAR should comprise a permanent, unmanned, but refurbishable national observatory. Because it will complement the scientific capabilities of the 1.2-m x-ray telescope in many critical studies, it should become operational as soon after the 1.2-m telescope as feasible. A low, circular, permanent orbit of the lowest practical inclination is required.

5. SORTIE INSTRUMENTS

(a) MISSION DESCRIPTION Large instruments and spacecraft will be developed for cosmic-ray, gamma-ray, and x-ray sortie missions lasting a few days and using a standardized 3-m × 4.5-m pallet element to provide power, telemetry, and pointing control. Numerous flights per year of recoverable, refurbishable spacecraft can provide nearly the flexibility of balloons and rockets with vastly greater collecting power. A tabulation of 24 possible high-energy astronomy instruments for the Shuttle era has been prepared by the Ad Hoc Planning Group of the High Energy Astrophysics Management Operations Working Group (July 1974). Of these, 18 are listed as possible sortie missions and 19 as possible free-flyers.

(b) SCIENTIFIC RATIONALE High-energy astrophysical processes manifest themselves in many ways, including x rays down to the low energy limit set by interstellar absorption, gamma rays to hundreds of GeV, and cosmic rays from $\sim 10^6$ to $\sim 10^{20}$ eV in energy and from $Z = 1$ to $Z = 100$ in charge. Observations of these radiations rely almost exclusively on instrumentation carried into space. Sortie missions combine large collecting power and weight-carrying capacity with the flexibility for relatively quick reaction to newly discovered or variable phenomena. New regimes of wavelength, energy, and charge can be explored with substantial sensitivity and observation times. Sorties also allow for the flight and testing of newly conceived techniques and experiments prior to assignment to free-flyers.

(c) REQUIREMENTS AND SHUTTLE POTENTIAL The frequent monitoring of emissions high in energy but weak in flux requires great weight-volume capability and large numbers of missions. A continuing program of unmanned automated spacecraft can provide the continuity of observations required to ensure a succession of new discoveries in high-energy astronomy. The large weight-lifting capability, flexibility, recoverability, and short turn-around time to reflight are essential features of the sortie.

A standard pallet with the capabilities outlined in *Scientific Uses of the Space Shuttle* (p. 9–10) must be developed. New management techniques similar to those applied to research with balloons and rockets must be applied to sortie missions. These include acceptance of relaxed quality assurance and reliability standards, greater reliability on the performance of the principal investigator, and standardization of most system interfaces.

E. Cooperative Missions

1. OUT-OF-THE-ECLIPTIC MISSION

(a) MISSION DESCRIPTION Missions to study interplanetary space and the sun outside the ecliptic plane are under consideration. In one proposal, the Pioneer H spare spacecraft, equipped with instruments identical with those carried on Pioneers 10 and 11, will make a polar pass through Jupiter's magnetosphere and then pass over one of the solar poles at a distance between 1 and 2 AU. Measurements will be made of energetic particles, the solar wind, magnetic fields, cosmic dust, the zodiacal light, and coronal streamers. An alternative mission being considered is to use the extra weight capability of a Titan to launch two spacecraft of Pioneer class in tandem. At Jupiter each will pass over a different Jovian pole and later pass over a different pole of the sun, making simultaneous measurements.

(b) SCIENTIFIC RATIONALE Our view of the sun and interplanetary space has been confined to the two dimensions of the ecliptic plane. There is every reason to believe that conditions on the solar surface and space are significantly different near the solar poles and near the ecliptic. For example, the interplanetary magnetic field is believed to be radial at the solar poles but to have a spiral structure in the ecliptic. The behavior of the solar wind plasma at the poles will be quite different. Cosmic rays entering the solar system along the poles may have a substantially unmodulated spectrum, comparable with that which could only be observed in the ecliptic at many tens of astronomical units. This mission will be of unprecedented value for particle, field, and plasma physics and can also provide continuous photographs of coronal streamers and polar coronal holes, as well as measurements of zodiacal light and cosmic dust, which are concentrated in the ecliptic.

An out-of-the-ecliptic mission will pass first through a 40° band of latitude dominated by activity at the sun and by fast and slow solar-wind streams in space. Situated within only ±7° of the solar equator, the earth may miss the most severe activity in the wind because the most intense activity at the sun occurs at somewhat higher latitudes. (Auroral and geomagnetic activity are known to be stronger when earth is at the extreme limit of ±7° from the solar equator.) Beyond the first 40° of latitude the spacecraft will pass out of the zone of violent activity into a steadier environment. Beyond about 60° a high-speed solar wind will appear, permitting direct study of its properties. There the low-energy galactic cosmic rays should be accessible for analysis of their elemental and isotopic abundance.

IV. BASELINE

A. Explorers

Explorer satellites have a long record of great achievement in space astronomy. To mention only two recent successes, *Uhuru* (SAS-1) has provided the bulk of the observational data in x-ray astronomy and SAS-2 has done almost the same for gamma rays. This class of relatively light and inexpensive vehicles has pursued a wide variety of astronomical goals with marked success. We enthusiastically endorse the Explorer concept of versatile satellites, each used for a small mission in space physics or astronomy. The support over the years of a level-of-effort Explorer program near $33 million/yr (the total includes both space physics and space astronomy) has been most productive. It is important to maintain this line item because it supports the first steps in new astronomical initiatives. (A possible Explorer spending schedule is given in Table A.6.) Indeed, the responses to Explorer Announcements of Flight Opportunities in recent years have included important proposals that have failed to receive support solely because of lack of funds so that it is now crucial to expand the Explorer budget in order to offset inflation and to sustain and build on the progress already achieved.

TABLE A.6 Possible Explorer Spending Schedule (in Millions of Dollars)

	Fiscal Year			
	1977	1978	1979	1980
Explorer A	5	8	8	7
Explorer B	3	4	4	4
Explorer C	—	—	1	5
TOTAL[a]	8	12	13	16

[a]These totals are equal to 50 percent of the space astronomy and physics Explorer planning wedge.

Several objectives are suggested below that would be strategic for astronomy Explorers in the next five years. [The final response to the recent request for Explorer proposals (Announcement of Planning Opportunities) should provide more extensive descriptions of possible Explorer missions.]

1. GAMMA-RAY BURSTS

Vela discovered, and other satellites (e.g., IMP) have confirmed, brief gamma-ray bursts with a frequency of ~10/yr and a duration of seconds. Wide-angle photon detectors with areas of 10^2 to 10^3 cm^2 to count photons of 0.1 to a few MeV can extend knowledge of those enigmatic high-energy events. Valuable directional information can be gained in several ways, including, for example, the use of a widely spaced satellite pair ($d \sim 10^8$ m) or the use of Dicke-type mechanical collimators on a single satellite.

2. X-RAY SOURCE MONITOR

Galactic x-ray sources are known to exhibit a wide range of complex time variations. Detectors have already been flown in SAS-1 in the 1-10 keV range, with time resolutions of 10^{-2} sec or better and modest pointing accuracy, under some degree of ground control. Time resolutions of 10^{-4} sec with three-axis stabilization will be achieved in SAS-C. A continued patrol of the varied and complex sources with the largest practical sensitive area and time resolution of 10^{-4} sec or better is of value both intrinsically and as an auxiliary to other systems. This patrol will give critical information on source-region sizes and often on their motions, and because it singles out active epochs it will permit more effective use of other more powerful specialized instruments.

3. COSMIC BACKGROUND

The 3 K blackbody flux that fills space is the only direct source of detailed information about the state of the universe long before the formation of stars and galaxies. This information is contained in fine detail—in deviations from isotropy and from a pure Planck spectrum and in residual polarization. Because the bulk of the energy is at millimeter and submillimeter wavelengths, most current studies are done at balloon and rocket altitudes. In a few years, such balloon and rocket studies will have reached the limits set by exposure time and residual atmospheric contamination.

Space detectors, taking advantage of the absence of atmosphere and of exposures of hundreds of days, will have unmatched advantages for obtaining the necessary accuracy of 10^{-4} or better, as a function both of direction and frequency, from about $\lambda = 1$ mm to $\lambda = 0.4$ mm. Cryogenic detection is probably a requisite. These measurements seem within reach of Explorer weights and orbits with possible development of two missions—one for direction, one for spectrum. The cosmic background problem is of great interest, but mission planning should await the results of current experiments. By about 1978–1980 this task should be a prime candidate for one or two Explorers.

B. Sounding Rockets

An increase in the NASA budget for sounding rockets, particularly in the years before the Shuttle becomes operational, is strongly recommended. It will provide continuity in the various subdisciplines of space astronomy, allow individual observations to be made on relatively short notice, and permit the testing and evaluation of new instrumentation that might later be incorporated into orbital missions. The additional funding will be used to increase the frequency of sounding rocket launches and the performance and reliability of rocket systems.

An example of the utility of sounding rockets is the recent Operation Kohoutek in which a number of payloads were prepared and launched on short notice; two of them returned important new data that could not be obtained by any other space mission (including Skylab).

A typical sounding rocket at present can carry a 200-kg payload and provide about 4 min of observing time above the atmosphere. New rockets, such as the Aries, can carry 800-kg payloads up to 1 m in diameter and provide the same or slightly longer observing times. Even after the advent of the Space Shuttle, sounding rockets will still be necessary, because they can be launched at a precise time specified by the principal investigator (e.g., to observe a solar eclipse) without disturbing other projects with unrelated experiments. Moreover, the lead-time and launch costs for individual small experiments will be less than for Shuttle operations. Experiments that are too unproven even for sortie missions can be tested in sounding-rocket flights.

C. Ballooning

The scientific ballooning program plays an extremely important role in space astronomy by providing a low-cost vehicle for exploration work in new fields, a means to develop and test new space payloads of large weight and size, an approach that permits creation of small low-cost experiments in rapid response to new discoveries, and a method of carrying on a sustained program of observation in fields where measurements at balloon altitudes greatly enhance the scientific value of space-obtained data at relatively low cost.

The following examples may serve to illustrate. The successful SAS-C gamma-ray survey instrument is a direct outgrowth of an instrument developed and tested with balloon flights. Massive HEAO experiments are currently being tested by balloon flights. The first discovery in far-ir astronomy of the high luminosity of the galactic center and of radiation from cool interstellar dust was made with a balloonborne telescope. Exploratory surveys, moderate resolution mapping, and tests of new instrumentation are now being carried out in this field. Balloon experiments have advanced the study of the cosmic background radiation and have helped to resolve a controversy over the submillimeter spectrum and the amount of energy flux in the radiation. Extensive advances in understanding hard x-ray spectra and time variations have been made with balloonborne experiments. Stratoscope 1 and 2 advanced the technology of high-resolution photography above the atmosphere.

The ballooning program should be continued and expanded as a necessary ingredient to effective development and utilization of space experiments. The current program of development of superpressure balloons for long-duration flights (up to six months) will provide an important advance in capability. Unfortunately, the combination of inflated costs for balloons and helium, increased weight of payloads, and increased testing of space payloads under development have completely outstripped the current funding level. Additional, continuing support is urgently needed.

D. Aircraft

Aircraft have been employed successfully for many types of astronomical observations. One notable example concerns the use of small airplanes for infrared observations from the lower stratosphere. These have provided substantial advances in our understanding of the heat balance of planets and the physical conditions of interstellar dust and have encouraged development of infrared spectroscopy. The NASA 91-cm airborne telescope is now in early stages of operation and promises to provide much expanded opportunities for far-ir observation in the period leading to major ir space facilities of the Shuttle era. We consider aircraft experiments a valuable adjunct to space astronomy and *recommend* that the program continue to receive adequate support.

E. Supporting Research and Technology

An increase in the NASA budget for supporting research and technology (SR&T), particularly in the years before the Space Shuttle becomes operational, is vitally needed to ensure that most efficient use is made of the capabilities of the Shuttle and Shuttle-borne scientific instrumentation.

A particular example of an area of SR&T needing a higher level of support is the development of new detectors. The full capabilities of the LST, for example, can only be utilized with photon-counting television systems—that is, detectors providing high-resolution, two-dimensional imagery but with unambiguous counting and localization of every photon-initiated event during the exposure. Electronographic imaging systems also provide this capability with film recording, as might be used with the uv-optical Spacelab telescope. Improved detectors are especially needed in the infrared, where the energy per photon is much lower than in the visible and uv.

Other important areas for SR&T are the development of new optical systems and optical components (e.g., holographic and toroidal concave grating) and laboratory spectroscopy.

V. LONG-RANGE PERSPECTIVES IN SPACE ASTRONOMY

The purpose of the missions described in this report is to further the goal of understanding the physical universe through extending the spatial, spectral, and temporal channels available to astronomy. These missions, and in particular the observatory-class instruments, will enormously improve our ability to resolve fine detail, permit observation of far fainter sources, and much expand the spectral regions accessible to long-term observations. We consider that they will provide a broad-based program of research into the major scientific questions of astronomy and astrophysics.

As a long-range goal in space astronomy, we suggest that the potential of the Space Shuttle be further exploited by using it to assemble very large telescopes in orbit around the earth. Because progress in astronomy is frequently limited by insufficient collecting area, such telescopes will allow study of many objects of extraordinary interest that are too faint for any existing or currently planned telescope. It seems practical to assemble, for example, a multiple-mirror telescope with light-gathering power equivalent to that of a single 10-m mirror and perhaps even a 30-m mirror. If such a telescope had a spatial resolution of 0.1 sec of arc, it could obtain spectra of stars and stellarlike objects 10–100 times too faint for the LST or the largest ground-based telescopes.

The assembly of a very large telescope in orbit could be phased over several years, beginning with a few mirrors in the 1-m class (see *Scientific Uses of the Space Shuttle*, pp. 114–115). More mirrors would gradually be added until the very large collecting area of a 10- to 30-m telescope was obtained.

The utility and observing efficiency of these instruments and those proposed in this report might be increased even further by boosting them to a synchronous orbit.

Another possible future activity in space astronomy would involve efforts to protect radio observatories from radio interference. Within the next decade the interference to ground-based radio astronomy will become increasingly great. Some serious problems are already being encountered, and competition for the available spectrum can be expected to grow rapidly. Transmissions from satellites will themselves cause special trouble as their number increases since radio astronomers cannot obtain protection by moving to other locations on the earth. Eventually it may only be possible to carry out some types of radio astronomy on the far side of the moon or at some other locale completely shielded from ground-based and earth-satellite transmissions. The national and international organizations involved in the allocation of frequency bands have already begun discussing the use of radio on the moon's far side, and we can expect that a substantial fraction of the spectrum will be reserved for astronomy.

As a first stage, the use of lunar orbiters for radio-astronomical observations should be extended to cover the whole radio spectrum. In addition, the possibility of using the far side of the moon as a shielded astronomical platform should be considered in long-range planning. These opportunities may even allow a search for signals from extraterrestrial societies.

To summarize, we believe many important and exciting long-range prospects exist in space astronomy, one of the most significant being the possible use of the Shuttle to assemble very large telescopes or collectors in orbit. It now seems timely to begin exploring these possibilities in more detail.

Committee on Space Physics

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B
Space Physics

I. SUMMARY

Space physics is concerned with the study of interactions in space among energetic charged particles, ionized gases (plasmas), electric and magnetic fields, and the outer portions of planetary atmospheres. Although most investigations are conducted in the region of space near the earth, each planet also provides a focus of interest, and important phenomena occur in interplanetary space. Despite the fact that matter is tenuous in the regions of interest, phenomena occur there that may affect man's activities on earth. In addition, the understanding of the physical processes involved in these environments contributes to progress in other fields such as plasma physics and astrophysics.

Although research on space physics has been pursued for over a decade, many important problems remain whose solutions only now appear attainable. Progress has been made to the point where there is a clearer view of what specifically needs to be investigated in order to solve some of these problems. One example is the flow of solar-wind plasma into, and in part through, the earth's magnetic field. This is, in principle, a straightforward and well-defined problem that involves the interaction of a streaming magnetized plasma with a static magnetic field—something that should be readily predictable in terms of plasma physics. Yet this problem has not been solved, nor have measurements in the earth's magnetosphere and in the laboratory been sufficiently detailed to guide the theory toward a solution.

What is needed in the future is increased emphasis on correlative experiments in which measurements are made simultaneously at several points within and adjacent to the magnetosphere in order to isolate particular physical phenomena. To pursue such correlative experiments, increased attention must be given to the data-handling systems to permit efficient rapid exchange of data among investigators and to keep track of the relative positions of spacecraft in order to identify the times when favorable observing opportunities will occur for study of specific problems. Individual instruments in satellites can be turned on and off, or have their observing ranges and modes of operation adjusted, by ground command in order to make desired observations when several satellites have favorable relative positions. Rapid acquisition of data in usable form is required in order to make use of the opportunities for which one set of measurements is used to make decisions concerning the observations to be made at the next observing opportunity. Increasing use can be expected of on-board propulsion systems that permit adjustments of orbits to bring about favorable relative positions of satellites. Active experiments in which the environment is perturbed by introduction of particles or waves will also be important in testing theories; the procedure for active experiments will resemble laboratory experimentation rather than passive, uncontrolled, geophysical observations.

Much of space-physics research is done near earth (which is the most accessible region of space), and the instrumentation that is required is well developed and does not place difficult demands on space vehicles. The cost of space-physics research in the terrestrial environs is therefore low by comparison with space projects in general. The relative simplicity and low cost make this type of research particularly well suited for cooperative international efforts in which each nation supports the activities of its own scientists. Also, there are larger numbers of scientists involved than would be assumed on the basis of the total cost of the space-physics program; a correspondingly high scientific productivity per unit cost is thus achieved.

The missions that are recommended in this report for *terrestrial* space physics, along with the year of launch, the year of start in parenthesis, and the total cost in 1974 dollars (not including launch costs) are as follows:

	FY Launch	FY Start	Total Cost*
Electrodynamic Satellites	1978	(1976)†	\$50M
Scout Explorer	1978	(1977)‡	\$10M
Solar Wind Explorer	1980	(1978)	\$15M
Atmospheric Explorer	1980	(1978)	\$45M
Scout Explorer	1981	(1979)‡	\$10M
Magnetospheric Multiprobes	1982	(1980)	\$60M

In addition, it is assumed that AMPS (the manned Spacelab instrumented for Atmosphere, Magnetosphere, and Plasmas-in-Space) will be operational from about 1981 onward and that it will perform important functions in remote sensing of constituents in the stratosphere and mesosphere and, in conjunction with other spacecraft (such as the Magnetospheric Multiprobes), active experiments in the magnetosphere.

In the area of interplanetary space physics, an out-of-the-ecliptic mission is recommended for launch in 1979§ to examine conditions at high solar latitude where the solar wind has different properties than it has near the sun's equatorial plane, and there the cosmic radiation is thought to be more representative of interstellar conditions than can be inferred from measurements near the ecliptic. Measurements made with spacecraft up to the present have all been within about ten degrees of the equatorial plane, and even this small range of latitude has revealed a considerable change in average solar-wind properties.

In the area of *planetary* space physics, orbiters can provide much more information than can flybys. Highest priority is placed on an orbiter for Jupiter because of the special characteristics of its magnetosphere. In particular, the effects of the rapid rotation of the planet appear to introduce additional physical processes that resemble those believed active in pulsars. A Jupiter orbiter mission is

*These are rough order-of-magnitude estimates.

†The Space Science Board decided that because of budget constraints the new start for this mission should be delayed until 1977.

‡Although the Space Science Board recognizes the importance of small satellites, it believes that it is more appropriate to fund them from the Explorer budget rather than identify them explicitly in the mission model.

§For the recommendations of the Space Science Board see Part I.

recommended for launch in 1980.* Also recommended is an orbiter mission to Mars or Mercury to be launched in 1983.*

The NASA budget item for supporting research and technology plays an important role in space physics, permitting analysis of spacecraft data after termination of spacecraft projects, ground-based research in support of space projects, theoretical research, and the development of new instruments. It also includes research with rockets and balloons. Although these activities greatly increase the productivity of space-physics research, the actual level of support and corresponding activity has decreased in recent years. It is *recommended* that the level be increased by 30 percent over the next three years, with additional increases to cover inflation. Without such support, the productivity of space research efforts must be expected to fall considerably.

The recommended programs for terrestrial space physics add up to about $35 million per year for spacecraft projects and about $10 million per year for supporting research and technology, or about $45 million per year total. This amounts to less than 10 percent of the budget of NASA's Office of Space Science. Considering the large number of scientists involved in these efforts and the correspondingly high scientific output, the expenditure seems well justified. This total exceeds the funding normally provided for Explorer-type satellites, however, and there are additional needs for satellites of this type for astronomy. We, therefore, specifically *recommend* that the level of support for Explorer-type satellites be increased.

II. PERSPECTIVES AND GENERAL RECOMMENDATIONS

Space physics involves observations and studies of regions of space farther from the earth's surface than the "meteorological" atmosphere—regions containing plasmas and energetic particles in addition to the tenuous neutral atmosphere, with important interactions among each other and with fields and waves of various kinds. A broader definition includes the study of rarefied atmospheres, plasmas, and fields in space. Terrestrial space physics concerns studies of the earth's upper atmosphere and magnetosphere and their

*For the program of planetary exploration recommended by the Space Science Board see Part I. For more details about the individual missions see the working paper of the Committee on Planetary and Lunar Exploration.

interaction with the solar wind; planetary space physics concerns similar studies around other planets; and interplanetary space physics concerns studies of phenomena in interplanetary space.

The transfer of energy and plasma from the solar wind to the magnetosphere and ultimately to the atmosphere is a major element of terrestrial space physics. The importance of these couplings and of the associated large-scale dynamical processes involved has become increasingly evident in recent years. At high latitudes, for example, neutral winds at ionospheric levels are driven to high velocities by magnetospheric plasma motions as a consequence of ion drag forces. These winds, in turn, have feedback effects that affect the magnetospheric plasma motions. The ultimate transfer of some of this energy to the lower atmosphere, either mechanically or through changes in chemical composition, is at present poorly understood but is potentially important and deserves extensive study.

Terrestrial space physics also includes investigations of atmospheric levels that are too dense to be directly probed with satellites. Knowledge of these regions can be advanced through the use of space techniques involving remote sensing from satellites and the use of rockets in *in situ* sampling.

Studies in terrestrial space physics provide the most convenient and accessible opportunity to examine some important physical phenomena on an astrophysical scale. The proximity of the terrestrial magnetosphere offers a unique opportunity to study, for example, collisionless plasmas and magnetic-field-line interconnection. Although the scales involved are not those of stars or interstellar clouds, they come close enough to be relevant. A number of phenomena are observed that were not foreseen or predictable from laboratory-scale simulations; the study of terrestrial space physics therefore provides a reservoir of experience that will be necessary to provide quantitative tests of astrophysical theories.

Such investigations also provide the reservoir of understanding needed to interpret planetary observations made elsewhere in the solar system. For example, the data gathered with Pioneer 10 on a single flyby of Jupiter were quickly placed in a coherent conceptual framework using the experience with the earth's magnetosphere. The magnetosphere of Jupiter has shown some interesting unique features; because of the planet's large size and rapid rotation, centrifugal effects appear to dominate in the outer portion of the magnetosphere. Thus the study of the Jovian magnetosphere provides new insights, some of which may be applicable to other objects such as pulsars. Experience has shown the need to test theories when they are applied to new situations, and the variety of magnetospheres in the solar system now provides a good opportunity to do this. Since the outer envelopes of the other planets offer a great variety of configurations and mechanisms that are not duplications but complements of those found in the earth's environment, their study in turn broadens our perspectives and in many cases increases the understanding of our own planet earth.

Space physics embraces a wide range of studies in plasma physics that contribute to the basic understanding that is needed for utilizing plasmas in modern and future technology. It also embraces a number of areas related to human activities, including the recent understanding of the stratosphere as a storage region or sink for atmospheric pollutants and the growing uneasiness about the possible inadvertent modification of the earth's climate that could arise from complicated sequences of photochemical reactions involving substances injected into the atmosphere at any level. Another area of applied interest is found in processes in the magnetosphere, ionosphere, and mesosphere that can interfere with communication systems, electric power transmission networks, and nuclear explosion detection systems. Even satellite communications that use frequencies well above those normally thought to be subject to ionospheric influences are sometimes affected strongly by the presence of ionospheric irregularities.

Although space-physics investigations have been made for over a decade in the earth's vicinity, many important problems remain that now appear susceptible to solution. The flow of solar-wind plasma into the magnetosphere, and in part through the magnetosphere, is one such problem that at present dramatizes the limits of understanding of interactions between plasmas and magnetic fields. Although this apparently straightforward and well-defined problem should be soluble in terms of plasma physics, it has not been solved; measurements in space and in the laboratory have not yet been sufficiently detailed to guide the development of theory toward the solution.

The steps that are appropriate during the coming decade to make progress in the solution of such problems include greatly increased emphasis on correlative experiments involving different spacecraft as well as other observational techniques, improved systems for data handling and efficient exchange of data among investigators, active experiments, and use of propulsion systems in satellites to permit orbit adjustment to bring about useful relative positions among spacecraft. These will permit the experimentation that will test specific predictions of theories, and thus further the development of the understanding of the detailed physical principles involved.

Terrestrial space-physics investigations involve the use of spacecraft of modest size and cost. These are programs in which relatively large numbers of investigators can be, and are, involved. As a result, this part of the total space-science program must be regarded as one of high productivity per unit cost. These same attributes make the program suitable for international cooperation.

A. The Stratosphere and Mesosphere

The altitude region between 10 and 80 km is characterized by reactive photochemical systems that are interwoven by reactions coupling the nitrogen–oxygen, hydrogen–oxygen, carbon, sulfur, and chlorine systems. Atoms and radicals, which result from the diffusion of stable constituents released at the earth's surface into the highly oxidizing region above the tropopause, constitute an intricate and delicate system containing some constituents essential to life at the planet's surface. An understanding of this region has a bearing not only on the origin of life but also on certain factors that could conceivably jeopardize life on earth.

The ionospheric D region is mostly contained in this altitude range (it extends a little above 80 km and into the lower thermosphere). It also involves a complicated set of chemical reactions that are dependent on minor constituents–ions, radicals, and excited species. Auroras are important in perturbing the composition locally at high latitudes. Important seasonal effects also occur at midlatitudes, as testified to by the winter anomaly in radio-wave absorption.

The altitude range from 10 to 80 km has proved to be difficult to study. A combination of instruments borne on aircraft (up to 20 km), balloons (up to about 45 km), and rockets have yielded occasional and rather widely separated observations, and improvements in understanding based on direct observations have come slowly. Radio-propagation techniques have been used successfully to explore the lower ionosphere, which lies near the top of this altitude range. Remote sensing from satellites can be expected to contribute increasingly to the accurate description of this atmospheric region, and the AMPS shuttle payload (a version of the manned Spacelab for investigation of the Atmosphere, Magnetosphere, and Plasmas-in-Space) promises a substantial capability for this purpose.

The present level of understanding of the stratosphere was recently tested by attempts to assess quantitatively the possible impact of increased injection of combustion exhaust products from SST's. The results showed the present lack of a satisfactorily complete picture of the important physical processes to this region. It also showed that improved understanding will require a considerable emphasis on the photochemical reactions and transport processes that occur in this region. This understanding will also be relevant to the ability to predict undesirable effects on the atmosphere that might follow the launching of Space Shuttles or the cumulative effects of the continued release of such gases as the chlorofluorocarbons into the atmosphere.

1. RECOMMENDED PROGRAM FOR THE STRATOSPHERE AND MESOPHERE

A satisfactory program of stratospheric and mesospheric research must emphasize the need to correlate experimental and theoretical objectives. The program must also take into account the time-dependent and spatially varying mechanisms that characterize the region. In particular, the experimental studies should be directed at the following goals:

1. Absolute concentration measurements of the key species with concentrations sometimes down to 1 part per trillion.
2. Measurements extending continuously over a major fraction of the altitude interval from 10 to 80 km. Since the concentrations of most of the interesting species are dependent on latitude, altitude, and solar zenith angle, and vary with time according to the meteorological situation, isolated measurements are frequently of little value.
3. Some measurements confined to a specific volume or specific altitude interval to ascertain the effect of integration over an extended path length by remote measurement techniques.
4. Simultaneous measurements of interrelated species within the same volume element over a diurnal period in order to determine the kinetic behavior of the photochemical system.

Although important observations of this region are obtained by remote observations from satellites, studies of the stratosphere and mesosphere rely heavily on techniques involving rockets, balloons, and aircraft. Therefore, strong continuing support for these programs remains essential. In addition to standard rocket techniques, the use and further development of high-altitude parachutes for rocket payloads is recommended. These are currently deployable at upper mesospheric altitudes and permit a controlled descent of the payload through the mesosphere, stratosphere, and troposphere, greatly extending the data-collecting capabilities of the sounding rocket for a minor increase in cost. The continued support and further development of high-altitude balloons as sampling platforms useful to an altitude of 45 km is also recommended. Balloons also provide a low-cost capability for testing some instrumentation intended for more elaborate rocket or satellite launches. Aircraft can be used for investigating the nature of exchange between the stratosphere and troposphere, which is an important factor in the overall response of the stratosphere to perturbations.

B. Earth's Atmosphere above 80 km

The extension of the atmosphere to altitudes above 80 km was discovered by its reflection of radio waves; throughout the 1940's and early 1950's the radio ionosonde was the principal tool for exploration of this region of the atmosphere, and radio physics was the main subject for investigation. Sounding rockets, which became available in the late 1940's, provided occasional samples of, or direct measurements in, the ionosphere and extended our knowledge of the details of this region.

It was with the advent of satellites in the late 1950's that enough information was accumulated to view the atmosphere as a whole, from the earth's surface up through the rarefied, ionized layers. It is now known that solar energy is deposited in varying amounts at all levels of the high atmosphere by means of excitation, dissociation, and ionization of neutral particles, and that this energy creates a complex thermal structure that in turn determines the altitude distribution of the neutral particles. Energy is also deposited in the upper atmosphere by energetic particles, electric currents (Joule heating), and absorption of wave energy from the lower atmosphere (the latter generating turbulence that appears to be widespread below 100-km altitude). Substantial coupling occurs between adjoining atmospheric regions.

1. RECOMMENDED PROGRAM FOR THE UPPER ATMOSPHERE

(a) SATELLITES Direct satellite probing of the upper atmosphere down to 130 km is now being accomplished with Atmosphere Explorer C (AE-C). Early indications of important dynamical processes taking place in this altitude region have made it apparent that in future programs more emphasis must be placed on measuring atmospheric motions. The concentrations of many minor atmospheric constituents also provide information on the behavior and reactive properties of the atmosphere, so future instrumentation will utilize the rapidly developing technology of neutral species detection. Constituents that should be measured include H, O, N, NO, H_2, OH, O_2, H_2O, and CO_2. These measurements will illuminate the fundamental problem of hydrogen escape (which is central to theories of atmospheric evolution); the photochemistry of odd nitrogen (N, NO, NO_2) in the thermosphere; major anomalies in auroral zone nitric oxide concentrations; and the latitude distribution of hydrogen that is correlated directly with polar escape mechanisms.

Additional Atmospheric Explorers beyond those in the current program will be needed to describe the structure of the upper atmosphere near the time of solar maximum and to investigate phenomena, primarily dynamical in character, that have emerged as important since the design of the current spacecraft. The ratio of solar-wind energy to ultraviolet heating of the upper atmosphere can be expected to be different near solar maximum than near solar minimum, as will the relationships between thermal structure and chemical composition. Thus two spacecraft are recommended for the period of solar maximum, one at high and one at low inclination, with increased emphasis on measurements of minor atmospheric constituents and on dynamical phenomena.

The Electrodynamic Satellite concept is discussed below in the section on Magnetosphere and Solar Wind, but it will also contribute substantially to atmospheric science. These satellites are designed to investigate the coupling mechanisms among the magnetosphere, ionosphere, and atmosphere and to examine atmospheric and ionospheric response to magnetospheric activity.

(b) SPACELAB The AMPS shuttle payload will be an important contributor to investigations of the upper atmosphere. It is expected to fly on one of the earliest Spacelab missions and to continue thereafter with two or three flights a year. AMPS will utilize a number of core instruments that can be flown many times with only minor changes to perform a wide variety of experiments. The atmospheric portion of these missions should emphasize remote sensing of the region below 120 km—the only technique that promises to secure the needed data on a global scale. The feasibility of such remote sensing can be enhanced by the development of atomic and molecular resonant photon sources that will provide great sensitivity for detection of minor constituents.

AMPS will offer an opportunity to unravel the complex coupling mechanisms down through the atmosphere as well as a unique opportunity to study fundamental and applied plasma-physics problems that cannot be solved in the laboratory, where limitations exist due to scale lengths, collisions, wall effects, and the short duration of discharges.

(c) SOUNDING ROCKETS For studies of the atmosphere above 80 km, as well as for the stratospheric and mesospheric studies discussed earlier, it is essential to maintain sounding-rocket capabilities. Rockets will be needed to investigate neutral and ionized species in the upper atmosphere, particularly for the study of sporadic or localized events in regions of the mesosphere and lower thermosphere that will remain inaccessible even to Atmospheric Explorer satellites. Sounding rockets provide the only means of making continuous vertical soundings over the 15- to 120-km region by means of *in situ* measurements. They provide great flexibility for rapid testing of new instruments, thus permitting more rapid and effective technological developments. They also help in the faster resolution of specific theoretical issues, since lead times are so much shorter than for satellites. The modest cost and simplicity of operation of sounding rockets are important advantages.

We *recommend* an increased sounding-rocket program, giving more attention to rocket firings coordinated with satellite measurements. Coordination with both Atmospheric Explorer and Electrodynamic Satellite programs will be especially valuable in providing a unified view of the coupled mesospheric–thermospheric and ionospheric–magnetospheric systems.

C. Magnetosphere and Solar Wind

Plasma is the normal state of most of the matter in the solar system, the galaxy, and very likely in the universe as a whole. Many, if not most, astrophysical phenomena are governed by the principles of plasma physics—that is, the physics of the interaction between ionized gases and electromagnetic and gravitational fields. The solar-planetary system, although relatively small on a cosmic scale, is governed by the same physical principles as cosmic systems. Thus the study of the physics of the solar system offers insight into an astonishing variety of fundamental plasma processes that may occur elsewhere on a cosmic scale.

Major advances have been made in recent years in the description and understanding of the interplanetary medium and the magnetosphere. On the basis of the results of the past decade of exploration, fundamental problems that must be attacked have now been identified. Also identified are the regions in space that need further study and the instrumentation, orbits, and time correlation of measurements that are needed. Two characteristic features must be considered in long-term planning of magnetospheric research. One is the complexity of the phenomena and their interrelations, demanding a clear separation of spatial and temporal effects in experimental observations. The other important feature is that physical mechanisms governing magnetospheric-ionospheric coupling are parts of complicated closed chains of cause-and-effect relationships that are difficult to unlink. The International Magnetosphere Study (IMS),* to be conducted during 1976–1978, will be an attempt to overcome some of the difficulties inherent in the first characteristic above, and the AMPS program will be oriented toward the active experimentation approach to the second characteristic.

The IMS program should answer many questions concerning the timing and spatial locations of changes in the outer magnetosphere. Progress should also be made on many other questions concerning the solar-wind input to the magnetosphere, the structure of magnetospheric boundaries, and the location of particle acceleration regions.

The coupling and feedback relationships within the magnetosphere can be investigated with AMPS through active experimentation involving tracer techniques for artificially inducing perturbations. Such active experiments offer one of the most exciting new ways to study and utilize the earth's high-altitude environment. These experiments—injection of accelerator beams, electromagnetic energy, and various cold plasmas—allow the perturbation of the

*_International Magnetospheric Study: Guidelines for U.S. Participation_ (National Academy of Sciences, Washington, D.C., 1973).

atmosphere–magnetosphere system under controlled conditions to study its response. For example, the stimulation and study of spatial and temporal evolution of plasma instabilities on a geophysical scale under a variety of boundary conditions can be performed via these techniques. By such an approach, the understanding of the physics of our environment will be extended and quickly followed by the application of this understanding to other environments (sun, planets, interplanetary medium, pulsars, etc.).

Because of the many variables involved, the interactive relationships in these natural plasma systems are not readily deduced from straightforward passive observations. Instead, the methods of laboratory experimentation are needed in which perturbations are introduced under controlled conditions. The past decade of magnetospheric research has produced three new techniques of active experimentation: alkali gas releases, energetic particle injection, and wave injection. Cold plasma injection offers yet another means of controlled perturbations. Using such active techniques, coupled with available passive measurements, these most difficult problems should be solved. A possible by-product of active experimentation is the development of techniques for modifying the ionosphere and magnetosphere for the benefit of man. If ways can be found for slowly draining off the energy of magnetospheric substorms it may be possible to reduce the frequency of occurrence of the large disturbances that disrupt ionospheric communications and power distribution systems.

In recommending a magnetospheric program, it is assumed that the bulk of the active experiments will be accomplished through the AMPS program. This assumption includes the necessity of launches to high altitudes of active payloads to be utilized in conjunction with AMPS as an observing platform.

The most important questions that will remain unanswered after the IMS will concern the coupling between the magnetosphere and the atmosphere. These will be approached directly by *in situ* measurements in the middle and upper ionospheric regions, building on the expected contributions of the IMS to the knowledge of the general structure of the magnetosphere. The study of magnetospheric–ionospheric coupling will require instrumentation and orbits different from those involved in the IMS, which are designed primarily for studies at greater distances.

The magnetosphere–ionosphere coupling arises in part from the variable anisotropy of electrical conductivity in the atmosphere caused by the earth's magnetic field, the variation in ion–neutral collision rates with altitude, the large horizontal as well as vertical gradients in ion density, and the relative motions of neutral and ionized species (i.e., neutral winds and ion winds). The geomagnetic field lines can, to a limited degree, be thought of as highly conducting paths embedded in the ionized gas. These field-line paths distribute electric fields between the collision-free outer magnetosphere and the collision-dominated, resistive ionospheres in both hemispheres. It is known that currents of tens to hundreds of thousands of amperes flow transverse to these geomagnetic field lines in response to the generating forces of the solar wind and the ionospheric neutral winds. It is also known that these transverse currents are interconnected with less intense, but not less important, currents flowing along the magnetic-field lines into and out of the ionosphere. Descriptions and understandings of the continuity of these currents are extremely vague. The complexity of the coupling is such that a variety of plasma instabilities and wave–particle interactions can be proposed to explain the wide range of observed spatial and temporal changes, but definitive separations of cause and effect are lacking.

It is expected that in the early 1980's the outstanding problems of magnetospheric research will have been narrowed down sufficiently so that a most detailed and most stringent planning of global satellite measurements will be possible. Indeed, one of the crucial remaining problems of magnetospheric physics will be to identify which processes are really "global," involving the magnetosphere–ionosphere–atmosphere system as a whole, and which are local in nature, involving only parts of the system. An analogy in meteorology would be the distinction between global weather patterns, local microclimates, and their mutual interaction.

The Magnetospheric Multiprobes will represent the logical solution to this need. By sending a family of spacecraft to make simultaneous measurements with similar instrumentation at several locations throughout the magnetosphere, both global and local views of magnetospheric dynamics can be obtained that will allow the separation of space variations from time variations, thus aiding in distinguishing cause from effect.

1. RECOMMENDED PROGRAM FOR MAGNETOSPHERE AND SOLAR WIND

The Electrodynamic Satellites (ES) are recommended for launch in 1978* to investigate magnetosphere–atmosphere coupling, thus covering a time interval in which most of the IMS missions are expected to still be operational. The concept, which replaces that of the Electrodynamic Explorers (EE), has matured considerably since the 1970 Summer Study.† This project is now considered by this committee to have the highest priority for new starts in space physics. The primary purposes of the ES mission will be to study the coupling between the atmosphere and the magnetosphere; the behavior and distribution of the plasma in the magnetosphere; and the generation, propagation, and absorption of various wave modes in magnetospheric plasma (i.e., wave–particle interactions). This program can follow the IMS, bridging the gap between 1978 and the availability of high-inclination Shuttle orbits (about 1983). Because atmospheric coupling is known to have a strong influence on the behavior of the outer magnetosphere, the effectiveness of the IMS will be greatly enhanced if the ES missions are undertaken on an overlapping schedule.

The ES missions call for three satellites in coplanar polar orbits, at least two of the satellites having a substantial capability for orbit adjustment. Two spacecraft will have eccentric orbits with apogees near 6 earth radii such that when their lines of apsides lie near the equatorial plane their orbital paths will nearly parallel magnetic-field lines for considerable distances near apogee. The two orbits can be made to coincide except for a phase difference in order to permit the separation of gradients along field lines from time variations. One of these satellites should be equipped with a TV imaging system to permit recording patterns of auroral activity at given instants in time. The third satellite, with a somewhat different payload, would have a lower, nearly circular orbit and would measure the behavior of the ionosphere and upper atmosphere near the feet of the field lines being examined by the satellites in elliptic orbits.

Small, Scout-launched Explorer satellites are also recommended in order to provide relatively short reaction times to new problems.

*The Space Science Board decided that because of budget constraints the new start for this mission should be delayed until 1977.

†*Priorities for Space Research 1971–1980* (National Academy of Sciences, Washington, D.C., 1971).

The large response to an Announcement of Planning Opportunity in July 1974 shows the great interest in such missions by the scientific community. This program should be flexible, and the objective should not be rigidly specified in advance. Such missions should be coordinated with other spacecraft missions. As an example of a Scout-launched Explorer mission an Equatorial Waves and Particles Explorer is described in the recommended program for launch in 1978.

In 1981, it is proposed to resume the program of simultaneous, coördinated measurements in the distant magnetosphere with the Magnetospheric Multiprobes. This involves the deployment of several simultaneous spacecraft with similar instruments in carefully chosen independent orbits. The measurements should be integrated with the program of controlled experiments from the Shuttle, aimed at obtaining a unified, quantitative picture of large-scale dynamic processes in the magnetosphere and filling the principal information gaps remaining after the IMS and the magnetosphere–atmosphere coupling missions (ES).

Since the magnetosphere is controlled by the solar wind, which acts as the main energy-momentum source, it is necessary to have continuous information on the solar-wind characteristics far enough upstream from the magnetosphere to be free from its perturbing influence. The Heliocentric Probe that is part of the International Sun–Earth Explorer (ISEE) will fulfill the requirement during the IMS, but a replacement for it will be needed by about 1980, and we have designated this replacement as a Solar-Wind Explorer.

Rockets also play an important role in magnetospheric research. In particular, sounding rockets represent a valuable technique as research tools for the study of the "microstructures" of particle beams in auroral arcs, field-aligned Birkeland currents and electric fields, and the interaction mechanisms between precipitating particles and the atmosphere. Sounding rockets will also continue to be used to inject ions and energetic particles into the ionosphere and magnetosphere for small-scale controlled magnetospheric experiments. By means of chemical releases from sounding rockets, it is also possible simultaneously to measure motions of the neutral and ionized gases and thus obtain detailed information on magnetospheric–atmospheric coupling as a function of altitude up to 300 km under preselected geophysical conditions. A vigorous sounding rocket program is thus recommended as a complement to satellite investigations of the magnetosphere.

D. Planetary Space Physics

As stated above, one of the fundamental objectives of space physics is the quantitative study of mechanisms controlling the dynamics of space plasmas and their interactions with neutral gas. Many of these mechanisms can be readily analyzed in the earth's magnetosphere and upper atmosphere, where they occur on a given limited scale, and under naturally determined boundary conditions. The program of planetary exploration offers the opportunity of an *in situ* study of space plasmas and atmospheres interacting under a variety of different conditions. Indeed, in the outer environments of the planets there is an assortment of configurations that look as if they had been planned to provide the right doses of complementarity to earth studies. For instance, Pioneer 10 measurements have verified the key role of the centrifugal force in Jupiter's magnetospheric plasma dynamics (negligible in the earth's case); the inner Jovian satellites are in suitable orbits to introduce measurable perturbations in the form of sinks and sources of particles in the Jovian magnetosphere; the intensity of the Jovian magnetic field extends the energy range of stably trapped particles far beyond that of the earth's radiation belts. In the case of unmagnetized or weakly magnetized planets, on the other hand, the direct interaction of solar-wind plasma with neutral atmospheres or the properties of "minimagnetospheres" such as Mercury's can be studied. Another dimension is added by the varying solar-wind input conditions that exist at different solar distances.

This complementarity can be used to develop a carefully balanced program of terrestrial and planetary missions in a way that exploits the possibilities of cross-fertilization, with each new achievement in understanding one domain (terrestrial or planetary) helping to further the exploration of the other. It should be stressed, however, that the magnetospheres and upper atmospheres of the earth and other planets are sufficiently different from each other so that each makes its own unique contribution to the interactions. This combined effort will ultimately lead to an improved, more quantitative understanding of the properties and behavior of stellar surfaces and atmospheres and interstellar gas.

The recent Mariner Venus Mercury flyby has produced an unexpected and important result: the planet Mercury is surrounded by a magnetic field. The experimentalists interpret the data as showing a bow shock, a magnetosheath and magnetopause, energetic particle radiation, and a magnetic tail. This is a solar wind–planetary interaction that is free of the complication of a strongly interacting atmosphere. A suitably instrumented Mercury orbiter would undoubtedly find a new class of solar wind–magnetosphere interactions. Thus, the opportunity will be opened for testing and generalizing theories of the transfer of energy from the solar wind to magnetosphere and of a variety of magnetospheric phenomena.

The study of planetary environments is now at the same stage as was terrestrial space physics 10 or 15 years ago—a stage of discovery, exploration, morphological description, and identification of the major physical processes involved. Based on what has been learned about the earth's environment and the planetary explorations carried out thus far, the challenge can be met of planning the next planetary missions in such a way as to gather, during the brief visitation of flybys, a comprehensive amount of data directed toward answering very specific questions. The availability of orbiters, on the other hand, will make it possible first to monitor and then to analyze systematically the quantitative behavior of planetary atmospheres and magnetospheres.

1. RECOMMENDED PROGRAM FOR PLANETARY SPACE PHYSICS

Orbiter missions in general, and especially at Jupiter, Mars, Venus, and Mercury, are of great interest to space physics, assuming that they carry the appropriate complement of space-physics instrumentation. Important contributions to the theories of planetary (especially atmospheric) evolution are to be expected from the space-physics measurements. The Venus Pioneer missions provide a good example.

Because of the importance of a Jupiter orbiter and the need for an optimum orbit for space-physics investigations, such a mission is recommended with high priority. Since the Pioneer 10 results indicated that many of the Jovian magnetospheric phenomena occur near the equatorial plane, a low-inclination, highly eccentric orbit is preferred. In addition, because of the Pioneer 10 and Mariner Venus Mercury results, it is recommended that all planetary orbiter and flyby missions contain field and particle instruments capable of observing the magnetospheres and/or pseudomagnetospheres of such planets. Of primary importance in these studies are measurements of magnetic fields; plasma waves; and the thermal, suprathermal, and energetic particle populations.

Orbiter missions to Mercury and Mars also have high priority.

When the Pioneer Venus mission is completed, a Pioneer-type mission to one of these planets should be undertaken. The choice of which of these is more appropriate can be delayed pending further study of the Mariner 10 data obtained at Mercury.

E. Interplanetary Space Physics

Early in the space program, interplanetary missions were undertaken for the purpose of making space-physics invesgitations. After considerable exploration of that part of interplanetary space near the earth's orbit, and as increasing numbers of planetary missions were flown, interplanetary space physics was pursued mainly by making measurements during the cruise phase of planetary missions. These missions provide splendid opportunities to study the interplanetary medium at low incremental cost. Furthermore, since the characteristics of planetary atmospheres depend significantly on this medium, appropriate cruise-phase measurements also contribute directly to planetary studies. Properties of interest include velocity, density, and temperature of the solar wind, direction and strength of the magnetic field, plasma-wave activity, cosmic radiation, dust, energetic-particle fluxes and spectra, and optical sensing of interplanetary neutral constituents.

It is recommended that all planetary missions carry similar particles and fields sensors so as to provide comparable data over a wide range of paths and times. With several probes in transit at one time, temporal and spatial variations in the interplanetary medium can be more easily separated.

Evidence is now available from radio-star scintillation measurements and from direct spacecraft measurements within a few degrees of the solar equatorial plane indicating that the solar wind is not spherically symmetric. These observations are consistent with the expectation that the coronal expansion may vary with solar latitude because of the known changes in the solar magnetic field, the structure of the corona, and the variation in the level of solar activity between the solar equator and the poles. The resulting configuration of the solar-interplanetary magnetic field probably produces important effects on cosmic-ray propagation in the vicinity of the sun. It is expected that this field configuration provides a preferred region of approach somewhat similar to that existing on a magnetized planet. Thus, it is expected that the interstellar cosmic-ray intensity can be inferred more accurately from the galactic cosmic-ray intensities measured over the solar poles than from measurements made near the ecliptic plane. Such information would significantly increase our understanding of the propagation of cosmic rays through the solar system and lead to improved estimates of the intensity and composition of interstellar cosmic rays.

Regions of space at high solar latitudes can be investigated directly only by an out-of-the-ecliptic mission, which requires a very high injection velocity to reach a high-inclination orbit. This can be achieved by passing close to Jupiter to perturb the spacecraft orbit appropriately or through use of solar-electric propulsion in a spacecraft launched directly from earth.

The Solar-Wind Explorer will provide data near the ecliptic plane that will be needed to solve problems regarding the phenomenology of the solar wind itself, particularly variations in solar-wind properties over solar-cycle time scales of 11 to 22 years. Such data will aid in the development of a more generalized solar-wind theory. Such data will also prove generally useful in connection with evaluations of geophysical responses to solar-wind changes, including the physical connection between solar activity and the weather in the event that the currently claimed relationships are validated by further studies. In this case, climate theory, which is now in its infancy, would be able to test theories of solar/weather coupling by utilizing, at least in part, data that show cyclic variations in solar-wind properties.

1. RECOMMENDED PROGRAM IN INTERPLANETARY SPACE PHYSICS

It is recommended that measurements be made of the interplanetary medium at high solar latitudes, by means of either a Jupiter swingby mission or a spacecraft using solar-electric propulsion to achieve a high-inclination orbit about the sun.

It is recommended that interplanetary space-physics measurements on magnetic fields, plasmas, and energetic particles be performed with standardized instrumentation during the cruise mode of planetary missions going far out in the solar system.

Finally, it is recommended that baseline data on the solar wind be gathered with the Heliocentric Probe (now scheduled for launch during the IMS) and later with the Solar-Wind Explorer, to relate solar-wind properties at 1 AU and attendant magnetospheric behavior to data that will be obtained from flybys and orbiters of other planetary bodies.

F. General Recommendations

1. COORDINATED PROGRAMS

To provide a quantitative description of the solar wind-magnetosphere-ionosphere-atmosphere system as a whole and the energy-transfer mechanisms operating within it, it will be necessary to achieve a more quantitative understanding of several fundamental plasma processes that are observed to occur within the system. It appears that, with but a few exceptions, experimental programs must take more coordinated, global approaches if the necessary advances in understanding are to be made. The IMS represents a natural evolutionary first step in this direction. We recommend that the terrestrial space-physics program of the future be largely oriented toward coordinated efforts involving coincident measurements made by several spacecraft at various positions within the ionosphere-magnetosphere-solar wind system. Attention must be paid to the coordination problem, with information made easily available on the relative positions of satellites at all times, on special opportunities arising out of favorable configurations. Ground-based measurements, including incoherent scatter, should also be included in the effort.

The need for correlative measurements will also apply to active experiments performed in AMPS. This is a logical step, both in magnetospheric investigations in particular and in the utilization of space technology for space research in general.

2. RAPID AND CENTRALIZED DATA HANDLING

The importance of efficiency in analyzing the great quantity of scientific data available from spacecraft cannot be overemphasized, particularly within the framework of present budgetary constraints. In the past serious delays have occurred both for a scientist obtaining the data from his satellite experiment and, subsequently, in the reduced data appearing in the data center where they are accessible to others who could contribute to the analysis of the observations. It is certainly not a wise balance of activity to make measurements in space at considerable cost and then not to follow up with equal enthusiasm the final dissemination of the results. We, therefore, endorse the use of advanced techniques for processing and transmitting data to provide for timely availability of the data in useful form to all interested scientists and engineers. It may be that augmented funding will be needed to ensure *de facto* accessibility to information such as instrument calibration data and other aids to understanding equipment peculiarities and computer time associated with the release of high-resolution data.

Two trends that impact data handling should also be noted. First, cooperation between experimenters on a satellite and prior agreement on data exchange among these experimenters are now well-accepted practices. The need is foreseen for a formal extension of this development to coordinated experiments involving several satellites and concomitant ground-based observations. Such experiments will need a carefully devised data-management plan, developed before flight, that includes the initial processing, the accumulation of the disparate data into appropriate sets for answering the scientific questions, and the placement of the reduced data sets in centers for studies by others.

The second trend is illustrated by the data-handling capabilities presently associated with the Atmospheric Explorer satellite. For proper operation at low altitudes, this satellite requires a more rapid availability of telemetry than has been customary with previous research satellites in space physics. The system has demonstrated the possibility of significantly reducing the time lag between measurements in space and the availability of those data for analysis. This improvement, which should be designed into all future systems, allows prompt and effective exploitation of space information. It is expected that the cost of the ground portion of the data-handling system will not exceed that of the traditional (slower) techniques; received signals are transformed into usable numerical quantities in essentially the same number of steps in either case.

It is also recommended that increased funds be made available for the analysis of past, present, and future space-physics data. An increase is especially endorsed in cases of coordination among disciplines and principal investigators and in cases of use of data from two or more satellites in orbit simultaneously or spatially organized in some particularly felicitous configuration, e.g., along the same magnetic-field line.

3. THEORETICAL RESEARCH

Theoretical and experimental studies in solar-terrestrial physics have a long history of productive partnership. "Physical understanding" really means to possess a theoretical description that has successfully withstood experimental testing. In addition, theoretical study is important in organizing experimental results within cohesive quantitative models, particularly when the observations themselves are made with numerous instruments under a variety of temporal and spatial conditions and when the physical phenomena under study are particularly complex (e.g., the aurora and atmospheric processes in the stratosphere). The lack of appropriate support for theoretical investigations actually reduces the effective accomplishments of space missions.

It is recommended that the funding available for theoretical research, which will increase in importance in the years immediately ahead, be increased to an appropriate level.

4. SOUNDING ROCKETS

A number of upper-atmospheric and ionospheric problems require data of a type that can only be provided by sounding rockets—for example, synoptic surveys of the latitudinal distribution of atmospheric species over a limited altitude interval and the altitude distribution of key species at a specific latitude and longitude. Sounding rockets also have an important role to play in magnetospheric physics and in testing equipment designed for future spacecraft.

As in previous studies,* it is again recommended that an increase in funds be allocated to sounding rockets for space-physics research as a high-priority item. Close attention should be given to the global distribution of rocket facilities in order to provide adequate coverage of important regions of interest.

In conjunction with continued sounding-rocket support, it is recommended that high-altitude parachute systems be developed to extend the flexibility of the rocket technique for making atmospheric measurements.

5. BALLOONS

High-altitude balloons (capable of achieving an altitude of 45 km) coupled with presently available parachute descent systems are capable of making fundamental contributions to atmospheric and magnetospheric research. For example, *in situ* measurements of the absolute concentration of atoms and radicals, which have been recognized as crucial to the photochemical balance in the stratosphere, will depend heavily on the existence of a strong balloon

*Committee on Rocket Research, *Sounding Rockets: Their Role in Space Research* (National Academy of Sciences—National Research Council, Washington, D.C., 1969).

program. Balloons not only offer a practical way to perform detailed studies on aspects of energetic electron precipitation and low-altitude electric fields (from which spatial and temporal characteristics can be separated) but also frequently represent the only way in which such measurements can be obtained. In addition, a large class of instrumentation intended for more elaborate spacecraft can undergo initial testing from such platforms. It is therefore recommended that balloon systems receive support and that their capabilities continue to be developed.

6. GROUND-BASED RESEARCH

Coordination of ground-based and space research is becoming increasingly important as space physics becomes more quantitative and definitive. The study of the effects of particle injection from rockets, balloons, or satellites will require simultaneous ground-based measurements at various locations, using incoherent scatter, photometers, magnetometers, ionosondes, vlf detectors, and riometers. It is recommended that NASA assure the funding of such auxiliary measurements, either through direct support or through cooperation with other interested agencies.

7. SUPPORTING RESEARCH

Supporting research (i.e., research not tied to specific spacecraft missions and including the balloon and sounding-rocket and ground-based research mentioned above) is an essential part of a well-designed space-physics program. The long-range health of the program depends on new instrumentation development, not aimed initially at a specific launch, so that future spacecraft payloads may be optimized. Analysis of data from past flights and theoretical work aimed at formulation of incisive hypotheses contribute to the wise selection of future missions. Supporting measurements made from platforms other than spacecraft enhance the range of interpretation of the measurements as well as adding directly to the understanding of major scientific questions of the impact of the near-space environment on lower regions of the atmosphere.

In spite of the well-defined role seen for these supporting activities, there is no way to calculate the exact fraction of the space budget that should be allotted to them in order to maximize the efficiency and assure the highest possible return for the investment. Some examples, presented as an indication of the importance of this type of support, include

1. The development of atomic and molecular resonant photon sources adapted for space application will provide the opportunity of accurate absolute concentration measurements in the part per trillion range that are not available by any other means. If the development of these devices precedes the immediate need, the final cost will be significantly reduced and the capabilities more fully realized. Such devices will become central to (a) stratospheric and mesospheric research, (b) planetary probes, and (c) thermospheric trace species studies from satellites.
2. Techniques now exist to perform charge composition measurements in the magnetosphere for energies below ~20 keV and above several hundred keV. The intervening energy range, ~20 to several hundred keV, is critical in the understanding of magnetospheric phenomena such as acceleration mechanisms, plasma processes, and wave-particle interactions. Composition measurements themselves are important in identifying sources of magnetospheric particles. The development of composition measurement techniques in the above energy range is an important task for future magnetospheric studies.

We believe that improved balance and efficiency would be realized if the funding level for supporting research were increased 10 percent per year over a three-year period to reach a 30 percent overall increase in level of effort. This proposal is intended to counteract the trend of recent years in which inflation has reduced the actual level of effort. The actual increases in funding should be larger than 10 percent per year to compensate for continuing inflation.

8. EXPLORER SPACECRAFT

The recommended program in space physics involves mainly Explorer-type spacecraft, which are relatively inexpensive. The total program is small (less than 10 percent) when expressed as a fraction of the budget of NASA's Office of Space Science. In view of the importance of the problems addressed, the relatively large numbers of scientists involved, and the consequently high scientific productivity, it is believed that the costs are well justified. Spacecraft of the Scout-Explorer size allow more flexibility in planning and greater opportunities for international cooperation than do larger spacecraft. To support the recommended program, however, and provide some Explorer-type launches for astronomy, the Explorer program itself

TABLE B.1 Recommended Program in Space Physics: New Starts and Baseline (in Chronological Order)—in Millions of Fiscal Year 1974 Dollars

Projected New Starts	Launch Dates	1976	1977	1978	1979	1980	1981	1982	1983	Total Project Cost[a]
Terrestrial Space Physics										
Electrodynamic satellites (3 vehicles)	1978	3	15	15	8	5	3	1		$ 50
Scout Explorer	1978		1	3	4.5	1	0.5			10
Solar Wind Explorer	1980			1.5	4.5	5.8	1.5	0.8	0.3	15
Atmospheric Explorer (solar maximum, 2 vehicles)	1980			4	16	13	5	4	3	45
Scout Explorer	1981				1	3	4.5	1	0.5	10
Atmospheric, Magnetospheric, and Plasmas-in-Space (AMPS)—Spacelab payload	1981 onward						X	X	X	
Magnetospheric Multiprobes	1982					6	21	25	7.5	60
TOTAL FISCAL YEAR COSTS (NEW STARTS)		3	16	23.5	34	33.5	35.5	31.8	11.3	
Planetary Space Physics										
Jupiter orbiter	1980		6	24	60	70	20	20	20	220
Mercury or Mars orbiter	1983					3	14	20	26	80
Interplanetary Space Physics										
Out-of-the-ecliptic mission	1979	1	16	29	33	14	4	2	1	100

Baseline

The baseline program for space physics includes $7.1 million per year for rockets, $0.4 million for balloon support, $2.4 million for data analysis, and $4 million for supporting research and technology. It is recommended that all these should be increased in level of effort by about 10 percent per year over a period of three years to establish a 30 percent overall increase in level of effort in these areas.

[a]Mission costs are rough estimates and they do *not* include launch costs. No cost information available for AMPS.

would have to be increased. It is believed that such an increase is well justified and that it would enhance the overall productivity of the space science program.

III. MISSION MODEL

The philosophy used in assembling the recommended mission model shown in Table B.1 has been to concentrate the activity alternately on atmospheric sensing and on magnetospheric sensing so as to maximize the number of active spacecraft that can contribute to coordinated measurements while limiting total expenditures for space physics and maintaining them at a relatively even level. The launch schedule of approved missions is shown in Table B.2. The next several years will be a period of active observation of the atmosphere by Atmospheric Explorers C, D, and E. In the latter years of this decade, there will be a period of concentrated magnetospheric observation associated with the International Magnetospheric Study. The next period of activity in atmospheric sensing is selected to be near the time of the next solar maximum,

TABLE B.2 Approved Programs of New Effort in Space Physics (for Reference)

Approved New Starts	1974	1975	1976	1977	1978
Atmosphere Explorer (AE) D and E	D	E			
Hawkeye	X				
Dual Air Density Explorer (DAD)		X			
International Sun-Earth Explorer (ISEE) A/B and C				A/B	C
Pioneer Venus					X

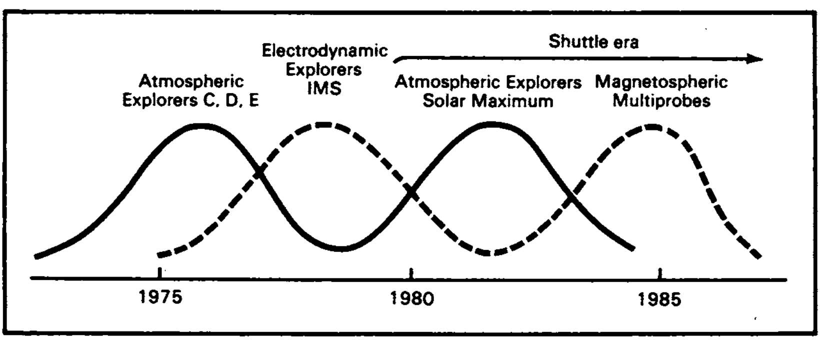

FIGURE B.1 Proposed phasing of atmospheric and magnetospheric missions. Solid curve: atmospheric; dashed curve: magnetospheric.

about 1981. (The present measurements with Atmosphere Explorers are near solar minimum.) Another period of renewed activity in magnetospheric sensing is projected for the mid-1980's when high-inclination orbits become available for the Shuttle. This phasing is illustrated in Figure B.1.

IV. RECOMENDED INDIVIDUAL MISSIONS

A. Electrodynamic Satellites

1. MISSION DESCRIPTION

The objective of the mission is to explore the details of the interactions that occur between the magnetosphere and the lower atmosphere and ionosphere. This exploration requires simultaneous measurements, especially of electric fields, in the interacting regions as well as in the intervening coupling region. The mission consists of closely spaced launches of three spacecraft into polar coplanar orbits. Two of the spacecraft are to be in eccentric orbits, and they should have an orbit adjustment capability similar to that of Atmosphere Explorer. It is planned that the apogee of these two spacecraft will be adjusted to cover an altitude range from 3 to 6 earth radii. The two eccentric orbits at times would have their lines of apsides in the equatorial plane and would move nearly parallel to geomagnetic field for considerable distances. The two spacecraft would traverse essentially the same orbit but at slightly different times to distinguish between spatial and temporal gradients along magnetic-field lines. One of the spacecraft should have a TV imaging capability to obtain nearly instantaneous images of the aurora. The third satellite, in a short-period, low-altitude orbit, would measure the atmosphere-ionosphere conditions near the feet of the magnetic-field lines traversed by the other two spacecraft.

2. SCIENTIFIC RATIONALE

The energy deposited in the high-latitude upper atmosphere by energetic particle precipitation, Joule heating, and the frictional drag between ion and neutral gases constitutes an appreciable fraction of the total thermal budget of the upper atmosphere. Similarly, the plasma convection pattern in the magnetosphere, which is driven by interaction with the solar wind, is constrained and influenced by the feedback effects from the atmosphere-ionosphere region to an as yet undetermined extent. Details of how this energy is transferred from the solar wind to the atmosphere are poorly understood at present, although it does appear that at least some direct coupling takes place in the magnetospheric cusp region. There are also suggestions that some particle acceleration occurs deep within the magnetosphere in association with the magnetic-field-aligned Birkeland currents that flow at auroral latitudes.

The interaction between magnetospheric hot and cold plasmas occurring when the plasmasheet overlaps the plasmasphere in the region of the plasmapause is not yet understood. This interaction may be responsible for magnetospheric phenomena such as sub-auroral red arcs, the amplification of electromagnetic waves, and the outer boundary of the cold plasma region. Unlike the reconstitution of the plasmasphere after a geomagnetic storm, its erosion during the beginning (main phase) of a storm is not well understood. Understanding of even the poststorm growth of the plasmasphere suffers from a serious lack of detailed knowledge of the geometrical and physical coupling between ionospheric regions near the feet of the field lines and plasmapause regions near the equatorial plane. In addition, the interaction of hot and cold plasmas on a geophysical scale under a variety of boundary conditions is an important topic in its own right, having applications at the sun, in the interplanetary medium, and in other planetary environments.

3. TIMING AND CONSTRAINTS

A 1978 launch of the Electrodynamic Satellites would enable them to make a strong contribution to the IMS.

B. Solar-Wind Explorer

The Solar-Wind Explorer will be placed in heliocentric orbit and positioned upstream of the earth's magnetosphere at a geocentric distance of the order of 1.5 million km, where there is an orbital libration point. This vehicle will have some onboard propulsion, although, once placed in the vicinity of the libration point, it will remain there ahead of the earth to study the properties and dynamics of the unperturbed solar wind and interplanetary magnetic field as they exist roughly 1 h before they strike the earth's magnetosphere.

1. SCIENTIFIC RATIONALE

The solar wind and interplanetary magnetic field provide the principal energy source and the modulating mechanism that regulate much of magnetospheric dynamics. Most studies based on measurements made within the magnetosphere need correlative data of the solar-wind input function. Solar-wind measurements made near the earth are complicated by the propagation upstream of both waves and energetic particles that are generated at or near the earth's bow shock. This perturbing influence must be minimized by placing the spacecraft approximately 250 earth radii upstream of the earth's bow shock. The data from the Solar-Wind Explorer, in combination with data obtained within the magnetosphere and geomagnetic tail, should enable the development of quantitative theories that describe the coupling of solar-wind energy into the magnetosphere. This knowledge can be put to practical use by increasing our ability to predict periods of high geomagnetic activity, which can cause disruptions in communications. The availability of a long-term data base on solar-wind properties over a solar cycle or longer will be useful in checking long-term correlative theories. For example, there exists the intriguing possibility that these data may someday be useful to the development of theories of terrestrial weather and climate that involve a physical coupling of solar activity with weather.

The basic process wherein the energy of one magnetized plasma (the solar wind) is transferred to another (the magnetosphere) is undoubtedly a universal one. The quantitative solution for the case of the solar wind–magnetosphere system, therefore, should be generally applicable to other systems of interest to space physics, astrophysics, and laboratory plasma physics.

2. TIMING AND CONSTRAINTS

The Solar Wind Explorer is recommended as a successor to ISEE-C and should be scheduled accordingly.

C. Atmosphere Explorers F and G

1. MISSION DESCRIPTION

The objective of the Atmosphere Explorer F and G mission is to improve our understanding of the dynamics, chemistry, and thermal structure of the upper atmosphere. The mission consists of the closely spaced launch of two orbit-adjustable spacecraft near the maximum of the solar cycle, one in a near-polar and the other in a near-equatorial orbit. These orbits will originally be eccentric but through the use of drag and propulsion will be made circular in the final stages of the mission.

2. SCIENTIFIC RATIONALE

Although the dynamic behavior of the upper atmosphere has only slowly been recognized, its importance is now beginning to be understood. Upper atmospheric motions are driven by tidal forces, solar ultraviolet heat inputs, gravity waves, and energy from the solar wind coupled through the magnetosphere. The need to examine these motions, together with their effects, has stimulated the development of new measurement techniques to determine the motions and to examine the distributions of minor but important atmospheric constituents affected by these motions. The corresponding additions to the payload, as well as magnetometers and electric-field antennas, were omitted from previous AE spacecraft. Neutral species detection techniques will study neutral wind velocities and the absolute concentration of H, O, N, NO, H_2, OH, O_2, H_2O, and CO_2. A major opportunity exists for cooperation between such an experiment and active experiments in other areas of space physics.

The polar-orbit satellite will measure the important energy inputs in and near the auroral zones. The equatorial-orbit satellite will measure the diurnal changes or local-time gradients. Thus, the chosen combination of orbits is considered much more valuable than alternative configurations.

3. TIMING AND CONSTRAINTS

The eliptic phase of AE-C, -D, and -E will occur almost exclusively during sunspot minimum. The uv heat input to the upper atmosphere and the solar-wind heating vary greatly, but not necessarily linearly, with solar activity. Some important atmospheric parameters in turn do not react linearly to these heat inputs. It will be of great value, therefore, to have Atmosphere Explorer F and G spacecraft launched near the next solar maximum, which is expected in 1981.

In addition, much of the technology required to implement the desired measurements of atmospheric motions and minor constituent concentrations was not available for AE-C, -D, and -E, but is being developed now and is expected to be ready for the solar maximum AE mission.

D. Magnetospheric Multiprobes

1. MISSION DESCRIPTION

Six spacecraft with compatible instrumentation and telemetry data systems will be placed in different parts of the magnetosphere to operate as an integrated system. One possible set of suitable vehicle orbits is as follows:

(a) one low-altitude polar orbit;

(b) one eccentric orbit with apogee near the equator at 6 earth radii geocentric distance and perigee near 3 earth radii;

(c) one geostationary orbit;

(d) two eccentric orbits, with apogees (about 180° apart in longitude) near equatorial latitudes at 40 earth radii distance;

(e) one eccentric orbit with apogee over a pole at 15 earth radii geocentric distance.

2. SCIENTIFIC RATIONALE

Magnetospheric physics has naturally been evolving toward a more coordinated program of spacecraft measurements. Yet coordination has so far mainly involved procedures in which data from satellites already in orbit are collected, pooled together, and analyzed for individual studies or more or less isolated events or periods of time. The Magnetospheric Multiprobes represent the logical solution to this need for coordinated measurements. By sending a family of spacecraft to make simultaneous measurements with similar instrumentation at several locations throughout the magnetosphere, a global view of magnetospheric dynamics can be obtained that will permit the separation of space variations from time variations and will aid in distinguishing cause from effect. The Magnetospheric Multiprobe experiment, with coordinated experiments and compatible data systems, will allow comparison and synthesis of disparate dynamical processes that occur throughout the magnetosphere. A powerful attack on the problem of entry of solar-wind plasma into the magnetosphere should then be possible. These spacecraft would also be logically used in conjunction with AMPS as remote detecting stations in support of active experiments.

3. TIMING AND CONSTRAINTS

The Magnetospheric Multiprobes are recommended for flight toward the end of the period under consideration to take advantage of information that will come from the earlier part of the program. The

Magnetospheric Multiprobes should be treated as a single facility with coordination and full data exchange among all Principal Investigators. Furthermore, all component vehicles of the multiprobes system should be designed with compatible data systems so that rapid coordination of data is feasible.

E. Scout-Launched Explorers

There is still a role to be played by the small satellites that can be launched from Scout vehicles and that also can be launched on a hitchhiker basis from the Shuttle. These provide more flexibility and shorter reaction times than the larger satellite projects, and their objectives should not be specified far in advance. Such satellites should, of course, be used in a coordinated way with other satellites. As an example of a Scout-launched Explorer, an Equatorial Waves and Particles Satellite is described, but other possible objectives should be equally considered.

1. MISSION DESCRIPTION

This mission consists of a Scout-launched spin-stabilized satellite launched into a near equatorial orbit ($< 20°$ inclination) with an apogee of ~ 1000 km and a perigee of ~ 250 km. The minimum payload would include instruments to measure fields, energetic particles, thermal plasma, and optical emissions. A reorientation capability on the satellite will probably be required.

2. SCIENTIFIC RATIONALE

The first objective of this mission is to study the anomalous presence of energetic particles (10 eV to 10 keV), vlf hiss, and optical emissions at low altitudes (< 1000 km) near the magnetic equator. Two sources of these particles have been theorized: first, from ring-current protons that charge exchange with exospheric neutral hydrogen, and precipitate as in the proton aurora, producing up to the order of 10 Rayleighs of Balmer α (and probably as much N_2^+ 3914 Å emission); second, from energetic plasmapause protons and electrons with pitch angles near 90°, which are transported to small L values by cross-field diffusion.

The second objective is to study the causes of the long-recognized "Appleton anomaly" of plasma distribution near the dip equator, including the north–south asymmetry of the ion concentrations and heights.

3. TIMING

To investigate the coupling (or lack of it) between the low-latitude ionosphere and the remainder of the magnetosphere, this satellite should be launched during a period in which data are available from other spacecraft. Specific examples would be the approved Mother-Daughter system or the proposed Electrodynamics Satellites or Magnetospheric Multiprobe Satellites. Particular emphasis should be placed on concurrent electric-field measurements. In addition, it would be advisable to have coordinated rocket launches to separate vertical and horizontal dependences.

F. Jupiter Orbiter

A Jupiter orbiter mission can provide a valuable opportunity for space-physics measurements near a planet very different from the earth. (The Venus orbiter, already an approved program, provides a similar opportunity on another very different planet.) The Space Physics Committee, therefore, *highly recommends* the incorporation of this mission into the planetary program. It should be equipped with adequate instrumentation to cover all relevant ranges of particle energies and wave frequencies, vector measurements of static magnetic and electric fields, and atmospheric and ionospheric measurements. A moderately low-inclination orbit is recommended as the most suitable for magnetospheric research. The space-physics measurements can contribute substantially to the planetological objectives of the mission, particularly giving information relating to the atmosphere, its evolution, and its interaction with the interplanetary medium.

G. Out-of-Ecliptic Mission

A spacecraft going to high solar latitudes in heliocentric orbit has value to several areas of space physics, among which are the following:

1. *Solar Wind.* Interesting north-south asymmetries of the solar wind have been observed in the ecliptic plane. These asymmetries have been attributed by inference to different solar-wind properties at midlatitudes where sunspot and flare activity is greatest. For lack of information, the solar wind has been treated in most theoretical

models as spherically symmetric. It would be of great value in generalizing solar-wind theory to check this assumption by direct measurement.

2. *Cosmic Rays*. The polar interplanetary magnetic field is probably less twisted than the Archimedean spiral structure that exists at low heliocentric latitudes. Low-energy cosmic rays from interstellar space should therefore be able to penetrate the inner solar system more readily over the polar regions of the sun than near the ecliptic plane. A cosmic-ray measurement at high solar latitudes could give valuable information on the intensity and composition of cosmic rays in interstellar space.

The recommended mission utilizes electric propulsion to increase the inclination of the orbit. This procedure maintains a heliocentric distance of 1 AU and provides repetitive scans in heliographic latitude—a feature that is much superior to the single scan provided by a Jupiter swingby mission.

V. OTHER MISSIONS UNDER CONSIDERATION

A. Outer Planetary Flybys

Planetary flybys can be compared to the early exploratory stage of rocket studies of the upper atmosphere. Much more systematic, comprehensive data on planetary atmospheres and planetary radiation environment can be obtained from orbiters, but the flyby opportunities are valuable first-look opportunities that can be used to provide better designed experiments for orbiters.

Outer-planet flybys that go far out in the solar system can provide valuable opportunities to examine cosmic-radiation gradients near the interface of the planetary system with interstellar space. It is recommended that such missions carry cosmic radiation and plasma instrumentation.

B. Relativity Experiment

The gyroscope relativity experiment (described in the 1970 SSB study, *Priorities for Space Research 1971-1980*, pp. 90-93) is regarded as potentially important because it is one of the few experiments oriented toward probing the present formulation of the basic laws of physics. The experiment would provide a unique test of Einstein's general theory of relativity. Because of its potential importance to the entire field of physics, the Working Group on Gravitational Physics recommended in the 1970 Priorities Study that this experiment receive continuing support, with the proviso that "It [the gyroscope experiment] should be subjected to detailed review with regard to both feasibility and cost at an appropriate time prior to commitment to flight in 1974." Work on this experiment is continuing, and it seems timely to implement the detailed review called for in the 1970 study before a final judgment is made.

Committee on Planetary and Lunar Exploration

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C
Planetary and Lunar Exploration

I. INTRODUCTION

As has been often and ably pointed out by predecessors of this committee, there are several forms of justification for space exploration. These fall into two basic categories: justification in the eyes of the populace that must pay the cost and justification in the eyes of the scientific community. Although the second of these is specifically within the purview of this group, we believe, particularly faced with limited resources for space exploration, that *any justifiable mission must have both strong scientific and public appeal.* The space-exploration program cannot survive otherwise.

It is the task of this group to ensure that the scientific justification is strongly met. It is appropriate clearly to delineate the forms that scientific justification can take. Any scientific investigation can be singly described as having two components: first, discover the questions, the mysteries that warrant our scientific attention and, second, apply those available scientific tools and methods of logic to derive the most logical model of understanding. Too often we as scientists may forget that the exploration, the search of the unknown—for mystery—is that precious substance upon which all science ultimately feeds. Those who cannot realize the basic value in exploring for new questions and offer only solutions for obvious questions fall short of the basic scientific goal.

Clearly a balance is necessary. We must conduct sober scientific study of questions posed. Only thereby do we expose and define the unknown. But we must assure that our sober conduct does not totally override and smother the search for mystery. To claim that exploration is not the motivation for the investigation of the solar system is to assert emphatically that the unknown scientific question is not worth knowing. It is with these convictions that we approached our task.

In this report, the Committee on Planetary and Lunar Exploration assesses the current and projected solar-system program and makes recommendations for future programs. Our conclusions were developed during the course of four meetings held in conjunction with the SSB 1974 summer study on Future Exploration of Mars.

II. MAJOR CONCLUSIONS

We have reviewed the rationale for the present program on planetary and lunar exploration, which arose out of several SSB summer studies.[1-5] Pioneer 11 is on the way to Saturn via Jupiter, Mariner Venus Mercury is about to encounter Mercury the second time, Viking is within a year of launch, and Mariner Jupiter/Saturn and Pioneer Venus are approved for launch in 1977 and 1978, respectively. This mix of missions reflects the great importance given to a balanced strategy for exploring the solar system, proceeding on a broad front to a variety of objectives by increasingly sophisticated missions. In our discussion of the objectives and rationale for continuing this program, we find that the criterion of a balanced, broad-scale approach remains overriding. Crucial missions for the next ten years are those with unique new approaches (e.g., comet Encke intercept, outer-planet entry probe, Venus radar mapper, Mars surface penetrators).

At the same time, the program should embody certain keystone objectives. The Viking project is NASA's current major effort aimed at elucidating the question of whether life exists on Mars. The Committee believes that the next decade is a timely opportunity for some concentration on the outer planets[6] (as recommended by the Outer Planets Scientific Advisory Committee), with a Mariner Jupiter orbiter as the focus of this effort. Subsequently, return of a Mars surface sample in the late 1980's is an extremely attractive major objective (as recommended by the SSB 1974 summer study on Future Exploration of Mars).

In an attempt to determine what kinds of future programs might realistically come about under a constrained space-science budget, we have developed program models that meet our criteria for a sound exploration strategy and that fit within a funding constraint. It was found that a reasonable, broad-based program is conceivable within the envelope used, which is half of the estimated NASA Office of Space Science planning wedge; however, the margin is quite narrow. Any substantial reduction in resources below this level would have an impact of significantly greater proportion, because of the disruption in program continuity. It is this continuity that provides much of the cost savings. We also recognize that a great deal of tension can exist between the large, expensive, keystone missions and the remainder of the program, and the cost overruns on these large projects erode future resources and threaten the viability of the entire program.

The program achieves cost efficiency by using generically related spacecraft series and experimental payload concepts that have already been developed. The pacing of the program is deliberate, thereby attempting to minimize funding peaks in any given fiscal year. In keeping with our support for "standardization," we urge the early incorporation of three new experiment concepts in the program: atmospheric probes, survivable hard landing probes (penetrators), and geochemical remote-sensing payloads. Each will be the prototype of a generic series of related missions to different bodies; thus, a Jupiter entry probe would be the prototype of eventual probes to Saturn, Titan, and Uranus; a penetrator mission to Mars would be the first of a series to Mercury, the moon, and other solid bodies; and a geochemical remote sensor on a lunar and Mars polar orbiter would be the baseline on which to build experiments for similar missions to Mercury and Jupiter.

III. PHILOSOPHY AND STRATEGY OF SOLAR-SYSTEM EXPLORATION

In this report, the Committee reviews and evaluates the solar-system exploration program and offers its best collective advice on how this program can be given continued shape and direction during the next decade under the constraints of tight funding. It is appropriate, then, that some discussion of the desired overall strategy be provided.

A. Stages of Exploration

As the first step of a qualitative scale of our progress on a given planet we may speak of *reconnaissance*, in which major global characteristics and parameters are sought. Reconnaissance tells us qualitatively what the planet is like and provides enough information to allow us to proceed to the stage of *exploration of the planet*.

Exploration seeks the systematic understanding of the planet on a global scale: the history, processes, and evolution of its surface and atmosphere. In the final step, that of *study*, specific problems of high importance and interest are pursued in depth.

B. Staging of Missions

The mission techniques, which parallel the stages of exploration, are ground-based, flyby, orbiter, probe (atmospheric and surface), soft lander, sample return. In the case of every body within the solar system that we may wish to examine in detail, we start with some basic knowledge obtained from earth-based observations. It is interesting that the impetus given to solar-system exploration by the space program has stimulated dedicated observation of solar-system objects with newly developed ground-based technology. There has resulted an explosion of new knowledge about remote bodies that has greatly assisted the planning of spacecraft missions to those bodies, making the early exploratory missions much more sophisticated and meaningful. It is important that continuing new sophistication on the ground continue to permit increased sophistication in space.

Even so, the best ground-based knowledge of distant objects is grossly inadequate and poorly definitive. Thus, the first step in spacecraft observations of remote objects should usually be brief visits (flybys) designed to define the important parameters of the objects; this will allow the design of more specific investigative missions. Flyby spacecraft usually carry remote-sensing instrumentation, imaging, infrared, ultraviolet, and sometimes radio experiments, as well as *in situ* experiments designed to measure particles.

One type of follow-on mission involves a lengthy stay near the target object (orbiting and rendezvous missions). Such a spacecraft will usually carry a set of instruments similar to those on a flyby, but other specialized experiments may be added to assist in geophysical and geochemical mapping on a global scale. For example, for a body with little or no atmosphere the additional instruments might include an x-ray fluorescence spectrometer, a gamma-ray spectrometer, a visual-infrared reflectance spectrometer, color imaging, a radar altimeter, and a radar sounder. Many of these instruments involve newly emerging technologies of remote sensing, and the orbiting and rendezvous spacecraft of the future will have vastly greater capabilities than those of the last few years.

Another type of follow-on mission involves *in situ* measurements at one or a few locations on the target object (atmospheric entry probes, hard and soft landers). An atmospheric entry probe might carry an accelerometer, thermometer, barometer, radiometer, and mass spectrometer, as well as other specialized instruments. A new and promising type of hard lander is the penetrator, designed to plunge a few meters below the surface and make local measurements of chemical composition, heat flow, and seismic activity. Soft landers carry instrumentation designed for geophysical, geochemical, and possibly biological investigations of the surface at the place of landing.

The above types of mission are elements in a progressive exploration strategy for some object. There is some recent tendency to have a mixture of these elements in missions designed for remote objects. The optimum strategy for a particular object usually depends on spacecraft trajectory and local environmental constraints.

The ultimate exploration strategy utilized or contemplated at present involves the return of samples from the target object for examination in terrestrial laboratories. In more sophisticated missions, the collection of samples would involve a roving capability in the vicinity of the landing site, a suitable documentation of the samples recovered, and possibly a preliminary examination and selection of the returned samples.

C. Status of Current Program

Table C.1 summarizes the stage of exploration for each major body or class of bodies. It may be seen that exploration is further advanced for the closer and more massive objects. The dominance, for example, of the moon and Mars as being of special interest to the wider public is clearly manifest. In both instances an advanced and more expensive technology was undertaken due to the high priority assigned the moon landings and the search for life on Mars. Otherwise, the program has maintained the balance required to return information over the full range of possibilities. Some of the most meaningful findings have come in fact from the comparisons made possible among all the planets studied.

Table C.1 also shows the major proposed missions for a period of approximately ten years in the future. Many of these have been incorporated into a program model (see Table C.2 in Section V) that is designed to achieve a balanced overall effort and to address itself to the major scientific issues of this stage of exploration.

TABLE C.1 Exploration Stages

	COMETS	ASTEROIDS	MERCURY	MOON	MARS	VENUS Atm.	VENUS Surface	JUPITER Planet	JUPITER Satellite	SATURN Planet	SATURN Titan	URANUS	NEPTUNE
First Look	Routine Ground-Based	Conventional Ground-Based	Mariner Venus/Mercury ('73)	Ranger	Mariner 4	Mariner 2	Ground-Based Radar	Ground-Based Optical & Radio	Ground-Based	Ground-Based	Ground-Based	Ground-Based Stratoscope	Ground-Based
	Kouhoutek Watch	Ground-Based IR survey	MVM pass 2–3	Lunar Orbiter	Mariner 6, 7	Mariner 5	PV '78	Pioneer 10 & 11	Pioneer 10 & 11	Pioneer 11	MJS '77	LST	
(Recon)	Ballistic Flyby(s) (Encke '80)	Flyby(s) or Rendezvous(s)	Mercury Orbiter ('83)	(Surveyor)	Mariner 9	MVM '73	VOIR ('84)	MJS '77	MJS '77	MJS '77	Atmospheric Probe Mission	MJU '79	
	Rendezvous (Tempel II '86)	Sample Return	Penetrometer Mission	Lunar Polar Orbiter ('78)	Mars Polar Orbiter ('81)	PV '78		Pioneer Jupiter Orb. & probe ('80)	Mariner Jupiter Orbiter ('85)	Probe Mission			
Exploration	Sample Return		Sample Return	Penetrometer Mission	Mars Penetrometer ('84)	Survivable Lander (~1990)			Penetrator Missions	Mariner Saturn Orbiter			
				Apollo Sample Return	Viking 1975								
Study					Mars Surface Sample Return (late '80's)								

Completed Missions | Approved or In Flight | Recommended Missions

D. Major Scientific Objectives for the Next Decade

In the preceding sections we have discussed the rationale and strategy for exploration. In the discussions on the major program areas we will review the scientific issues of interest. A full scientific review and discussion about each planet in detail would run to several volumes and is far beyond the scope of this report. References 1–7 are among the documents that review and discuss major scientific objectives.

The major scientific objectives that give form to our recommended program are

1. The earthlike bodies, Mars and Venus, remain of special interest. We seek to understand the decisive factors that make them so unlike earth, especially as regards their conditions of formation. We hope to resolve the question of whether life exists on Mars; more generally, we want to know what the history of water has been on that planet and whether much water remains below the surface. For both bodies we seek to characterize their bulk composition and general energy budgets. The apparent existence of large impact basins seemingly makes Venus even less like the earth than had been thought and increases the desirability of getting high-resolution radar images.

2. The giant planets, Jupiter and Saturn, with their near-solar composition, their complex atmospheric and particle envelopes, and their elaborate satellite systems represent a macrocosm that was visited for the first time by Pioneers 10 and 11. We *recommend* that a major exploratory effort be devoted to these planets during the period of favorable opportunities 1977–1985. This effort should include *in situ* determination of the composition of the Jovian atmosphere; remote sensing of the surfaces of the satellites; monitoring of the particles, fields, and radio emissions; as well as the interactions of the satellites with the particle/field envelope.

3. The icy/rocky objects are an important intermediate class of bodies that should provide critical constraints on the history of the planets. These objects include Uranus and Neptune, many of the satellites of the outer planets, and the comets. A mission to study the composition of the gas and dust in a cometary coma is of high priority. A first visit to the outer solar system (Uranus) is also a major objective.

4. Examination in earth laboratories of samples returned from bodies in the solar system remains the most fruitful and therefore a guiding long-term objective. Decades of automated study *in situ*

cannot provide the profound and detailed level of understanding afforded by returned samples. Return of samples from Mars for biological and geological study in the late 1980's is the most attractive choice for the next sample-return objective.

IV. RECOMMENDATIONS FOR MAJOR PROGRAM AREAS

A. Mars

The material on Mars appears separately in Working Paper E, which resulted from a special summer study entitled "Future Exploration of Mars," held August 20–26, 1974. The Committee on Planetary and Lunar Exploration has reviewed the findings of the Mars study and has included them in the overall considerations and recommendations of the lunar/planetary program.

B. Venus

Long called earth's sister planet, Venus bears a superficial resemblance to earth in size and mean density. There the similarity ends; a thick atmosphere of predominantly carbon dioxide shrouds the planet, producing a surface pressure near 100 bars and a surface temperature near 750 K. No optical imaging of the surface is possible through the perpetual cloud cover, nor are substantial cloud features seen at optical wavelengths, although high-altitude cloud structure is evident in the ultraviolet.

Because of its dense and unusual atmosphere, most interest in Venus to date has centered on its unknown meteorology and cloud composition. Mariners 2 (1962), 5 (1967), and 10 (1974), all flybys, were oriented almost entirely toward atmospheric and ionospheric studies. The Soviet Venera series of probes has provided a vertical cloud-structure profile and some limited information on surface composition.

Most of our information on the surface of Venus, oddly enough, has come through relatively recent developments in ground-based planetary radar. Early radar observations in 1961 and 1962 established the slow (243-day sidereal period) retrograde rotation of the solid planet, gave estimates of its average surface electrical properties, and determined the mean equatorial radius (6050 km). In the years since, radar has given us a detailed picture of the equatorial height profile, has established the presence of differentiated scattering features, and in the most highly resolved recent radar maps has shown the existence of many large craters. Within the next few years, improved ground-based facilities are expected to provide 5-km or better resolution of surface features over much of the planet, together with extended topographic data.

Our questions about Venus are legion, extending from the nature of the interaction between the outer reaches of its atmosphere and the solar wind down to the composition of its inner core. How can we best utilize spacecraft to answer these questions? Three Mariner flybys have studied the upper atmosphere and given us an accurate estimate of the planet's mass. Five Venera probes have measured the atmospheric temperature–pressure profile and sampled the gaseous composition. One Venera probe has observed the ambient light intensity and measured the surface uranium/thorium ratio. The obvious next step is to emplace a long-lived orbiter and to attempt multiple, simultaneous probe entries; and this is, in fact, planned using a pair of U.S. Pioneer Venus missions in 1978. These missions should extend the earlier spacecraft and ground-based measurements over nearly the whole planet. Detailed in-depth atmospheric probing at a few spatially limited sites, together with global observations from orbit over a long period of time, should provide valuable and complementary scientific data.

High-resolution optical photography of the surface of Venus is not possible, and the wealth of geological inference that flowed from such orbital observations of Mars must be sought for Venus by using imaging radar to penetrate the cloud cover. The suggestion from ground-based radar that Venus has large impact basins of presumably great age is yet another unexpected way in which Venus differs from earth. An imaging radar with a resolution of 100 m is technically feasible in the early 1980's. A high-quality imaging mission of Venus is a basic building block of solar-system exploration and should be flown as soon as feasible. The results could be quite novel and evoke wide interest.

Further understanding of Venus would depend on the ability to deliver and operate a survivable surface probe to monitor composition, radioactivity, and seismicity. For the time being, we can only regard this kind of mission as highly desirable, if possible, in the late 1980's or early 1990's.

SUMMARY The natural exploration sequence for Venus would consist of

(a) an orbiter/atmospheric probe mission;
(b) imaging radar;
(c) survivable surface probes.

The missions available are

		Earliest Possible Launch Date	Launch Date in Mission Model
(a)	Pioneer Venus	1978 (approved)	1978
(b)	Venus Orbiting Imaging Radar (VOIR)	ca. 1981	1983
(c)	Undefined surface probe mission	ca. 1987	later than 1987

A Pioneer balloon mission is available for detailed study of the atmospheric circulation but is not at present an essential part of the main exploration sequence.

RECOMMENDATION It is the committee's recommendation that Pioneer Venus '78 and the Venus Orbiting Imaging Radar form the basic two-pronged exploration sequence for the foreseeable future. The United States has had a fairly weak Venus exploration effort over a period when the Soviet Venus program has been substantially larger. These missions would change the situation entirely and help to resolve many of the mysteries still surrounding Venus.

C. Outer Planets

The task of planetary exploration is twofold: First it is to provide as much information as possible about the nature of each planet, its composition, the structure of its interiors, its surfaces, its atmospheres, its dynamics, and its satellites. Apart from information obtained from imagery, a spacecraft can give good data on the mass, radius, oblateness, atmospheric composition, gravitational coefficients, surface temperature, heat flux, magnetic field, and other parameters. While some of this information is available from earth, it is usually not accurate and subject to great uncertainties. Second, on the basis of the available information about the planets one should be able to construct a self-consistent model of the very early history and evolution of the planetary system from the original solar nebula. The outer planets, Jupiter, Saturn, Uranus, Neptune, and Pluto, play a crucial role in the second of these endeavors because their combined mass and angular momentum represent more than 99 percent of the mass and momentum of all planets. We have already considerable initial knowledge of Jupiter, but the factual information about the other planets decreases rapidly with increasing distance, so that for Pluto even the density is poorly known. So far only Jupiter has been explored by spacecraft (Pioneer 10); another spacecraft is en route (Pioneer 11), and the Mariner Jupiter Saturn Mission is approved for 1977.

The major planets are seen by their mean-density differences to fall into two compositional groups. Jupiter and Saturn seem to have approximately solar composition with possible excess volatiles. Uranus and Neptune seem to be mostly rock and ices with significantly less than solar H and He, perhaps similar in composition to the comets. Modeling of the formation of the solar system indicates that the compositional differences reflect the conditions prevailing in the respective planetary orbits at that time. A major objective in solar-system exploration, then, is to increase the data base in order to increase our knowledge of the composition of these planets. Atmospheric probes for direct measurements down to several bars pressure and precision determination of the gravitational coefficients J_2, J_4, and M and the radius are the available possibilities for the large planets themselves, while various indirect methods of study of the satellites can also contribute. Variations in the mean densities of the satellites seem to indicate systematic compositional effects within the satellite systems due to accretion late in the history of these systems.

Telescopically observed differences in the atmospheres of the major planets suggest systematic effects related to the differing dynamic regimes, solar distances, and atmospheric compositions. Jupiter is characterized by strong zonal shear and large-scale turbulence, the red spot(s), and manifold cloud systems. Uranus, by contrast, may be both cloud-free and dynamically nearly quiescent. The unique disk-shaped magnetosphere of Jupiter raises the question of the magnetic fields of Saturn and Uranus, which may be present even in the absence of observed radio emissions. Uranus, with its 98° obliquity, would have unique magnetic-field interactions with the interplanetary plasma.

The satellites of the outer planets present a whole new class of geological and planetological evolution problems, for bodies with varying ratios of rock/ice. The high-resolution imaging possible with Mariner spacecraft could return to us by 1986 pictures of the major satellites of Jupiter, those of Uranus, and some of the satellites of Saturn. Those satellites that are comparable with Mercury and the moon in size are of special interest in the comparative planetology of the solid planets. Differences in cratering history of the rock surfaces as well as the processes acting on the icy surfaces will be of great interest. Titan, the only satellite known to have a substantial atmosphere, is a unique object that will become a major objective of study in the near future. Proposed gas envelopes around the satellite orbits may be significant to the physics of the planetary upper atmospheres and will be studied by missions to these planets. Finally, the rings of Saturn have their own special attraction and remain largely a mystery to be unraveled by spacecraft visits.

Interest in Uranus is now growing, because of the opportunity to use a Jupiter swingby for a Mariner visit to Uranus arriving there in 1986 after being launched in 1979. An entry probe can be carried on this mission. The icy composition, the expected stable, cloud-free atmosphere, the 98° obliquity, the anomalously compact satellite system in the equatorial plane—all these phenomena are different from anything seen on planets interior to Uranus. The opportunity will not recur until nearly the end of the century.

1. MAJOR SCIENTIFIC OBJECTIVES

With so many interesting objects for study, the strategy for exploration is quite unconstrained; it matters mainly that the reconnaissance of the outer planets should proceed. The Outer Planets Science Advisory Committee recommended that the next decade or so be a period of emphasis on the initial missions to the outer planets; we concur in that recommendation. The following major objectives may be used as a checklist for this period of reconnaissance:

1. Imaging of the planet and its major satellites with resolution substantially improved over that possible using earth-based observations;
2. First closeup look at the atmospheres of the planets and satellites by uv spectrometry, radio occultation, and ir spectrometry;

3. *In situ* sampling by mass spectrometer/entry probe of the atmosphere of one giant planet (Jupiter or Saturn) and one of the ice–rock planets (Uranus or Neptune);
4. Reconnaissance of the physics and morphology of the magnetic field and energetic particle envelope of each planet;
5. Precise determination of the gravitational parameters, masses, J_2, and J_4.

2. MISSION POSSIBILITIES

1. Mariner Jupiter/Saturn (MJS) 1977 (approved) and Mariner Jupiter/Uranus (MJU) 1979 provide a three-axis stabilized platform for flyby of the named planets. These missions would make major advances in all objectives except the direct entry probe. MJU with an entry probe to Uranus is a possibility but more costly. It has the disadvantage of a 7-year trip time on the flight of the first outer-planet probe.
2. A class of Pioneer probe missions to the outer planets is feasible. It is cost-effective only if two or three missions are flown. An entry probe may be put on the MJU spacecraft for Uranus entry or on a Pioneer Jupiter orbiter.
3. Orbital missions to Jupiter can provide spatial and time detail with respect to imaging, atmospheric sensing, and particle/field studies. A Mariner Jupiter orbiter is proposed, using gravity-assist techniques in equatorial orbit to achieve repeated close encounters with the satellites. Its objectives would be (a) the satellites and (b) synoptic study of the Jovian atmosphere. A Pioneer Jupiter orbiter is proposed, planned for a higher-inclination orbit, with objectives (a) the magnetic-field–energetic-particle envelope, (b) synoptic study of the Jovian atmosphere, and (optional) (c) an entry probe.

3. MISSION MODEL ADOPTED

We have adopted for reference a mission model that meets as many of the listed objectives as possible and that fits the following three additional criteria, which may be called the "balance" criteria:

(a) There should be at least one visit to the giant planets and one to the Uranian planets.

(b) There should be a Mariner Jupiter orbiter mission. This mission offers the greatest diversity and exploration potential.

(c) There should be an entry probe.

The Committee adopts the following:

		Earliest Feasible Launch Date	Launch Date Adopted in Mission Model
(a)	Mariner Jupiter/Uranus	1979 only (1986 encounter)	1979
(b)	Pioneer Jupiter orbiter and probe	1980-?	1980
(c)	Mariner Jupiter orbiter	1981 (conventional L.V.) 1981-1985 (Shuttle-Centaur)	1985

The exploration of the giant planets and especially of Jupiter has a high priority because it is of fundamental interest not only to planetology but also to space science and to the theory of stars such as small white dwarfs. The Mariner Jupiter orbiter mission can provide data about the planets and its satellites, while the Pioneer Jupiter orbiter mission is of special interest to space studies.

The 1981 opportunity permits the accomplishment of the Mariner Jupiter orbiter mission *without* the Shuttle or with the Shuttle–transstage system, while the Shuttle–Centaur–Double Kick-stage system would also be sufficient for the 1985 opportunity. These missions rely on the Ganymede swingby to attain a suitable orbit. Without this swingby only the Shuttle–Advanced TUG system would permit a 1985 mission.

We would recommend the following, if faced by the prospect of fewer funds:

(a) Mariner Jupiter/Uranus (with Jupiter probe)
(b) Mariner Jupiter orbiter

We also anticipate that a Mariner Saturn orbiter will be an attractive mission in the late 1980's.

4. COMMENTS

(a) JUPITER During outer-solar-system reconnaissance, Jupiter will become a frequently visited planet, because of both its richness of phenomena and its role as a booster to Saturn and Uranus. There is no real redundancy involved in these multiple visits; indeed, the multiplicity is justified by the phenomena alone.

(b) SATURN The flyby of Saturn by Pioneer 11 in 1980 and Mariner/Jupiter/Saturn in 1981 followed by a Mariner orbiter launch late in the decade constitute a possible program for this unique planet. The longer trip time, combined with substantial launch vehicle requirements, means that a substantially longer time will be required to achieve the same level of understanding of Saturn as we can achieve of Jupiter by the late 1980's. Following the Mariner/Jupiter/Saturn encounter, definition of a dedicated mission to Titan will become possible and highly desirable for a visit in 1988 through 1995.

(c) URANUS/NEPTUNE The 1979 Jupiter swingby opportunity will not recur until 1992. The mission can use a spare MJS spacecraft. The long trip time to Neptune effectively precludes a mission until nuclear electric propulsion is available.

(d) SPACE PHYSICS The physics of the interplanetary medium—the study of the particles and fields—is largely studied by experiments carried aboard specifically planetary spacecraft. A program of visits to the outer solar system would provide a major opportunity to define the behavior of the solar wind in this region and to study its interaction with the magnetospheres of the major planets. The disk-shaped Jovian field and the 98° oblique Uranian field both present novel phenomena. Visits beyond 10 AU will seek to find indications of the interface between the solar wind and the interstellar medium. The Jovian particles and fields environment, first sampled by Pioneer 10, is now seen as an extremely interesting system. Perhaps Io interaction with this environment is the most unique phenomenon requiring study. A Pioneer Jupiter orbiter in 1980, carrying experiments to study the Jovian magnetosphere, can be carried out in a way especially complementary to the planetary-oriented Mariner Jupiter orbiter.

(e) SPACE ASTRONOMY The contributions that might be made by the Large Space Telescope to the study of Uranus and Neptune require special assessment at this time to ensure that a 1979 MJU mission, which would be launched before the LST, is justifiable.

RECOMMENDATION We urge continued emphasis on outer-planet reconnaissance, begun with MJS, to take advantage of favorable launch opportunities, to culminate in a Mariner Jupiter orbiter.

D. Comets

Comets represent a unique group of bodies in the solar system. It is generally agreed that young comets represent material that has been recently perturbed from storage in distant orbits (~50,000 AU). The observation that comets are rich in volatile substances leads to models in which they are the least disturbed samples available of the primitive solar nebula and to models that give them a genetic relationship with the Uranian planets. Detailed study of the elemental, isotopic, and molecular composition of comets is of major importance in unraveling the cosmogony of the solar system.

Within the last few years, a picture of the structure and activity of a comet has emerged. The basic mass of the comet is contained in a nucleus that is only a few kilometers in radius. This nucleus contains a large number of solid dust particles with a matrix of frozen volatile gases. A major constituent of the solid gases is water, but there are many more exotic molecules such as CH_3CN, which was discovered in Comet Kohoutek. As a comet approaches the sun, the volatile gases start to sublime, and a large number of icy particles are ejected from the comet by the resulting gas streams. These particles lose their icy mantles by sublimation in the direct heat of the sun, giving rise to an extended coma of dust and gas surrounding the nucleus. Radiation pressure from the sun blows small dust particles away from the comet, and ionized gases are swept up by the solar wind, producing dust and ion tails that often have slightly different directions in space. As this activity continues, the surface of the cometary nucleus may become largely covered by a layer of dust that has not been blown away, and there may be smaller regions in which the underlying volatiles are directly exposed to sunlight, from which fountains or volcanoes of gas continually erupt, often in a highly erratic manner. It is these gas jets that are responsible for the nongravitational forces acting on a comet that continually modify its orbit and introduce some uncertainties into its predicted position in space.

Comet Encke is an ideal object for a first cometary mission because it has a smaller orbital period, 3.30 years, going between an aphelion distance of 4.09 AU and a perihelion distance of 0.34 AU. This close proximity to the sun has resulted in the exhaustion of most of the volatile gases in Encke, so that its activity is less than that of most comets, and hence its position in space is more accurately predictable. Nevertheless, it exhibits enough gaseous activity to give a good insight into typical cometary behavior.

Following a ballistic flyby of Encke, a rendezvous mission with Encke or a similar comet would be most productive. In a typical rendezvous, a solar-electric propulsion (SEP) module is required to bring the spacecraft into a station-keeping operation for ~100 days around perihelion. This would permit monitoring the composition and physical phenomena of the coma and tail during the solar heating episode.

It now appears that it is not possible to use the highly desirable SEP for a slow flyby of Comet Encke in 1980. Hence, the optimum scientific mission would involve a ballistic intercept of Comet Encke at its perihelion position, with a relative velocity of approach of 8 km/sec. This mission would require a Titan III vehicle for launch in 1980. The greatest scientific return would come from the use of the Mariner spacecraft carrying a camera having capabilities similar to that used on Mariner 10, with the usual complement of instruments measuring a variety of electromagnetic wavelengths and particles and fields in space, among which should be included a mass spectrometer. The ballistic flyby of about 8 km/sec at perihelion should be about optimum for the mass spectrometer, minimizing the effects of recombination on the walls of the spectrometer chamber and not sufficiently great to cause dissociation of the molecules in the cometary coma. It is reasonable to expect that a flyby distance of 500 to 1000 km can be obtained on such a mission, so that the Mariner 10 television camera capability would then allow photography of the cometary nucleus with a resolution substantially better than that obtained of the satellites of Mars in the Mariner 9 mission.

An alternative mission, which requires only an Atlas launch vehicle, would involve a ballistic encounter at a distance greater than perihelion of the cometary orbit, but the relative approach velocity in such a case would lie in the range 16 to 20 km/sec, and this is less scientifically desirable because it is likely that a substantial amount of dissociation would occur of the parent molecules detected by the mass spectrometer, and fewer pictures would be obtained during the flyby itself.

1. SUMMARY

The natural strategy for studying comets is as follows: (a) flyby, (b) rendezvous, (c) sample return. A sample return mission cannot be defined let alone flown at this time, and many aspects of a rendezvous will remain uncertain pending the findings of the first flyby. Rendezvous requires SEP development. Our proposed exploration sequence is thus

(a) Encke Mariner flyby, 1980.
(b) Tempel II rendezvous (SEP), 1986.

Both missions are limited to typical launch windows. The Tempel II rendezvous is suggested as representing the best opportunity to visit an interesting comet in the mid-1980's. The Encke flyby, however, is strongly recommended, and is the subject of a recommendation following. A Halley's flyby in 1985 would attract great public interest but involves such high encounter velocities as to be of very low scientific potential.

We note that cometary SEP rendezvous normally allows flyby of one or two interesting asteroids, which would make such a mission even more attractive.

RECOMMENDATION The question of identifying a first mission to a comet has been under study for several years. The Science Advisory Committee on Comets and Asteroids has proposed a baseline mission consisting of a 1980 ballistic flyby of Encke, which may be done with a spare Mariner Venus Mercury spacecraft. We strongly urge that NASA consider this mission as one of its key new programs for a prospective new start in fiscal year 1978.

E. Mercury, Moon, Satellites, Asteroids

The enormous scientific success of the Apollo missions has generated new momentum and interest in the exploration of the other smaller, moonlike bodies in the solar system. These objects, which include Mercury, the asteroids, Phobos and Diemos, the satellites of the outer planets, and, parenthetically, Mars, lend themselves to investigation by spacecraft in basically the same ways: optical imaging, to show craters, volcanism, erosion, and tectonic phenomena and their geological history; orbital x-ray and gamma-ray studies to show the distribution of the chemical composition over the surface at intermediate resolution; altitude orbital gravity and magnetic profiling to study the bulk properties; and basic techniques of lander and/or rendezvous instrumentation for studying surface samples.

Understanding the evolution of these bodies in the history of the solar system requires that they be approached as a common class of objects. Regarding the satellites in the outer solar system, where ices contribute to the surface and bulk properties, these statements may be premature; however, orbital or lander missions to these objects are not likely in the 1975-1985 period.

The planets in the inner solar system exhibit systematic variations in mean density that betoken comparable systematic variations in chemical composition. Current theoretical models of the formation of the solar system provide parametric means of explaining these variations in terms of the conditions in the solar nebulae at the time of formation. An understanding of these conditions thus awaits the precise determination of the chemical differences. By direct sampling it has been established that the moon is depleted in volatile elements relative to the earth and enriched in refractory elements. From the mean densities we infer an increasing iron content through the sequence moon, Mars, Venus, earth, Mercury. Water, which is essentially absent on the moon and Mercury, seems to be underabundant on Venus and Mars relative to the earth, although this question is as much one of escape of water as it is one of original water content.

Similarities and differences in the geological histories of these planets are sought as clues to their comparable internal evolution. Crater populations seen on Mars, the moon, and Mercury suggest that the impact histories of all the inner planets are similar and concentrated in the first billion years of history. Tectonic phenomena, however, as seen in volcanic structures, faulting, and iostatic adjustment differ from planet to planet and lend us an understanding of the relationships between internal heating and magma generation, differentiation, lithosphere, and the observed surface geology. Further great insight has been afforded by the lunar seismic stations, which indicate directly the modern incidence of seismicity, as well as the internal structure. Comparable information on the other solid bodies will eventually be sought.

A major objective, then, for the next decades is to establish the chemical differences between planets. First, major element and radiogenic element surface abundances may be determined globally by orbital gamma-ray and x-ray experiments. Lander experiments can provide more precise information about more elements at one point. Sample return will be ultimately needed to establish the full range of elemental and isotopic abundances and to provide the vital radiometric age information by which all the solid planets may be integrated into a single evolutionary framework. To relate this information to the bulk properties of the planets, and to assess the degree of compositional differentiation, we will seek to place seismic and heat-flow stations on these bodies.

Reconnaissance photography, the first step in studying a planet, has been achieved for the moon, Mercury, Mars, and Phobos/Diemos. The recommended Mariner Jupiter orbiter and Mariner Saturn orbiter should achieve this objective for the major moons of those planets by 1990. The constraints of the 1973 Venus/Mercury mission leave us with over 50 percent of the planet photographed poorly or not at all and the remaining surface photographed at only one lighting angle. Extension of reconnaissance photography to other bodies probably depends on the availability of SEP in the 1980's, particularly for Mercury and the asteroids.

Other techniques have been tried on the moon, in degrees ranging from sophisticated multiple coverage to first trial of a concept. We have included in the mission model a 1978 lunar polar orbiter, which employs a low-cost spacecraft to apply orbital techniques to the systematic study of the chemistry and physics of the lunar crust. We propose, as the basic strategy for the coming decade, that comprehensive orbital geochemical/geophysical satellites be placed around the moon, Mars,* and Mercury. In all three cases, completion of the reconnaissance photography and conduct of more advance imaging experiments would be included.

The experience with lunar samples has shown clearly the great value of precision measurements on earth of returned samples. In addition, techniques of *in situ* sample study, while not competitive with laboratory analysis, have a potentially useful role in an orderly strategy for study of a planet. The inorganic and organic analysis experiments on Viking 1975 are of this sort. In the orderly progression of missions, then, landing/rendezvous and sample return will eventually take their place as the highest priority activities.

A sample return from Mars in the 1980's would be the first such mission beyond the Viking landers. Equally feasible, if not more so, in the same time period is a sample return from Phobos, the most easily accomplished sample return of all the possibilities. We have no recommendations about a full program of landing on and sample

*This is just one of several Mars objectives, which are taken up in the section of this report devoted to Mars (Working Paper E).

return from the minor planets, which would certainly take several decades. We merely point out that all missions of this class are bound to be substantially costlier than the space-science budget presently allows, regardless of the spacing between missions. Progress in studies of the smaller planets and Mars beyond 1984 will be dependent on a decision by NASA to carry such activities as a part of its program.

The Mariner 10 results have focused scientific interest on Mercury as an object of great interest and uniqueness. It will be necessary to plan a return mission to Mercury sometime in the next decade, probably an advanced orbiter to follow up these tantalizing first results. Surface phenomena connected with a large multiringed basin-forming impact show distinct differences from those on the moon. A noticeable magnetic field was detected. Markings and albedo variations seen from the earth indicate that the half of Mercury in darkness for Mariner 10 will have major differences from the photographed sunlit side. These observations fit into models of Mercury having a major iron core or iron-rich interior. Magnetic anomaly and gravity anomaly mapping from orbit will have an important bearing on this question.

Hard-impact penetrometer probes that can emplace sensors several meters below the surface of the solid planets would provide an alternative to the costly, soft-landing missions of the Viking and Surveyor classes. A multistation seismic/heat-flow/chemical analysis network would be feasible with a constrained program. We urge that NASA continue studies to define the capabilities and costs of penetrometer missions and *recommend* that the scientific community begin thinking about framing a quality science effort with this technology.

1. SUMMARY

The natural exploration sequence for any one of these bodies would be (a) flyby; (b) orbiter; (c) survivable hard landers (penetrators); (d) sample return, with possible soft-landed precursors. For the moon, the early completion of sample return has put the scientific strategy a bit out of order. Geochemical and geophysical orbital mapping are needed to develop a global view, integrated with the sample data. Eventually survivable hard landers would be the way to extend sample coverage to non-Apollo areas of the moon, short of an unmanned, Soviet-style sample return. We thus have, for the moon, this strategy:

	Earliest Feasible Launch Date	Proposed Launch Date in Model
Lunar polar orbiter	1978	1978
Surface probes	1984?	later than 1986
Sample return	1984?	later than 1986

For Mercury, the sequence is straightforward:

	Earliest Feasible Launch Date	Proposed Launch Date in Model
Mercury polar orbiter (dual Venus swingby)*	1981	1983
Surface probes	1984?	later than 1986

For the asteroids, it is doubtful that any mission short of a surface probe or sample return would be meaningful, especially in view of the active ground-based observation of reflectance spectra of these objects. At this time, the SEP propulsion to carry out such missions is not available, and no science rationale is yet evident upon which to base a choice of object(s) for study.

From the Pioneer 10 and 11 flybys, we are seeing the Galilean satellites of Jupiter as moonlike objects. Mariner Jupiter/Saturn 1977 will be available to conduct reconnaissance photography of much of their surfaces, and a Mariner Jupiter orbiter would have the potential capability to obtain more detailed geophysical and geochemical data on these bodies. We do not envisage a dedicated orbiter or probe mission to one of these bodies before about 1990.

RECOMMENDATIONS The most attractive way to learn about the moonlike planets in the solar system at this stage in exploration is by a sequence of missions in which a geochemical/geophysical polar orbiter is placed around each body. The payloads would be close generic relatives of one another and would share a common

*To achieve a near circular orbit at a sufficiently low attitude will require SEP. The capabilities of the crucial gamma- and x-ray experiments may be badly degraded by the eccentric orbit achievable by dual Venus swingby.

instrument development. We *recommend* a sequence such as the following, which is incorporated into the mission model:

Lunar polar orbiter (1978)
Mars polar orbiter (1981)
Mercury polar orbiter (1983)
[Mariner Jupiter orbiter (1985)]

Survivable hard landers, such as penetrometers, provide a potential means of studying the surfaces and interiors of solid planets at relatively low cost. We urge advanced development and instrument development toward this objective. The feasibility and cost of planting penetrators on a planet without atmosphere must be established. Fully defined and developed instruments are needed to make seismic (three-axis) and heat-flow measurements and to perform chemical analyses. The precision and sophistication of the last two types of experiment will have a direct bearing on the scientific value of missions of this kind.

V. THE FUTURE PROGRAM IN A CONSTRAINED BUDGET

We have attempted to highlight the issues involved when the program strategy in each area of planetary and lunar science is fitted into a total program. The planning wedge made available to the committee on August 27, 1974, was taken as the basis for considering this question; we then attempted to develop a planetary/lunar program whose costs would lie under the constraint of half of this wedge.

The model program given here has both short-term and long-term aspects. It can be assessed whether new starts that are desired for fiscal years 1976–1978 can be sought realistically within the current constraint. Over the longer term, we can assess the consequences of the half-wedge funding level on the pace and accomplishments of the program.

Table C.2 shows the mission model adopted by the committee for the purpose of these assessments. The missions for new starts in the next 5 years are reasonably well defined. Discussion of their scientific objectives and capability is found earlier in this report. The tabulated figures are in 1974 dollars; the yearly totals have been inflated at a rate of 5 percent annually for comparison with the half-wedge.

TABLE C.2 Planetary and Lunar Exploration Mission Model (in Millions of Fiscal Year 1974 Dollars), 50 Percent of Planning Wedge

Mission	Launch Dates	Fiscal Year '76	'77	'78	'79	'80	'81	'82	'83	'84	'85	'86	'87	'88	'89	'90	Total
Mariner Encke	1980			5	27	33	15										80
Tempel II Rendezvous	1986								8	31	68	39	14	8	9		117
Mariner Mercury orbiter	1983						6	28	53	20	18	6	3				134
Mariner VOIR	1983						26	86	77	16							205
Lunar polar orbiter	1978	4	13	13	9	2	1										42
Viking extended mission			17	15													32
Mariner Mars polar orbiter	1981				11	32	62	28	23	9	4						169
Pioneer Mars penetrator	1984							7	28	26	9	4	2	2			78
Pioneer Jupiter orbiter with probe	1980		6	29	43	51	26	4	6	5	2	2	2				176
Mariner Jupiter orbiter	1985								26	91	74	34	6	12	25	10	278
Mariner Saturn orbiter	1987									9	77	123	46	6	8	11	280
Mariner Jupiter/Uranus without probe	1979		16	64	41	16	5	7	6	6	9	5					175
SEP OOE	1982				3	31	29	14	6	5							88
TOTAL		4	52	126	134	165	170	174	233	218	261	213	73	28	42	21	
Total with 5% Annual Inflation		4	60	154	172	221	240	258	361	355	446	383	138	55	87	46	
50% OSS Planning Wedge		2	57	124	198	200	245	290	333	333	333	333	333	333	333	333	

The criteria adopted in this model are as follows: (1) attempt to maintain balance by initiating a new exploration step in each important program area in the next decade; (2) retain Mariner Jupiter orbiter and Mars surface sample return as major objectives but remain within the cost limits by delaying these objectives, as well as other missions that are not rigidly bound by a launch date.

A. Short-Term Issues

The key fiscal year 1977 new starts involve a lunar polar orbiter (inner planets) and two outer-planets missions. The Mariner Jupiter Uranus mission must get a new start in fiscal year 1977 because of its constrained launch window. Under fiscal pressure, a Pioneer Jupiter orbiter could be delayed a year. An MJU with Jupiter entry probe, which combines many of the attributes of both missions into one mission, appears at this time to be technically unfeasible.

The extended Viking mission costs would be a function of the success of the initial Viking experiments. It is hard to escape the need for six months of extended operation past the present nominal 90-day mission, a grossly inadequate baseline to adopt.

The only new start for 1978, the Encke flyby, is regarded as unique and basic to the program.

B. Long-Term Issues

The total model realizes many cost savings by the use of generically related spacecraft and spare spacecraft, as well as similar experiment packages. Reduction of the available resources below the half-wedge level would reduce the program continuity in a way that would make each new mission remaining considerably costlier. Further time stretching of the program into a longer future would have a similar effect. A level of resources below about 40 percent of the planning wedge would make it necessary to reassess the entire philosophy and approach to planetary exploration.

VI. RELATED PROGRAMS AND SUPPORT

The planetary exploration program does not consist solely of spacecraft to planets but depends on parallel programs in physics and astronomy, as well as on proper premission and postmission support. In this section we discuss some of the issues and offer recommendations where they seem needed.

A. Astronomy

Continuing astronomical studies of the planets will be required to build up the meager data base we have on the outer planets and satellites and on asteroids and comets. A sequence of studies of Titan, recommended by the 1973 workshop[7] now begun, will be the prerequisite to an eventual entry-probe mission to that unusual object. Continued spectroscopic and photometric studies of the satellites are needed to define the scientific issues for objects whose attributes are now poorly known. The Large Space Telescope is especially important in its ability to achieve 10 times the imaging resolution now possible from the ground. This capability would be of special interest in the study of objects now only poorly resolvable: Uranus and Neptune and their satellites, as well as details of Saturn and Titan.

The capabilities of a Large Space Telescope for studying Uranus should be clearly understood before the Mariner Jupiter Uranus mission is evaluated for a new start in fiscal year 1977.

Ground-based photometric studies are at the present time the most productive means of exploration of the asteroids. The increased spectral range and image resolution of an LST will be of special value in the study of comets, as special opportunities arise.

The LST will be a major new tool for the study of the planets. *We strongly urge that planetary investigations be included in the scheduling of effort on the LST.*

B. Space Physics

Space physics may be regarded an intrinsic part of planetary exploration in that the particle and field envelopes of the planets are outward extensions of their surface and atmospheres and are different for each planet. Nevertheless, space physics is not always adequately represented in planning activities in planetary exploration. We recognize this problem; many aspects of planetary space physics have been treated in the report of the Space Physics Committee, rather than here. We emphasize our commitment to a planetary program that takes proper cognizance of the objectives of planetary space physics. In the next decade, the missions to the outer planets must continue to integrate, as they have done (Pioneers 10-11, Mariner Jupiter Saturn), studies of the interplanetary and cisplanetary environments with specific planetological experiments. High-quality experiments in the physics of the interplanetary medium should be considered for inclusion in flight payloads as an intrinsic part of the range of studies on planetary missions. Experiments in other areas of physics and astrophysics should be considered as candidates for inclusion on planetary payloads. Increasing precision of tracking of planetary spacecraft is leading to a substantial improvement in our knowledge of the classical celestial mechanics of the solar system and promises to lead to tests of general relativity.

C. Supporting Research and Technology; Instrument Development

The success of flight programs is determined in large measure by the existence of tested experimental concepts. The SR&T program of NASA is the mechanism intended to develop and test experiments for possible future spaceflight. Often, a mission program is forced into costly, crash development of instruments because of an earlier scarcity of SR&T funding.

In developing mission models for the next decade, we seek to improve the basis for anticipating instrument needs sufficiently far in advance. NASA should husband its SR&T resources carefully, using them in an overall plan that reflects the anticipated needs for instruments. For each defined mission the status of instrument development and experiment definition should be continually under review.

D. Advanced Development

From the exercise of developing a balanced planetary program over the next decade, one finds a mixture of the old—the tried and tested—with new ways of doing things. The use of spare spacecraft and proven designs from ongoing programs is an example of the former. The latter lies in new spacecraft and experiment concepts, which will require engineering development. We highlight two new types of experiment platforms that need engineering development in the near future: outer-planet probes and penetrators.

1. OUTER-PLANET PROBES

We have urged an early flight of an entry probe to a major planet; the example mission model takes this as a Jupiter probe carried aboard a 1980 Pioneer Jupiter orbiter. By far the most satisfactory course is to base this probe on the Venus '78 entry probe. This suggests that development cannot commence later than 1978 and flight later than about 1982 if this genealogy is to be exploited. An entry probe to an outer planet is a keystone of a planetary exploration program. We urge continued effort to define and develop this important module and parallel SR&T support to provide an optimum payload when the time comes to fly the first mission.

2. PENETRATORS

The feasibility of a penetrator concept for survivable hard landing and implantation of experiments has been established by field testing of a military design on earth. In the Mars study we highlight the need to proceed with definition and field testing of a Martian probe. Some kind of baseline design and concept must be adopted as a result of an independent evaluation of the penetrometer concept and of other hard landers. Development of the key instruments matched to this design is then required (e.g., seismometer, heat flow, chemical analysis, water analysis), and a tested power and communications capability is needed. We urge adoption of a baseline design for 1976, to permit the completion of basic instrument and payload development by 1979, preparatory to a potential launch in 1981 or 1984. We urge study of the means and costs involved in delivering penetrometers to the other solid bodies in the solar system, to permit a sound scientific evaluation of the priority for such missions to bodies other than Mars.

3. TRANSPORTATION

The execution of a sound planetary program is dependent on the provision of adequate launch vehicles and spacecraft propulsion. The present stable of launch vehicles is capable of delivering all the missions in the mission model except four. Three of these are dependent on the development of solar electric propulsion (SEP). The switch to a Shuttle transportation system in the early 1980's leaves additional missions vulnerable to the design parameters of the Shuttle interim upper stage (IUS).

(a) SOLAR ELECTRIC PROPULSION A comet rendezvous, such as the Tempel II (1986) mission included in this mission model, can only be undertaken if SEP is available. An asteroid rendezvous, or preferably a multiple asteroid rendezvous, which seems to be the only approach to these objects, would also require SEP. A Mercury orbiter can be achieved without SEP, using the dual Venus swingby. It is likely, however, that the mission objectives (geochemical sensing and relativity experiments, for example) may require orbits and orbit-change capability not available in the dual Venus swingby; SEP would then be required. SEP also offers an attractive means of achieving an out-of-the-ecliptic mission. We urge development of SEP in time to permit the staging of an out-of-the-ecliptic mission, a Mercury orbiter, and a comet rendezvous in that order, in the period 1980–1986.

(b) INTERIM UPPER STAGE Every planetary mission requires additional velocity Delta V propulsion to escape from the low-elevation circular Shuttle orbit to the appropriate hyperbolic orbit. An IUS is now being defined to permit the placement of earth satellites into their required orbits and to permit earth escape. Several of the designs under consideration would not meet the requirements of many of the planetary missions. In particular, the Mariner Jupiter orbiter, a very high-priority mission, requires the high-capability Centaur IUS. Missions that could not be flown on the Shuttle IUS would then be dependent on the retention of conventional launch vehicles (typically the Titan/Centaur) in the NASA capability. We strongly urge NASA to anticipate the needs of planetary missions for the 1980's by incorporating either a Shuttle IUS or a conventional launch vehicle capable of transportation for these missions. To do less would be to retreat from the present capability of the agency.

Uniquely for the missions discussed by this Committee, a Mariner Saturn orbiter demands a launch capability not now available either through conventional means, SEP, or Shuttle Centaur. This mission could not fly without the Shuttle–full TUG combination or a specially augmented IUS or conventional launch vehicle. This mission lies so far into the future, however, that the issue is not pressing at this time.

E. Postmission Data Analysis

The objective of a given mission is to obtain, reduce, analyze, and interpret data. The practice has normally been to encompass the data gathering and preliminary reduction and analysis into the mission plan in a very short time. This has led to unfortunate situations, such as Mariner 9, in which the termination of the baseline mission leads to the premature cessation of data analysis and interpretation. It is unrealistic to expect experimenters to complete the analysis of their data within a typical 90-day postencounter period. Experience shows that mere acquisition of digital data by the experimenter takes months, and that one to three years are required for the analysis and interpretation of the data. These are carried out by the small investigator groups or by individuals; there is typically no way of shuffling personnel and resources around to speed up the process.

The post-Apollo lunar program represents, by its uniqueness in the NASA space-science program, an excellent example of how the need for postmission data analysis can be met.

Experience shows that data analysis breaks down into separate functions, which follow each other in time: (1) postmission data reduction and preliminary intepretation and (2) data analysis, interpretation, and synthesis. The former is the responsibility of the Principal Investigator on each experiment, and it is the responsibility of the mission to provide support for these basic operations for an adequate period of time. Analysis and interpretation should be adequately supported beyond each given mission and should be open to qualified scientists under proposal competition.

We, therefore, *recommend*

1. That adequate time and support for postmission data reduction and preliminary interpretation by the Principal Investigator be provided by each mission;

2. That NASA take steps toward instituting a planetary data-analysis and synthesis program, along the lines of the lunar program, within its baseline effort. The priorities and needs of this program would vary from year to year as the flight program of the agency progresses and would require periodic assessment by NASA with the assistance of a scientific advisory group.

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Committee on Space Biology and Medicine

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D
Space Biology and Medicine

During the past year, the Skylab missions were completed and the biomedical data analyzed. The experiments have clearly demonstrated that there are some severe physiological problems that man in space will face. There is no *a priori* reason to think that these are insurmountable, however, and we are now in a position to investigate these problems with new insights and opportunities.

Three physiological effects in particular were observed: (1) a severe shift of body fluids, (2) a severe vestibular dysfunction, and (3) a continuing bone decalcification. All three, which are potentially disabling, need further study, not only to ensure success of future missions but also to gain a basic understanding of the mechanisms involved in these physiological phenomena.

I. ALTERATIONS IN DISTRIBUTION OF BODY FLUIDS

There is evidence of a redistribution of the body fluids in the zero-*g* environment, and it has been estimated that about 700 ml of blood and interstitial fluid is transferred from the lower limbs to the thorax and head with the onset of weightlessness. This shift is consistent with the fullness of the head and the congested sinuses experienced by the astronauts during the Skylab missions and with the obvious distension of their neck veins and the rapid decrease in the volume of the legs. Most of the cardiovascular changes noted during and immediately after the space missions, such as exaggerated responses of heart rate and blood pressure to the application of suction to the lower body, can be attributed to these shifts in body fluids, but the mechanisms by which these changes occur have still to be elucidated. Little is known of the complex systems concerned with the control of extracellular and intravascular fluid volumes. These systems operate through (1) a detection element (the location and nature of which are unknown) that perceives departures from the steady state, (2) the nervous and humoral pathways by which the information is conveyed, and (3) the renal tubules which must respond with appropriate changes in sodium chloride and water excretion.

In order to understand the precise mechanisms involved, two approaches are recommended:

1. A series of ground-based studies on animals and man, including water immersion studies in normal volunteers. Such studies should lead to information on the hormonal mechanisms involved in the sodium chloride and water loss.
2. A series of studies on the Space Shuttle to determine whether a diuresis and natriuresis occur during the first few hours of entering a zero-*g* environment and the nature of any associated hormonal changes. Such a demonstration is essential to establish whether current speculations on the body-fluid shifts are valid.

II. VESTIBULAR FUNCTION

The Skylab missions brought back valuable information concerning vestibular function during and after weightlessness and also, for the first time, clearly revealed the serious debilitating nature of space motion sickness. These observations have led to the conclusion that the effect of extended weightlessness on the human orientation system is still not understood and that the results of motion sickness in this environment can seriously hamper the ability to perform useful work during the first few days in space and to function effectively during re-entry and shortly thereafter.

The major conclusions concerning vestibular function resulting from the Skylab missions were as follows: there is (1) a high incidence of motion sickness, especially during the first three days after entering the orbital workshop; (2) the ability to make unlimited cross-coupled angular acceleration head movements with eyes closed in weightlessness; (3) the carryover of this ability to immediate postflight periods; (4) following re-entry, a severe degradation of postural equilibrium, especially with eyes closed—a problem that lasted for considerable time; (5) a disorientation upon head movement following re-entry; and (6) a lack of correlation between motion sickness in the orbital workshop and conventional crew selection criteria.

It is not possible at this time to support any single theory for changes in vestibular function in or following 0 *g*. That the cross-coupled angular acceleration tests were never performed with eyes open in the orbital workshop and that they were not performed during the first few days after entry during the motion sickness period makes any such theories risky at this point. Therefore, further research should be done to identify the nature of vestibular changes associated with weightless experience, lest we run two major risks for future manned space exploration and especially for scientific shuttle missions. These are (1) the probability of the loss of effective working time accounting for 50 to 100 percent of weightlessness time because of moderate to severe motion sickness among the scientist crew and (2) the risk of severe pilot disorientation associated with head movements during the prolonged 1-*g* re-entry phase. Research should be undertaken to develop an improved understanding of the nature of vestibular changes in weightlessness, improved selection techniques that emphasize the ability to adapt rapidly to the weightless environment, improved habituation training for astronauts, and improved re-entry display and control systems so as to minimize head movements and disorientation.

III. CALCIUM LOSS IN SPACEFLIGHT

On the Skylab mission of 84-day duration, there was a continuous loss of calcium from the bones, with no indication of any cessation of the downward trend toward the end of the flight. It is estimated that calcium was lost at the rate of 0.4 to 0.5 percent per month of total body calcium. This loss is not homogeneous but occurs in selective areas of bone. The bones of the lower extremities are especially involved, and in the heel bone the loss may approach 2 to 4 percent per month.

Thus, while no significant lasting impairment of bone function is to be expected in spaceflight currently planned for the Shuttle (7 to 30 days), the metabolic studies conducted during the Skylab missions strongly suggest that unless effective protective countermeasures are developed, potential difficulties are to be expected in flights of longer than 6 to 9 months.

It is *recommended* that further studies be pursued to determine the factors concerned in the calcium loss, because this is a major question to be answered if man is to engage in long-duration spaceflight. This may include an analysis of the role of gravity on the body as a whole as compared with the stresses on individual bones. The mechanisms concerned in decalcification require special study.

IV. OTHER EXPERIMENTS

Other experiments were also carried out during the Skylab missions. It was confirmed, for instance, that the light flashes seen by astronauts were indeed caused by ionizing particles. The frequency of flashes dramatically increased when Skylab traversed the South Atlantic anomaly wherein the flux of particles increased. It is still not certain if these light flashes are the result of Cerenkov radiation in the vitreous humor or direct hits upon the retina. If it turns out to be the latter, then basic studies will have to be carried out to determine how an ionizing particle can cause nervous discharge.

It was also found that the bacterial flora of the astronauts changed during the mission, but the changes observed in microbial ecology and the patterns of cross-colonization between crew members appeared to follow patterns observed in closed environments of other types. This confirms the expectation that ground-based studies are adequate for prediction and, where necessary, control of undesirable changes in the microbial environment.

Some of the experiments carried out on Skylab were less well designed than the foregoing ones, however, in that they did not seem designed to answer specific biological questions. In this category fall those experiments in which the blood of the radioactive-isotope-laden astronauts was examined for chromosome aberrations. The results showed that radioisotopes can cause aberrations and that there is no interaction between the radiation and the space environment to produce these effects. The same conclusions had been reached previously on theoretical grounds as well as from numerous land and space experiments. Since this system can be used as a biological dosimeter to record exposure to radiations, there might still be a need to check the blood of astronauts for aberrations before and after missions, but there no longer seems to be a need to carry out these simple studies under the guise of experimentation.

Skylab also contained a large experiment on proliferation and growth of cells in tissue culture. Numerous study groups had pointed out that there were no theoretical reasons to expect changes in cell biology under conditions of weightlessness. Further, previous experiments from the biosatellite experiments confirmed this empirically. The extensive negative studies on Skylab should now lay this approach to rest. It is perhaps time to paraphrase an earlier report of the Space Science Board* and reiterate the attitude that all biological experiments need to be repeated in the space environment should be resolutely rejected.

V. CONCLUSION

If a biological Shuttle will exist, then there are certain experiments that should be carried out. The physiological experiments dealing with vestibular function, mechanisms of body fluid shifts, and bone decalcification need be carried out with high priority. Further, the advent of a laboratory in which man can attend his biological experiments and also have access to a centrifuge to provide a 1-*g* control environment should make it possible to carry out fundamental studies on the nature of the gravity receptors in plants and animals. This will entail studies in developmental biology as well as whole-organism physiology.

*SSB Environmental Biology Committee, Panel on Radiation Biology, *Position Paper on Theoretical Aspects of Radiobiology as Applied to the Space Program* (National Academy of Sciences–National Research Council, Washington, D.C., Aug. 1963).

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E
Future Exploration of Mars

I. INTRODUCTION

This report, dealing with the future exploration of Mars, is the result of a special Space Science Board summer study, held August 20–26, 1974, at Snowmass, Colorado, at the request of NASA, to consider the strategy that should be adopted following the Viking landings in 1976. The need for special attention in this area arose because previous Space Science Board studies, most recently in 1970,[1] have little bearing on the period beyond 1976 and are out of date because of changed circumstances. Recent reports prepared for NASA by in-house advisory groups have addressed the post-1976 period and were useful inputs to this study by the SSB.

The major issue faced by this study, and one not previously considered by a scientific advisory group, is that a return to Mars in the 1979–1981 period by a Viking mission would be difficult to bring about in a period of limited funding.

The formal recommendations developed by the study are presented in Section V of this report. In these recommendations, return of Martian samples is urged as the benchmark of Martian exploration over the long term. Interim missions to Mars with an orbiter and a survivable hard landing mission are urged for 1979–1981. The conditions are specified that would make a 1981 Viking mission worthwhile as an alternative. Viking 1976 extended operations and data analysis, as well as future instrument development, are discussed.

In addition to the formal recommendations, we have developed mission definitions and experiment payloads for four possible missions. Section III of this report contains the description and evaluation of each of these mission candidates. The recommendations of this study take a substantially different direction from what was until recently thought to be the established strategy for Martian exploration; in Section IV, we highlight the reasons for this major change in recommended strategy.

RATIONALE FOR MARTIAN EXPLORATION Section III.A of the report by the Committee on Planetary and Lunar Exploration gives a framework for the orderly exploration of a planet. That material was developed in the course of this study and found appropriate for inclusion in the discussion of the full planetary program. It serves also as the rationale for the recommendations in this report.

II. REVIEW OF SCIENTIFIC ACCOMPLISHMENTS AND OBJECTIVES

Each of the planets is in some ways unique, but Mars is of peculiar interest to man because of its special similarities to the earth. It appears to be the only planet other than earth that retains an atmosphere of light elements *and* has a temperature regime compatible with the evolution of complex organic matter. It is therefore the most plausible target for the search for life elsewhere in the solar system. Its atmosphere and surface, and geological and surface modification processes, are earthlike in many ways. Intermediate in mass and evolutionary development between the earth and the moon, it is a key piece in understanding the history of the terrestrial planets. And, next to earth, it is the most accessible planet in the solar system.

Our present knowledge of the planet and its atmosphere in most instances derives from the results returned by the 1971 Mariner 9 mission. The comprehensive and detailed coverage of the planet by all the Mariner 9 experiments has made it possible to consider the scientific questions in detail. The 1976 Viking landings are intended to obtain information on the biological aspects of Mars, as well as a chemical analysis of the upper and lower atmosphere and the surface material. Our assessment of the proposed future missions to Mars will be based on the potential value they have in dealing with the major scientific questions.

The major scientific questions relevant to Mars are given in the report of the 1973 Science Advisory Committee on Mars.[2]

A. Geology, Geophysics, Geochemistry

1. BASIC STRUCTURE OF MARS

Our data on Martian (solid phase) chemistry are confined now to density and moment of inertia; these are combined with plausible inferences from surface spectroscopy and our knowledge of the composition and structure of the earth, moon, sun, and meteorites to provide current models of Mars. On this basis, it is believed that Mars has a small iron or iron/sulfur core and a silicate mantle. There is no internal magnetic dipole comparable with the earth's. The surface-sample composition and atmospheric composition to be measured by Viking will give much stronger evidence on the bulk composition. The Viking seismometer might provide some crude information on the internal structure.

A seismic network is the decisive means of determining internal structure, limited, of course, by the rate of seismic activity. Accurate, global determination of surface elemental and isotopic chemistry is the best that can be done, in principle, toward finding bulk composition. The rest must be done by inference. Some of the candidate missions (described in the next section) would contribute in a major way to this question.

2. GLOBAL ORIGIN AND HISTORY

Simple, first-order experiments are of little use to this vital problem, because of the enormous extrapolations required in discussing the past. Experience with the earth and moon shows that a wide variety of precise measurements is required to provide a sound base for these often subtle inferences. Viking should provide an atmospheric argon isotopic composition. A surface sample would be needed to provide the required precision for such determinations as the xenon isotopes, the Rb/Sr systematics, and crystal chemistry. A determination of the present dynamic regime of Mars would be quite important, requiring knowledge of the distribution and magnitude of seismic activity, the average and varying heat flow, and the distribution of elastic properties. Gravity and topographic data on a global scale would further clarify current ideas about isostatic compensation, e.g., that uncompensated younger features, such as the Tharsis ridge, were formed on thick lithosphere, while compensated older features, such as the Hellas basin, were formed on thin lithosphere.

3. HISTORY OF THE SURFACE

The geological history is a vital aspect of the total global history. Mariner 9 data provide the detailed imaging required to pose questions. On the grounds of morphology, and by comparison with the moon and earth, it is believed that surface features covering a wide range of ages are present. Volcanic and erosional features are ubiquitous but have not wiped out the record of early intense cratering. The polar caps seem to be associated with time scales somewhat shorter than the age of Mars and raise numerous questions about their role in shaping the polar regions on a geologically modest time scale. The chemical makeup of the major surface provinces and high photographic resolution of critical geological sites are a needed extension of the Mariner 9 data base. The vital radiometric age information about the different surface provinces can only be obtained by examination of returned surface samples.

4. PROCESSES FOR MODIFYING THE SURFACE

These are essential to an understanding of the history of the surface. Mariner 9 observed the process of wind transportation and deposition in the course of the great perihelion dust storm, as well as changes in the south polar cap and northern polar hood over part of a Martian year. The channels observed by Mariner 9 strongly suggest water modification of the surface in the geologically recent past. A high-resolution imaging capability that can monitor the surface of the planet over a Martian year is most likely to see the widest range of phenomena. Detailed *in situ* study can only be done by a Viking, but our present experience with similar phenomena on earth does not permit profound experimentation. The role of trace quantities of volatiles in promoting the disintegration and weathering of rock can only be studied by laboratory analysis of returned (unsterilized) samples.

B. Atmosphere

The most general question about a planetary atmosphere is, What is the origin and history of the atmosphere? In order to get at this most difficult question, one must first answer two questions for which data are experimentally accessible: What are the properties of the present atmosphere (mass, composition, temperature distribution, wind systems, variability, etc.)? What are the processes now controlling these properties (photochemistry, distribution of heating, surface interaction, etc.)?

Mariner 9 provided a large base of radio occultation, infrared and ultraviolet spectroscopy, and imaging data on the state of the atmosphere. Viking will provide two atmospheric entry profiles and measurement of the composition and meteorology at the surface. We should be in possession of a reasonably satisfactory model of the present atmosphere (with some gaps) after Viking. Understanding of the processes, however, cannot be obtained through a few easily defined experiments, a situation found as well on earth. Aeronomy investigators would be needed to obtain a full set of physics measurements in the thermosphere and exosphere. The Viking entry probes and landers will also contribute to understanding atmospheric processes.

The major area of investigation where new missions can contribute, however, is the *in situ* detection of and role played by condensable volatiles in the atmosphere; e.g., H_2O ice, water of hydration, chemically bound nitrogen, and CO_2 ice in the polar caps. Determination of the total amount of CO_2 stored at the poles is especially important. Viking is not particularly suited to this study. A plausible means of direct study of condensed volatiles is needed (e.g., hard-landing probes), however, no clearly defined experiments are yet available. Information about the volatiles might be inferred by monitoring the polar caps and other areas of interest from orbit. If needed instruments, such as a laser altimeter, could be included, it is likely that fairly reliable knowledge of the thickness and properties of the polar caps would result.

Samples of the soil and coexisting microatmosphere returned to earth would probably be the most decisive way of settling the present role of water and nitrogen. Many aspects of the past history of the atmosphere, as inferred from the soil chemistry, would be most readily dealt with as well, by surface-sample return. The history of the atmosphere also is strongly related to the present and past rates of escape from the top of the atmosphere. If evidence seems to support the notion that the atmosphere has been bistable and existed in states substantially different from the present, the question of escape from such past states may be quite difficult to attack.

C. Biology

The question of life on Mars is perhaps the most interesting of all questions about the planet; it is also the one about which the least is known at this time. The problem can be broken down into the following specific questions: Is there present life on the planet? If so, what is its fundamental chemical organization? Is there evidence for past life on Mars?

The Viking 1976 landers contain three experiments designed to detect metabolic activity under various environments, as well as the imaging system, a gas chromatograph/mass spectrometer, and an inorganic analysis experiment to provide needed data on the local environment. If strong positive results are found, productive characterization of Martian life can be done only by returning samples for study in terrestrial laboratories. The procedures for determining composition, structure, and function of the Martian genetic system and of Martian metabolism is far too complex to conduct on a Viking-type lander. Determining the conditions under which Martian life remains viable may be a necessary objective, precursory to sample return.

If an ambiguous signal is found, it seems unlikely that a second Viking mission would resolve the ambiguity; the best response would be a sample return, with the capability of studying trace quantities of the organic material in the context of the soil substrate and the atmosphere. Evidence for past life could be obtained from fossils, from biological artifacts, from stereochemical evidence, or from isotopic fractionation unaccompanied by active biology.

Many biologists would consider the probability of Martian life to be negligible if Mars were found to be depleted of nitrogen. If Mars contains nitrogen, there is presently no disproof of the existence of life, beyond the empirical testing of samples from favorable sites. Viking 1975 can measure the nitrogen content of the atmosphere to about 50 ppm, and the pyrolysis-gas chromatography-mass spectrometry experiment will give values of nitrate and organic nitrogen in the soil for each landing site.

III. MISSION DESCRIPTIONS AND EVALUATIONS

In preparation for this study, NASA was requested to define certain possible Mars missions, ranging from a simple particles and fields orbiter to surface-sample return. In subcommittees, this study group devised for each mission a minimum complement of scientific experiments that would make the mission attractive and justifiable. In full committee, each mission was evaluated for timeliness and suitability of its potential contribution to Martian exploration. The strategy we recommend follows from these evaluations and is described in Section V.

The five mission concepts we considered were a Pioneer aeronomy orbiter; a Pioneer with survivable, hard-landing multiprobes (penetrator); a Mariner polar orbiter; a Viking lander; and a surface-sample return. In addition, a combined mission, in which hard-landing multiprobes are carried aboard a Mariner orbiter, was considered.

A. Pioneer Aeronomy Orbiter

1. MISSION DESCRIPTION

Spacecraft: Basic Pioneer spacecraft, carrying particles and fields experiments.

Orbit: Twenty-four-hour eccentric orbit of Mars, at inclination 118° and periapse 115 km.

Objectives: Measurement of composition and physical processes in atmosphere at altitudes above 115 km, relative to the problems of escape and the evolution of the atmosphere. Also the study of the structure and origin of the magnetic field, including the mapping of crustal magnetic anomalies.

Launch: Nominal launch by Atlas/Centaur in 1979. The mission is possible at any biennial launch opportunity but is most favorable in 1979 for energy considerations.

Cost: $60 million (in fiscal year 1974 dollars) for a 1979 launch, requiring a fiscal year 1977 new start.

Status: Prephase A studies completed for Ames Research Center by Science Applications Inc.

2. POSSIBLE EXPERIMENTS

Key experiments: neutral and ion mass spectrometers, magnetometer, two-frequency radio occultation.

Other relevant experiments: electron temperature probe, retarding potential analyzer, solar-wind analyzer, plus 52-kg weight margin for other proven experiments.

3. RATIONALE

A Pioneer aeronomy orbiter is capable of addressing two major areas for which the other missions are unsuited. The history of water and other constituents of the Martian atmosphere is closely related to the state and processes of the thermosphere and exosphere, where escape can take place. Aside from the two Viking entry science packages, only an orbiter of the type described here can obtain repeatedly the *in situ* measurements needed. High-resolution mapping of crustal gravity and magnetic anomalies would be an important step in the geophysical characterization of the planet and its history and dynamics. These studies also require the low periapsis altitude of the aeronomy orbiter. The 1000-km circular orbit of an unsterilized Mariner orbiter is incapable of providing more than a marginal information increase on the upper atmosphere or the internal magnetic field. Since the 1964 Mariner 4 flyby of Mars, no further U.S. attempt has been made to determine the nature of its interaction with the solar wind. An aeronomy orbiter would be the optimum mission for the study of the magnetic field.

B. Pioneer Survivable Hard-Landing Multiprobes (Penetrators)

1. MISSION DESCRIPTION

Spacecraft: Basic Pioneer Venus spacecraft, modified to carry six surface probes (penetrators) in launch tubes.

Mission profile: Vehicle placed in eccentric orbit, deploys penetrators to selected surface targets on command. Penetrators are implanted 1-10 m into surface, return data on command to orbiter for relay to earth. Lifetime, 400 days.

Objectives: Basic study of the geophysics and geochemistry of Mars, through seismic and heat-flow stations, and detailed elemental/isotopic analyses at six selected sites. Also the study of volatiles, particularly water, in the soil, as well as the *in situ* analysis of the structure and composition of the material in the polar caps.

Penetrators: Each unit is a hardened steel projectile, approximately 2 m long by 15 cm in diameter, with a pointed nose, a detachable afterbody with aerodynamic fins, and an interior cavity for instrumentation. Upon vertical entry at 150 m/s, the probe comes to rest at depths of 1-10 m (for hardest rock to soft soil), leaving the afterbody at the surface, attached to an umbilical. Maximum entry force is about 500 *g*. A power source combining a radio thermoelectric generator and a battery would provide a daily data capacity of 10^8 bits at a lifetime of 400 days.

Launch: Nominal launch by Atlas/Centaur/TE364 in 1979. Mission is possible at any biennial launch opportunity but is most favorable in 1979.

Cost: $78 million (in fiscal year 1974 dollars), for a 1979 launch, 1977 new start.

Status: Prephase A studies completed for Ames Research Center by Science Applications Inc.,[3] Hughes Aircraft Co.,[4] and Sandia Corp.[5]

2. POSSIBLE EXPERIMENTS

Key experiments: three-component seismometer (on a minimum of four probes); chemical analysis by alpha backscatter/x-ray fluorescence instruments and/or neutron/gamma-ray activation instruments (on most, if not all probes); heat flow (on as many probes as feasible, although two would be minimally sufficient if constrained); entry accelerometer.

Other relevant experiments: simple meteorology (in afterbody); detection of water or other volatiles; elastic, thermal, electrical properties of material; erosion/transportation monitor (in afterbody); magnetometer (in afterbody).

3. RATIONALE

This is a Pioneer-type mission with multiple, separately targeted probes that may be implanted as an array on the surface in a variety of interesting areas with no fundamental constraints on elevation, terrain type, or latitude. The probes are ejected from an orbiter at arbitrary times to impact at preselected points. The orbiter serves only as a relay link. The number of probes depends on the launch vehicle but will probably be from four to six and can be as large as nine.

The array, thereby, provides unique advantages for seismic exploration and surface geochemical analyses and is the only possible way to obtain heat-flow information. Consequently, this mission would represent the first substantial attempt to understand the dynamic processes of the interior of Mars and the surface composition. The seismic network, with no attendant spacecraft noise, can allow operation at higher sensibilities and will permit determination of seismicity and internal structure; from measured velocities and attenuation one can infer structure, composition, and state of the interior. The Mars surface layer will probably not reflect the chemistry of immediate subsurface rocks. With chemical weathering and leaching, elemental ratios from surface samples would be significantly different from the ratios of abundance in the crust. Even planetwide mixing of surface rocks by extensive eolian activity would have led to size (mineralogic) sorting by wind. Every attempt should be made to determine the composition of rocks beneath the active surface.

Subsidiary advantages of the mission are multiple-site meteorology, multiple atmospheric entries, near-surface stratigraphy from de-acceleration profile, subsurface electrical and thermal properties, and surface erosion and transport. Moreover, the diverse site coverage obtainable with a multiprobe mission would be an extremely effective means of providing site-selection data for a future surface-sample return.

The state of definition and development of the penetrators and their experiments is insufficient for a flight program to begin. We estimate that two or more years of continued supporting research and technology and advanced development work will be required before this payload can be in a state of mission readiness.

C. Mariner Polar Orbiter Mission

A Mariner-class orbiter mission, because of the low circular orbit and large instrument capacity, is the most comprehensive mission proposed to perform studies of global geophysics, surface geochemistry, and meteorology, as well as to search for biologically attractive sample-return sites. The addition of penetrators (as described previously) to the experiment package would provide a capability for the first investigations of the interior of Mars.

1. MISSION DESCRIPTION

The spare Viking orbiter spacecraft could be flown to Mars in 1979 or 1981 without the lander and with new instruments. Nominal mission parameters would be

Launch date:	November 1979
Encounter date:	September 1980
Lifetime in orbit:	2 years
Gross spacecraft weight:	~ 2400 kg
Instrument weight:	~ 411 kg
Launch vehicle:	Titan III-E Centaur
Mission cost:	$169 million, $210 million with penetrators (fiscal year 1974 dollars)

After Mars orbit insertion in September 1980, a minimum of two orbit adjustments would be needed to achieve the desired orbit:

Period:	$\leqslant$ 2.5 h
Inclination:	$\simeq$ 95 deg
Altitude:	$\leqslant$ 1013 km
Eccentricity:	$\simeq$ 0

The orbit would be oriented such that the angle between the orbit plane and that of the terminator is roughly 45 deg. An attempt should be made to reduce the orbit height as low as possible to increase the spatial resolution of the gamma-ray experiment; however, planetary quarantine and propulsion constraints will limit altitudes to 700–1000 km, which in turn will constrain resolution on the surface to 500 km. The large scale and variety of morphological provinces and features indicate that 500-km resolution would still be very worthwhile.

Various modifications to the spacecraft are required to permit some high-priority experiments to be carried out during a one-Mars-year orbital mission:

1. Extra attitude control gas (45 kg) in tanks external to the bus, replacing the internal tanks.
2. X-band radio system to permit high-rate data to be played back at all earth–Mars distances.
3. Rotatable boom for gamma-ray spectrometer and radar antenna.
4. Second star sensor on opposite side of bus from Canopus tracker to avoid use of gyros.
5. Simultaneous playback of all seven tracks of tape recorder to minimize head and tape wear (significantly simplifies ground data reconstruction also).
6. Possibly add penetrometer storage and launch capability.

The spacecraft will have the following instrument load capability;

Weight:	400 kg
Power:	200 W
Data rate:	32 kbps (real time)
	120 kbps (recorded data)
Data storage:	800×10^6 bits
Commands:	50

TABLE E.1 Primary Experimental Payload

Experiment	Weight (kg)	Power (W)	Data Rate (kbps)
Gamma-ray spectrometer	17	10	1
Reflectance spectrometer	6	10	1
Radar altimeter	12	17	0.3
Gravity	–	–	–
Occultation	–	–	–
ir radiometer/sounder/H_2O/limb	7	4	1
Penetrator (4 each)[a]	300 total	?	–
Multispectral imaging	2	10	20
High-resolution imaging	50	45	2000 (recorded)
TOTAL	392	96	
AVAILABLE	400	200	

[a]*Penetrator Option*: A Viking orbiter bus has enough weight margin to carry four penetrators, thus combining many of the virtues of that mission with the geochemical orbiter.

2. INSTRUMENTATION

The instruments shown in Table E.1 are selected as the primary experimental payload.

3. RATIONALE

Probably the most striking characteristic of Mars is the global scale of so many aspects of that planet—its figure, gravity, and topography—and the surface terrain and materials. It is this characteristic that makes it an ideal target for geochemical and geophysical mapping techniques, which require relatively low spatial resolution. Further, an orbital mission such as that described here is extremely well suited for intensive study of the vertical, lateral, and temporal nature of the atmospheric structure, dynamics, and processes.

One of the most exciting prospects for this mission is the ability to characterize the chemical and geochemical provinces on Mars with gamma-ray spectroscopy and the extension of these data to higher resolution with multispectral imaging and reflection spectroscopy. Mars is ideal for this approach because it shows tremendous varieties of materials including polar volatiles, complex polar deposits, and ancient cratered crust in the southern hemisphere; a much younger system of smooth plains in the northern hemisphere; complex erosional systems throughout the northern equatorial zone; immense volcanic systems that exhibit a variety of morphologies, sizes, and ages; complex eolian mantles with latitudinal symmetry that surround both poles; and two wide-latitude bands (10° S to 30° S and 50° N to 75° N) apparently stripped free of dust by wind. All of these systems are of immense scale measured in hundreds to thousands of kilometers. The gamma-ray spectroscopy experiment is ideally matched in sensitivity and resolution to study such systems.

This is a particularly ideal meteorological mission. With approximately 10 orbits per day, the infrared sounding and imaging experiments will provide excellent global determination of atmospheric structure, dynamics, and water content. The long life of the spacecraft will permit determination of seasonal variations. Particular questions that should be resolved in large part are the nature of the general circulation (tidal components, instabilities and turbulent components, and topographic influences); seasonal abundance and migration of water vapor; the nature of dust storms; polar flows and their influence on seasonal polar cap behavior.

Some information on the general circulation was obtained during the Mariner 9 mission, particularly on the tidal component, but coverage was not complete enough to determine this or the other components well. The meteorology experiments on the 1975 Viking landers will provide two *in situ* pressure and wind determinations. These measurements are not sufficient to determine global flow but will be important aids in interpretation and analysis of the remote-sounding data from the long-lived orbiter.

Beyond its capabilities in examination of the surface and atmosphere on global scales, the mission represents the most scientifically logical step in the search for life after Viking 1975 regardless of the outcome. If the biological signs are negative, sample return is mandatory for a more detailed search. If the signs are positive, a sample would be of incalculable value. The difficulty in selection of a landing site for Viking underlines the necessity of first searching for those environments that might be favorable to life. The geochemical orbiter described here is clearly the most effective tool for discovering those areas.

In summary, the geochemical and geophysical orbiter satisfies the basic scientific goals of comprehensively studying (1) the atmospheric structure and dynamics; (2) the global chemical nature of rock materials and volatiles, particularly exotic materials in the polar regions; and (3) the advancement in the search for life, which must be predicated on a firm understanding of the chemical environments on the planets.

D. Viking Lander, 1981

There are several possible ways of configuring a Viking return to Mars in 1979–1981. Several levels of redesign of the instrumentation are possible, and options providing mobility on the surface have been proposed. In the report of the Mars Science Advisory Committee,[2] the issue of a scientifically optimum payload of new instruments was considered. Following the release of that report, however, a major change developed in the mode of mobility; i.e., a "tracked" system for moving the entire lander was proposed. In light of the wide variety of options for Mars exploration in 1979–1981, it seemed advisable to develop in this report a redefinition of the most attractive and plausible Viking mission that would provide a basis for comparison with the other candidate missions.

1. MISSION DESCRIPTION

Launch: 1981.
Number of spacecraft: One lander/orbiter mission.
Lander: Modified from previous Viking to provide tracked mobility with effective radius of 20 km or more. New biology instrumentation plus other payload modifications.
Orbiter: Some instrumentation modifications.
Objectives: Conduct geochemical, organic chemical, biological studies along a traverse; provide access to as many terrain types as possible.
Lifetime: Approximately one year on the surface.
Cost: $350 million to $400 million (fiscal year 1974 dollars).
Status: New start required in fiscal year 1977.

2. INSTRUMENTATION (Assuming Positive Unambiguous Result from Viking 1976 Biology Experiments)

Lander: Replace present biology experiments and gas chromatograph/mass spectrometer (GCMS) by updated instrumentation for the biology objectives. Add direct inlet oven to GCMS. Add grinder-siever. Modify sample feed and disposal system to permit analysis of many more samples than now possible on Viking. Substitute alpha-backscatter/x-ray experiment. Add water sensor. Delete or upgrade seismometer (assuming negative or marginal 1976 seismology result).
Orbiter: Infrared thermal mapper and Mars atmospheric-water-detector experiments. Add the Pioneer Venus multichannel

radiometer for atmospheric analysis. Add a geochemical mapping instrument (gamma-ray spectrometer or reflectance spectrometer).

3. RATIONALE

A return Viking with the ability to visit diverse sites up to 20 km or more from the landing point would be many times more productive than a single-point lander. A properly sited landing would permit visits to volcanic and sedimentary terrains and investigation of a braided channel. With a water-vapor detector, the lander would be able to detect environments having excess water in or near the surface. In the course of its travels, the lander could chemically analyze many more samples than six penetrators; with a proper choice of landing site and traverse, these samples might provide a basis for extrapolation to the planet as a whole.

The biological capability of a Viking 1981 lander cannot be adequately judged prior to the receipt of the Viking 1976 results. If a strong positive signal is received in 1976, then Viking 1981 could be used to determine the conditions under which Martian life could be kept alive (a precursor to sample return), to determine the local microenvironments in which life is most favored, and to characterize in a rough way the chemistry and biochemistry of Martian life. In this instance, new biology instrumentation would be required to address these questions, and a strong supporting research and technology effort would be needed, beginning now, to provide a definitive instrumentation concept by 1977. If a weak or ambiguous biological result is obtained in 1976, a return Viking would have only marginal impact on Martian biology. It could be used to determine whether different and more favorable microenvironments could be found. It could be used to further address the problems implied by an abiological planet with conditions at least marginally permissive of the existence of life.

A return Viking could contribute substantially to an understanding of the role of volatiles on Mars, particularly water. An inorganic analyzer using alpha-backscatter/x rays in the lander, as well as an orbital gamma spectrometer, would enable an assessment of nitrogen and carbon deposited in the surface. The water sensor and orbital atmospheric monitor, along with camera and gas chromatograph/mass spectrometer data would enable us to estimate whether subsurface permafrost is an important phenomenon.

As an alternative, it is not practical to incorporate any information obtained in 1976 landings into the design of instruments for a 1979 Viking follow-on mission. At costs involved ($250 million to $400 million), it would be unwise to freeze the 1979 design prior to the 1976 landings. Without mobility, a 1979 lander is not competitive; with mobility, the sample delivery system would have to be completely redesigned.

Launch energy requirements after 1981 make alternative launch dates impractical until 1988. Delay of one Viking 1975 spacecraft to a 1977 launch (to provide the need for a follow-on mission), as another type of alternative to a 1981 mission, is a costly (over $100 million) type of insurance on the 1975 mission; moreover, no additional science objectives could be met by such a delay.

E. Mars Surface-Sample Return

During the past year, the concept of a mission to return samples from Mars to earth has been under discussion[6,7] to help NASA answer certain questions:

1. What is the scientific rationale for sample return? Is sample return strongly endorsed by the scientific community? Is sample return a major objective?
2. How would sample return be staged with respect to other Mars exploration? Should it be taken up as soon as possible or deferred?
3. What are the major areas where technological and scientific development would be required to make such a mission practical?
4. How can the sample be brought back without taking the risk of backcontamination of the earth by Martian organisms? Is sterilization an acceptable solution? What development is needed to meet the demands of an acceptable system of protection?
5. How can such a mission be carried out?

1. MISSION CONCEPT

Several variants on returning a Martian sample are under discussion, but most depend critically on Mars orbit rendezvous.[8] The major steps are

Delivery of a soft-landing spacecraft, with sample collection capability and ascent stage to the surface of Mars, either from orbit or by direct entry.

Delivery of an orbiter to Mars, to serve as the vehicle for Mars-earth transportation.

Collection of sample and transfer to ascent stage.

Liftoff of ascent stage and insertion into Mars orbit. Rendezvous with orbiter. Transfer of sample.

Return of orbiter to earth. Re-entry via direct entry or via earth orbital storage.

The mission would take over three years, including one year of waiting time at Mars to await a favorable launch opportunity for the return. A sample weight of 1 to 5 kg can be achieved with this kind of mission. Of the various modes of earth launch, a vehicle consisting of a single Titan/Centaur/kick stage for delivery of both orbiter and lander to Mars appears barely feasible. A mode using two separate launch vehicles for the orbiter and lander appears more likely, given the need for flexibility this early in the definition of a mission. The cost of a minimum mission, with no scientific instruments aboard and no special provision for quarantine or sample handling, was given at $900 million* by the JPL study.[10] We are adopting a cost figure of $1500 million for discussion in this report, to allow for the inclusion of necessary features above the minimum mission above, in particular for the provision of the needed measures to guard against backcontamination and for the provision of an advanced sample-acquisition capability.

2. DISCUSSION

We emphasize that this mission is, at the present time, merely a concept, and that most aspects of its design, instrumentation, and cost are quite speculative. The issue facing this study is whether sample return is a desirable goal and whether to incorporate this goal appropriately into a strategy for the exploration of Mars. It is helpful to review here the major development issues involved, which must be resolved before such a mission can be undertaken.

(a) BACKCONTAMINATION, STERILIZATION, QUARANTINE From our present knowledge of Mars, it is conceivable that returned samples would contain living Martian microorganisms and that these

*In the recommended mission model (see Part I), the SSB adopted a cost estimate of $1000 million (1974 dollars) for a sample-return mission. Inflation could raise this cost level.

organisms could act as pathogens to terrestrial forms of life. In a less frightening, but equally serious vein, they could become established in terrestrial environments, in a variety of harmful ways. Consequently, containment of the samples in secure, gas-tight isolation from the terrestrial environment is imperative. This precaution operates equally to protect the earth from Martian organisms and to protect the samples from biological and nonbiological contamination by the earth's environment. We foresee that the following procedures, or similar procedures, will be required.

Containment of an untreated Martian sample and a return canister with gas-tight seal and sterilization of exterior of canister;

Return of the canister to the earth's surface with assured security against disruption and dispersal in the event of malfunction;

Examination, testing, and scientific investigation of the sample behind secure barriers within a fully contained quarantine and analysis facility;

Incorporation of verification and fail-safe procedures throughout all these procedures to prevent escape of possible contaminants into the earth environment.

On one scale or another, most of the individual procedures involved in an effective quarantine have been tested and implemented. For example, extremely pathogenic disease bacteria are routinely studied behind barriers. Radioisotope thermal generators (RTG) for spaceflight have been constructed with enough ruggedness to resist disruption in any conceivable earth-entry mishap. A total system for secure sample return, however, is enormously complex, and many of the steps involved have never been attempted automatically in a spacecraft.

(b) SAMPLE ACQUISITION STRATEGY The value of a single sample taken at random within a landing ellipse of several thousand square kilometers has been a matter of substantial discussion. The experience with lunar samples showed that the particular processes of mixing and transportation of the lunar regolith provided in each soil sample a wide variety of materials of diverse origin. The ability of the analytical laboratories to work with exceedingly small samples makes it possible, by application of many different techniques to the full variety of fragments, to draw many inferences about lunar history, processes, and structure. It has been plausibly argued, however, that the surface processes on Mars tend less toward mixing of materials than toward sorting and segregation. Both the Mariner 9 images and our terrestrial experience with wind transport and erosion support this assertion. The scientific value of a randomly acquired sample might then be highly unpredictable.

It may be necessary, then, for the sample acquisition program on the Martian surface to have a moderate degree of sophistication and could thereby require lander mobility (tracked footpads), imaging, or a chemical analysis device (e.g., alpha backscatter, water detection). It is necessary for the scientific community to establish what minimum capability would be required to return meaningful samples. The sophistication and cost of the required lander hardware must then be assessed. The sample-return mission cannot be regarded presently as adequately defined, and no plausible cost estimates will be possible until this question is settled.

IV. MAJOR ISSUES

A. Biological Issues

The two major biological issues that were encountered in this study were (a) How is the biological exploration of Mars continued past the 1975-1976 Viking landings? (b) Can the backcontamination of the earth be prevented during a sample return mission, without sterilization of the sample?

The further strategy for exploration, issue (a), was taken up by the special summer study on Martian exploration, with the assistance of the biologists attending. It was further developed by the SSB Exobiology Panel* in its meeting the week of August 26, 1974. We include herein two statements of the panel deliberations that we wish to incorporate into our findings.

STATEMENT ON MARS SAMPLE RETURN MISSION

The Exobiology Panel strongly endorses the return of an unsterilized sample from the Martian surface. We consider the return of a surface sample from Mars to be the most important goal of the planetary exploration program. We believe that the sample should be contained and protected during transit and on earth in such a way as to maintain it in its pristine and viable state as far as possible. The

*Members of the Exobiology Panel of the Committee on Space Biology and Medicine are Norman H. Horowitz, Chairman; Elso S. Barghoorn, Thomas H. Jukes, Issac R. Kaplan, Lynn Margulis, and Peter Mazur.

receiving station should be designed to maintain the sample in a Martian environment and to protect it against terrestrial contaminants. The latter precaution would be sufficient, *per se*, to isolate the earth from any Martian organisms in the sample.

We cannot support a sample return mission that would return a sterilized sample because sterilization would needlessly destroy a major portion of the unique information this very costly mission was designed to obtain.

The reasons for our position are (a) a returned sample would be the definitive source of information about possible life (contemporaneous or extinct) on Mars, and (b) we believe the arguments against embargo of Martian samples are persuasive. [See References 9 and 10.]

POST-VIKING BIOLOGICAL INVESTIGATIONS OF MARS BASED ON VIKING '75 OUTCOMES

At the present time, Mars is the only real target for exobiological searches in the solar system. All other objects, with the possible exception of Titan, appear to be excluded as possible habitats of life, owing either to the lack of an atmosphere or to temperature regimes that are incompatible with complex organic chemistry. This being the case, the return of unambiguous biological data, either positive or negative, from the two Viking '75 spacecraft can be expected to have a major impact on the planetary program. A positive result will initiate a new scientific discipline, that of Martian biology. A negative result may terminate the search for extraterrestrial life as a motivation for planetary exploration, although interest will remain in the organic chemistry of the solar system.

Assuming that the instruments work as planned, data will be returned that are either clearly positive, clearly negative, or ambiguous. It is relatively easy to plan responses for positive or negative outcomes; but since the consequences of a misjudgment are very large, we anticipate that the data will be interpreted conservatively and that only the strongest evidence will be accepted for a yes or no answer.

Most of the conceivable outcomes of Viking '75 are likely to be viewed as ambiguous or unconvincing. It is difficult to plan responses for this contingency, since much depends on the precise nature of the ambiguity. A few important kinds of ambiguous results can be foreseen, however, and we outline here our preferred responses for these and other major possible outcomes.

First a few comments on the life-seeking instruments of Viking.

There are three metabolic experiments, called the Pyrolytic Release, Labelled Release, and Gas Exchange experiments, respectively. These instruments are designed to detect microbial life in the soil. Since they are based on different models of Martian life and measure different metabolic processes, they are required to be unanimous only if there is no life in the Martian surface. Besides these instruments, a pyrolysis-gas chromatograph-mass spectrometer system will perform atmospheric and soil-organic analyses. A positive biological result would imply the presence of organic matter—almost certainly including nitrogenous compounds—in the soil. The reverse need not be true—i.e., soil organic matter does not necessarily imply life. The cameras on the Viking lander may also be considered as life-seeking instruments, since they can detect large organisms or structures which would also imply the presence of organic compounds in the soil. Failure of the cameras to detect life would not contradict positive findings by other instruments, however.

Personal differences in the interpretation of the Viking data will doubtless arise. We assume, however, that a consensus view will be discernible and will determine the follow-on strategy. For planning purposes, the following are our recommended options for the major classes of outcome.

Positive outcomes. The strongest possible case for Martian life would be given by positive responses at both landing sites by one or more of the metabolic instruments, confirmed by the imaging experiment and by the finding of amino acids and other complex organic molecules in the soil and nitrogen in the atmosphere. Less overwhelming, but still convincing, would be the same case without the imaging data. A strongly positive result from one site only could be convincing if it is not contradicted by evidence from the other site, e.g., with regard to atmospheric composition.

For all positive outcomes, we support a follow-on Viking mission designed to support and optimize an eventual sample return from Mars. Among the objectives of the follow-on Viking would be those of confirming the initial finding and obtaining a preliminary description of the organisms with respect to parameters, such as thermostability, that would be important in planning the sample-return mission. This Viking would be launched in 1981, as a precursor to a Mars Surface Sample Return Mission launched in the period 1984-90. The specific program of Martian biological investigations should be developed by special studies convened for the purpose.

Negative outcomes. We would regard as negative all combinations of Viking results with the following features: negative or doubtful responses from the three metabolic experiments, no organic compounds in the soil beyond those expected from the known chemistry of CO, CO_2, and H_2O, no (or only traces of) atmospheric or soil nitrogen. Such results at both sites would be conclusive for those sites and suggestive for Mars as a whole.

If the Viking outcome is negative, we recommend discontinuation of further life-detection and -characterization packages unless subsequent orbital missions (e.g., 1979 geochemical mapper) should present convincing evidence for biologically favorable regions. Such findings would justify reexamination of the policy of search for life on Mars. We would continue in any event to support investigations designed to throw light on the chemical evolution of Mars, particularly on the history and present status of Martian light elements. We believe that such information is important for understanding the particular history of light elements on the earth that made an origin of life possible. The remote possibility also exists that evidence for past life on Mars will be found. In regard to a sample-return mission, we could not justify such a mission on primarily biological grounds, given a negative Viking outcome and negative geochemical orbiter results, but we would be interested in such a mission for the reasons mentioned above and because of the interest inherent in such an achievement.

Ambiguous or unconvincing outcomes. Into the ambiguous category would fall such results as the following: marginally positive signals from one or more biology instruments and from the organic analysis experiment; negative results from the biology instruments combined with positive indications of complex organic matter in the surface; or the reverse—a positive signal from the biology package together with failure to detect *N*-containing organic compounds in the soil.

Ambiguous planetary data are not uncommon, and it will be surprising if the Viking data are free of ambiguity. A recent example, still unresolved, is that of the temperature of the atmosphere of Jupiter, where the result given by the Pioneer 10 S-band occultation is in conflict with that derived from ir measurements. Another is the interpretation of the sinuous surface features observed on Mars by the Mariner 9 cameras: the features appear to be river beds, but physical evidence shows that liquid water cannot exist on Mars. Contradictions such as these lead to reexamination of the models on which the data are being interpreted, and their resolution can yield new fundamental insights.

Of the ambiguities listed above for Viking, the first—marginally positive signals—if not due to instrumental errors would imply insufficient sensitivity of the Viking instruments. The following solutions suggest themselves: (a) return to Mars at a subsequent opportunity with more sensitive instruments, (b) return to a more favorable (e.g., wetter) site, if any are identified by the Viking or subsequent orbiters, with the same or similar instruments, and (c) bring a sample of Mars to earth for closer study. Of these alternatives, the last seems to us to offer the most promise of a definitive solution. We do not think that this marginally positive case by itself provides justification for a sample-return mission, however.

Of the two kinds of contradiction between the biology instruments and the GCMS, the more easily understood is that involving negative signals from the biology instruments. On the one hand, organisms might be present on Mars, but not recorded by the biology instruments because some necessary condition for their metabolism—a sufficiently low temperature, for example—was not fulfilled. On the other hand, life could be absent and organic compounds of nonbiological origin present. Carbonaceous chondrites are a plausible source of nonbiological organic matter, including amino acids, for Mars. Resolution of the contradiction is thus reduced to determining whether or not the observed organic matter is of biological origin.

There are several approaches to this problem. The first is a comparison of the compounds found on Mars with those found in meteorites. The Murchison meteorite contains a total of about 20 ppm of 18 different amino acids. A much higher concentration on Mars would suggest a nonmeteoritic source, especially if they constituted a different assemblage. Another approach could be based on the well-known fractionation of carbon and sulfur iostopes that occurs in metabolism. Most conclusive of all would be a determination of the optical activity of the amino acids. We would consider the discovery by Viking '75 of high concentrations (relative to chondritic sources) of unusual amino acids (relative to the same sources) to be of potential biological importance and would recommend that the source of these compounds be investigated by a follow-on Viking in 1981. We recommend life-detection and -characterization instruments developed for Viking '81 be kept in the "definition stage" (i.e., flexible) and not proceed into the development of flight hardware until after results of Viking '75 have been interpreted.

The third kind of ambiguity—a positive result from one or more biology instruments combined with failure of the GCMS to detect *N*-containing organic compounds in the soil—would seem at the present time to have no rational explanation other than instrumental error. Nitrogen is so important in the construction of terrestrial living matter that any suggestion that this might not be the case on Mars would be received with extreme skepticism. If a result of this kind is transmitted by Viking, a fundamental reassessment of our models may be in order.

Summary: Clearly positive outcomes of the Viking life-seeking instruments would justify a follow-on Viking and a later sample-return mission. The discovery of complex organic compounds in kinds and amounts not explainable by a chondritic origin would justify a follow-on Viking. Other kinds of results considered here would give further biological investigation lower priority.

The question of backcontamination is much more complex and depends on many matters that involve the 1976 findings and future development of techniques. To resolve this matter fully will require several years of intensive study. However, we concur with the position of the NASA workshop on surface-sample return,[5] which recommends study and development in an orderly way, preparatory to an eventual sample return.

B. Sample Return

The substantial lead time required and the substantial cost of a sample-return mission dictate that it be considered as a candidate long-term objective of Mars exploration not as a candidate for flight during the 1979–1981 opportunities. The study concluded that over any longer range view sample return provides by far the most satisfactory and comprehensive means for unraveling the key questions of Martian origin and evolution. Moreover, earth's place in the evolution of the solar system and the question of its uniqueness as a harborer of life can only be answered with satisfying certainty and detail if we first provide sufficient hard facts about the nature of related planetary bodies such as Mars. From Martian surface materials we can expect to discover the times of occurrence of the major steps in the evolution of Mars and the paths of its chemical and petrological development; we have the best chance for determining the extent to which Mars evolved toward the creation of living organisms.

Following the Viking 1976 landings, microanalytical techniques of great complexity are needed to study meaningfully the biochemistry and organic chemistry of Martian materials. As is pointed out in the biological issues, if strong positive indications of life are obtained by Viking, only the return of Martian organisms, preferably alive, can enable us to characterize them in a basic way. If the Viking results are weak or ambiguous, then only earth laboratory methods can continue the biological study of Martian soil at the very low concentrations of biological or protobiological substances involved. The wealth of information available from such samples is evident from the extensive results obtained on 3 g of material from the Luna 20 mission. Interpretation of those results, however, was immensely helped by the studies of Apollo samples; consequently, the capability for interpretation of results from small samples is now quite high.

Comparatively, the detailed knowledge of the material that is so essential to working out Mars history cannot be obtained by any remote experiments, which lack the flexible, multidisciplinary capability for microanalysis demonstrated by the earth laboratories. Orbital study of Mars obtains qualitatively different types of information. The proposed polar orbiter is justifiable on the basis of its global coverage of basically new types of information. On this basis, it is a one-time mission, unless the time variations observed at high imaging resolution disclose phenomena of unusual and broad interest. The polar-orbiter information would be important complementary and supporting data for the study of any subsequent sample returns or, for that matter, any type of surface visit. Geophysical measurements taken by a hard-lander (penetrator) mission deal with questions of the state and composition of the Martian interior. Such measurements are of great intrinsic interest independent of other missions; however, they are also complementary and supporting to the sample-return results.

A mobile Viking would provide some on-site experience with sample selection and manipulation. Such operational experience, however, is more readily obtained in earth simulation. The additional on-site geological and operational information required by the sample-return mission can only be obtained by the mission itself, since a precursor landing to exactly the sample point would be nearly impossible and would certainly be redundant. A strong positive indication of life in 1976 may raise the question of how Martian organisms could be preserved during sample return. This precursory information could be obtained either by an appropriately instrumented Viking in 1981 or in connection with a first-generation sample-return mission.

The importance of acquiring Martian material for laboratory study is emphasized in the recent reports by Johnson Space Center,[11] the Mutch committee,[2] and the report of the sample-return workshop.[6] The undesirability of sterilizing the returned sample was highlighted by the last group. Much of the most important information about the biology, atmosphere, and surface of Mars would be destroyed by heat sterilization protocols. To return an unsterilized sample gives rise to the imperative need to fully isolate the Martian samples and any possible Martian organisms from the earth's environment; it is quite unreasonable at this time to assume that such isolation is impossible and therefore that the Martian organisms must be killed. If present, these would constitute the most exciting and important part of the returned sample. As long as the mission is nominal, careful quarantine is the proper mode for containing and protecting any Martian organisms. In addition, there should be a fail-safe sterilization mechanism, to be operated by an Earth Safety Officer, in the event that a significant risk develops that the earth might be contaminated with Martian organisms.

C. Strategy for 1979–1981

In selecting a strategy for 1979–1981, we are guided by the following criteria: maximum responsiveness to major scientific questions, diversity and comprehensiveness of experiments, appropriateness in a long-term sample-return strategy. Each of the candidate missions described earlier is the most attractive option in its general class, and each has been asserted to be scientifically worthwhile.

The Pioneer aeronomy orbiter is a mission of limited objectives. It is responsive to the problems of the history of the atmosphere and the structure of the crust, but in a moderately indirect fashion. It would be a worthwhile mission whenever flown, since none of the other missions could conduct the experiments flown by the aeronomy orbiter. Its low cost makes it an interesting off-the-shelf concept, which might be appropriate for flight if the preferred missions could not be flown.

The mobile Viking represents the only means, short of sample return itself, for *in situ* biological and biochemical study of the surface. It is possible, given positive biological results from Viking 1975, that such remote *in situ* studies of the Martian biology would be regarded as a prudent step preparatory to sample return. Whether such an effort would be worthwhile, given an ultimate return of samples, cannot be foreseen. The mobile Viking has the capability to do interesting geosciences; it is, however, inferior to either the Mariner polar orbiter or the Pioneer penetrator mission for comprehensiveness and diversity of objectives.

In the baseline strategy, the most productive way to proceed before sample return would be to conduct broad-based orbital studies and to emplace a network of surface probes. The orbital and surface experiments are mutually complementary; they address all the major questions except biology in a direct way; the experiments would be new in Martian exploration; they are helpful precursors to sample return. They are, except as regards *in situ* biology, more productive than a Viking follow-on mission.

This strategy for exploration could be carried out in two ways. The two missions could be flown separately, either at the same opportunity or at different times. It appears conceivable that the orbital experiments and the penetrators could be carried aboard a single Viking orbiter bus, which has the weight capacity for the penetrators. Combining the missions produces an attractive, cost-effective, single mission. It is obtained, however, by the reduction of the number of penetrators and by an increase in the complexity and cost of the single mission.

	Cost (in FY 1974 dollars)*	Penetrators	Feasible Launch Dates
Polar orbiter	$169 million	–	1979, 1981
Pioneer penetrator	$ 78 million	6	1979, 1981, 1984 ...
Combined orbiter/penetrator	$220 million	4	1979, 1981

D. Summary of Strategy†

The natural *a priori* sequence for exploring Mars would be

(a) Mariner 9 orbiter (1971)
(b) Mariner geochemical polar orbiter
(c) penetrator (survivable surface probe) mission
(d) soft lander (Viking)
(e) sample return

The high priority accorded an immediate search for life in 1970 caused Viking to be moved up in this sequence. At that time, the penetrometer techniques and orbital geochemistry experiments were at too early a state of development to be flown; indeed, the same was true of the Viking experiments.

*These are rough order-of-magnitude estimates.

†Mission model: The foregoing discussion is the result of the special summer study on future exploration of Mars and summarizes the recommended strategy in a purely Martian context. The Committee on Planetary and Lunar Exploration in developing a strategy for the solar system that assumes some degree of funding constraint developed a mission model that is responsive to the recommendations of the summer study on Mars, while satisfying the criterion of a balanced program under the funding constraint.

		Earliest Launch Date	Launch Date in Mission Model
(a)	Mars polar orbiter	1979	1981[1]
(b)	Penetrometer mission	1981[2]	1984[3]
(c)	Sample return	1984[2]	1989

[1] Delay to 1981 fits the funding constraint of this exercise. It also puts this mission in sequence, following the 1978 lunar polar orbiter.
[2] Based on satisfactory completion of development requirements.
[3] Pioneer missions can be flown during the unfavorable launch windows in the mid-1980's.

At present, the development status of the Mariner polar orbiter is most satisfactory, and a preferred exploration sequence, following Viking 1975, would be

(a) Mars polar orbiter
(b) penetrator mission
(c) sample return

In the event that the Viking biology results are so compelling as to scientifically justify a second Viking mission in 1981, the alternative exploration sequence would be

(a) Viking 1981
(b) Mars polar orbiter; penetrometer mission (in either order or combined)
(c) sample return

V. CONCLUSIONS

We present a recommended strategy for Mars exploration in the 1977–1990 period that focuses on (a) the favorable 1979–1981 launch opportunities, (b) a major decision point in 1976 based on the 1976 Viking results, and subsequently (c) the return of Martian samples to earth for laboratory analysis toward the end of the 1980's.

Mars exploration at this time, through the Viking 1976 landings, is pursuing the detection of life as the major immediate goal. We are faced in this report with the difficult task of setting out a scientific strategy for the exploration of Mars after these landings. This strategy must permit the continued biological study of Mars but in the context of the study of the atmosphere, surface, and interior. While we are able to set out in orderly form the way this broader study should proceed, we emphasize that approach to future biological investigation must depend on the new and unforeseeable 1976 Viking results.

While the goals and approaches to Martian exploration remain in consonance with earlier SSB recommendations,[1,12,13] the particular strategy recommended here is somewhat different from that envisaged previously for the period after the first soft (Viking) landing. We have discussed in part the variety of new circumstances that led us to this outcome.

A. Sample Return and Long-Term Strategy for Exploring Mars

Return of samples of Martian material to earth for laboratory analysis would be the most fruitful approach *by far* of those available to us. We *recommend* that sample return be adopted as the keystone of Martian exploration, and that it be incorporated into the NASA baseline plan for solar-system exploration over the next 15 years.

The possibility of sample return profoundly affects our long-term strategy for exploration. It reduces the importance of continued *in situ* study of surface materials and atmosphere by the less sophisticated soft lander experiments while highlighting the desirability of complementary and precursor investigations aimed at the characterization of the structure and environment of the planet. We have recommended missions for 1979–1981 that use orbital and survivable surface-probe experiments as orderly, logical exploration steps in their own right and that provide complementary and precursory data to later sample return. The earliest launch we can foresee for sample return would be 1984, to allow sufficient lead time for needed developments in quarantine, sampling, and sample handling. The period between 1984 and 1990 is an appropriate time for Mars sample return to receive emphasis in solar-system exploration following a period of some emphasis on the outer planets in 1973–1984.

The issue of backcontamination of the earth by possible Martian organisms has been under discussion for a year, both in this study and in special workshops.[6,7] As a result of these discussions, we *recommend* that verifiable gas-tight isolation of the sample, from the time of acquisition, be adopted as the baseline approach to this problem, providing verifiable isolation of the earth from the sample and of the sample from the earth's atmosphere and biosphere. We urge an immediate, continuing effort to develop this approach into a fully tested quarantine procedure and to define the total sample acquisition strategy, i.e., rationale for site selection, collection, packaging, transportation, quarantine, and analysis system, including adequate verification and safeguards. This recommendation does not constitute a finding by this group that such an approach can surely be made to work; it is an assessment that the approach is a plausible one, and that significant research and development is needed to establish that it can be made to work.

We recognize that the large cost of a sample-return mission implies severe problems of balance in the planetary program; indeed, the mission would be impossible to fund under any scenario at the present level of OSS funding and the present rate of inflation. We do, however, consider this as an achievable long-term goal, insofar as the cost estimates are sufficiently reliable. A program of this magnitude is still within a range that may plausibly be incorporated into a NASA long-term plan as a major objective. The strength of the planetary-exploration program remains in its flexibility, balance, and diversity; while sample return would imply emphasis on Mars over a period of 4–5 years, it must be fitted into a total solar-system program that retains its balance over a longer period.

B. Strategy for 1979–1981*

The mission(s) recommended following the Viking 1976 landing will depend on the results of the Viking experiments. Two attractive options exist, which are discussed more fully in the two recommendations following:

1. A baseline program, consisting of orbital and survivable hard lander experiments, aimed at the characterization of a broad range of characteristics of the Martian environment.
2. A Viking soft lander in 1981, with mobility and a new generation of experiments, aimed at specific investigations of a Martian biota.

Option (2) is inferior to (1) in respect to all objectives except *in situ* biological investigations and is recommended only if strong positive results are obtained from the 1976 biological investigations, and if these results are of a particular kind justifying a second soft landing before sample return.

*See mission model, Table C.2 of Working Paper C by the Committee on Planetary and Lunar Exploration for disposition of recommended Mars missions in the context of the entire planetary program.

We *recommend** that NASA keep these two options open for two years; that is, until the Viking 1976 results have been received and interpreted. This means that resources for supporting research and technology and advanced development during this two-year period must be invested in both orbiter/penetrator missions and the Viking 1981 mission. A decision must then be taken in 1976 to move ahead with one option only.

C. Orbital and Surface Investigations, 1979–1981

We *recommend* a baseline mission(s) for the 1979–1981 period that employs orbital remote sensing and multiple, survivable surface probes for global exploration of the surface and interior. The objective is the determination of a broad range of major aspects of the chemistry, interior, dynamics, history, surface environment, and atmosphere of Mars. The most important investigations from an orbiter are (a) surface chemistry and radioactivity maps (500-km resolution) using a gamma-ray spectrometer; (b) surface geology from an infrared spectrometer and a multispectral color imaging system (20 km, 100 m resolution, respectively); and (c) study of the presumed water-produced surface features and the polar caps by very-high-resolution imaging.

Emplacement of simple experiment packages on survivable probes designed to come to rest within a few meters of the surface should provide (a) detailed elemental chemistry by an alpha/x-ray experiment or a neutron/gamma-ray experiment; (b) distribution and frequency of earthquakes on Mars and the determination of the structure and properties of its interior by a net of seismic instruments; (c) heat flow (energy budget) from the interior; and (d) occurrence of subsurface water.

No sophisticated surface biology experiments can be conducted by this approach. It does, however, provide a wide range of useful contextual information in support of returning samples to earth for biological investigation at a later stage. The particular complement of orbital imaging and spectral experiments may afford the only way of advancing our biological

*It should be noted that this is a recommendation to the Space Science Board. For the SSB findings and recommendations see Part I, Section VIII, Continuing Exploration of Mars.

understanding of Mars if Viking returns certain types of negative or ambiguous results.

The mission options for this baseline strategy are

1. A Mariner-class polar orbiter with 1000-km circular orbit and a lifetime of one Martian year, using the spare Viking orbiter, launched in 1979.

2. A Pioneer-class eccentric orbiter carrying six survivable penetrators, launched in 1979.

3. A Mariner orbiter, as 1, fitted to carry four penetrators, launched in 1981.

Of these options, we believe the hybrid mission 3 is the most economical way to achieve the objectives. However, separate missions 1 and 2 could be flown at the same or different launch opportunities. We urge continued effort to define an optimum mission.

D. Viking Lander, 1981

We *recommend** that a Viking lander be retained as an alternative mission for 1979–1981, pending the results of the Viking 1976 mission. The nature of these results is difficult to anticipate. It is conceivable that a strong need might exist to conduct a second Viking landing in 1979–1981, the importance of which could be overriding in the planetary program. We identify two possibilities:

1. A 1977 or 1979 Viking with minimal change from 1975–1976 might be kept as an option in case of certain kinds of major failure of the 1975 mission.

With this possible exception, a 1979 Viking mission is not recommended. It cannot respond adequately to the findings of 1976 in the crucial biology area and would have difficulty responding in other areas.

2. A 1981 lander/orbiter with mobility and a new experiment payload that takes best advantage of the mobility and

*It should be noted that this is a recommendation to the Space Science Board. For the SSB findings and recommendations see Part I, Section VIII, Continuing Exploration of Mars.

that is designed in light of Viking 1976 results is the most attractive possibility for a repeat Viking. This mission can do worthwhile planetological studies but would be less productive in that regard than the recommended orbiter/probe approach. Viking '81 is the recommended choice of mission only if the biological results from the 1976 landing show the clear need for and importance of another surface-automated biology study. It follows that sacrificing either the mobility or the upgraded science complement would make the mission less attractive, regardless of the 1976 results.

The yield of information from returned samples is so great in comparison, that we strongly favor sample return at some point over continued automated soft landers.

E. Extended Viking 1976 Mission*

The 90-day post-landing period provided for mission operations in the 1975–1976 Viking mission is inadequate for conduct of all but the minimal soil sampling and analysis activities. We, therefore, *recommend* that NASA make provision for a Viking extended mission, contingent, of course, on the existence of operating, landed spacecraft. We emphasize the scientific importance of the added data from the lander imaging, the atmospheric analysis, the great increase to the data base of the seismometer and meteorology experiments, the erosion data for the radio science, the completion of a fourth sample for the biology on Mission B, the completion of analysis of samples on the inorganic chemistry, and the extensive coverage and seasonal changes that can be observed by the three orbiter instruments. We believe this to be a highly cost-effective task on the basis of the scientific return.

We further *recommend* that the analysis of the Viking data in the post-mission period be supported. There should be a specific plan for the support of scientific teams engaged in the analysis and interpretation of the Viking results.

*See Part I, Section VIII, and the recommended mission model for funding allocated by SSB to Viking Extension. For comparison, see Part II, Table C.2, the report of the Committee on Planetary and Lunar Exploration.

F. Instrument Definition and Development

In considering both the general strategy of Mars exploration and of particular Mars missions, it is recognized that a variety of critical experiments must be developed. This will require the commitment of supporting research and technology funds to define certain experiments adequately and a specific commitment of phase B funding to assess and develop experiments for particular missions.

The polar orbiter mission will require adequate development of the gamma-ray spectrometer, the reflectance spectrometer, and the multispectral imager for the purpose of determining the composition of the planetary surface. These instruments should be developed as part of a complement of experiments available for the broad class of remote-sensing studies of the solid surfaces in the solar system; the generic relationship of a Mars polar orbiter to the proposed lunar polar orbiter is an example, and an orderly program of instrument development should proceed with this broad perspective.

Survivable hard landers, such as penetrators, which are used for studying the interior of the planet, will require some development of seismometers and, most urgently, the more complete evaluation and development of heat-flow measurement and chemical analyses by different techniques. The instrumental techniques indicated here will find broad applicability in planetary exploration, and their development is urgently needed. Efforts must be directed toward analyses of critical elements such as bound nitrogen, which may be of biologic importance.

From considerations of possible Viking-class missions with an emphasis on biologic problems, it is evident that any further experiment packages of such complex nature will demand careful study and development. The amino acid resolution apparatus and the "unified" biology experiment are currently under consideration. Both of these experiments will require further development both conceptually and in terms of instrument definition. Individual experiments must be selected, fully defined, and tested along with overall unit evaluation of the unified biology package.

The identification of a Mars sample return as a benchmark in planetary exploration will require that procedures be developed for the remote selecting and packaging of Martian samples. These investigations should be directed so that they may possibly be verified by or be incorporated into any precursor missions.

The rational development of a quarantine procedure and possible quarantine facility must be initiated. This work must be carried out in conjunction with the development of both organic and inorganic analytical procedures to investigate Mars samples.

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