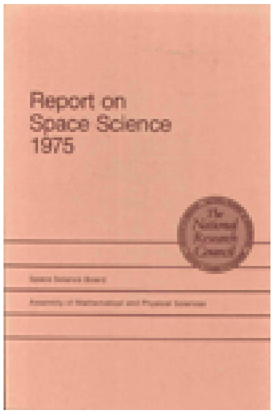

Report on Space Science

Space Science Board, Assembly of Mathematical and Physical Sciences, National Research Council

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Report on Space Science 1975

Space Science Board
Assembly of Mathematical and Physical Sciences
National Research Council

NATIONAL ACADEMY OF SCIENCES
Washington, D.C. 1976

NOTICE: The project that is the subject of this report was approved by the Governing Board of the National Research Council, whose members are drawn from the Councils of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine. The members of the Committee responsible for the report were chosen for their special competences and with regard for appropriate balance.

This report has been reviewed by a group other than the authors according to procedures approved by a Report Review Committee consisting of members of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine.

Available from

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Space Science Board

Preface

The purpose of the Space Science Board is to counsel the federal government in the interest of obtaining the best U.S. space science program achievable within complex political, institutional, and fiscal constraints. It must work with limited resources, including the time available from its members and supporting committees. Therefore, it must select with care its tasks and its procedures.

For the past two years, we have prepared an annual report based upon material submitted to us by our disciplinary committees. The terms of reference for the yearly study are determined at a series of meetings during the winter, followed by Committee deliberations and "mini-studies" during the summer, and culminating in a three-day Board meeting in October. With such a tight schedule, the Board must concentrate on essential findings and recommendations.

Why should the SSB adopt this procedure rather than large summer studies, which characterized its activities in the period 1965–1970? Such studies concentrate the thinking of some hundred or more science advisers into a session of two to four weeks' duration. The Board itself could not devote the time equivalent to a conventional study but can compensate by maintaining continuity of expertise and building its case in steps from year to year. In the past, when the impetus of a large summer study has been spent, the Board has found itself without expert appreciation of developments in the federal government, which it may therefore be unable to influence. On page 5 of our 1974 report we wrote:

> 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.

To whom is our report principally addressed? The choice of matter and the language of our findings and recommendations are principally directed to NASA management and, through them, to the Executive. Congress is addressed by the Board through letters and individual testimony based upon our published findings and recommendations. Although our reports should be useful to the general public, that audience is usually served by science writers in the media and by gifted individual scientists.

Finally, let me comment upon the evolution of the SSB approach to its advisory function, which is evident from its more recent reports. We have come to focus our attention on scientific strategies on the basis of which shorter-term decisions can be made. The reasons for this are twofold. On the one hand, we find that there exists no other body devoted to providing this long-term guidance. On the other hand, NASA has evolved a capable science management and a hierarchy of advisory committees to which it looks for short-term decisions. Such decisions cannot be based solely on scientific criteria but involve also political, institutional, and managerial considerations which the SSB cannot always evaluate adequately.

In the past, strategy has often been stated in terms of specific missions and the priorities among them. This is a restrictive framework when we come to a consideration of scientific objectives. The logical step from missions models to longer-term strategy was taken by the Committee on Planetary and Lunar Exploration when it presented to the Board a strategy for outer-planets exploration in strictly scientific terms. This approach was welcomed by the Space Science Board and will influence our activities of the near future.

For the contents of this year's reports we are indebted first to the disciplinary committees of the SSB and particularly to their Chairmen: Gerald Wasserburg, Peter Meyer, Robert Helliwell, Sheldon Wolff, and Peter Mazur. We are equally indebted to the chairmen of two small summer studies: Gerry Neugebauer and Eugene Parker.

At its final meeting in October 1975, the Board invited a number of colleagues from other advisory groups to join us: George Field (NASA

Physical Sciences Committee); Reimar Luest and H. C. van der Hulst (Space Science Committee of the European Science Foundation); and Richard Garwin (NRC Assembly of Mathematical and Physical Sciences). James Arnold provided continuity with previous Boards.

We received the usual unstinting support from NASA program officers and management; also John Naugle, Noel Hinners, and Ichtiaque Rasool were present at various times during our deliberations.

The Space Science Board is fortunate in the dedication and effectiveness of its professional staff: Milton Rosen, Dean Kastel, Richard Hart, and Kenneth Janes. LallyAnne Anderson, Mildred McGuire, Cynthia Stetzer, and Carolyn Andrews provided us with the secretarial support essential to a successful outcome.

RICHARD GOODY, *Chairman*
Space Science Board

Contents

I
Report of the Board

1
Purpose of the Report

In its 1974 report, *Opportunities and Choices in Space Science, 1974** (National Academy of Sciences, Washington, D.C., 1975), the Space Science Board started to lay the basis on which it could provide counsel to NASA involving interdisciplinary priorities, long-term strategy for space science, or other matters that require a level of consensus in the scientific community. The task was not completed in 1974, and this year's report must be regarded as a continuation of that effort. The 1975 and 1974 reports should be taken together as a statement of Board policy.

For those unfamiliar with the Board's 1974 study, a brief summary is given here. The study was carried out in the following stages:

1. The planning wedge† in current use by the Office of Space Sciences 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

*Abbreviated here as OCSS 1974.

†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.

Astronomy and on Planetary and Lunar Exploration; 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.

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.

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 Board's examination culminated in the adoption of findings and recommendations on the following nine subjects:

A. APPROVED PROGRAMS Three approved programs are of such standing that their implementation must be given highest priority. They are High-Energy Astronomy Observatories A, B, and C; Pioneer Venus; and Mariner Jupiter Saturn.

B. RECOMMENDED MISSION MODEL The Board examined in detail seven new starts proposed for fiscal years 1976 and 1977 and found that each is well conceived and of first-class scientific importance. These proposed new starts are Lunar Polar Orbiter, Pioneer Jupiter Orbiter, Mariner Jupiter Uranus, Large Space Telescope, Solar Maximum Mission, Infrared Survey Satellite, Gamma-Ray Explorer, and Electrodynamic Satellites. Details of these missions and their scientific rationale are given in the 1974 report. The Board 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 were endorsed by the Board with some reservations expressed in the report.

C. THE EXPLORER PROGRAM The Board restated the importance of the Explorer Program, which supports new astronomical initiatives and much of the terrestrial space-physics program. The Board recommended an increase in the Explorer Program, even within a level science budget, to offset inflation and to build on the progress already achieved.

D. OUT-OF-THE-ECLIPTIC MISSION An out-of-the-ecliptic mission was supported by three disciplines: space physics, space astronomy, and planetary and lunar exploration. The scientific rationale for this mission was endorsed, and continuing study was recommended.

E. SOLAR ASTRONOMY A group of scientists should (1) review progress being made in solving the major problems of the sun, both in relation to fundamentals of astronomy and to terrestrial interactions and (2) recommend priorities for instrumentation and mission objectives.

F. THE LARGE SPACE TELESCOPE The Board strongly endorsed the Large Space Telescope as a new start in fiscal year 1976. While recognizing the desirability of a 3-m telescope, fiscal considerations appeared to limit the instrument diameter to the 2-m class. Such an instrument would represent an order-of-magnitude increase in capability over the best existing ground-based telescopes and could accomplish most of the science anticipated from the 3-m instrument, with only an increase in observing time.

G. INFRARED AND MILLIMETER ASTRONOMY A study of goals and priorities in space infrared and millimeter astronomy should be undertaken in 1975. Moreover, the Board's recommendation for an infrared survey satellite (see Section B, above) was conditional upon endorsement by such a study.

H. CONTINUING EXPLORATION OF MARS The Viking mission presently involves only 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. The Board endorsed the Viking Extension as a means of exploiting success in the mission. In addition, the Board stated that the long-term objectives of exobiology and surface chemistry on Mars are best served by the return of an unsterilized surface sample to earth.

I. EXPLORATION OF THE OUTER SOLAR SYSTEM 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; therefore the strategy for outer-planets exploration should be reassessed.

Since the 1974 report was issued, the disciplinary committees and panels of the Space Science Board have completed a year of studies and submitted reports for consideration at the 53rd meeting of the Board on October 9, 10, and 11, 1975.

The terms of reference for the current cycle of studies were evolved in a series of meetings of the Space Science Board and an *ad hoc*

Steering Committee consisting of the Chairmen of disciplinary committees. These called for the development of scientific strategies for the next 10 years, together with consideration of missions in the medium and short term to the extent that such missions exposed problems in the achievement of long-term strategy. This was a change from the approach followed in 1974, which emphasized mission priorities within the constraints of a planning wedge (funds expected to be available for new starts in space science). After two years' experience with shifting mission priorities within a constantly shrinking planning wedge, the Board believed that the adoption of a specific mission model could be too rigid a framework to suggest being imposed upon NASA management. Priorities can and should be set up to the point that the programs under consideration are uniformly excellent in quality. We believe that all the programs supported in our 1974 report qualify in this respect and that the task of the Board should now be to demonstrate their crucial role in the further exploration of space.

At the outset of the current cycle of studies, the SSB had not developed a consistent approach to the definition of science strategy. Traditionally, space scientists have stated their aspirations in terms of specific missions. This has two drawbacks. In the first place, the science objectives may be unnecessarily constrained by a mission concept that later turns out to be less than optimum. In the second place, the initiation of a particular mission involves managerial, institutional, and political factors that may be incompletely appreciated by the Board. The impact of our advice is lessened the more that unfamiliar issues are involved.

This question was faced by the Committee on Planetary and Lunar Exploration (COMPLEX), which formulated a strategy for the exploration of the outer planets solely in scientific terms, without specific mission recommendations. The SSB welcomes this report. It should not be regarded as the only approach to strategic planning, but it does represent one upon which in general too little emphasis has been placed in the past. The Board, therefore, agreed to depart from its 1974 procedure and to incorporate the COMPLEX report into the Board report, rather than to extract specific issues. It was possible to review and adopt this report in the time available because the issues, stated solely in scientific terms, were within the professional expertise of Board members.

Following the procedure established in 1974, the Board sought opinions from those not directly involved in its studies by circulating OCSS 1974 to over 200 other scientists with a request for comments.

These comments were considered by the SSB committees as input material.

The 1974 study also made three specific requests for further work: (1) priorities in outer-solar-system research (OCSS 1974, p. 20); (2) goals and priorities in infrared and millimeter astronomy with particular consideration of the proposed infrared survey satellite (OCSS 1974, pp. 18, 19); and (3) a review of solar astronomy, its priorities, objectives, and relationships to other disciplines, with particular consideration of the proposed Solar Maximum Mission (OCSS, pp. 16, 17). The first was undertaken by the SSB Committee on Planetary and Lunar Exploration. The second and third were the subjects of special summer studies chaired by G. Neugebauer and E. Parker, respectively. The reports of the summer studies were reviewed and transmitted to the Space Science Board by the Committee on Space Astronomy and Astrophysics.

Reports from the disciplinary committees and summer studies were reviewed by the Board at its 53rd meeting held October 9, 10, and 11, 1975. The findings and recommendations, which constitute the essence of the Board's report, were drawn up and approved at that time. The order in which these are stated bears no relationship to their relative importance.

We have not repeated material from the 1974 report except where an important purpose is served by so doing. *The material in the 1974 report remains current SSB policy unless superseded by specific statements in this report.* For example, the Committee on Space Physics has reaffirmed the strategy endorsed by the Board last year. This strategy appears in their summary report appended as a working paper.

2
Science in the Space Program

Budget restrictions have steadily reduced the number of scientific initiatives (new starts) in recent years and made it difficult to exploit the fullest potential for scientific return from successful space missions. This trend, if not reversed, could lead to a national space program with minimal science, in contradiction to the stated objectives of the Space Act.

The National Aeronautics and Space Act of 1958, in its Declaration of Policy and Purpose, lists eight major objectives, including

— the expansion of human knowledge of phenomena in the atmosphere and space;

— the establishment of long-range studies of the potential benefits to be gained from the opportunities for, and the problems involved in, the utilization of aeronautical and space activities for peaceful and scientific purposes.

New starts are an important measure of continuity of support, which is essential to scientific productivity. To maintain a healthy level of productivity, the science program should be continuously upgraded by new initiatives. Any prolonged hiatus in new starts tends to put off the infusion of creative ideas and to discourage young scientists from entering the field.

Orbiting observatories offer a steady flow of fundamentally new knowledge of the universe. Improved understanding of the complex environment of the earth in space becomes increasingly important as its relationship to societal problems becomes clearer. Visits to the planets and other bodies of the solar system reveal their nature and refine our understanding of the evolution of the solar system and the origins of life. Such an expansion of human knowledge is one of the highest intellectual goals of a civilized society.

The vigorous pursuit of space science provides an essential basis for and will inevitably lead to rewards in new technology and economic benefits from applications. The space-science portion of the national program needs to be supported on a growing scale if we are to realize the goals envisaged when NASA was founded.

3
Findings and Recommendations

I. ACTIVITIES IN SUPPORT OF SPACE MISSIONS

The National Aeronautics and Space Administration, whose charter commits the agency to expand knowledge of phenomena in space, emphasizes those areas of knowledge that are best treated using space vehicles. Nevertheless, exclusive emphasis on space missions will not produce the optimum program of space science without support from certain related activities. The level of support for nonmission activities, including development of new instruments, techniques, and detectors; certain balloonborne and ground-based observational programs; and analysis of data from space missions generally falls short, sometimes far short, of the level that would provide an optimum return for the total expenditure on space science.

In support of this statement, the Board notes that

1. Results from ground-based observations and theoretical modeling have been of critical importance in planning and supporting space missions.
2. Instruments, techniques, and detectors developed for use in ground-based programs, in balloons, and in high-altitude aircraft have been applied later to space missions with great effectiveness.
3. Data-analysis programs must be well funded if missions are to accomplish their full objectives in terms of new knowledge.

The Board believes it to be a matter of urgency for NASA to re-examine its policies in this area, with a view to improving the balance between support for missions and for nonmission activities.

II. SPACE BIOLOGY AND MEDICINE

The Board believes that certain problems in space biology are likely to require the Shuttle and Spacelab for answers. These include aspects of physiology that are of direct concern to astronaut health and safety. From the biological viewpoint, the unique feature of the Shuttle and Spacelab is the absence of gravitational effects. The question remains whether terrestrial gravitational effects play a sufficiently fundamental role in biological processes to warrant experimentation in the orbiting laboratory. This question can be answered affirmatively in a few specific areas, but in other areas the relevant experiments to be performed in space cannot yet be clearly defined. Although previous studies have outlined general areas of potential biological significance, we believe that clearly defined specific experiments and hypotheses of fundamental biological significance still remain to be generated by the biological community. We, therefore, approve of NASA's ongoing efforts to publicize the availability of Spacelab facilities and to encourage the submission of innovative questions and approaches applicable to Spacelab experimentation. Solicitations for potential flight experiments need to be supplemented with support for ground-based experimentation and workshops, pertinent to questions of gravitational biology. Furthermore, an important adjunct to the solicitation of proposals is that they be subject to critical appraisal by appropriate review panels.

Decisions on the extent to which Spacelab needs to be designed for biological and medical experimentation and the details of that design should await this further definition of the biological problems.

III. EXPLORATION OUT OF THE ECLIPTIC

The Board continues to endorse the desirability of expanding our direct knowledge of the sun and interplanetary space from two to three dimensions by means of scientific instruments carried out of the ecliptic plane.

The study on solar physics includes a scientific rationale for out-of-the-ecliptic exploration, which is supported by the Committee on Space Astronomy and Astrophysics. In addition, the Space Physics Committee has strengthened its recommendation that measurements be made of the interplanetary medium at high solar latitudes.

We recommend that NASA continue to evaluate different means of sending a spacecraft to high solar latitudes in order that the Board can better understand the probable scientific return in terms of the cost.

IV. PLANETARY AND LUNAR EXPLORATION

The Committee on Planetary and Lunar Exploration responded to the Board's request for a 10-year strategy with a statement that addresses itself solely to scientific criteria independently of those institutional and political issues that are also involved in mission definition.

Because the report is concerned solely with scientific judgments, the Board was able to review the report and its recommendations in detail and adopted it as expressing SSB policy. The report constitutes Part II and should be consulted for particulars.

It is the view of the Space Science Board that planetary exploration will be an area of major scientific importance over the next decade and that vigorous activity should continue in this field.

The report presented in 1975 concentrates on the strategy for exploration of the outer planets. It is the Board's wish that the Committee on Planetary and Lunar Exploration extend its considerations to the inner planets and that other disciplinary committees consider the applicability of this approach to their own areas.

V. EXPLORATION OF THE OUTER SOLAR SYSTEM

In OCSS 1974, the Board recommended that NASA undertake an immediate re-examination of the strategy for exploration of the outer solar system during the next decade and requested the Committee on Planetary and Lunar Exploration to recommend to the Board a program consistent with the anticipated fiscal constraints.

The Committee has responded in the following terms:

> Based upon intensive investigation, the Committee on Planetary and Lunar Exploration recommends that in-depth exploration of Jupiter and its satellites and reconnaissance of Uranus are worthy objectives that should be achieved within the period 1975–1985. We further *recommend* that NASA be in an adequate position through advanced planning to initiate the exploration of Saturn and its satellites, subsequent to the return of some basic data from the reconnaissance missions.

The Committee report states that a sufficient number of different opportunities for Jupiter exploration and Uranus reconnaissance exist, so that the funding conflict identified in the 1974 study need not be a constraint. The SSB accepts this view and supports the recommendations of the Committee with respect to outer-planets strategy.

VI. THE LARGE SPACE TELESCOPE

The Board continues to support the Large Space Telescope (LST) for a new start in fiscal year 1977. The LST, with appropriate focal-plane instruments, will make available high-resolution observations of unprecedented sensitivity and scope in spectral regions from the far ultraviolet through the infrared. This single facility will form a major component of the space-science program in the coming decade. Its use will provide important data in many diverse fields from the planets out to the most distant galaxies. During the past 12 months, the LST project has been the subject of extensive reviews and evaluation by NASA, assisted by many groups of outside specialists. *It is clear that the time is now ripe for a start on this important project; nothing is to be gained by delay.*

This opportunity to establish a refurbishable LST facility in earth orbit, using the Space Shuttle, represents, for the near term, one of the most clearly defined uses of the Shuttle system for scientific purposes. We consider it important, therefore, that the development of the LST be phased to the development of the Shuttle. *This phasing of LST development should be done so that other critical components of the space-science program can evolve concurrently and without conflict.*

The Board understands that focal-plane instruments have been through a definition phase and at least four will be incorporated in the initial LST configuration. *We urge that adequate support for the development of LST instruments and their detectors be made available in a timely fashion to assure the highest quality for these crucial components.*

The availability of the LST, a space observatory with a lifetime of 10–15 years or more, will raise many new challenges for the national and international scientific communities. The scientific returns from the observatory will depend on the creativity and the imagination with which these challenges are met. Ground-based observations and the vast quantity of data obtained by means of space telescopes will both be important for a long time to come. Proper programming of the observations, organization, and use of the data call for a thorough consultation with the scientific community at large. The Board encourages such a consultation and hopes to address at later meetings the question of whether new institutions and international arrangements should be recommended in this situation. The LST is the first of a series of space observatories currently being discussed for the next decade. Many of the issues, such as the effective use and management of LST, also apply to these other observatories. The commitment to LST provides the opportunity to develop operating procedures in this area, and this will provide the necessary basis for planning of future space observatories.

VII. SOLAR PHYSICS

In its 1974 report, the Board called for a review of progress in solar astronomy and recommendations for priorities for instrumentation and mission objectives.

A special study was held during August 1975; subsequently, the study was reviewed and reported to the Board by the Committee on Space Astronomy and Astrophysics. The report of the summer study is attached to this report as a working paper.

The Board welcomes the excellent review of the status of solar physics contained in this report. Its view of the Solar Maximum Mission (SMM), which has been proposed for funding in 1977, is stated below. The longer-term programs recommended by the study will require further consideration as information on Shuttle operations becomes available.

The study group identified the nature of the terrestrial environment as one of the important reasons for maintaining a vigorous program in solar physics, and the Board endorses this finding. The scientific basis for any influence of solar activity on weather and climate is poorly understood at the present time, and more extensive investigation is needed before the appropriate missions and instruments can be identified. *The solar-physics program must be responsive to earth-environmental needs, and the solar-physics missions (including SMM) should be examined from the point of view of their best possible performance in this respect.*

Among the most important but least understood types of solar phenomenon is the solar flare. An observational study of solar flares will require the best attainable spatial and temporal resolution in measurements made with a variety of instruments over a broad wavelength region, extending from gamma rays to the near infrared. Such instruments must be sighted on the same region of the sun in a series of attempts to record the histories of flares, including their preliminary buildup. The characteristics of energetic particles accelerated by these flares should also be measured. The time to make these studies is during solar maximum, when flares occur frequently with a wide range of intensities. *The Board recommends that these studies be carried out. In order to accomplish these objectives in the forthcoming period of solar maximum, spacecraft will have to be launched no later than 1979.* A combination of the presently planned SMM and one or more International Sun–Earth Explorer spacecraft appears to be adequate for this task.

VIII. INFRARED AND SUBMILLIMETER ASTRONOMY

In its 1974 report, the Board raised a number of technical questions about the proposed infrared survey satellite mission and recommended that a study of goals and priorities in space infrared and millimeter astronomy be undertaken in 1975.

A study was held during August 1975, and the results have been reviewed and reported to the Board by the Committee on Space Astronomy and Astrophysics. The study report is appended as a working paper in this report.

On the basis of this information, the Board believes that an infrared survey satellite should be flown at an early opportunity.

A consortium of European and United States astronomers concerned with the infrared survey has emphasized the need for a long-wavelength (30–200 μm) capability in addition to the shorter wavelength (7–30 μm) instruments. This emphasis appears to be consistent with the strategy for an infrared sky survey recommended by the study group. Improvements in detector technology for the far infrared must be encouraged in order to achieve the best scientific results from a broadband sky survey.

The study group further recommended the installation of infrared instruments on the Large Space Telescope. The Board agrees that this would be a valuable development.

The question of contamination of cryogenic telescopes in the environment of the Shuttle was also raised by the study group. The Board agrees with the proposal to continue the study of cryogenic systems for space application and believes that measurements on the early Shuttle flights must be made in order to understand the contamination environment prior to final definition of a Shuttle cryogenic telescope.

II
Report of the Committee on Planetary and Lunar Exploration

Committee on Planetary and Lunar Exploration

Gerald J. Wasserburg, *Chairman*
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Dean P. Kastel, *Executive Secretary*

1
Introduction

The Committee on Planetary and Lunar Exploration (COMPLEX) submits the following report to the Space Science Board. This report represents our effort to

1. Evaluate the status of existing planetary and lunar programs currently under way;
2. Propose a long-term strategy for exploring the outer solar system for the period 1975–1985; and
3. Assess the strategy for exploration of the terrestrial planets for the short-term period 1975–1978.

The evaluation of existing programs is part of the continuing function of the Space Science Board. The proposed strategy for exploring the outer solar system is in response to the recommendation of the 1974 report of the Space Science Board (*Opportunities and Choices for Space Science, 1974,* National Academy of Sciences, Washington, D.C., 1975).

In preparing this report, the committee adopted the view that it would be useful to present a series of goals that are independent of specific missions. Consequently, we have omitted reference to specific missions for the outer planets in order to provide a more flexible format for program planning. However, the urgency of initiating deep-space missions even on a long-time basis is great because of the restrictive launch windows, the lead times required for system development, and the flight time. We have thus responded to the request of the Space Science Board not in terms of recommending specific missions but rather by presenting a strategy and the goals that should be accomplished over a time frame of a decade. We emphasize that the urgency for action is not removed by this approach.

Because of the limited time available to the committee we did not attempt to provide a long-term strategy for the terrestrial planets but instead outlined a short-term strategy for general guidance in this area.

2
General Overview

The committee considers exploration of the solar system as one of the most fundamental activities of the space program. Over the past ten years, the planetary program has achieved major success in a series of reconnaissance and exploration missions. These deep-space missions have permitted close encounters with other planets for the first time in human history. In general, programs have been carried out with the effective utilization of both the available scientific and engineering resources. These programs were conducted by the United States in cooperation with other nations and have established this country as a leader in planetary exploration and space science.

The past accomplishments are comprised of reconnaissance missions to Mercury, Venus, Mars, and Jupiter. Intensive studies have been carried out on the moon by the Apollo missions, and an intensive exploratory mission to Mars is now on its way. A reconnaissance mission to Saturn is in preparation, and the first mission with atmospheric entry probes is now being prepared to go to Venus.

3
Scientific Goals, Sequence, and Strategy

The primary scientific goals in investigating the solar system are to determine the composition, structure, and environment of the planets and their satellites in order to define the present morphology and dynamics of the solar system and with the purpose of making major steps in understanding the processes by which the planets formed from the solar nebula and how they have evolved with time and how the appearance of life in the solar system is related to the chemical history of the system. The investigation of the interplanetary and interstellar medium is considered an intrinsic part of such an endeavor.

Scientific interest in the planets lies in the expectation that investigation of these bodies will contribute greatly not only toward unraveling the evolution of the solar system but also that it will enhance our understanding of the processes that take place in the atmosphere, the oceans, and the deep interior of the earth.

Our deliberations have depended greatly on the strategy for planetary and lunar exploration given in the priorities section of the 1974 Space Science Board report and in the extensive working papers of COMPLEX that were appended to the 1974 report.

In the following, we present a categorization of the levels of scientific investigation and of the techniques of planetary encounter by spacecraft. These categories are used qualitatively in the discussion of various options and are defined here in order to expose more clearly our recommendations and the basis for these views.

I. LEVELS OF INVESTIGATION AND GOALS

The investigation of any solar-system object can be divided into three categories: *reconnaissance, exploration,* and *intensive study.* As the first step of our qualitative scale of progress on a given planet, we may speak of *reconnaissance* in which major characteristics are first sought and identified. Reconnaissance tells us qualitatively what the planet is like and provides enough information about the character of the planet and its environment to allow us to proceed to the stage of exploration of the planet. *Exploration* seeks the systematic discovery and understanding of the processes, history, and evolution of the planet on a global scale. In the final step, that of *intensive study,* sharply formulated specific problems of high importance are pursued in depth. *The sequence of investigations should follow the order of reconnaissance, exploration, and intensive study.* Intensive study should, in most instances, be based on adequate reconnaissance and exploration. The actual sequence of investigation will be determined by the funding, the scientific conceptions and instrumentation, technology, and celestial mechanical constraints. All of these restrictions will change with time so that specific approaches used to study a solar-system object must be reconsidered and guided in part by major new discoveries and technologies, but in general, certain guidelines should be followed to preserve the overall scientific rationale.

In addition to describing general strategic goals for planetary exploration an enumeration of specific scientific objectives and observations is presented. *These specific scientific objectives will consist of primary objectives that are the principal basis for defining the mission and secondary objectives that greatly enhance the value of the mission. All missions will consist of both types of objective.* The primary objectives should serve as the key in guiding the use of resources. The secondary objectives should not be considered as parenthetical but of lesser priority insofar as spacecraft design and mission requirements are concerned. *We suggest that NASA assess the acquisition procedures so that this distinction can be made in the choice and management of experiments and principal investigators without injury to this endeavor but so that the primary scientific objectives are governing factors.*

II. MISSION TECHNIQUES

Many of the key physical characteristics of the planets are obtainable only from instrumented spacecraft that encounter these bodies.

Earth-based instruments have provided much important data for the planets and in the past have allowed a limited reconnaissance of the planets. Continued planetary observations from the ground, from high altitude, and, in the future, from earth-orbital platforms must be vigorously pursued in order to better define these objects and to provide the fundamental data that are complementary to spacecraft measurements. However, the best data on these distant objects taken from earth-based and earth-orbital platforms must remain grossly inadequate for many of the fundamental observations, so that a sequence of deep-space missions is required to achieve advances in understanding the nature, evolution, and origin of planetary bodies of the solar system.

The techniques used in deep-space missions should begin with *flybys* that are designed to define and identify important characteristics of an object in brief visits, at a relatively low cost, and that fulfill the role of reconnaissance. These brief encounters should be followed by spacecraft with capability for a more lengthy stay near the target. Such spacecraft would carry experiments for specific investigations based on the information obtained from the flyby mission and updated earth-based observations. *Orbiting* spacecraft are an example of such mission techniques. These orbiting spacecraft should be instrumented to measure the planetary properties on a global scale by remote sensing and are used in the beginnings of the exploratory stage. Another type of follow-on mission involves *in situ* measurements on the target object by means of *entry probes* (either atmospheric or hard landers). These missions are of the greatest importance in defining the chemical composition of a planet. In the case of rocky planets, these may be followed by *soft landers,* which can carry out a wide variety of geophysical, chemical, and biological investigations at the surface on the place of landing. The latter missions would represent more advanced systems and should, in general, follow after the orbital and entry-probe missions.

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

The above types of mission techniques are elements in a strategy of progressive exploration for some object and parallel the stages of investigation. There is some tendency to have a mixture of these elements in missions designed for remote objects. The optimum tactics must depend on the environment of a particular planet, but, in general, the information from each successive type of mission is necessary before the subsequent, more complex mission techniques can be reasonably initiated and must be placed in the context of an overall strategy.

III. PRESENT STRATEGY

In its deliberations, COMPLEX discussed the relative emphasis that should be given to reconnaissance as compared with exploration and intensive studies. In consideration of the extensive reconnaissance that has already been carried out so successfully or is under way, the committee believes that the major questions can now only be answered by a greater emphasis on the exploratory phase with new or improved scientific instruments. The committee also believes that some continuing level of reconnaissance should be carried out in parts of the solar system where our ignorance is greatest and where the opportunity of new discovery is large.

The committee supports the previous view of the Space Science Board that the approach to planetary investigations should be a balanced program involving the first reconnaissance of the planets coupled with the more systematic exploration of selected planets and in limited cases followed by intensive study. However, *as a result of the successful reconnaissance program carried out by NASA, the committee recommends that there should be a shift in emphasis toward systematic exploration with emphasis on selected planets, but with some continuing level of reconnaissance.* In consideration of the constrained NASA budget and the major commitment toward the development of the Space Shuttle, we have developed a conservatively paced strategy for the next decade. Within this framework, *we have identified limited goals of the highest importance, which should be achieved within this time period and which can only be obtained with planetary encounters by spacecraft.*

It is the unanimous view of the committee that planetary exploration will continue to be an area of major scientific importance over the next decade and that continuing vigorous activity in this field is fully justified.

IV. LAUNCH CAPABILITIES FOR PLANETARY EXPLORATION

Most of the planetary exploration programs currently under way and those that are being contemplated for the next decade place substantial

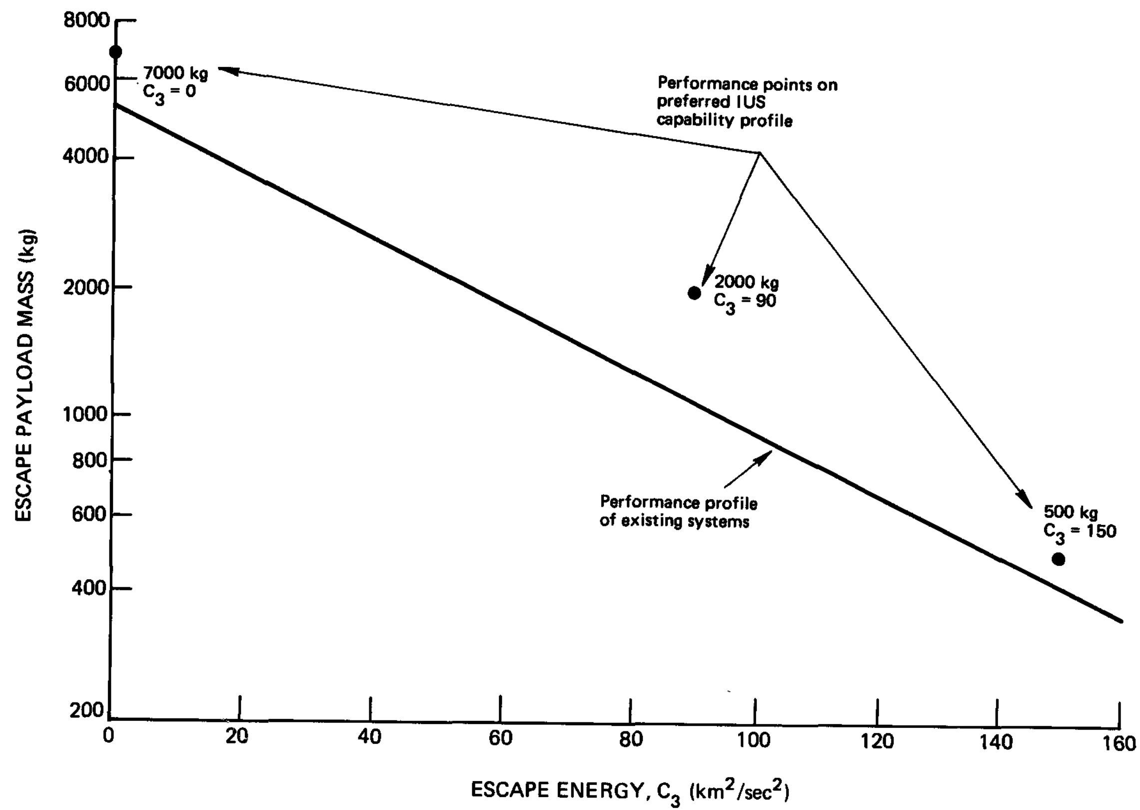

FIGURE 1 Launch capabilities for planetary exploration: current capability and preferred IUS capabilities.

requirements on launch capability. A commitment to maintain sufficient launch capability is essential if the planetary program is to continue. At present, many planetary missions require the maximum capability now available (Titan III E/Centaur/TE-364). The need for high-energy capability exists because of the conditions of planetary encounter and the mass of a basic payload. While some reconnaissance missions can be carried out with relatively low launch capability, it is not possible to continue intensive planetary exploration without fully utilizing the current maximum capability because of the basic physical constraints. An increase in this capability may be necessary in order to carry out some of the planetary orbiter missions recommended here. Because of the strong scientific need for planetary probes and planetary orbiters for Jupiter and its satellites and a polar orbiter and lander for Mars, we urge that every effort be made to ensure a transportation system adequate for this purpose.

The program for exploration of part of the outer solar system presented in this report covers the period 1975–1985 and overlaps the first active period of the Space Shuttle transportation system. The basic Shuttle system has many potential advantages for earth-orbiting missions; however, their characteristics do not directly aid in accomplishing planetary missions that must be propelled out of earth orbit. In addition, the current schedule for test and validation of the Shuttle Interim Upper Stage extends until 1980 so that any significant delay in this program would eliminate planetary missions for some time. We therefore *recommend that NASA maintain a direct launch capability fully adequate for planetary missions until the development of an adequate Shuttle upper stage. We further recommend that any intermediate upper stage (IUS) considered for transearth payloads have at least the same capability with regard to spacecraft payload and injection energy as presently exists (see Figure 1) and be sufficient to carry 500-kg payloads with a C3 of 150 km^2/sec^2, payloads of 2000 kg at C3 of 90 km^2/sec^2, and payloads of more than 7000 kg for low C3.* The curve in Figure 1 shows the performance capability of existing systems in terms of the injected mass and escape energy. This performance is the minimum necessary to carry out the continuing exploration of the solar system as recommended by the Space Science Board. An injected mass/escape energy profile for the Shuttle IUS, which includes the three points shown, represents the preferred capability that would provide a fully adequate launch capacity for executing the recommended program without being highly restricted by celestial mechanical constraints. Consideration should be given to very high-energy systems so that mission objectives are not totally constrained by the rare gravity assist trajectories currently necessary to shorten the trip times.

Even with these launch capabilities, many goals will require placing larger payloads in planetary orbit. This will require a continuing evolution of spacecraft and their subsystems. *We endorse the ongoing effort by NASA to reduce the weight and cost of standardized available spacecraft without a corresponding reduction in capability using new developments in technology.* This should include assessment of the greater use of de-spun platforms on spinning vehicles, the trade-offs between spinning and three-axis stabilized configurations, between multiple small missions and single large ones, and between communications capability on the spacecraft and on the ground. The optimum spacecraft designs for the Shuttle era may differ significantly from those in use today.

V. EARTH-BASED AND EARTH-ORBITAL OBSERVATIONS

A rational approach to solar-system exploration requires the prudent and timely use of the many distinctive tools available to collect fundamental information about the planetary bodies. Integrated use of ground-based measurements, measurements from balloons and high-altitude aircraft, observations from earth-orbiting telescopes, and appropriate experiments on spacecraft encountering planetary bodies will lead to the highest scientific return for our efforts in planetary sciences.

The limited funds available for spacecraft exploration and the high cost and long time scale for deep-space missions require us to make sure that any important information available using cheaper and quicker means be promptly collected so that the use of spacecraft capabilities in deep-space missions can be optimized.

NASA has for many years supported, at a modest level, ground-based optical and radio planetary astronomy with the result that a significant amount of time has been made available at the largest ground-based observatories for carrying out planetary studies. Unfortunately, the earth's atmosphere precludes a large number of critical observations from the ground because of the unstable optical properties and limited transparency of the atmosphere. NASA has also supported studies with both balloons and high-altitude aircraft to partially circumvent these limitations.

We strongly endorse this effort on the part of both NASA and the astronomical community and recommend that the adequacy of NASA *support be regularly reviewed by the Space Science Board in terms of the above objective.*

These limitations on ground-based and airborne observations have provided the main impetus for the Large Space Telescope (LST) project. We anticipate that a major improvement in our planetary exploration capability should result from the LST. A number of extremely important observations of planets, satellites, asteroids, and comets will be possible with the presently planned instrument package for the LST, and by developing additional instruments this capability could be substantially expanded.

Of particular importance are the faint-object spectrograph needed for high-spatial-resolution atmospheric compositional sounding of the Jovian planets and the high-resolution planetary camera needed for study of the cloud structure and cloud motions of the Jovian planets and for synoptic studies of the Martian and Cytherean atmospheres. The ir photometer will be important for the study of the thermal balance of the Jovian planets as well as an improved determination of the thermal limb-darkening functions of Jupiter and Saturn. The monitoring of the 5-μm flux from Jupiter with at least twice earth-based resolution will also be valuable. Measurements of satellites and asteroids with the planned narrow-band ir filters will also improve our understanding of the surface mineralogy of these bodies. Second-generation instruments for the LST should include an ir interferometric spectrometer to study, with high spatial and spectral resolution, both atmospheric structure and the solid surface mineralogy of planets and satellites.

Based on the premise that the spacecraft used in planetary encounters in deep-space missions should be devoted to critical observations that are not possible from ground-based or earth-orbital observing platforms, *we recommend that a more active program be developed using earth-based and earth-orbital systems for planetary observations to complement and aid deep-space missions.* Such a program should include support for ground-based telescope studies and for a modest balloon activity and substantial support for instrumentation and observations with high-altitude aircraft and earth-orbital platforms.

We strongly recommend that a significant portion of the LST schedule be made available for planetary studies and that NASA begin immediate development of instruments that are oriented toward planetary studies for the LST and for other Shuttle deliverable payloads.

4
The Outer Planets

I. GENERAL OVERVIEW

The planets more distant from the sun than Mars are all of very low density as compared with the inner planets and contain most of the mass and angular momentum in the planetary system. The chemical composition of these bodies is not well determined but must certainly be complementary to the material that condensed to form the terrestrial planets. The major planets all have well-developed satellite systems (the most spectacular of which is Saturn's) that may reflect the stages of solar-system development. These bodies range in distance from about 5 to 40 AU from the sun and have extremely cold surface temperatures. The more distant of these bodies lie near the boundary between the solar wind and the interstellar medium. In general, our knowledge of these bodies is quite limited because of their great distance and because measurements from the neighborhood of the earth rarely permit the determination of even their most basic physical properties. The major planets are seen by their mean-density differences to fall into two gross compositional groups. Jupiter and Saturn appear to have approximately the composition of the sun, while Uranus and Neptune seem to contain large amounts of ice and rock. Knowledge of the internal structure and composition of these planetary bodies is considered of major scientific importance.

Comets are a unique group of bodies representing material that has been recently perturbed from storage in very distant orbits. The observation that comets are rich in volatile substances suggests a genetic relationship with the Uranian planets, and study of the composition of comets is considered of major importance.

A review of our knowledge of the outer planets—Jupiter, Saturn, Uranus, Neptune, and Pluto—shows that we now have some initial knowledge of Jupiter but that factual information about the other planets decreases rapidly with increasing distance, so that for Pluto, for example, even the density is poorly known. So far, among the outer planets only Jupiter has been encountered by spacecraft.

In general, there is a paucity of substantial information on the major planets. For brevity, we will use Jupiter as a standard of comparison because we have for it the highest level of information. On assessing the important information that is necessary to obtain a significant increment in our basic knowledge and understanding, it was apparent that several of the principal observations were common to all the outer planets. The first of these is the chemical composition, which relates to the bulk properties of the planet and to its origin, as well as to the nature of clouds and atmospheric phenomena. At present, the composition is only roughly surmised even for Jupiter, where the abundance of He, the second major species, is inferred only approximately. Other molecular species have been identified for Jupiter, but their precise abundances are not established. Indeed, the cloud compositions are not definitely known. The composition of the satellites of the major planets, which are a link with the terrestrial planets, is only suggested from the available density data and a few reflectance observations. In addition to determining the major chemical species and key trace elements that may characterize the bulk planetary properties, it will be of especial interest to establish the chemical state of carbon, nitrogen, oxygen, sulfur, and phosphorus in the atmospheres of the outer planets and their satellites; the detection of large-molecular-weight compounds of these elements would have profound implications for concepts of prebiotic chemistry and the origins of life.

Knowledge of the net energy balance for the major planets and their large-scale atmospheric structure as determined from clouds is basic for understanding the broad dynamics and dominant modes of atmospheric motion. These observations in conjunction with compositional data are the principal means of establishing the dynamical and internal state of gas-rich planets. A significant amount is known about the zonal circulatory system and the very-large-scale net energy balance for Jupiter as a result of ground-based and spacecraft observations. Broad atmospheric structures are known for Saturn, and nothing is currently known for Uranus or the more distant bodies.

Observations of the state of magnetization of a planet are necessary to provide a gross characterization of the dynamical and electrical character of its interior and to determine the nature and development of radiation belts of energetic charged particles resulting from interaction with the solar wind. The magnetosphere of Jupiter has been outlined by spacecraft, but there are no data on any of the other outer planets. Demonstration of the existence or absence of planetary magnetic fields for the outer solar system, as well as their specific character and acceleration mechanisms, is of basic importance. A comparison of the planetary magnetic fields for different objectives in the solar system will provide a basis for the ultimate understanding of the specific mechanisms that generate planetary magnetic fields.

We have assessed the anticipated results of two Jupiter encounters by the Mariner Jupiter Saturn (MJS) mission and concluded that there will be a distinct increase in knowledge about the general atmospheric structure and heat budget of Jupiter and that the grossest character of some satellites will be established.

In terms of the strategy outlined in Chapter 3, we conclude that these spacecraft will have fulfilled the goal of reconnaissance of Jupiter and that the next stage should be a more intensive exploration of the diverse and important phenomena of the Jovian system.

With regard to Saturn, our current information is meager, but the results from the MJS missions, which are in preparation, as well as from the retargeting of the expended Pioneer 11 spacecraft, should bring our knowledge of Saturn somewhat above the current level of Jupiter. The MJS mission is approved for a launch in 1977 and has an extensive set of excellent quality scientific instruments and communications and power systems compatible with an extended deep-space mission. With the successful completion of MJS, we will have achieved planetary encounters of a reconnaissance nature with Saturn. The commonality of basic observations needed for both Saturn and Jupiter suggests a common approach for future exploration, although major changes in direction could result from the first Saturn encounter.

With regard to Uranus, the available information is truly minuscule, although there should be some significant advances in the near future. There are adequate data to show that this planet has a chemical composition distinct from Jupiter and Saturn (possibly cometary) and that the axis of rotation of its satellite system and its presumed axis of rotation are almost parallel to the orbit plane. There are few substantial data on Uranus and much less on the more distant planets.

From the nature of the class of observational data required for a significant increase in knowledge, it was concluded that important complementary data on the outer planets could be achieved from earth-based systems but that much of the fundamental data could only be obtained from appropriate spacecraft encounters. Considerable care is required to ensure that the observations from spacecraft are major advances over those that will be achieved from earth-based systems. The accessibility of the outer solar system has been demonstrated by the Pioneer 10 and 11 missions, which showed that the asteroid belt can be crossed successfully, established quantitatively the nature of the radiation belt hazards at Jupiter, and determined the basic form of the magnetosphere and particle environment of that planet.

II. PARTICLES AND FIELDS IN THE OUTER SOLAR SYSTEM

The spacecraft that are used for planetary investigations must traverse the interplanetary medium and may ultimately reach the interstellar medium. The use of these same vehicles for studying particles and fields in the outer solar system is therefore a natural association with planetary exploration. The study of plasma and field parameters and the energetic particle populations at heliocentric radii of less than 5 AU has achieved significant maturity and observational sophistication. The study of high-energy galactic cosmic rays (greater than 10 GeV/nucleon) can and is being pursued directly from earth-orbiting spacecraft; however, no definitive data on the geometry and dynamics of the solar wind-dominated medium at large heliocentric distances exist, and their study must rely on *in situ* observations during outer-solar-system missions. Similarly, observations of low-energy galactic cosmic rays (less than 300 MeV/nucleon) cannot be performed in the inner solar system and must be carried out by *in situ* measurements from spacecraft at large heliocentric radii, effectively outside the solar modulation region.

The core questions in the area of plasma and fields are concerned with basic (unknown) features of astrophysical plasmas, such as the dissipation of magnetohydrodynamic waves in collisionless plasmas, field-line merging, the interaction of the solar wind with the interstellar medium, including its shock or nonshock termination, the charge exchange of solar plasma with interstellar gas, the penetration of interstellar gas into the solar system, and the angular momentum transfer from the sun to the Galaxy, which can be pursued from the series of plasma and field observations over a range of large heliocentric radii and longitude at different phases of the solar cycle.

In the energy range of cosmic rays, outer-solar-system exploration permits direct measurement of interstellar parameters. While plasma and low-frequency field measurements reflect primarily local physical parameters, cosmic rays probe and sample the Galaxy over widely differing scales, depending on their energy. The true interstellar cosmic-ray spectrum gradually unfolds toward lower energies as the measurements are performed at increasingly larger heliocentric radii, where solar modulation progressively is reduced. In particular, the heliosphere is essentially opaque to low-energy cosmic rays (less than 300 MeV/nucleon) in the inner solar system, and all relevant interstellar questions must be ultimately answered from observations in the spectral domain beyond the solar modulation regions at large heliocentric radii. Extending the spectral coverage in energy, charge, and mass, and their streaming patterns, to those low energies is crucial to an understanding of the role of cosmic rays in interstellar heating, their suspected acceleration in the outer solar system, and their role in interstellar dynamics and in the possible identification of specific cosmic-ray generators, such as nearby pulsars and supernovae, some of whose material and electromagnetic properties could be deduced from their cosmic-ray progeny.

A distinct increase in knowledge in these areas should be achieved by the MJS mission, and it is recognized that further major advances can be achieved by properly instrumented spacecraft that reach other planetary objectives in the far outer solar system.

III. RECOMMENDATIONS FOR STUDY OF THE OUTER PLANETS, 1975–1985 GOALS

The outer planets and their satellites represent a wholly new class of planetological problems, which are fundamental to the understanding of the formation of the solar system and about which we have little information. Based on these considerations, *we recommend that a significant effort in the planetary program over the next decade be devoted toward a study of the outer solar system.*

Based on the reconnaissance missions to Jupiter that have been completed and the information obtained from astronomical observations, there are a host of profound and exciting scientific problems, which are well identified for this planet and its satellites. On this basis, it was clear that well-defined exploratory investigations were indicated. *Jupiter is the largest planet of the solar system and demonstrates a wide range of dynamical and physical behavior that makes it the primary object of outer-solar-system exploration.*

The first reconnaissance mission to Saturn will occur with the expended Pioneer 11 spacecraft in 1979, and the fully instrumented reconnaissance MJS mission will encounter Saturn in 1981. It is therefore necessary and reasonable to await results from these encounters prior to initiating exploratory missions.

Because of enormous distances from the earth to the outermost planets, the time required by existing spacecraft to reach them is rather large. We had therefore to consider the extent to which significant efforts should be directed toward deep-space missions of long duration. From the gross chemical composition as now known, it may be seen that Jupiter and Saturn are similar but distinctive from the Uranian planets. A Uranus encounter is achievable with existing spacecraft and technology. Reconnaissance of Uranus would yield information that is not obtainable from earth-orbital or earth-based observations on this chemically distinctive body and would give the first clear view of a planet and satellite system that are in a bizarre orientation compared with all the other planets. *The flight time to Uranus is about six years, and it represents the most distant planetary object that was considered a worthy goal for the next decade. It was the opinion of the committee that a reconnaissance of Uranus would significantly enhance our knowledge.*

Based on intensive investigation, COMPLEX recommends that in-depth exploration of Jupiter and its satellites and reconnaissance of Uranus are worthy objectives that should be achieved within the period 1975–1985. We further recommend that NASA be in an adequate position through advanced planning to initiate the exploration of Saturn and its satellites, subsequent to the return of some basic data from the reconnaissance missions.

COMPLEX has considered the role of a comet encounter mission for the primary purpose of determining the composition and structure of these objects. There is at present only limited experience in sampling gases and dust grains in conditions that approximate a comet encounter. The committee is most attracted to the possibility of investigating a comet by a spacecraft encounter. Such a mission would be a first reconnaissance investigation of considerable merit in the framework of the strategy presented here for study of the outer planets if it were possible to obtain critical data on composition and structure. *COMPLEX considers the study of comets to be of major importance and recommends that efforts be directed toward establishing the nature and quality of scientific experiments that could yield important data in a comet encounter so that the role of a comet mission can be properly assessed in the framework of the current strategy.*

The program discussed above is the most conservative posture that we can recommend for the outer solar system consistent with an active space program. It considers only one major reconnaissance effort (Uranus) and one major exploratory goal (Jupiter and its satellites) for a period of one decade and utilizing essentially the current technology base.

IV. RESPONSE OF COMPLEX TO THE SSB REQUEST

COMPLEX views its most effective response for a 10-year strategy to be independent of specific mission opportunities; we also recognize that we were asked by the Space Science Board to assess a possible funding conflict in the recommendation for outer-solar-system exploration that appeared in the 1974 report. We discussed in detail a variety of approaches to Jupiter exploration and Uranus reconnaissance during 1975–1985 and found that a number of additional mission possibilities had been developed subsequent to the 1974 study. The Committee is informed that there are launch opportunities from 1978 to 1981–1982 (the latter being less favorable in terms of energy requirements and available launch vehicles) that would permit Uranus encounter prior to and within a couple of years subsequent to 1985. In recognizing these opportunities and in terms of the strategy developed for the indicated decade, it is expected that the exploration of Jupiter and reconnaissance of Uranus can be achieved within anticipated tight funding constraints and that a vigorous program for outer-solar-system investigation can be maintained. *We therefore recommend that the strategy developed in this report be followed.*

V. INSTRUMENTATION FOR THE OUTER PLANETS

As a consequence of the completion of the reconnaissance carried out to Jupiter, there should be a change in emphasis with regard to balance between reconnaissance and exploratory missions. Some of the key scientific questions will now require exploratory investigations with a change in type of some flight experiments and some technological developments. The emphasis on purely reconnaissance missions may be considerably diminished. In particular, *determination of the chemical composition of the outer planets and satellites is considered an essential component if there is to be a significant advance in our understanding of solar-system evolution.* This will require the development of atmospheric entry probes capable of determining the precise chemical composition and some isotopic abundances down to high pressures by *in situ* sampling. While extensive sampling has been carried out with rockets in the earth's atmosphere, the first experience with *in situ* atmospheric analyses of another planet will be carried out by Pioneer Venus in 1978.* The results of those experiments should provide a test of the present entry-probe systems; however, because of the difference in environment, chemical composition, and entry conditions, the committee considers it necessary to initiate the study and development of atmospheric entry probes and associated instruments for investigating the major planets. This is in accord with the Space Science Board views for the past several years. Preliminary studies by NASA indicate the general technical feasibility of entry probes for the outer planets.

We consider that atmospheric entry probes are urgently needed in order to study the nature of the "gas bag," or major, planets and *recommend that NASA vigorously pursue the study and development of entry-probe techniques and scientific instruments for the purpose of measuring the chemical composition, partial isotopic composition, temperature, and pressure down to a level of approximately 100 bars for use in the atmospheres of Jupiter and Saturn.*

An analogous problem exists with regard to the composition of the Jovian satellites and the Saturnian satellites and rings and also appears in the discussion of the terrestrial planets and the moon. Determination of the gross chemical composition and the nature of the icy and rocky substances on the satellite surfaces comprise the critical problem area. There has not yet been adequate development of instrument concepts and instrument prototypes, because only limited spaceflight experience has been obtained in this area. However, advances in analysis by remote-sensing techniques from flybys or orbit should permit resolution of the first-order questions.

We consider the chemical composition and state of the icy and rocky surfaces of the Jovian satellites to be of major scientific importance and recommend that NASA promptly initiate the study and development of remote-sensing instruments to be operated from flyby or orbiter platforms to carry out such determinations.

The reconnaissance mission to Uranus that is recommended should not require any major technological advances beyond that used for the

* Information on the two Soviet Venus probes Venera 9 and 10 became available subsequent to the submission of this report. There is as yet no detailed account of the science objectives of these missions nor an account of the full complement of observations that were actually carried out, but they probably will include data from a neutral mass spectrometer on an entry probe. Spectacular imaging results were obtained with both landers. From the available data on the Venera instruments, we anticipate that the possible Soviet observations will not provide those data that are critical to an understanding of the global meteorology and the precise chemistry of the Venusian atmosphere.

approved MJS mission; however, substantial development effort on the scientific instrument capabilities will be necessary in order to obtain significant compositional information, data on atmospheric structure at good resolution, and estimates of the thermal flux from the flyby encounter.

Severe environmental conditions will be encountered during Jupiter exploration, including an intense radiation belt and a high entry velocity for probes. The serviceability of two spacecraft that have already undergone close Jupiter encounters is encouraging. NASA is fully aware of these problems, and efforts are being made to cope with them. Testimony provided by several experts indicated that these difficulties may reasonably be overcome. *We strongly endorse continued vigorous efforts to carry out a program of adequate radiation hardening and of entry-probe studies, which are necessary for the operation of spacecraft in the Jovian environment.*

VI. OBJECTIVES FOR JUPITER AND URANUS

In this section we outline the *primary* objectives for the exploration of Jupiter and its satellites and the reconnaissance of Uranus and its satellites. We also state the measurements in terms of primary and secondary objectives that are considered critical to achieve these objectives. Based on our current understanding, approximate limits of error or ranges of sensitivity and spectral range are given that we believe are necessary to fulfill the purpose. For experimental fields in which there exists extensive flight experience we have only given a broad outline of the extensive measuring techniques that are already available and indicated the most basic observational requirement. For new areas or for experiments where there is only limited flight experience, we have been much more specific. The primary observational objectives are considered to be achievable only by means of spacecraft encountering the planet.

A. Jupiter Exploration Objectives

The primary objectives in the exploration of Jupiter and its satellites for the period 1975–1985 in order of importance are (1) determination of the chemical composition and physical state of its atmosphere, (2) the chemical composition and physical state of the satellites, and (3) the topology and behavior of the magnetic field and the energetic particle fluxes. We believe that these same objectives will also apply to the intensive study of Saturn.

In order to carry out the first exploration of Jupiter, it will be necessary to utilize orbiting spacecraft and probe-delivering spacecraft. From consideration of the primary objectives, it is conceivable that all of them could be carried out in a single mission by an orbiting spacecraft that would release a probe into the atmosphere and then tour the Jovian satellites by the elegant scheme of pumping and cranking while simultaneously measuring energetic particles and fields. The sequence given above is compatible with the order of priorities of the primary objectives. However, the payload mass required for remote sensing of the Jovian satellites and the mass of an entry probe may be too great for a single mission. In this case, the goals could be fulfilled by two missions, each having payload requirements that are readily achievable.

1. CHEMICAL COMPOSITION AND PHYSICAL STATE OF THE ATMOSPHERE

The first primary observational objective is the *in situ* measurement of the chemical composition at depths between 0.1 and approximately 100 bars. The information required is (1) the determination of a pressure and temperature profile within an accuracy of ± 1 percent and $\pm 0.5°C$, respectively, measured several times per scale height, and (2) the atomic and molecular composition measured at several points between 0.1 and 100 bars. Some specific compositional quantities are of primary importance: the mass fraction of helium should be known to within 1 percent of the total; and the identification and relative concentration of the three most abundant molecular and atomic species other than H_2 and He should be determined to an accuracy of approximately 10 percent and should include NH_3, CH_4, and H_2O. Identification of other constituents with lower abundances should be attempted. At least one measurement of the relative isotopic abundances of ^{20}Ne and ^{22}Ne should be made with an accuracy of 5 percent. (3) Ancillary measurements of the particle density should be carried out in conjunction with these chemical measurements to determine if the sampling point is inside a dense cloud layer. (4) To determine the vertical distribution of radiative sources and sinks, profiles of the upward and downward fluxes should be measured in the intervals 0.4 to 1.0 μm and 5 to 150 μm. These measurements should be made several times per scale height and should be accurate to ± 5 percent relative to the outgoing flux at the surface of the atmosphere.

The secondary objective of considerable importance is determination of the isotopic composition of H, O, C, N, and other elements in the atmosphere. These are of extremely great importance and were not listed as a primary objective because of possible technical difficulties. Determination of cloud particle composition and of particle size distribution would be a valuable complement to the first-order measurements.

Remote-sensing data collected in coordination with the probe would increase the likelihood that the probe data would be useful for the interpretation of all the global remote-sensing data and thus would be a highly desirable secondary objective of such a mission. These measurements should include visible imaging of the site at a resolution of 30 km or better, 5-μm measurements at comparable spatial resolution, and measurements in the 15–30 μm region at lower spatial resolution.

Secondary observational objectives that would be of considerable importance in understanding the very upper atmosphere and the injection mechanisms and energy balance of the ionosphere are (1) the density and temperature of neutral and ionic species, (2) the determination of the major constituents to within an accuracy of 20 percent or better, and (3) information about the energy spectra of the charged particles.

2. CHEMICAL COMPOSITION AND PHYSICAL STATE OF THE SATELLITES

The primary objectives in the study of the satellites are to determine their chemical composition and physical state and to identify the major processes on their surfaces and, inferentially, their interiors.

The Jovian satellites provide samples of the icy and rocky types of planetary bodies, and a primary objective is to study at least one icy satellite (probably J3 or J4) and one rocky satellite (possibly J1). The morphological characteristics are obviously of primary importance and may be achieved by imaging in the visible with a resolution of better than 1 km and with a coverage of at least 50 percent of the satellite surface.

Other remote-sensing systems to be carried out by satellite flybys are considered to be the major means of obtaining significant information on the composition and state of the satellite surfaces. The future use of penetrator probes may possibly alter this view. Some remote-sensing techniques appropriate for this purpose are under development and some have been used with great success on a limited basis in two Apollo missions. However, no experience exists in the Jovian system, where the radiation environment is severe and some of the satellite surfaces are covered with ice. Remote-sensing systems, using detectors in the x-ray, gamma-ray, infrared, and visible regions appear to be appropriate techniques for obtaining compositional and mineralogical information. However, there has not been adequate development of such systems, particularly where materials possibly rich in various ices may be present, so that a specific recommendation is not now possible. The status of the technology and its applicability in the Jovian radiation environment should be reappraised next year. These techniques are useful not only for the satellites of the outer planets but are of primary concern for the inner solar system, in particular Mercury, the moon, and Mars. This matter is discussed further in the section on the inner planets.

In the following discussion we outline *possible* sensing techniques related to the satellites of the outer planets. *Because of the urgent need for adequate methods of remote sensing for any further exploration of icy–rocky planetary bodies, we strongly recommend that a wider variety of techniques be sought and that research and development of appropriate methods be carried forward as expeditiously as possible.*

Reflectance spectroscopy is one of the remote-sensing techniques that has proven highly successful for the determination of some chemical elements and possibly the identification of major phases. Ground-based spectral measurements have demonstrated the power of this technique for iron and titanium on the moon and in the identification of ices and silicates on the Jovian satellites and water ice in the rings of Saturn. The measurement of reflectance spectra from the spacecraft in the wavelength range of from 1 to 5 μm with the spectral resolution of about 1 percent and a relative photometric precision of 1 percent should be adequate to identify possible variations in surface chemistry. These spectra should be obtained at a spatial resolution of better than 100 km from the spacecraft, and average spectra from the whole disk should be obtained from ground-based and LST observations. The basic instrument necessary for these measurements will also have many common characteristics with the high-resolution infrared spectrometer for the determination of atmospheric spectra and structure on a major planet and should have wide utility.

Measurement of the characteristic *x-ray spectrum* excited by solar photons can be used to determine major elements on the surface. This technique has been used for the moon on Apollo missions. The measurement may prove more difficult in the Jovian system because of the lower solar flux and because of possible interfering background generated by the intense radiation environment. It is quite possible that the Jovian radiation field may be an advantage because it could serve as a sufficiently strong excitation source for x-ray analysis. It is also possible that intense proton bombardment of the Jovian satellite would produce a sufficient neutron flux near the surface to provide a measurable neutron-induced prompt gamma-ray spectrum, which could be related to chemical composition. We recommend that these possibilities be studied in detail.

Orbital measurements of the characteristic *gamma radiation* from the natural radioactive species K, Th, and U have proven to be highly successful based on the results of the Apollo mission. The gamma-ray data are element-specific and are not obtainable from earth-based systems. With the improvement of detectors and with broader scale coverage, it should be possible to characterize some of the important elements in the outer meter layer of the Jovian satellites. It is possible that this approach can be extended to several elements that have radioactive isotopes induced by cosmic-ray or charged-particle bombardment. The problems of interference due to Jovian radiation should be assessed. In the case of rocky planets there is no doubt of the applicability of this technique, and some studies are necessary for icy–rocky bodies to assess the full versatility of remote gamma-ray analysis. The effectiveness of flyby gamma-ray measurements of the Galilean satellites requires a more complete evaluation with regard to areal resolution and sensitivity due to the short encounter times. The Jovian radiation environment may prove a serious hazard, and it may be necessary to carry out several satellite studies well outside the radiation belt.

3. MAGNETIC-FIELD AND ENERGETIC-PARTICLE FLUXES

The broad observational goals of planetary magnetospheric and plasma studies are to make measurements comprehensive in both space and time of the absolute energy spectra, angular distributions, and spatial distributions of the most abundant species of charged particles (electrons and protons) as well as ionic species of lesser abundance. The kinetic energy range of interest is from thermal energies (~1 eV) to hundreds of MeV. Complementary measurements of the vector magnetic field and of electrostatic and electromagnetic waves round out the body of observations, which are then subjected to overall interpretation. Such an interpretation is concerned with matters of the following nature:

1. Sources and sinks of ionized particles,
2. Acceleration and diffusion of particles,
3. Sources of energy and transport of energy through the system,

4. Coupling between the external magnetic field and the ionosphere of the planet,
5. Sweeping and accelerative effects of satellites,
6. Transient variations.

Two bodies of excellent observations on the magnetosphere of Jupiter have been obtained during the flybys of Pioneer 10 and 11. The two MJS flyby missions are expected to contribute additional detail.

The primary goal to achieve the next marked advance in study of the Jovian magnetosphere requires an orbiting spacecraft to yield comprehensive data as a function of latitude, longitude, radial distance, and local solar time over an extended period of time (one including a diversity of temporal fluctuations).

The most suitable orbit is one in the equatorial plane with periapse as close to the planet as the exposure to radiation (both instantaneous and cumulative) permits and with apoapse at a radial distance of ~ 150 radii of Jupiter. Such an orbit can be subjected to perturbations by the Galilean satellites in a planned way such that its line of apsides can rotate over a large range of local time (at least 180° or 12 hours) within about 3 years. Many near passes by satellites occur and are desirable to investigate local plasma phenomena.

(a) TECHNICAL REQUIREMENTS FOR PARTICLE AND FIELD MEASUREMENTS By virtue of extensive spaceflight experience, all the necessary instrumentation for outer-planet and outer-solar-system measurements of particles and fields is, broadly speaking, state of the art. The principal efforts that are required for any specific mission are concerned with (a) mechanical and electrical interfaces; (b) adaptation to the data-handling system; (c) radiation hardening; (d) magnetic cleanliness; (e) elimination of rf interference; (f) stability and reliability over necessary range of environmental conditions; and, of course, (g) physical calibration of all detectors.

Magnetometers The range of magnetic-field strengths in the outer solar system is now known to be from about 10^{-6} G in the interplanetary medium to 15 G in certain regions on the surface of Jupiter. Hence, the noise level and zero offsets of an appropriate magnetometer must be below 10^{-7} G (10^{-2} gamma), as should also be the stray field of the spacecraft as the position of the sensor. Some relaxation of the latter specification is possible if inflight determination of the stray field is feasible, but considerable effort on magnetic cleanliness of the basic spacecraft is required. A three-axis vector magnetometer is necessary. Desirable accuracy is of the order of 1 percent on each axis over a dynamic range of 10^7 in scalar magnitude. Such multirange instruments exist. Time resolutions of the order of 1 sec are desirable.

Plasma Analyzers Two different types of plasma instrument are required: one for measurements of the flow velocity, plasma density, ionic composition, and electron and ion temperatures in the solar wind and another for the measurement of magnetospheric plasmas. Electrostatic analyzers of suitable form exist for both types of measurement. The former is mounted to be approximately sun-pointed throughout the mission. High spectral resolution in the vicinity of the solar-wind flow velocity (~1 keV for protons)—roughly 20 energy channels—and angular distributions over an angle of about 20° from the spacecraft–sun line are required. Time resolutions of the order of a few seconds to a minute are desirable.

A magnetospheric plasma instrument must make comprehensive angular distributions and spectral measurements point by point along the trajectory with time resolutions per measurement of the order of 1 sec. The necessary energy range is from about 1 to 50,000 eV in roughly 20 energy channels.

Suitable instruments of both types exist. The principal unsolved problem is shielding from penetrating radiation in regions such as the inner magnetosphere of Jupiter.

Instruments for Magnetospheric Energetic Particles By convention among workers in this field, "energetic" means particle kinetic energies exceeding about 30 keV. In the Jovian magnetosphere, kinetic energies range as high as hundreds of MeV. The principal instrumental problems are maintaining linearity of response over an enormous dynamic range in intensity (about 10^{-2} to 10^{8} cm^{-2} sec^{-1} sr^{-1}) *and* in energy and in unambiguously identifying electrons, protons, alpha particles, and heavier ions in the presence of each other. Comprehensive angular distributions are of great importance, as are spectral measurements. Angular resolutions of the order of 10° (ideally over the entire unit sphere) are desirable and spectral resolution to $\Delta E/E \sim 1$ is needed. Upgraded versions of Pioneers 10 and 11 and MJS energetic-particle instruments cover this field well. The principal practical problem is to achieve adequate angular distributions without excessive dependence on mechanical scanning. This is usually achieved with a continuously rotating spacecraft. Time resolutions of the order of 1 sec are necessary for angular distribution determinations.

Instruments for Interplanetary Energetic Particles The particles involved are those emitted by the sun and the planets *and* the galactic cosmic radiation. The energy range of interest for the first class of particles extends from, say, 30 keV to tens of MeV. Angular distributions, crude spectra ($\Delta E/E \sim 1$), and unequivocal separation of electrons, protons, and heavier ions up to at least iron, with $\Delta Z \simeq 0.2$, are desirable. Time resolutions of the order of 1 min are adequate.

For the galactic cosmic radiation, the emphasis is on stability of absolute intensity calibrations, on crude energy spectra from, say, 1 to 500 MeV/nucleon, and on isotopic and elemental compositions ($\Delta Z \leqslant 0.2$) to as high an atomic number Z as practical. Suitable instruments exist. Time resolutions of the order of hours are adequate.

Plasma-Wave Instruments Radio receivers covering the frequency range 10 Hz to 40 MHz enjoy a high state of technical development in a form suitable for spacecraft use. The technical problems are principally suitable antennas that are compatible with spacecraft constraints and the reduction of radio-frequency interference by other spacecraft electronics. Prototypical solutions to these problems have been achieved in Helios, IMP, Pioneer, and Mariner spacecraft. Moderate spectral resolution $\Delta f/f \sim 0.2$ is desirable in the frequency range 100–15,000 Hz, and crude spectral resolutions elsewhere. The sensitivity, dynamic ranges, and noise levels of existing receivers are adequate.

B. Uranus Reconnaissance Objectives

In establishing the first-order objectives for a reconnaissance mission to Uranus, we have tried to identify a set of diversified observations that (1) would grossly characterize the planet and its satellites, (2) provide critical data necessary for further advances from earth-based instruments over a long time scale, and (3) would be sufficiently broadband to have the greatest possibility of discovering new and unexpected phenomena. Because of the long flight times, it is considered of great importance that spacecraft systems directed toward Uranus be utilized to obtain critical data at intermediate planetary encounters and of the interplanetary medium en route to the primary objective.

The *primary objectives* for reconnaissance of Uranus are, in order of priority:

1. Determining if there exists a planetary magnetic dipole field, its strength and orientation;
2. Measurement of J2 for Uranus and the masses of the major satellites;
3. Measurement of the gross morphologic structures of the planet and satellites;
4. Compositional information and vertical and horizontal atmospheric structure at good resolution for Uranus; and
5. Estimating the planetary heat source.

The first primary objective could be obtained by a single flyby of Uranus, which would yield definitive information on the magnitude, orientation, and offset of the magnetic dipole moment of the planet and the period of rotation of the interior of the planet. A possible set of instruments that could achieve this objective includes (a) a three-axis vector magnetometer covering the range of 10^{-6} to 10 G; (b) energetic particle detectors measuring electrons, protons, and heavier ions in several energy ranges upward of about 30 keV; (c) plasma analyzers for measuring the number, density, and temperature of the ambient plasma; (d) radio receivers for measuring the fluctuating electromagnetic fields, including the frequency range 1 Hz to 30 kHz. All of these instruments are within the state of the art and outlined in the previous section on Jupiter.

The second objective can readily be achieved by tracking and with appropriate encounter distances without the excessive demands of a close pass. Measurement of the shape of Uranus, the gross features of horizontal atmospheric structure, and the gross surface morphology of the satellites can be obtained with imaging systems. The nature of the atmospheric structure of Uranus is not known. An instrument with broad spectral response from 0.37 to 0.9 μm with sufficient sensitivity to allow narrow-band imagery in one or more selected wavelength regions at 100 Å resolution appears to provide the most reasonable means for finding such structure. Full disk coverage of Uranus with a resolution of about 200 km in the spectral range that may reveal significant features is fully adequate. For the satellites, it is necessary to obtain images near the peak of the solar flux and of sufficient spatial resolution to identify broad morphological features. Full disk coverage for at least one major satellite with a resolution of better than 1 km is necessary.

Information on atmospheric thermal structure and composition at very low spatial resolution is obtainable from the earth using aircraft and earth-orbiting platforms. A Uranus flyby should provide the opportunity to obtain basic data for vertical profiles of temperature and composition at much higher spatial resolution and on the dark side. Measurements over the range of 2 to 250 μm with spectral resolution of 1 percent and with special attention to the region 10 to 250 μm are required to determine the variations in outgoing flux and basic data on temperature distribution between 0.1 and 1 bar. Photometric and polarization measurements in the visible and the near infrared between 0.4 and 0.9 μm would provide data of sufficient spectral and spatial resolution and photometric precision to define spatial variations in scattering properties. Measurements of the visible, near ir, and ir should cover most of the planet at a resolution of at least 30 resolution elements per diameter. Coverage of the dark side with the infrared measurements is particularly important. A significant fraction of the planet—on the order of 10 percent—should be covered with a resolution of at least 200 resolution elements per diameter.

The last of the primary objectives is to define or place limits on the magnitude of the planetary heat source. Measurement of this quantity, even with modest precision, would be valuable. It will be necessary to define the phase function for the interaction of Uranus with sunlight in the visible and to measure the thermal emission in the range of 25 to 250 μm. The infrared and visible photometric observations may be capable of providing the required data. An effort to achieve an absolute accuracy of about 10 percent for the total emission and the phase function should be made.

The following are *secondary objectives,* which need not all be achieved in the first reconnaissance mission. (1) A more complete set of magnetospheric and plasma studies is an extremely important secondary objective for the Uranian system. The unique orientation of the rotational axis (presumed magnetic axis) provides an opportunity to study the interaction with the solar wind with an exceptional geometry. During 1985, its rotational axis will lie at an angle of about 8° to the planet–sun line and hence to the solar-wind flow vector. This situation recurs at 42-year intervals and would be the most interesting orientation for study. (2) Definition of the radii of the satellites. This is achievable by use of adequate imaging systems. If at all possible, the observation should permit density determinations to at least 10 percent for at least three satellites. (3) Gross composition of the satellite surfaces is characterized by the spatial major elemental abundances, major minerals, and frost species. If carried out with a spatial resolution significantly greater than attainable with the LST, these measurements would give important information about areal distribution of the satellite composition and environment. Infrared measurements are especially appropriate. (4) To obtain information on temperatures at pressures greater than 1 bar would require microwave measurements or high spectral resolution measurements of the weak methane bands in the visible. These are highly desirable secondary objectives of the Uranus mission.

VII. SATURN OBJECTIVES

After the flybys of Saturn by the Pioneer 11 and MJS spacecraft the reconnaissance phase of Saturn investigation will have been completed. This phase should raise questions of profound scientific significance about the Saturn system. It will be the task of follow-on missions to investigate these questions. We anticipate that these missions will have the following principal goals, in order of importance:

1. Intensive investigation of the atmosphere of Saturn. Of first importance here are *in situ* measurements of the chemical composition made with an atmospheric entry probe (see Jupiter objectives).
2. Determination of regional surface chemistry and properties of the surface features of satellites and properties of ring particles. These require remote-sensing measurements (see Jupiter objectives) from a Saturn-orbiting spacecraft and would exploit the earlier information returned from flyby spacecraft.
3. Intensive investigation of Titan. This satellite is a fascinating planetary body for which detailed further planning cannot be carried out until the atmosphere is better characterized. It is clear that an atmospheric entry probe will be required; whether this can also be a surface lander will depend on whether that surface is solid or liquid. In addition to determining the major chemical species in the atmosphere and on the surface of this planet, attempts should be made to detect higher-molecular-weight carbon compounds as well as to determine the chemical state of nitrogen in the atmosphere and surface of Titan. Such observations will have a major effect on our understanding of prebiotic chemistry in the early solar system.
4. Atmospheric dynamics and structure that will require the synoptic coverage of an orbiting mission.

In general, the accuracy with which physical measurements can be made depends on instrumental limitations, and Saturn is sufficiently like Jupiter that the same quantitative goals for accuracy in entry probe and remote-sensing instruments apply to Saturn as to Jupiter.

The comparative planetology of the satellites will also be of great interest. Titan has a mass larger than the moon, comparable with those of the Galilean satellites, and a radius larger than that of Mercury, but it has a much greater quantity of volatile gases. The other regular Saturnian satellites are smaller in diameter by one order of magnitude and in mass by three orders of magnitude compared with the Galilean satellites. Thus, these regular satellites of Saturn are probably less evolved in a planetological sense and therefore better preserve the record of the events responsible for their formation. The rings of Saturn are potential treasure-houses of information bearing not only on the formation of the Saturnian system but also on the properties of any extensive disk of gas and dust from which has formed a system of bodies in roughly circular orbits around a massive primary.

For these reasons, we consider it essential that NASA *be in an adequate posture to initiate exploration of the Saturnian system in the middle 1980's following the reconnaissance of this system.*

VIII. SCIENTIFIC INSTRUMENT DEVELOPMENT

The outstanding success of the U.S. space-science program is the result of the close cooperation between NASA and the scientific community in developing useful objectives and the appropriate experimental protocol to achieve these goals. However, the evolution of the planetary-sciences program from reconnaissance to exploration and intensive studies will require the development of a new generation of experiments. These experiments must result from new and innovative approaches by capable people in university laboratories, industrial research groups, and in federal centers. Such efforts will require conceptually new approaches and laboratory development, which must be financially supported. The total level of support in fiscal year 1976 for the development of new experiments and instruments not directly related to flight programs is $2.3 million. It is clear from both the current instrumental needs and the level of fiscal support that the development of scientific experiments has not kept pace with advances in spacecraft development and flight capabilities. *We recommend that basic research on the development of new instrumental capabilities be carried forward on a broad front and in a vigorous manner, consistent with the overall strategy for planetary exploration but without overriding regard for specific missions.*

IX. POSTFLIGHT DATA ANALYSIS

Mission investigations are usually supported for a short time beyond the actual termination of the flight. In most cases this is only enough for preliminary data reduction by the experiment team. *The actual time required to carry out adequately data reduction and analysis frequently extends for a significant period beyond the flight mission. Members of the more general scientific community as well as the experiment team should be involved in the study and data analysis.* In most cases there is no mechanism by which data analysis can be supported if it extends beyond the usual contract period for data reduction. Many missions are now returning extremely important data that will require sustained analysis for periods of up to a few years after the termination of flight.

Important scientific studies have been carried out on data and samples returned by the Apollo missions well after termination of the Apollo flights. In general, this program has been highly successful and has enormously increased the scientific returns of the Apollo missions.

Mariner 10 is an outstanding example of the high scientific return that could be obtained by additional operations, data reduction, and analysis after completion of the initial mission objectives. However, the level of support for these functions is exceedingly low. The average annual allocation for postmission data-reduction analysis for Mariner 9 and 10 was only $1.1 million for the period fiscal year 1973 to fiscal year 1976. *We recommend that sufficient support be provided to complete the analysis of Mariner 9 and 10 data, and that additional operations, data reductions, and analysis of Pioneer 10 and 11 be supported to exploit their scientific value adequately. We further recommend that future missions contain as part of the original mission costs sufficient funds for data reduction and analysis beyond the flight mission stage in order to ensure that the scientific return is reasonably exploited.*

X. THE BACKWARD VIEW FROM 1985

It is of interest to consider what the anticipated increment of knowledge will be from the program outlined for the period 1975–1985. When viewed from 1985, we would most probably point to the following.

Two of the major planets, Saturn and Uranus, will have been encountered, and the reconnaissance of the planets will be extended from 5 to 19 AU. Excellent photographic coverage of the satellite systems and rings of Saturn will exist showing a diversity of strange and wonderful objects. We will have the first high-resolution view of Uranus along with some of its satellites, and the rotational axis and period of Uranus will be established.

The magnetic fields and radiation belts of Saturn and Uranus will be known, and the relation of the field axis to the rotation determined. The planetary interaction with the solar wind will be understood and the behavior of the interplanetary field sketched out. The boundary between the solar system and the interstellar medium will be approached with spacecraft.

The cloud structure of Saturn will be well established and cloud patterns of Uranus detected. The approximate composition of the planets Saturn and Uranus will be known, estimates of their satellite composition obtained, and the satellites roughly classified.

The composition of Jupiter will be established and the relationship to the sun and terrestrial planets fixed. The heterogeneity of the main reservoirs of original solar-system material will be known. The heat balance of Jupiter will be basically understood and the overall internal structure outlined. The nature of the clouds will be reasonably well established. Spacecraft will have survived the radiation belt hazards, and the magnetosphere will be described in considerable depth. The interaction of the magnetosphere with satellites will be described and understood. The sources and mechanisms of particle acceleration will be thoroughly studied and basically understood. The nature of the connection between magnetospheric disturbances and radio emission and, further, with stellar sources will be established.

The general composition of Jovian satellites will be established and their surface morphology revealed. Evidence of exciting and confusing processes will be found because of the strange nature of icy–rocky planets. The Jovian satellites will be classified into captured objects and primary objects that coalesced in Jupiter orbit. Discussion of the sources for the captured satellites will be a joy to hear, and comparisons with the formation of the solar system will be popular and it is hoped profound.

The morphological observations on these outer planets and their satellites will have generated an enormous number of working hypotheses and a great deal of excitement about the origin of the solar system. A broad view of the solar system as revealed by the major planetary types and their connection to the mass reservoir of the sun will then exist. The minuscule role of the earth in the system will be incredibly clear. The hostility of the outside environment will be abundantly evident and the grandiosity of the universe beautiful and cold. The results from a scientifically based comparative planetology will lead to a more realistic assessment of man's place in the universe; studies of comparative planetary characteristics and processes in the atmospheres, surfaces, and interiors of the planets will provide us with realistic information concerning the nonbiological processes that form and drive the atmospheres. Such information will be crucial for an ultimate understanding of the highly distinctive planet earth.

5
Inner Solar System

We have evaluated the current level of knowledge of the inner solar system that has been acquired over the past decade. In terms of the overall strategy outlined in this report, it is clear that the missions to the moon, Mars, Mercury, and Venus have all satisfied the first basic step of reconnaissance. The Apollo missions have extended our understanding of the moon to intensive study, and the Viking mission will inaugurate the intensive study of Mars. The Pioneer Venus mission, which is currently under development, should begin the first stage of exploration of that planet. *It is apparent that the reconnaissance mode of study of the inner solar system is completed and that a new generation of exploratory studies is just beginning.* The purpose of an exploratory program is to attempt the systematic discovery and understanding of the processes, history, and evolution of a planet on a global scale. In this sense a program of planetary exploration will require a distinctly different observational tool than was utilized for the reconnaissance missions. With the exception of the moon and the earth, the chemical and structural state of the inner planets is almost totally unknown. The processes that govern their evolution are only guessed at. These characteristics are not determinable from earth-based observational systems and require the use of properly instrumented spacecraft. The general type of remote-sensing instrumentation necessary to accomplish these goals has been, in part, identified and when utilized with a spacecraft in polar orbit around a planet should provide full global coverage of the physical and chemical state of the planetary surface and identify the major structural provinces and degree of evolution. The limited Apollo coverage of the moon by gamma-ray, x-ray, magnetic, and gravity measurements provides a tantalizing peek at the wide range of new information that planetary orbiters can obtain from Mars, Mercury, the moon, and outer planet satellites. *It is the committee's view that exploration of the inner solar system is one of the principal components of planetary exploration.* A specific strategy for inner-solar-system exploration has not been prepared for this report because of the desirability of awaiting the observations of the Viking mission to Mars, and because the major effort of COMPLEX in 1975 has been devoted to the strategy for the outer solar system. From the basic strategy, unless there is a positive indication of life on Mars, it is clear that efforts to explore either Mars, Mercury, or the moon by orbiting spacecraft with adequate remote-sensing instruments in the next three years is the next step in exploring the inner solar system. *The committee recommends that a significant effort begin toward this end, and that the study and development of appropriate remote-sensing devices be initiated at the earliest possible time for the purpose of determining lateral variation in chemical and mineral composition, gravity and magnetic fields correlated with planetary structure, and the measurement of local and global planetary heat flow.* It was the considered view of the committee that global exploration was a necessary objective for planetary study. While in some cases (the moon, Mars) intensive site studies have already been carried out or are under way, it was considered most important that a broad exploratory planetary view be obtained before continuing further toward overly specialized goals.

In addition to the use of global exploration, the committee also considered the role of hard landers in order to obtain fundamental planetary data. The concept of survivable hard landers had considerable appeal and was considered to have application on a wide variety of hard planets, including the satellites of the outer planets. These missiles may permit wide planetary coverage at low cost. The basic role was considered to be similar to the entry probes for the major planets, but with the use of a larger number of probes. The general technology appears to exist, and *the committee recommends that a strong effort be made toward assessing and developing possible science instruments that could be used to exploit hard landers as a technique in planetary exploration.*

6
Status of Existing Programs

I. VIKING

At present, both Viking spacecraft have been successfully launched and are en route. With regard to the science systems, close attention needs to be paid to the following potential problem areas:

1. There is uncertainty as to the mixing ratio of argon for Mars. If the concentration is as high as the current upper limit estimate, based on the results of the Soviet Mars 5 mission and evaluations of spectroscopic data, atmospheric analysis by the gas chromatograph–mass spectrometer (GCMS) could prevent subsequent organic analysis of soil samples by incapacitating the ion pump. On the other hand, if atmospheric analyses are done subsequent to soil-sample analysis, contamination will substantially reduce the sensitivity of the atmospheric analysis. The current plan is to delay the first atmospheric analysis by the GCMS until after all soil analyses are completed, unless other Viking measurements give a clear indication that the argon content is sufficiently low that the GCMS functions will not be jeopardized. We wish to emphasize that one of the first-order scientific objectives is the precise measurement of isotope atmospheric composition. Determination of nitrogen and the isotopic compositions of the rare gases argon and neon are of particular importance. We urge, consistent with the integrity of

one complete soil sample analysis by the first lander, that every effort be made to obtain precise measurements of atmospheric composition and, further, that the optimum tactics for obtaining these two critical measurements be fully evaluated in regard to the procedures to be employed on the second lander.

2. Some problems with on-board experiments have been identified since the launches. These include a possible Freon contamination of the GCMS system of one vehicle and a compromise in the meteorology experiment that may limit the accuracy of temperature measurements to 10°C. *We recommend that efforts be made to evaluate, with the inflight instruments, the impact of such problems on the planned experiments and that a parallel effort by the Viking scientific team employing the backup system be conducted to optimize the scientific return as these compromises are evaluated.*

3. The present level of understanding of the performance of the orbiter instruments appears adequate. However, the lander represents a radical departure from previous spacecraft experience, and, in our view, adequate understanding of the performance of the lander instruments and their expected operating environments does not in all cases exist. *We recommend an increase in effort sufficient to ensure that all lander instrument teams and, in particular, the biology teams will have the opportunity to understand fully the performance of their instruments in their actual operating environment.* It is anticipated that such an effort will probably require further ground testing and additional acquisition of inflight data beyond that currently planned.

II. PIONEER VENUS

The committee has reviewed the current status of the Pioneer Venus multiprobe and orbiter missions in some detail. No unusually severe problems were identified; those problems that exist appear normal for the current stage of mission development. We were once again impressed with the large scientific capability afforded at relatively low cost. This capability has resulted from many years of careful planning and study by a large number of scientists and is achieved at the cost of considerable interdependence among the various instruments on both spacecraft. For this reason, we are concerned over the potentially significant impact on the scientific capability that even a small decrease in the current scope of the mission might cause.

The Pioneer Venus mission represents a low-cost approach to the closeup and *in situ* investigation of a planet, which we believe merits support in the face of continuing severe fiscal constraint.

III. MARINER JUPITER SATURN

The committee has reviewed the status of the Mariner Jupiter Saturn mission and is impressed by the capability of the spacecraft and the scientific management of the mission. The chosen scientific investigations address important scientific issues, and the instrumentation appears to be fully compatible with the major objectives. Serious attention is being given to the problem of the radiation environment of Jupiter by both the engineering and scientific staffs. The science teams are knowledgeable about their instruments and are intimately involved with the scientific instrumentation and planning of mission profiles. They also appear to be aware of systems requirements and interact positively with the engineering staff. We cannot identify any severe problems. Those problems that exist are normal for the current stage of mission development, except for the matter of radiation hardening to which full attention is being given.

This mission will provide the first fully instrumented reconnaissance of Saturn and answer important questions about several of the Jovian and Saturnian satellites and the Saturn ring system. We strongly endorse this mission as an essential part of the ongoing reconnaissance and exploration of the outer solar system.

IV. PIONEERS 10 AND 11

Both Pioneers 10 and 11 are now in their extended mission phases. The two spacecraft and most of the experiments are functioning well. Data are being acquired at the maximum feasible bit rate on either the 28-m or 64-m antenna of the Deep Space Network on a duty cycle exceeding 60 percent of real time. This coverage is satisfactory to experimenters.

Because of the uniquely valuable role of these two missions in the far outer solar system, it is important that coverage be continued as long as the power and communication systems permit. *We recommend that the coverage on Pioneer 11 be restored to a sufficient level by mid-1979 in order to optimize the science return at the Saturn encounter.*

V. THE LUNAR PROGRAM

A. Lunar Programs Office

The Lunar Programs Office is charged with the continuing exploration of the moon through the analysis and interpretation of lunar data obtained from manned and automated spaceflights, the acquisition of new data, and planning for future lunar missions. The Office administers programs for lunar sample research, lunar synthesis (including the continuing of ALSEP operation and data reduction and supporting research and technology), cartography (including laser ranging), and advanced programs and technology. The Program Office obtains advice on science policy through a science advisory committee and conducts peer review of lunar science proposals through the Science Review Panel (SRP) of the Lunar Science Institute. This science management system is the only one of its kind operating within NASA. The general quality of research carried out within this program is high, and there is a healthy commitment to the program on the part of the scientific community. Many of the observations and techniques used in lunar research have direct applicability to the general aspects of planetary sciences. The committee would enthusiastically support efforts to increase the transfer of information from within the lunar program to the broader problems in planetary sciences.

B. Future Missions

The committee was informed that there have been studies conducted on mission designs and concepts for future lunar exploration, and it received a detailed briefing on the major effort directed toward planning a Lunar Polar Orbiter mission for obtaining global coverage of lunar physical and chemical properties.

C. Lunar Sample Curatorial Facility

The Apollo lunar samples are a unique scientific treasure, being man's first samples returned from another planet. In the six years since the first samples were returned, over 200 scientific research groups have contributed to a remarkable variety of studies of the lunar materials, bearing on such diverse problems as the condensation/accretion temperatures of the moon, the degree of radial differentiation, the history of the lunar surface, and the history of the solar wind. These materials are distinctive in chemical composition and formed in a water-free environment. Many of them are saturated with solar wind and have extremely delicate surfaces. At present, these samples are relatively uncontaminated by terrestrial materials. Given the great variety and size of the sample collection and the important solar-system history that they represent, these materials will be the object of intensive study for years to come. In this context, it is the function of the Office of Lunar Sample Curator at the Johnson Space Center to provide the services and facilities required for the preservation of lunar samples for future generations and the preparation, distribution, and cataloguing of the samples for ongoing scientific investigations. The present facility, which was established during Apollo mission operations, has been found to have serious functional drawbacks and to present major long- and short-term hazards to the collection. The upgrading of this facility is now under consideration by NASA.

We strongly support plans (1) to provide an upgraded main curatorial facility for the Apollo lunar samples and (2) to provide a separate clean and secure storage area for some of the lunar samples at another site. Continued curatorial operations under present conditions expose the samples to unacceptable high levels of risk; the facility is inadequate for many mandatory functions of the Curator.

We therefore emphasize the imperative need to proceed with the separate storage plan and to carry out the upgrading of the main curatorial facility as soon as possible. *We strongly recommend that NASA complete its efforts to adequately store and process lunar samples in a pristine environment and that this be carried out with the participation and evaluation of knowledgeable members of the scientific community.*

III
Working Papers

Introduction

The following documents are reports to the Space Science Board from its disciplinary committees: Committee on Space Physics, Committee on Space Biology and Medicine, Panel on Exobiology, and Committee on Space Astronomy and Astrophysics.

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. As noted previously in the text, the report of the Committee on Planetary and Lunar Exploration was adopted *in toto* by the Board and is Part II of this report.

The Space Science Board is deeply indebted to its disciplinary committees and panels and particularly to the Chairmen, Robert Helliwell, Sheldon Wolff, Peter Mazur, Peter Meyer, and Gerald Wasserburg, for the efforts 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 their 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.

Two special studies, requested by NASA, were conducted during the summer of 1975 at Snowmass, Colorado. The first of these was devoted to infrared and submillimeter astronomy and was chaired by Gerry Neugebauer. The second, chaired by Eugene Parker, was devoted to solar physics. These studies were conducted immediately preceding, and therefore constituted an input to, the final deliberations of the Committee on Space Astronomy and Astrophysics, and through that committee to the Board. The reports of these studies are included here as working papers, but they have been written so that they could stand alone as separate reports. The Board is deeply indebted to these study groups and especially to their chairmen, Eugene Parker and Gerry Neugebauer.

Committee on Space Physics

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James G. Anderson
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A
Space Physics

I. INTRODUCTION

The Committee on Space Physics submits the following summary report to the Space Science Board. It gives the results of the Committee's review of its 1974 report to the Board (see *Opportunities and Choices in Space Science, 1974,* National Academy of Sciences, Washington, D.C., 1975, p. 77). Although that report continues to represent the current views of the Committee, this review introduces a change in emphasis, as discussed below. The recommended mission model reflects this change in emphasis as well as changes in the fiscal constraints.

II. DISCUSSION

In the area of plasmas, the Committee sees an increased role for planetary missions. The interplay between particles and fields that is observed in plasmas encompasses physical processes that are still poorly understood. Yet, these processes are of fundamental importance for the evolution and behavior of cosmic systems. What is needed, therefore, is a coordinated study of a variety of magnetospheres and other magnetic structures such as the interplanetary medium. The earth's magnetosphere plays a central role in this strategy because it is the most accessible natural plasma in space. Other quite different examples of magnetospheres are found at Jupiter and Mercury. *We see, during the next decade, an evolution in magnetospheric research proceeding from our present experiments, through the major effort of the International Magnetospheric Study (IMS), and culminating in programs that use coordinated experiments and systematic planetary investigations to achieve a broad, global view of the behavior of cosmic plasmas and fields.*

III. MISSION MODEL

To achieve the scientific objectives of space physics through the mid-1980's the Committee *recommends* the program shown in Table A.1 for new missions.

A key element of the terrestrial part of the space-physics program is the Electrodynamics Explorer mission. Although the mission model

TABLE A.1 Recommended New Missions

	Start Dates	Launch Dates	Total Costs (in $Millions)
New Starts in Terrestrial Space Physics			
Electrodynamics Explorer[a]	1977	1979	60[b]
Spacelab[c]	1977	1981+	100[b,d]
Atmosphere Explorers	1978	1980	50[b]
Magnetospheric Multiprobes	1982	1985	60[b]
Solar Wind Explorer	1980	1982	15[b]
Scout Explorers[e]	1977+	1978+	50[b]
Relativity Experiment[f]	—	—	—
New Starts in Extraterrestrial Space Physics[g]			
Jupiter Magnetospheric Orbiter	1977	1981	175[h]
Exploratory Mission to Uranus	1977	1979	175[h]
Mars Aeronomy Orbiter	1979	1981	170[h]
Out-of-the-Ecliptic Mission	1978	1980	25[h]
Mercury Orbiter	1981	1983	135[h]

[a] Also known as Electrodynamics Satellites. This mission consists of three spacecraft.
[b] Costs are rough estimates and do not include launch costs.
[c] The AMPS (Atmospheres, Magnetospheres, and Plasmas-in-Space) Mission, as presently conceived, addresses the objectives of a Spacelab mission.
[d] Costs represent the initial development and first few flights of the mission. Ongoing costs are estimated to be approximately $20 million per year.
[e] Five missions (launched during the period 1978–mid-1980's) costing approximately $10 million each.
[f] Recommendation delayed until program has been reviewed.
[g] The Committee on Space Physics has considered only the space-physics aspects of these planetary missions and thus lists related mission data only to show possible ways to accomplish the necessary objectives.
[h] These costs are part of the planetary program.

recommends three spacecraft, this number could be reduced to two without jeopardizing the basic objectives of the mission. Both would be polar-orbiting satellites, one at high altitude and one at low altitude. The other key element in the terrestrial program is a Spacelab mission (e.g., AMPS) with a subsatellite capability. For extraterrestrial research the key element is a Jupiter orbiter designed to study the Jovian magnetosphere.

Committee on Space Biology and Medicine

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B
Space Biology

The goals for space biology in the next decade are to establish and maintain a balanced program in those areas where the space environment or space technology can shed light on biological questions. The program should consist of

1. The study of exobiology, defined as the search for extraterrestrial life as well as measurement of the conditions to be found beyond the earth, which are prerequisites to the origin and support of life. The study of life, which has arisen and evolved completely independently from that on earth, will have a profound influence on our understanding of the nature of life itself.
2. The use of NASA's remote-sensing capabilities to monitor the earth's global ecology. Interaction of biota with such factors as global changes in atmospheric composition and temperature constitutes both a fundamental biological problem and a problem of immense direct concern to mankind. The remote-sensing capabilities can also be used to study problems in animal orientation and navigation. It is unknown how animals can navigate with pinpoint accuracy over thousands of kilometers.
3. The use of the zero-g environment to study problems of developmental and functional biology that can only be approached under these conditions. For example, the development and function of the vestibular apparatus in animals is presumably gravity-dependent. In

manned biological space laboratories we will now have the capabilities to study, in a gravity-free environment, the development and function of those biological systems that depend on gravity.

4. Positive actions by NASA to generate critical experiments and hypotheses on gravitational biology from the biological community at large. These actions should include support for ground-based research as well as potential flight experiments. A procedure for critical reviews should also be included.

5. The maintenance of a program in space medicine to ensure the safety of the astronauts. Such a program should go beyond simple empirical medicine and aim at gaining an understanding of the underlying physiological problems.

I. EXOBIOLOGY

The probability of the existence and detection of extraterrestrial life in the solar system is not high, but its detection would have a major impact on biology. Consequently, exobiology continues to claim a high priority. Critical to future strategy is the outcome of the biology experiment and the gas chromatograph–mass spectrometer on the 1975 Viking landers. These strategic considerations are discussed in the report of the Committee's Exobiology Panel, which follows.

II. REMOTE SENSING

The greatest contributions of satellites to biology in the short run may well come from their use in the study of the earth. The great variety of sensors and analytical techniques that have been developed enable continuous, or at least periodic, monitoring on a global scale. This methodology has been demonstrated to be useful for measurements related to the phenology of vegetation; to stresses in natural plant communities and cultivated crops; and to atmospheric, climatological, hydrological, and geological phenomena. The capability to obtain detailed data at regular intervals over extensive areas independent of a geopolitical framework offers tremendous opportunities for improving our ecological understanding of the globe.

Another area of interest to biology concerns the possibility of using radiotelemetry for the study of animal behavior, including the mechanisms involved in animal navigation, migration, and response to environmental conditions. Sensors of three types can be used to obtain important information: those that fix geographic position, those that characterize climatic parameters, and those that monitor physiological parameters of the animal.

III. GRAVITATIONAL EFFECTS

One unique characteristic offered by space is the absence of gravitational effects. Our current understanding of these effects is incomplete. For example, a fundamental property of most multicellular biological organisms is their capacity to orient themselves with respect to the gravity vector. The mechanism by which this capacity arises during embryological development and the method by which it operates in the mature animal is difficult or impossible to study under our constant 1-*g* acceleration. For one of the most important graviceptors, the otolith, the weightless environment provides a unique opportunity to study its development, enervation, and function. The importance of the questions warrants the study of otolith development and associated animal orientation behavior in combined flight and ground-based experimental programs. Specific areas for emphasis include the influence of weightlessness on structure and behavior as a function of flight duration and animal maturity for different species and the investigation of the use of visual information for orientation as a substitute for inadequate otolith information. Furthermore, possible interactions among fluid shifts, calcium loss, and vestibular function in the etiology of motion sickness should be explored.

In other areas, the relevant experiments to be performed in space cannot yet be clearly defined. Although previous summer studies have outlined general areas of potential biological significance, we believe that clearly defined specific experiments and hypotheses of fundamental biological significance *must be generated by the biological community itself*. We, therefore, applaud NASA's ongoing efforts to publicize the availability of Spacelab facilities and to encourage the submission of innovative questions and approaches applicable to Spacelab experimentation. However, solicitations for potential flight experiments should be supplemented with support for ground-based experimentation and workshops pertinent to questions of gravitational biology. An important adjunct to the solicitation of proposals is that they be subject to critical appraisal by appropriate review panels.

Until fundamental biological questions are formulated, a secondary priority should be accorded to the search for gravity-related phenomena in space. Decisions on the extent to which Spacelab needs to be configured to biological and medical experimentation, and the details of that configuration, should await a further definition of the biological problems.

IV. SPACE MEDICINE

A. Bone Formation and Demineralization

There remain important basic questions in bone formation to be answered in addition to the evident need to protect against the effects of continued calcium loss in man during long-term spaceflights. The role of gravity in stressing bone and its role in bone growth and decalcification can, to a certain extent, be investigated in bed-rest studies. Flight experiments, using both animal and human subjects, will probably be needed to check the validity of the simulation of weightless effects, as well as to evaluate any proposed countermeasures.

B. Cardiovascular, Renal, and Hematological Effects

The Skylab results went far toward explaining the complex cardiovascular, renal, fluid shift, and hematological changes associated with entry into orbit and prolonged weightlessness. To the extent that these phenomena are examples of the system's more general adaptation to environmental change, they are deserving of further basic physiological research.

C. Radiation Effects

The space environment contains atoms of high energy and high atomic number (HZE particles) that cause biological effects such as cell death. These particles upon striking the eyes cause light flashes. Since such particles exist in the space environment, their effect on nonrenewable cell systems such as found in the brain should be studied. *Such studies, however, should be carried out on the ground in accelerator facilities where the dose can be localized and measured.*

Exobiology Panel

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C
Exobiology

The Exobiology Panel continues to endorse the exobiological strategies submitted to the Space Science Board in September 1974 by the Panel and by the Future Exploration of Mars Study and published in part in *Opportunities and Choices in Space Science, 1974*. These strategy statements encompassed three areas: (1) post-Viking biological investigations of Mars based on Viking '75 outcomes; (2) exobiology strategy for solar-system objects other than Mars; and (3) Mars sample return. We restrict our present comments to (1).

At present, 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.

The chief criteria for judging "positive" and "negative" will be (a) the results of the three metabolic experiments, (b) the detection of nitrogen in the atmosphere, (c) the detection of complex organic molecules (especially pyrolytic products of amino acids) in the soil, (d) the results of the imaging experiment, and (e) consistency in the data from the two landers. Unfortunately, even if life exists on Mars, we do not know its biochemical and physiological characteristics. This, combined with the complexity of the instrumentation and the stringent limitations placed on the number of experimental parameters and the number and types of transducers, makes the likelihood of unequivocal results rather small.

Ambiguity in the data is likely to lead to a major controversy over the interpretation and significance of the results. To reduce this possibility, the Viking Biology Team has initiated experiments on Test Standard Modules (TSM) to aid in establishing in advance of the landing sets of standards for interpreting the data and criteria for distinguishing between biologically significant results and potential instrumental artifacts. We *recommend* that NASA provide the Biology Team with sufficient funds, technical assistance, and facilities to prevent these factors from imposing limits on the performance of the necessary tests. Even with this support, the limiting factor between now and the landing will be time itself.

Despite these efforts at setting baseline criteria, the interpretation of the Viking data is likely to remain equivocal, controversial, and perhaps contentious. To reduce the possible contentiousness, the Team plans to engage in a limited dialogue with the scientific community via workshops and published articles prior to the landing. We endorse this effort.

Whatever the nature of the Viking biology data, conclusions as to whether they represent positive results or negative results are almost certain to require extensive ground-based model experiments in flight-configuration instruments, in TSM's, and in orthodox laboratories. We doubt seriously that the necessary experimentation can be completed by the March 1977 termination date for Viking and, therefore, strongly urge the funding of a Viking Extension. This essential aspect of the Extension is supplemental to its value in permitting repeat experiments and altered experimental protocols on the Lander itself.

As mentioned in the introduction, post-Viking strategy will depend importantly on the outcome of Viking '75. We summarize here the major recommendations of the 1974 report, which we continue to endorse:

(a) *Positive Results.* For all positive outcomes, we *recommend* a follow-on Viking-type mission designed to support and optimize an eventual sample return from Mars. In the event that funding considerations preclude the former, we would continue to support the latter, preceded by a geochemical orbiter and penetrators to aid in site selection.

(b) *Negative Results*. If the Viking outcome is negative, we *recommend* a moratorium on further life-detection and life-characterization packages. We would continue to support investigations designed to throw light on the chemical evolution of Mars and on the question of the existence of past life on the planet.

(c) *Ambiguous Results*. The recommended strategy will depend on the nature of the uncertainty. Some possibilities were outlined in the 1974 report. The return of a Martian soil sample to earth offers the most promise of resolving ambiguities; however, we do not think that this promise by itself provides justification for a sample-return mission.

Committee on Space Astronomy and Astrophysics

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D
Space Astronomy and Astrophysics

I. INTRODUCTION AND SUMMARY

In 1974, the Space Science Board of the National Research Council made an overall assessment of NASA's space-science program as contained in the document *Opportunities and Choices in Space Science, 1974* (National Academy of Sciences, Washington, D.C., 1975). As part of its yearly work, the Committee on Space Astronomy and Astrophysics has reviewed scientific and technical progress in astronomy and NASA's progress in implementing the space-astronomy program. This report on its findings is within guidelines issued by the Space Science Board.

In the past year, there have been several activities that have clarified and extended various elements of the space-astronomy program. These include

1. Publication of *Interim Report of the Astronomy Spacelab Payloads Study* (NASA, Goddard Space Flight Center, Greenbelt, Maryland, 1975), which contains payload descriptions, engineering considerations, mission analysis, and cost information.
2. Phase B studies of the Solar Maximum Mission and Gamma-Ray Explorer; Phase A study of the Infrared Survey Explorer (by Goddard Space Flight Center).
3. Phase A study of the cryogenically cooled Shuttle Infrared Telescope Facility (by Ames Research Center).

4. Continuing study and refinement of the Large Space Telescope (by Marshall Space Flight Center).
5. Initiation of Phase A studies by Marshall Space Flight Center of the 1.2-m x-ray telescope, the Large Area Moderate Resolution x-ray telescope (LAMAR), and the free-flyer High Energy Astronomy payloads (HEAO-Block II).
6. National Research Council-sponsored summer studies by specially convened panels of scientists in the areas of infrared astronomy and solar physics (see Appendixes E and F).

Our major recommendations (see Section IV) for fiscal year 1977 are

1. Of highest priority is a new start for the Large Space Telescope (LST).
2. A new start for the Solar Maximum Mission (SMM).
3. A new start for the Spacelab Payload Development program as part of the Spacelab portion of the Shuttle program.
4. Initiation of the Gamma-Ray Explorer (GRE) and the Infrared Survey Explorer (IRSE) within the ongoing Explorer program.
5. Increased funding for supporting research and technology, sounding rockets, balloons, airplane programs, data analysis, and theoretical studies.

These recommendations reiterate the ones made last year and are made only after the most serious study of the scientific questions and in consideration of the severe fiscal constraints. Their implementation will ensure the vitality of space astronomy within the NASA program. Together with the long-term strategy, these recommended missions constitute a program that can significantly add to mankind's knowledge of the universe in the broadest sense. They would exploit the new discoveries of the past decades and address fundamental questions that have challenged scientists for centuries.

These recommendations take particular advantage of the unique capability of the Space Shuttle. The LST and SMM are long-term space observatories made possible by the capability of the Shuttle for recovery and servicing. The Spacelab payload development is oriented toward realizing the potential of economical orbiting of a variety of individual experiments that formerly would have required separate rocket or launch vehicles.

In this report, we have not attempted to duplicate the large amount of material contained in the 1974 report or in the several reports referenced above; rather, we have tried to summarize certain key elements.

Section II is a scientific overview of space astronomy; Section III describes the various programs and the strategy for advances in space astronomy; Section IV contains our recommendations.

II. SCIENTIFIC OVERVIEW

The intellectual goals of mankind have long been dominated by an ever recurring theme: to understand the place of man in the vast and complex universe. The heart of our philosophical and scientific gropings, from the most primitive of men to modern scientists, has been a struggle to obtain those key facts that could establish our place among the stars. Within the last few decades, powerful new techniques and instruments, spanning the entire electromagnetic spectrum and the realm of high-energy particles, have allowed us to venture into a domain of science where answers to some of the oldest questions can be sought. The scientist of today, able to observe in space, has been thrust from the confines of our earth and its atmosphere into the light of a dazzling panorama—a universe whose richness and fascination we could hardly have imagined.

We have found near us rapidly spinning stars, the pulsars, in which each cubic centimeter contains almost a billion tons of material and which dwarf our relatively feeble technology as each square centimeter emits as much power as all of mankind's electrical generating stations. Relatives of these supercompact objects digest matter by the most efficient conversion of mass to energy permitted by the laws of the universe. The crushing gravity of these objects liberates energy as high-energy particles and x rays, creating beacons visible from the farthest reaches of the Milky Way. Some of these objects may well be the parents of cosmic rays.

But even these fantastic objects are pale when compared with the brilliance of a quasar. What is a quasar? We still do not know. Perhaps, a galaxy in which somehow vast populations of stars are sucked into an irresistible gravitational abyss. As the stars fall ever faster, their mass, too, is largely converted into energy. The result is a radiation so brilliant that we can see it even at the edges of the observable universe, and thus nearly to the beginning of time. The energy-producing processes that cause the huge luminosities of quasars are yet to be understood by us.

As we look still farther away we see further back in time, until we find a mysterious glow that covers the sky no matter in what direction we look. We understand this glow to be nothing less than the remains of the primordial fireball from which the universe formed, cooled down by the expansion of the universe to a temperature of 3 K.

As we have obtained our first views of this grand picture we have stumbled across other phenomena that surely play a crucial role in the evolution of the cosmos. We may have seen evidence for the existence of black holes—the most extreme form of matter. Here is matter so dense that space near the object is sharply bent; no light or matter can leave the object. Its presence is revealed only by the strong gravitational field that extends from it. We suspect that some of the matter of the universe has been locked into black holes, both as a result of the violent forces in the primordial fireball and in the cataclysmic explosions of dying stars.

For the first time, we are perceiving the kaleidoscope of phenomena that is our universe. It is the unique capability of observations from space, combined with ground-based observations, that can convert our present glimpse of the panorama of the universe into a sharper and more detailed view. The astronomical universe is becoming the laboratory of the physicist.

Wide ranges in the spectrum of electromagnetic radiation are now open for observation outside of the narrow windows accessible from the earth's surface: gamma rays, x rays, short-wavelength ultraviolet, long-wavelength infrared. In addition, space astronomy has let us study in great detail other types of radiation, notably the high-energy particles, nuclei, and electrons that reach the solar system from interstellar space. It is not surprising that the existing opportunities have already led to fascinating new results and promise a wealth of new discoveries in the years to come.

In trying to transmit the excitement and promise of space astronomy and astrophysics, we shall not attempt to cover the entire field of observations. This was done in the 1974 report of this committee. Rather, we wish to highlight some of the problems that await major advances and solutions through observation from space in the next decade.

Almost all wavelengths of radiation bring testimony of violent events and the release of enormous amounts of energy from all parts of the universe. Our sun, the star most accessible to direct observation, exhibits such outbursts in the form of solar flares. Such flares are among the most energetic and impulsive phenomena in the solar system. Their effects are manifested throughout the solar atmosphere, in interplanetary space, and in the magnetosphere and ionosphere of the earth. Solar activity, including flares, and long-term changes in the solar brightness may even have a profound effect on terrestrial weather. While the details of the mechanisms that lead to the flare phenomenon are still shrouded in mystery, we know that the sudden release of energy accelerates charged particles and creates a hot plasma. Both the accelerated particles and the hot plasma radiate a rich electromagnetic spectrum, extending from radio waves to gamma rays. An understanding of the solar-flare mechanism may well play an important role through similarities with other phenomena involving explosive phases and particle acceleration, which seem to occur in the universe with high frequency and on a wide range of scales.

Some explosive phenomena, such as solar flares, stellar flares, novae, and rapid intensity variations from binary stars, leave the initial object more or less intact; while others, such as supernovae and possibly the violent phenomena associated with the central parts of galaxies, lead to the destruction of the initial object and a distinctly different remnant. The investigation of such remnants from supernovae are at the forefront of stellar astronomy because they contain some of the fascinating new astronomical objects such as neutron stars or pulsars and probably black holes.

The binary x-ray stars are a primary source of information on neutron stars and black holes. Binary systems act like a natural astrophysical laboratory, and, unlike their close relatives, the pulsars, the collapsed stars comprising the binary x-ray sources provide a multitude of data as they are subjected to a variety of perturbing effects from their stellar companions. Their mass can be determined from orbital analysis, and the flow of matter around them helps to probe the intense gravitational and magnetic effects in their immediate vicinity. Equally important, the study of the companion star provides the essential ingredient needed to solve the evolutionary puzzle as to how these stellar systems originate. One class of binary x-ray stars always contains a massive, young star. The existence of these stellar systems has allowed a credible scheme to be developed for their evolution from birth, through a supernova explosion, and into a black hole or neutron star phase whence they become x-ray sources. Not only is this remarkable in itself, but also, for the first time, we appear to have direct evidence for the kind of star that leads to a supernova.

A direct link to these highly condensed states of matter (neutron stars and black holes) is provided by another member of this family of objects that has been known for a considerably longer time—the white dwarf. White dwarfs and neutron stars provide us with a laboratory for studying material in the density range 10^5 to 10^{10} g/cm^3. In no other way can we investigate conditions of degenerate matter on such a large scale. Fundamental concepts of solid-state physics, quantum mechanics, and the properties of nuclear matter are directly involved. More importantly, the physics of these objects, and in particular of the black holes, is dominated by macroscopic, first-order effects of general relativity and hence constitute the test objects of this theory.

Four rapidly pulsing sources have now been observed beyond the radio band—the Crab nebula pulsar, Cen X-3, Her X-1, and Vela. Observations in x and gamma rays confirm that both the Crab and Vela, at least, are still accelerating particles to very high energies. These acceleration mechanisms are important for understanding the origin of cosmic rays—energetic nuclei and electrons with energies up to 10^{20} eV. Direct evidence relating to the possible origin of cosmic rays comes from high-energy gamma-ray astronomy. Of particular significance are studies of the region of the Vela supernova remnant. A constant high-energy gamma-ray flux and a pulsed flux (at the radio pulsar period) are observed in this region. The constant gamma-ray flux suggests that nuclear cosmic rays interact with matter in the Vela supernova region (thereby tying cosmic rays to the supernova phenomenon), and the pulsed flux suggests that cosmic-ray electrons with energies up to 10^{16} eV are still being generated.

We are in the process of learning a great deal about cosmic rays. Accurate determinations of the elemental composition and energy spectra over a wide range of energies and for all known elements have become possible and have contributed to a clearer picture of the nature of the particle sources and the acceleration mechanisms. Experimental techniques are on the threshold of identifying the isotopic composition of many elements and will be applied to both solar and galactic particles. This will be of significance in understanding the sun and in determining, for example, the temperatures at which nucleosynthesis occurs in the cosmic-ray sources. Such information can be gained in no other way. Separation of isotopes permits the investigation of radioactive nuclear species, which serve as clocks to determine the age of the particles. Further, the discovery of even one complex antinucleus such as anticarbon would imply the existence of antimatter stars and element building and would have profound significance regarding the nature of the Galaxy and the universe.

Energetic events also appear to take place in the central regions of galaxies. Such regions are among the most fascinating and mysterious in the universe. The center of our own Galaxy, considered to be rather normal, shows evidence from radio, infrared, and x-ray observations that complex physical processes have striking peculiarities that may be indicative of even more complex phenomena. The nuclei of the close peculiar galaxies NGC 1068 and 4151 are just beginning to show structure when observed from the ground (i.e., with a resolution of 1 sec of arc). Higher-resolution imaging in various emission wavelengths will reveal the nature of the cloud structure in the nuclear regions, and higher spatially resolved spectra will reveal how the clouds are kept hot, whether there are regions of different densities, and how the various clouds are moving. Increased spatial resolution will also provide insight about the smaller nucleus inside the already tiny cloud structure.

These questions are of central interest in the study of galaxies because so little is known, yet so much suspected, concerning the role of the nuclei of galaxies in their evolution. Is gas, which has been shed by old stars falling into the nucleus, reionized or formed into the next generation of stars? Are explosive phenomena in nuclei propelling gas clouds high above the plane or into the disk renewing spiral structure? Are all nuclei active, or is activity just one phase in the life cycle of a galaxy?

An understanding of the nature of galactic nuclei is of increased interest because of the suspicion that quasars may be no more than unusually active nuclei of galaxies—nuclei so overluminous that they dwarf the light from their background galaxy. If this were the case, as many astronomers believe, just what has brought the nucleus to its present state? Perhaps gravitational collapse plays a central role. But, collapse of what?

The other constituent parts of galaxies also pose many questions. One of these questions concerns the origin of stars and planets. Its understanding can be advanced in two ways: (a) observation of stars thought to be in the process of formation and regions of the interstellar medium thought to be likely sites for star formation; (b) observations of objects in our own solar system, in particular, those thought to be least altered since their formation 4.5 billion years ago, such as the comets, certain asteroids, and the outer planets and their satellites. Here space astronomy finds its link with the planetary sciences. In the case of the earth, space astronomy has close ties with geology, the origin and evolution of the atmosphere, and the origin of life.

The interstellar clouds where star formation is occurring are opaque to all but radio and infrared radiation. Although ground-based studies of the infrared and radio emissions from contracting clouds provide information on many new aspects about the processes operating, observations from space are expected to lead to breakthroughs in understanding them. For example, because it is presumed that molecular hydrogen is the major constituent of contracting clouds of interstellar material, the ultraviolet observations of this molecule by the Copernicus satellite are significant. Such observations, however, could be made only in relatively low-density clouds of the general interstellar medium. Observations of molecular hydrogen in dense protostellar gas clouds require studies of the strongest infrared lines expected, the rotational lines at 17 and 28 μm. Studies of these two important lines cannot be made from the ground.

As an interstellar cloud contracts, the liberated gravitational energy heats the gas and dust. Eventually the heated dust will become hot enough to be seen by its continuous infrared emission. A thorough understanding of the heating and cooling of protostellar clouds is essential for understanding the processes that operate during star and planet formation.

The interstellar medium, already observed in some detail in our Galaxy by radio and other measurements, contains gas in turbulent motion as well as dust, magnetic fields, and the extremely hot cosmic-ray gas. Recent observations of cool giant stars have indicated that they are ejecting gas at a high rate into the interstellar medium. The range of temperature and density in these expanding gas clouds is such that condensation of refractory materials, such as graphite and silicates, as well as formation of molecules such as H_2O and CO, can occur. Thus, such stars may be largely responsible for the composition and state of the interstellar clouds. In particular, it is difficult to account for the origin of the interstellar dust grains by any other process, because the density in the general interstellar medium is far too low to allow the formation of such grains in times less than the age of the Galaxy.

Observations from space, by virtue of the extension of the accessible wavelength range to include the far infrared and far ultraviolet, the improved angular resolution obtainable, and the increase in sensitivity enabling us to look at very dim objects, will contribute much to our understanding of these processes.

The hot cosmic-ray gas contributes significantly to the energy in interstellar space, having an energy density comparable with that of magnetic fields and turbulent gas motion. The cosmic rays are therefore an important factor in the dynamics of the interstellar medium, and their presence may well influence the rate of star formation. Gamma-ray observations, through measurements of π^0-meson production, which depends on the density of cosmic rays and the density of the interstellar gas, are beginning to reveal the distribution of cosmic rays throughout the Galaxy. Future work in gamma-ray astronomy is bound to improve this knowledge. Low-energy cosmic rays have no access to the inner solar system because of solar modulation. Only experiments on deep-space missions and perhaps out-of-the-ecliptic missions that reach beyond the solar modulation region will reveal the role of low-energy cosmic rays in interstellar space.

Another component of galaxies, globular clusters, has recently presented a striking phenomenon. X-ray observations by the *Uhuru* and OSO-7 satellites have suggested the existence of massive black holes lurking near the centers of some globular clusters. The extreme gravitational fields of these objects may cause them to swallow stars, one by one. To add to our surprise, variable x-ray sources occur in globular clusters with a frequency 100 times the average frequency of x-ray sources in the Galaxy as a whole.

The significant clue to the nature of these objects is the fact that at least two of the clusters with x-ray sources have central densities that are among the highest of all the known clusters. Optical observations of one of these, M15, have revealed that the density of stars increases steadily toward the cluster center, down to the limits of resolution imposed on ground-based observation. This is consistent with a picture in which a deep gravitational well exists around a massive black hole. Confronted in M15 with projected stellar concentrations of several stars per square second of arc, and stellar magnitudes of 17 and greater, there is a clear demand for high-resolution studies that only an LST can offer.

Another recent discovery is the existence of the quasars. Although a great deal has been learned about quasars since their identification with extragalactic objects in 1963, the basic nature of these objects still remains a mystery. Direct high-resolution images of the nearer quasars are required to establish the apparent fact that quasars are located in the nuclei of galaxies. Should such an association with galaxies be established, low-resolution spectra taken within a few tenths of a second of arc of the quasars will have to be obtained to determine whether the associated galaxies are normal. Such spectra will establish the emission-line red shifts and absorption-line red shifts as being of cosmological or local origin by showing whether the quasar and associate galaxy have the same red shifts.

The relationship of red shift to distance, the velocity–distance relation, has significant cosmological consequences. The generally accepted cosmological theory assumes that the universe started explosively some 15 billion years ago and has been expanding and cooling ever since. As a consequence of this expansion, the radiation field from the earliest stages of the universe, the primordial fireball, has now cooled to a temperature of 3 K, and the wavelength of the associated photons is about 1 or 2 mm. The observations of this spectrum and the directionality or anisotropy of the 3 K "relic" radiation are the best tools available for the study of the structure and evolution of the early universe. It is not possible to map the spectrum and directionality of the 3 K radiation from ground-based observations, for the earth's atmosphere and the Galaxy are radiating at temperatures above 3 K and hence produce signals more intense than those from the cosmic background. To minimize these sources of interference, high-frequency observations above the earth's atmosphere are needed. While excellent observations have been made with ground-based, balloon, and rocket instruments, questions concerning the directionality and magnitude of the anisotropy are still unanswered. Only more powerful studies at satellite altitudes can be expected to define the early history of the universe.

One of the crucial parameters in cosmological theories is the total amount of mass in the universe. The value of the gas density in intergalactic space is particularly significant; densities above critical correspond to a closed universe; values below it to an open universe. Observations of absorption in the ultraviolet light emitted by quasars, assuming a cosmological interpretation of quasar red shifts, suggest that the density of neutral gas is perhaps 6 orders of magnitude below the critical value. But could the missing mass be in the form of ionized gas? Determinations of the ionized gas density are less definitive. They may be obtained from x-ray observations. In the hard x-ray region (> 3 keV), where the sky is dominated by a diffuse background, the x rays are sufficiently isotropic to indicate that most of the emission originates from well outside the local group of galaxies. Determining the true component of diffuse emission, however, involves subtracting contributions from discrete sources. Future space missions are needed to provide the required improvement in sensitivity and spatial resolution.

As if to remind us that nature is infinite in its diversity, several new transitory phenomena have burst on the scene in the last few years, telling us about conditions of which we had no prior suspicions.

A prime example is the gamma-ray bursts, discovered in 1973 as part of the Vela satellite program. Their study, to date, has been confined to the detection of events in single, widely separated instruments. They are characterized by a short, frequently repetitive burst of hard x rays and gamma rays that lasts only tens of seconds or less. Their distribution in the sky appears to be isotropic and therefore is compatible with either an extragalactic or nearby galactic origin. None have been identified as originating from a known celestial object. If the history of high-energy astronomy is any guide, however, they may be found to originate in highly compact matter configurations.

The transient x-ray sources, also first discovered during the Vela program, have been known for a longer time, but their true nature is equally elusive. Within the past year, a new element in their behavior, periodicity with periods of the order of several minutes, has been discovered that might provide a key to their understanding. They may represent a different version of ordinary novae whose appearance they mimic in many respects, although so far there is no observational or theoretical basis to make such a connection.

We have exhibited here some of the stimulating and important objectives of space astronomy. They involve phenomena ranging over dimensions of a kilometer or less to cosmic distances. They include investigations ranging from well-defined, specific programs to the exploration of the unknown. But most significantly, they confirm the role of astronomy in revealing basic physical laws and the arrangement and history of the universe. A dramatic example is the physics of collapsed matter—perhaps the starting point for the creation of the universe and now present in the form of black holes. The physical laws describing such matter require a blending together of concepts of general relativity and of quantum mechanics. They involve the most fundamental questions that scientists can ask relating to the origin of matter and space.

Certain fields of astronomy—cosmic-ray, gamma-ray, x-ray, and ultraviolet astronomy—would not exist except for the ability to put instruments high above the earth's surface. Other fields, traditionally pursued from the ground, have continued to make important contributions, but with limitations imposed by the atmosphere. They too will benefit from observations in space. Fulfilling the potential opened to us by space technology is the challenge before us.

III. STRATEGY AND PROGRAMS FOR SPACE ASTRONOMY AND ASTROPHYSICS

A. Strategy

The program of research that was sketched in the preceding section covers, and in many instances combines, the efforts of the several astronomical disciplines. It must be incorporated into appropriate missions suitable to achieve the objectives. A wide variety of requirements has to be met, including national facilities that provide observations for many research teams and that may be active for a decade, dedicated free-flying satellites, and individual experiments that are part of multi-experiment satellites. The development of the Space Shuttle plays a dominant role in the planning of an astronomy program. It will become the major launch facility for free-flyers (the LST, for example), and it will be the vehicle for the Spacelab, whose scientific potential will be largely used by the astronomical disciplines.

For scientific reasons and because of the financial constraints of NASA's space-science activities, the recommended programs must be sequenced in time and a strategy toward the future pursuit of astronomy in space must be found that takes into account the scientific impact, the ripeness of the field, the readiness and capabilities of the experimenters, and the status of the technological development of the instrumentation as well as the spacecraft. The strategy should work to maintain strength in the institutions responsible for the research.

The appropriate strategy comprises a sequence of missions, beginning with simple exploratory and survey experiments, continuing with more complex instruments, and culminating in large, multipurpose observatories whose performance is limited only by intrinsic, physical characteristics. The requirement for single experiments, however, with specific objectives of high importance, will always be present. The details of the strategy vary among the disciplines depending on the scientific objectives and the state of knowledge in the field. Optical astronomy pursued the exploratory and survey phases for the last three centuries with ground-based telescopes, limited by the atmosphere, and with UV observations during the early years of the space program. Hence, this branch of astronomy has advanced to the point where the LST, with its diffraction-limited performance, is the required major mission to meet the scientific goals outlined in Section II.

Solar physics employs a different strategy because it deals with a single object and set of conditions. It does so with a dual purpose: to understand in detail the nature of an average star, thus providing a keystone for stellar astronomy, and to understand the radiation environment of the earth, which is of fundamental importance to the earth sciences. The strategy has been developed in terms of solving specific problems, for example, the physics of the solar-flare phenomenon and the transport of energy in the solar atmosphere. In general, the solution to these problems requires a concentrated set of coordinated observations. The Solar Maximum Mission represents the coordinated approach to solar flares. While this mission is aimed principally at the astronomical, rather than the terrestrial, objectives, it suggests that additional studies may be desirable to satisfy the two purposes through a single set of missions.

X-ray astronomy is now in its exploratory and survey phase with the recent *Uhuru* and SAS-3 missions. This phase will continue with HEAO-A. With HEAO-B and the 1.2-m telescope, the discipline will enter into a new phase with observatory-class facilities. A similar strategy can be found in the sequence of infrared and gamma-ray missions. Cosmic-ray astronomy is in a somewhat different category because there are no definable "observatories"; rather, there is a sequence of increasingly sophisticated experiments studying various characteristics of this radiation.

The implementation of this strategy requires a variety of space opportunities—missions of brief duration ranging from sounding rockets to Spacelab, free-flyers such as the small satellites of the Explorer program, and the much larger payload capacity of Shuttle. Cost and continuity are prime considerations of the HEAO program and the newly formulated solar program. A major element of the strategy requires the development of permanent national facilities to be launched, recovered, refurbished, and launched again by Shuttle and the development of the more modest facilities for continuing use as part of Spacelab.

This committee has presented and discussed the opportunities for space astronomy in its 1974 report (*Opportunities and Choices in Space Science*, 1974). It has given the details and the rationale for individual missions, which we shall not repeat here and to which we refer the reader. In its 1975 deliberations, the committee arrived at essentially the same priorities that it recommended in the past year, although sharpened and slightly modified in the light of recent developments and the recommendations of the summer studies on solar physics and infrared astronomy.

To provide a simplified overview of the activities of space astronomy and astrophysics, demonstrating the balance among the disciplines and the commonalities and differences of the space vehicles required to achieve the scientific goals, we have prepared matrices displaying the existing programs (Table D.1a) and the candidate programs (Table D.1b), without attempting priorities or completeness. Table D.2 contains the study status of candidate missions. Table D.3 reflects our decisions of priority and timing.

In the following paragraphs we discuss (1) the urgent requirements for 1977 new starts and (2) the long-range program in space astronomy.

B. New Starts in 1977

Our deliberations have led us to the conclusion that three new starts and the initiation of two Explorer missions in 1977 are essential for the

TABLE D.1 Existing and Candidate Programs

				High-Energy Astrophysics		
	Optical and uv	Ir and Radio	Solar	X-Ray	Gamma-Ray	Cosmic-Ray
(a) Existing and Approved Programs						
Ongoing	Technology development Ground-based Balloons Rockets	Technology development Ground-based Balloons Rockets Aircraft	Technology development Ground-based Balloons Rockets Aircraft	Technology development Balloons Rockets	Technology development Balloons	Technology development Balloons
Recent space missions	OAO (Copernicus)		ATM OSO-8	SAS-1 (*Uhuru*) SAS-3 OSO-8	SAS-2	IMP-7[b] IMP-8[b] ~ 25% Pioneer 10, 11[c] MVM[c] ~ 25%
Approved missions[a]	IUE		ISEE[b] ~ 15%	HEAO-A HEAO-B	HEAO-A	ISEE[b] ~ 15% MJS[c] ~ 25%

(b) Candidate Programs[a]

Spacelab experiments	SPI	SPI	SPI	SPI	SPI	SPI
Free-flyers	Explorer	Explorer (IRSE) ORI	Explorer Out-of-the-Ecliptic[b,c] ~ 25%	Explorer HEAO Block II	Explorer (GRE) HEAO Block II	Explorer HEAO Block II MJU[c] ~ 25% Out-of-the-Ecliptic[b,c] ~ 25%
National facilities (free-flyers and Spacelab facilities)	LST[c] SUOT VLST	LST[c] SIRTF	SMM SSS Sortie telescope LSO	1.2-m telescope LAMAR 2.2-m telescope		

[a] See Section VI, Glossary, for explanation of abbreviations.

[b] Shared with Space Physics Program. Percentage figures indicate approximate portion of payload dedicated to astronomy disciplines.

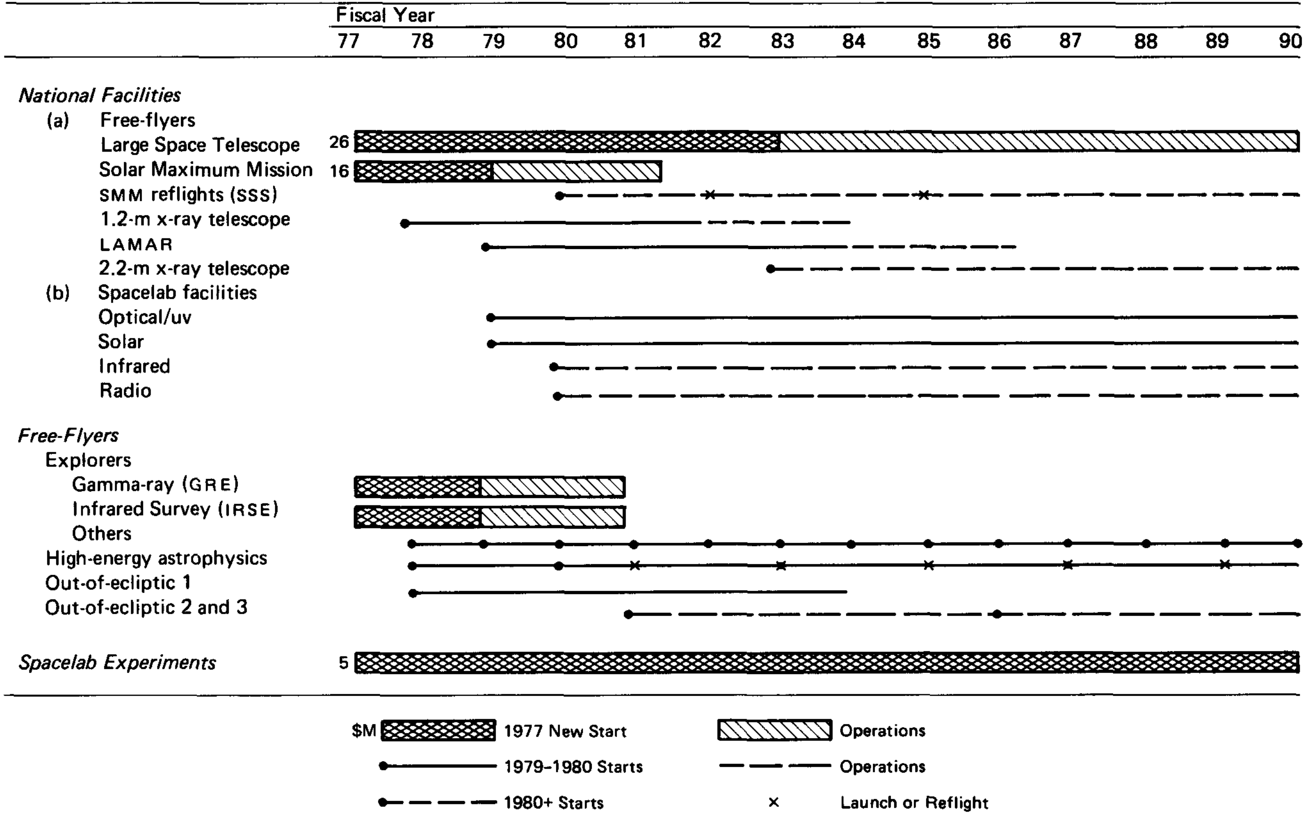
[c] Shared with Planetary Program. Percentage figures indicate approximate portion of payload dedicated to astronomy disciplines.

TABLE D.2 Study Status of Candidate Missions[a]

	Phase A	Phase B	Responsible NASA Center
National Facilities			
LST	Complete	Complete 1976	MSFC
SMM	Complete	Complete 1976	GSFC
1.2-m x-ray telescope	Begun May 1975		MSFC
LAMAR	Begun May 1975		MSFC
SSS			
2.2-m x-ray telescope			
LSO			
Free-flyers			
x ray, gamma ray, cosmic ray	Begun May 1975		MSFC
MJU	Complete		JPL
OOE	Complete 1976		ARC (Joint U.S.–ESA)
ORI			
Explorers			
IRSE	Complete 1976		GSFC
GRE	Complete	Complete	GSFC
Spacelab Facilities			
SIRTF	Complete 1976		ARC
SUOT	Begun 1974		GSFC
Sortie Telescope	Begun 1974		GSFC

[a] Where no entry appears, it means that NASA has not authorized formal study activities for the particular mission. Phase A is a feasibility study, and Phase B is the program definition phase. Phase B must be completed before NASA can initiate hardware development leading to flight.

TABLE D.3 Mission Model



viability of space astronomy and astrophysics within the NASA program. This conclusion is not new but, in fact, reiterates the conclusions of last year's study (see *Opportunities and Choices in Space Science, 1974,* pp. 33–74). Two of these new starts, the Large Space Telescope and the Solar Maximum Mission, and the initiation of the Infrared Survey Explorer and the High-Energy Gamma-Ray Explorer, represent a broad-based program of attack on the scientific questions at the frontiers of our knowledge of the universe—the active sun, the stars, the interstellar gas, galaxies, and cosmology. Each of these programs marks a major advance in instrumental techniques in the spectral range of interest. The history of astronomy of the past decade has taught us that the maximum increase of knowledge comes from the interplay of observations in all spectral ranges of objects of a staggering variety. Hence, we can anticipate that the interactions among these programs will herald a decade in which our knowledge of the universe will grow at a rapid rate.

We make one additional and important recommendation for new starts in 1977 because we recognize that a mature space astronomy and astrophysics program will be based on the Shuttle, and we believe that there should be no delay in turning to this mode of operation. We strongly support the development of equipment in fiscal year 1977 to fly on the early Shuttle flights of 1980 and 1981. Lack of active support of such instruments could mean that the first few flights would not live up to their scientific capabilities.

1. LARGE SPACE TELESCOPE

The Large Space Telescope will represent an advance in observational capability in the visual part of the spectrum comparable with all the developments in visual ground-based astronomy over the last 70 years. The enormous capabilities of the LST compared with those of even the largest ground-based telescopes are fourfold: (1) the spatial resolution is greater by a factor of 10 than is possible from the ground; (2) the LST can obtain observations of objects that are ten times fainter than is possible with any ground-based telescope; (3) observations of bright and very faint objects can be obtained in the far ultraviolet; (4) the whole infrared spectral range from 1 μm to 1 mm is available. With these capabilities, the LST will play a major role in astrophysical research in the decade or more after launch.

It is the consensus of this committee that the LST merits the highest priority of all space-astronomy missions. In fact, this magnificent instrument will represent one of the most important scientific projects ever undertaken by the United States. Therefore, we reiterate the recommendations made in the 1974 report that a new start for the LST be made in the next fiscal year, which is now fiscal year 1977.

The scientific rationale for the LST has been described in great detail in previous reports, i.e., *Opportunities and Choices in Space Science, 1974* (National Academy of Sciences, Washington, D.C., 1974), *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), and in the Phase B studies. Even so, all the exciting problems that it will be called upon to solve cannot yet be envisaged. For example, the discovery of x-ray sources in globular clusters just a few months ago (see Section II) represents an area of research in which the LST capabilities will be essential. New discoveries that require unforeseen auxiliary instruments and techniques will readily be handled because it will be possible to update the scientific instruments from time to time.

During the last year, detailed studies were made to determine the relative costs of 3.0-, 2.4-, and 1.8-m telescopes. It was concluded that although substantial savings in cost could be achieved if the size were decreased from 3.0 to 2.4 m, any further decrease in size would affect costs only minimally. Furthermore, a 2.4-m configuration is capable of achieving all the major goals envisaged for the LST provided longer observing times are employed. Major aspects of the science would be sacrificed with a 1.8-m configuration. This committee therefore endorses NASA's decision to pursue the 2.4-m design and strongly agrees that a diameter smaller than 2.4 m should not be contemplated. During the last two years, some 50 scientists from institutions all over the United States, Canada, and Europe have worked with contractors in Phase B preliminary design studies of candidate scientific instruments and of the LST itself. These studies have demonstrated the feasibility of the overall program and have shown that substantial improvements in detectors are desirable and possible.

Large numbers of scientists continue to be deeply involved in the LST project. Contractors are also participating both through contracts with NASA and through their own resources. With so much involvement and momentum it is highly advantageous from both scientific and cost standpoints that the LST project be initiated in fiscal year 1977 for a projectal launch in 1983.

2. SOLAR MAXIMUM MISSION

The Solar Maximum Mission (SMM) is a well-conceived, problem-oriented effort to understand the physics of flares. It also has the capability of contributing significantly to our understanding of active region structure and dynamics. Simultaneous interplanetary particle measurements (from the ISEE satellites) will contribute to the SMM effort to understand the acceleration mechanisms and energy balance in a solar flare. Ground-based radio measurements will provide auxiliary information on active regions and on the containment and release of accelerated particles. Information on the magnetic structure of active regions and the chromospheric effects of particle accelerations and thermal plasma will be provided by ground-based optical measurements.

The importance of the physical problems manifest in the solar-flare phenomenon has been alluded to in Section II and is described in detail in the report of the 1975 SSB Study on Solar Physics (Working Paper F). Key questions have been defined concerning the relationship between precursor signals and the beginning of the flare instability, the mechanism that converts magnetic energy into "flare" energy, the location and nature of the primary and secondary particle acceleration processes, the relationship between these and the associated radio bursts, whether the energy content in the initial accelerated particle beam is sufficient to account for all later manifestations of the flare event, the nature of coronal storage of energetic particles, the nature of ^{3}He-rich events, and the origin and physical characteristics of ejected plasmoids and the mechanisms producing them.

The recent instrumental, observational, and theoretical progress in solar-flare research points the way to the solution of these and other flare problems. Instruments and analytical tools to probe and model the particle acceleration region and the reaction of the solar atmosphere are now available. This reaction is so diverse that a coordinated effort must be made to observe many flares with sophisticated ground-based and satelliteborne equipment; the upcoming solar maximum provides the opportunity to study sufficiently diverse flare events.

The leading edge of this coordinated attack on the flare problem is the group of specialized instruments that comprise the SMM payload. This payload, which has now been defined, will *simultaneously* observe all emissions from the major phases of the flare, including spatial measurements of the extreme ultraviolet (EUV and XUV) enhancements and coronal transient events. The SMM thus provides the capability to determine, for the first time, the spatial and temporal relationships between the various forms of flare emission. The physical mechanisms involved in the various flare phases and their causal relationships can then be determined.

The SMM has long been in the planning stage. A study of a flare payload was first conducted in 1972, and definition of the SMM, including spacecraft and payload, was completed in January 1974. Since then a candidate payload has been selected. Thus, from both the scientific and the technological point of view, the time for the mission is ripe.

A new start in the coming fiscal year (FY 77) must result in a late 1979 launch. An analysis of flare statistics during the past solar cycle indicates that for a period of activity comparable with that expected in 1980 (peak sunspot number about 110) the SMM might achieve *optimum coordinated* birth-to-death observations of approximately five flares of class 2 and larger during the mission. Observations of smaller flares (class 1) will be more easily obtained since approximately 50 completely observed events are expected. When the probable period of peak activity is estimated, on the basis of statistics from the past 19 solar cycles, it becomes apparent that the probability of such activity in the late 1979–1981 time frame is 0.31 (6 out of 19 cases). A one-year delay reduces this probability to 0.05 (1 out of 19 cases), while an advance of a year (impossible on programmatic considerations) only improves the probability slightly to 0.37 (7 out of 19 cases).

This simple analysis dramatically indicates the impact of the timing of the launch on the potential success of the SMM as a flare mission. This consideration, coupled with the desirability of flying coincidently with the ISEE series, makes a fiscal year 1977 new start imperative if the SMM is to be successful.

If it is not possible to begin funding in 1977, the arguments in support of the SMM are severely weakened.

3. INFRARED SURVEY EXPLORER

The Infrared Survey Explorer (IRSE) has been given the highest priority by the 1975 SSB Study on Infrared and Submillimeter Astronomy (see Working Paper E). This instrument is a cryogenically cooled scanning telescope covering multiple spectral bands out to 100 μm. The spectral emphasis is on the 7–30 μm region, where detector technology is most advanced and potential for new scientific discovery especially great. Proven technology in detectors, cryogenics, and spacecraft has made the time ripe for this survey. It is now possible for a satellite survey to provide more than two orders of magnitude greater sensitivity than can be obtained by any other approaches.

Every area of astronomy (most recently, x-ray astronomy) has made significant and unexpected advances from systematic sky surveys. The proposed survey is likely to reveal new and unexpected objects and will have the impact of the Palomar and Cambridge surveys, which opened the way to spectacular advances in optical and radio astronomy. It will also provide a complete systematic list of objects for the further study by present and future instruments.

Infrared observations are required to aid in the understanding of several important astronomical phenomena, including the early stages of star formation, stellar associations and planetary systems, the last stages of stellar evolution, the formation of interstellar dust and molecules, the extraordinarily high energy output of the nuclei of some galaxies and of QSO's, and the early history of the universe.

Guidance for selecting objects to study in the infrared has been primarily from optical and radio data and from the 2.2-μm ground-based survey. The proposed survey satellite will provide a much better selected set of objects to study and may discover new classes of objects not revealed by surveys at other wavelengths.

A definition study of the Infrared Survey Satellite is now under way. Discussion of a possible international cooperative effort with Dutch astronomers has been initiated.

4. GAMMA-RAY EXPLORER

The discovery by SAS-2 of discrete gamma-ray sources and resolved emission from specific features such as the spiral arms of our Galaxy provided the rationale for a new and larger high-energy gamma-ray mission. In 1974 the Space Science Board recommended such a gamma-ray satellite.

The Gamma-Ray Explorer (GRE) will carry a large (1200-kg) gamma-ray detector with a sensitivity that is 26 times that of the SAS-2 instrument and 12 times that of COS-B. Hence the energy range covered can extend beyond 10 GeV. Its angular resolution will be 1 deg compared with about 3 deg for the other spacecraft, and the energy resolution is about 10 percent, an improvement by approximately a factor of 3. The GRE will make a detailed study of the high-energy gamma-ray emission from our Galaxy and thereby yield a direct survey of the product of the cosmic-ray density and interstellar matter in the spiral arms, the dense clouds, and other features of the Galaxy. These results will provide an important new clue to the sources and fate of the cosmic rays. The GRE will also extend the measurements of gamma-ray pulsars already observed and will have the sensitivity and energy range needed for discovery and extended measurements of new gamma-ray pulsars and sources of other types. This high sensitivity and energy resolution will make it possible to measure the uniformity of the intensity and the energy spectrum of the diffuse celestial gamma radiation. The instrument will also be sensitive enough to observe the recently discovered fast low-energy gamma-ray bursts. Being launched just before the solar maximum, GRE will observe the gamma rays from the sun during large solar flares, thereby giving valuable information on the energetic solar particles accelerated in these flares.

The results of SAS-2 have confirmed that the theoretically predicted rewards of gamma-ray astronomy can be achieved by an instrument with the capabilities of the GRE. Supernovae, pulsars, the origin and propagation of cosmic rays, cosmology, and galactic structure are all now open for gamma-ray observations, which can provide important new insight into the most energetic cosmic processes. Since the GRE has already completed a Phase B study, it should be initiated immediately within the Explorer program.

5. SPACELAB SCIENCE

The Shuttle/Spacelab system will become operational about mid-1980. In order to make the most scientifically productive and cost-effective use of this new space transportation and support system, the development of payloads must begin immdiately.

Recently, several studies have shown that individual relatively simple and inexpensive astronomy and high-energy astrophysics experiments could be made ready in time for the earliest Spacelab missions and some even for the engineering flights. Several experimenters are already fully prepared. The early scientific payloads would consist of several of these experiments, which would have individual Principal Investigators and could be developed at far less cost than the facility-class instruments to be built later.

Many of these experiments would be an outgrowth of current balloon and sounding-rocket experiments. Possible experiments, however, need not be restricted in size and weight to what has been flown on rockets and balloons in the past. For example, in high-energy astrophysics, cost and complexity are not necessarily a direct function of payload weight. Also, optical astronomy experiments having very long optical paths, impractical to package in existing sounding rockets, can be flown. Spacelab missions provide the advantages of enormously increased observing time, greater weight capability, and the means for developing and testing new instrumentation for later use on free-flyers.

A small, cryogenically cooled contamination monitor should be included on an early Spacelab flight. This would be of value for the feasibility study of later, facility-class instruments, particularly the cryogenic infrared telescope (SIRTF).

Problems in astronomy and astrophysics that can be addressed by these individual experiments include many that are not covered by the larger facility-class instruments. Several examples of possible experiments were suggested at the Small Astronomy Payloads Workshop held at Goddard Space Flight Center in February 1975 (optical, uv, and ir astronomy), by the Quick Reaction and Special Purpose Experiment Definition Team (solar physics), and by the *ad hoc* planning group of the NASA High Energy Astrophysics Management Operations Working Group. A few of the suggested experiments include the following:

Very high resolution ($\lambda/\Delta\lambda = 3 \times 10^5$) far-uv spectroscopy of bright stars;

Far-uv imaging and objective-spectrographic all-sky survey;

Very accurate absolute photometry and monitoring of the solar flux over wide wavelength ranges;

Studies of high-energy cosmic rays (energy spectra and composition);

Very high sensitivity x- and gamma-ray measurements of specific regions;

Far-infrared spectrophotometry of the 3 K background radiation; and

An orbiting radio interferometer utilizing a 2- to 3-m dish on the Shuttle in conjunction with a ground-based dish.

The development of the Spacelab astronomy and astrophysics payloads is urgently needed and should be funded beginning in fiscal year 1977. In order to assure that good use is made of the early Spacelab flights, primary emphasis should be placed on the preparation of the individual experiment payloads discussed above. For example, such investigations should be planned for Spacelab Flight #2, presently scheduled for late 1980.

C. Long-Range Programs

1. OPTICAL AND ULTRAVIOLET ASTRONOMY

Optical and ultraviolet astronomy has much to gain by getting above the blurring and obscuring effects of the earth's atmosphere. Although this committee considers the LST to be the prime optical and uv research facility for the 1980's, there are other missions that are required to support and extend the work of the LST. These are briefly described below.

(a) SPACELAB ULTRAVIOLET/OPTICAL TELESCOPE We recommend a new start on the Spacelab UV-Optical Telescope facility (SUOT) for fiscal year 1979. This general-purpose 1-m telescope will provide a wavelength coverage from 912 Å to 4 μm and images of excellent quality (0.2 to 0.3 sec of arc) over a wide angular field (0.5 deg). (For instrumental details and scientific goals see the *Interim Report of the Astronomy Spacelab Payloads Study, Volume 2, Ultraviolet and Optical Astronomy*; and *Opportunities and Choices in Space Science, 1974,* p. 48.) This instrument will complement the LST program by providing a test facility for new LST instrumentation and the photometric calibration of the LST. Some programs (i.e., $\lambda < 1150$ Å; wide field imaging) are included that will not be covered by the LST.

(b) EXPLORERS Explorers will still continue to be useful in uv/optical astronomy in the Shuttle era, in that (a) some experiments are best performed from higher orbits or different orbital inclinations than can be reached directly by the Shuttle; and (b) some experiments require or can utilize longer observing times than can be obtained in Spacelab missions, yet are sufficiently small and simple that they fall in the Explorer class of instruments. In category (a), the payloads could be launched either with Scout or Delta rockets or with Shuttle plus an interim upper stage. In category (b), the spacecrafts would be released in orbit by the Shuttle. Payload size, weight, and capability would be in the range of current sounding-rocket payloads, but observing times of a year or more would be available.

(c) SMALL ASTRONOMY PAYLOADS IN SPACELAB There exist many investigations in uv/optical astronomy that can be attacked with payloads analogous to current sounding-rocket-class payloads. These payloads, carried on Spacelab, could take advantage of the enormously increased observing time of a 7- to 30-day Spacelab mission relative to a typical 5-min rocket flight. The size, weight, and capability of such payloads is in the same range as Explorer payloads. Being permanently attached to the Shuttle, however, they have the advantage of greater simplicity and lower cost than free-flyer Explorer experiments but the disadvantages of shorter observing times and a more restricted range of orbits.

(d) VERY LARGE SPACE TELESCOPE As a very long-range goal in optical and uv space astronomy, it seems reasonable to consider the construction of large multimirror telescopes (VLST) with light-gathering powers of a single 10-m mirror or even a 30-m mirror. If such a telescope had a spatial resolution of 0.1 sec of arc, it could obtain spectra of objects 10 times too faint for the LST. Such an instrument would be the natural follow-on to the LST in the late 1980's or early 1990's.

2. SOLAR PHYSICS

The long-range strategy for solar physics has been developed in detail in the 1975 SSB Study on Solar Physics (see Working Paper F). The great diversity of problems confronting solar physics have been broadly classified, and many specific problems have been defined with the types of effort necessary for their solution. Following the study of flare phenomena by the Solar Maximum Mission, there is envisaged an orderly procession of study of the quiet sun, and the less extreme forms of activity, with combined Shuttle instruments and facilities, in coordination with various free-flyer (earth-orbiting) missions, interplanetary missions (including out-of-the-ecliptic and stereoscopic missions), and, of course, ground-based observations and theoretical studies.

The elements of the space program include the following:

(a) FREE-FLYING SOLAR SATELLITES These satellites would be dedicated to specific problems in four major categories:

1. Studies of the large-scale structure, energy balance, and composition of the quiet solar atmosphere, of active regions, and of the magnetic structure of the extended corona.
2. Observations properly phased with the major solar shuttle observatory instruments flown on the Shuttle, either individually or as a cluster, and directed toward support of specific problems to be investigated.
3. Coordinated observations with missions principally directed toward the study of the interplanetary phenomena.
4. Long-term measurements of the solar "constant" over the complete solar spectrum with good spectral resolution and with high accuracy (0.1 percent). Since long-term stability is difficult to achieve in flight, periodic flights would be required.

Specifically, it is envisaged that following Shuttle recovery, refurbishment, and instrument replacement, the Solar Maximum Mission satellite—then labeled the Solar Synoptic Satellite—would be dedicated toward synoptic observations of the sun, coordinated with and complementary to Spacelab and interplanetary missions. Both the Solar Synoptic Satellite and Explorer-class satellites could be employed toward the goal of highly accurate measurements of the solar luminosity in various spectral ranges. Additionally, an Explorer-class mission that places a spacecraft in solar orbit 90 deg east of the sun–earth line could permit observations of the three-dimensional spatial solar structure, in conjunction with the Solar Synoptic Satellite or other near-earth platforms.

(b) SPACELAB Spacelab capabilities permit larger payloads in orbit, which will also be returned, updated, and reused. Solar physics envisages major national facilities of large instruments such as (1) a 1-m-aperture visible and uv telescope capable of high spatial resolution studies of chromospheric structure and magnetic and velocity fields; (2) an euv–xuv instrument capable of probing the solar atmosphere over the range 10^3 to 10^6 K; (3) a hard x-ray imaging facility that will permit measurements with high sensitivity of the size, structure, and location of nonthermal emitting regions. Such instruments are particularly suited toward coordinated attacks on detailed, well-defined problems, outlined thoroughly in the Solar Physics Study report (Working Paper F). The instruments themselves are described in detail in the *Interim Report of the Astronomy Spacelab Payloads Study*.

In addition, many solar-physics problems have been defined that can be attacked with rocket- and balloon-class instruments. These smaller payloads will consist of instruments designed for specialized observations and will often be needed to test new ideas and instrument concepts.

(c) OUT-OF-THE-ECLIPTIC The European Space Agency and NASA are presently considering a joint out-of-the-ecliptic mission (OOE-1). Such a mission represents a logical first step in out-of-the-ecliptic exploration. On such an initial step, emphasis should be placed on obtaining the observations over as wide a range of solar latitude as possible, i.e., carrying out the basic *exploratory* aspect of the mission and measuring the three-dimensional characteristics of interplanetary plasma, magnetic fields, and particles. It would be appropriate to carry out as much solar physics as consistent with these basic objectives. Even within the constraint of a small spacecraft important new solar observations can be made.

Following this first step, additional missions (OOE-2, for example) should be planned to study specific interplanetary phenomena in greater detail, e.g., the concentration of transient solar activity (and the resulting interplanetary effects) at midsolar latitudes. Other studies should be suggested by the initial phase exploration. These missions also should extend high-resolution solar observations to exploit the out-of-ecliptic viewpoint without the severe spacecraft restraints likely to apply on the initial mission. These observations will prove of extreme importance toward an understanding of the general circulation of the sun, will represent an important amalgamation of the goals of solar and interplanetary physics, and will substantially increase our understanding of the outward expansion of the solar wind and the interplanetary medium.

Programs for the very-long-range future of solar physics, e.g., near solar spacecraft and large solar observatory, merit additional future study.

3. INFRARED ASTRONOMY

The planning in infrared space astronomy reflects the recommendations of the 1975 SSB Study.

(a) COOLED SPACE OBSERVATORY—SHUTTLE INFRARED TELESCOPE FACILITY (SIRTF) The SIRTF is a cryogenically cooled 1- to 2-m telescope diffraction limited at 5 μm and designed for 30-day operation. It has a multiple instrument chamber providing for photometry, imaging, spectroscopy, and polarimetry over the range 5 μm to 1 mm. A cold telescope above the atmosphere will provide an enormous reduction of thermal radiative background on the detectors and consequently greatly improve the sensitivity. Such an instrument could study the infrared radiation of galaxies billions of light years away from the earth. It would allow rapid and sensitive searches for sources in stages of currently unstudied stellar evolution.

In the 7- to 30-μm spectral region this telescope has the spectroscopic and photometric sensitivity to make observations in hours that would require centuries for ambient temperature telescopes of the same diameter.

Because of the need for replenishment of the cryogen, the SIRTF is a logical facility for operation in the Spacelab. This places stringent requirements on Shuttle-generated contamination. Current planning of the cooled telescope should keep open the option of a free-flying observatory revisited by the Shuttle in case the contamination requirements cannot be met by the Shuttle.

(b) AMBIENT-TEMPERATURE SPACE OBSERVATORIES Large ambient-temperature space telescopes provide for high spatial resolution, precision photometry, and high-resolution spectroscopy throughout the infrared. In particular, infrared use of the LST offers unique performance not available in any other existing or proposed instrument. The high optical quality of the LST will permit diffraction-limited performance at all infrared wavelengths. A 2- to 3-m ambient temperature telescope (LIRTS) for Spacelab is under study in Europe. This telescope would provide lower resolution than the LST but could be optimized to achieve greater sensitivity and could realize the flexibility of interchanging focal plane instruments offered by Spacelab operation.

(c) SMALL INFRARED PAYLOADS A variety of small infrared payloads have been suggested for studying the early history of the universe. If the present view of cosmology is correct, the 3 K background is the residual radiation from the optically dense plasma just prior to the time that radiation broke loose from matter. The 3 K radiation is as far back into cosmological evolution as can be "seen" and, therefore, is one of the few clues to the early history of the universe. Measurements of deviations from a thermal spectrum and of anisotropies in the angular distribution of the background radiation would provide clues to the details of this early history. Another difficult but important measurement is the determination of the diffuse background between 1 and 30 μm. The energy released in the uv at an epoch when galaxies were just beginning to condense out of the primordial gas would now appear in the infrared and would provide information on this period.

(d) LONG-RANGE PROSPECTS The long-range prospects for infrared astronomy include (1) a long-baseline infrared interferometer with two high-performance space telescopes, (2) a second-generation (2.5-m) cooled telescope, and (3) a large (10-m) uncooled telescope.

4. RADIO ASTRONOMY

(a) ORBITAL RADIO INTERFEROMETERS One of the most powerful of astronomical techniques is the formation of a radio interferometer from two radio telescopes greatly separated in distance, and "connected" by radio lines or through the correlation of tape recordings of the signals received by the telescopes. Such systems, called very-long-baseline interferometers, can achieve resolutions as good as 0.001 sec of arc, but with poor image quality at present. Their image quality has been poor because only a small number of antenna pair separations and orientations were available utilizing earth-based antennas. The use of an antenna in orbit is a remarkably effective means of overcoming these limitations and gives promise of producing high-quality image information for small objects of great interest such as quasar nuclei and galactic water vapor clouds.

A particularly appealing orbiting radio interferometer (ORI) system would employ the Space Shuttle to carry one telescope, while the second would be on the ground. If the earth-based telescope were the Arecibo telescope, the orbiting antenna would need to be only 8 feet in diameter to achieve the sensitivity of terrestrial interferometers. If the antennas of the Deep Space Net were used, a 30-foot antenna on the Spacelab would be desirable. With either system, the combination of the orbital motion of the spacecraft and the rotation of the earth would, in less than a day, provide so many effective interferometer baseline lengths and orientations that the data could be converted into a superb image of the object of interest. Such "pictures" could in principle have a quality that approaches the performance of a single telescope 1.3 to 2.0 earth radii in size.

Another promising application of such interferometers is to achieve extremely high resolution by placing an antenna in an orbit with an apogee of tens of thousands of kilometers. This would call for a free-flyer, considerably more sophisticated than with a Spacelab-borne system. This system was described in more detail in the SSB report, *Opportunities and Choices in Space Science*, 1974.

5. HIGH-ENERGY ASTROPHYSICS

High-energy astronomy encompasses the studies of cosmic gamma rays, energetic particles, and x rays. Its scope includes all astronomical objects, and its results have brought to light unexpected objects and phenomena of great astrophysical interest as well as new aspects of objects already known to optical and radio astronomy.

(a) X-RAY ASTRONOMY The most highly developed of the three disciplines is the area of x-ray astronomy, where the exploratory phase has culminated recently in the publication of comprehensive catalogues of x-ray sources, the identification of many individual sources with both galactic and extragalactic objects, and the discovery of x-ray binaries and, probably, black holes. This discipline now confronts a large body of specific observational tasks requiring instruments of high angular, temporal, and spectral resolutions. The progress of x-ray astronomy will be served best by a program that includes orbiting observatories in the class of National Facilities with capabilities comparable with what can be achieved with optical and radio telescopes.

The National Facilities in x-ray astronomy are the 1.2-m telescope, LAMAR, and eventually a 2.2-m telescope. In addition to these are the requirements for individual experiments, using Spacelab, the high-energy astrophysics free-flyers (HEAO Block II), and Explorers. These programs, the x-ray National Facilities and the free-flyers, have been discussed in the report of the Ad Hoc Committee on High Energy Astrophysics* and the SSB report, *Opportunities and Choices in Space Science, 1974*. Specifically, in the SSB report, the 1.2-m telescope was recommended for a fiscal year 1978 start to be followed by a fiscal year 1979 start for one free-flyer. These programs are now in a phase A study at Marshall Space Flight Center.

(b) GAMMA-RAY ASTRONOMY The results from the initial exploratory satellite work (SAS-2) have determined the broad features of gamma rays from the galactic disk and several discrete sources. The mysterious and completely unexpected phenomenon of gamma-ray bursts is among the most recent discoveries in this field. Gamma-ray astronomy will undoubtedly yield further important information as instruments of greater sensitivity and improved angular and energy resolution are brought to bear on its problems. A number of different individual experiments are needed to achieve this progress. These are described in the 1974 report of the Ad Hoc Committee on High Energy Astrophysics.* No role is presently seen for a mission in the class of a National Facility.

In addition to Spacelab, and the HEAO free-flyers, the Explorer missions are a key element in the gamma-ray astronomy program. One of these, the GRE, is recommended for initiation now.

(c) COSMIC-RAY ASTRONOMY The investigation of energetic particles is the oldest of the three disciplines of high-energy astrophysics. Its objectives are well defined, and recent technical developments promise a rich field of new results. Accurate determinations of elemental composition and energy spectra of electrons and nuclei throughout the entire periodic table of the elements are now possible. Experimental techniques are emerging, capable of determining the isotopic com-

* Ad Hoc Planning Group of High Energy Astrophysics Management Operations Working Group B, "A Program for High Energy Astrophysics, 1977–1988" (July 15–18, 1974).

position and, hence, details of the cosmic-ray sources. The experiments required for further progress in this discipline are inherently of an individual character and will not require National Facilities of the kind anticipated for optical and X-ray astronomy. Certain central, technical facilities, however, will be needed in this field (i.e., cryogenically cooled superconducting magnets) that would benefit several experiments.

Cosmic-ray experiments are being developed and are in a state of readiness for the Spacelab, for the HEAO free-flyers, and for Explorer missions. It is anticipated that additional Explorer missions will be initiated in the coming years based on the response to the NASA soliticitations of 1974. Those opportunities must continue in the Shuttle era.

In addition, cosmic-ray experiments on deep-space missions are needed to elucidate the characteristics of the lower energy particles and their interactions with the interstellar gas.

All three subdisciplines of high-energy astrophysics have attracted outstanding groups of scientists who have developed technologies, instrumentation, and ideas that are ready to utilize fully the expanded capability afforded by early Spacelab operation modes. The Shuttle provides heavy payload capability, freedom from atmospheric interference, long exposure times, and frequent flight opportunities, all of which are essential to the full realization of the potential discoveries in this field.

IV. RECOMMENDATIONS

1. The Large Space Telescope will be a major step in the history of observational astronomy and will represent one of the most important scientific projects ever to be carried out by NASA. We *recommend* that a 2.4-m LST be given the highest priority for a new start in fiscal year 1977.

2. Recognizing the importance of solar-flare studies to astrophysics and the strong endorsement of the mission by the SSB Study on Solar Physics, we *recommend* a new start for the Solar Maximum Mission in fiscal year 1977. This will permit observations at the upcoming solar maximum. We also concur with the judgment of the Solar Physics Study that if additional delay in the SMM program occurs, the objectives of the mission would be so compromised that the mission should be canceled.

3. To assure that significant science results from the early Spacelab flights, a fiscal year 1977 new start is urgently needed for the development of astronomy and astrophysics payloads. We *recommend* a fiscal

year 1977 new start for Shuttle payload development, with emphasis on individual experiments. We also *recommend* that an Announcement of Opportunity for such experiments should be made in the fall of 1975, so that selection of experiments for the engineering and first Spacelab flights can be completed by the summer of 1976.

4. We *recommend* the initiation of two Explorer missions, the High Energy Gamma-Ray Explorer (GRE) and the Infrared Survey Explorer (IRSE) in fiscal year 1977. Both of these missions are of foremost importance, ready for implementation, and promise highly significant results in unexplored areas.

5. We *recommend* continuing effort on the part of NASA toward early implementation of an out-of-the-ecliptic mission (OOE).

6. We *recommend* increased funding for supporting research and technology (SR&T), sounding rockets, and scientific balloons and continued support for the airplane program. These recommendations were also made in the 1974 report of this committee, where detailed justifications were given. They become increasingly urgent as the erosion of purchasing power progresses. In addition, we believe that the current level of support for data analysis and for theoretical studies in areas related to the scientific goals of the NASA astronomy program are inadequate. We therefore *recommend* an increase in the NASA funding available for supporting theoretical studies, as well as for the analysis of data.

7. Recognizing that several astronomy disciplines, notably solar physics and cosmic-ray astronomy benefit from the availability of space on planetary missions and space-physics missions, we *recommend* that the practice of making adequate payload space available in these missions continue.

8. The Explorer program has been the source of many discoveries of space astronomy and astrophysics, and it has laid the scientific and technical foundations for most of the larger missions. The large number of promising proposals for Explorer-class astronomy and astrophysics experiments received by NASA in response to last year's Announcement of Opportunity demonstrates the continuing need for small and relatively inexpensive satellites to carry out a variety of astronomical goals. With the current level-of-effort budget for the Explorer program ($33 million/year) many important proposals must go unsupported for lack of funds, particularly in view of an anticipated increase in the use of the program by other disciplines. Therefore, we *recommend* that the Explorer budget be increased by $10 million/year, which would support one additional Scout Explorer per year or one additional Delta Explorer per 3 years.

9. We *recommend* that a study be made by a representative group of

radio astronomers to establish the configuration of desirable radio-astronomy instruments in space. Particular emphasis should be placed on the use of the Shuttle to implement orbital radio interferometers.

10. We *recommend* that NASA continue its formal studies of the 1.2-m x-ray telescope, LAMAR, and the HEAO Block II spacecraft in order that these programs may be ready for new starts in the near future.

11. We *recommend* a continuing technical study toward the feasibility of the cryogenically cooled infrared telescope facility (SIRTF) for Spacelab flight.

V. ACKNOWLEDGMENTS

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VI. GLOSSARY

A. Approved Programs

IUE—International Ultraviolet Explorer
  Optical and uv:
    Observing facility for high-resolution uv spectra of stars of all types.
ISEE—International Sun–Earth Explorer
  Solar:
    X-rays.
    Energetic solar nuclei and electrons, fluxes, spectra, anistropies.
  Cosmic Ray:
    High-resolution elemental composition.
    Isotopic composition $Z = 1$ to 26 at low energies.
HEAO-A—High Energy Astronomy Observatory (A)
  X-Ray:
    High-sensitivity survey, diffuse x-ray background.

Gamma-Ray:
Low-energy gamma-ray survey.
HEAO-B—High Energy Astronomy Observatory (B)
X-Ray:
Accurate location and spectra of x-ray sources with a grazing-incidence telescope. Four focal plane instruments.
HEAO-B will be operated as a national facility.
HEAO-C—High Energy Astronomy Observatory (C)
Cosmic Ray:
Elemental composition of nuclei with $Z > 26$.
Isotopic composition of nuclei at high energy.
Gamma-Ray:
Search for celestial gamma-ray lines.
MJS—Mariner Jupiter Saturn
Cosmic Ray:
Determine the cosmic-ray intensity and composition as function of distance from the sun. Measure low-energy galactic cosmic rays.

B. Candidate Programs

Spacelab SPI—Special Purpose Instruments (individual, principal-investigator-initiated experiments on pallets carried by Shuttle)
Solar:
X-ray imaging and spectroscopy.
White-light coronagraph.
Optical and uv:
High-resolution far-uv spectroscopy, photometry, polarimetry.
Ir:
Narrow-band photometry.
Spectroheliograph.
Magnetograph.
X-Ray:
Large-area x-ray detector with concentrator.
Sources at high energy.
Spectroscopy, polarimetry.
Gamma-Ray:
High-energy gamma rays.
Low-energy gamma rays and nuclear lines.
Cosmic Ray:
Composition and spectra to 10^{13} eV.
Electron–positron ratio.
Isotopic abundance.

Explorer—General Purpose Satellites in Various Orbits

- Ir:
  - Infrared Survey Explorer (IRSE).
  - First survey satellite, 7 μm to 30 μm.
- X-Ray:
  - Long-term monitoring of galactic x-ray sources.
  - Patrol of x-ray bursts.
- Gamma-Ray:
  - Gamma-Ray Explorer (GRE).
  - 25 MeV to 10^4 MeV.
  - Map of the galaxy, gamma-ray sources.
  - Low-energy gamma rays, line spectra.
  - Gamma-burst patrol.
- Cosmic Ray:
  - Elemental abundance.
  - Isotopic abundance of solar and galactic particles.

ORI—Orbital Radio Interferometer

- Radio:
  - Radio antenna, either carried on Spacelab or launched as a free-flyer, correlated with a large ground-based antenna.

Out-of-the-Ecliptic Mission

- Cosmic Ray:
  - Particle distribution at high solar latitudes.
  - Structure of the interplanetary medium.
  - Particle transport.
- Solar:
  - Solar wind at high solar latitudes.
  - Magnetic-field structure.

HEAO Block II—High Energy Astronomy Observatory (Block II) (Shuttle-launched standardized free-flyer)

- X-Ray:
  - Large-area array.
  - Bragg crystal spectrometer.
  - Bragg crystal polarimeter.
  - High-energy source study.
  - Hard x-ray imaging.
  - All-sky monitor.
- Gamma-Ray:
  - Wide-field, high-energy gamma-ray telescope.
  - Narrow-field, high-energy gamma-ray telescope.
  - Low- and medium-energy gamma rays.
  - Medium-energy gamma-ray burst monitor and spectrometer.

High-resolution gamma-ray spectrometer (0.05–10 MeV).
Cosmic Ray:
High-energy spectra, charge composition, arrival direction.
Elemental abundance of superheavy nuclei.
Isotopic abundances.
Electron and positron energy spectra.

MJU—Mariner–Jupiter–Uranus
Cosmic Ray:
Particle spectra and composition at large heliocentric distances

LST—Large Space Telescope (2.4-m)
Optical, uv, and ir: cameras, photometers, spectrographs.

SUOT—Shuttle UV–Optical Telescope (general-purpose 1-m class telescope)

VLST—Very Large Space Telescope (multimirror telescope 10-m class)
Optical, uv, and ir: cameras, photometers, spectrographs.

SIRTF—Shuttle Infrared Telescope Facility (1- to 2-m cryogenically cooled telescope)

SMM—Solar Maximum Mission
Solar: Study of solar flares and active regions on the sun.

SSS—Solar Synoptic Satellites (reflights, after refurbishment, of the Solar Maximum Mission spacecraft)
Solar:
Coronal structures, active regions, streamers.
Monitor solar "constant."

Sortie Telescopes
Solar:
General-purpose 1-m class uv telescope.
Xuv telescope.
Soft x-ray telescope.
Hard x-ray imaging facility.
Euv telescope.

LSO—Large Solar Observatory
1.2-m Telescope
Focusing x-ray telescope of 1.2-m aperture.
Several focal-plane instruments

LAMAR—Large Area, Moderate Angular Resolution X-Ray Detector
X-Ray: Sky survey, x-ray structure of galaxy, x-ray background, absorbing matter, imaging of large-scale features.

2.2-m Telescope
Focusing x-ray telescope of 2.2-m aperture.
Several focal-plane instruments.

APPENDIX A: LARGE SPACE TELESCOPE

The Large Space Telescope (LST) is presently envisaged as a 2.4-m aperture, Ritchey-Chrétien telescope with a limiting resolution of better than 0.1 sec of arc. The wavelength range that can be covered by the telescope and associated instruments is from 1100 Å to 1 mm. The faintest star that can be detected above sky background with 10:1 signal-to-noise ratio is about visual magnitude 27 (4 magnitudes fainter than the ground-based limit). The scientific instruments that are currently being studied for use with the LST are described below.

High-Resolution Camera

This instrument consists of three subsystems: an *f*/24 field camera, an *f*/48–*f*/96 planetary camera, and an *f*/96 faint-object camera. The first camera does not operate at the diffraction limit but has a wide field of view useful for search and survey work. The latter two cameras provide diffraction-limited-resolution imagery of bright and faint objects, respectively. The wavelength range is 1150–8000 Å.

Faint-Object Spectrograph

This instrument provides moderate- to low-resolution spectra ($\lambda/\Delta\lambda$ = 100 to 1000) of selected objects with very high (photon-limited) sensitivity. It also retains the very high spatial resolution (0.1 sec of arc) provided by the telescope in the direction along the spectrograph slit. Thus, stellar objects as faint as visual magnitude 23 can be studied. An optional high-resolution mode ($\lambda/\Delta\lambda = 10^4$) can be incorporated. The wavelength range of 1000–8000 Å will be covered by two or three separate subsystems.

High-Resolution Spectrograph

This instrument operates in the 1150–4100 Å wavelength range, with spectral resolutions of 3×10^4 and 10^5. A low-resolution mode (10^3) can also be utilized. Its primary objective is spectroscopy in the ground-inaccessible ultraviolet.

Infrared Photometer

This instrument is intended for narrow-band photometry in the 2–1000 μm wavelength range, with diffraction-limited resolution at wavelengths longer than 10 μm. Spectral resolution is determined by the particular filter used (a total of 47 filters will be available). The field of view is determined by selectable circular apertures.

Astrometry

The astrometry experiment has the objectives of (1) measuring parallaxes and proper motions of stars 10 times more accurately than is presently possible, (2) measuring the angular diameters of stars and the nuclei of galaxies, and (3) determining individual stellar masses through observations of spectroscopic binaries (unresolved by ground-based telescopes). A number of methods for accomplishing these measurements are currently being studied.

Ultraviolet–Visible Photometer

This device is used for accurate brightness measurements of various objects with high spatial and time resolution, using selectable apertures and filters. The wavelength range is 1150–6500 Å.

APPENDIX B: SOLAR MAXIMUM MISSION

The Solar Maximum Mission (SMM) will contain eight scientific instruments, covering a broad spectral range, that will simultaneously observe the same region of sun to study all the radiation emitted during the lifetime of a solar flare. Although the final selection of instruments has not yet been made, a proposed instrument complement is described below.

SMM Instruments, Typical Payload	Spectral Range	Spatial Resolution
Solar gamma-ray and neutron monitor	0.1–150 MeV	Full sun
Hard x-ray, high-time-resolution spectrometer	20–300 keV	Full sun
Hard x-ray imaging spectrometer	3.5–20 keV	8 × 8 sec of arc
X-ray polarimeter	5–1000 keV	Full sun
Soft x-ray imaging spectrometer	1–23 Å	10 × 10 sec of arc
Xuv spectroheliometer	40–630 Å	4 × 4 sec of arc
High-resolution uv spectrometer and polarimeter	1000–3000 (4000) Å	≤3 × 3 sec of arc
White-light coronagraph	4000–7000 Å	≥8 × 8 sec of arc

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E
Infrared and Submillimeter Astronomy

I. INTRODUCTION

In the last ten years, new and sometimes revolutionary results have come from observations in the infrared, revealing large unexpected amounts of energy. Stars were found that emit most of their radiation in the infrared, unveiling new chapters in the story of stellar evolution, both in the last stages and especially in the very early stages, where stars, stellar associations, and planetary systems are formed. The discovery of intense infrared emission from galactic nuclei and other extragalactic sources compounds the difficulties in understanding the most energetic bodies in the universe. Excess infrared emission from the planet Jupiter was measured, revealing the existence of an internal source of energy. The common understanding of the energetics of H II regions was dramatically altered by the discovery of great amounts of infrared emission. In the case of the 3 K cosmic background, infrared measurements have established that the spectrum has a peak and is consistent with the interpretation of its origin as the primordial black-body radiation.

Infrared observations of circumstellar and interstellar dust have led to the current understanding of grain composition and origin. The extreme visibility of dust in the infrared accounts for its prominence throughout the universe. But it is also the relative transparency of the universe in the infrared that permits areas such as the center of the

Galaxy to be studied in detail, revealing not only the ordinary stars but also many extraordinary objects.

The enormous impact infrared observations have made on astronomy and astrophysics has so far been the result of comparatively modest efforts made, for the most part, with existing telescopes or inexpensive aircraft and balloon systems. NASA has just put into operation the first large-scale infrared facility consisting of a 91-cm telescope mounted in a large jet transport (the Kuiper Airborne Observatory), and already it has detected water vapor in the atmosphere of Jupiter.

We are now at the threshold of a new era, in which large and ambitious programs may be undertaken in space in order to take advantage of long-lived cryogenic systems and more than two-orders-of-magnitude improvement in detector sensitivity. The purpose of this report is to show how these space programs will help in the solution of many fundamental problems in astronomy.

In addition to specific recommendations, a basic plan is shaped, which, if implemented, will ensure a more rapid rate of progress than has been achieved in the past decade. The essential elements of this plan are (a) a satellite sky survey and the related balloon and ground-based surveys that will provide the data needed to exploit fully the larger space facilities; (b) development of a large (1- to 1.5-m) cryogenically cooled telescope that will capitalize on the enormous sensitivity of ultralow-background infrared detectors—depending on the outcome of contamination studies, this instrument would be operated in Spacelab or as a free-flyer to be launched and retrieved by the Shuttle; (c) utilization of the considerable capabilities of the Large Space Telescope as an infrared telescope to provide high angular and spectral resolution at critical wavelengths not accessible by any other means.

II. SCIENTIFIC PROBLEMS

Infrared astronomy reveals the cool states of cosmic matter—solids and molecules—and the earliest stages of the universe. Since the temperatures of cosmic solids are between 3 and 2000 K, most of their emission is in the infrared. The energy of many sources such as galactic nuclei emerges in the infrared, frequently because of the high opacity of solids in the visible and ultraviolet and the extreme visibility of dust in the infrared. Radio astronomers have discovered a seemingly endless number of molecules in infrared stars and nebulae, demonstrating clearly the intimate relation between radio and infrared astronomy. Most of the vibrational and rotational transitions of molecules occur in the infrared. The energy of the primeval fireball and of the first galaxies appears in the infrared in the present epoch.

Infrared astronomy can make basic contributions wherever cosmic solids or molecules are encountered—in the solar system, in molecular clouds and the interstellar medium, in protostars, and in galactic nuclei. It is essential for determining the energetics of many sources. In addition, infrared observations are one of the few ways to probe the early development of the universe.

A. Solar System

The solar system has a special significance because it is our home. An understanding of the solar system also has a broad impact on problems related to the interstellar medium and star formation. Important aspects of the process of star formation and of the nature of the interstellar medium can only be explored in the solar system. In particular, no other planetary system can be studied in the detail possible for the solar system.

Infrared spectroscopy is one of the most powerful and broadly applicable tools for determining the chemical and isotopic composition of the sun and planetary atmospheres. It also provides basic data on atmospheric temperature structure, which is essential for understanding planetary meteorology and atmospheric dynamics. Taken together, these subjects bear on the composition of the protoplanetary plasma and the processes by which planets condense.

Important clues about the condensation of solid material can be found through infrared studies of comets, asteroids, and zodiacal light. Studies of meteors and laboratory simulations of cosmic material can be directly connected to observations of the interstellar matter and circumstellar shells. For example, infrared observations have shown that silicate materials of the kind that compose the mantle of the earth are present in comets, in the interstellar medium, and in the circumstellar shells of cool stars.

B. Molecular Clouds

The discovery that dense molecular clouds represent a significant fraction of the mass of interstellar gas is one of the most recent and exciting accomplishments of astrophysics. Because of the high cosmic abundance of hydrogen, H_2 is by far the most common interstellar molecule. A large number of more complex molecules have also been discovered, some consisting of as many as nine atoms. Since most of the rotational and all the vibrational transitions of interstellar molecules fall in the infrared, and since certain molecules (light hydrides like HCl, for example, and nonpolar molecules like CO_2 or CH_4) can probably only be studied in the infrared, spectral observations in the infrared are crucial to an understanding of the many questions raised by these discoveries.

How and why do interstellar molecular clouds collapse and fragment into associations and clusters of stars? How do complex organic molecules like ethyl alcohol and dimethyl ether manage to form in the cold and rarefied environment of interstellar space? And what implication, if any, does the cosmic synthesis of these substances hold for the chemistry of the solar system and the primordial earth? These questions rank among the most hotly debated and fundamental issues in current astrophysical research, with broad implications for other areas of science.

A number of more traditional astronomical subjects will also benefit greatly from the wealth of data provided by infrared spectral observations of molecules. Isotopic ratios, for example, can be comparatively easily measured in the infrared spectra of molecules. Such observations can be expected to provide detailed information on the nuclear evolution of the Galaxy since the formation of the solar system some five billion years ago.

C. Star Formation

After years of theoretical speculation on the formation of stars, infrared observations are now providing observational data on this complex problem in astrophysics. In recent years, infrared and radio observations have revealed the existence of stars in a very early stage of stellar evolution. These stars, which are embedded in dense molecular clouds of dust and gas, require infrared observations to determine many of their physical properties including the important question of their evolutionary stage.

The properties of regions of star formation on which infrared observation will have a direct impact include total luminosity, dynamics, structure, composition, and temperature of individual protostars and associated dust clouds. In particular, the total luminosity of a system and the dust temperature require photometric observations at all infrared wavelengths. Determination of the structure and size of protostars and of the dense dust clouds in which they are immersed needs high-angular-resolution observations at infrared wavelengths, especially beyond 30 μm. High spectral resolution ($\lambda/\Delta\lambda$ of 10^2 to 10^5), when coupled with good spatial resolution, can be used to observe various molecular lines and bands and thus determine the composition, density, temperature, and mass of both the dust and gas within the clouds. With a spectral resolution of 10^5 it will be possible to measure the radial velocities of individual components and thus attempt to determine the dynamics of the system. Measurements of the polarization of the infrared radiation may give information on magnetic fields in the clouds.

The most important contribution of infrared observations to the understanding of star formation will come in establishing a full sequence of protostellar objects. Surveys at all infrared wavelengths should reveal enough different objects so that a full evolutionary sequence of objects similar to the stellar Hertzsprung-Russell diagram can be made.

D. Galaxies and Quasars

The energetics of extragalactic sources—how quasars and galaxies sustain their immense luminosities—is recognized to be one of the most fundamental problems in physical science. Its solution will provide new insights into the formation and evolution of galaxies and into the application of physical laws to extreme conditions. It may even result in important modifications or extensions of the laws of physics.

Only a few hundred galaxies and quasars have been observed in the infrared, and only eight between 25 μm and 1 mm, compared with many thousands in the radio and optical regions of the spectrum. Nonetheless, it appears that some of these sources emit predominantly between 5 and 100 μm, so that many more infrared observations are needed to fill a serious gap in our knowledge of extragalactic sources.

In addition to measurements of known high-luminosity galaxies, sensitive infrared searches for additional sources are needed. Together with radio, optical, ultraviolet, and x-ray studies, these surveys will explore the full variety of energetic extragalactic phenomena. Unstudied phases of galactic evolution—such as very young galaxies or dust-embedded sources—may be revealed.

The center of the Galaxy deserves special attention in the infrared because of the high spatial resolution that can be achieved there and because optical studies are prevented by the large interstellar extinction. The study of the fascinating complex of infrared sources in the galactic center must be extended to the highest possible limits of angular and spectroscopic resolution in order to determine the nature of the sources and the dynamics of the region. The nuclei of other galaxies also need to be studied in detail to learn the origin of the infrared radiation, whether, for example, it is thermal or nonthermal, and to explore its relation to the sources of radio, optical, ultraviolet, and x-ray flux.

E. Background and Cosmology

Precious little observational evidence is available to decipher the cosmological riddle of the origin, dynamics, and evolution of the universe. The fundamental cosmological questions are: How did it all start? Was there a primeval cosmic explosion? Is the universe on a grand scale homogeneous and isotropic? Will the universe expand forever, or will it collapse again? How and when did galaxies form?

Observations in the infrared region of the spectrum address almost all of the cosmological questions. The 3 K background radiation has the bulk of its energy in the millimeter and submillimeter region. A sensitive survey for extragalactic objects may uncover specific infrared sources missed by radio and optical surveys and thereby revise the estimates of the mass density of the universe.

A fruitful way to study the early universe is to measure the properties of the diffuse background at all wavelengths. If the present view of cosmology is correct, the 3 K background is the residual radiation from the optically dense plasma just prior to the time that radiation broke loose from matter. The 3 K radiation is as far back into cosmological evolution as can be "seen" and therefore is one of the few handles on the early history of the universe. Deviations from a thermal spectrum would be strong evidence that the presently accepted cosmological history is wrong. Inhomogeneities in the primordial plasma as well as nonspherical expansions of the universe would appear as anisotropies in the angular distribution of the background radiation. Furthermore, it is expected that an overall anisotropy should be measurable because of the earth's motion relative to the reference frame defined by the 3 K radiation.

Another important but difficult measurement is the determination of the spectrum of the diffuse background in the region between 1 and 30 μm. If the "big-bang" cosmology is correct, there would have been an epoch when galaxies were just beginning to condense out of the primordial gas. The energy released at this epoch in the ultraviolet would now appear as a faint glow in the infrared. A measurement of the diffuse background in the 100-μm region would help to complete the picture of the energy content of the universe.

F. Other Problems of Interest

In the previous sections we have presented only a few of the exciting scientific problems to which infrared astronomy has addressed and will address itself. There are, in addition, a wide variety of other galactic and extragalactic problems for which infrared observations play a key role. One such example is the understanding of H II regions around bright early-type stars. The luminosity from an H II region comes out predominantly in the infrared. By studying the spatial distribution of infrared radiation at many different wavelengths with good angular resolution, it will be possible to learn how H II regions evolve. High spectral resolution can be used to determine the composition and density of neutral gas surrounding the H II region.

Another important type of object for infrared studies is the dust-imbedded late-type stars. Dust and the formation of dust also play an important role in novae and planetary nebulae. The size, mass, and composition of the dust cloud and the mass loss rate from these objects can be determined through high angular and spectral resolution in the infrared. These quantities in turn are extremely important in the understanding of the formation of interstellar dust gains and the origin of interstellar material. Infrared observations have been, and will continue to be, critical to the understanding of a wide variety of astronomical problems, such as ordinary stars and stellar systems, x-ray sources, supernovae, and pulsars.

III. PRESENT STATUS AND FUTURE DEVELOPMENT

In the following sections we discuss the state of development of ground-based telescopes and instruments used from aircraft, balloons, and rockets. Fundamental limitations to the performance of different systems are discussed, with indications of how closely such limits have been approached. In some cases the technology has not been pushed to within an order of magnitude of the optimum, and significant improvements are possible.

A. Ground-Based Observatories

Most major discoveries in infrared astronomy have come through ground-based observations. Ground-based telescopes will continue to be important because of their versatility and economy of operation. In addition, these instruments have been developed to high efficiency, so they form the standard of comparison for other techniques.

For infrared photometry between 2.5 and 40 μm with small fields of view, large ground-based telescopes approach fundamental limits of performance set by their environment, i.e., background photon noise and atmospheric transmission. Telescopes up to 2.3 m in aperture have been successfully modified to reduce the background noise to within about a factor of 2 of that from the sky alone. In 1 hour of integration, the largest telescopes can measure sources as faint as 5×10^{-5} Jy at 2 μm and a low-background 1.5-m telescope can measure sources of 0.025 Jy at 10 μm. Absorption by water vapor becomes increasingly severe beyond 10 μm, so that the 1.5-m instrument requires an hour to measure a source of 3 Jy at 34 μm even under exceptionally dry atmospheric conditions. Ground-based observations with large fields of view are severely limited by high background and by excess noise from atmospheric thermal fluctuations.

The only practical means of improving these performance figures is to build larger telescopes at good sites. The planned 3-m instrument for Mauna Kea should better the performance of the 1.5-m telescope discussed above by about a factor of 4 when the seeing permits operation with a diffraction-limited (3–6 sec of arc) field of view.

Atmospheric absorptions are a problem for infrared spectroscopy, particularly in planetary work, where the molecules of greatest interest are usually abundant in the atmosphere of the earth. Nonetheless, ground-based infrared spectroscopy is a rapidly expanding and productive field. Between 1 and 5 μm, Fourier transform spectrometers are frequently used at resolutions up to the order of 10^4; short of 2.2 μm, these instruments are limited by detector noise and therefore achieve a multiplex advantage. Beyond 2.5 μm, spectrometer performance is limited by background noise, and most work has been done at low resolution ($\lambda/\Delta\lambda \sim 100$). Higher resolution instruments, some with small arrays of detectors to increase their speed, are coming into increasing use in this spectral region. Relatively little ground-based spectroscopy has been done between 15 and 40 μm because of the large number of atmospheric absorption lines at these wavelengths.

Since efficient all-sky surveys require large fields of view, ground-based sky surveys at wavelengths beyond 3 μm are not practical. The Cal Tech 2-μm sky survey was complete to about 40 Jy at 2.2 μm. The extension of the 2-μm catalogue to the southern hemisphere would be extremely useful. Such surveys, however, detect primarily normal, already known stars.

B. Airplanes

The NASA C-141 (Kuiper Airborne Observatory) and Lear jet with their 91- and 30-cm telescopes, respectively, provide unique observing platforms for infrared astronomy. Important advantages of these platforms are that they give the astronomer the opportunity to interact directly with his instruments and provide a quick response time to new discoveries. The Kuiper Airborne Observatory (KAO) has recently become operational (1974) and is demonstrating performance and operational characteristics of instruments and airborne astronomical systems. It has achieved pointing stability of 2 sec of arc rms on 10th magnitude stars and pointing accuracy of about 5 sec of arc while operating at 12.5-km altitude with flight durations of 7 hours. Present observations of solar system objects, H II regions, planetary nebulae, and galaxies have increased the wavelength baseline in order to better understand the energetics of these objects.

The KAO provides the capability for making the photometric measurements in the regions of the spectrum for which the atmosphere is totally opaque from the ground and for making spectroscopic measurements at wavelengths where the atmosphere is relatively transparent but where spectral lines of the astronomical objects come close to and are confused by atmospheric lines. As operations become more efficient and the first-generation instruments are followed by more sensitive, sophisticated instruments, the KAO will provide the capability for attacking a number of central astronomical questions.

The limitations of aircraft for infrared astronomy are caused by the residual atmosphere. At present, aircraft observations are affected by "sky noise," presumably due to air turbulence. It is not clear whether this sky noise can be eliminated from airplane platforms in order to reach the background photon noise limitations imposed by the environment.

The Lear jet, while supporting a smaller telescope, remains a valuable tool for infrared astronomy. It is particularly useful because of its ability to fly to higher altitudes of 14 km, where the overburden of water is considerably reduced; it permits mapping of extended objects; and it makes available much needed opportunities to test techniques that can be used later on the C-141.

Both aircraft play important roles for future Shuttle experiments, providing test opportunities for instrumentation and training for young scientists in the field.

C. Balloons

The use of balloonborne telescopes to make far-infrared observations at wavelengths that are inaccessible from the ground is now well established. Many important discoveries have been made, in particular a large number of objects such as H II regions have been observed that radiate predominantly at 100 μm with an energy output many million times greater than that of the sun. The galactic center has been mapped, and luminous galaxies have been observed at these long wavelengths. Measurements of the spectrum and angular distribution of the 3 K cosmic background radiation have established the thermal nature of this radiation and have demonstrated a high degree of isotropy.

In contrast to aircraft, rockets, and satellites, balloon measurements have been made with relatively unsophisticated equipment, and great improvements are possible. At present, the largest infrared balloon payload is a 1-m telescope with 30 sec of arc angular resolution and modest pointing stability. The best stabilized (30 sec of arc) is a 40-cm telescope. Other balloon instruments include a liquid helium-cooled interferometer for measurements of the cosmic background, an ambient 20-cm low-emissivity survey telescope, and differential radiometers at several wavelengths to measure the isotropy of the cosmic background.

Major scientific goals at the present time are to make high-resolution spectral and spatial observations of individual sources, to use low-background ambient-temperature telescopes to make all-sky surveys at low angular resolution, and to continue efforts to measure both the spectrum and isotropy of the cosmic background. Of special significance is access to the 28.2-μm molecular hydrogen line.

Major efforts are required in order to capitalize on the low-background conditions available at balloon altitudes. In particular, better detectors, low-emissivity telescopes, finer pointing systems, larger lightweight mirrors, high-resolution spectrometers, and extension of balloon flights to many days are needed.

Beyond 30 μm, the currently available detectors are relatively insensitive, so that balloonborne surveys would not be background-limited. Therefore, the comparative sensitivities of balloon and satelliteborne experiments will depend mostly on the relative exposure times. With present detector sensitivities, a major balloon survey at wavelengths around 100 μm could attain results similar to those from a satellite. This situation will change when more sensitive detectors are available for the 30–300 μm regions. As a specific example, a balloonborne instrument using a 25-cm telescope and 10 detectors with a NEP of 10^{-14} W/Hz$^{1/2}$ could be designed to survey the sky to ~100 Jy near 100 μm. Substantially better detectors could be used to maximum advantage only in a satellite.

Measurements of the atmospheric background at balloon altitudes show that, beyond 30 μm, detector-limited operation can be achieved, leading to a distinct advantage over observation at airplane altitudes. In addition, excess noise ("sky noise") is essentially absent. This makes the balloon a viable and cost-effective platform for first-rate observations at all infrared wavelengths. Ambitious programs are under way in Europe to exploit the underdeveloped potential of balloon systems for infrared astronomy. The opportunity exists for the United States to support a vigorous and competitive program in this area that can be of special benefit to small groups developing new ideas and techniques.

D. Rockets

Sounding rockets have been used to make two quite different types of infrared observation: observations of large extended objects at low spatial resolution (~1/4 degree) and sky surveys at relatively high spatial resolution (~3 min of arc).

The program for observing extended objects has detected diffuse 100-μm radiation from the galactic plane as well as small (~1 deg) dark clouds. Large H II regions have been observed at 5, 10, 20, and 100 μm, and preliminary observations of the zodiacal light have been made. The infrared brightness of the zodiacal light will limit the ultimate sensitivity of many satelliteborne instruments. It is therefore essential that these missions be well understood prior to final definition of the design of an infrared survey satellite. Two rockets will be launched during 1975 to further observe the zodiacal light.

Eighty percent of the sky has now been surveyed at wavelengths of 4.2, 11.0, and 19.8 μm to a limit of 100 Jy using a liquid helium-cooled telescope in a small sounding rocket. The results of the survey are contained in a catalogue of observations of 3200 infrared sources, which serves as a "finding list" for further ground- and space-based observations.

This program is continuing to complete total sky coverage at 11 and 19.8 μm and provide additional observations at a wavelength of 28 μm for 60 percent of the sky.

Limitations on the future use of rockets for infrared sky surveys are primarily based on their lack of cost effectiveness. Extending surveys to fainter magnitude limits requires, in general, larger telescopes and larger rockets capable of higher altitudes and extended observing periods. The necessity for repeated observations to confirm the presence of a source and to assess its variability requires a large number of rocket launches with costs comparable with those of a small survey satellite.

E. Satellites

The Celestial Mapping Program (CMP) was a classified all-sky survey carried out by the Air Force from earth orbit in 1971. Although this program was only partially successful, it could provide important data for the design of more sensitive infrared surveys. A map from this program and some 2.2-μm data are the only infrared results from satellites that have been partially declassified by the Department of Defense.

F. Expected and Necessary Technological Improvements

1. DETECTORS

The availability of detectors that can take advantage of the very low backgrounds encountered in space is critical to the success of any infrared space program. Continuing developmental programs in the solid-state sciences pursued over the last decade by the Department of Defense have resulted in photoconductor detectors for the 1- to 30-μm region that approach the theoretical limit of sensitivity under a wide range of background conditions. These detectors are now readily available both as single-element devices and complex arrays of single elements. Future developments in this spectral region can be expected to yield arrays of a large number of detector elements produced on a single solid-state chip and integrated with signal conditioning and readout electronics similar to the charge-coupled devices now available for the visible portion of the spectrum. These devices, when available, will permit infrared images to be produced in a manner completely analogous to visual photographs and at resolutions limited only by the telescope optical system. Photoconductive detectors exist that are sensitive out to 100 to 200 μm. The limiting sensitivity of present detectors decreases rapidly beyond 30 μm.

Most of the far-infrared astronomy (30 μm to 1 mm) of the last decade has been done with bolometer detectors operating at 1–2 K. The present limiting sensitivity of these detectors is not sufficient to take advantage of the low backgrounds expected in space environments.

It appears that both bolometer and photoconductive detectors for wavelengths beyond 30 μm can be significantly improved with a modest technological effort. For example, composite and metal film bolometers can substantially improve the radiative coupling, particularly in the submillimeter region. In addition, decreasing the operating temperature of a bolometer detector to 0.3 K should result in more than an order-of-magnitude improvement in the limiting sensitivity.

Finally, gallium-doped germanium photoconductors, sensitive to 120 μm, are presently limited by contact noise and material nonuniformities. Application of state-of-the-art solid-state techniques to this device promises a substantial improvement in its performance. A continuing effort to upgrade the long-wavelength performance of both bolometer and photoconductive detectors is clearly necessary.

2. FILTERS

Cooled filters are required to define the spectral range of infrared detectors and to reject unnecessary background radiation. Commercial multilayer dielectric filters with adequate properties are available for wavelengths less than 20 μm. Similar filters for the far infrared are technically feasible but not available commercially. In order to improve the performance of far-infrared detector systems, a sustained low-level program for the fabrication of high-performance filters that are rugged and that work at cryogenic temperatures is required.

3. SPECTROSCOPY

Measurement of narrow spectral lines with instrumental resolutions of 10^3 to 10^5 can be done in two ways. A high-resolution interferometric filter can be placed in front of an incoherent detector such as a photoconductor or a bolometer. The techniques for making such filters generally exist, but relatively few efficient cooled ones have been built. For studies of sources of large angular extent or in spectral regions where measurements are not background-limited there is considerable advantage in using multiplex spectrometers such as Fourier spectrometers.

Another approach to line spectroscopy is the heterodyne mixer followed by an intermediate-frequency amplifier and a square-law detector. Such receivers can have a substantial frequency multiplex advantage and are capable of extremely high spectral resolution. Heterodyne receivers are expected to be more useful at long wavelengths, and indeed they dominate radio technology. In practice, however, the only infrared mixers that approach the optimum operate at a wavelength of 10 μm, where, because of their limited bandwidth, the advantages of heterodyne over other techniques are marginal, unless resolving powers greater than 10^5 are required. It is important to develop low-noise, broad-band mixers for operation in the 50- to 1000-μm region, where they should, in principle, be the best technique for the exploration of rich molecular spectra that are shared with centimeter and millimeter radio ranges.

G. Spatial Interferometry

The first breakthroughs have been made in this area with the successful operation on ground-based telescopes of two types of spatial interferometers. Heterodyne detection at 10 μm, the infrared analogue of radio interferometry, has been demonstrated. The classical Michelson interferometer has been adapted at 5 μm to measure the diameters and shapes of several infrared stars. As explained above, at wavelengths around 10 μm, the incoherent detector using broad bandwidths yields much higher sensitivity than the coherent heterodyne detector, which has inherently very narrow bandwidths.

At present, the atmosphere limits the efficiency of ground-based interferometers in the infrared as well as limiting spectral coverage and sensitivity. Progress can be made in circumventing the atmospheric seeing, but the ultimate development of this extraordinarily powerful technique for expanding angular resolution will require large space facilities.

As an example, a two-element interferometer could be operated in the Space Shuttle. The separation of 15 m would provide the angular resolution necessary to study structures on the order of 0.05 sec of arc at 10 μm and 0.5 sec of arc at 100 μm. Such an instrument could attack the problem of structure in galactic nuclei, where the interactions and relationships between structures of similar scale at radio wavelengths may hold the key to the understanding of the energetics of galactic nuclei.

H. Cryogenic Space Systems

Two types of system are being developed for the basic long-duration ($\sim$1 year) cooling of infrared space instruments. For temperatures down to 8 K, reservoirs of supercritical He at pressures of up to 30 atm are preferred. Large systems of this type are in advanced stages of flight qualification. Utilizing multiple vapor-cooled radiation shields, multilayer insulation, and, where possible, radiative cooling of the outer vacuum enclosure, hold times of a year appear to be feasible.

For lower temperatures, superfluid He below 2 K may be used. Similar amounts of cooling are achieved with comparable weights and volumes. The problem of containment in zero gravity is potentially solved by a porous plug that separates phases and passes superfluid to allow cooling by evaporation. A limitation for large systems of this type is the motion of fluid and gas within the chamber leading to uncontrolled motion of the center of mass and "sloshing" against the walls. These problems are minimized for containers that are small relative to the spacecraft or for spinning systems.

A combination of two methods, where the telescope and part of the electronics are cooled by vapor from the supercritical He reservoir to 8 K and where a small vessel of ordinary ^4He or ^3He is used to provide temperatures from 0.3 to 5 K for the detectors, seems to offer great advantages. Future possibilities for study include mechanical refrigerators and solid cryogen systems. Both solid H_2 at about 10 K and low pressure and solid He at 2 K and 30 atm pressure offer worthwhile savings in weight and volume.

IV. PROPOSED SPACE PROJECTS

The major astronomical problems for which research in the infrared will provide essential information are discussed in Section II. Large space telescopes at ambient temperature, such as the Large Space Telescope (LST) planned by NASA and the Large Infrared Telescope on Spacelab (LIRTS) proposed by the European Space Agency, can achieve crucial advantages for these measurements. These instruments will observe all infrared wavelengths without interference from atmospheric absorptions and will be able to achieve high spatial resolution consistently without degradation by atmospheric turbulence. In addition, increases in sensitivity can be achieved by reducing the thermal background and its associated noise. Gains by several orders of magnitude will result from cooled telescopes. These benefits will be fully exploited by an infrared all-sky satellite survey and by the Shuttle Infrared Telescope Facility (SIRTF). In order that full use be made of the reduced background at spacecraft altitudes, very low levels of contamination must be maintained (see Appendix A).

A. Survey Satellite

There is wide agreement both among infrared astronomers and in the general astronomical community that better all-sky infrared surveys are urgently needed. Deep infrared surveys can be expected to yield breakthroughs much as the first radio catalogues resulted in the discovery of quasars and the *Uhuru* x-ray survey discovered variable x-ray sources associated with dying stars. It will also give infrared astronomers a complete, systematic list of objects to study.

Revolutionary improvements over existing infrared sky surveys can now be made. The 7–30 μm region is, at present, the highest priority region for a satellite sky survey. There are two reasons for this: the best available infrared detectors work in this wavelength range, and the proportion of unusual and exciting objects (both galactic and extragalactic), relative to normal stars, is higher than at shorter wavelengths. In addition, because of its extreme sensitivity, a 7–30 μm satellite sky survey provides the best opportunity for discovering unknown sources of infrared radiation. With the expected sensitivity, the survey satellite will be able to detect a significant number of the known extragalactic objects. The unbiased aspects of the survey would give the first opportunity to establish the phenomenology of infrared extragalactic objects.

The most sensitive 7–30 μm sky survey to date, the AFCRL rocket survey, is complete to approximately 100 Jy at 11 μm. Sensitive ground-based all-sky surveys at these wavelengths are not possible because of the atmospheric emission. As discussed above, improvement by more than an order of magnitude with rocketborne telescopes will involve expenses comparable with those for a satellite. Improvement by nearly an order of magnitude should be feasible with balloonborne telescopes, but improvement by a significantly larger factor would be nearly impossible with these instruments because of the background atmospheric emission. Improvement by nearly three orders of magnitude, limited only by the thermal emission of the zodiacal cloud, could be obtained with a satelliteborne survey.

The sensitivity of a satelliteborne survey can be estimated by extrapolation from previous experience. For example, the AFCRL survey was obtained in a total exposure time of approximately one-half hour. If the same instrument were placed in orbit to achieve an exposure time of approximately one month, an increase in sensitivity by a factor of 40 would result from the longer integration time per resolution element. Through the elimination of stray background and the use of current state-of-the-art detectors, the sensitivity of the AFCRL instrument can be improved by nearly an order of magnitude, so that a total increase in sensitivity by a factor of a few hundred is indicated for the satelliteborne survey, resulting in a detection limit of ~0.5 Jy. An increase of telescope aperture to 40 cm would yield a detection limit of 0.2 Jy. Calculations of the sensitivity based on the parameters of a baseline instrument and our best estimate of the zodiacal emission yield the same result. The development of this satellite would benefit from a thorough understanding of the already completed AFCRL rocket survey and the CMP satellite survey (Section III.E).

It is now possible to carry out an all-sky survey that concentrates on wavelengths in the 7- to 30-μm region and is complete to ~0.2 Jy at 10 μm. To maximize its usefulness, this survey should be able to observe spatial frequencies from 1 min of arc to 1 deg and should attain positional accuracy of 1 min of arc to allow verification and study of the sources with ground-based telescopes. The inclusion of longer wavelengths should be considered if a far-infrared channel will not compromise the performance of the instrument in the prime 7- to 30-μm spectral region. The survey should repeat sky coverage on at least a time scale of weeks to provide self-confirmation, eliminate noise pulses, identify moving sources such as asteroids, and determine the variability of the detected sources. The same vehicle could survey restricted regions to much deeper limits. For example, 1 percent of the sky could be studied to a limit of 0.02 Jy.

B. Major Space Telescopes

1. LARGE SPACE TELESCOPE

The Large Space Telescope (LST) is a free-flying space telescope that has been optimized for visual and ultraviolet spectral regions. The proposed first-generation infrared experiment on the LST consists of a photometer stressing sensitivity and high spatial resolution covering the range from 2 μm to 1 mm. Proposed second-generation instrumentation will also include a high-resolution spectrometer. Under realistic assumptions, the limiting sensitivity set by the warm mirrors of the telescope is maximum at 10 μm and improves monotonically with increasing wavelengths, equaling the sensitivity of the best present bolometers at approximately 100 μm. Thus, the LST will probably be detector-limited rather than telescope-limited for wavelengths longer than 100 μm. The high optical quality of the LST will permit diffraction-limited performance at all infrared wavelengths. The capability for high-angular-resolution observations, especially at wavelengths less than 10 μm, is a major advantage of the LST in comparison with any other proposed space telescope. Many problems related to spatial resolution in regions of star formation will require the high optical performance of the LST. The LST should not be viewed as a single-purpose instrument, however, since it retains enormous scientific capability for both precision photometry and high-resolution spectroscopy across the entire infrared spectrum. For example, it can be estimated that all the galaxies in the Shapley-Ames catalog could be measured at 100 μm with the LST if they contain nuclei similar to that in our own Galaxy. Since the LST represents the first generation of space telescopes available to infrared astronomy, it will have a significant impact on all the major scientific objectives that have been described in Section II.

2. LARGE INFRARED TELESCOPE ON SPACELAB

The 2- to 3-m LIRTS telescope is being studied for use as a dedicated, ambient-temperature infrared observatory on Spacelab with low-emissivity mirrors to reduce thermal noise. It will be diffraction-limited beyond approximately 20 μm with a guidance stability of about 1 sec of arc rms. The telescope will be used primarily for high-resolution spatial and spectral observations at long wavelengths. At these wavelengths, the capabilities of the LIRTS and the LST are similar. Hence, the scientific problems studied by the two instruments would be the same at the long wavelengths. The angular resolutions of the LIRTS and the LST are substantially the same beyond approximately 20 μm, but the sensitivity of the LIRTS may be somewhat better than that of the LST because of its possibly larger aperture and optimized low-emissivity design. In the case of the LST, long observing periods are possible but the spacecraft has to be revisited to change instruments. For the LIRTS, the observation time is shorter, but emphasis is placed on the flexibility for interchange of focal-plane instruments.

3. SHUTTLE INFRARED TELESCOPE FACILITY

Ambient-temperature telescopes cannot take full advantage of the extremely low infrared sky brightness available in space. A small amount of thermal radiation from the optical components and their associated supporting structure will always be present in the focal plane of such instruments. Even in the most carefully designed ambient-temperature telescope, this thermal radiation produces an effective sky brightness that will be several orders of magnitude above the limiting infrared sky brightness set by zodiacal light at orbital altitudes. Cryogenic cooling of the entire telescope is therefore required to achieve the full potential sensitivity of orbiting infrared telescopes. In fact, high-performance cryogenic telescopes have been successfully operated on orbital and suborbital missions. A one-year feasibility design study has concluded that existing cryogenic and detector technology can be used to construct cryogenic telescopes up to 1.5 m in diameter.

As with the cryogenic satellite survey, beyond 30 μm, the current state of detector technology falls short of the fundamental background photon noise from a cooled telescope in space by approximately two orders of magnitude. Therefore, the full potential of such instruments will not be realized for wavelengths longer than 30 μm unless a substantial improvement in detector sensitivity can be achieved.

At present, the SIRTF is planned as a Spacelab mission. It is possible, although it appears unlikely, that the full sensitivity of the SIRTF will not be realizable because of Spacelab contamination (see Appendix A). The design of the SIRTF should therefore be made compatible with its being used as a free-flyer. Such a telescope could be visited by the Shuttle for replacement of cryogens, change of detectors, and general updating, or it could be recovered and reused many times.

The use of infrared cryogenic telescopes in space is an exciting prospect for astronomy. Such an instrument could study the infrared radiation of galaxies billions of light years away from the earth. It would allow rapid and sensitive searches for sources in stages of currently unstudied stellar evolution. In the 7- to 30-μm spectral region, this telescope will have the spectroscopic and photometric sensitivity to make observations in hours that would require centuries for ambient-temperature telescopes of the same diameter.

C. Other Proposed Space Projects

1. 3 K SPECTRUM MEASUREMENTS

The background spectrum is consistent with a thermal spectrum of 3.5 to 2.7 K in the band 10 cm to 0.6 mm. A peak in the spectrum is clearly seen. Because of its fundamental importance to cosmology, measurements of the spectrum should be made with higher precision and extended to shorter wavelengths. Balloon measurements, using liquid helium-cooled high-resolution Fourier transform spectrometers to observe the spectrum between atmosphere emission lines, may improve by a factor of 3 within the next few years. The extension to shorter wavelengths, however, is more difficult.

A cryogenic Fourier transform spectrometer in space, unhindered by atmospheric emission, could be a moderate-resolution instrument using present-day technology. The principal difficulty shared by all instruments is the control of systematic errors, primarily radiation entering side lobes of the beam.

2. 3 K ISOTROPY MEASUREMENTS

The angular distribution of the background radiation is known to be isotropic to a few parts in 10^3 on the scale of tens of degrees of arc over about one quarter of the sky in the wavelength range 3 cm to 1 mm. A global anisotropy caused by the earth's motion in the universe has not been reliably determined.

Anisotropy measurements to at least the 10^{-4} level should be carried out over spatial frequencies extending from minutes of arc to tens of degrees of arc and wavelengths from 1 cm to 0.5 mm with total sky coverage. The limits imposed by discrete astronomical sources are unknown, but extended spatial frequency, wavelength, and sky coverage will help to discriminate against them. Present technology is adequate for the sensitivity required, but again the systematic errors, especially side lobes, are a problem. Eventually, space experiments should be considered, as they offer the ultimate opportunity for wavelength and sky coverage.

3. ABSOLUTE SKY BRIGHTNESS IN THE INFRARED

Measurements of the diffuse cosmic background from 5 to 300 μm may uncover the red-shifted radiation of the earliest stages of galaxy formation, which most likely is detectable in the 5- to 30-μm region and may complete the picture of the energy content of the universe at all wavelengths. Estimates for the radiation of integrated starlight and zodiacal light are an order of magnitude larger than estimates for the extragalactic components. It may be possible, however, to distinguish these sources by differences in angular distribution and spectrum. For example, a 20-cm diameter, low-side-lobe cryogenic telescope with angular resolution of a few degrees and 10 percent spectral resolution from 5 to 300 μm, above the atmosphere and airglow, would be a suitable instrument for this experiment.

4. EARLY SHUTTLE MEASUREMENTS

Spacelab will permit exploratory short-term flights and observatory-class long flights of astronomical instruments covering the whole infrared spectrum from 1 to 1000 μm. Spacelab is attractive for research in the infrared in that it will permit replacement of cryogens. Of great concern, however, is the amount of infrared radiation from the contaminant environment of the spacecraft. In order to determine the amount of contamination and provide information for the most desirable operational modes, a small (10–35 cm) cryogenically cooled column-density monitor should be included on early Shuttle flights. Such a radiometer could detect water vapor, which has its signature throughout the infrared spectrum, CO_2, and other gases, as well as particulates. All of these may arise from the Shuttle by outgassing, venting, leakage, and rocket firings. This instrument could measure the zodiacal light and distinguish it from the contaminants.

V. SUMMARY AND RECOMMENDATIONS

Infrared radiation, spanning three decades of the electromagnetic spectrum, originates from sources covering a wide range of astronomical scales and physical conditions. Detection and measurement of this radiation consequently play a central role in the investigation of virtually all major astrophysical problems, from the origin and formation of the solar system, stars, and galaxies to the origin and evolution of the universe itself. This study identifies and emphasizes the importance and feasibility of studying infrared radiation from the following known classes of sources: solar-system objects, including planets, comets, zodiacal dust, and asteroids; protostellar objects and stars in their early evolutionary phases; the interstellar medium, including H II regions and dense condensations of molecular gas and dust; galactic-scale sources, including the galactic center, energetic galaxies, and quasars; and the 3 K background radiation. Of equal, or perhaps even greater, significance, this study concludes that infrared and space technologies have now matured to a state where a sensitive, unbiased search for new infrared phenomena is possible.

Space observations have three primary attributes that allow the dramatic improvements in our observational abilities due to current infrared technology to be realized:

1. The absence of atmospheric absorption allows all infrared wavelengths to be observed;
2. Near elimination of the thermal background and its associated noise permits achievement of very high sensitivity;
3. The absence of atmospheric turbulence makes possible very high angular resolution.

Examination of the current status of infrared astronomy, infrared technology, and the potential of space observations leads us to the following recommendations for programs and facilities needed to realize the exciting promise of observations in the 1–1000 μm spectral range.

Recommendation A—Infrared Surveys

The highest priority should be given to a multiband infrared all-sky satellite survey. In the 7- to 30-μm region, the sensitivity of existing surveys should be surpassed by more than two orders of magnitude.

The known infrared sources provide fundamental clues to the energy budget and evolution of planets, protostars, and galaxies. Understanding of these objects remains hidden, however, because of the limited data available. For example, we know of too few protostellar systems to establish clearly an evolutionary sequence. We expect the proposed satellite to discover many thousands of new objects. Detailed studies of these, using infrared as well as radio, optical, ultraviolet, and x-ray techniques, will result in a more complete understanding of the fundamental processes involved in their formation and evolution.

Every area of astronomy (most recently, x-ray astronomy) has made significant and unexpected advances from systematic sky surveys. The proposed survey will reveal new and unexpected objects and will have the completeness of the Palomar and Cambridge surveys, which opened the way to spectacular advances in optical and radio astronomy. Proven technology in detectors, cryogenics, and spacecraft has made the time ripe for this survey. The survey specifications are given in Section IV.

The satellite survey is only one part of an exhaustive and comprehensive study of the infrared sky. We recommend two additional surveys required to complement the satellite survey:

1. An extension of the existing 2.2-μm infrared survey to the southern hemisphere;
2. Balloon sky surveys extending the wavelength range to 1 mm.

Recommendation B1—Cooled Space Observatories

We strongly recommend that cryogenically cooled telescopes, as characterized by the Shuttle Infrared Telescope Facility (SIRTF) design, be carried forward vigorously in the coming decade.

The cooled telescope will provide an unequaled opportunity for extending the search for extragalactic infrared objects and determining their position in the evolution of quasars and galaxies. It will permit photometry and spectroscopy of objects a thousand times fainter than can now be observed. We realize that the problem of telescope contamination in the Shuttle environment must be solved in order for the cooled telescope to achieve its full potential. We therefore recommend that a contamination monitoring program be carried out during the initial operation of the Shuttle. We further recommend that the cooled-telescope design be made consistent with its use on either Spacelab or in a free-flyer mode.

Recommendation B2—Ambient-Temperature Space Observatories

We recommend that the potential of the Large Space Telescope (LST) for infrared astronomy be vigorously exploited. Large, accurately pointed ambient-temperature telescopes in space can attack problems whose solutions depend critically on angular resolution. When high spatial resolution is combined with high spectral resolution, in the absence of atmospheric limitations, a number of crucial problems may be solved. Study has shown that the LST offers great potential in this area. The infrared experiments on the LST can use all available time, including full-sunlight time, thus greatly increasing the utilization of the facility. The extremely high pointing precision and stability of the LST make it a unique facility for infrared observations.

The European project, LIRTS, a 3-m ambient-temperature telescope for the infrared, offers high spatial and spectral resolution at wavelengths longer than 10 μm and versatility in rapid interchange of instruments. We encourage the European Space Agency to continue the development of this instrument.

Recommendation C—Small Infrared Payloads

We recommend development of small infrared payloads to measure the diffuse cosmic radiation throughout the infrared spectrum.

The absolute sky brightness in the 1–300 μm range conveys fundamental information concerning the epoch of early galaxy formation and the energy budget of the universe, as well as information on emission from the zodiacal cloud and integrated starlight. Measurements of this flux must be made above the atmosphere and the airglow. Low-background instruments for Spacelab or free-flying satellites will be needed to carry out these measurements.

The submillimeter background is one of the few fossils available to study the early history of the universe. Measurements of both the spectrum and angular distribution over a range of wavelengths and spatial frequencies constitute some of the most fundamental observations in modern cosmology. Spectral measurements of the 3 K background are now ripe for space experimentation. Isotropy experiments should be continued from balloon platforms, with reassessment in two years of the technology and limitations due to the atmosphere.

Recommendation D—Supporting Programs

We recommend expanded support of ground-based observations. This support is essential for the exploitation of the discoveries made in space. For example, in the investigation of one object discovered in a rocket survey, a dozen groups collaborated using ground-based telescopes of apertures from 0.3 to 5 m at wavelengths from visual to radio, and the techniques of spectroscopy, photometry, polarimetry, and high-resolution spatial mapping.

We recommend that existing airplane programs be continued. The C-141 Kuiper Airborne Observatory is currently the most highly developed far-infrared telescope facility. Its continued support will guarantee many important observations.

We recommend increased support for balloon programs. Although balloon systems are in a relatively primitive state of development, they have enormous potential. At balloon altitudes, photon noise from the atmosphere is reduced below the levels detectable by most present-day far-infrared instruments. A relatively small investment in scientifically sound balloon programs will yield a large return in science and will aid in the development of instruments for future space missions.

Recommendation E—Technical Developments

A sustained program should be initiated to improve far-infrared technology in four areas:

1. The development of better detectors of all varieties—broad-band mixers, photoconductors, and bolometers—including two-dimensional detector arrays. In the region longward of 30 μm the performance of spaceborne and balloonborne instruments is fundamentally limited by present-day detectors. This same recommendation has been made by several prior panels but has not been implemented.
2. The continued development of cryogenic systems for use in space with emphasis on large systems suitable for long-duration cooling of instruments in free-flying satellites.

3. Research leading to low-emissivity surfaces and low-side-lobe optics.

4. The development of cryogenic, rugged, high-transmission far-infrared bandpass filters for the spectral region longward of 20 μm.

Recommendation F—Long-Range Program

In order to realize the full capabilities of space to answer the fundamental questions in infrared astronomy, second- and third-generation infrared projects should be carried out as the necessary technology is developed to make them feasible. We recommend a long-range plan that differs in letter, though not in spirit, from the launch schedule outlined in *Scientific Uses of the Space Shuttle* (National Academy of Sciences, Washington, D.C., 1974), pp. 83–101. A point of difference is that no clear advantage of a 1-m ambient-temperature telescope is seen.

It is clear that higher spatial resolution than will be available from the proposed space telescopes such as the LST, the LIRTS, and the SIRTF will be important for studying the fine-scale structure of infrared sources. When the technology for accurate servo control over long baselines becomes available, an interferometer that makes use of two high-performance space telescopes will clearly have high priority.

In addition to the important scientific research that will result from the proposed space telescopes, it is expected that these observatories will provide the engineering and operational experience necessary for the design of larger-class telescopes such as a 2.5-m cooled and a 10-m uncooled instrument. Although the feasibility of these larger instruments cannot be ascertained without the experience gained from first-generation telescopes, these larger cooled and uncooled telescopes should stand as a goal for the infrared astronomy program.

APPENDIX A

Even extremely low levels of environmental contamination near a spaceborne infrared system can seriously limit the system's performance. The types of contamination that are a potential problem can be divided into two broad categories: Type 1, contamination produced by the spacecraft; and Type 2, contamination natural to the system's operating location. The most serious single contaminant of Type 1 appears to be dust particles that drift away from the spacecraft and enter the field of view. These particles can appear as false sources. Other spacecraft-produced contaminants include gaseous materials that are either outgassed, offgassed, leaked, or discharged from the spacecraft. Contamination of Type 2 includes high-energy charged particles (both cosmic rays and trapped radiation belt particles), residual atmospheric gases, infrared background radiation from the zodiacal dust cloud, and strong radiation from the sun, earth, and moon.

Type 1 Contaminants

Spacecraft-produced contaminations are expected to be worse for manned than for unmanned programs. Detailed studies of contamination levels expected for the Space Shuttle have shown that the gaseous levels can be reduced to an acceptable level (less than 10^{12} atoms/cm^2 in the telescope line of sight) by careful management. Some question of time scale for outgassing still remains. Some satellites have exhibited outgassing time constants of days. The reaction motors must not fire into the hemisphere surrounding the telescope's line of sight. In fact, it will probably be necessary to restrict the reaction motor firings to once or twice per orbit. In addition, excess water from the fuel cells (approximately 100 kg/day), as well as other wastewater, will have to be stored on board or vaporized and discharged through carefully designed and located nozzles.

Despite considerable effort, somewhat less certain conclusions about the particulate contamination problem exist. A study of the contamination near Skylab and several other satellite and sounding-rocket payloads indicates that careful cleaning before flight may be sufficient to reduce the frequency of particulate sights to several per orbit. If this limit is to be reached for the Shuttle, all vents capable of emitting dust or water in any form must be manually controllable.

In addition to reducing the number of particles in the line of sight, there are data-analysis techniques to identify and partially remove the effects of residual particles. For example, a near-field point source appears as an easily recognized donut-shaped ring in the focal plane. The effects of very near particles can be reduced by the use of a spatial chopping technique. Although particulate contamination is a serious problem, it appears to be a manageable one.

Type 2 Contamination

Three of the four natural contaminants—charged particles, the residual atmosphere, and the zodiacal background—require careful treatment. The radiation from the sun, earth, and moon can be handled by a carefully baffled system. High-energy particles and infrared photons both produce signals in infrared detectors. They differ in that a single charged particle produces a much larger signal than a single photon. The seriousness of the problem depends on the radiation environment, the detector volume, and the shielding provided by the spacecraft. All of these factors are known, and calculations for a 600–900 km orbit and typical detector volumes (~1 mm^3) and shielding thickness (~10 g/cm^2) predict an event rate of 0.2/sec. At this counting rate, pulse shape discrimination techniques can be used to remove the particle counts from the data. Near the South Atlantic anomaly, the event rate might well exceed this value by 100. Depending on orbit inclination, serious contamination may exist over 5–15 percent of an orbit.

Residual atmospheric gases can potentially result in several types of degradation. These include direct interference via emitted radiation, reduced transmission through the optics, and increased scattering by the optics (resulting in degraded off-axis performance) caused by condensed gases. In addition, atmospheric gases will strike the cold surfaces with relatively high energy (~1 eV). If a significant fraction (0.1 to 1 percent) of this energy is re-emitted as infrared photons, a serious background is produced. These effects can best be eliminated by a continuous He purge of the optical cavity and by preventing the telescope from looking into the forward direction. The simple expedient of moving to a higher altitude would reduce the atmospheric problem while aggravating the radiation belt problem.

The final natural contaminant, the zodiacal light, affects both infrared and visual systems in the same way. It increases the background and its attendant noise level. The SIRTF telescope, operating with a 1 min of arc beam at 13 μm with $\Delta\lambda = 10\ \mu$m, will be degraded by a factor of 2–5 from its "dark-sky" performance level. Observations with either a smaller bandwidth or field of view will be affected correspondingly less. These estimates are based on model calculations of the strength of the zodiacal light that are consistent with the only direct measurements. Three programs are planned to measure the zodiacal light by the end of 1975. Because the zodiacal background affects all high-sensitivity near-earth infrared systems, it is important to many programs that complete measurements of the background be made.

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F
Solar Physics

I. SUMMARY

A. Introduction

Solar physics has become one of the most complex subjects in astrophysics. The sun, our daytime star, is sufficiently near that its surface can be studied in detail, revealing the many phenomena that make up the complicated personality of an average stellar object. Similar effects and variations are part of the makeup of every star, and collectively of whole galaxies, but are blissfully suppressed in the unresolved radiation from the "night-time" stars. Observations of the sun present a variety of phenomena that at first sight defy rational explanation in terms of the familiar concepts of physics but ultimately stimulate the theoretical understanding of new effects. Historically, the chromosphere and corona are outstanding examples, having stood in contradiction to the views of thermodynamics at the time their properties were determined.

The sun presents such diverse phenomena as the cool prominence gas phase immersed in the hot coronal gas phase, differential rotation, the 22-year magnetic cycle, the sunspot, the flare, and the predicted but still missing neutrino emission from the core (which provides the only independent test of the theory of stellar interiors). Each of these effects has proved to be an enigma, with only partial understanding available. Yet each limits the understanding of all stars. One of the solar contributions of greatest importance to astrophysics has been the detailed study of nonequilibrium thermodynamic systems in the solar photosphere, chromosphere, and corona. The knowledge of such systems developed from the sun is now applied to the less tractable problem of the analysis of the radiation from the unresolved disks of other stars.

The active ejecta of relativistic particles and hot plasma seen in solar flares provide the only examples of this violent phenomenon subject to detailed diagnostic examination. Many active stars and galaxies display ejecta of similar appearance. The solar wind is the only example of a stellar wind subject to comprehensive observation and theoretical modeling. The solar flare is the only example of the stellar-flare phenomenon (that appears in such colossal form in the dwarf-M emission stars) subject to close scrutiny and analysis. It is possible to diagnose the effects of many active phenomena only with the assistance of the closeup observations and theory of the solar analogue.

Recent studies of the sun and its variable activity, together with the long record of variation of weather and climate of earth, show that there may be both a short- and a long-term relationship. Numerical modeling suggests a close connection between global circulation at low altitudes and the ozone-induced thermal gradients in the upper atmosphere. Modeling has also suggested that a minor change in solar luminosity or ultraviolet emission may produce a major change in climate, which indicates the importance of precision synoptic studies of the luminosity of the sun. Indeed, the whole problem of variation of terrestrial climate and solar luminosity suggests that it would be important to monitor the luminosities of other stars of the same class as the sun.

The convective zone beneath the photosphere is responsible for the chromosphere, the corona, and the solar wind. It is also responsible for the differential rotation and for the generation of the magnetic field of the sun, whose emergence through the surface is the agent that converts the quiet sun into an active sun.

Many phenomena on the quiet sun depart remarkably from thermodynamic equilibrium. The quiet sun is active in many ways, but on so small a scale as to be inconspicuous. The convection beneath the photosphere causes superheating of the tenuous atmosphere above the photosphere, producing the chromosphere (10^4 K) and the transition to the corona (10^6 K) above. The quiet sun exhibits magnetic fields over the entire surface, appearing either as the small ephemeral bipolar regions or as individual compressed flux tubes in the boundaries between the supergranule convective cells. The spicules evidently leap up through the chromosphere along these flux tubes. The transition region is rendered extremely inhomogeneous by small-scale activity.

The conspicuous features of solar activity are large, and their forms are easily observed from the ground in visible light. Their general character was discerned decades ago. Their internal workings, however, are of small scale (100 km or less) and, in some cases, visible only in radio, ultraviolet, or x rays. Their general causes are conjectured from the general observations in visible light, but their particular effect can be probed in detail only from instruments carried out of the terrestrial atmosphere on spacecraft or by such sophisticated ground-based facilities as the Very Large Array (VLA). The high-resolution ground-based observations, together with the OSO series of spacecraft and the Skylab observations, have provided the exploratory observations that define the general nature of the complex atmosphere and activity of the sun and have begun to probe the internal working of the various phenomena. The next stage is the detailed diagnostics, coordinating the necessary high-resolution observations to determine the precise physical nature of each phenomenon.

The variety of effects presented by the sun has led to the development of a broad observational and theoretical program aimed at exploring and understanding this complex behavior. This is not the appropriate place to review the milestones that have already been passed. Suffice it to say that a variety of problems still lie ahead, baffling and challenging and beckoning to be solved. We can group these problems loosely into two categories, the quiet sun and the active sun, each of which presents many separate problems.

This report outlines a general, coordinated assault on the many questions and problems presented by the sun. Sections II and III explore the various problems, Section IV describes the observations and theoretical studies that are needed to probe the many phenomena, and Section V outlines the organized observational programs from the ground and from spacecraft that will provide the necessary information.

B. Recommended Program

1. SOLAR MAXIMUM MISSION

Of the several major space efforts required, the most timely is the Solar Maximum Mission (SMM), aimed at probing the active regions and particularly the solar flare. The instruments for the mission have been defined, designed, and in many cases already tested. The experimental groups are prepared, many of the needed diagnostic techniques are tested, and the solar maximum of 1979 is approaching. We consider the SMM a pivotal step in space research. A new start is required in fiscal year 1977 in order to launch it by late 1979. The predicted behavior of the active regions and flares to be observed by the SMM is discussed in Section V.B. and in Appendix A.

If it is not possible to begin funding in 1977, present experimental teams will dissolve and the cost of a later start will be increased, while the declining chances of obtaining complete coordinated observations of moderate-sized (importance 2) flares will seriously imperil the scientific objectives of the mission. In that case, we feel compelled to recommend abandoning the SMM and the serious study of flares until the next solar maximum in 1990. Since solar flares and active regions are central to the understanding of solar physics, the loss of the SMM would be a serious blow to the exploration of solar activity, leaving a conspicuous gap in our knowledge at a crucial point.

Whether or not the SMM is approved, we recommend proceeding with the study of the quiet sun, and other forms of activity, with the combined Shuttle instruments and facilities in coordination with various free-flyer missions and ground-based observations.

2. EARTH-ORBITING FREE-FLYERS

The Solar Synoptic Satellite (SSS) series would be a series of free-flyers designed to achieve substantial improvements in the quality of observations and in the continuity of coverage of the evolution of quiet and active coronal structures such as coronal holes, active regions, and streamers. These satellites will provide (a) basic solar research information; (b) long-term history of features selected for high-resolution, short-term Shuttle sortie studies; (c) observations of coronal structure to complement the Solar Stereoscopic and out-of-the-ecliptic missions; and (d) monitor the long-term variability of the solar "constant" and ultraviolet emissions.

Possible vehicles for the SSS would be follow-on SMM-type spacecraft or Explorer-class spacecraft.

Other free-flying satellites to be considered for flight in the 1980's include the "Pinhole" satellite and the Large Solar Observatory (LSO).

3. SPACELAB INSTRUMENTS

NASA is currently studying five facility-class instruments for Spacelab flight in the early Shuttle era. These include a 1-m-class optical/UV telescope facility, an XUV telescope facility, a soft x-ray telescope facility, a hard x-ray imaging facility, and an EUV telescope facility. Their capabilities, presently under definition, will clearly make them the cornerstone of much of the observational research discussed in the body of this report. It is hoped that by mid-1980, one or more of the facility-class instruments will be available for flight during the period around the solar maximum and during the flight of the SMM. While such facility instruments could be an enormously valuable complement to the SMM, they could in no way be a substitute for it, because of the short flight time of Spacelab missions and the fact that they lack the extremely broad range of instrumental capability contained on the SMM.

The special-purpose instruments on Spacelab are more specialized than facility instruments. Some, such as solar gamma-ray and neutron telescopes, should be flown as early as possible in Spacelab in order to overlap with flare studies on the SMM.

4. INTERPLANETARY MISSIONS

Interplanetary missions, such as the Interplanetary Monitoring Platforms (IMP), the International Sun–Earth Explorers (ISEE), the Solar Stereoscopic Mission, and the Out-of-the-Ecliptic Missions are of great importance for solar physics, both because of their direct measurements of particles and fields and (for the latter) their capability to view conditions over the poles of the sun.

The missions perhaps most unique in their exploratory scope are the out-of-the-ecliptic missions (OOE). Their purpose is to probe the conditions in the space outside the thin layer to which satellite orbits are now confined. The OOE spacecraft would be instrumented to study the solar wind, cosmic rays, and fast solar particles at middle (solar) latitudes, looking directly into the most intense solar active regions and, over the poles of the sun, looking directly into the large polar coronal holes. They would be instrumented to observe the azimuthal form of the coronal structures and to observe the convection, circulation, and magnetic fields in the polar regions in order to develop an understanding of the global structure of the circulation and convection in the sun.

The possibility of sending a probe directly into the sun (the solar "plunger"), in order to return information from distances as close as two or three radii above the photosphere, should also be studied carefully. Planning for the earth-orbital Solar Synoptic Satellites (discussed above) should ensure their operation concurrently with interplanetary missions to obtain coordinated observations.

5. LEVEL OF EFFORT PROGRAMS

The above recommendations stress the requirements of solar space research. Important research from ground-based observatories remains to be done even if all the space missions are implemented. The value of both space and ground observations should be measured by our ability to understand them, to interpret them in terms of basic physical mechanisms that are also applicable to other fields of science, and to relate them to other phenomena of interest to mankind. We recommend, therefore, a strong level of effort program in theoretical solar research, diagnostics, computer simulation of solar processes, and laboratory astrophysics to complement and enhance the space effort.

For ground-based observations, the following will continue to be of interest:

a. Small-scale dynamics in the lower solar atmosphere related to coronal heating, convection, waves, and mass loss. (If implemented, the large optical Shuttle telescope might reduce the significance of this type of ground-based effort.)

b. Large-scale global circulation on the sun and the shape of the sun itself. Both are related to solar structure, solar dynamo, and solar activity cycles.

c. Synoptic observations of solar variability.

d. Thermal radio emission of the outer solar atmosphere.

e. Nonthermal radio emission associated with solar transients.

f. Neutrino flux energy spectrum.

Many of these ground-based observational programs are an important part of the overall space effort, both as an essential and integral part of the SMM and as part of a level of effort program. They will also be an important complement to the other space missions discussed in this report. The threat to the continued existence of excellent ground-based facilities, and the actual closing of one facility, is a matter of extreme gravity for the space program. We urge, therefore, that adequate support be provided to ensure the continued pursuit of high-quality ground-based solar research.

II. THE ROLE OF SOLAR PHYSICS

A. Introduction

Research into solar physics is pursued for many reasons. The sun reveals processes that, once understood, have wide application elsewhere in physics and astrophysics. It is an important source of disturbances to the earth's environment (magnetosphere, ionosphere, and possibly the atmosphere). And as we have been newly reminded, the sun offers the only long-term energy supply for mankind.

The sun reveals itself to us in far more detail than does any other astronomical object. It is unique among stars in that it may be observed in three dimensions, in time, and over more than fourteen decades of the electromagnetic spectrum. In addition, we are able to measure the sun's particle emissions and to map its magnetic field, which plays a key role in almost all of solar activity.

The satellite and ground-based observations carried out in the last 15 years have provided new insight into the basic mechanisms responsible for many of the fundamental solar phenomena. These include mechanisms for heating the corona, for the generation of the solar wind, for the dissipation of magnetic energy (in field reversal regions known as "current sheets"), and for two or more types of particle acceleration in flares. Conversely, the sun has been, and continues to be, the "testing ground" of astrophysics, where theories of nuclear energy, convection, radiation transport, and other phenomena may be confirmed or refuted. The sun is also valuable for the development and testing of diagnostic procedures and new astronomical instrumentation, notably uv and x-ray telescopes and spectrometers. Because the earth responds in many ways to solar variations, when these processes are fully understood we will have increased our knowledge not only of the sun but also of the earth itself.

Understanding solar variability, and ultimately predicting it, must become one of the long-term goals of solar physics. It will be an increasingly needed input into climatic modeling and prediction. Although all progress in solar physics contributes to this end, we must improve the sophistication with which we monitor significant solar radiations.

We therefore propose that the major goals of solar astrophysics over the next decade should be the detailed understanding of the basic astrophysical mechanisms as they occur on the sun, with application of the results to problems in interplanetary studies, in solar terrestrial relations, and in the study of other astrophysical objects.

B. Basic Mechanisms in Solar Physics

1. NUCLEAR PROCESSES

During most of the lifetime of a star, nearly all of its energy comes from thermonuclear reactions taking place in a dense, hot core. For main-sequence stars like the sun, these reactions convert hydrogen into helium, generating various types of radiation, including neutrinos. The neutrinos should be directly observable at the earth, providing an observational check on theories of stellar nuclear physics and nucleosynthesis. The measured flux of neutrinos, however, is at least an order of magnitude lower than the predicted level, leading to the suspicion that there is something basically wrong in our theory of stellar interiors. Solar physics is now placing great emphasis on the overall structure of the solar interior, searching for an answer to the problem of the missing neutrinos. Possible solutions include long-term solar variability or an absence of heavy elements in the core.

At the surface of solar-type stars we also expect nuclear reactions that result from particle accelerations in the low-density coronal matter, followed by collisions with material in the photosphere and chromosphere.

It was not until mid-1972, however, that nuclear surface reactions were actually detected in flares by means of their gamma-ray emission line spectrum. From direct observations of solar-flare x rays and radio bursts we see highly accelerated particles within flares, some escaping to produce the solar cosmic rays. The mechanisms for these accelerations are important not only for understanding the flare process itself but also for understanding the origin of galactic cosmic rays. Chemical abundances in solar cosmic rays reflect standard solar abundances at energies above 10 MeV/nucleon, but are very different at low energies. For example, very high deuterium, tritium, and ^{3}He abundances are observed occasionally, even to the extent that ^{3}He is sometimes more abundant then ^{4}He. These abundance differences are probably the result of both propagation effects and the flare nuclear reactions, and their interpretation requires new insights into the flare mechanism, particle acceleration processes, surface nuclear reactions, and particle propagation. Events similar to solar flares occur elsewhere in the universe, specifically on dMe stars, where the flare activity vastly exceeds that of the sun. These stars, which represent a major fraction of the mass of the galaxy, now have been observed at soft x-ray, optical, and radio wavelengths, and the analogy with solar flares appears to be strong.

2. ATOMIC PHYSICS AND RADIATIVE TRANSFER

For most astrophysical objects the only way of measuring physical properties is by deciphering the spatially unresolved emitted radiation field. This radiation field is created by the excitation, ionization, dissociation, and recombination of atoms, ions, and molecules and by the interaction of this radiation with matter before it leaves the system under study. Since our measurement tools are indirect, it is necessary that the interaction between radiation and matter be properly understood.

In the simplest approximation, the ionization state of a gas is presumed to be in local thermodynamic equilibrium (LTE) so that the degree of ionization of a gas depends only on density and temperature. Although the LTE presumption is commonly used in interpreting stellar spectra, it is known to be valid only rarely. In the solar context, various deviations from this ideal state have been studied in many physical situations. The helium ionization balance in the solar chromosphere is an example of photoionization/recombination equilibria, which also may occur in T Tauri stars, x-ray binaries, Seyfert galaxies, and quasars. In the solar corona, ionization equilibria are often dominated by conditions that are probably valid also in stellar coronas and interstellar shock fronts.

Recent observations have made it necessary to develop a theory of nonlocal ionization balance in which the ionization equilibrium is modified by mass motions, thermal diffusion of ions, and penetrations of electrons from elsewhere in the atmosphere. These theoretical studies, which have already been checked and confirmed in the case of the solar wind, may help in understanding nonlocal and time-dependent equilibrium phenomena that occur in novae, supernovae, flare stars, shock fronts, and the newly discovered hot component of the interstellar medium.

The sun has proved most useful in developing physical insight into the various mechanisms of spectral-line formation. A variety of types of excitation conditions that occur in stellar atmospheres have also been studied in solar contexts. For example, much of the study of spectral-line formation in the presence of systematic and random velocity fields and velocity gradients has derived its impetus from solar astrophysics. Solar research has also provided examples of radiative transfer mechanisms involving multidimensional transfer, polarization, and radiative transfer in the presence of magnetic fields. These mechanisms are particularly relevant to extended gas and dust envelopes, Be stars, magnetic A stars, and Of stars.

In theoretical studies of heating mechanisms and energy balance in astrophysical plasmas, it is essential to understand radiative cooling. Detailed observations of transient solar phenomena provide a stringent test of the general cooling functions developed for optically thin astrophysical plasmas. At higher densities, some transitions become optically thick and the simple optically thin approximations are no longer valid. Radiative loss calculations for such gases were developed to understand the solar photosphere and chromosphere and are utilized to study flare stars and stellar atmospheres in general.

3. HYDRODYNAMICS

The systematic observation of sunspots by Galileo, beginning in 1611, led to the knowledge that the sun rotates. Carrington and others discovered in the 1850's, from sunspots, that the sun exhibits differential rotation. More recently, however, it has been found that weak magnetic-field regions on the sun seem to exhibit rigid-body rotation. Thus, it is clear that hydrodynamic motion in at least one star is indeed complex. The mechanisms responsible for this behavior are yet to be identified.

The granular structure of the sun's photosphere, from its first realization, has been attributed to the process of convection—one of the dominant mechanisms of energy transport in stars. Analytical theories of convection have been developed, but only for simplified model atmospheres that do not yield the preferred length scales characteristic of the solar atmosphere. Further understanding of convection will be derived from computer modeling.

Our understanding of the hydrodynamics of the outer atmosphere is much more detailed, primarily because of the greater observational knowledge derived from ground-based telescopes and space experiments. The theory of the solar wind represents a major triumph of modern astrophysics, which has led in turn to the concepts of a "polar wind" from the earth, of "stellar winds," and even of "galactic winds."

Sustained effort is being made in developing theories for the generation, propagation, and dissipation of waves in the sun's atmosphere and of the resulting structure of the corona and chromosphere–corona transition region. An important part of these efforts is the development of models derived from observations with resolution less than 1 sec of arc and good spectral definition. These theories have yet to be applied in detail to the problem of stellar atmospheres, and their possible role in other objects has yet to be explored.

4. MAGNETOHYDRODYNAMICS AND PLASMA PHYSICS

Almost all solar activity depends on the role of magnetic fields, and it is a fair guess that much of what is termed "high-energy astrophysics" will also prove to involve magnetic fields as an essential element.

The existence of magnetic fields in the sun's outer envelope is attributed to "dynamo action" in the convective zone. Sophisticated theories that depend on the transport properties of turbulent plasmas are now being developed. These theories suggest that the solar cycle is to be understood as a mode of dynamo action. The interplay between magnetic fields and convection is likely to prove important in many classes of stars; but so far the problem can be analyzed only within the context of solar physics.

Although it has been widely accepted that the energy of solar flares is derived from magnetic energy, detailed models of the configuration and detailed energy-conversion mechanisms have emerged slowly. The key magnetic configurations now appear to be current sheets and force-free fields. The probable instability of current sheets was first proposed in relation to solar flares, and much of the subsequent theoretical analysis has resulted from this impetus. Similarly, the studies of force-free fields now in progress have, as their driving application, their role in solar activity. Although limited information concerning current sheets and force-free fields is obtained by analytical methods, the more detailed knowledge made necessary by solar observation can come only from numerical modeling using high-capacity computers.

Following extensive analysis of ground data and space data, theoretical analysis has suggested the possibility that almost all of the radiations observed from solar flares are ultimately attributable to particle acceleration.

This lesson may prove to be relevant to other objects that exhibit strong emission-line radiation, such as quasars, Seyfert galaxies, x-ray stars, and supernova remnants. Solar observations have recently indicated that two, and possibly three, different acceleration mechanisms are at work in solar flares. Hence, our understanding of particle acceleration in astrophysics—a topic crucial to all of high-energy astrophysics—is receiving important input from solar physics.

Studies of radio emission in astrophysics have for many years been dominated by the concept of synchrotron emission. Of the five types of radio burst that have been identified in solar radio astronomy, however, only one (Type IV) is believed to be caused by synchrotron radiation. Three types of burst (Types I, II, and III) are produced by mechanisms other than synchrotron radiation, and one (Type V) is still in doubt. As more data are acquired, we may find that plasma oscillations and other collective processes also play an important role in some nonsolar radio sources.

C. Diagnostics

The sun is the principal laboratory for developing and testing theoretical diagnostic tools with which to determine such physical properties as temperature, density, ionization equilibrium, systematic and random velocity fields, mass loss, heating and radiative losses, and chemical abundances. When such diagnostic techniques are proved, they have general application throughout astrophysics. In particular, these diagnostics will be used extensively in the analysis of ultraviolet and x-ray spectra to be obtained by the IUE, the LST, and the HEAO satellites and data now being obtained by the Copernicus satellite.

Techniques for determining the temperature and density structure of astrophysical plasmas through wide ranges of temperature (5×10^4 to 5×10^7 K) and density (10^4 to 10^{12} cm^{-3}) have been developed in studies of the corona and transition region at extreme ultraviolet (euv) and x-ray wavelengths. These techniques include density-sensitive or temperature-sensitive ratios of spectral-line intensities and intensities of lines that can be directly related to the ionization equilibrium. A number of intensity ratios have also been found for lines of different elements, which are only weakly dependent on temperature and therefore make excellent diagnostic tools for determining the relative abundances. The refinement of these techniques and the development of new diagnostic techniques often involve line–intensity ratios that are insensitive to the detailed thermal structure of the plasma being observed and, consequently, allow these parameters to be inferred in a manner independent of the particular model atmosphere employed in the analysis.

Solar observations with high spectral and spatial resolution are needed to improve our knowledge of the fundamental atomic processes on which the diagnostics are based. These techniques will be of critical importance in the study of astrophysical objects that contain plasmas at temperatures similar to the solar corona. Examples of such objects include interstellar shock waves generated by supernova ejecta, accretion-powered x-ray binaries, the hot component of the interstellar medium, dMe flare stars, and the coronas and transition regions of stars.

Important temperature diagnostics for lower temperatures ($<10^4$ K) have been developed on the sun, as well as density or pressure diagnostics for temperatures less than 20,000 K. This work is generally relevant to the study of late-type stars and is being used to interpret the Copernicus satellite observations.

Systematic and random velocity fields manifest themselves in various ways, including broadening, shifts, and asymmetries in spectral-line profiles. Detailed diagnostics of these phenomena are currently being developed and tested against solar observations of high spatial resolution becoming available from OSO-8 and from ground-based observatories. Such diagnostics permit us to determine the upward propagation of mechanical energy and its dissipation.

Finally, methods of accurately measuring the chemical abundances of astrophysical objects are of fundamental importance to an understanding of the evolution of the universe. The recent controversy over the solar iron abundance is a good example of the multiplicity of consistency checks that must be satisfied before a solar abundance is considered reliable. In this case, a large inconsistency between the coronal and photospheric iron abundance precipitated a wholesale re-examination of the abundance determination process. It was discovered that the traditional value for the photospheric iron was wrong, as a result of an incorrect laboratory determination of f-values, and the coronal value was correct. This discovery affects the determination of photospheric iron abundances in other stars. At present, the solar CNO (carbon, nitrogen, and oxygen) abundances are being given similar scrutiny, including the development of non-LTE techniques for analyzing molecular spectra. Solar abundances and isotopic ratios are crucial to astrophysics because they are usually adopted as "cosmic" abundances for a wide range of theoretical work, including cosmology and stellar evolution. In this regard, solar measurements of the light-element abundances (^3He, ^4He, Be, and B) are most important. The analysis of the coronal x-ray and EUV spectrum is especially useful in abundance studies, since the radiation is optically thin, and the atomic constants are more accurately known for the simple ions that are observed at coronal temperatures. Abundances for noble gases (He, Ne, Ar) which cannot be inferred from the visible spectra of late-type main-sequence stars, may be derived from coronal observations.

The ionic abundances in the solar wind and the atomic and isotopic abundances (through iron) in solar cosmic rays can be measured directly. Their variability suggests that fractionation does occur in the atmosphere of the sun, probably in the corona or transition region. In the future, measurements of the isotopic composition of solar cosmic rays, from hydrogen through iron, will provide a new dimension to the study of solar cosmic rays.

D. Instrumentation

The range and intensity of solar radiation, electromagnetic and particulate, have favored the development of sophisticated instrumental techniques. In recent years, spectroscopic and imaging instruments have been developed in the visible, euv, x-ray, and gamma-ray regions of the spectrum that have subsequently found applications in other areas of astrophysics. The grazing incidence x-ray telescope is an important example.

New instrument configurations are now being developed that combine the large collecting areas and high angular resolution typical of x-ray telescopes with the high spectral resolution typical of soft x-ray and euv spectrometers, and techniques using multigrid collimators are being developed to extend high-angular-resolution observations to the hard x-ray range. These instruments are designed to make coronal and transition region observations with angular resolution in the second of arc and sub second of arc range with high spectral resolution. This will allow the diagnostic techniques discussed above to be used to test models for the mechanisms responsible for energy and mass transport in the corona; for the nonthermal acceleration processes that are the major energy release in most flares; and for the development of detailed models of coronal temperature, density, and atomic abundances. Improved satellite instrumentation, currently under development for observations at gamma-ray energies and for solar electron and cosmic-ray observations, will also be an important part of future solar satellite and Shuttle observatories.

E. Solar–Terrestrial Relations

The prime driving force for all physical processes occurring in the terrestrial environment is the energy received from the sun. The magnetosphere is influenced primarily by the solar wind. The ionization balance and chemical composition of the earth's upper atmosphere are controlled by solar ultraviolet (uv) and euv emissions. Disturbances of the ionosphere are attributable to variations in the solar uv, euv, and x-ray fluxes. Variations in ionospheric structure at the magnetic polar caps are due primarily to fluxes of solar particles in the MeV energy range. The basic mechanisms relating solar radiative and particulate emissions to the terrestrial environment are known. Improved detailed understanding of these processes will require prolonged monitoring of solar radiation from space vehicles.

There is much uncertainty, however, concerning the effects of solar activity on the lower atmosphere, as manifested by weather and climatic variations. Although a great deal of statistical evidence has been accumulated for such associations, the significance of this evidence has not been established. The associations, if real, will help to resolve some challenging meteorological problems and will have an impact on the problem of weather prediction and climate modeling.

F. Interplanetary Medium

The sun controls the basic physical processes within a region of space with a radius about 50 times that of the earth's orbit. In this interplanetary space, the solar-wind plasma drags lines of force of the solar magnetic field with it and interacts with planetary magnetospheres and with the local flow of the interstellar medium. In addition to transporting mass, energy, and angular momentum away from the sun, the solar wind produces such planetary phenomena as airglow, radiation belts, and the complex radio emission observed from Jupiter. Research into this flow, including sector boundaries, shocks, cosmic-ray modulation, and radio scintillation, presents exciting problems and new effects, as well as information about the local interstellar medium.

III. SOLAR RESEARCH—PROGRESS AND PROSPECTS

A. Introduction

Solar physics has evolved from ground-based observations through exploratory space missions to detailed astrophysical investigations from space. Over the past decade this work has led to an impressive series of advances in our understanding of the sun. In this section we review recent progress in several areas of solar research and indicate fruitful areas for future research.

B. Solar Flares and High-Energy Phenomena

1. OVERVIEW OF THE FLARE PHENOMENA

Solar flares are one of the most important of solar phenomena, not only for their geophysical effects or for the many secondary solar phenomena that they generate but also for the opportunity that they offer to study processes important to astrophysics and the physics of plasmas. These processes include energy storage and its sudden release in magnetized plasmas, particle acceleration, explosive heating and evaporation, mass ejection and shock waves, and the formation of high-temperature plasmas. In spite of the complexity of the flare phenomenon, a tentative model of the flare has recently gained wide acceptance. The basic concepts of this model are the following.

Prior to flare onset, energy is stored in a current-carrying magnetic field that is in a metastable state. The sudden release of the free energy of this magnetic field appears in the form of energetic electrons that interact with the atmosphere to produce bursts of microwave radiation and hard x rays. The energy flux in this beam is high and leads to explosive evaporation of the chromosphere, producing a hot ($>10^7$ K) plasma. This, in turn, accounts for many subsequent observed flare phenomena, including soft x rays, chromospheric radiation, solar cosmic-ray acceleration, and possibly mass motions and a blast wave. Finally, the coronal region surrounding the initial event is left filled with a hot plasma, which maintains the decay phase of the flare for hours.

2. MAGNETIC FIELDS AND THE FLARE PLASMA

Although the first observations of a solar flare and its effect on the earth's magnetosphere were made in 1859, little progress in understanding flares was made until the late 1930's when flare occurrence rates were found to increase with the magnetic complexity of the associated sunspot group. Radio bursts and solar cosmic rays from the sun were first detected in the early 1940's, indicating that solar flares are extremely complex and energetic phenomena and that electromagnetic radiation and particles are emitted over a broad spectrum.

More recent research on the structure of the preflare atmosphere has shown that magnetic fields are the only possible energy source sufficient to produce the observed flare effects in the times and volumes involved. Flare statistics show that the larger the flare, the longer the interval required before another flare will occur in the same magnetic center. Theoretical considerations indicate that there are two ways in which free energy of the current-carrying magnetic field may be stored: the current sheet and the force-free field. Hence, one of the key problems of flare research is to determine whether the metastable preflare magnetic-field configuration has one of these forms.

Although the impulsive phase particle acceleration marks the beginning of the rapid flare energy release, many flares are preceded by a gradual heating phase, which can be observed at centimeter and millimeter wavelengths in the radio band, in the visible at Hα, and in the euv and soft x-ray emissions. Simultaneously, Hα filaments become activated and may begin to erupt from the flare location. A key question is whether these flare precusors signal the beginning of the flare instability. High-resolution magnetic and radio observations have shown that the underlying magnetic fields begin to change in this preflare period, but whether or how this destroys the metastable state is unknown.

The impulsive phase is clearly nonthermal, as shown by the appearance of hard x rays with power-law spectra and by the emission of impulsive microwave and Type III radio bursts. Although much has been learned about this impulsive phase, we do not yet know where the electrons and ions are accelerated. Since the energetic electrons on many occasions appear to contain the bulk of the total flare energy, a fundamental problem of flare research is to locate the site, or sites, of particle acceleration.

The main phase of a flare is complex and includes mass motions (surges and eruptive prominences), chromospheric brightenings, soft x-ray emissions, radio emissions, and possibly expanding clouds of nonthermal particles. Much of our information concerning these complex phenomena has been derived from two decades of radio observations and, more recently, from the highly successful Skylab (ATM) mission. Images of the soft x-ray emitting flare plasma have been obtained and clearly show loops of bright plasma. Theories of the cooling of such loops have been developed, primarily on the basis of data from the OSO series of satellites, and the regimes in which cooling by conduction or radiation is dominant have been established. Spectrograms, showing flare spectra simultaneously throughout the visible and near ir, have established the temperature and density distributions of the low-temperature flare plasma, while space observations of x-ray emission have yielded models of the temperature and density structure of the high-temperature flare plasma.

During the main phase, immediately following the impulsive phase, a shock wave is sometimes produced. Radio-heliographic observations have shown that behind the shock wave there is often a plasmoid of very energetic electrons. This mass ejection is clearly seen in the white-light photographs from the ATM mission. The ejected materials and shock wave then travel through interplanetary space and eventually cause disturbances in the terrestrial magnetosphere.

During and after the main phase, the heated plasma expands into the looplike structures formed by the active region magnetic field and then cools. The trapped plasma apparently condenses, and eventually the temperature drops to 10^4 K and the material falls back to the solar surface. The exact balance between radiation and conductive cooling in the different coronal regions is unclear. There also may be additional energy input through the late phase of the flare.

Accelerated particles also propagate through the coronal field structure. Some of these, as determined from radio-heliograph observations, become trapped in very large magnetic loops. The details of the propagation, trapping, and injection into the interplanetary medium, when understood, will clarify the questions associated with particle behavior in dynamic magnetic fields and plasmas.

3. ACCELERATION PROCESSES

Our present knowledge of solar particle-acceleration processes has come primarily from observations of hard x-ray and radio emission from the sun and from direct observation of the accelerated particles released into the interplanetary medium. Since the electron–proton bremsstrahlung radiation process is better understood than the processes of radio emission and absorption or particle escape and propagation, most of the quantitative information about nonthermal electrons on the sun has been obtained from the interpretation of the hard x-ray measurements. Direct interplanetary observations and gamma-ray spectral-line measurements provide information on the acceleration of protons and nuclei.

At present, we know of at least two distinct acceleration processes in solar flares. One process is a highly efficient accelerator of electrons (and perhaps protons) to energies of about 10^2 keV, which must be closely related to the conversion of magnetic energy into flares. A second process follows the first one in some flares and accelerates protons and other nuclei, as well as electrons, to MeV and even GeV energies. This latter process apparently is able to duplicate, at low energies, the galactic cosmic-ray acceleration mechanism and may be linked to the presence of flare-induced shock waves in the solar atmosphere. Estimates suggest that the initial electron beam carries the bulk of the flare energy, but additional electron acceleration and heating may occur throughout the flare.

4. ELECTRONS AND X RAYS

The electron energy spectrum inferred from x-ray observations usually fits a power law in energy, sometimes with a steepening above 100 keV. For a small flare, although the duration of the acceleration process obtained from the x-ray observations is typically about 100 sec, the overall event appears to be made up of short bursts of acceleration with time scales of 1–10 sec. The true time scale of the acceleration may be even shorter, but current x-ray instrumentation is limited in temporal resolution to about 1 sec.

Observations in the interplanetary medium provide a direct sample of accelerated electrons. Comparisons of the number of escaping electrons with the inferred number of x-ray emitting electrons indicate that the acceleration region is located in the lower corona and that generally only a small fraction of the accelerated electrons escape. The presence of energetic electrons at all times in the near-earth interplanetary medium, even though the solar wind is always sweeping them out of the inner solar system, suggests that the sun may be accelerating these particles almost continuously. Additional evidence in support of this suggestion is the observation of many solar Type III radio bursts that are not flare-associated and do not coincide in time with hard x rays.

While the acceleration of electrons is intimately associated with hard x rays, impulsive microwave bursts, and Type III bursts, the acceleration of nuclei appears to have a strong association with shock waves. Type II radio bursts, which also are believed to be associated with shock waves, accompany proton flares.

5. NEUTRONS AND GAMMA RAYS

Gamma rays associated with solar flares were first detected during the large flares of August 4 and August 7, 1972, with an instrument on OSO-7. Further observations with higher sensitivity and energy resolution can provide information on the number, energy spectrum, and time evolution of energetic protons and nuclei at, or near, the site of the nuclear accelerator. Solar gamma rays and neutrons are produced by interactions between accelerated charged particles and the ambient solar atmosphere and propagate directly to the earth, undeflected by magnetic fields.

Gamma-ray spectral lines from excited nuclei, such as that at 4.4 MeV from ^{12}C, are considered as "prompt" emissions, because the excited levels decay in time intervals that are much shorter than the characteristic times of the flare. These gamma-ray line intensities therefore follow the time dependence of the flux of accelerated particles in the interaction region. It appears that the important question of whether protons and electrons are accelerated by the same mechanisms can best be studied by the simultaneous observation of gamma-ray lines and the hard x-ray continuum.

The production of the 2.2-MeV and 0.51-MeV gamma-ray spectral lines involves the more complex processes of neutron capture and positron annihilation. As a result of the finite capture time of neutrons in the photosphere, the half-lives of the positron emitters, and deceleration times of the positrons, both the 2.2-MeV and 0.51-MeV lines are considerably delayed with respect to the prompt nuclear de-excitation lines.

Neutrons of energies 10^6 to 10^8 eV result mainly from the disintegration of ^{4}He nuclei in proton–alpha particle interactions in the chromosphere or lower corona. Those neutrons initially moving upward escape from the sun and should be detectable near the earth. The majority of the downward moving neutrons are thermalized in the photosphere before they decay or are captured by protons or ^{3}He nuclei. Capture by protons produces gamma-ray line emission at 2.2 MeV, but capture by ^{3}He results in tritium formation without any photon emission. The latter process is the only method of detérmining the photospheric ^{3}He abundance, because, for a given number of neutrons, the 2.2-MeV emission from proton capture will decrease as the ^{3}He abundance increases. This abundance is of considerable importance for the problem of solar neutrino emission and also for the problem of element synthesis in the "primeval fireball" that may have started the universe.

The search for neutrons from the sun constitutes one of the major observational problems in solar astrophysics at present. In spite of the certainty that the sun *must* generate neutrons, both near the photosphere and during particle acceleration, no solar neutron flux has yet been detected unambiguously in balloon or satellite experiments to intensity levels of 10^{-2} neutron cm^{-2} sec^{-1}.

Energetic positrons in solar flares from the decay of π^+ mesons and radioactive nuclei decelerate by interactions with the ambient solar atmosphere to energies less than 10^3 eV before they can annihilate. This deceleration time depends on the density and magnetic field of the medium. A fraction of the positrons may also escape from the flare region and could be detected in the interplanetary medium. The positrons can annihilate with free electrons to produce two 0.51-MeV gamma rays per annihilation; or they may form a positronium atom, which also produces gamma rays. It appears that future gamma-ray detectors with good energy resolution may resolve radiation from positronium annihilation and thereby detect the existence of this exotic atomic species in solar flares.

6. ENERGETIC PARTICLES

The charges and chemical composition of the energetic particles accelerated at the sun are a function of both the acceleration process and the properties of the source region; therefore, observations of energy spectra, particle composition, and event time histories provide information on these processes. The presence of rare isotopes such as ^{3}He on the solar surface provides information on the dynamics of particle acceleration and storage in the source region.

The charge composition of solar cosmic rays is observed to be strongly energy-dependent. Above about 10 MeV/nucleon it appears that a representative sample of the elements helium through iron is accelerated. Measurements from satellites and sounding rockets have shown that the charge composition of these elements (He–Fe) with energies >10 MeV/nucleon does not change markedly from event to event and is in good agreement with the spectroscopically determined abundances in the solar atmosphere. In contrast, below 2 MeV/nucleon, all measurements show a systematic enhancement of heavy nuclei at low energies relative to their abundance at higher energies. The enhancement factor is an increasing function of charge from $Z = 2$ to at least $Z = 44$.

The isotopic analysis of solar cosmic rays is of basic importance in understanding the nucleosynthesis of the solar material and the dynamics of the acceleration process. New detection technology permits all individual isotopes of elements up to iron to be measured and will be of fundamental importance in understanding the origin of the elements in the sun.

The only isotopes identified so far are those of He and H. A most surprising result is the frequent presence of large amounts of ^{3}He in solar cosmic-ray events. The expected value of ^{3}He/^{4}He was on the order of 5×10^{-4}, the value measured for the solar wind. A large number of ^{3}He events are observed, however, with ^{3}He/^{4}He ratios ranging from 0.1 to >1. Assuming that these must represent the spallation products of energetic He, C, N, O on the nuclei of the solar atmosphere, a ratio of 0.2 requires passage of the nuclei through some 6 g/cm^2 of material, which is quite unrealistic, particularly as there is an almost complete absence of deuterium and tritium. From spallation these isotopes would be expected in abundances almost equal to that of ^{3}He, but the measured upper limits are frequently several orders of magnitude lower.

C. Solar Active Regions

1. ACTIVE REGIONS AT VISIBLE, EUV, RADIO, AND SOFT X-RAY WAVELENGTHS

With the development of the spectroheliograph in the last decade of the nineteenth century, it was discovered that sunspot groups, the earliest known manifestation of solar activity, are accompanied by a complex of vortical structures, bright plages, and dark filamentary clouds that generally outlive the sunspots by many months. Synoptic observations with spectroheliographs led to the concept of the chromospheric active region, and by the early 1950's a clear picture of the evolution of the chromospheric structures had emerged. The coronagraph also made it possible to observe the corona above active regions as they pass the solar limb, showing that the active region is a coronal phenomenon as well. The arched or looplike structures observed there show that active regions are primarily regions of closed magnetic fields. One of the most astonishing discoveries of this period was that the coronal emission comes principally from highly ionized iron, and that the corona is 500 times hotter than the underlying chromosphere. The development of radio interferometers in the 1950's made it possible for the first time to observe the slowly varying thermal emission from coronal active regions against the disk; and beginning in the 1960's, active regions have been observed both against the disk and at the limb with increasing resolution at uv, xuv, and soft x-ray wavelengths. These observations have made manifest the complex three-dimensional structure of active regions from their base in the photosphere, through the chromosphere and transitional region, into the inner corona and eventually to the critical point where the solar wind originates. The observations carried out from spacecraft, especially the high-resolution ATM observations, have led to the development of comprehensive models of active regions, which include, in some cases, modeling of the complex magnetic structures. Active regions are, in a sense, the fundamental unit of solar activity and the most persistent and visible manifestations of solar magnetism, so that a detailed physical understanding of active regions and their evolution is basic to an understanding of solar activity.

A typical active region as seen at centimeter radio wavelengths is composed of a compact bright core of a few seconds of arc angular size with an extended weak halo several minutes of arc in size. The intense core radiation, which is strongly polarized, is believed to be caused by gyro radiation, while thermal bremsstrahlung is responsible for the weaker halo. The radio observations are generally consistent with euv and soft x-ray observations and suggest the model of active regions as magnetically contained loop structures. At wavelengths from 1 cm to several millimeters, the core is not dominant and the entire active region can be explained by thermal bremsstrahlung. Interpretations of these data lead to estimates of magnetic-field strength, electron temperature, and density above active regions.

2. MASS AND ENERGY TRANSPORT IN ACTIVE REGIONS

The problem of mass and energy balance in the chromosphere, transition layer, and corona of active regions is more difficult than in the case of the quiet sun. Magnetic fields clearly play a fundamental role in determining the structure of active regions, and observations, carried out primarily on rockets, have shown that the spectral-line profiles of transition region lines exceed thermal width by a factor of 2 to 3, suggesting a nonthermal heating process. The magnetic-field network may form a channel for the development of waves into shocks and result in chromospheric and coronal heating. Thermal models have been developed that take such nonthermal heating processes into account and reproduce, in a general way, the observed thermal structure of active regions. The possibility of abundance variations in coronal loops and the need for precise structural models (density and temperature) that allow the computation of the conductive and radiative flux throughout the active region established the need for a more powerful observational capability before the fundamental problem of the energy and mass balances of active regions and their relationships to fundamental magnetic processes in the sun can be solved.

The coronal magnetic structure must be important in the energy balance of the solar atmosphere, as the thermal conductivity of the coronal plasma depends on the field. Skylab observations of loop structures and the inferred steep thermal gradients associated with cool features such as prominences and with hot structures such as x-ray emitting loops confirm the important role of the coronal magnetic field. The inhomogeneity of the coronal field and the subsequent control of thermal conduction and inhibition or enhancement of the conductive energy loss represented by the solar wind may be the principal factors that distinguish the quiet corona and active regions.

The chromospheric and coronal layers of active regions may be heated by two types of mechanism: dissipation of mechanical energy carried upward from the solar convection zone, dissipation of energy stored in the magnetic field. A major problem for the future is to determine the regimes and locations where each kind of heating occurs.

3. STRUCTURE AND DYNAMICS OF CORONAL PLASMA LOOPS AND ARCHES

The general morphology of coronal structures, first described in 1944, is based on observations of the typical brightness changes of the emission from plasma structures confined by the magnetic field. In general, changes in coronal structures fall into three categories. These are (in order of decreasing frequency): changes in the emission along pre-existing magnetic features; changes in the shapes of loops and arches, presumably in response to changes in the magnetic field caused by photospheric motions; reconnections of the coronal field. All of these can occur on either very short (10 sec) or very long (10^5 sec) time scales.

Little progress has been made in the theoretical description of these structures because of our lack of understanding of the basic magnetohydrodynamic processes. This is really the study of the dynamics of a tenuous plasma with changing magnetic fields and in which currents distort the plasma and field distributions. Although the fields that thread galaxies and nebulae will never be studied in as much detail as the coronal structures, we may come to understand the structure and evolution of distant objects better by understanding the dynamics of the solar corona. The similarity of much of the structure observed in active galaxies to the structures and bursts observed over active regions and in prominences on the sun is striking, suggesting similar physical effects.

4. MASS AND ENERGY TRANSPORT IN PROMINENCES AND FILAMENTS

Prominences present intriguing problems in their mass and energy balance. The maintenance of their low temperature in the midst of the much hotter corona is presumably caused by shielding by magnetic fields against thermal conduction. Analyses of OSO and ATM measurements of EUV radiation from prominences indicate that individual threads of prominence material are sheathed by a transition layer with a thickness comparable with that above the quiet chromosphere. Thus, a magnetic field perpendicular to the temperature gradient is necessary to prevent conductive flux into the core from exceeding radiative losses from the core. Since the individual prominence threads are narrow, a few seconds of arc or less, high spatial resolution of a second of arc or better is required to resolve individual threads.

With high spatial resolution EUV and XUV measurements, the radial and axial variations of temperature and density can be determined for individual threads, as well as the conductive and radiative fluxes. By monitoring, as a function of time, the EUV and XUV emissions from individual threads and the surrounding corona, the heating and/or cooling of material in the threads and their surroundings can be measured. From these measurements, information needed to develop a self-consistent model will be obtained, making possible a detailed understanding of how the prominence interacts with the surroundings, including, for example, how material cools and condenses out of the corona and falls into the chromosphere.

It will be extremely helpful to measure the effects of mass motions for material at temperatures intermediate between coronal and chromospheric temperatures. Clearly, measurements of intensities, line profiles, and Doppler shifts for a range of spectral lines, covering the temperature range 10^4 to 10^6 K, would yield considerable insight into the dynamics and physical conditions in prominences and how prominences interact with their surroundings.

5. SUNSPOTS AND THE MAGNETIC CYCLE

The sun has long been recognized as the first-discovered magnetic star. It has been only in the past 5 years, however, that we have had observational descriptions of the emergence and evolution of the magnetic flux everywhere on the solar surface, especially outside sunspots.

In the past 5 years, extensive use of many new techniques of measuring the magnetic fields at high resolution has led to the realization that most of the magnetic flux at the solar surface exists in knots only a few hundred kilometers in diameter with field strengths of 1000–2000 gauss. Cinematic techniques have revealed that sunspots decay through outward streaming of these elemental magnetic features toward nearby supergranule boundaries. No theory has yet succeeded, however, in explaining how concentrated fields of magnetic knots and sunspots can be stable and as long-lived as observed or, indeed, why they exist at all.

Sunspots offer a unique opportunity to study Alfvén waves—an energy transport process of great astrophysical importance. High-resolution observations have revealed that sunspots are the seat of strong oscillatory phenomena. Oscillations (umbral flashes and photospheric oscillations) with a 160-sec period are seen in the umbra, and running waves with 300-sec period flow out through the penumbra. These waves may be an important key to why sunspots are dark. They are just at the limit of present-day instruments, and high-resolution observations from space are needed for further study.

Knowledge of how the solar magnetic fields interact with the plasma in the photosphere, chromosphere, and corona has accumulated slowly. It has become clear from the ATM pictures that fields from widely separated regions on the surface can reconnect to form large-scale loops linking sunspots even in different hemispheres. The study of flare activity in one region and its influence on another region, possibly producing sympathetic flares and radio emissions, is now benefiting from these direct observations of the interconnections. The problem remains, however, of how rapidly and in what manner the magnetic fields dissipate their strength and reconnect in the solar corona.

D. Solar Interior, Convection, and Activity

The interior of the sun contains the source of energy that drives the convection and creates the magnetic fields and activity at the surface. The convection and nonuniform rotation of the sun are a consequence of the outflow of energy, and the magnetic fields follow from the nonuniform rotation and convection. The sunspots, the flares, and explosive eruptions, as well as the chromosphere, the corona, and the solar wind are, in turn, consequences of the convection and magnetic fields. After 40 years of work, this qualitative picture is now fairly well established for the sun, and although it is generally presumed that most other main-sequence stars operate in much the same way, there are as yet few, if any, direct observational checks for other stars. Thus, the sun must be the guinea pig for the general study of stellar activity.

Close scrutiny of the sun, however, shows that there are several basic effects that were unexpected and remain unexplained. The most fundamental of them is the observed absence of neutrinos from the nuclear-energy production in the central core. Current theory and laboratory experimentation demand their presence in large numbers. The explanation for this discrepancy could be as exotic as a nonvanishing rest mass for the neutrino or as straightforward as an incorrect nuclear reaction rate. Whatever the final explanation, the crisis in solar theory is a crisis for the whole theory of all stellar interiors and evolution.

The activity of the sun is the direct consequence of the convection and subsurface circulation. The problem of deducing the form of the convection depends on the depth at which it begins, which is determined by the (now uncertain) properties of the solar interior and the hydrodynamics of the outer envelope of the sun. The hydrodynamic problem is difficult, because, among other things, the convection extends upward from the interior, across a density decrease of a factor of 10^5, to the photosphere. There are several large-scale circulation patterns that are theoretically possible, and there is observational evidence that the sun switches from one to another, with remarkable consequences for solar activity. The present circulation pattern generates strong azimuthal fields beneath the surface, leading to the 22-year cycle of spots, flares, and solar wind. Observations also show that during the period A.D. 1640 to 1710, solar activity, as well as the corona, was absent.

The question of the convective origin of magnetic activity is fundamental to an understanding of the activity of the sun and the activity of many other stars and to an understanding of the extension of the sun into space with its effects upon earth. The hydrodynamic theory of convection and circulation in a stratified atmosphere is developing rapidly, so that in the next few years we should have a much better understanding of the physics and should have available reliable numerical models for comparison with observation. It is imperative that ground-based optical observations of the circulation should advance to test the various theoretical possibilities. Consequently, it is most disturbing to note that at present there is a backward trend at many observatories away from such difficult observations. Because the sun is the one star where the convection and circulation can be observed, it is of central importance to stellar physics to pursue this basic problem. Solar convection is the only circumstance provided to us for working out the basic physics of solar and stellar activity.

E. Structure and Energetics of the Quiet Solar Atmosphere

Solar physicists have long been concerned with understanding the details of the nonradiative energy flow that maintains the corona and the density, temperature, and velocity structure that are thereby created.

A decade ago the general picture of the chromosphere and corona was available from ground observations (eclipses, radio observations), concepts of coronal heating were crudely defined from ground observation and theory, and the existence of a sharp temperature rise between the chromosphere and corona was well recognized. The first experimental probing of the sharp transition zone between the 10^4 K chromosphere and the 10^6 K corona had been obtained from rocket flights that observed the unimaged spectrum of the chromosphere and corona in the extreme ultraviolet region.

A major step forward occurred in the late 1960's when the OSO-4 and OSO-6 spacecraft provided imagery and spectroscopy at about 1 min of arc resolution of the quiet atmosphere in the extreme ultraviolet leading directly to quantitative models of the mean structure of the quiet chromosphere, transition zone, and corona and, for the first time, precise estimates of energy balance (e.g., energy losses from the corona by radiation and thermal conduction). There was also steady progress on the important problem of determining the thermal structure of the temperature minimum immediately above the photosphere, where the solar temperature ceases its outward decline at a minimum value of about 4200 K and begins to increase to chromospheric and coronal values. The thermal structure of this region is important because most of the nonradiative energy available in the photosphere is dissipated near the temperature minimum and only a small fraction leaks through to heat the chromosphere and corona and drive the solar wind. The analysis of far ultraviolet and infrared observations and line profile data, studied with new theoretical capabilities in nonthermodynamic equilibrium radiative transfer, is leading to a rather satisfactory model of this region.

At the same time, moreover, the critical role of inhomogeneities in interpretating the physics of the upper solar atmosphere was becoming clearer. Improvements in ground-based instrumentation revealed the complexity of the Hα and K line chromosphere. Rocket flights of prototype ATM instrumentation gave the first detailed observations of the inhomogeneous nature of the higher atmosphere—the transition zone and the low corona.

Finally, the ATM instruments have begun to reveal, with a resolution of about 5 sec of arc, the basic structures of the chromosphere and corona that actually govern the energetics of the solar atmosphere. While it is too early to assess properly the enormously important results of the ATM for even the restricted problem of coronal energetics, the following highlights are noteworthy:

1. The upward extension of the chromospheric emission network, representing areas of strong magnetic field and localized heating, spreads out at coronal levels. It has been shown that such a structure leads to a tenfold reduction in the thermal conductive loss from the corona over that if the atmosphere were homogeneous, so that detailed knowledge of the structure is crucial to an understanding of the energy balance.

2. The discovery that even the quiet corona is completely dominated by large-scale magnetic-field structures, thus demonstrating the fundamental role of the magnetic field in the coronal energy and mass balance.

3. The detection of short-lived nonperiodic pulses of ultraviolet emission that may well be associated with wave propagation carrying energy into the upper atmosphere.

An important aspect of the "quiet" atmosphere that emerges from recent high-resolution observations is its dynamically active state. The ATM data reveal order-of-magnitude intensity fluctuations in the XUV emission from the chromosphere and transition region. Interferometric observations at centimeter wavelengths also show time-varying structures of 10 sec of arc size, possibly associated with groups of spicules.

At the time of this writing, the final satellite in the OSO series has just been launched, with one aim being the observation of the velocity field and upward propagation of mechanical energy in the chromosphere and transition zone with the greatest possible spatial resolution. This experiment promises to resolve important questions of energy balance and dynamics of the lower solar atmosphere, as well as to broaden our understanding of radiative and hydrodynamic processes in the solar atmosphere.

F. Structure of the Corona

Even before the advent of photography, it was recognized from visual observations of the white-light corona during total solar eclipses that the corona had a complex and variable structure. Photography, and the recognition that the white-light corona consists of two components, one due to the scattering of photospheric light by electrons and the other by interplanetary dust, made it possible to derive the average density structure of the corona from its base to more than 10 solar radii. Observations of the zodiacal light, radio observations, and *in situ* observations by space probes have extended our knowledge of the mean density structure of the solar plasma and interplanetary medium beyond the orbit of earth.

The theoretical prediction of the solar wind, and its triumphant confirmation by observations from spacecraft near the earth in the early 1960's, has led to a unified, dynamical picture of an extended solar corona that fills the solar system. Fluctuations that occur in the local density, temperature, magnetic fields and the flux and spectrum of energetic particles near the earth are thus largely governed by fluctuations in the sun's inner corona. These coronal fluctuations are of two kinds: transient ejections of fast plasma usually associated with solar flares and rising prominences; and more or less stationary patterns in the inner corona that revolve with the sun and produce changes at a fixed point out in the solar system. It is the relatively slowly varying inner corona that structures the solar wind and provides its varying lower boundary.

Until recently, most of our knowledge of coronal structure has come from photographs of infrequent total solar eclipses, from balloon and rocket flights supplemented by radio studies, and from coronagraph observations of white light and forbidden lines in the inner corona. More recently, the OSO spacecraft and the ATM have provided long series of effectively continuous observations of the white-light corona, of the inner corona as seen in soft X rays, and of the base of the corona as seen in the extreme ultraviolet. These will provide a much clearer picture of both coronal structure and the closely related large-scale magnetic fields in the sun.

The accepted present view of large-scale coronal structures is, in fact, most easily stated in relation to the magnetic field. Briefly, there appear to be three types of coronal region:

1. Above young active regions the field is mostly closed and the plasma density is relatively high.
2. Near the poles and in the so-called coronal holes at all latitudes, the field lines are mostly open, apparently drawn out into interplanetary space by the expanding solar wind. The density in such regions is very low.
3. In old, complex active regions and above magnetic neutral lines in other regions the field lines are closed at low altitudes and open at high altitudes, leading to a coronal streamer extending out for many solar radii and containing a current sheet.

Evidently, most of the solar wind is produced in open field regions, which ultimately expand to fill the entire solid angle about the sun. We do not now know what fraction of the sun's surface is "open" and contributes to the normal solar wind nor how this fraction varies with solar activity or the solar cycle.

At present, there is no direct technique for measuring coronal magnetic fields, so crucial to an understanding of coronal structure. Indirect methods, however, are being pursued, such as coronagraph observations of forbidden-line polarization and radioheliograph observations. The radio evidence has come essentially from observations of Type IV radio bursts, which appear spatially either in the form of a wide ascending arc of multiple sources or as an arch-shaped magnetic flux tube that gradually expands. Precise tracking of low-frequency Type III radio bursts has verified the spiral curvature of the interplanetary field lines between the sun and the earth's orbit.

The nature of the coronal heating mechanism remains an outstanding problem of both solar physics and theoretical astrophysics. The theory that the corona is heated by waves generated at the top of the convective zone and dissipated in the corona remains widely accepted even though there is no concrete observational evidence for these waves in the corona. Fluctuations of velocity and intensity in the chromosphere provide the strongest evidence for the presence of waves in the atmosphere, and the recently detected uv pulses observed from the ATM may be the first evidence of wave propagation into the upper chromosphere.

G. Solar Wind

The observational verification of the existence of the solar wind was made less than 15 years ago. Subsequent years of space observation have led to a detailed and comprehensive understanding of the properties of the solar wind near the orbit of earth. In few astrophysical situations can one so accurately determine the density and flow speed of plasma, the temperatures of the electrons and the most abundant ions, the rate of electron heat conduction, the strength and direction of the magnetic field, or the abundances of the most common elements. The basic patterns of variability in the wind appear to have been identified and have found interpretation in terms of the expected hydromagnetic waves in a magnetized plasma, the streams and shocks produced by spatial and temporal variations in the corona, and large-scale magnetic structures related to similar features at the solar photosphere.

This wealth of observation and interpretation has led to a rapid advance in the complexity and variety of theoretical models of solar-wind phenomena. The basic origin of the solar wind in a fluidlike expansion of the corona has been treated in models incorporating such physical effects as noncollisional electron heat conduction, magnetic forces, the effects of wave pressures and energy dissipation, and the presence of minor chemical constituents. This advance has proceeded to the point where the observational knowledge that inspired it is no longer adequate to distinguish between alternate models or evaluate the importance of any of several competing effects. Models of the large-scale solar-wind structures (i.e., structures covering large parts of the solar system) have not been developed to a similar degree of physical sophistication; they are only now confronting the tests (and the resulting refinement) provided by observations made over a wide range of heliocentric distances.

There are three important remaining problems in the solar wind that appear to stand out in the context of this report:

1. What is the quantitative relationship of the physical state of the solar wind to conditions at its source in the corona? This problem is significant both in the area of solar–terrestrial relations and in the broader context of mass loss from stars.

2. What is the detailed chemical composition of the solar wind, and how does it relate to that in the corona, the solar chromosphere, and the photosphere? This problem is discussed in the following section, but the observations of solar-wind ions necessary to attack it also give direct information on the ionization temperature at the coronal source of the solar wind, and are thus related to problem 1 above.

3. What is the angular momentum carried by the present-day solar wind, and is this loss to the sun significant in the evolution of the sun? Although observations made to date indicate that this is a significant effect, they are plagued by possible calibration uncertainties so that the result is not free of question. The careful calibration, perhaps by inflight roll maneuvers, of a high-resolution solar-wind detector system is required to resolve the problem. The observations must ultimately be extended out of the ecliptic plane.

Another important area of future interplanetary research deserves emphasis here even though it is not a specific problem in the same sense as those listed above. All interplanetary observations made to date have been confined to the ecliptic plane, or the region within 7° of the solar equator. The solar wind in this region is known to be modulated by solar activity, and the influence of the modulated wind on the planets (from Mercury to Jupiter) has been widely explored. Since we know that the solar corona encloses the entire sun, we expect that the solar wind extends outward in all directions from the sun. The radio scintillation observations also indicate that the solar wind at very high latitudes is quieter but faster on the average, presumably because of its origin in the extended coronal holes that cover the polar regions of the sun.

The greater part of the interplanetary region remains unexplored. The latitudes removed from the solar equator probably contain more extreme examples of solar-wind structures. They may, of course, also display entirely new phenomena. The expected variability is closely related to the problem 1 described above. Only over the poles of the sun can we expect to observe the wind from a large coronal hole without disturbance produced by neighboring active regions; the coronal hole must be understood if we are to have a picture of the simplest form of the coronal expansion. Only at middle latitudes can we observe squarely into the strongest solar activity. The solar-wind velocity, density, variability, and temperature, as well as the magnetic field carried from the sun, the cosmic rays admitted along the field, and the energetic solar particles escaping outward along it, all bear the marks of their origins in a latitudinally dependent corona. The corona itself, the profiles of active regions, the magnetic fields, and patterns of convection and circulation in the polar cap can all be viewed with a new perspective from a spacecraft at high solar latitudes.

H. Solar Composition

Nearly half a century ago, H. N. Russell first estimated the chemical composition of the sun's atmosphere. Since then, photospheric abundances have been refined by the increased availability of atomic data, by improved spectrophotometry, and by theory. Independent determinations of the solar abundance in the corona, using both visible coronal lines and xuv lines arising in the chromosphere–corona transition region and in the corona, in many cases yield somewhat different abundances. These findings have stimulated the study of diffusion and loss processes in the corona and transition region.

The delineation of the acceleration and escape mechanisms for solar mass loss and their relationship to distortions of the mean solar abundance distribution in solar wind and solar cosmic-ray particles, and to possible abundance inhomogeneities in the lower corona, are important new areas of solar research.

The substantial improvements in spectral and spatial resolution, sensitivity, and photometric accuracy at soft x-ray and euv wavelengths promise significant, and in many cases definitive, new observational advances. Abundance studies carried out at these wavelengths are especially valuable since the lines are optically thin and the theoretical atomic rate constants required for the analysis are more accurately known for the simple ions that are most abundant in the corona.

The chemical composition of the solar wind is of interest in the broader context of solar abundances and enrichment of the interstellar medium. The solar-wind observations made to date have shown that the abundance of helium in the solar wind is 30 to 50 percent smaller than the normal solar abundance and varies widely over a range of time scales. The largest variations (as much as an order of magnitude) occur on the scale of hours and appear to be related to solar-flare activity, while variations by nearly a factor of 2 may have occurred on the time scale of the solar cycle. Although observations of other isotopes and elements (^{3}He, Ne, O, Ar, and Fe) have been made only occasionally, similar degrees of fluctuations are clearly present. The evidence points to an extreme fractionation of elements in the solar wind and probably in the solar corona as well.

Solar cosmic rays provide another method of obtaining solar abundances. Measurements from rocketborne nuclear emulsions and spacecraft detectors indicate that the composition of cosmic rays with energies above 10 MeV/nucleon is representative of normal solar composition. These measurements provide the best means of obtaining isotopic composition. Recent spacecraft observations and track studies in plastic sheets, lunar rocks, and other materials show a consistent enhancement of the abundance of heavy elements among solar-flare particles below 10 MeV/nucleon. The enhancement increases with increasing atomic number and with decreasing energy. This enhancement has recently been shown to prevail in the long-lived nonflare-associated low-energy particle fluxes as well, suggesting that fractionation as well as selection processes are operating. These low-energy solar cosmic-ray abundances are more similar to galactic cosmic-ray abundances than to normal solar abundances. It is possible that similar enhancement processes occur in the acceleration of galactic cosmic rays.

Spacecraft measurements in recent years have also yielded isotopic abundances of ^{2}H, ^{3}H, and ^{3}He in solar cosmic rays. Many flare events, mostly small ones, produce an anomalously high abundance of ^{3}He. These ^{3}He-rich flares do not produce ^{2}H and ^{3}H, which would be expected if the ^{3}He were produced by the interaction of the accelerated protons with the solar atmosphere. Thus, more complex acceleration and interaction processes must be occurring. Gamma-ray line emissions have been recently observed from two large solar flares. These measurements are able to provide the first upper-limit estimate of the solar photosphere ^{3}He abundance, which in turn provides a measure of the primordial deuterium abundance in the universe.

The future problems in this area and the methods to solve them are clear. We need to understand the fractionation processes that occur in the solar atmosphere and the selection and acceleration processes for solar wind and solar cosmic particles. These processes are likely to be important in understanding the acceleration of galactic cosmic rays and in the interpretation of stellar coronal spectroscopy. We also wish to obtain the isotopic abundances, in particular for isotopes such as ^{3}He.

I. Solar–Terrestrial Interactions

The magnetosphere, ionosphere, upper atmosphere, weather, and climate of the earth are all controlled, to some degree, by the various emissions from the sun. Plasma and particle emissions from the sun interact with the terrestrial magnetic field forming the magnetosphere, while instabilities induced by variations in the solar wind propagate downward, disturbing the ionosphere, possibly changing the energy balance in the troposphere with resulting changes in weather and climate. High-energy solar electromagnetic radiation ($\lambda \leq 1030$ Å) ionizes the neutral constituents of the earth's atmosphere producing the ionosphere, and the longer wavelength radiation ($1200 < \lambda < 3000$ Å) causes photodissociation of major atmospheric constituents, notably O_2, O_3, playing a key role in the chemical balance of the upper atmosphere. Fluctuations in those short-wavelength emissions produce corresponding variations in upper atmospheric charged and neutral composition. The bulk of the solar electromagnetic radiation penetrates to the troposphere and to the earth's surface and is responsible for atmospheric circulation and weather. Long-term variations in this region of the spectrum (3000 Å $\leq \lambda \leq 2.5$ μm) are a possible cause of long-term climate changes. Knowledge of both the output of the sun and its influence on the terrestrial environment have grown in the past decade of space observations, but there are still many unanswered questions.

1. CLIMATE SENSITIVITY TO SOLAR CONSTANT CHANGE

One of the major problems confronting mankind is an understanding of the terrestrial climate. Such an understanding is critical if we are to be able to foresee future climatic variation that may affect food production and similar activities.

Perhaps the most fundamental goal for climate theory is to determine the way in which the global mean-surface temperature of the earth changes in response to changes in the solar constant. Global radiation balance models in which surface temperature–ice cover–albedo feedback is included agree in predicting a temperature drop of 15 K and a catastrophic transition to an ice-covered earth for a 3 percent decrease in the solar constant. Recent work with such a model with parameters adjusted in the light of satellite albedo and infrared radiation measurements gives a 20 K temperature decrease and catastrophic ice cover for an 8 percent decrease in the solar constant. These results are all for an equilibrium climate, which, because of the thermal properties of the ocean, could take several centuries to be reached. An important missing component in these models is the surface temperature–ice cover–albedo feedback, which is presently of uncertain sign.

Long-term variations in the solar constant are expected as a result of the general evolution of the sun (75 percent increase in 10^9 to 10^{10}

years), variations in the conditions at the core of the sun (10^6 years), and variations at the base of the convective zone (10^4 years). Paleoclimatological evidence and observations of the scatter of luminosity in main-sequence stars indicate that the sun may have varied by as much as 10 percent in the past 10^6 years. On a shorter time scale, there is evidence of an 11-year sunspot cycle with an apparent 80-year modulation in amplitude.

Solar-constant measurements made during the past 50 years from mountaintops, high-flying aircraft, and balloons place limits of about ± 1 percent on the variability of the luminosity. Considerations of the brightness of sunspots and faculae indicate a possible variation of a few tenths of a percent because of solar activity, and instruments capable of measuring the solar constant to this accuracy are being developed for flight on balloons and satellites.

2. CLIMATE SENSITIVITY TO OZONE CHANGES

The effect on the energy balance of a change in NO_2 and O_3 concentrations due to changes in solar radiation and thus on global surface temperature has been found to be small for ozone changes of order 10 percent. Model calculations of the sensitivity of the general circulation to the infrared radiative energy balance suggest, however, that a change in the radiative properties of the stratosphere arising from a change in the O_3 concentration can have a significant effect on the circulation. This suggestion is of such importance to the question of solar–terrestrial relations that it should be investigated further with model calculations. Since the O_3 concentration is now being accurately measured, it would be useful to look for correlations in the real atmosphere between O_3 concentration and circulation indices.

O_3 is formed primarily by chemical processes involving the dissociation of O_2 by solar uv radiation. Thus, there is a need for careful observational and theoretical study of the relationship of O_3 concentration to variations in solar uv and to energetic particles dumped into the earth's atmosphere. The ultraviolet and extreme ultraviolet regions of the solar spectrum contribute less than 1 percent to the total solar radiation incident on the earth's upper atmosphere but show more extreme variations in intensity, because radiation in those wavelengths is formed in higher temperature regions of the solar atmosphere and is hence considerably more affected by solar activity. Absolute flux measurements vary in accuracy from about ± 20 percent in the ultraviolet region of the spectrum to about ± 50 percent in the extreme ultraviolet.

3. INFLUENCE OF SOLAR PARTICLES ON THE TERRESTRIAL ENVIRONMENT

Another area of intense interest is the possible link between solar particle emissions and the state of the earth's lower atmosphere, specifically, the influence of solar activity on terrestrial weather.

Measurements of solar-wind particle fluxes at 1 AU show significant short-lived variations in density and speed associated both with flare and prominence activity and with the passage of recurrent quiet features across the solar disk. Typically, the enhancements in density associated with these disturbances are of the order of 4–6, while velocities are increased by a factor of 2–3. Other studies have been made of the long-term variations in particle fluxes as a function of solar cycle during the period 1965–1970. Although there is some slight indication of a possible enhancement in energy flux during the period of solar maximum, the effect is only of the order of 20 percent and its statistical significance is questionable.

The interaction of the solar-wind particles and the solar magnetic field with the earth's magnetic field determines the basic structure of the magnetosphere. The dipolelike magnetic field of the earth is compressed on the sunward side and "combed" out into a cometlike tail on the nightside. Electrical currents driven by the interaction extend downward into the ionosphere. This basic structure is modulated by changes in the solar wind and may be intrinsically unstable. These changes modify the currents in the ionosphere and accelerate some of the plasma present in the magnetosphere. These locally energized particles then join the energetic particles from the sun to bombard the upper atmosphere and provide a second indirect link between it and the sun.

On the basis of the known energy fluxes in the solar wind and solar energetic particles, it can be argued that their influence on the troposphere must be small. The delicate chemical balance of the upper atmosphere, however, provides a means by which variations that themselves contain small amounts of energy can produce significant changes in weather and climate. Correlative evidence for solar-induced changes of this sort has been claimed for many years. The continued search for the physical chain of cause and effect by which solar plasma, magnetic fields, or particles can perturb the state of the earth's atmosphere is a necessary step in pursuing these matters and provides a justification both for study of the variations in the solar particle emission and its interaction with the terrestrial magnetosphere and atmosphere.

4. FLARES AND SHORT-TERM VARIABILITY

During flares, enhancements occur in the total solar radiative flux that range from a few percent at near ultraviolet wavelengths to factors of 2 in more energetic extreme ultraviolet lines and to orders of magnitude in soft x-ray wavelengths (< 20 Å). Although such enhancements typically last only a few minutes, they produce intense ionization in the upper atmosphere, particularly at D-region altitudes (< 100 km). The effects of high-energy particles emitted during the flare typically last for hours to days after the enhancement in radiative flux. These enter the upper atmosphere in the polar cap region and produce polar cap absorption and sudden commencement of magnetic storms. While the physics of these phenomena is relatively well understood, their effects in terms of communications disturbances and power failures, for example, are undesirable. Although they cannot be prevented, they can be predicted and their effects somewhat alleviated. At present, a network of solar-flare patrols, supported mainly by the Department of Defense, detects the onset of a flare and issues a warning. No method has yet been found of accurately predicting the onset of an event before it occurs, although such an ability would clearly be desirable.

5. CONCLUSIONS

There are indications on the basis of current theoretical models that small changes in solar luminosity (of the order of a few percent) can cause drastic variations in the terrestrial climate. While it is true that such variations represent only one of many factors that may produce such changes, it is equally true that variations of this magnitude in the solar constant, over a period of a solar cycle or longer, cannot be ruled out on the basis of existing experimental evidence. Recently, techniques have been developed that will enable the necessary solar constant and solar spectral irradiance measurements to be made to an accuracy sufficient to remove this uncertainly from the climate models. Clearly, such an action would contribute significantly to this area of research and should be undertaken as soon as is practically possible. The possible mechanisms linking solar output and the weather are far less well defined. It is difficult at this time to define the measurements required to provide a definitive solution to the problem, although it is evident that this area of research should receive serious attention. Since the variability of solar radiative flux, from visible to extreme ultraviolet wavelengths, is known to affect the chemistry and dynamics of the earth's upper atmosphere, it is clear that an effort should be made to determine the magnitude of the changes in solar emission as a function of both wavelength and activity. Once more, recent advances in detector technology now make such measurements feasible. Finally, it has been argued that solar-particle emission may provide the clue to the possible interrelationships between solar activity and weather. The current level of knowledge of the magnitude of the variability in solar-particle flux impinging on the magnetosphere and of the interaction between the solar wind and the magnetosphere is insufficient for us to understand the nature of possible linking mechanisms.

IV. PROPOSED SOLAR RESEARCH

A. Introduction

There are presently six general problem areas of solar research: flares, the active sun, the quiet sun, the solar interior and long-term variability, solar wind and coronal structure, and solar–terrestrial effects. Each of these research areas requires observations over an extended energy range of both electromagnetic and particulate emission and utilization of a variety of observational, theoretical, and analytical techniques.

In this section we outline a comprehensive program of solar research into the six major problem areas listed above and indicate which of the major modes of observational and theoretical procedures are appropriate to each problem. In each case, the observational requirements for spectral and angular resolution, sensitivity, time resolution, and duration will determine the required techniques. Table F.1 (at the end of this section) summarizes the major problems and the tools needed for their solutions.

B. Flares

An effective strategy for studying flares must contain three basic elements: a broad-band space program, capable of observing the range of flare emissions from gamma rays to ultraviolet wavelengths and providing comprehensive coronal transient and high-energy particle measurements; a worldwide ground-based network of observatories providing both synoptic and high-resolution studies of solar magnetic and velocity fields, mass motions, photospheric and chromospheric flare phenomena, and highly resolved solar radio burst observations; a strong theoretical, modeling, and data-analysis program to unite observation and theory into a comprehensive picture of the flare problem.

Key elements in this integrated attack are likely to be the Solar

Maximum Mission (SMM), a comprehensive, flare-oriented, free-flying satellite designed to study the entire event from preflare energy storage to the emission of high-energy material into the corona; high-resolution, high-energy, and facility solar Shuttle instruments; the particle and field monitoring International Sun–Earth Explorer (ISEE) satellites; the Air Force, Air Weather Service Solar Observing Optical Network (SOON) and other specialized solar observatories; the Very Large Array (VLA) and solar-dedicated radio observatories such as Clark Lake, Nancay, and Calgoora; a sophisticated computer modeling capability.

1. MAGNETIC FIELDS AND SOLAR-FLARE PLASMA

The study of preflare phenomena requires two basic observational modes: low-time-resolution monitoring at selected wavelengths and occasional very-high-time resolution at every accessible wavelength. For example, in studying the preflare buildup, space observations of the gradual growth of the active region, complemented by half-hourly magnetograms and velocity maps of active centers, will probably be sufficient. When activity is expected to be high, however, the time interval between observations should be decreased to 10 min (a practical goal for the SMM period) and then to seconds (for Shuttle-era flare research). Previous observations have established that the flare build-up period is of the order of hours for medium-size flares. Radio observations have demonstrated the preflare buildup of energy in the 2–3 sec of arc cores in this period. This particular property of active regions as seen at centimeter wavelengths might well determine the choice of the active region to be observed during intensive alert periods for the SMM. The buildup time is much shorter for the smallest events, but study of preflare conditions for these events, including the flares in coronal bright points, will require the highest spatial and temporal resolution. This will be a primary goal of flare research missions with facility-class instruments operating during short Shuttle missions when the chances of observing small flares will be excellent.

In order to understand preflare heating, we must study soft x-ray structures in active centers and preflare filament activations. These observations, when matched with calculations of electric currents based on high-resolution magnetic-field data, should reveal the detailed mechanisms for preflare heating and energy buildup. Observations of nonthermal x rays at the very highest sensitivity are necessary to determine if there is any particle acceleration associated with the preheating. The problem of preheating also requires theoretical work on heating mechanisms in plasmas located in slowly changing magnetic fields.

An important question is whether energy is stored in the corona prior to the flare. This requires simultaneous high-resolution and high-sensitivity soft x-ray observations to construct thermal models that might reveal dissipation by coronal currents; extremely high-sensitivity observations of hard x rays, gamma rays, and radio emissions from trapped energetic particles; observations of energetic interplanetary particles to understand possible storage, leakage, and propagation into the interplanetary medium.

Solution of the central problem of how the preflare state becomes metastable and how the magnetic-field configuration disrupts requires intensive observations during the precursor and impulsive phases. Microwave radio observations using the VLA can probe the upper chromosphere. Although complete synthesis of solar burst sources will not be possible with the VLA since flares are such short-lived phenomena, it will be possible to combine the outputs of each of the three arms of this facility independently to form three separate fan beams of a few seconds of arc resolution. Since these fan beams will have different position angles, it should be possible to determine the position of preflare and impulsive phase burst sources unambiguously and also to determine their two-dimensional sizes. Every effort should be made to get generous allotments of time on the VLA and other large arrays on a contingency basis during crucial periods of the SMM.

In order to learn the nature of the instability condition for the acceleration mechanisms to operate, and the types of electric fields and waves that are the accelerating agents, it is necessary to determine the acceleration site of the electrons and nucleons; to make measurements of the magnetic field, plasma temperature, density, and electric fields at the site; to determine the wave phenomena from velocity fields or from radio measurements.

Inferences about the magnetic-field configuration in the particle acceleration region may be made from satellite photographs of coronal structure. During the SMM period, it will be essential also to have occasional measurements of all the Stokes parameters for deduction of the total vector field in the photosphere below the flare center. Full vector field measurements are necessary for computer modeling of the particle acceleration region. For a later, more precise, attack on flare-related field configurations, a Shuttleborne Stokes polarimeter is needed. This instrument will provide observations of magnetic fields in long time sequences with a quality presently unattainable from the ground.

Several regimes have been identified in which different acceleration phenomena may occur, but a basic question is if particle acceleration occurs in all flares and whether there are quasi-thermal flares without any suprathermal particles. This question requires continuous monitoring of energetic interplanetary electrons and nucleons at high sensitivity, continuous observations and cataloging of Hα events and microwave events, and observations of hard x-ray events over a significant fraction of the solar cycle. The microwave and x-ray events will require higher sensitivity than has previously been available. Diagnostic studies at soft x-ray wavelengths are also necessary to identify nonthermal emission. These observations must be made during solar space missions such as the SMM and need to be combined with an effective data synthesis program.

If there are two kinds of flare, it is necessary to establish what the plasma conditions and the magnetic-field configurations are that cause their differences. Additional observations required for this problem are high-resolution (1 sec of arc) Hα pictures during a significant number of flares, high-resolution magnetograms, and measurements of the overall spectrum and intensity with time and the location of the thermal and nonthermal x rays. The determination of the flare classes can be made during this solar cycle; but determination of detailed differences may require measurements to smaller scale lengths (higher resolution) during the following cycle.

Problems of flare plasma heating and cooling require that the spatial distribution of hot plasma be known as a function of time and magnetic field, while turbulent velocities and plasma motions will contribute to our knowledge of the energy transport mechanisms. Ground-based Hα observations are needed to outline the pattern of heating by the hot coronal plasma created during the impulsive phase and to determine the response of the chromosphere to the energy input. Estimates of the mass of the plasmoid ejected during flares clearly show that the material must come from the chromosphere. Spectra in the region 3300–11000 Å will give quantitative measurements of mass motions and the flare temperature and density variation with height and time. Simultaneous sequences of high-resolution spectra of both the low-temperature and high-temperature flare plasmas will allow us to test proposed mechanisms of chromospheric evaporation.

2. ACCELERATION PROCESSES

The detailed investigation of the physical mechanisms that produce both impulsive and secondary particle accelerations rely heavily on space observations of the hard x-ray and gamma-ray emissions, *in situ* measurements of the accelerated particle streams, and radio observations of the associated burst events.

(a) IMPULSIVE PHASE A key question for understanding the impulsive phase of a flare concerns the location of the region of primary acceleration. Because of the small size of the region and its rapid development, Hα, microwave, and soft x-ray observations to the highest possible angular resolution ($\leq$1 sec of arc) on a time scale comparable with the impulsive phase ($\leq$1 sec) are required. Isotopic and chemical abundances are necessary to determine the path length of the accelerated particles, as are observations of nuclear gamma-ray lines and neutrons to determine the presence of accelerated nuclei in the more dense regions. Measurement of changes in coronal structures associated with the flare and stereoscopic views of the soft x-ray emitting region where the energy is dumped may also be useful. In short, to find the regions of primary acceleration will require detailed analysis of a number of simultaneous observations.

Related questions concern whether impulsive flare acceleration is identical with the acceleration mechanism that produces Type III radio bursts in nonflaring regions, whether the acceleration is composed of one burst or many short-lived "elementary bursts," and whether these bursts come from the same place or from different positions. These problems require a careful synthesis of data for a number of events in order to develop an understanding of the plasma parameters and the scaling involved in various possible acceleration mechanisms. The proper combination of hard x-ray and microwave observations could do much for this problem. In particular, the highest possible time resolution (as fast as 1 msec) is required of hard x-ray events as well as high time resolution ($\leq$0.1 sec) of x-ray spectra and x-ray locations to about 4 sec of arc. Also useful will be low-frequency radio observations of Type III bursts and a study of the magnetic-field structure, temperature, and density in the Type III emission region.

In considering the overall energetics of the flare event and the relationship between the primary particle acceleration and the radio emissions, it is important to know what the velocity field of the accelerated particles is, where they deposit their energy, and how these particles are related to those that produce the microwave radio burst. To answer these questions, observations are necessary at the highest possible spatial resolution with accurate spectral and time measurements of hard x-ray and microwave radio bursts and accurate spatial, density, and temporal measurements of soft x-ray emission. A stereoscopic view of emission regions would be particularly useful.

(b) SECOND-STAGE ACCELERATION PROCESSES Finding the distinction between the continuous and second-stage acceleration processes is an important problem. Does the acceleration to high energies occur in two steps? The second stage may be stochastic acceleration in turbulent fields; it is necessary to learn when and where it occurs and what are the plasma and magnetic-field conditions that instigate the second-step acceleration. It is possible that the energetic particles may be stored in the solar corona, so that one also needs to ask where and for how long they are stored and how they propagate in and escape from the corona.

Other questions regarding this stage of particle acceleration include whether there are any other acceleration processes that might produce a nearly continuous acceleration of particles in addition to flare and Type III and IV burst-associated mechanisms and how the acceleration process is related to the Type II and moving Type IV bursts. Finally, there may be as many as *three* different acceleration processes on the sun, and it is necessary to learn what are the conditions of injection and acceleration of particles in all of them.

To answer these questions a wide-ranging observational program is necessary. Particularly important are accurate spectral, spatial, and temporal measurements of the hard x-ray emission, which is the signature of the accelerated electrons, and similar measurements for gamma rays, which are the signature of the accelerated protons. The determination of continuous or nearly continuous acceleration from active regions requires extreme sensitivity to hard x rays and gamma rays. Simultaneously, observations are required of the physical conditions of the acceleration region, including high-resolution (~1 sec of arc) soft x-ray measurements of thermal and nonthermal heating, the density of the hot plasma, and accurate measurements of the magnetic field and its changes during the latter phases of the flare when the accelerator is operating. Observations of the accelerated electrons, protons, and nuclei released into the interplanetary medium are necessary, including the ionization state and elemental and isotopic abundances of the low-energy ions. Other observations that are useful are radio observations over the entire spectrum and coronal observations including observations of magnetic fields and high-sensitivity measurement of hard x-ray, gamma-ray, and radio emission from coronal regions. Finally, this observational program must be supplemented with theoretical modeling for the different acceleration processes.

3. THEORY, COMPUTER MODELING, AND DATA ANALYSIS

Our understanding of solar flares is becoming progressively more detailed, and the theoretical exploration of these new ideas must be pursued vigorously, in order that the flare problem might be finally solved.

At a certain stage in the exploration of theoretical concepts, it becomes essential to obtain detailed results for specific and realistic parameters and configurations. This requires "computer modeling" or "numerical experiments."

We foresee that understanding the collisionless shock waves produced by flares will require the development of computer codes to simulate interstreaming instability leading to empirical transport coefficients that can then be incorporated in a magnetohydrodynamic code. A separate code could explore test-particle motion in and near the shock front to determine whether the model leads to particle acceleration. Similar sets of interlocking codes could be developed to describe other flare-related processes. Such model studies would check theoretical ideas and would lead eventually to precise estimates of the radiation that would be produced by proposed flare models. These predictions would then be available for comparison with the observations.

The crucial interface between model analysis and observations will be the data derived from space experiments. If these data are to be fully effective, they must be readily accessible. Care must therefore be taken to ensure that data are compiled in a few standardized formats. Not only will this make the data readily comprehensible to many users, but also it will facilitate comparison with the output of computer modeling.

C. The Active Sun

Solar activity includes a wide range of phenomena at different levels of the solar atmosphere, with time scales from 10^{-1} to 10^{9} sec. We distinguish five fundamental problem areas:

1. Stability and structure of sunspots;
2. The structure and energy balance of active regions, plages, fibrils, and coronal condensations;
3. The structure and evolution of coronal loops and arches;
4. Mass and energy transport in chromospheric filaments;
5. That part of the variation of the solar constant attributable to active regions.

Underlying all of these problems is the necessity for a thorough understanding of the solar magnetic field as the basis for understanding the solar activity that it causes.

1. SUNSPOTS

The study of sunspots is important because of interest in basic plasma physics in the presence of strong magnetic fields, their effect on coronal heating, and their provision of an energy source for flares and plages.

There are important questions remaining to be answered in this area. First is the problem of the cooling of sunspots. This may be caused by Alfvén waves—a process of energy transport of great astrophysical importance. They have large velocity amplitudes (1–2 km/sec) and sizes less than 1 sec of arc; thus, to observe them, high temporal, spatial, and spectral resolution observations of umbral and coronal fields are required. Equally important is the question of why the sunspots are as stable as they are. Current ideas lead us to expect them to dissolve quickly.

It has been realized in recent years that most of the solar magnetic flux outside sunspots is concentrated in small regions with very high field strength, comparable to the magnetic fields in sunspots. This raises important questions concerning the properties of these knots and filigree elements, specifically size and field strength.

The study of sunspots requires a large optical facility to make routine observations at 0.1 sec of arc. Although the continued use of the most powerful ground-based telescopes is of the utmost importance, the development of a large (~1.25-m) diffraction-limited solar telescope for use in the Shuttle is also important for sunspot studies.

2. STRUCTURE, MASS, AND ENERGY BALANCE OF ACTIVE REGIONS

The observation of the relationship between the magnetic-field network and the structures of the transition zone has provided definitive evidence for the dominant role of the magnetic field in channeling the energy propagation modes responsible for the heating of coronal structures. The major problems posed by the dynamics of active regions include the relative importance and precise mechanisms of shock and magnetic heating, the relative magnitude of heat loss by radiative and conductive processes, and the connection between the wave motions and oscillations observed in the chromosphere and shock propagation in the transition region and corona. All of these require the development of a detailed dynamic model over the full range of temperatures in the solar atmosphere. Temperature, density, composition, magnetic field, and the ordered and random velocity field must be determined with at least 1 sec of arc resolution in the corona, and with sub second of arc resolution in the transition region and chromosphere. Requirements for spectral purity will approach $\lambda/\Delta\lambda \sim (2\text{–}3) \times 10^4$ for many observations.

In order to achieve these observational goals, a vigorous and coordinated program of ground-based and space observations is necessary. Observations with high resolution at XUV and soft X-ray wavelengths, which can result in stereoscopic information on active region structures, would prove useful. Existing radio, optical, and especially X-ray and EUV instruments flown on early Shuttle Sortie missions can make important new observations that were not possible with the ATM. The payload selected for the SMM also has capabilities that exceed those of the ATM in several areas.

Two major theoretical efforts, both requiring extensive computational support, are important for the study of active-region dynamics. One is improved theoretical calculations of atomic rate constants, especially for highly excited ions, in order to improve the accuracy of diagnostic techniques. A vigorous program in laboratory astrophysics is an important component of the effort to provide the basic atomic data that will be required to develop active region models with sufficient precision from the observation data base discussed above.

For the immediate future, the continued analysis of the ATM observations, and the analysis of the high-resolution OSO-8 chromospheric observations, combined with continued photospheric, coronagraph, and millimeter–centimeter observations should be pursued with the utmost vigor, in order to provide the improved theoretical base that will be required to analyze the higher resolution observations obtained with the SMM and in the Shuttle era.

3. STRUCTURE AND EVOLUTION OF CORONAL LOOPS, ARCHES, FILAMENTS, AND PROMINENCES

The structure of coronal loops and arches reflects the structure of the coronal magnetic field and influences the configuration of the outer corona and interplanetary medium. There are three major aspects of coronal loops and arches: the delineation of the relationship between coronal magnetic fields and the photospheric fields and the configuration of coronal currents, the energy balance and thermal structure of the loop material itself, the evolution of the coronal field and of the coronal current structure as reflected in changes observed in coronal loops.

Three types of observation program are required to carry out this scientific program. For the first program, direct coronal magnetic-field observations are highly desirable, and the development of an effective coronal magnetograph should be given high priority. Millimeter polarization data with even one order of magnitude lower resolution (~10 sec of arc) would be useful in estimating chromospheric magnetic fields, but high-resolution (<0.5 sec of arc) chromospheric magnetograms, combined with high-resolution (~0.5 to 1 sec of arc) euv, xuv, and soft x-ray images will eventually be required, along with sophisticated computational models of the coronal field. The second objective, the study of loop structure, will require high-resolution spectroheliograms at euv, xuv, and soft x-ray wavelengths. The final objective will require synoptic observation at high resolution to provide magnetograms and euv, xuv, and soft x-ray spectroheliograms with resolutions of ~1 sec of arc. Spectroscopic observations, obtained from spacecraft at other solar longitudes and latitudes (i.e., out-of-the-ecliptic missions) will be important. Observations on the Shuttle also will be of great importance for this problem area. Finally, the development of a cluster of euv, xuv, and soft x-ray imaging telescopes, which could be routinely flown on the Shuttle, could prove to be a useful basic tool for synoptic studies.

4. VARIATION OF THE SOLAR "CONSTANT"

The variation of the solar luminosity has a profound influence not only on theories of the solar interior but also on the interplanetary medium and on the earth. While the total energy in the ultraviolet, compared to the solar luminosity, is small, the effect of small changes in the ultraviolet flux on the chemistry of the earth's upper atmosphere, ionosphere, and magnetosphere is profound. The basic instrumentation required to monitor the total solar flux, with good spectral resolution, need not be complex and should in fact be compact so that it can be flown routinely on available free-flyers. Complete time coverage and reproducibility are important aspects of this program. The SMM provides an excellent opportunity to initiate a comprehensive synoptic monitoring program in the euv, xuv, and soft x-ray bands, to extend the observations currently being carried out on the Atmospheric Explorer Satellites.

D. The Quiet Sun

There are, at present, four major problem areas relating to the nature of the quiet sun: How is the outer solar atmosphere (chromosphere and corona) heated? What is the role of magnetic fields in the mass and energy balance of the quiet sun? How important are the effects of nonlocal thermodynamics? What is the solar chemical composition?

1. HEATING OF THE OUTER SOLAR ATMOSPHERE

Although most of the solar energy output escapes in radiation when it reaches the solar surface, a small fraction is responsible for most of the heating of the solar chromosphere and for the hot solar corona and solar wind. How this heating occurs is not at all clear, but it probably involves dissipation of some form of acoustic or magnetohydrodynamic waves, although such other effects as magnetic-field annihilation have been suggested. Present theories compare radiation, conduction, and other energy fluxes at different heights and derive the required dissipation of mechanical flux to account for the balance. This is then compared with dissipation theories to search for the origin of the heating.

Observations of the velocity (from Doppler shifts) and density (from line-intensity ratios) with 1 sec time and 1 sec of arc spatial resolution are required to determine the mechanical energy flux and dissipation at different heights. The resulting diagrams of velocity and energy dissipation versus spatial and temporal frequency (k, w diagrams) at different heights will provide the tool for comparison with wave propagation and dissipation theories. In particular, high-resolution observations of the structure of the velocity (k, w) diagram in the photosphere will test suggestions that the origin of mechanical wave motions responsible for coronal heating is to be found in acoustic waves in the outer convection zone and in nonradial pulsations. Needed for this work are ground-based and sub second of arc spacecraft observations of the solar velocity field with better than 20 m/sec sensitivity.

2. ROLE OF MAGNETIC FIELDS IN THE MASS AND ENERGY BALANCE OF THE QUIET SUN

The role of the magnetic fields in the overall energy balance of the solar atmosphere requires the determination of the flux of conductive, radiative, and kinetic energy, as well as the mass flow, for regions of different magnetic geometries. One might hope to see direct evidence for motions associated with magnetic heating or at least to find empirical relations from which the role of the magnetic field may be inferred. Solar features of prime interest include the chromospheric network, where fields of perhaps 10^3 G are concentrated in small areas; spicules, where mass and energy flow along field lines into the corona; prominences, which include a transition zone surprisingly similar to that of the average quiet sun in a region of vastly different geometry and magnetic field; x-ray and euv bright points; open and closed field regions inside and outside coronal holes, where the energy balance may be completely governed by the topology of the field.

Our present knowledge of these features suggests the following strategy:

(a) Develop theoretical modeling and diagnostic techniques and critically examine the assumptions upon which present theory is based.

(b) Continue the vigorous program of analysis of existing ATM and OSO-8 data.

(c) Determine the geometry and time dependence of magnetic fields in the quiet sun, using Stokes polarimeter observations in the radio, optical, and uv and using xuv and x-ray imagery, all with sub second of arc spatial resolution and several second temporal resolution.

(d) Determine the mass and energy flux associated with different magnetic structures, from visual, uv, and euv line profiles with sub second of arc spatial resolution and several second temporal resolution.

3. EFFECTS OF NONLOCAL THERMODYNAMICS

Models of the quiet sun and determinations of chemical abundances are typically made assuming one-dimensional plane-parallel geometry. It is essential to learn the errors inherent in this approach, including the importance of horizontal inhomogeneities. Spectroscopic measurements in the visible and ultraviolet with the highest possible spatial resolution (better than 0.5 sec of arc) are needed as a data base to answer this question, and efficient theoretical computing schemes for multilevel atoms in three-dimensional geometries must be developed.

At present, available diagnostics for the physical conditions and chemical abundances in the chromosphere, transition region, and corona assume time-independent local steady-state conditions. At least in the transition region, these assumptions cannot be valid, as the rates of diffusion, mass motion, and hot electron penetration are comparable with ionization and recombination times. Theoretical techniques for treating nonlocal and time-dependent ionization equilibria must be developed, as well as plasma physics studies of non-Maxwellian electron energy distributions. In addition, time sequences of profiles in many lines with less than 1 sec of arc spatial resolution are needed in the uv, euv, and soft x-ray regimes to determine the importance of these nonlocal effects.

4. SOLAR CHEMICAL COMPOSITION

Often cosmic abundances, particularly isotopic ratios, are assumed to be the solar values. It is, therefore, essential to derive accurately these abundances and develop reliable diagnostics for use elsewhere in astrophysics. The abundance problem requires accurate atomic parameters derived from theoretical calculations and laboratory measurement. Also, multilevel, non-LTE radiative-transfer computer codes, including codes for diatomic molecules, must be further developed. High-spectral-resolution observations in the uv, euv, xuv, and soft x-ray regimes are needed.

Preliminary calculations indicate that helium and probably other elements can diffuse upward in the transition region because of the steep temperature gradients, as well as settle downward through the corona. Consequently, the abundance of some elements in the transition region may be anomalously high. This mechanism may account for the variable chemical abundances seen in the solar wind. The importance of the effect can be studied by theoretical modeling and by observations of lines formed by different stages of ionization of the same element. Simultaneous cospatial spectroscopic observations covering a wide spectral range (uv to xuv) are needed with time resolution of 10 seconds or better.

E. Solar Interior, Convection, and Long-Term Variability

An understanding of the interior structure and long-term variability of the sun, including convection and circulation, is of major importance for possible implications for solar–terrestrial relations and for applications to the study of other stars. For some of the problems in this area, our knowledge of the sun can be increased by observing other stars. There are four major problem areas that need study at the present time.

The first problem is the development of a detailed theory of convection and large-scale circulation, which takes into account the 10^5 variation in density across the solar convective zone; predicts phenomena such as the observed scales of granulation, supergranulation, circulation, and nonuniform rotation; and can be applied to stars of different gravity and effective temperature. This problem requires extensive theoretical and numerical modeling of convection and energy flux in a stratified atmosphere, as well as precise ground-based measurements of the flow pattern as a function of height in granules, supergranules, and possible giant cell structures and meridional circulation for precision mapping of the velocity field down to 20 m/sec. Measurements of these properties at the poles by out-of-ecliptic spacecraft will be important because of the changing circulation with latitude.

Next, the development of the theory of the solar dynamo is required, based on a realistic convection model, which predicts the unfolding of the solar magnetic cycle and which can then be applied to stars of different gravity and effective temperature. This also requires extensive theoretical and numerical modeling, as well as ground-based measurements of the field pattern with moderate resolution over an extended time base. The theoretical models should be pursued to explain sector structure and the quantitative behavior of both the high- and low-latitude fields. The theoretical properties of magnetic fields in a stratified atmosphere must be developed to the point of understanding the self-compression of the fields emerging through the surface, where the field appears as separate "quantized" flux tubes of great intensity. Although these observations can, in principle, be carried out from the ground, the high spatial resolution required indicates the need for large Shuttle instruments.

The third goal is the determination of the physical properties, nuclear processes, and long-term variability of the solar interior. Observations of solar neutrinos tell us that the foundations of stellar evolution and interiors theory must be scrutinized. In view of this problem, theoretical studies on nuclear reaction rates, Debye shielding, and opacity should be pursued, together with critical examinations of the usual assumptions of stability on all time scales, no core mixing, Maxwellian energy distributions, and energy transport by only radiation and convection. Important clues to the nature of solar evolution can be obtained from stars similar to the sun, such as α Centauri A, τ Ceti, and ξ Boötis A, which are at different stages of evolution.

Finally, studies are needed of the variability of solar activity and luminosity and their terrestrial effects, beginning with the individual solar active events, the sunspot cycle, the switching on and off of the sunspot activity over periods of a century or more, and the possible long-term variations of solar output connected with changing conditions in the interior. The theoretical models of convection, circulation, and magnetic-field generation mentioned above must be studied for instabilities and alternative modes. Observational support for this fundamental question is largely synoptic, involving quality long-term monitoring of solar radio emission, chromospheric lines, white-light corona, red and green coronal lines, and magnetic fields, all with modest resolution from the ground and the general solar-wind properties and uv, x-ray, and fast particles in space. Both the ground-based and spacecraft monitoring of the sun is the fitting responsibility of the National Observatory facilities.

F. Coronal and Interplanetary Research

Since the corona and solar wind are intimately linked physical systems, many of the problems in the two regions are closely related and depend for their solution on similar observations and theoretical techniques. For this reason, we discuss here the strategy for future research in both the coronal and interplanetary regions.

It must first be noted that a large body of observations pertinent to the large-scale structure of the solar corona is already available from ground-based coronagraphs, magnetographs, and radio telescopes, as well as such space missions as the OSO series. The coordinated observations from the ATM constitute a record of structure in the inner and intermediate corona unparalleled for continuity and duration. These existing observations will be the backbone for future studies of the form and evolution of large-scale coronal structure. These studies, with emphasis on theoretical understanding and modeling as well as the analysis and interpretation of data, must be funded on a continuing basis.

The present lack of direct coronal magnetic-field observations implies a continued dependence on theoretical methods (largely numerical) for computing coronal fields from observed photospheric magnetic fields. Improvements in these techniques by comparison of the models with observed coronal features may be one of the paths by which we expand our ability to handle more self-consistent and general problems in magnetohydrodynamics. These improvements may demand better photospheric observations to serve as boundary conditions. The direct observation of magnetic fields in the inner corona remains a difficult task that should be pursued through ground-based techniques (coronagraph and radio observations) and spacecraft. Radio observations appear to provide the only method of deducing the field in the outer corona. Ground-based synoptic observations of solar activity provide a fundamental context for understanding the development of the large-scale coronal structure as well as ancillary evidence regarding the validity of the inferred structures.

Most theoretical modeling of the corona (and its natural extension into solar-wind models) requires detailed knowledge of the temperature structure from the top of the transition region to the inner corona (the region where energy deposition is a dominant effect). Present knowledge of this structure is extremely scanty. There should be an immediate attack on this problem based on available EUV observations from the ATM and the OSO series, supplemented by radio and eclipse observations. The UV coronagraph, as developed for rocket flight and extended to form part of a Shuttle payload, would provide new and extremely useful observations of coronal temperature from Lyman-alpha line profiles. Since the chromosphere and transition region form the lower boundary to the corona, it is of great importance to coronal physics to continue rigorous development models of the chromosphere and transition regions using the ground-based, OSO-8, and ATM observations.

Many of the Shuttle-era observations pertinent to coronal studies will be natural extensions of those mentioned above. In particular, there are many potential uses of a high-resolution X-ray telescope and a UV coronagraph that have been discussed by the Facility Definition teams. Observations from an out-of-the-ecliptic spacecraft would provide an opportunity to obtain a new insight on many solar properties relevant to the corona. A white-light coronagraph and/or a UV telescope would give valuable information on both the polar caps and the nature of low-density equatorial structures whose low brightness render them nearly unobservable from the vicinity of the earth.

In evolving a strategy for solving the basic solar-wind problems, it should again be recognized that a large body of interplanetary data is or soon will become available from continuing *in situ* observations near and beyond the orbit of earth, from the Helios spacecraft, as close to the sun as 0.4 AU, and from such indirect sources as interplanetary radio scintillations. Future spacecraft missions include the ISEE, SOLRAD, and Mariner Jupiter Saturn, all of which will provide a continuing set of the "standard" solar-wind observations, plus some additional information on solar-wind chemical composition.

In view of both the quantity and quality of these observations, it is clear that much can be learned concerning the relationship between the states of the corona and the solar wind from the analysis of existing data. It is imperative that this analysis, and theoretical efforts based on it, be supported on a continuing basis. Of special significance is the analysis and digestion of data from the Helios mission and its coordination with studies of coronal structure based on ATM data, as well as ground-based data from the same epoch.

G. Solar–Terrestrial Effects

In order to better understand the role of solar corpuscular and plasma radiation on the magnetosphere and terrestrial environment, simultaneous measurements are required of solar-wind structure, magnetospheric response, and some critical indicators of large-scale weather patterns. A considerable amount of data has already been gathered on the character and variability of the solar wind as a function of solar activity, mainly as a result of the IMP, Vela, Pioneer, and Mariner missions. Sharp enhancements, a factor of 2–3 in magnitude and approximately one day in duration, occur in the solar-wind energy flux as a result of flares and prominence activity. Similar disturbances, which frequently exhibit a 27-day periodicity, are apparently associated with coronal holes. Measurements of solar-wind density, velocity, energy flux, and magnetic-field variability over one complete 11-year solar cycle have shown no significant change in wind characteristics between solar minimum and solar maximum.

In order to study the wind/magnetosphere interaction in greater detail, the first two members of the International Sun–Earth Explorer series (ISEE-A and -B) will be launched into the same highly eccentric earth orbit in 1977 to obtain particle and field measurements both inside and outside the magnetosphere. A third spacecraft of the series (ISEE-C or Heliocentric) will be launched in 1978 and will be placed at the inner libration point of the sun–earth system, where it will act as an early warning of impending flare disturbances. Because of the complex nature of flare–solar wind–magnetosphere interactions, the SMM, dedicated to studying flares, will be able to provide much detailed information relating to solar–terrestrial effects. The SMM is thus an integral part of a coherent program of studying solar–terrestrial relations.

There is considerable statistical evidence of a relationship between variations in large-scale weather problems and the passage of magnetic sector boundaries past the earth. A vast body of data also exists that implies a relationship between weather variations and the 11-year and 22-year solar cycles. Although such variations are difficult to understand, sufficient indicators now exist to justify a more detailed investigation of the problem. Already theories are being proposed linking variations in ozone concentration with short-term climate changes. Clearly, a great deal more theoretical effort is required in order to identify the mechanisms that may account for such an effect.

In order to support studies of both short-term and long-term solar–terrestrial interactions, it is desirable that equivalent variability in short-wavelength solar emission be established. At present, this can

TABLE F.1 Program of Solar Research

Area		Problem	Tools for Its Solution
I.	Flares	(a) Buildup and dissipation of magnetic fields	(a) Temporal and spatial magnetograph data, with concurrent soft x-ray and radio imagery, theoretical dynamical studies
		(b) Location of the energy release and particle acceleration	(b) Hard x-ray imaging and spectroscopy; gamma-ray and energetic beam impact-point observations; high-resolution microwave mappings; fast meter–decameter radioheliograph
		(c) Thermal history of the flare plasma	(c) High-resolution visible, uv, xuv, x-ray, and centimeter-wave imaging, and spectrophotometry
II.	Active regions	(a) The nature of sunspots; why are sunspots and flare knots stable?	(a) Theoretical studies of basic dynamical effects in sunspot structure; observational studies of wave fluxes from sunspots; very highly resolved magnetograms and optical data; theory
		(b) Structure and dynamics of coronal condensations	(b) High-resolution visible, uv, xuv, x-ray, and radio imaging, and spectrophotometry
		(c) Evolution of active regions	(c) Synoptic studies at visible, radio, uv, and soft x-ray ranges
		(d) The properties of prominences	(d) Highest resolution euv, visible, and radio observations; theoretical studies
III.	Magnetic cycle	(a) Dynamics of sunspots, energy balance of spots	(a) Highly resolved magnetograms and euv and optical data; theory plus time-resolved magnetograms, optical data (including velocities)
		(b) How rapidly and in what way do coronal fields evolve (dissipate)?	(b) ATM data analysis; coordinated synoptic x-ray/xuv and magnetograph data with adequate time resolution (minutes to hours?)

IV.	Solar interior	(a) Hydrodynamics of convection and circulation	(a) Theory, computer modeling
		(b) Large-scale circulation	(b) Extremely high sensitivity measures of velocity and rotation
		(c) Origin of the solar magnetic fields	(c) Synoptic observations of solar magnetic field with modest resolution, velocity, and magnetic-field observations of polar regions of sun
V.	Quiet sun	(a) What is the velocity field in the transition zone and corona?	(a) Xuv spectroscopy with high spatial and spectral resolution, moderate time resolution; x-ray, xuv, radio imagery
		(b) What is the role of magnetic fields in heating?	(b) High-resolution magnetograph, visual, xuv, x-ray data; ATM data analysis
		(c) Are time-dependent ionization effects important in the dynamically varying quiet sun?	(c) Xuv line ratio observations, velocity observations, theory
VI.	Coronal structure	(a) The distinctive structure of the quiet corona and coronal holes; coronal transients	(a) Imaging and spectroscopy in visible uv, xuv, x rays; high-resolution magnetic fields; fast meter–decameter radio heliography
		(b) Coronal magnetic fields	(b) Radio polarization observations, white-light coronagraph polarization studies
		(c) Heating mechanisms	(c) Development and coordination of theoretical modeling with empirical models; observation of coronal temperature and density structure with white-light and Lyman-α coronagraphs; xuv and soft x-ray line profiles
VII.	Solar wind	(a) Relation of solar wind to coronal holes, active regions	(a) Complete coordinated data on corona and solar-wind parameters; theory and computer modeling; extend interplanetary data closer to sun and out of the ecliptic
		(b) Angular momentum of solar wind and loss of angular momentum from sun over its evolution	(b) Well-calibrated observations of angular momentum of solar wind, ultimately out of the ecliptic
		(c) Solar wind and cosmic-ray conditions at middle heliocentric latitudes and over the polar coronal holes	(c) Out-of-the-ecliptic missions

TABLE F.1 *(continued)*

Area	Problem	Tools for Its Solution
VIII. Composition	(a) Chemical composition of solar wind and relation to corona (b) Chemical and isotopic composition in solar flares (c) Fractionation in the solar corona (d) Fractionation in solar flares	(a/b) Better composition measurements of solar wind; theory of ionic diffusion in transition zone and separation in solar wind (c/d) Solar cosmic-ray observations with good elemental and isotopic resolution; gamma-ray data with high sensitivity; xuv and soft x-ray abundance studies
IX. Solar–terrestrial relations	(a) Ozone variations with uv, cosmic-ray variations, geomagnetic activity (b) Effect of ozone concentration on global circulation pattern (c) Possible relationship between weather, climatic, and solar activity (d) Long-term climatic changes due to changes in solar constant	(a) Synoptic observations of solar ultraviolet emission; upper atmosphere, magnetospheric studies (b) Improved atmospheric modeling (c) Continue the coordination and statistical study of solar, interplanetary, and terrestrial conditions (d) Precision synoptic observation of total solar luminosity

only be achieved by some form of spaceborne monitoring package that should fly at periods throughout the coming solar cycle and remain in space for several consecutive solar rotations.

V. A PROGRAM OF SOLAR PHYSICS 1975–1990

A. Overview

In the survey of the current status of solar physics, contained in the preceding sections, a number of problems were identified and methods were suggested for their solution.

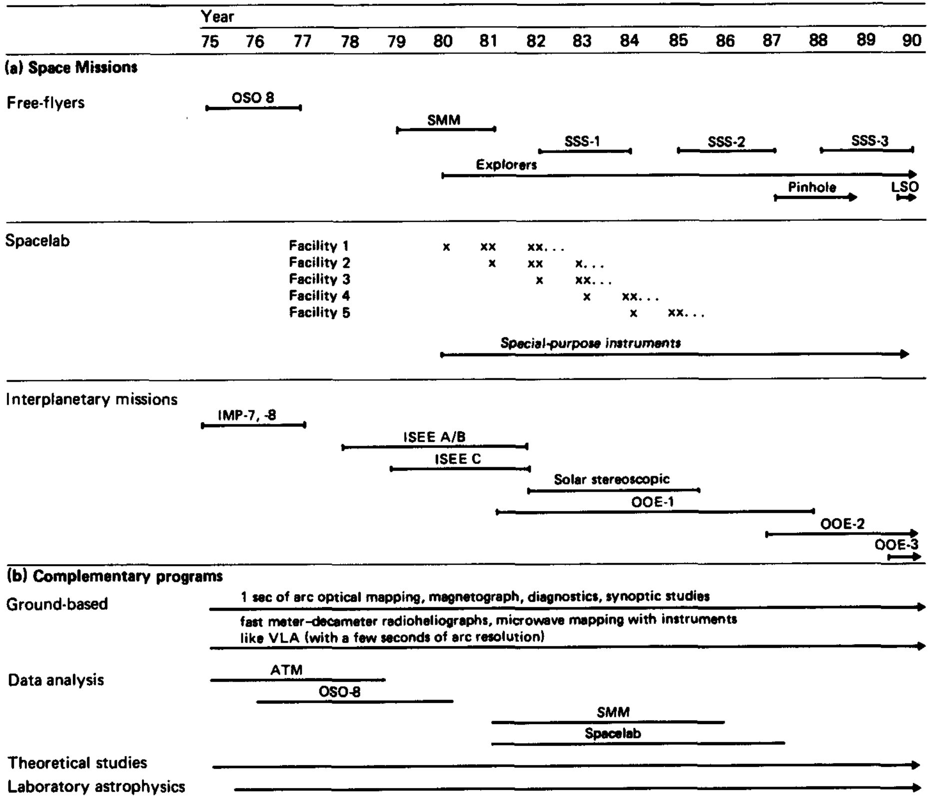
In order to assemble an integrated program of solar research capable of addressing these problems, consideration must be given to five basic factors: the status of relevant data analysis and theory, the state of the technology, the availability of suitable space opportunities, the likely level of funding available to support such opportunities, and the period in the sunspot cycle best suited to particular investigations. In addition, missions must be suitably phased to ensure that all complementary measurements are made. An optimized program of space and ground-based observations, data analysis, theory, and laboratory spectroscopy required to achieve these goals is shown in Table F.2.

Orbiting Solar Observatory 8, the first major solar space mission identified in the table, was launched in June 1975 near the minimum of solar activity. Major problems to be addressed by this mission concern the structure of the quiet chromosphere and transition region and the different modes of energy transport present. The OSO-8 experiment is complemented by a comprehensive guest investigator program comprising both coordinated ground-based observations and theory.

The next major element of the solar space program, the Solar Maximum Mission (SMM), is scheduled for launch at the peak of the forthcoming maximum in solar activity to examine the major flare and active region problems identified in the preceding sections. The overlap between the SMM and the International Sun–Earth Explorer (ISEE) series significantly enhances the scientific capabilities of the flare mission by enabling direct comparisons to be made between the properties of high-energy particle beams arriving at 1 AU and those inferred on the basis of the SMM high-energy x-ray and gamma-ray observations.

The SMM will be complemented by vigorous data analysis and theoretical and computer modeling programs. Its success also depends on a coordinated program of ground-based optical and radio-flare observations, including observations with the Air Weather Service

TABLE F.2 Solar Programs



SOON system and possibly the VLA. The plans for the Solar Maximum Year (April 1979–October 1980) to coordinate worldwide observations on individual active regions would provide strong support to the SMM.

The large weight-carrying capability of the Shuttle makes it an excellent platform for the new generation of "National Facility" solar instruments designed to achieve very high spectral, spatial, and temporal resolutions in wavelengths ranging from the near infrared to hard x rays. Although five basic facility-class instruments are currently identified, budgetary constraints will probably prohibit the simultaneous early development of all five instruments, and a phased development is envisaged.

In addition to facility-class instruments, there are a variety of other Shuttle instruments with broad applications, including the white-light and Lyman-alpha coronagraphs, high-resolution gamma-ray and neutron monitors, and a solar monitoring package. These special-purpose instruments should be developed as required and flown, either individually or in groups, together with the facility-class telescopes, according to the observational requirements of the scientific mission objectives.

Many of the observational programs now carried out on rockets and balloons will be carried out on Shuttle Sortie missions. The great productivity and importance of these components of the solar program, however, suggest that rocket and balloon flights should continue to be supported until after the Sortie program is firmly established.

Beyond the SMM and ISEE series of satellites, a program of interdependent earth-orbiting and deep-space satellites is planned. The three-dimensional structure of the outer solar atmosphere will be explored by means of the Solar Stereoscopic Mission, which involves the placement of a relatively simple spacecraft approximately 90 deg east of the sun–earth line to accommodate stereoscopic measurements. The spacecraft will also measure solar-wind plasma and magnetic-field and high-energy particles and act as an early warning system for launch of Spacelab missions. The SMM payload, suitably refurbished and equipped with complementary instrumentation (called a Solar Synoptic Satellite—SSS1), will provide the second axis for many of these stereoscopic observations, while also serving as a vehicle for the ongoing program of long-term solar constant, uv, eux, xuv, and soft x-ray flux measurement.

The European Space Agency and NASA are presently considering a joint out-of-the-ecliptic (OOE) mission in which two spacecraft are placed on an interplanetary trajectory bringing them first close to Jupiter and thence out of the ecliptic and over the north and south poles of the sun.

The mission represents an exploratory first step in out-of-the-ecliptic investigation and will emphasize measurements of the three-dimensional characteristics of interplanetary plasma, magnetic fields, and particles. An effort will also be made to carry basic solar experiments, possibly a relatively low-resolution coronagraph, so that the three-dimensional structure of the outer solar atmosphere may be studied.

Since the payload carrying capability of this early OOE mission is limited, most of the direct observations of the structure of the outer solar atmosphere, required to properly interpret the OOE-1 data, must be made from an earth-orbiting satellite. A refurbished SMM vehicle, with some parts of the original payload exchanged for a soft x-ray telescope and, possibly, also a Lyman-alpha coronagraph, is planned to provide those complementary observations. This Solar Synoptic Satellite (SSS-2) will, once more, serve as a suitable vehicle for the ongoing program of long-term solar constant, uv, and euv flux measurements.

Further OOE (and complementary SSS) missions are envisaged in order to study specific phenomena in greater detail with more sophisticated instruments.

All the planned Solar Synoptic Satellites (SSS's) play an important role in the integrated solar space program by providing observations of the long-term evolution of features and phenomena selected for intensive high-resolution study with Shuttleborne instrumentation.

Two important missions are planned for the 1990 solar maximum. One is the "Pinhole Satellite," which will determine the location and size of the nucleon acceleration and precipitation region in flares by imaging the nuclear gamma-ray and neutron region to about 10 sec of arc. The other is the Large Solar Observatory (LSO), a free-flying version of the Shuttle-facility-class instrument complement, to be used to investigate a variety of long-lived, small-scale solar features. It may also act as the near-earth complement to the Pinhole Mission.

The full potential of the space program can only be realized if it is complemented by a coordinated ground-based program of high-resolution optical observations dedicated to the measurement of magnetic fields, velocity fields, and various photospheric and chromospheric phenomena and by fast, high-resolution radioheliograph and microwave observations. Data obtained from space missions, together with those gathered in the complementary ground-based studies, must be analyzed in detail and the results used in theoretical studies and computer modeling exercises to better define problems for further investigation. A strong program of laboratory astrophysics is required to aid in the interpretation of spectroscopic data and to develop more sophisticated plasma diagnostic techniques. Throughout the program, concerted efforts must be made to assure that there is a free flow of ideas between the solar-physics community and related disciplines (astronomy, high-energy astrophysics, and atmospheric sciences) so that new theories developed to explain fine detail in the solar atmosphere may find immediate application in these related fields.

B. Solar Maximum Mission

Flare phenomena confront solar research with a series of important physical problems. Key questions have been defined, including the relationship between precursor signals and the beginning of the flare instability, the mechanism that converts magnetic energy into "flare" energy, and the location and nature of the primary and secondary particle-acceleration processes. The recent progress in solar-flare research, described in this report, points the way to the solution of these and other flare problems. Instruments and analytical tools to probe and model the particle-acceleration region and the reaction of the solar atmosphere are now available. The reaction phenomena are so diverse that a coordinated effort must be made to observe many flares with sophisticated ground-based and satelliteborne equipment. The upcoming solar maximum provides the opportunity to study a wide range of flare events.

The leading edge of this coordinated attack on the flare problem is the group of specialized instruments that comprise the Solar Maximum Mission payload (Table F.3). The SMM is a well-conceived, problem-oriented effort to understand the physics of flares. It also has the capability of contributing significantly to our understanding of active region structure and dynamics. Although most of the flare phenomena have been studied separately in past missions, only by determining the spatial and temporal relationships among the various forms of flare emission can the physical mechanisms involved in the different phases, and their causal relationships, be completely determined. SMM provides this capability.

Simultaneous interplanetary particle measurements (from the ISEE satellites) will contribute to the SMM effort to understand the acceleration mechanisms and energy balance in a solar flare. Ground-based radio measurements will provide auxiliary information on active regions and on the containment and release of accelerated particles. Ground-based optical measurements will provide information on the

TABLE F.3 Proposed SMM Payload

Instruments	Spectral Range	Spatial Resolution
Solar gamma-ray and neutron monitor	0.1–150 MeV	Full sun
Hard x-ray, high-time-resolution spectrometer	20–300 keV	Full sun
Hard x-ray imaging spectrometer	3.5–20 keV	8 × 8 sec of arc
X-ray polarimeter	5–100 keV	Full sun
Soft x-ray imaging spectrometer	1–23 Å	10 × 10 sec of arc
Xuv spectroheliometer	40–630 Å	4 × 4 sec of arc
High-resolution uv spectrometer and polarimeter	1000–3000 (4000) Å	<3 × 3 sec of arc
White-light coronagraph	4000–7000 Å	>8 × 8 sec of arc

magnetic structure of active regions and the chromospheric effects of particle accelerations and thermal plasmas.

The SMM has long been in the planning stage. A study of a flare payload was first conducted in 1972, and definition of the SMM, including spacecraft and payload, was completed in January 1974. A candidate payload has been selected, and the mission has undergone a rigorous cost analysis that has provided an independent verification of the SMM funding requirements.

An analysis of flare statistics (see Appendix A) during the past solar cycle dramatically indicates the impact of the launch timing on the potential success of the SMM as a flare mission. If the mission were delayed for even one year (beyond the 1979 launch), the probability of achieving the goal of coordinated birth-to-death observations of a suitable number of flares would be seriously reduced. This consideration, coupled with the desirability of flying coincidently with the ISEE series, and the fact that any delay will now increase costs because of the need to provide intermediate support to retain the necessary engineering expertise in the participating experiment groups, makes a fiscal year 1977 new start imperative if the SMM is to be successful.

If it is not possible to begin funding in 1977, we feel compelled to recommend abandoning the SMM and the serious study of flares until the next solar maximum in 1990. The solar-flare and active region are central topics in understanding solar physics, so the loss of the SMM would be a serious blow to the exploration of solar activity, leaving a conspicuous gap at a crucial point in our knowledge.

C. Spacelab—A Major Space Facility for the 1980's

The arrival of the Shuttle era signals a major change in the way astronomy missions will be carried out in space: larger payloads can be placed in orbit, payloads can be recovered and updated, lead time for space missions can be reduced. The idea of national Shuttle facilities has been conceived. Under this plan, several major instruments covering all spectral regions of interest would be used by "visiting" scientists from the entire solar community. There are presently four Facility Definition Teams planning the facilities and examining the requirements of the Shuttle for smaller instruments of the rocket, balloon, and ATM class. Tables F.4–F.6 summarize the capabilities of the facilities presently under study for observing the quiet and active sun and solar flares. The large facilities will form the solar space observatories of the next decade, while the smaller payloads will consist of instruments designed for specialized observations or to test new ideas and instrument concepts.

Once the individual experiments are better defined and the SMM experiments are determined, the rationale for selection for early development must be based on the maximum mutual complement to the SMM scientific objectives.

D. Free-Flying Missions

There is a need for free-flying satellites dedicated to specific areas of interest in solar physics. These include the following:

1. Synoptic and stereoscopic studies of the structure, energy balance, and composition of the quiet solar atmosphere, of active regions, and of the magnetic structure of the extended corona.
2. Observations coordinated with other major missions including those whose primary goal is the study of interplanetary phenomena.
3. Synoptic observations of the solar "constant" over the complete solar spectrum with high accuracy (0.1 percent) and broadband ultraviolet flux with an accuracy of 10 percent or better. Since long-term stability is difficult to achieve in flight, periodic calibration flights would be required.

Several categories of satellite are envisaged for these projects:

1. Explorer-class spacecraft as part of the present level of effort program;

TABLE F.4 Spacelab: Major Facilities

Shuttle Facility	Parameters	Scientific Objectives		
		Quiet Sun	Active Region	Flare
1-m telescope	125-cm aperture 1100–11000 Å Spectograph ($\lambda/\Delta\lambda = 3 \times 10^5$) Filters $\Delta\theta \leq 0.1$ sec of arc	Heating of the solar chromosphere and corona Properties of solar convection zone Magnetic-field behavior on quiet sun Effect of magnetic field on coronal heating and energy balance	Dynamics of sunspots, especially origin of sunspot cooling and sunspot evolution Magnetic-field structure in active photosphere, chromosphere, and corona	Properties of flare at photospheric and chromospheric level Magnetic-field behavior in photosphere and chromosphere Fine structure (<70 km) of 10^7 K flare plasma from Fe XXI line at 1358 Å Localize loci of impact of high-energy particle streams
Hard x-ray imaging focality	Range 5–100 keV $\Delta\theta \sim 4$ sec of arc FWHM $\Delta t \sim 1$ sec $\Delta E/E \sim 20$ percent		Storage of electrons in coronal region Slow or continuous acceleration process	Location of acceleration regions in relation to hot thermal region, $H\alpha$ kernel, magnetic loops, etc.
Xuv telescope	40–1200 Å $\Delta\theta \sim 0.5$–1.0 sec of arc[a] $\Delta t \sim 1$–10 sec[a] $\lambda/\Delta\lambda \sim 1$ to 3×10^4[a] FOV = 8–48 min of arc[a]	Structure of the lower corona and transition zone The chromospheric network Mass and energy transport Coronal holes Composition Coronal bright points Velocity fields, waves, and shock propagation		Time, morphology, location, and the thermal structure of preflare heating phase Structure of chromospheric material heated by nonthermal electrons

		Magnetic structure		Conductive cooling of the flare plasma
Euv telescope	360–1400 Å $\Delta\theta \sim 0.5$ sec of arc $\Delta t \sim 0.1$–10 sec $\lambda/\Delta\lambda \sim 10^3$ to 10^5 FOV = 2–8 min of arc	Structure of the chronomsphere and transition zone The chromosphere, network, spicules, wave, and shock propagation Mass transport, velocity fields Chromospheric magnetic structure		Structure of the chromospheric material heated by nonthermal electrons
Soft x-ray telescope	1.75–100 Å $\Delta\theta \sim 0.5$–10 sec of arc[a] $\Delta t \sim 1$ sec[a] $\lambda/\Delta\lambda \sim 10$ to 2×10^4[a] FOV ~ 8–48 min of arc[a]	Corona: structure, dynamics of plasma loops and arches, mass and energy balance, composition Mass and energy transport Atmospheric structure, velocity fields, mass flow Coronal bright points	Magnetic and mechanical heating phenomena in coronal loops Prominences and filaments Coronal currents	Time, morphology, location, and temperature structure of preflare heating phase Location and evolution of flare plasma region and relation to impulsive phase during electron acceleration Density, temperature, and height structure during development and cooling phase to determine plasma dynamics, conditions for particle acceleration, and energy balance

[a] Depends on focal-plane instrument.

TABLE F.5 Spacelab: Examples of Special-Purpose Facilities and Payloads

Instrument	Parameters	Scientific Objectives		
		Quiet Sun	Active Region	Flare
Nuclear gamma-ray line spectrometer	50 keV to 10 MeV $\Delta E \sim 5$ keV Whole sun Sensitivity $\sim 10^{-3}$ photons/cm^2-sec			Time, density, and spectral history of accelerated nucleons near the site
Energetic neutron detector	$\gtrsim 20$ MeV Whole sun Neutrons $\sim 10^{-3}$ neutrons/cm^2-sec			
X-ray polarimeter	5–100 keV Whole sun			Mechanism and directivity of nonthermal electron beams
Visible coronagraph	2000–10000 Å $\Delta\theta \approx 5$ sec of arc	Density and magnetic-field structure of the corona and inner solar wind	Structure of active corona	Properties of mass ejecta (e.g., coronal bubbles)

Euv coronagraph	300–2000 Å $\Delta\theta = 5$ sec of arc $\lambda/\Delta\lambda = 10^4$	Temperature of corona and inner *solar wind* H/He abundance ratios and their changes	Temperature structure of coronal active regions	Thermal properties of coronal transients Velocities associated with coronal transients Abundance anomalies in mass ejecta
Xuv spectroheliograph	170–2000 Å $\lambda/\Delta\lambda \sim 10^3$	Transition region structure, *energy* balance		Location of impulsive xuv burst Mass motion during impulsive phase Thermal structure of flare plasma during impulsive phase
X-ray spectroheliograph	3–100 Å $\lambda/\Delta\lambda \sim 3 \times 10^3$ $\Delta\theta \sim 3$ sec of arc	Coronal bright *points* *Coronal structure* Coronal abundance	*Coronal magnetic* structure Fractionation of coronal abundances	Evolution of the coronal flare plasma

TABLE F.6 Spacelab: Examples of Monitoring Instruments

Instruments	Parameter	Scientific Objectives
Precision solar-constant monitor	1–10^5 Å Measures solar constant Uv flux to ~0.1 percent	Determines total luminosity of sun in all wavelengths Important for solar interior, stellar evolution, climatic changes
Solar monitoring package	Broadband uv, xuv, and soft x-ray (0.5–500 keV) spectrometer Full disk Hα Magnetograph Full disk xuv	Provides basic information to complement space and ground-based observations on solar conditions at important wavelengths

2. Spacecraft dedicated to stereoscopic and synoptic studies, some of which might well be based on the retrieval and reflight of the SMM spacecraft;

3. Very large facilities such as a Large Solar Observatory, built to accommodate instruments of the size developed for the Solar Shuttle Cluster, or a major imaging gamma-ray facility.

1. ISEE AND FOLLOW-ON

The International Sun–Earth Explorer (ISEE) is an approved and funded joint NASA–ESA program designed to study the solar wind and cosmic rays and their interaction with the earth's magnetosphere. It consists of ISEE A/B, a pair of spacecraft to be launched in 1977 into a highly eccentric earth orbit, and ISEE C (Heliocentric) to be launched in 1978 and placed "upstream" (in the solar wind) at 0.9 AU within 5° of the sun–earth line. ISEE C acts as a monitor of the varying conditions in the solar wind, while ISEE A/B measures the response of the magnetosphere. All three spacecraft carry experiments to study the solar-wind plasma, magnetic fields, electric fields, and galactic and solar cosmic rays. The cosmic-ray experiments include electron measurements and ion measurements, which will provide an invaluable complement to the near-earth gamma-ray and hard x-ray observations planned for the SMM and Spacelab. Also, full-disk solar x-ray and low-frequency (10 to 10^4 kHz) radio experiments with direction-finding capability will be aboard ISEE C. These spacecraft are designed for 3–5 year lifetimes, so they will overlap the scheduled SMM lifetime and provide the needed interplanetary particle and fields measurements for SMM support.

Follow-on Explorer-class spacecraft for near-earth studies of the solar wind and interplanetary medium should be planned for launch about every four years. These follow-on spacecraft are ideal for the required extended and improved measurements of solar wind and solar cosmic-ray composition, low-frequency solar radio bursts, for example, to complement later SMM and Spacelab experiments.

2. SOLAR STEREO MISSION

We recommend that a spacecraft to be placed in a heliocentric orbit, stationed 90° to the east of the sun–earth line, be seriously considered. This spacecraft should be launched in 1981–1982 to provide measurements that would enhance the information obtained from Shuttle facility solar instruments, out-of-the-ecliptic missions, and other solar space missions. In conjunction with those missions and other near-earth spacecraft, it would provide stereoscopic measurements of solar hard and soft x rays, low-frequency radio emission, and white-light coronal observations. These are needed to obtain the three-dimensional spatial structure of the active features of the sun. This spacecraft would also measure solar cosmic-ray electrons and ions and their composition from flares in the eastern hemisphere of the sun, thus providing an invaluable complement to the near-earth gamma-ray and hard x-ray observations. Furthermore, it would act as an early warning system for launch of Spacelab solar-physics facility instruments, by observing flare and hard x-ray emitting regions two weeks before they pass the central meridian.

This is a 3–5 year mission to be accomplished with spacecraft appreciably simpler than the present ISEE configuration. Its launch should be in 1981–1982 to complement Spacelab solar-physics facility instruments, the out-of-the-ecliptic mission, and the SMM.

3. OUT-OF-THE-ECLIPTIC AND NEAR-SUN EXPLORATION

The European Space Agency and NASA are presently considering a joint out-of-the-ecliptic mission. One possible mission involves two spacecraft that would be placed on an interplanetary trajectory bringing them close to Jupiter, where they would be diverted by Jupiter's gravity and leave the ecliptic plane, one to the north, the other to the south. The spacecraft would then continue along two nearly symmetric orbits of high inclination (attaining up to 90° solar latitude).

Such a mission represents a logical first step in out-of-the-ecliptic exploration, and emphasis should be placed on obtaining the observations over as wide a range of solar latitude as possible; measuring the three-dimensional characteristics of interplanetary plasma, magnetic fields, and particles; carrying out as much solar physics as consistent with the basic objectives and constraints.

It is anticipated that even within the constraints of a small spacecraft, important new solar observations can be made. Such solar observations would be exploratory and need not be judged by the same standards of spatial and temporal resolution of the more traditional (and problem-oriented) observations from the earth or earth satellites.

Beyond this first step, additional missions should be planned to (1) Study specific phenomena in greater detail. Obvious examples are the concentration of transient solar activity (and the resulting interplanetary effects) at midsolar latitudes, and the detailed structure of the solar and interplanetary polar regions. Other phenomena are certain to be suggested by the initial phase exploration. (2) Extend high-resolution solar observations to exploit the out-of-the-ecliptic viewpoint without the severe spacecraft restraints likely to apply on the initial missions.

A similar area for frontier or exploratory observations is the extension of *in situ* measurements inside of the orbit of Mercury. We highly recommend that a study be made of a spacecraft probe sent straight into the sun, making use of the probe's thermal inertia to permit observations to heliocentric distances of the order of several solar radii. The acceleration of the spacecraft would yield highly accurate information on the quadrupole moment of the sun and on the solar mass distribution. The possibility of such a "Solar Plunger" should be considered in any long-range mission plan for solar–interplanetary physics.

4. LONG-TERM SOLAR MONITORING

Adequate monitoring of solar activity and variability is necessary not only for discovering the response of the earth to fluctuations in the radiation, particles, and magnetic fields from the sun but also for discovering the mechanisms responsible for this variability with the goal of ultimately predicting it. Synoptic observations from the ground and from space, and their thoughtful interpretation, are all needed. For this purpose, a series of Solar Synoptic Satellites (SSS series) should be flown as a key element in synoptic studies. This series would serve to achieve continuity of coverage of solar activity and variability of the solar "constant" and broadband ultraviolet flux, as well as act as a complement to the Solar Stereoscopic and out-of-the-ecliptic missions. A possible set of vehicles for the SSS series would be a series of follow-on SMM-type spacecraft.

This program must include a strong laboratory component directed at radiometric calibration and aging effects in detectors and optical systems. Since this program will cover decades of time and involve the intercalibration of instruments on a large number of satellite missions, it should be implemented by a government agency such as the National Bureau of Standards with sufficient in-house laboratory capability, stamina, and specific agency charter mandate to accomplish the objectives.

5. LATER MISSIONS

Determining location, size, and structure of the nucleon acceleration and precipitation region in flares requires imaging of the nuclear gamma-ray and neutron-emitting region to about 10 sec of arc.

Techniques for these observations are limited and difficult but are

variations in solar activity follow in a *statistical* pattern. Thus, any such estimates of future observations must be stated in terms of probabilities. Let us adopt the five class-2 flare observations in a two-year mission deduced above for the past activity cycle as a reasonable goal, and ask how often that goal would have been achieved in the past 20 cycles based on sunspot number. (This analysis should be performed using flare occurrence rates as in Table F.A.1, but such rates are available for only the most recent cycles.) Since we know only that the maximum of the last sunspot cycle occurred near the end of 1968, we will use this epoch in assessing the chances of attaining the goal for two cases:

1. The scheduled 1979 launch of the SMM;
2. A one-year delay to a 1980 launch.

Figures F.A.1 and F.A.2 show the smoothed Zurich sunspot numbers for the 20 sunspot cycles since 1750. The shaded areas cover two-year periods (the anticipated lifetime of the SMM) beginning at times after sunspot maximum equal to the time between the last maximum in 1968 and the presumed launch in 1979 (Figure F.A.1) and in 1980 (Figure F.A.2). If the rate of occurrence of flares with importance ≥ 2 bears the same proportion to sunspot number as in the last cycle, the goal based on the above criterion would have been achieved for any of these "samples" only if the average sunspot number during the 2-year mission in that cycle is >100, the value at the peak of the last cycle. This criterion is achieved in 6 of the 19 cases with the "scheduled" SMM timing and in only 1 of the 19 cases with a delayed timing. If the postulated launch were advanced by a year, the goal would have been achieved in 7 of the 19 cases, little different from that for the proposed SMM launch date.

This simple analysis indicates dramatically the impact of launch timing on the probability of an SMM mission obtaining more than a handful of complete observations. Past delays have placed the SMM mission in a precarious position. The currently scheduled launch date will occur after the next maximum in solar activity (this is the case of 7 of the 19 "examples" in Figure F.A.1). Thus, any further delay moves the mission onto the steeply declining, postmaximum part of the expected activity cycle and sharply reduces the chance of obtaining even as many as five complete, coordinated flare histories. Although this analysis depends on many assumptions and definitions and should be viewed in such a light, its broad implications do not depend on these factors.

2. RADIO OBSERVATORIES

High-resolution observations of solar radio emissions, from millimeter to decameter wavelengths, are indispensable for the understanding of the quiet sun, the active sun, and, in particular, the flare process. Solar radio astronomy in the United States has been neglected for almost 20 years despite the potential for considerable advances in understanding solar phenomena. We recommend that the following programs be implemented:

(a) The Clark Lake facility should be expanded as a multifrequency radio heliograph in the meter–decameter band (20–120 MHz) to produce two-dimensional pictures over many frequencies at a fast rate (100 per second). This expansion requires only a modest investment.

(b) Sufficient time should be made available for centimeter and millimeter solar observations on nonsolar large telescopes, in particular the VLA. The VLA will be ideal for producing synthesized maps of active regions with a few seconds of arc resolution. Large millimeter telescopes will be useful for polarization measurements of solar active regions and hence to estimate chromospheric magnetic fields.

(c) The Stanford E–W array at 2.8-cm should be used to the fullest extent for solar observations, and consideration should be given to the possibility of adding a N–S arm in order to produce fast (a few seconds) two-dimensional pictures with angular resolution of about 20 sec of arc.

(d) High-quality measurements of dynamic spectra and position of bursts and of total flux, including polarization in the frequency range 10 MHz–10 GHz should be carried out.

Further, we believe that the funding available for solar radio astronomy has not been adequate to exploit the observational capabilities that exist for the study of major solar-physics problems. We recommend that the Space Science Board conduct a study to examine the need for major solar radio astronomy facilities using centimeter wavelengths capable of producing two-dimensional pictures in a few seconds with angular resolution of 1–5 sec of arc.

3. NEUTRINO OBSERVATORIES

The solution of the problem of the missing neutrino flux is central to our understanding of the sun. Both theoretical and experimental efforts should be vigorously pursued to explain the discrepancy. More sensitive neutrino facilities are needed.

4. SOLAR MONITORING FACILITIES

To solve some of the questions on the solar interior structure, long-term monitoring of the large-scale global circulation on the solar surface will be necessary. This should be done with a dedicated ground-based facility that measures Doppler velocity with respect to an absolute-zero velocity reference.

The SOON telescope network and other existing flare patrol, magnetograph, and ground-based monitoring facilities should fulfill many of the requirements for flare forecasting and space mission support.

F. Coordination of Observations

The sun offers a unique opportunity for studying in detail a wide variety of complex physical processes that are important to all branches of astronomy and astrophysics. The extreme range of temperature and density in the outer solar atmosphere, and the necessity of observing several different physical parameters simultaneously, makes coordinated observations necessary for most solar investigations. Shuttle and satellite payloads must be configured with appropriate complements of instruments, and care must be taken to ensure that co-alignment, pointing, and data-taking capabilities facilitate the simultaneous observations. Arrangements must also be made with appropriate ground-based observatories to ensure that complementary measurements are made.

Furthermore, when data taken by individual spaceborne and ground-based instruments are complementary and must be combined to solve the problem under investigation, appropriate data-sharing arrangements must be defined prior to launch and data reduction must be accomplished in a timely manner. These considerations apply equally to spaceborne and ground-based results.

G. Supporting Activities

1. THEORY

If the proposed program of solar observations is to yield its full potential, it is essential that it be matched by a vigorous and effective program of theoretical research. The principal disciplines involved in these theoretical studies are atomic physics and radiative transfer, hydrodynamics, magnetohydrodynamics, and plasma physics. Studies in atomic physics and radiative transfer will be related to diagnostics, ionization equilibria, and abundances; studies in hydrodynamics, to processes in the sun's convection zone and atmosphere including the solar wind; studies in magnetohydrodynamics, to the solar cycle and dynamo action; and studies in plasma physics, to the many phenomena involved in solar activity.

2. COMPUTER MODELING

Although theoretical studies can outline models of solar phenomena and investigate the consistency and principal mechanisms of these models, the models are now becoming so detailed that the only feasible method of analysis is computer modeling. We recognize that computer modeling will play an increasingly important role in solar physics. We urge NASA to consider the organization of a computer modeling facility, accessible by remote terminals at the principal centers of solar research.

3. DATA ANALYSIS

Our appreciation for the importance of specific data sets and our ability to interpret these data progresses with the development of theoretical concepts and diagnostic techniques. As a result, NASA must support the analysis of data from past missions, such as the ATM, and future missions as they occur in response to well-conceived proposals. In particular, programs established to bring together scientists to study specific problems with existing data sets are valuable and must be supported. In this effort it is important to support groups not involved with the original data-acquisition effort who approach the data from fresh perspectives. Data analysis is essential to refining future experiments and should be funded as an integral part of future missions instead of the present inadequately funded level of effort program.

4. THE ASTROPHYSICAL CONNECTION

This report has described the similarity of phenomena, theoretical concepts, and diagnostic techniques between solar astrophysics and other branches of astrophysics. We wish to emphasize the importance of theoretical and observational programs that are interdisciplinary in nature. These should be funded by NASA and NSF, even though they cut across established responsibilities of individual program offices.

5. LABORATORY ASTROPHYSICS

Many problems in solar astrophysics require accurate diagnostics, which in turn require accurate values for basic atomic and molecular parameters. We stress the importance of a strong theoretical and experimental atomic program oriented toward obtaining these fundamental atomic parameters and urge NASA, NSF, and NBS to support this program.

APPENDIX A: THE TIMING OF THE SOLAR MAXIMUM MISSION

The next maximum in solar activity, expected to occur in 1979–1980, will be the second since the advent of solar observations from space. The progress in observational and theoretical solar research described in the preceding sections of this report has brought us to the point of defining the major problems in understanding solar flares. We are now prepared to seek their solution, and the Solar Maximum Mission (SMM) has been planned to provide the crucial observations.

A study of a flare payload was conducted in 1972. Definition of the SMM, including both spacecraft and payload, was completed in January 1974. A rigorous cost analysis was conducted throughout 1974, and this analysis has been independently verified. An Announcement of Opportunity was issued by NASA in February 1974, and a candidate payload selected in February 1975. The failure to grant New Start status to the SMM in the past two fiscal years is regrettable and has consumed all the flexibility with respect to the timing of an SMM launch. Failure to obtain a New Start in fiscal year 1977 will place the mission so late in the next activity cycle as to jeopardize seriously its chances of obtaining comprehensive observations.

In discussing the number of fully coordinated flare observations to be expected from the SMM, begin by considering the last activity cycle (1964 to 1975). Table F.A.1 shows the numbers of flares of importance class ≥ 2 for each year of that cycle. In estimating the number of flares that would have been observed by the SMM, three factors must be considered:

(a) The portion of each spacecraft orbit spent in darkness reduces the chances of observing a flare. For a flare lifetime of t minutes and an orbital period $T = 90$ min, the chance of observing a flare from birth to death is reduced by the factor $(0.5T - t)/T$, or by 1/6 for a flare lifetime of $t = 30$ min. Column 3 of the table lists the number of flares with

TABLE F.A.1 Flare Statistics

Year	Flares with Importance ≥2	Number Observable from Birth to Death	Number with Full Ground-Based Coverage
1964	2	0	0
1965	6	1	0
1966	50	8	2
1967	63	11	3
1968	51	9	2
1969	64	11	3
1970	52	9	2
1971	12	2	1
1972	17	3	1
1973	13	2	1
1974	13	2	1

importance ≥2 that would therefore be fully observable by the SMM if the satellite were pointed at a region of the sun including the flare site.

(b) The chance of obtaining concomitant ground-based observations (e.g., magnetograms) is reduced by cloud cover and the limited geographic distribution of ground stations. This reduction is estimated to be equal to a factor of 4, depending on the observations required and on the full availability of the SOON telescope network. Column 4 of the table gives the number of flares that would be observed both by the SMM and from ground stations.

(c) If n active regions capable of producing flares were present on the sun, chances of the SMM observing a flare would be reduced by the factor $1/n$. This effect will, it is hoped, be mitigated by improvements in flare-forecasting techniques and has not been directly incorporated into Table F.A.1. Its importance should not, however, be forgotten.

The above considerations indicate that an SMM mission of two years' duration, near the maximum of the last activity cycle, would have achieved the coordinated, birth-to-death observation of approximately five flares of class 2 and larger during the entire mission, even with no confusion caused by the presence of several active regions in the solar disk. Observations of smaller (importance 1) flares would have been more easily obtained, with approximately 50 expected from the same considerations. The observation of an importance 3 "superflare" would have had a 50 percent probability.

In estimating the number of "complete" flare observations expected by the SMM in the next solar cycle, and in assessing the impact of a one-year delay in launch on that number, it must be recognized that the

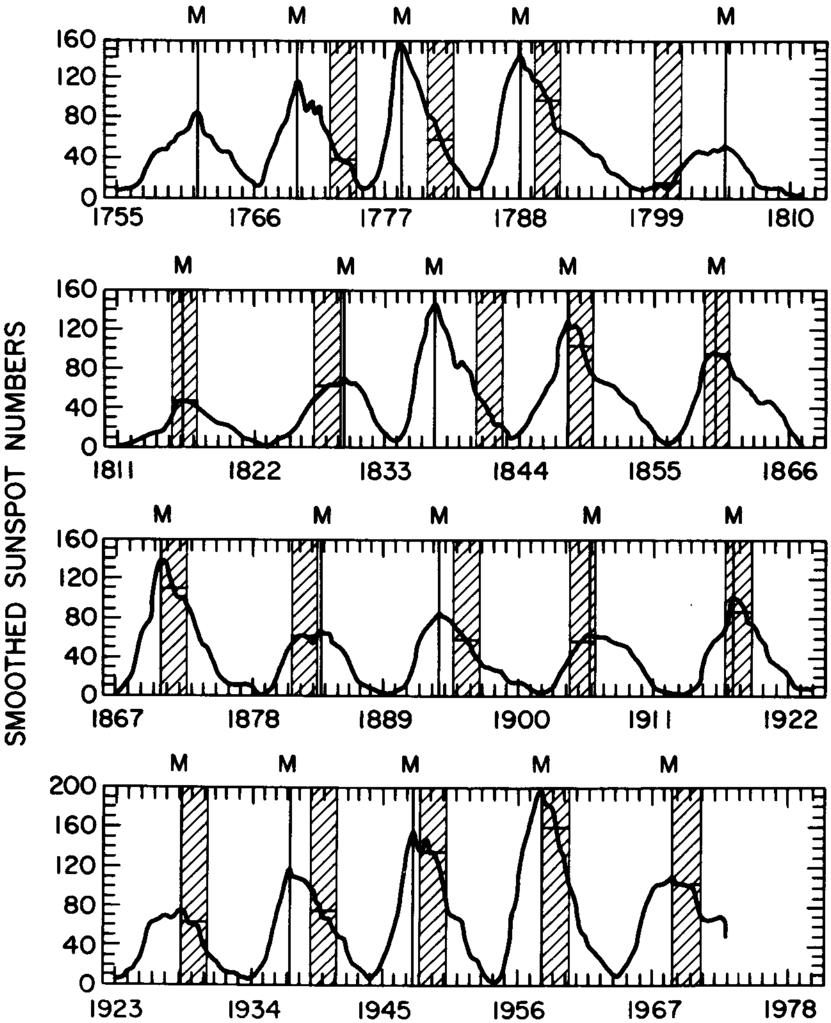


FIGURE F.A.1 The SMM as scheduled.

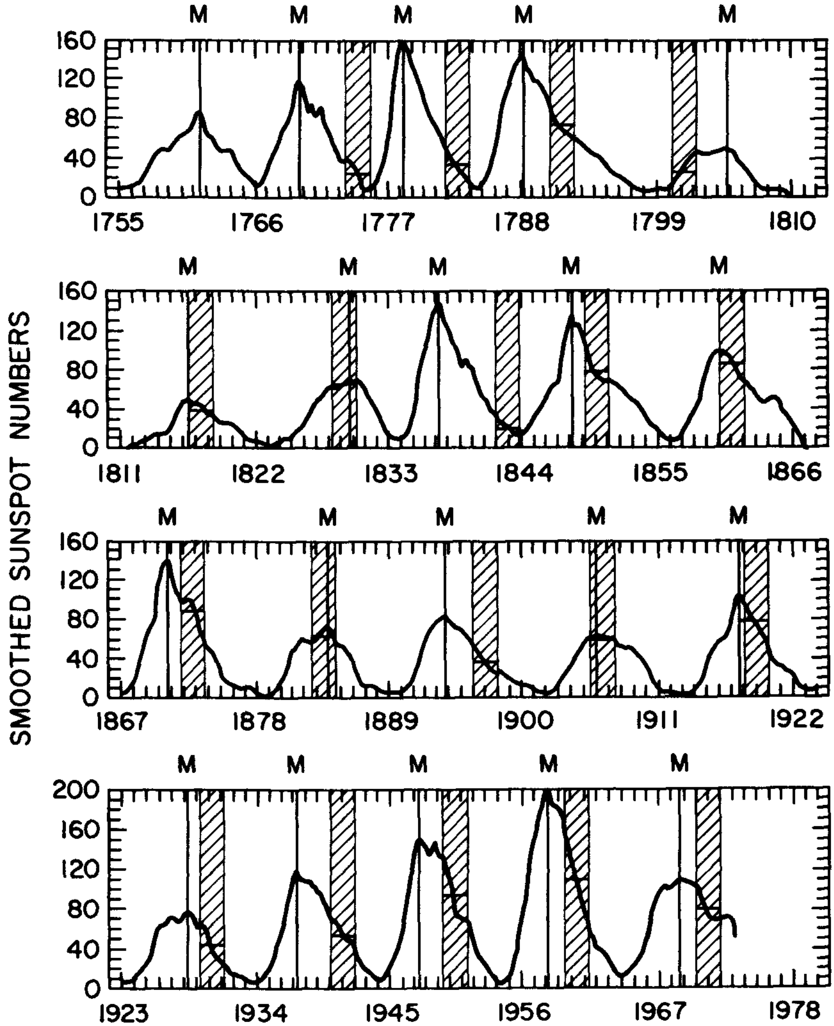


FIGURE F.A.2 The SMM slipped one year.

variations in solar activity follow in a *statistical* pattern. Thus, any such estimates of future observations must be stated in terms of probabilities. Let us adopt the five class-2 flare observations in a two-year mission deduced above for the past activity cycle as a reasonable goal, and ask how often that goal would have been achieved in the past 20 cycles based on sunspot number. (This analysis should be performed using flare occurrence rates as in Table F.A.1, but such rates are available for only the most recent cycles.) Since we know only that the maximum of the last sunspot cycle occurred near the end of 1968, we will use this epoch in assessing the chances of attaining the goal for two cases:

1. The scheduled 1979 launch of the SMM;
2. A one-year delay to a 1980 launch.

Figures F.A.1 and F.A.2 show the smoothed Zurich sunspot numbers for the 20 sunspot cycles since 1750. The shaded areas cover two-year periods (the anticipated lifetime of the SMM) beginning at times after sunspot maximum equal to the time between the last maximum in 1968 and the presumed launch in 1979 (Figure F.A.1) and in 1980 (Figure F.A.2). If the rate of occurrence of flares with importance ≥2 bears the same proportion to sunspot number as in the last cycle, the goal based on the above criterion would have been achieved for any of these "samples" only if the average sunspot number during the 2-year mission in that cycle is >100, the value at the peak of the last cycle. This criterion is achieved in 6 of the 19 cases with the "scheduled" SMM timing and in only 1 of the 19 cases with a delayed timing. If the postulated launch were advanced by a year, the goal would have been achieved in 7 of the 19 cases, little different from that for the proposed SMM launch date.

This simple analysis indicates dramatically the impact of launch timing on the probability of an SMM mission obtaining more than a handful of complete observations. Past delays have placed the SMM mission in a precarious position. The currently scheduled launch date will occur after the next maximum in solar activity (this is the case of 7 of the 19 "examples" in Figure F.A.1). Thus, any further delay moves the mission onto the steeply declining, postmaximum part of the expected activity cycle and sharply reduces the chance of obtaining even as many as five complete, coordinated flare histories. Although this analysis depends on many assumptions and definitions and should be viewed in such a light, its broad implications do not depend on these factors.