OUTER PLANETS EXPLORATION 1972-1985

NATIO
ACADE
SCIEN

Outer Planets Exploration 1972-1985

SPACE SCIENCE BOARD
NATIONAL RESEARCH COUNCIL

NATIONAL ACADEMY OF SCIENCES Washington, D.C. 1971

Library of Congress Catalog Card Number 78-183585
ISBN 0-309-01939-7

Available from

Space Science Board
2101 Constitution Avenue

Space Science Board

Outer Planets Exploration Study

Francis S. Johnson[1]
The University of Texas at Dallas, *Chairman*

Kinsey A. Anderson[3]
University of California, Berkeley

Alan H. Barrett
Massachusetts Institute of Technology

Michael J. S. Belton[3]
Kitt Peak National Observatory

Thomas M. Donahue[3]
University of Pittsburgh

Herbert Friedman[1]
U.S. Naval Research Laboratory

Richard M. Goody[1,3,4]
Harvard University

Martin Harwit
Cornell University

Michael B. McElroy[2,3,4]
Harvard University

Roman Smoluchowski[1]
Princeton University

Charles H. Townes[1,5]
University of California, Berkeley

Rochus E. Vogt[3]
California Institute of Technology

Gerald J. Wasserburg[2]
California Institute of Technology

Rainer Weiss[2]
Massachusetts Institute of Technology

William C. Bartley, *Executive Secretary*

Superscripts indicate other advisory functions in which members of the study group are involved as follows: [1]Space Science Board; [2]NASA Physical Sciences Committee of Space Program Advisory Council; [3]NASA Outer Planets Missions Science Steering Group; [4]Science Advisory Group to JPL and to the Planetary Programs Office of NASA; [5]NASA Space Program Advisory Council.

Preface

Several studies of the Space Science Board have dealt with the outer planets. In 1965, the Board conducted a general review of space research. One purpose was to outline a program of planetary exploration and to suggest relative emphasis between studies of the moon and planets. The report of that study, *Space Research, Directions for the Future*, recommended a shift in emphasis from the moon and to planetary exploration in the 1965-1975 period, giving highest emphasis on the exploration of Mars. In regard to the outer planets, it recommended the following sequence of investigations: first, ground-based, balloon, rocket, and earth satellite studies; second, flyby missions of the planets; third, orbiters, starting with general-purpose and then more specialized missions; and, finally, landers. The report discussed flybys beginning in 1975 that could be sent to four planets early in the period or alternatively could concentrate on Jupiter alone.

The Board next examined the planetary program in June 1968. This study, reported in *Planetary Exploration: 1968-1975*, did not cover the time period envisioned for outer planets missions and, therefore, concentrated primarily on the inner planets. However, it made two recommendations covering planetary exploration in general: that planetary programs be presented not in terms of a single goal but rather in terms of contributions to a broad range of scientific disciplines and that a substantially increased fraction of the total National Aeronautics and Space Administration (NASA) budget be devoted to unmanned planetary exploration.

In June 1969, the Board examined the subject in more detail in its report, *The Outer Solar System: A Program for Exploration*. This study endorsed prior Board studies and recommended again that an increased fraction of the total NASA budget be devoted to planetary exploration. Without considering costs or balance among major areas of space research, it recommended the following series of missions *in order of scientific significance*: Jupiter deep-entry probe or flyby in 1974, a Jupiter orbiter in 1976, multiple planet Jupiter-Saturn-Pluto and Jupiter-Uranus-Neptune missions in 1977-1979, and Jupiter-Uranus-Neptune missions with entry probes in the early 1980's.

In the summer of 1970, the Board undertook a study, *Priorities for Space Research 1971-1980*, to develop criteria for priorities and to recommend levels of effort among seven areas of NASA's Office of Space Science and Applications program: planetary exploration, lunar exploration, astronomy, gravitational physics, solar-terrestrial physics, earth environmental sciences, and life sciences. In addition to seven working groups, which developed recommended programs at three budgetary levels for each of the discipline areas, a fourteen-member Executive Committe, interdisciplinary in composition, assimilated the working group proposals into an overall priority system. The emphasis that the Executive Committee placed on planetary exploration was reflected in its allocation of 40 percent of NASA's total scientific budget for planetary studies in its BASE program. For *outer planets exploration* the Executive Committee gave highest priority to detailed exploration of Jupiter using Pioneer-level technology; these Jupiter orbiter or flyby missions were assigned to the BASE program and thereby were recommended at all three budgetary levels. The Thermoelectric Outer Planet Spacecraft (TOPS) Grand Tour was not included in either the BASE or INTERMEDIATE budget level categories but only in the HIGHER budget program because of the impact of its cost on possibilities for accommodating other highly desirable scientific missions.

The 1970 study expressed interest in an examination by NASA of other, less costly, systems for outer planet investigations. NASA was sympathetic and arranged for some appropriate engineering studies, inviting the Board to consider these studies and their implications for outer planet studies.

Thus the points of departure for the present study were (1) new information on technical systems and (2) the Board's prior studies noted above and as summarized in the opening remarks of the chairman of this study (see Appendix A). In connection with the latter, it may well be noted here that in the present study no attempt was made to reappraise the overall priorities among various fields of space research as was done in the 1970 Priorities Study, but the group did reconsider priorities within the outer planets portion only of the area of planetary exploration.

The charge to the study, which set forth the background noted above, raised four questions:

1. Do low-cost and high-cost alternatives exist? The only available cost figures are for missions for which phase A studies have been made. The alternatives are then Pioneer and TOPS; presumably the former is meant to be low cost, but this is only obvious for Jupiter flybys and has not been established for probes, orbiters, or flybys of Saturn and more distant planets.

2. What is the proper balance between flybys that provide "first looks" at unknown planets and satellites and entry probes that permit diagnostic investigations of important scientific questions, probably aimed chiefly at Jupiter?

3. Is electric propulsion likely to reduce the need for Grand Tour or to provide a lower-cost means of achieving the same objectives?

4. What case can be made for the importance of satellite imaging? This question relates to first looks and their value compared to entry measurements. This question is deceptively innocent. It may in fact constitute the most solid justification for Grand Tour.

The intensive period of the study was the week of August 8-14, 1971, at Woods Hole, Massachusetts. This was preceded some months earlier by the choice of participants and by the submission of useful documents to participants, including prior Board studies and technical documents requested from NASA. The study group consisted of 13 scientists, under the chairmanship of Francis S. Johnson.

The first two days of the working session in August were devoted to presentations by NASA scientists and engineers and by others as invited by NASA in response to the Board's request for specific information, particularly on the flight systems. The NASA presentations included estimated costs for space systems, and these are used in this report; these figures are preliminary and reflect planning estimates rather than actual contractual obligations. Both the study group and the Board acknowledge here with appreciation such contributions by more than a score of experts, for their information was crucial to the study.

A preliminary draft of the recommendations of this study was approved by the study group on the concluding day of the study, August 14. Reviews of the draft were carried out by both the study group and the Board. The final report was approved by the study group and the Space Science Board as of September 27, 1971.

The Board is grateful to the members of the study group for their execution of the study; to NASA personnel and the scientific representatives of NASA's scientific planning groups, who provided descriptions of spacecraft systems, proposed experiments, and cost estimates on alternate mission strategies; and to William C. Bartley of the Board's staff, who served as Executive Secretary of the study. The Board also acknowledges with appreciation the support of the National Aeronautics and Space Administration, which helped to make this study possible.

Charles H. Townes, *Chairman*
Space Science Board

Contents

1 Recommendations

An extensive study of the outer solar system is recognized by us to be one of the major objectives of space science in this decade. This endeavor is made particularly exciting by the rare opportunity to explore several planets and satellites in one mission using long-lived spacecraft and existing propulsion systems and to study selected planets intensively using intermediate-lived spacecraft. Furthermore, these missions will permit the exploration of the boundary between our solar system and the interstellar medium and *in situ* measurements in the interstellar medium. Such a balanced program is a major step toward recognizing and understanding both the diversity of objects present within our solar system and the interaction between our solar system and the galaxy.

We have reassessed previous National Academy of Sciences reports relating to the outer planets. Taking into account the desirability of maintaining a balanced program of outer solar system exploration, we conclude that a program combining TOPS Grand Tour with the more intensive study of Jupiter and Saturn with flybys, probes, and orbiters is fully justified. Our specific recommendations on outer planetary exploration are as follows:

1. The combination of planetary configuration, scientific opportunity, and technological capability represented by the TOPS Grand Tour concept provides an impressive opportunity to acquire important data on the organization and evolution of the solar system and on galactic space. The proposed TOPS-type spacecraft incorporates a powerful complement of instruments for observation of planets and the interplanetary and possibly interstellar media. It should, however, be fur-

ther developed to carry probes for entering distant planetary atmospheres. *We recommend that TOPS-type spacecraft be developed and used in Grand Tour missions for exploration of the outer planets in a series of four launches in the late 1970's, subject to the condition expressed in recommendation 2, below. Entry probes to make measurements in the atmospheres to at least a pressure depth of a few atmospheres or, if these are not feasible, turbopause (upper-atmosphere) probes should be included on two of the missions.* It will be reasonable to decrease the flyby payload weight on those missions that carry entry probes, since data acquired from probes reduce the requirement for data acquisition by remote sensing. If the full Grand Tour program cannot be mounted, the number of missions should be decreased to two in preference to reducing the payload.

2. There is a need for spacecraft that can be used to deliver orbiters and probes to Jupiter and Saturn for a more intensive research program than can be maintained with TOPS Grand Tour spacecraft. This capability should be based on an advanced spacecraft below the full TOPS technology (modified Pioneer, modified Mariner, or reduced TOPS) with reduced costs commensurate with the more limited lifetime and mission requirements. A program based on this capability should be maintained at all budget levels--as a supplement to a full TOPS Grand Tour program or as a supplement to a restricted TOPS Grand Tour program if the full program cannot be mounted or in the absence of a Grand Tour program. *We recommend that a program based on Pioneer-level technology or its equivalent be maintained for the exploration of Jupiter and Saturn and their satellites at a launch rate of about one every two years irrespective of whether the Grand Tour program can be mounted.*

3. The radiation belt of Jupiter constitutes a hazard of undetermined magnitude for close-in Jupiter flybys, orbiters, and entry probes. *We recommend that Pioneers F and G be utilized to evaluate the radiation environment of Jupiter as fully as possible, even at the risk of possible disablement of the spacecraft, and that Pioneer H be held in readiness for use as a Jupiter magnetosphere mission for further evaluation of the radiation hazard if it has not been clarified by Pioneers F and G.* This will permit the choice of safe trajectories for both Grand Tour missions and those for the more intensive study of Jupiter. Studies of instrument design for Pioneer H to operate in a high-intensity radiation environment should also be started soon in case such hardened instrumentation should turn out to be the only solution for Jupiter exploration conducted within its radiation belt.

4. Any program of outer planetary exploration must have an accompanying base of ground-based research to improve the interpretation of the data and to provide supplementary observational data. *We therefore recommend that NASA continue a strong program of earth-based studies, including observation using satellites, rockets, balloons, aircraft, and ground-based instruments as well as laboratory and theoretical studies.* These should include studies of the Jupiter radiation hazard.

5. The full versatility of the TOPS spacecraft in Grand Tour missions cannot be realized without the development of a seven-segment Titan or its equivalent. A program focused on Jupiter and Saturn based on Pioneer-level technology is considered to be essential to outer planet exploration at all budget levels; this too would benefit from the seven-segment Titan--specifically, the 2.5-year mission to Saturn is impossible without this capability. *We therefore recommend the development of the seven-segment Titan or its equivalent for outer planet exploration at all levels of support.*

6. It is important that members of the scientific community become involved in detailed planning activities for the intensive exploration of Jupiter and Saturn and that they should have the opportunity to develop mission strategies in the same way as has been done in planning TOPS Grand Tour missions. A continuing interaction with the scientific community is needed as the mission is developed. *To this end we recommend that support be provided for activities of scientific advisory committees, development of instrumentation, and studies of alternative mission strategies for the exploration of Jupiter and Saturn.* Such activities could be integrated with those already in existence for TOPS Grand Tour missions.

7. *We recommend that NASA continue to support the development of advanced methods of solar and nuclear electrical propulsion.* These will be needed in the 1980's when Jupiter-assisted Grand Tour missions are no longer favored by the alignment of the outer planets.

2 Introduction

The major part of the sun's planetary system lies beyond the orbit of Mars. Almost all of the mass and angular momentum and diversity in observed physical properties that exist in the planetary system are associated with the major planets--Jupiter, Saturn, Uranus, Neptune, Pluto, and their attendant satellites. The phenomena by which the expanding atmosphere of the sun interchanges mass and angular momentum with the galaxy occur in this region also and bear a direct relation to the evolution of stars and the interstellar medium. If we are to be truly capable of meeting the challenge of the broad questions in space science concerning origins and evolution of planetary systems, then this part of the solar system must be fully explored, quantitatively described, and meticulously studied.

The state of our knowledge of the outer solar system is fragmentary and limited. Nevertheless, a cursory review reveals the outer planets as the seat of phenomena on a massive scale. Jupiter, the nearest, and about which we are most confident in our knowledge and which may share the characteristics of all, exhibits an extensive and powerful magnetic field, a prodigious source of internal energy, and an atmosphere in violent motion. It is the center of a large family of circling satellites as diverse in their properties as are the planets themselves. The scale on which these satellite systems exist is enormous: three of the Galilean satellites of Jupiter equal or exceed the earth's moon in size, as does Titan in Saturn's system and Triton in Neptune's. The satellites' known properties give promise of rich rewards from an exploratory program. In many cases they exhibit density and gross surface properties which indicate bodies of composition and structure entirely

different from any sampled in the inner solar system--a class of objects completely separate from that represented by the earth's moon.

The importance then in outer solar system exploration lies in defining the quantitative state of the sun's extended atmosphere and its family of planets and satellites. We are assured, even on the basis of the meager facts available to us at this time, of discovering new physical phenomena and gaining new insight into partially understood phenomena in a broad range of scientific endeavors including planetary magnetism, dynamical meteorology, geology, and astrophysics. The outer solar system provides a vast laboratory for experimentation in scientific desciplines that deal in scales that are impossible to simulate on earth. Much of this experimentation will be done in a continuing program of intensive study of individual planets; however, before reasonable priorities can be set in such a program, a broad reconnaissance in both a scientific and programmatic sense is required. The opportunity for conducting such a program is available to us with Grand Tour-type launches in the 1975-1980 time frame. Jupiter and Saturn are much more accessible than are the more distant planets, presenting the opportunity for more intensive exploration of two of the outer planets in a much shorter time frame than that required to reach the more distant planets.

It is clear that the exploration of the outer planets and their satellites is important to our developing understanding of the solar system and its evolution; that experimental apparatus has reached the state of development where it is ready to make both exploratory, first-look measurements of the outer planets and definitive measurements on those about which we already know enough to formulate key questions; and that spacecraft technology is ready to carry instrumentation to the outer planets and send back a wealth of data. The scientific opportunity is clearly at hand and ready for exploitation. Two principal spacecraft systems are immediately in prospect, and they are complementary in their capability. The Thermoelectric Outer Planet Spacecraft (TOPS) system offers the opportunity to send spacecraft to the most distant planets with launches in the late 1970's, making use of gravitational assist on passing Jupiter; its capabilities are matched mainly to the requirements of exploration beyond Saturn. The Pioneer-type system has less lifetime and communication capability and is better matched to the requirements for exploring Jupiter and Saturn, although its less flexible and less powerful imaging capability puts it at some disadvantage by comparison with TOPS. Beyond the 1970's, another propulsion system will be required to reach the outer planets, and the obvious candidate is electric propulsion.

The research opportunity is great, and the technical capability is at hand. The time to commence exploration of the outer planets has arrived, and such exploration should proceed during the 1970's and 1980's.

3 Scientific Questions

In planning a major experimental or observational program, it is important to keep in mind the diversity of uses for the information to be obtained. For exploratory purposes in new areas, essentially any qualitative or semiquantitative information may be significant. In contrast, major progress in areas that have been investigated previously--even crudely--may depend upon obtainment of more precise values of a few parameters or quantities. Usually the more advanced the understanding or the theory, the more precise the questions that can be asked.

PLANETARY INTERIORS

Planetary interiors are probably more difficult to study than any other parts or aspects of the solar system because essentially all pertinent information is based on indirect observational evidence and on not too certain theoretical assumptions. The construction of models of planetary interiors is based on solutions to hydrostatic equilibrium equations using equations of state appropriate to the expected chemical composition of the planet. The chemical composition is deduced from a variety of factors including atmospheric composition, solar composition, and density. The solutions provide, ideally, the radial dependence of all the important quantities such as density, composition, and temperature; also, they have to be consistent with a number of boundary conditions, which can be deduced from observation.

The pertinent observational information, in rough order of importance, is as follows: (1) total mass; (2) radius (and oblateness); (3) atmospheric composition (especially the H/He ratio); (4) gravitational harmonic coefficients J_2, J_3, J_4, etc.; (5) shape and strength of the magnetic field at several planetary radii and close to the surface; (6) pattern and magnitude of heat flux, surface temperature, and heat balance at various latitudes and phase angles; (7) shape and intensity of the tail of the magnetosphere or of the cavity in the solar wind; and (8) local anomalies such as gravitational or magnetic fields above the Red Spot of Jupiter. The gravitational coefficients are very sensitive to the density profiles in the outer parts of the planet; the heat flux and surface temperature have to be interpretable in terms of heat balance, internal heat sources, and internal heat transport mechanisms; the magnetic field has to be accounted for either by a primordial field (limited by the internal electrical conductivity) or by a hydromagnetic dynamo mechanism operating either in a deep, necessarily liquid, interior or in liquid outer layers; the tail of the solar-wind cavity throws light on the electrical conductivity of the planet as a whole. Items (1) and (2) above are known for Jupiter, Saturn, Uranus, Neptune, and Pluto, with rapidly decreasing certainty in that order. The new low values of the density of Uranus and Neptune have upset all previous speculations concerning their interiors, but, on the other hand, they seem to have narrowed the possible choice of the internal chemical composition.

So little is known about the mass, radius, and density of Pluto, of planetary satellites including Saturn's rings, and of asteroids that any reliable quantitative information about them would be of tremendous importance for a better understanding of the nature of the interior structure and perhaps even of the history of these bodies. Any additional data concerning other parameters, such as those of the magnetosphere or solar-wind tail, or temperature or albedo, would be welcome.

The assignment of relative priorities to the various classes of information desired is a subjective matter dependent on its ultimate use. To those who are interested in developing acceptable models of the interiors of the giant planets, probably Saturn, Uranus, and Neptune present the outstanding questions because some of the most important parameters are entirely missing. For Jupiter the situation is somewhat better, but much improvement in the reliability of the parameters is desired. From the purely exploratory point of view, studies of the smaller bodies such as satellites, asteroids, and Pluto (which probably had a different origin than the other planets) are of primary interest. Any information about their density would be particularly welcome.

Prerequisite and important to all studies of planetary interiors is the availability of (1) equations of state (at high temperatures and pressures) of hydrogen, helium, and hydrogen-helium mixtures; (2) theories of transport properties of these substances in liquid and solid forms (electrical conductivity, thermal conductivity, viscosity, diffusivity); (3) theories of deep convection, especially for the experimentally inaccessible systems in which the angular velocity is perpendicular to the gravitational acceleration; and (4) theories of the origin of magnetic fields, i.e., quantitative theories of the hydromagnetic dynamo mechanism. Unfortunately, not much work is being done in these areas although the cost of such theoretical work is small compared with the cost of experimental studies. We urge that these areas be strongly supported in the future.

ATMOSPHERES

Studies of planetary atmospheres should be strongly emphasized in any orderly program for the exploration of the solar system. These studies will contribute significantly to our understanding of the early history of the solar system and will also increase our understanding of the earth's atmosphere and how planetary atmospheres develop.

There are three broad areas of interest: atmospheric composition, atmospheric structure, and atmospheric dynamics. We know that the atmospheres of Jupiter, Saturn, Uranus, and Neptune contain large amounts of H_2, and we suspect the presence of significant amounts of He. Models for the origin and evolution of the solar system suggest that Jupiter's and Saturn's atmospheres should contain a cosmic mix of gases. Detailed studies of their atmospheres should therefore yield invaluable data on conditions in the early solar system. Definitive models cannot, however, be advanced until detailed compositional and structural data are acquired. This information can only be acquired by direct *in situ* measurements, radio occultation experiments, infrared and ultraviolet spectroscopy on flyby vehicles, and, untimately, mass spectroscopy carried out on entry probes.

The outer planets offer fascinating targets for study by the meteorological disciplines. Jupiter is especially notable in this regard. It exhibits a remarkable diverse, dynamic condition evident on ground-based photographs. There are a variety of multicolored cloud formations. Short-period fluctuations are apparent, as are changes with much longer time constants. The Red Spot is well known and has been present for at least three centuries. As yet there are no acceptable models for this remarkable phenomenon, although many theories have been advanced (for example, the Taylor column hypothesis in which a large body deep in Jupiter's atmosphere is postulated which affects the atmospheric flow at all higher levels). There is little doubt that the range of dynamic phenomena in the outer planets will influence the development of meteorology to a large extent. Techniques already available and proven for earth observations (for example, on Nimbus, Tiros, and Applications Technology Satellites) can be applied to the outer planets.

SATELLITES

The satellite systems of the outer planets appear to be a highly heterogeneous group of objects in terms of their known properties: their estimated densities span a range of at least a factor of 4; their albedos a factor of 5; their color indices show a range of 3 (many display a variability in their reflected light); and the largest of their diameters is some 500 times that of the smallest. It is quite clear that many of these satellites represent classes of objects quite different from that of the earth's moon or, for that matter, any of the known planets. In the exploration of the outer solar system, great emphasis should therefore be placed on the study of the satellites. Measurements in the thermal infrared should be made to evaluate the thermophysical properties of their surfaces; high-quality imaging of their surfaces should search for evidence on the strength and nature of their surfaces and indications of the presence of past and present endogenic activity. On those satellites without an atmosphere or other erosion agents, the surface can also be expected to reveal significant information on the past meteoroidal environment and the age of the surface. Radio science measurements involving echoes from satellites will yield important surface electrical properties, and a combination of observations in the visual, thermal infrared, and ultraviolet should allow estimates of atmospheric properties whenever an atmosphere is indeed present.

The satellites of the outer planets by virtue of the diversity of their presently known properties offer the potential of exceptional scientific rewards and should be considered with equal emphasis to that placed on the planets themselves when planning for outer planets missions.

METEOROIDS AND ASTEROIDS

Beyond Mars, a spacecraft going to the outer planets passes through regions populated by comets and asteroids. The comets slowly disintegrate under action of solar heating and solar-wind bombardment; the asteroids disintegrate, too, as they collide with one another. The debris slowly spirals inward, ultimately to be vaporized by the sun: it ranges in size from huge boulders to a swarm of fine dust grains that permeate the ecliptic plane.

The spacecraft may pass through this interplanetary dust without much damage; with low probability it may also hit a large rock and suffer partial or extensive damage. Meteoroid particle counters of the optical type aboard the craft can tell us much about the size and velocity of the dust grains and about the occasional more distant passage of larger fragments. We may, therefore, learn more about the origins of this debris. Is it all cometary and asteroidal, or does some of it come from the further reaches of the solar system? Is any of it interstellar, coming from further away in the galaxy? The orbital characteristics of the grains may tell; so also might their chemical composition. Some of the matter may represent substances little changed since the formation of the solar system. Other components may have had a complex history.

We would like to answer these questions: Is there much dust beyond Jupiter? Is it more prevalent there than inside Jupiter's orbit? Is the dust in Saturn's rings similar to that in interplanetary space? Where does it come from? Do other planets have dust rings like Saturn's only more tenuous? We do not know. Until we do, we will not fully comprehend the origin and nature of the outer solar system.

PARTICLES AND FIELDS

Exploratory missions to the far reaches of the outer solar system launched during the 1976-1980 period provide an unprecedented opportunity for the direct study of astrophysical problems of widely differing scales. These missions will give us the opportunity to measure quantitatively, *in situ*, the essential quantities to describe the space surrounding a star and its planets and to sample the space in the interstellar galactic region beyond the influence of the sun. These quantities are the magnetic field strength and the characteristics of plasma and energetic particle populations. A significant part of radio-astronomical information is directly related to the dynamics of these particles and fields.

In the *intensive phase* of outer-planet investigation, measurements of magnetic fields, plasmas, energetic particles, and radio waves are of great interest. Orbiting spacecraft of Jupiter, for example, will provide detailed mapping of the particle populations trapped in its magnetosphere. These orbiters will permit for the first time detailed comparisons of planetary magnetospheres, the earth's and Jupiter's. This comparison should lead to a more fundamental understanding of the processes of energy conversion and driving forces in planetary magnetospheres. We expect many of these results to be valuable in understanding more striking astrophysical phenomena and pulsars.

Astrophysics

Outer-solar-system missions will provide the first opportunity to measure galactic cosmic rays essentially free of the effects of solar modulation. From the astrophysical point of view, the observations will be of fundamental significance. The value of these energetic particle data will be further enhanced by simultaneous *in situ* measurements of magnetic fields, plasmas, and radio waves. Cosmic rays are energetically the dominant component of the interstellar medium; they may to a large extent control the dynamics of the interstellar gas. (For example, in the galaxy they play a critical role in forming interstellar gas clouds, and hence, in the formation of stars.) Cosmic rays are responsible for most of the galactic radio emissions. Their composition and energy spectra carry information on their origin, age, mode of propagation, and the signature of their sources. Galactic cosmic rays represent the only known material from stars in the galaxy that penetrates into the solar system at medium and high energies (>150 MeV/nucleon) and that exists to much lower energies immediately outside the solar system (i.e., beyond the influence of the solar modulation).

A significant amount of the angular momentum of the solar system rests in its planets and in the expanding solar corona--the solar wind. Missions to and beyond the outer regions of the solar system provide an opportunity to explore and, to a large extent, to determine quantitatively, the dynamics and energetics of the interplanetary medium. Magnetic fields, plasmas, and energetic particles are the major components of the interplanetary medium, and their topology and modes of interaction are only known in a narrow region near the inner planets. The termination of the solar wind and the region of

transition from solar plasma to the interstellar plasma, which defines the heliosphere, is of considerable interest. Since the sun is moving with respect to the surrounding interstellar gas, the heliosphere is not spherical; its outer boundary will be closest in the direction of relative motion of the sun, which is in the approximate direction of the contemplated outer-planet-mission trajectories. The disturbed magnetic fields in the plasma of the solar wind inside the heliosphere affect the propagation of galactic cosmic rays into the solar system, considerably reducing their intensity below GeV energies, and are believed essentially to exclude cosmic radiation below energies of several hundred MeV. Many aspects of the solar modulation of galactic cosmic rays are understood qualitatively, but quantitative knowledge is lacking, awaiting the comprehensive topological study offered in outer-solar-system missions. Such investigations will benefit from the study of both galactic cosmic rays and the dynamics of solar injected energetic particles, serving as probes of the medium.

Planetary Physics

We know presently of two planetary objects that are surrounded by magnetic fields containing plasma and energetic particles. One of these magnetospheres, the earth's, has been explored directly by spacecraft and rockets. Indirect exploration of Jupiter's magnetosphere has been done from earth by means of radio astronomy. The outer-planets, outer-solar-system missions provide not only the opportunity to explore Jupiter's magnetosphere and radiation belts directly, but they will also determine if magnetospheres surround the other major planets as well. As the spacecraft approaches such a planet, the exploration will begin by scanning the emission of electromagnetic waves: x rays, ultraviolet, and radio waves. An imaging experiment can search for auroras caused by precipitation of particles from the magnetosphere on the night side of the planet. Upon reaching the outer limits of the planet's magnetosphere, its interaction with the streaming solar wind will be investigated. Inside the planet's magnetic field, the plasmas, waves, and energetic particles will be directly accessible for measurement. Such information from different magnetospheres under different physical environments will give a basis for identifying the basic driving forces in the space surrounding a planet and its ionosphere. In addition, they will allow deductions on the constitution and dynamics of the planetary interior, the question of planetary dynamics, and the related subject of the quantum mechanics of metallic hydrogen as functions of pressure.

RADIO ASTRONOMY

The ability to observe the outer planets close up offers exciting opportunities in the area of planetary radio astronomy. Jupiter has fascinating radio properties that defy explanation in spite of 20 years of detailed study. Much has been learned about the planet through observations of its radio emission, but little is known about the origin of the intense radiation at wavelengths in excess of 10 m. It is well proven by earth-based observation that Jupiter's decametric radiation is intimately coupled with the juxtaposition of Jupiter and its satellite, Io, but the nature of the coupling is still the subject of considerable speculation. The other distant planets are not known to radiate at long wavelengths, but observations made in the vicinity of the planets will lead to a vast increase in detectability of any such emission. Decametric observations of Jupiter from a spacecraft will allow the determination of the spectrum at wavelengths not observable from earth, the detection of weaker sources on the planet, the determination of the positions of the sources, the determination of the radiation pattern of the sources by observations over a range of planetocentric latitudes, and the correlation of occurrence of radio emission with other planetary phenomena.

At shorter wavelengths--a few meters or less--radio observations offer the distinct possibility of measuring thermal emission from the planetary atmosphere or surface or both. This will provide a significant contribution to the understanding of the planets' thermal regimes because of the ability to penetrate clouds and deep atmospheres.

The radio receivers used for the planetary studies can also study the low-frequency emission from the galaxy. The galactic emission originates in the interstellar medium and will be modified by the interplanetary medium; thus observation of this emission provides information on both media.

4 Initial Exploration and Intensive Investigation

The 1969 Space Science Board report, *The Outer Solar System: A Program for Exploration*, offered an exploration strategy that is a compromise between two important alternatives: (1) a broad-brush exploratory first look at the outer solar system and (2) investigations designed to answer specific questions of crucial scientific importance about the outer solar system. The first-look, exploratory approach places emphasis on the least-known objects--Uranus, Neptune, Pluto, and the satellites. It makes use of flyby science and places great importance on television imaging. These features make the approach particularly suitable for the Grand Tour opportunity, and it has generally received preference in NASA planning for outer planetary exploration. The investigations designed to answer specific questions of crucial scientific importance can be conducted only when enough is already known to identify the crucial questions and hence are appropriate to a more intensive phase of exploration focused on the most accessible of the outer planets.

Arguments for and against the two alternative mission strategies can be summarized as follows:

For first-look exploration. To look into the unknown is one of the most exciting activities known to man. The value cannot be estimated except by hindsight--the possibility of a completely revolutionary discovery must be considered. A quick first look is needed before more sophisticated investigations can be effectively designed. The Grand Tour flyby approach provides access to a large number of objects, especially if we count both planets and satellites; the view has been expressed that satellites offer the best approach to an understanding of the history of the solar system.

For intensive investigations. The most effective methodology of planetary exploration involves an interaction between theoretical ideas and direct observations. For Jupiter and to a lesser extent Saturn, first-order theoretical models are available and sophisticated measurements are within the state of the art. Jupiter is by far the largest planet in the solar system; it contains most of the angular momentum; it is the only planet besides earth known to have a magnetic field and a magnetosphere; it can be observed in some detail from earth in many spectral regions, so rare opportunities for observation from spacecraft can be related to continuing ground-based programs. As regards the importance of satellites, many are accessible in the investigation of Jupiter and Saturn.

The 1970 Space Science Board study on space science and earth observations priorities* gave emphasis to the intensive exploration of Jupiter using entry probes and orbiters in addition to flybys. The 1969 SSB study† recommended a mix of probes and orbiters to Jupiter and Grand Tour flybys, with priority to the former. There are protagonists for both points of view, and we are led to the conclusion that a balance between the two approaches should be maintained. This has implications with regard to the Thermoelectric Outer Planet Spacecraft (TOPS).

The fundamental importance of TOPS is its 10-year design lifetime. This long lifetime, however, is achieved at the expense of increased weight and cost. It is not a particularly suitable spacecraft for use as an orbiter or probe carrier, for its long life is not needed for Jupiter and Saturn, the two targets appropriate for intensive exploration, and it is more expensive than possible alternatives. The principal alternative for which studies are available is the modified Pioneer, whose cost as a Jupiter probe carrier is 3.7 times less than TOPS. Other possibilities could be a Mariner or a five-year lifetime TOPS, but adequate studies have not been made of these two concepts.

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

†*The Outer Solar System: A Program for Exploration* (National Academy of Sciences, Washington, D.C., 1969).

5 Mission Planning

For an orderly sequence of missions in any exploration program, it is important that the opportunity exist to revise or alter the plans as new scientific knowledge develops from the program. This is obviously necessary in the case of environmental hazards that may be found to be injurious to spacecraft to the point of destruction; in such a case, sturdier spacecraft must be built or the mission plan altered to avoid the hazard. For the orderly development of knowledge, it is just as important to make use of increased understanding or knowledge provided by early measurements to design instrument packages that can answer critical questions that can be formulated in the light of the increased understanding. The temptation to fly the same payload several times may be self-defeating--duplicating measurements will not, in many cases, provide any new information, and it can eliminate the opportunity to use the spacecraft for definitive observations that answer critical questions. This factor assumes great importance when mission lifetimes are as long as those required to reach the outer planets.

NATURAL HAZARDS FACING SPACECRAFT

Two hazards await spacecraft venturing close to Jupiter. First, the belt of asteroids and meteoroids lying beyond Mars has to be crossed. Next, as Jupiter is approached, the bombardment of the spacecraft by high-energy electrons and protons becomes increasingly intense.

The continuous, small-scale erosion by fine meteoroidal debris should be assessed on early missions and, if necessary, factored into the design of more advanced spacecraft. With very low probability, the spacecraft may also suffer a more disastrous encounter with a large fragment; there is no ready antidote to such collisions, but the risk is very slight. A second meteoroid belt probably lies beyond Jupiter. It is not known if it is a greater or lesser hazard than the inner belt; but the first mission beyond Jupiter should carry meteoroid sensors that will return information about these grains and the hazard they pose.

The electron and proton components of the Jupiter radiation belt produce a different kind of erosion. They degrade the performance of transistors and other solid-state devices. At low irradiation levels the instruments may recover; at higher levels they may be irreversibly destroyed.

To assess these spacecraft hazards and their effects on exploration of the outer solar system, use can be made of data that will be provided by Pioneers F and G. At least the first of these should be brought as close to the Jovian surface as is compatible with quarantine specifications--about 1.3 Jupiter radii. The importance of accurately assessing the radiation hazard is such that possible degradation of the spacecraft to the point of failure should not compromise the assessment; that is, the mission requirements should put higher priority on radiation belt measurement than on postencounter science. Should Pioneer F and G spacecraft prove inadequate to this mission requirement because the bombarding particle flux is too high, Pioneer H should be redesigned specifically to probe the Jovian radiation belt. Until then, further orbiter or flyby missions involving Jupiter should avoid approach closer than 6 Jupiter radii--a distance judged safe by all estimates. Such a large distance still is compatible with some of the Jupiter-assisted trajectories to the outer planets and with high-altitude orbits, and missions of these types should receive priority over low-altitude Jupiter orbiters until the severity of the Jovian radiation hazard has been assessed.

The feasibility of delivering a turbopause or entry probe to Jupiter and perhaps also to Saturn has brought attention to the question of the influence on instrument operation of induced radioactivity in the probe after passage through an intense radiation belt. This problem occurs even in earth-orbiting spacecraft that pass through the relatively mild (compared to Jupiter) South Atlantic anomaly and should be immediately studied to ensure that any impact on a probe concept is fully understood at an early date.

SCIENCE PLANNING

The priority of different missions to the outer planets and the relative importance of different types of observation should be assessed by a specifically selected Science Steering Group. Of particular importance in its considerations should be the orderly planning of new goals through use of information gathered on earlier missions. The danger of rigid plans, faithfully carried out long after they are obsolete, must be avoided. Accordingly, flight schedules must be developed that permit an orderly reassessment and a replanning of missions based on new information. This will not be simple; such flexibility sometimes tends to conflict with the extensive reliability tests demanded by the scope of a mission to the outer planets. Scientists and engineers should, therefore, develop schemes that will assure flexibility combined with high reliability.

The most effective service from spacecraft and the highest information yield from the instrumentation are expected when a flyby reconnaissance mission with or without probes precedes the first orbiter sent to a planet.

In the past, we have greatly relied on visual pictures relayed from spacecraft, and our missions have largely been designed with this factor in mind. The past decade has, however, brought about great advances in imaging at different infrared wavelengths of prime importance to the planetary spectrographer. We must expect that infrared and possibly other devices will require the same high data transmission rates now reserved for television pictures from spacecraft, and our new missions should be designed with this new priority in mind.

6 Spacecraft Systems

Many spacecraft systems can be visualized that might play a role in outer planetary exploration. To avoid speculation on the capability and cost of many possible spacecraft systems, we confine our attention to only those systems that have received some serious engineering consideration.

THERMOELECTRIC OUTER PLANET SPACECRAFT (TOPS)

The TOPS is a new spacecraft designed with the Grand Tour missions in mind but with sufficient flexibility and versatility that it could become the principal workhorse for scientific space exploration in the outer solar system for the next 15 years. Even if the Grand Tour program is not mounted, the future will ultimately contain outer planetary missions utilizing solar electric or nuclear electric propulsion, and the TOPS spacecraft could play a vital role in these programs.

The principal characteristics of TOPS are as follows:

Total spacecraft launch weight	About 1400 lb
Spacecraft power	400-500 W from radioisotope thermoelectric generator (RTG)
Science instrument payload	205 lb
Science power	130 W
Communications system	*S* band earth to spacecraft; coherent *S* and *X* band on spacecraft

Communications data rate	100,000 bits/sec from 5 AU 4,000 bits/sec from 30 AU
Data storage	2×10^9 bits using on-board tape recorders; 8×10^6 bits in buffer storage
Attitude control	Three-axis stabilized
Command and control	On-board decision making by a self-test and repair computer system (STAR) with backup ground control
Navigation	Earth-based ranging and Doppler tracking complemented by on-board optical measurements for planetary and satellite approach guidance
Design lifetime	10 years minimum

The TOPS spacecraft differs from the present Pioneer spacecraft in three important aspects. First, it is a three-axis stabilized spacecraft rather than a spinning one. This is an important consideration for imaging experiments, which are performed most easily and with the best signal-to-noise ratios from stabilized platforms. Although particle and fields experiments are served better by a spinning spacecraft, modulation of instrument direction is possible in TOPS. Second, TOPS is designed for a 10-year minimum lifetime; this is crucial for Uranus, Neptune, and Pluto exploration as well as for the investigation of the interstellar medium unperturbed by the solar system. Third, the mass data-storage capability of TOPS requires only a once weekly commitment of the deep-space network for complete data transmission from the spacecraft during interplanetary cruise. This is a very important factor in reducing loads on the deep-space network.

TOPS can be used as a probe carrier with little modification and only a modest reduction in scientific payload. With the addition of orbit capture propulsion, TOPS can be converted to a planetary orbiter. However, this would result in a sacrifice in scientific payload because of the weight of the orbit capture propulsion system.

Development costs for TOPS are of the order of $440 million. Conversion to a probe carrier and orbiter would cost of the order $20 million and $130 million, respectively. Funding for study and design of TOPS spacecraft has been approximately $25 million to date.

We recognize the vital role that TOPS can play in outer planetary and outer solar systems exploration, and we recommend its development for use in Grand Tour missions to be launched in the period 1976-1980. We also recognize its value in connection with solar electric or nuclear electric propulsion for outer planetary missions during the 1980's.

We specifically recommend the following:

1. TOPS and the scientific payload should not be designed for a specific single mission. The spacecraft and, in particular, the scientific payload must be sufficiently flexible that a substantial part of the science payload can be changed for each flight. In consonance with this viewpoint, we recommend that if four Grand Tour missions are flown, two of them carry entry or turbopause probes to make *in situ* measurements in outer planetary atmospheres.

2. If the TOPS Grand Tour project must be curtailed for budgetary reasons, we consider that the first reduction should be in the number of missions, reducing it to two with the full planned payload; if further reduction must be made, we consider the minimum justifiable program to consist of two launches with scientific payloads not smaller than 130 lb. The minimum scientific payload for the outer planets Grand Tour missions ought to provide for the attainment of the following minimum objectives:

(a) Measurement of atmospheric and ionospheric refractivities by *X*- and *S*-band radio occultation at the same precision attainable on the full-scale TOPS;

(b) Measurement of heat balance and temperature profiles in the atmospheres of the outer planets and their satellites;

(c) Measurement of H, He, and H_2 abundances in the planetary atmospheres down to levels below the turbopause, in order to obtain the hydrogen/helium abundance ratio;

(d) Observation of minor atmospheric constituents, particularly those that will help to determine the ratio of isotopes (such as H and D) in the atmospheres;

(e) Observation of the interstellar hydrogen distribution;

(f) Measurement of magnetic fields up to 3 G;

(g) Measurement of the proton component in the distant solar-wind plasma;

(h) Measurement of the trapped particles in planetary magnetospheres;

(i) Measurement of the intensity, composition, and differential energy spectrum of galactic cosmic rays in the energy range 0.1-500 MeV;

(j) Interaction of a planet with the solar wind, planetary emissions, and interstellar and interplanetary waves by means of electric field measurements;

(k) Imaging of the planets and satellites to the degree compatible with the attainment of objectives (a)-(j).

PIONEER-LEVEL TECHNOLOGY

It is recognized that an advanced spacecraft much less expensive than TOPS, with good scientific payload, a good communications system, and a lifetime of about five years, will be required as a basic system for the intensive exploration of Jupiter and Saturn. Such a craft could be either a modified Pioneer or Mariner type; in the latter case it might be reasonable to utilize the TOPS design without the requirement of ten-year lifetime. This spacecraft requirement is approximately satisfied by Pioneer-level technology, so the means by which modified Pioneer could satisfy the requirement are described here.

The Pioneer class of spacecraft is basically a spin-stabilized spacecraft designed originally for interplanetary particles and fields missions. In its most recent manifestation--Pioneers F and G--for Jupiter flyby missions to be launched in 1972 and 1973, the basic spacecraft has been modified with the addition of a new radioisotope thermoelectric generator (RTG) and provision for course correction and precise antenna pointing. Two SNAP-19 RTG's provide 150 W of power. The spacecraft weight is 560 lb including 65 lb of experiments, in contrast with 140 lb and 40 lb, respectively, for earlier Pioneers. The reliability requirements are set to provide a five-year lifetime, a figure that has already been exceeded by Pioneer 6, which was launched in 1965. It is not clear whether a ten-year lifetime is achievable with Pioneer technology.

A well-studied modification of Pioneer (called modified Pioneer) can be produced for outer planetary missions that do not require longer than five-year lifetimes. These missions include Jovian orbiters and probes as well as Saturn missions via direct flight or with Jovian gravitational assist. The

SNAP-19 power generators of F and G would be replaced with two multihundred-watt generators. An *X*-band link would be added to the present *S*-band to increase the data bit rate communicable from 5 AU from 512 bits/sec to 16,000 bits/sec. No data-storage system other than buffer storage would be provided. The modification studied carries bigger fuel tanks than Pioneer F and G. These will permit the use of a storable bipropellant propulsion system to provide 2200 m/sec velocity increment for insertion of the spacecraft into orbit at Jupiter.

The nonrecurring costs associated with modification of the F and G models are $75 million for the orbiter, $50 million for a Saturn flyby version, and $66 million for a Jupiter probe carrier. A Saturn orbiter or probe version of this system would not be feasible with presently available launch systems. However, the flight time for a direct voyage to Saturn with the 560-lb spacecraft of the F and G class is only 2.5 years, and opportunities for such flights occur every year. The lower cost for the flyby modification mainly reflects the elimination of the bipropellant storable propellant system needed for the orbiter. Recurring costs would be $48 million, $25 million, and $32 million per spacecraft for orbiter, flyby, and probe, respectively.

Because these spacecraft are spinners, they are effectively limited to imaging systems of the spin-scan type. However, this represents a very substantial capability when combined with a stepping mirror, as illustrated by the earth pictures obtained from Applications Technology Satellites (ATS). Such pictures are probably adequate and suitable for planetary studies from orbit or flyby but appear to have some shortcomings where satellites are concerned.

Spinning spacecraft have advantages over three-axis stabilized spacecraft for certain particles and fields experiments. Measurements of inhomogeneities and pitch-angle distributions of particles are facilitated. Modulation of the magnetic field components along two axes permits correction for background fields.

Pioneer-type spacecraft or their equivalent are particularly suited for the intensive exploration of Jupiter and Saturn, delivering entry probes to both planets and orbiters to Jupiter. The relatively short transit times to these planets, compared with transit times to the more distant planets, offer the opportunity to explore progressively and to formulate definitive questions for succeeding missions to answer. Thus we feel that a program focused on Jupiter and Saturn, based on Pioneer-level technology or its equivalent, with a launch rate of about one every two years, should be retained in the program at all budget levels, including that at which the Pioneer-level and a minimum TOPS program cannot both be supported.

The emphasis of the Pioneer-type program should be the intensive investigation of Jupiter. If the Jupiter radiation belt proves an insuperable obstacle to entry probes and orbiters, concentration should be on Saturn; in any case, a significant priority should be given to Saturn missions and also to cosmic-ray investigations out of the ecliptic plane and in interstellar space.

The optimum science strategy for such a program focused on Jupiter and Saturn should be determined by a Science Steering Group supported by adequate funds for thorough studies of mission alternatives. Pioneer-type spacecraft can be handled relatively flexibly, without unduly restrictive time schedules and with maximum opportunity to respond to the results of previous missions. We regard this feature of the program as highly desirable because it corresponds closely to the optimum *modus operandi* of the science community.

PLANETARY ORBITERS

In Chapter 3 it was pointed out what observational information is needed for the development of satisfactory models of planetary interiors. While some of it can be obtained from flybys, such data are fragmentary and usually unsatisfactory. Thus the use of orbiters, which provide systematic surface coverage and longer observation times, is essential. In particular, useful values of gravitational coefficients, magnetic fields (which may permit differentiation between fields generated in deep interiors and fields generated in outside layers), heat flux, and temperature distributions are not obtainable without orbiters. Orbiters are also absolutely necessary as a source of information about the dynamics of the atmospheres of Jupiter and Saturn and about any changes occurring in the magnetosphere or solar-wind tails.

Various studies of orbiter missions made by NASA have been aimed primarily at atmospheric and magnetospheric studies, which are of limited value for improving models of planetary interiors. They assume long life of the spacecraft and highly eccentric orbits (ratio of axes of the order of 20 to 100), which imply short observation periods in close proximity of planetary surfaces. If the radiation belt close to the Jovian surface is indeed as strong as feared, one may not be able to get more than a few orbits and the periapsis may have to be as high as 6 Jovian radii. For the determination of the gravitational coefficients, the magnetic field close to the planetary surface, and any local anomalies, a rather low eccentricity is favored. For a perijove 6 R_j, the apojove should be not greater than 20 or 30 R_j. From the point of view of optimizing both the magnetic and the gravitational measurements, an orbit with an inclination of 30 to 45 deg is favored. In the equatorial plane, the odd harmonic gravitational coefficients, which are essential for studies of planetary rigidity, are not measurable.

It may be worth pointing out again that a better knowledge of the orbit of JV (Amalthea) is of particular interest for Jupiter. The presence of a periodic variation of its eccentricity and perijove would provide important clues about the rigidity of the planet.

ENTRY PROBES

It appears technically feasible to put entry probes into the atmosphere of Jupiter using a carbon heat shield. The development of a dielectric reflector heat shield offers another possibility. Such an entry probe, having survived the entry, could make valuable measurements of the composition and physical properties of the atmosphere to a pressure level of several atmospheres or even several tens of atmospheres, with an ultimate potential of operating to many hundreds of atmospheres. However, the value of measurements made even to a depth of a few atmospheres would be so great that we recommend the development of such a probe to be carried to Jupiter either by TOPS or modified Pioneer-type spacecraft. This would eliminate a vast uncertainty in atmospheric composition relating to those gases that do not have infrared absorption properties that permit their identification by remote observation.

TURBOPAUSE PROBES

Theoretical studies of the Jovian upper atmosphere suggest that an unshielded entry probe would penetrate below the turbopause, i.e., into the region where molecules are mixed homogeneously, before encountering significant heating. A few scale heights below the turbopause an unshielded probe would burn up, but some important measurements are possible before it does so.

The two main classes of measurement are concerned with the upper atmosphere and the region below the turbopause, respectively. Upper-atmosphere measurements are conventional and well developed on earth (airglow, electron density, plasma temperature, ion density). Such data would be invaluable for aeronomical purposes. Below the turbopause, the probe gives brief access to the material forming the bulk of the planet. The expectation is that this material is closely related to that of the early solar system hydrogen-to-helium ratio, and various isotopic ratios could give information of significance to cosmological studies; such information may not be available by any means other than *in situ* mass spectrometry.

The weight of a turbopause probe is about 200 lb, and it can be carried by Pioneer or TOPS spacecraft. The costs are relatively low compared with the costs of orbiters and deep entry probes ($18 million nonrecurring and $9 million recurring costs). The possibility exists of delivering them also to Saturn.

It is possible that deep entry probes cannot be launched before the 1980's. It could be important to obtain definitive compositional information on the Jupiter interior before this time. The turbopause probe offers a relatively simple concept which may be able to meet this need. Such a development is contingent upon a solution to the problem of the Jupiter radiation belts. However, measurements of the radiation belt may show that entry is feasible at high latitudes even if the probe could not survive passage through the belt at low latitude.

ALTERNATIVE PROPULSIVE SYSTEMS

The great advantage of the Grand Tour concept is that by using the gravity assist afforded by Jupiter, it is possible to fly by the outer planets with current Titan booster technology. However, alternative propulsion systems utilizing ion acceleration are either under development or being studied. In particular, a Solar Electric Propulsion (SEP) and a Nuclear Electric Propulsion (NEP) capability are being considered. These will be needed to provide a basis for deep-space exploration in the 1980's, after the opportunity for gravitationally assisted trajectories has passed.

The SEP concept uses solar cells to provide electric power to accelerate ions that provide low thrust for long periods of time. The SEP will provide direct-flight capability to any planet in any year with flight times of 2.3 years to Saturn, 5.2 years to Uranus, 7.3 years to Neptune, and approximately 10 years to Pluto with payloads of 1300 lb. It is estimated that the SEP system could be available in 1980 at a development cost of $90 million to $200 million.

The NEP system utilizes nuclear energy as the source of power and could provide a large amount of electric power for scientific experiments as well as for propulsion. The NEP could be launched by the seven-segment Titan vehicle and provide for 150-lb payloads. Direct flyby missions to Uranus, Neptune, and Pluto with flight times less than four years would be possible with NEP. Development cost of NEP is estimated to be between $285 million and $515 million. The nuclear electric power source would be useful in other applications, such as direct television broadcasts from a geostationary transmitter, and the development program would provide opportunities for international cooperation.

SEVEN-SEGMENT TITAN

The seven-segment Titan or its equivalent would greatly improve the launch capability for TOPS and Pioneer-type missions to the outer planets. It would make it possible to reach targets that cannot otherwise be reached with given payloads, and hence would greatly extend the flexibility of mission planning.

7 Earth-Based Observations

Earlier reports* have emphasized the importance of observations that can be carried out from the ground, aircraft, balloons and rockets, and earth-orbital observatories.

Much can be learned about planetary atmospheric properties through use of the orbiting astronomical observatory. In recent years, rocket observations have started to give valuable ultraviolet observations, and infrared measurements have yielded values for the temperature and thermal emission of the larger planets. A great deal, however, remains to be learned.

Wider availability of ground-based telescopes for the study of the outer planets should be encouraged. Multiplexing spectrometers should be developed for use in infrared observations from aircraft at higher altitudes as well as from the ground. Improved radio and radar facilities for continued study of the radiation belts and deep atmospheres of the major planets will also be needed. For a complete picture, a continued updating of our understanding of physical processes is needed too. Continued support is needed for laboratory spectroscopy; experiments concerned with primitive atmospheres and the formation of biogenic molecules through ultraviolet irradiation or electric discharges; theoretical studies concerned with the equation of state, elastic properties, and other physical characteristics; and studies on the origin of planetary magnetism.

We therefore recommend that NASA continue a strong program of earth-based studies including observation using satellites, rockets, balloons, aircraft, and ground-based instruments, as well as laboratory and theoretical studies.

---

*See, for example, Panel on Planetary Astronomy (J. S. Hall, Chairman), *Planetary Astronomy: An Appraisal of Ground-Based Opportunities*, NAS Publ. 1688 (National Academy of Sciences, Washington, D.C., 1968).

8 Recommended Programs

HIGHER BUDGET PROGRAM

The recommendations here are predicated upon a budget level of about $400 million a year ($350 million in 1970 dollars) for planetary exploration, referred to in *Priorities for Space Research 1971-1980* as the HIGHER Budget Program. At this level, we *recommend* that four TOPS Grand Tour missions be flown in the 1976-1980 time period, with entry probes (or, if these are not feasible, turbopause probes) carried on two of the missions. Those missions carrying probes would have a lesser requirement for remote-sensing instruments, and hence might reasonably carry less flyby instrumentation. However, the need for Pioneer-type spacecraft carrying probes or orbiters for the exploration of Jupiter and Saturn is not fulfilled by the TOPS Grand Tour program, and we *recommend* that spacecraft based on Pioneer-level technology be further developed for this purpose, with launches at a rate of about one every two years.

To maintain the capability to reach the outer planets after the opportunity for Jupiter gravity-assisted trajectories has passed, we *recommend* continued development of nuclear electric and solar electric propulsion schemes.

To improve the capabilities of both TOPS and Pioneer-type spacecraft for outer planetary exploration, we *recommend* the development of the seven-segment Titan or its equivalent.

Although inner planet exploration and cometary missions were not included in this study, the continuance of these programs was assumed and should be possible at the projected budget level without conflict with the recommended outer planet program.

INTERMEDIATE-LEVEL PROGRAM

The Committee recognizes that embarking on the full-scale Grand Tour may violate fiscal constraints and, hence, considered alternative options at a budgetary level of $250 million a year for outer planetary exploration, consistent with the INTERMEDIATE Budget Program of the Priorities Study. This necessarily involved tradeoffs or compromises or both between Grand Tour missions and other outer solar system programs, such as modified Pioneer-type missions. From this, two strong opinions of the Committee emerged:

1. The TOPS spacecraft will be a valuable, indeed necessary, tool for future space missions, and its development should proceed unhampered during the early to mid-1970's. Its use is envisaged not only for Grand Tour but also for long-life Jupiter or Saturn orbiters or both, cometary missions, and, possibly, external solar system missions. Even if the Grand Tour program is never funded, the future will ultimately contain outer planetary missions through SEP or NEP, and the TOPS technology will play a vital role in these programs.
2. Much valuable Jupiter and Saturn science can be accomplished with Pioneer-type spacecraft, and, even with the Grand Tour, this program must be continued. The ability of Pioneer-type spacecraft to deliver probes and flybys to Jupiter and to fly by Saturn at a relatively low cost is a valuable asset which should be fully utilized whether or not the Grand Tour ever comes into existence.

To accommodate to this budget level, the number of TOPS missions might have to be reduced to two, and finally the payload reduced to 130 lb, which was considered the minimum that could justify the development of a system of TOPS capability. The reduced TOPS missions and an outer planetary program based on Pioneer-level technology might both be accommodated at this budget level depending upon what is done in other areas of the planetary program, especially Viking follow-on. If both reduced TOPS and modified Pioneer-type outer planetary missions cannot be accommodated at this budget level, we *recommend* that the Pioneer-type program be maintained even at the expense of dropping TOPS. Development of the seven-segment Titan or its equivalent is also recommended.

To provide the capability of reaching the most distant planets during the 1980's, development work on nuclear electric and solar electric propulsion should proceed at this budget level.

LOW-LEVEL PROGRAM

This budget level corresponds to the BASE level defined in the Priorities Study. Approximately $400 million could be allocated for outer planetary science over one decade. At this budget level, the TOPS Grand Tour is no longer possible. Nevertheless, a substantial and varied program of investigation of the outer solar system could still be undertaken using modified Pioneer-type spacecraft.

It may be possible to devise such a program based upon Mariner spacecraft. Such studies as were presented to us suggested substantially higher costs for Mariner missions as compared with similar Pioneer missions. It was not clear to us whether increased capability justified this greater cost, and we believe that further studies of Mariner should be made.

We have no doubt, however, that the modified Pioneer is a highly capable spacecraft that can fly by Jupiter in 600-900 days and (with an appropriate booster) Saturn in 900 days with substantial payloads and communication bit rates. In addition, Pioneer could deliver turbopause probes and, perhaps, deep entry probes to Jupiter and Saturn and orbiters to Jupiter. A program containing many of these elements could be contained within the total cost for the decade of $400 million.

With our present state of knowledge, we do not expect that Pioneer or modified Pioneer will be a satisfactory spacecraft for multiplanet missions, i.e., for the Grand Tour. Nevertheless, it could be used for an orderly and intensive investigation of Jupiter and Saturn, particularly the former. Therefore, if budgetary restrictions make it impossible to develop the TOPS spacecraft, intensive investigation of the largest and best observed of the outer planets should proceed at the highest level possible. If we can add to this program some important investigations of Saturn and simple particles and fields measurements as far from the sun as technology and budget will permit, we shall have a program that will provide scientific returns well worth the cost, even though it will not allow us to have our first look and exploratory measurements at the planets beyond Saturn during this decade.

As regards exploration strategy, we do not at this juncture recommend between the possible Pioneer missions except to note the great desirability of probes down to a pressure depth of several atmospheres on both Jupiter and Saturn. If such probes prove too expensive to develop, or if they cannot

be launched before the 1980's, we believe that turbopause probes should be delivered as soon as practicable.

At this budget level also, the seven-segment Titan or its equivalent should be developed. In addition, support of nuclear electric and solar electric propulsion should proceed in order to provide the capability of reaching the most distant planets of the solar system during the 1980's.

APPENDIX

A Opening Remarks by the Chairman of the 1971 Outer Planets Exploration Study

NASA has asked the Space Science Board to convene a short summer study to make recommendations on exploration of the outer planets. The recommendations that may be made are highly dependent, of course, upon the opportunities that are recognized as reasonable and timely, and this depends a great deal upon just what engineering studies have been completed. Substantial presentations will be made during the first two days on the science opportunities that exist, the hardware systems that have been studied, and the projects that might be mounted.

There are also a number of previous Space Science Board studies to be taken into account, with particular attention to be paid to some inconsistencies among the earlier recommendations. Needless to say, unnecessary changes in recommended objectives are not helpful to NASA, and they tend to discredit study groups of scientists as a responsible source of advice. This is not to say that any study group should feel bound by the conclusions of earlier groups, but if two differing sets of conclusions or recommended programs put forth at different times are both to be valid, it should be possible to identify the changed conditions that are responsible for the changes in the recommendations. In any case, some attempt should be made to maintain reasonable continuity in recommendations and not to differ from earlier recommendations unless there is a good reason for doing so. Because of the magnitude of the individual projects involved, NASA cannot respond to frequent changes in recommendations after projects are once initiated.

The basic rationale for planetary exploration as put forth by the Space Science Board is rather general. It was stated in a 1965 report entitled *Space Research, Directions for the Future, Part One, Planetary and Lunar Exploration*, that the basic objectives were investigation of: (1) the origin and evolution of the earth, sun, and planets; (2) the origin and evolution of life; and (3) the dynamical processes that shape man's terrestrial environment. These objectives have been restated without significant change in most studies since 1965. However, the relative emphasis accorded these three objectives has changed with time. For many years, the investigation of possible life forms on Mars received high if not overriding priority in many recommendations. However, as the atmospheric parameters on Mars have become more precisely known, the likelihood of finding any form of life on Mars has diminished, and over the past several years there has been some downgrading of priority for the search for life forms on Mars as a means of investigating the origin and evolution of life. There has also been an evident reluctance to recommend projects that are regarded as being disproportionately large, such as the Voyager program; even Viking has clearly been less enthusiastically regarded with a cost in excess of $700 million than it was when the expected cost was near $350 million.

The 1965 Space Science Board study, *Space Research, Directions for the Future*, recommended that primary emphasis be given to Mars, secondary emphasis be given to Venus and the major planets, and significant attention be paid to comets, asteroids, and Mercury, roughly for the time period of the 1970's. A 1968 Space Science Board study on *Planetary Exploration*, focused primarily on the inner planets, recommended no single goal for planetary exploration during the period 1970-1975, but rather a broad program of planetary exploration. (This study nevertheless adhered to the concept of primary emphasis on Mars, at least through 1975.) A 1969 Space Science Board study on the *Outer Solar System* specifically recommended higher priority for Jupiter missions, including orbiters and probes, than for Grand Tour-type missions for the decade of the 1970's. This study was made without any constraints on the size of the program that might be mounted or on the impact that it might have on other areas of space science research.

The Priorities Study in 1970 was conducted by an Executive Committee and seven working groups for different areas of space science. The Executive Committee developed recommendations on appropriate balance among the various science areas for the decade of the 1970's, while the working groups developed recommendations within financial limitations specified by the Executive Committee. Each working group felt severely constrained by the financial limitations imposed by the Executive Committee; and without the constraints, recommendations would

have been put forth that would have added up to several times the funds that could realistically be expected. Consequently, there was frequent disagreement between the Executive Committee and the working groups; however, in retrospect, the Executive Committee seems to have exercised good judgment in its relative allocations. It recommended the allocation of an increased fraction of available resources (between 40 and 45%) to planetary exploration, in agreement with earlier Space Science Board recommendations, and it recommended low priority for funding of space biology other than exobiology, which is adequately covered in the Viking project. In any case, something near the Executive Committee recommendations must be adhered to if a reasonable degree of balance is to be maintained among the various areas of space science in the NASA program. Further, we cannot at this study undertake to reconsider the whole Priorities Study; in the main, we must accept its overall conclusions on level of effort as a reasonable constraint.

The Planetary Exploration Working Group of the Priorities Study gave highest priority to Planetary Explorers to Venus and Mars. Beyond that, it clearly favored the Grand Tour concept. It recommended a program focused on Jupiter only as an add-on in addition to the Grand Tour missions. It accepted Pioneer-type missions for Jupiter orbiters and Saturn flybys only as an ultimate fallback if Grand Tour missions could not be mounted.

The Executive Committee of the Priorities Study concluded that Grand Tour missions could be fitted into a reasonably balanced program only at the highest space science budget level considered, $792 million, in which case $350 million a year for planetary exploration was deemed a reasonable allocation. At the lowest budget level, in which case $100 million might be allocated to planetary exploration out of a total science budget of $241 million, the Executive Committee recommended Jupiter missions with Pioneer-type spacecraft in addition to the Planetary Explorers. An explicit conclusion was stated by the Executive Committee to the effect that a thorough study of Jupiter is, for the near future, the most rewarding objective among the outer planets and will contribute the experience needed for successful missions to more distant planets at a later time. In this regard, it differed with the conclusion of the Planetary Exploration Working Group but agreed with the conclusions of the 1969 study. Clearly, this is a difference on which we should express ourselves at this study.

There are several important factors that must be taken into account in considering the differing conclusions of the Executive Committee and the Planetary Exploration Working Group; it was consideration of these factors that led the Executive Committee to its conclusions. The first factor is the relative merits of the intensive approach (i.e., entry probes and repeated observations from orbit) concentrated on a single planet, Jupiter, and of the extensive approach of flyby observations of many planets and satellites (the first-look approach). This can only be judged on a subjective basis. The second factor is the meteoroid risk in passing through the asteroid belt and near the major planets, and the disadvantage of a heavy commitment to a program before this risk can be assessed observationally. Hence, the Executive Committee favored a cautious approach, starting with data from Pioneers F and G and holding back on major new commitments until the risk was assessed by observation. In this regard, it regarded a commitment to a Grand Tour program as involving high risk, as little could be done to modify the program if the meteoroid problem should prove to be severe. The third factor is the cost difference between a Jupiter program and a Grand Tour program, in which the Executive Committee believed that an intensive exploration of Jupiter could be accomplished at less than half the cost of Grand Tour exploration of the outer planets. The validity of this assumption might be questioned on the basis of the differing levels of engineering appraisal for the two programs; we should have better information on this during this study. A fourth possible factor is the differing opportunity for energetic particle and magnetic field measurements near the outer limits of the solar system. This might be accomplished either by the Grand Tour approach or by a Jupiter flyby and solar system escape based on Pioneer technology. The desired approach from this viewpoint might depend upon the type of spacecraft most suited to the measurements, in which each of the two suggested approaches has some advantages over the other.

I hope that in this study we can recommend programs to NASA at the approximate annual budget levels of $350 million, $225 million, and $100 million, in consonance with the expenditures thought appropriate by the Priorities Study Executive Committee at different total space science budget levels thought likely for NASA. To avoid inconsistency with earlier studies, the existence of the Planetary Explorer program should be assumed, unless it is really the considered opinion of this group that the earlier conclusions were wrong or that conditions have changed enough to warrant a change in direction in the NASA program. The question of Jupiter-first versus first-look at more distant planets should be addressed explicitly, although the conclusion will necessarily be implicit in the recommended programs. To assist us in the assembly of programs, NASA has arranged presentations on a number of possible programs and their costs.