Strategy for Exploration of the Inner Planets: 1977-1987

Committee on Planetary and Lunar Exploration, Space Science Board, Assembly of Mathematical and Physical Sciences, National Research Council

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Space Science Board
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Foreword

This is one in a series of documents prepared by committees of the Space Science Board that develop strategies for space science during the coming decade. The first such document, developed by the Committee on Planetary and Lunar Exploration (COMPLEX), appeared as a section of *Report on Space Science 1975* (National Academy of Sciences, Washington, D.C., 1976). That document approached the exploration of the outer planets in terms of scientific objectives to be achieved rather than the traditional emphasis on individual missions. As the committee states in the present report, it continues to believe that this approach provides greater flexibility for program planning and permits a perspective to be maintained despite the vagaries of year-to-year funding variations. When the Board adopted that report as its policy for outer-planet exploration, it commenced a program for the development of scientific strategies as its approach to policy guidance for investigations in space science. It has reviewed and accepted portions of the present report on the inner planets over the past two years, and it adopted the present document as Board policy at its May 1978 meeting. A third document developing a strategy for the investigation of comets, asteroids, and dust in space will be released within the next year and will complete the strategy for planetary exploration in the coming decade. Similar documents are in the process of development or in review in other areas of space science.

The Board appreciates the intensive effort required by COMPLEX to develop this approach to planetary exploration. It particularly appreciates the contribution of the Chairman of COMPLEX, Gerald J. Wasserburg, whose term ends with this major effort.

A. G. W. Cameron, *Chairman*
Space Science Board

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1
Introduction

In October 1975, the Committee on Planetary and Lunar Exploration (COMPLEX) submitted to the Space Science Board (SSB) a report proposing a long-term strategy for exploring the *outer solar system* for the period 1975–1985. The Space Science Board approved and adopted the report *in toto* as a statement of its policy on planetary exploration for the outer solar system. In its publication, *Report on Space Science 1975* (National Academy of Sciences, Washington, D.C., 1976), the Board requested that the committee "extend its consideration to the *inner planets*" in order to provide a strategy for the solar system as a whole.

In response to the SSB request COMPLEX submits this report, which

1. Proposes a long-term science strategy and goals for exploring the inner solar system for the period 1977–1987;
2. Assesses the key program elements that support and implement the strategy;
3. Re-examines the role of U.S.-U.S.S.R. cooperation in the strategy for planetary exploration;
4. Evaluates the status of *existing* planetary and lunar programs currently under way; and
5. Assesses the science content of *potential* missions in terms of the existing strategy and defined scientific goals.

It is our understanding that the SSB approval of the Committee's 1975 report included not only the recommendations and policy statements but also the approach employed in developing the strategy for the outer planets. Consequently, in considering the inner solar system, the committee retains its position that the science strategy will be defined in terms of a series of goals that are independent of specific missions. The Committee is confident that in separating strategy from specific missions (1) greater flexibility is provided for program planning and (2) the strategy remains intact regardless of the short-term fiscal fluctuations that can beset specific mission development and programs. However, this strategy can only be fulfilled by missions, and mission opportunities are constrained by restrictive launch windows, system development lead time, the flight time, and the period of investigation and assimilation of results, which together consume substantial portions of a decade. Consequently, we wish to re-emphasize that science strategy and mission planning are complementary efforts and that the urgency for action is not removed by the approach employed by the committee.

All of our discussions and preparations of this report have depended and been built upon the previous statements on planetary exploration by the SSB and its committees.

2
General Overview

In retrospect, it is again clear that the vigor and major achievements of planetary missions over the past decade reflect the fundamental role of solar-system exploration in the nation's space program. The effort and rewards of these attempts to extend man's senses to the vicinity and surface of neighboring planets have had pronounced effects on the culture of our people. While it is beyond the purview of the Committee to assess these effects, we believe that they are profound and of the highest value. These achievements have brought recognition to the United States for its leadership and capabilities and its policy of cooperation with other nations.

The reconnaissance mission phase for the inner planets is in its latter stages. Some intensive study of the moon has been carried out, the first intensive exploratory mission to Mars is near completion, and the first exploratory mission to Venus is being readied. A first reconnaissance of Mercury has been completed. Study phases for the outer planets are still mixed. Reconnaissance has been carried out for Jupiter, and, as recommended in the 1975 report, reconnaissance of Saturn and Uranus and exploration of Jupiter should be achieved within the next decade.

While a retrospective assessment of planetary achievements is gratifying, the Committee is deeply disturbed by the near-term outlook for the program. With the completion of the highly successful Viking mission to Mars there remained only two approved mission projects in the planetary program: Voyager and Pioneer Venus (PV). The PV mission approved in fiscal year 1975 (FY 75) was followed by an absence of any further planetary-science new starts up to FY 78, which represented a hiatus of three fiscal years. The

Committee was very pleased to note the final approval of the proposed Jupiter Orbiter Probe (JOP) mission contained in the FY 78 budget request. In its strong endorsement, the committee is keenly aware that JOP represents the single element required to complete the ten-year strategy for the outer planets. The struggle to achieve approval, however, has again pointedly reminded us that a national policy for planetary exploration is still to be defined. In the absence of such a policy, a coherent planetary program, based on a reasoned, conservative strategy and sound mission planning, will likely continue to be subjected to severe stress in the budgetary cycle. At stake in the survival of the planetary program is U.S. leadership in deep-space exploration, the basis for international cooperation in planetary ventures, and, perhaps most importantly, the opportunity to understand the history and evolution of our planet and the implications, from a planetary viewpoint, for its long-term environment.

3
Scientific Goals, Sequence, and Strategy

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. It is our view that by acquiring an understanding of the solar system and its components, our ability to decipher the evolutionary course of the earth and its environments will be significantly enlarged.

The earth is an inner planet, and, in developing a strategy for the inner solar system that responds to the primary scientific goal, we have viewed the earth from the perspective of deep space and as inhabitants on its surface. In doing so we have acquired a degree of objectivity that sees the earth and its sister planets, in a comparative sense, as objects of inquiry that are considered collectively in the strategy.

To better clarify our recommendations and strategy and the basis for these conclusions, we provide in the following text a brief categorization of the levels of scientific investigation, the related goals, and the techniques of planetary encounter by spacecraft.

LEVELS OF INVESTIGATION, GOALS, AND MISSION TECHNIQUES

There is a general relationship between the levels and goals of planetary investigations and the type of mission and technique employed. Further, the levels of investigation and their respective goals are generally progressive; that is, achievement of the scientific goals associated with a level of investigation forms the base for the next highest level of investigation and its attendant goals. And, as the complexity of achieving goals increases, there is a corresponding increase in the complexity of the mission type and technique.

Earth-based observations of the planets provide important data, which constitute a limited reconnaissance level of investigation and form the basis for defining the scientific goals of the first deep-space mission to a planet. The quality of observations from earth, however, will always be attenuated by the earth's atmosphere, and those from near earth, by distance. With improvement in instrumentation and techniques, the quality of these data will improve and may prompt redefinition of some scientific goals for deep-space missions. For the foreseeable future, however, the primary scientific goal of understanding the composition, structure, and dynamics of a planet and its history cannot be achieved solely from earth-based or near-earth-based observation. Further, in our opinion all the scientific objectives recommended in the strategy in this report are achievable only by deep-space missions.

As the first level of investigation, *reconnaissance* reveals the major characteristics of a planet in sufficient detail to allow us to define the goals for the next level of investigation. *Flyby* is the mission technique usually employed to fulfill reconnaissance goals with brief encounters of the planet. *Exploration* is the second level of investigation, which attempts to identify and understand the state of a planet and the large- and small-scale processes that have shaped a planet and its environment. To achieve these goals requires longer encounter periods with the target planet. Long-duration *orbiters* equipped with remote-sensing instruments to measure planetary properties on global scales are typical of the mission technique employed as one enters the exploration stage of investigation. During the *exploration* phase, *in situ* measurements by *entry probes* are typically required to establish more complete information of major importance on the chemical composition of the planet and its atmosphere and the internal state of the planet. In *intensive study*, the last investigative level, we seek to define or refine the remaining scientific questions of the highest order that have been revealed by reconnaissance and exploration and that can be studied in depth. In late stages it becomes increasingly difficult to classify or assign scientific objectives to a level of investigation. It is clear, however, that more advanced mission techniques and instrumentation are required to fulfill the science strategy and achieve the objectives. Sophisticated entry probes (including hard landers) and *soft landers* may be used to carry out extensive geophysical, geochemical, and biological measurements at single points on the planet's surface.

There exist a variety of approaches for carrying out intensive investigations of a planet. Some of these must require *in situ* investigations, and others may be attacked by the return to earth of materials selected on the planet. The choice of a particular approach must depend on the nature of the problems under investigation.

In developing a science strategy for a planet it is recognized that *in situ* studies and return of samples are important modes of the *intensive* stage of investigation and that the sequence and goals of some of the earlier investigations are to some extent *precursor* efforts. In proceeding through the sequence of investigations toward the scientific goals, the definition of objectives becomes correspondingly important to the point where the few missions preceding a sample return or *in situ* investigation must be viewed not only as scientific ventures in their own right but also as preparatory missions that will acquire information about the planet in order to optimize the scientific gain from these intensive study modes. To a large extent, the interactive and mutually supportive roles of return sample analysis and *in situ* studies will determine the soundness of the science strategy during the intensive stage of investigation.

For descriptive purposes, the relationship between science objectives, levels of investigation, and mission techniques must be treated in a generalized manner. They are all elements in developing a strategy for a designated planetary object, and their optimal tactical use ultimately is dictated by the character of the target planet, available funding, adequate instrumentation, technology, and celestial mechanics. Consequently, it is not uncommon to combine various elements in designing individual missions. Generally, however, information from any particular mission in the sequence of investigation should be adequately assessed within the context of the overall strategy, before initiating subsequent missions.

In order to provide more specific guidance to achieve the general strategy goals of planetary exploration, primary objectives, which are the principal basis for defining a mission, and secondary objectives, which greatly enhance the value of a mission, are presented. All specific missions will consist of both kinds of objectives. Primary objectives, however, should serve as the baseline for cost definition and guiding the use of resources. While it is fully recognized by COMPLEX that all the objectives may not be addressed in any single mission for both technical reasons and limitations in resources, it is our view that a mission of planetary exploration should be directed toward fulfilling the primary objectives and should reflect the order of priority as contained in this report. A mission that addresses the items of highest priority would in our view be of major importance. Secondary objectives are to be considered of lesser priority with regard to spacecraft design and mission definition but should not be viewed as parenthetical. *We reaffirm our earlier* *suggestion that NASA assess the acquisition procedures so that this distinction in objectives is apparent in the choice and management of experiments and Principal Investigators.*

From experience with *mission operations* on previous space missions, we anticipate that there will be even greater demands on data acquisition, processing, and storage; on mission coordination; and on interaction with the spacecraft and scientific experiments. The complex nature of mission operations and the long time scale required to prepare, certify, and transmit routine commands in previous missions indicates that substantial changes will be necessary. We believe that significant technical and managerial advances must be made in anticipation of future planetary missions, in order to provide reliable, more efficient, and lower cost systems for operation of the spacecraft and scientific instruments.

The testing of these systems on the ground as operational units including the participation of science teams should be carried out well before the mission. These tests should include the operation with possible failure modes. These problems will be more important in the future when extensive coordination must be obtained by use of more intelligent or autonomous control systems. The choice of onboard preprocessing versus earth-based processing and the utility of block telemetry formating and distributive data handling and control subsystems will require assessment. In the past, computing facilities and command and data-processing software were not always efficient, and early attention was not given to overall system design in laying out missions. Further, experience with past and current spaceflight missions has shown that complicated systems with higher levels of intelligence are difficult to handle without substantial experience.

We are apprehensive about recommending that radical new approaches be utilized without further study; nonetheless, it appears that some significant changes must be considered. Recognizing that mission operations is the key to the success of any complicated undertaking, *we therefore recommend that an assessment of mission operations, including spacecraft control and scientific instrument and data management and the design and management of software control systems, be studied by the Agency at the earliest possible time and the evaluation be presented to the Committee.*

PRESENT STRATEGY

In developing the strategy and objectives for the inner solar system, the Committee reviewed the current level of investigation for the whole solar system and the degree of progress. On the basis of objectives that have been achieved, we again conclude that the reconnaissance phase is nearly complete. The objectives required to narrow the gaps in our present state of knowledge indicate that the strategy for the decade under consideration should be exploratory in nature.

It is clear that the NASA budget, and to a greater degree some program elements, will be restricted for at least the foreseeable future. *As a consequence, the inner-solar-system strategy that we propose, like its counterpart for the outer solar system, is very conservatively paced and recommends only those goals that are of the highest importance and that can be achieved only by planetary encounter by spacecraft.* In presenting this strategy and its goals, the Committee fully recognized that the planetary exploration enterprise has advanced to a magnitude requiring major national commitment. In the opinion of the Committee, the mission planning to achieve a rational phased sequence to implement the major goals would constitute a minimally viable, coherent program that is compatible with resource projections.

The policy of the Space Science Board, which COMPLEX has supported, is that a program of planetary investigations should be balanced, i.e., that it should move forward on a broad front to all the accessible planetary bodies beginning with reconnaissance, into exploration of selected planets, and lastly to intensive study of a limited number of cases. As the reconnaissance phase is nearing completion, the Committee repeats its recommendation that *for the next decade there should be a shift in emphasis toward systematic exploration with emphasis on selected planets, but with some continuing level of reconnaissance to parts of the solar system where our ignorance is greatest and the opportunity for new discovery is large.*

In view of the budgetary outlook for the near future, however, it may not be possible to maintain the "balance" aspect of the planetary program as it has been traditionally viewed. The shift in emphasis to exploratory investigations, which are more complex and costly, and the foreseeable limitation in resources may require that the efforts in the planetary program be concentrated in selected areas. However, this concentration of efforts should be carried out so that the major scientific advances exhibit continuity and diversity. Over a time period of two decades we believe that planetary exploration should achieve a more general balance. *It remains 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."*

Both the United States and the Soviet Union continue to have interests in a few selected planets where our individual investigations have advanced to the more complicated and costly exploration phase. From the testimony provided to the Committee during its deliberations, it became patently clear that cooperation between the United States and the Soviet Union in planetary exploration has not achieved its potential. However, the desirability for cooperation is apparent, the potential for scientific return is high, and the

Committee strongly endorses a new initiative to achieve cooperation. Toward this end the Committee has provided in the following section a general policy statement on U.S.-U.S.S.R. cooperation, which is implemented later in the report with specific recommendations for initiating cooperation in further exploration of Venus and the Moon.

ROLE OF INTERNATIONAL COOPERATION IN PLANETARY EXPLORATION

In general, the intermediate-range plans for planetary investigation by the United States have been publicly enunciated, and the participation of the scientific community has been solicited. These efforts have involved the participation of scientists on an international basis through the existing national organizations with formal connections with NASA. The degree of participation by the general scientific community for the identification of science goals and the proposal of experiments for inclusion in a specific mission have increased over the years. Those nations that are vigorously involved in planetary missions launched by the United States include Australia, Canada, Denmark, France, the Federal Republic of Germany, Italy, The Netherlands, Sweden, Switzerland, and the United Kingdom. In the specific area of lunar exploration, over 20 nations are now or have been involved in the study of lunar samples returned by the Apollo missions and regularly take part in scientific meetings and publish scientific papers in this field.

The overall effect of international involvement in the planetary-exploration program has been to widen substantially the breadth of scientific knowledge and intellectual exchange. Further, this has vitalized the scientific and technical skills of all the participating nations. The reason for the lack of participation in flight experiments by other nations that have the full scientific and technical skills requisite for making major contributions is not known but presumably is related to their national priorities and interests.

The United States has played a key and leading role in planetary exploration for over a decade. This has resulted from the fact that the United States is one of two nations with a major spacecraft launching capability and because the United States has committed a major effort to planetary investigations. The Soviet Union is at present the only other nation with a major launch capability and with a commitment to carry out planetary investigations as a part of their national goal. The Sputnik spacecraft launched in 1957 by the Soviet Union was the first earth-orbiting space vehicle, initiating the whole enterprise of outer-space investigations. The planetary program carried out by the Soviet Union has been vigorous and continuous and has led to several major scientific and technical achievements that have not always received the recognition in the United States that they deserve. The Soviet Union has established a program of lunar exploration in which the returned lunar samples have been widely available to the international scientific community. Some significant tentative steps toward cooperation between the United States and the Soviet Union in the area of planetary exploration have been taken through the exchange of lunar samples and meteorites obtained separately by the two nations and in exchange of some engineering and scientific data on spacecraft missions to Venus and Mars. However, there has not been a policy on the part of the Soviet Union to enunciate publicly their goals or plans to the general community. Limited participation in scientific payloads in Soviet planetary probes has involved France. In most cases, it has been necessary for the United States in the planning of its own planetary program to judge the objectives and commitments of the Soviet Union in the area of planetary exploration on the basis of previous experience. In the past, no details of scientific objectives or payloads have been made publicly available by the Soviet Union until well after the completion of a mission. The release of substantial data has rarely been made during a mission, and any detailed reports have only been made available long after completion of the mission. In general, there has only been a limited amount of Soviet technical and scientific data available through the normal medium of scientific journals and presentations at meetings. More recently, however, there has been a significant increment in the amount of published scientific information resulting from the Soviet planetary program.

This approach, when coupled with the historically competitive character of space exploration which so intimately reflects national pride, has not permitted either coordination or true cooperation in planetary exploration between the two major launching nations of the world. When viewed in the light of our individual national commitments and remarkable scientific and technological achievements in planetary exploration, we consider the degree of cooperation that has so far been realized to be minimal.

It is our belief that to continue planetary exploration in this manner during the next decade would not yield the optimal scientific return nor the most effective investment of the national resources and scientific talents of both countries. To change this pattern requires appreciation, based on adequate information, of each other's investigations and coordination of U.S.-U.S.S.R. programs in selected areas of mutual interest. In view of the lack of direct and substantial information on Soviet plans for planetary exploration and because of the limited extent of past scientific exchanges, it appears desirable to establish a much improved vehicle for discussion of U.S.-U.S.S.R. interests in planetary exploration. *We recommend that an* *effective mechanism be established by the United States and the Soviet Union for:*

(a) *The establishment of active open working relationships between Soviet and American scientists so as to discuss the scientific problems and to understand the nature and quality of the scientific experiments that might be carried out;*

(b) *The mutual identification of important scientific goals that are of a substantial nature;*

(c) *The early reciprocal communication between the United States and Soviet Union of the specific scientific objectives, both current and planned, in the area of planetary exploration;*

(d) *The establishment of concurrent commitments by both nations to achieve the goals in selected areas of mutual interest utilizing the agreed upon scientific objectives;*

(e) *The coordination of missions to a planet with full disclosure of mission planning and objectives so as to optimize the scientific contributions of both nations;*

(f) *The establishment of true cooperation between both nations through a reciprocal arrangement that allows the incorporation of significant scientific experiments by both parties on the same spacecraft; and*

(g) *The timely communication and exchange of information both during and after completion of a mission.*

Advances in implementing this recommendation will have a significant impact on the long-term objectives of the U.S. and U.S.S.R. strategies for planetary exploration. *We therefore recommend that progress in carrying out the above functions in planetary exploration be reviewed and assessed annually by segments of both governments. We request that the SSB be briefed on the results of the review so that progress in coordination and cooperation may be properly evaluated in the SSB yearly study of the Space Sciences Program.*

In order to implement these recommendations, it will be necessary to develop mechanisms for identifying and fostering long-term bilateral commitments. This is necessary, since a commitment to coordinate or cooperate in achieving science objectives must be made early in the mission definition stage; there is no significant opportunity to do so after mission approval. From the Soviet point of view, for example, the political and budgetary process by which U.S. missions are defined, proposed, and approved or rejected may be a significant barrier to cooperation. Consequently, there is an element of risk for any nation who may wish to cooperate with the United States in space exploration, because of the process by which space missions are defined and approved. Those nations that are cooperating with the United States to date have accepted this risk, since access to the U.S. launch capability has been a key element. The Soviet Union maintains its own launch capability, and the risk is of a different nature. It may be advisable, therefore, at policy-making levels to identify these kinds of barriers and to examine ways in which risks can be minimized and more assurances provided, without circumventing the selection process or overcommitting our national program. A reciprocal posture on the part of the Soviet Union should be expected.

In any attempt to expand the current level of cooperation, basic assurances of a continuing commitment must be perceived in a national space policy. The degree to which our interests and intentions match those of the Soviet Union and other nations will then determine the primary basis for long-range cooperation. In order to provide useful guidance to executive and legislative principals, such a statement of national policy should contain, in our opinion, both broad and specific national goals in space for prescribed time periods and should be able to be used as a basis for negotiation for achieving cooperation.

If positive steps are not taken in the immediate future, both the United States and the Soviet Union will of necessity further commit themselves individually to major long-term planetary-exploration programs without the benefit of either coordination or cooperation. Given the long lead time and development decision points inherent in mission planning, no substantial progress can be expected in improving communications or fostering coordination and cooperation between the United States and the Soviet Union over the period of the decade to which this science strategy is directed unless there is agreement and sustained progress toward implementing the functions recommended above. In such an unfortunate circumstance, we consider that a unilateral planetary program by the United States with appropriate participation by other interested nations should be pursued.

We believe that both nations will maintain their national commitment to planetary exploration for the foreseeable future. Thus it would seem rational to coordinate each nation's efforts so that the results of planetary exploration are complementary and mutually supportive and not unnecessarily redundant nor competitive. It is the purpose of our recommendations to build toward a state of equity between the United States and the Soviet Union in planetary science in particular and space science in general.

Because of the very limited extent of U.S.–U.S.S.R. cooperation in the area of planetary exploration which has so far been achieved, it does not appear reasonable to design an overall science strategy that is intrinsically or fundamentally based on coordination and cooperation. It is the Committee's view that efforts toward coordination and cooperation will have the greatest chance of success by advances in specific and limited areas of mutual interest in which the strategies and objectives of the United States and the Soviet Union overlap and complement each other and where the major national programs are not jeopardized or inhibited.

It is hoped that substantive advances in these limited areas of planetary exploration may lead to more extensive endeavors in the future.

We recommend that scientific planning, mission coordination, and cooperation between the United States and the Soviet Union be directed to those areas of planetary exploration where both nations have a vigorous and sustained effort and when the relative scientific strength and technological capabilities are mutually complementary in order to realize the maximum interest and scientific benefit.

In other sections of this report we will recommend specific areas of planetary exploration that we believe are of strong mutual interest to the United States and the Soviet Union and are possibly suited to coordinated and cooperative endeavors compatible with the recommendation stated above.

LAUNCH CAPABILITIES FOR PLANETARY EXPLORATION

In conjunction with the 1975 report, this report outlines the science strategy for investigating the planets of the solar system over the next ten years. In addition, some of the elements of a longer-term strategy are already apparent (e.g., an exploratory investigation of Saturn and its satellites, a return of a surface sample from Mars, and intensive exploration of the Jovian satellites). On the basis of the 1977-1987 strategy, we can anticipate that much of the first-order reconnaissance will have been completed and that most of the investigations in the 1986-1996 time frame will reach more intensive levels of exploratory studies. The launch capabilities that will be required to carry out such investigations should be anticipated. Consequently, *we recommend that NASA adopt a policy that places the planning and development of launch capabilities for unmanned space exploration in a 10- to 20-year perspective and that focuses on the requirements of our long-term objectives.* We believe that vehicles developed under this policy should be able to (1) reach all the major objects of the solar system ranging from Mercury to Uranus, (2) achieve the mission profiles and the transport and payload masses required for the appropriate level of investigation, and (3) launch without the excessive constraints of very rare celestial mechanical circumstances. Further, such vehicles should exhibit sufficient flexibility in performance so that the range of capabilities required to support a diversity of missions is maintained, while minimizing the number of types of launch vehicles.

The missions required to fulfill the strategy for the next decade recommended in this report and in the Committee's 1975 report are dependent on an adequate launch capability. We have previously stated that the planetary program has progressed to the exploratory stage of investigation, and as a result many planetary missions currently approved or under consideration will require a launch capability at least as great as that of the Titan Centaur system currently available but due to be discontinued after 1977. For both the inner planets and the outer planets, an increase in capability may be necessary to carry out missions that effectively implement the recommended science objectives.

The Space Shuttle transportation system is scheduled to become operational during the latter half of the decade (1977–1987) to which this solar-system strategy is directed. The Shuttle will be primarily used for earth-orbiting missions, and consequently the basic system must be supplemented with an upper stage to launch missions out of earth orbit. The Shuttle Interim Upper Stage (IUS) is under development and is nominally scheduled to begin its operational stage in mid-1980. Any significant delay in the development of the IUS or of the development of the Shuttle orbiter would introduce a major hiatus into the program of planetary exploration, which has already been subject to serious interruption. The Space Science Board previously recognized the debilitating effects that could occur during this transition phase. Development delays with no alternative capability would cause, at a minimum, lengthy postponement of high-energy missions in the early 1980's; e.g., an orbiter with probe mission to Jupiter proposed for launch in late 1981 would not be possible again until about 1988 because of celestial mechanical constraints. Further, the continuation of the program as proposed was vitally dependent on the choice of IUS and its performance capability for planetary exploration. For these reasons the Board was obliged to recommend in 1975 that NASA maintain a direct launch capability fully adequate for relevant planetary missions until the development of the Shuttle IUS was complete. Given the budgetary outlook for the near term, NASA felt it could not justify the expenditure of funds to maintain the capability for the three-year interim (mid-1977 to mid-1980), since it would jeopardize new starts to be considered during this period. Consequently, the direct launch capability for missions requiring high-energy performance (Titan/Centaur) is currently being dismantled, and future missions requiring this capability are now dependent on the availability of the multistage IUS. This will result in a three-year hiatus starting in 1977 for any aspects of the planetary program that require a high-energy capability. Considering the possible consequences of development delays, *we recommend that every effort be made to keep development of the Shuttle orbiter and a high-energy IUS on its nominal schedule.* We request that the Board be kept informed of development progress so that any delays and their effect on science strategy and mission planning can be assessed in a timely manner. In particular, we advise that careful attention be paid to the actual IUS capability as the development program advances.

The Committee is deeply concerned that there be a commitment to maintain a sufficient launch capability if the planetary program and deep-space exploration in general are to continue. In the 1976 SSB report it was recommended *"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 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 indicated curve in Figure 1 illustrates the performance capability of present systems and is the *minimum* necessary to carry out the exploration of the solar system as recommended by the Space Science Board (1976) without consideration of those missions that require low thrust and high payload. The three performance points in the 1976 recommendations are also indi-

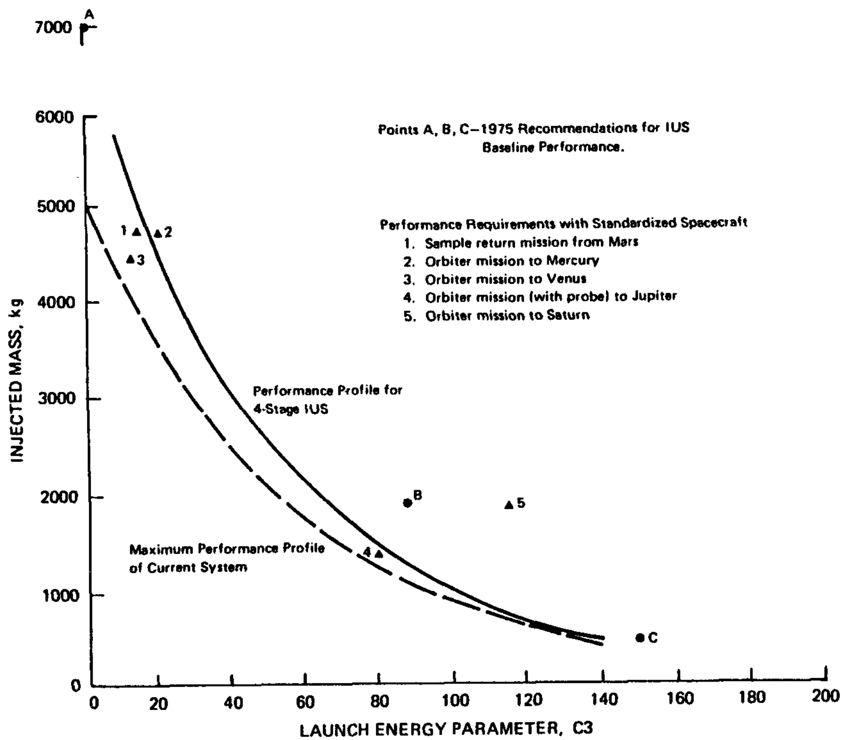

FIGURE 1 Performance profile for current and IUS launch systems.

cated (A, B, C) and were intended to represent broadly the preferred performance baseline of an IUS to maintain the planetary exploration program and carry out the strategy objectives for the next decade without being overly restricted by celestial mechanical constraints. These points were based on estimates of instrument payload and the use of currently available spacecraft, i.e., Mariner- and Pioneer-class spacecraft.

After study and deliberation on the possible multistage use of an IUS, NASA has decided to use a modular concept. For deep-space planetary missions this concept will allow some flexibility in adjusting the launch capability required for a specific mission by appropriate selection of propulsion stages and staging options. For comparison purposes, the performance capability of the four-stage IUS as requested by NASA is also shown in Figure 1. The IUS clearly provides a superior capability in high-injected-mass/low-escape-energy regions, but in low-injected-mass/high-energy regions the IUS is virtually the same as the present system capability. The proposed IUS profile is clearly less than a profile that would include the recommended performance indicated by Point B, which would allow major exploration of the Jovian system by an orbiter without being heavily restricted by celestial mechanical constraints and which lies in the region that would further allow exploration of the Saturnian system with an orbiter and probe.

In the 1975 SSB report, *the Committee endorsed the ongoing efforts by NASA to reduce the weight and cost of standardized available spacecraft without a corresponding reduction in capability using new developments in technology*. The Committee was informed that NASA studies have shown that it is possible to utilize a new generation of standardized spacecraft for planetary exploration which included a spin-stabilized platform and a de-spun platform. For a Jupiter orbiter-probe mission, this spacecraft can thus accommodate both an atmospheric probe and an orbiter with significant capability for exploring the Jovian system at lower injected mass and launch energy levels, if a favorable launch opportunity is used. This spacecraft was estimated to have a significant decrease in weight relative to the Mariner spacecraft so that access to the Jovian system with an orbiter alone may be possible at every launch opportunity (see Point B, and Point 4, Figure 1). Such a spacecraft could have wide applicability in planetary exploration. However, a more recent report indicates that this anticipated decrease in spacecraft weight may not be achieved, thereby restricting the accessibility of the Jovian system and other possible planetary objectives. Because of the importance of placing larger payloads in planetary orbit, the commitment to a spacecraft design that is to be utilized for a prolonged time period during the Shuttle era will be a matter of substantial interest and concern to this Committee. We therefore request that NASA representatives regularly brief

COMPLEX on the state of these spacecraft developments and the implications of these developments for ongoing and future planetary exploration.

In considering the exploration of both the inner and outer solar system, we note that many of the major objectives are highly constrained by ballistic trajectories. As shown in Figure 1, the performance requirements for planetary exploration using the proposed new standardized spacecraft lie close to the estimated maximum profile of the IUS/Shuttle; this can be seen in the case of exploration of Jupiter and its satellites as discussed above. A similar situation exists for a Mars sample return mission, which will restrict the size of the recovered sample to approximately 1 kg. Orbiter missions to Mercury and Saturn and low-velocity comet encounters require propulsion capabilities that are above the maximum provided by the IUS. Use of the currently proposed IUS would thus demand strict limitation in weight of the payload and continue to impose strict constraints on orbit parameters and launch opportunities.

We conclude that many of the high-energy missions to carry out the strategy for investigating the solar system during the period 1977-1986, and in the early portions of the subsequent decade, will remain limited by launch opportunities, flight times, and other celestial mechanical constraints. Alternative propulsion systems, solar electric propulsion (SEP) and solar sailing (SS), have been under consideration as supplements to the modular IUS; both are classified as low-thrust systems and could offer substantial increases in available energy, apparently are unencumbered by launch window constraints imposed on ballistic techniques, and in some cases could substantially reduce the mission flight time. Such characteristics could enhance the recommended solar-system strategy by introducing a degree of flexibility into mission planning that has heretofore been absent. The capability of such systems to deliver an injected mass of approximately 7000 kg at a low C3 level and their availability beginning in the mid-1980's will be critical in the exploration of comets and asteroids, orbiter missions to Mercury and Saturn, and a Mars sample return. A comparative assessment of these two techniques, based on the concept and feasibility of system design, capabilities, and cost effectiveness in terms of specific scientific objectives and expected lifetimes of the systems and the relative advantages of low-thrust techniques and ballistic techniques to the same planetary targets has been carried out. The SEP has been selected to provide a low-thrust/high-payload mass propulsion capability for continued planetary exploration. The Committee is informed that feasibility studies have been carried out for a variety of SEP designs and capabilities. *We recommend that a comparative assessment be carried out of SEP design options in terms of feasibility, cost-effectiveness, and ability to meet the scientific objectives of* *solar-system exploration over the next two decades. This assessment should take into account currently stated objectives, and major emphasis should be given to the search for currently undiscovered exploration opportunities that may become available as a result of new propulsion capabilities.*

The design of spacecraft subsystems that can be common to several planetary missions has potential for cost savings and is in accord with the Committee's previous endorsement of a NASA effort to reduce the weight and cost of standardized available spacecraft with a corresponding increase in capability using new developments in technology. Recent NASA studies of a spinning spacecraft with a stabilized de-spun platform and with subsystem commonality, especially in electronic and data-processing subsystems, are highly commendable, and we urge that they be continued. We caution, however, that the economic advantages attributed to subsystem commonality should not predetermine the capability to accommodate diverse scientific and instrumental payloads for future missions. Flexibility in mission profiles and in the choice of potential scientific instruments must remain the key factor in any effort to standardize subsystems.

EARTH-BASED AND EARTH-ORBITAL OBSERVATIONS

To conduct an efficient program of planetary study with limited resources, in addition to spacecraft probes, it is essential to integrate carefully a wide variety of techniques to most effectively obtain fundamental information about the solar system. These techniques include ground-based optical and radio telescope and radar observations, high-altitude aircraft with specialized equipment, and the use of near-earth orbital telescopes with instruments capable of solar-system measurements. Ground-based and near-earth orbital techniques are especially valuable since the results are likely to be available very promptly and the costs of individual observations tend to be low. 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.

A rapid evolution in the capabilities of ground-based observers to obtain planetary observations has resulted from the modest but continuous support that NASA has provided. The recent discovery of rings around Uranus is a clear example of the fundamental contributions to planetary studies that can be made by earth-based and earth-orbiting observatories. The presence of the rings must now be a major factor in developing future objectives for the investigation of Uranus from earth, near earth, and with deep-space probes. Further, in recent years high-altitude aircraft have allowed observations of planets in some of the spectral regions where the lower atmosphere is opaque. The resulting discovery of water vapor by high-resolution infrared spectroscopy of the Jovian "hot spots" seen near 5 μm is a specific example of the critical value of such efforts. This discovery has been a significant factor in the design of the atmospheric probe for the proposed Jupiter orbiter-probe mission.

Another ground-based technique that is still developing rapidly, and that has the capability for yielding new results of considerable importance, is radar. Well proven in the investigation of the inner solar system, radar is now able to reach out effectively to the Galilean satellites of Jupiter, to the rings of Saturn, and to a number of asteroids. It is also capable of imaging significant portions of the surface of Venus, with a resolution of about 5 km, and substantial portions of the surface of Mercury with somewhat lower resolution. The possibility should be considered of increasing the capability of ground-based radar instrumentation to extend topographic mapping over larger portions of the surface of Venus, with a contour accuracy of about 200 m, and over a significant fraction of the surface of Mercury.

In the 1975 report, *the Committee recommended that the adequacy of NASA support for the earth-based optical and radar programs, including aircraft and balloons for planetary observations, be reviewed regularly. The Committee was unable to conduct this review during its 1977 schedule but now recommends that this review be conducted no later than November 1978.*

On the basis of achievements in the earth-based program, it is vitally important that the greater capabilities of the proposed Space Telescope also be appropriately utilized for solar-system studies from earth orbit, where atmospheric attenuation and instabilities are not an inhibiting factor. The order-of-magnitude improvement in spatial resolution available by use of the Space Telescope (ST), as well as access to wavelength regions unavailable from ground-based observatories, led us last year to "*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.*" COMPLEX wishes to reaffirm this view and *to recommend further that the scheduling of ST observations be based on the potential scientific return from each proposed observational program with due regard for planetary mission planning and that the group charged with allocating telescope time be representative of the diverse interests of potential users.*

It is also very important that the first-generation instruments with fully adequate detectors on ST have wide observational capabilities, so as to service the diverse needs of deep-space and planetary observers. COMPLEX commends NASA for recognizing this need and for encouraging such diversity in the science instruments. Observational constraints such as sun-spacecraft and moon-spacecraft angles should be minimized to allow maximum scientific returns.

The following is a list of the highest priority measurements and their accuracies required for planetary observations from ST. These recommendations are made with full cognizance of the recommended objectives for deep-space exploration by spacecraft.

1. Synoptic high-resolution imaging of the Jovian planets in the ultraviolet and near infrared regions. The spatial distribution of several important atmospheric molecules with absorption bands in these spectral regions as well as the detailed cloud structure are critical to understanding the dynamics and chemistry of these atmospheres. A wide-band (1200 Å < λ < 11000 Å) camera with subdiffraction-limited pixel sizes and high (≤1 percent) photometric integrity capable of imaging substantial areas of the Jovian planets in a single exposure through a few selected filters would be adequate.
2. Measurement of the equivalent width of selected CH_4 and NH_3 absorption bands as a function of center-to-limb distance (i.e., airmass) is the only technique available in the near term to determine the vertical atmospheric structure of Saturn and Uranus. These same data, collected from Jupiter over a long time base, will allow better utilization of the direct measurements from the atmospheric probe proposed for a Jupiter orbiter mission. These measurements are dependent on a spectral resolution of about $\lambda/\Delta\lambda = 10^3$ and spectral response to at least 8000 Å in the proposed Faint Object Spectrograph.
3. High-resolution synoptic imaging of Venus to continue the study of the ultraviolet clouds and their complex motions, imaging of Mars for selected periods during the development of dust storms and major seasonal changes, imaging of cometary nuclei and high-spatial-resolution spectral studies of the molecular emission in the coma and tail of comets will require use of the same instruments described above. Imaging of comets with the Wide Field Camera on ST at Lyman α (λ = 1216 Å) is important, since the hydrogen seen in emission is thought to be the result of photolysis of H_2O boiled off the nucleus. Water ice is assumed to be the major constituent of comets, and the determination of the mass wastage rates is basic to understanding the history and evolution of these most primitive samples of the solar system.

The highest priority development for a second-generation instrument would be an infrared interferometric spectrometer to study planetary atmospheres, comets, and solid surfaces, at much higher spectral resolution. Since instruments of this nature are already being developed, it should not require major resources to adapt such an instrument to ST.

In summary, we strongly endorse the ongoing NASA support for ground-based optical, radio, and radar planetary studies, and for rocket, balloon, and high-altitude aircraft research on solar-system problems, and we look forward to the critically important data on planetary bodies that will be available from ST.

PLANETARY QUARANTINE STANDARDS

This report and the 1975 SSB report present a strategy for exploration of the inner and outer planets through 1987. The principal elements of this unified strategy and the primary scientific objectives to be achieved reflect the Committee's earlier recommendation that there should be a shift in emphasis toward systematic exploration of selected planets since the reconnaissance level of planetary investigation is virtually complete for the inner solar system. During this earlier reconnaissance period and up to the present time, the United States has imposed a rigorous set of standards to guarantee with a large factor of confidence that U.S. spacecraft and their payloads would not contaminate the atmosphere and surface of other planets with earth organisms onboard. Similar quarantine standards were established to prevent backcontamination of the earth from the first lunar samples. The conservative nature of this policy indicated U.S. intent to abide by the terms of the Outer Space Treaty, Article IX, and the standards established by the international Committee on Space Research (COSPAR). COSPAR acts as an outside advisor in these matters to the United Nations and monitors the practices of the launching nations to maintain the standards. During the early reconnaissance period when our ignorance of the environments of planetary surfaces and atmospheres was large, there was serious concern that contamination of other planets by earth organisms might preclude our eventual understanding of the nature and evolution of planets; further, the effect on lunar samples and their contents in an earth environment was unknown. In reflecting these early concerns, Article IX of the treaty stated that "State Parties to the Treaty shall pursue studies of outer space, including the Moon and other celestial bodies, and conduct exploration of them so as to avoid their harmful contamination and also adverse changes in the environment of the Earth resulting from the introduction of extraterrestrial matter and, where necessary, shall adopt appropriate measures for this purpose." It was recognized, however, in spite of its conservative bent, that this policy was not intended to inhibit or penalize scientific efforts to observe and understand the planets and that some level of risk needed to be formulated and accepted if exploration were to advance.

Even in early reconnaissance stages, planetary missions need to be justified with assurances that major scientific advances will be achieved. Consequently, the performance requirements of spacecraft instrumentation to implement scientific objectives of ever-growing complexity are constantly being pushed to the limits of current or anticipated technology. Typically, these requirements have been specified early in the mission development stage where actual performance after imposing quarantine standards is virtually unknown. As a result, there have been cases for which *pro forma* compliance with standards has placed severe development difficulties on science instrumentation and jeopardized objectives. A similar case can be made for quarantine and handling of returned samples.

Clearly, it is not intended to extend this conservative policy for unreasonable periods of time. The COSPAR designated period of biological interest for any planet is the first 20 years of spacecraft exploration. As knowledge is acquired, the basis for quarantine standards should be regularly reviewed with the intent of providing future missions with relief from taxing individual standards and where advisable shortening the period of biological interest. The review should be applied to standards for both outbound and inbound missions.

The recommended shift toward systematic exploration acknowledges our general advance in knowledge of the planets. Clearly, as a result of our increased knowledge the probability of contamination of planets by earth organisms is markedly decreased. Further, a strategy with primary scientific objectives now exists for the next 10 years, and objectives for the subsequent 10 years can be anticipated. Consequently, the outlines for planetary exploration now extend beyond the COSPAR 20-year period of biological interest for all the solar-system planets from Mercury to Neptune. *The Committee therefore recommends that NASA conduct an overall review of its planetary quarantine policy and the policy for return and handling of samples from other planets to the earth in the light of our knowledge of these planets and in consonance with the terms of Article IX of the Outer Space Treaty.*

4
The Inner Planets

SUMMARY OF ACCOMPLISHED MISSIONS

Earth-based, near-earth, and spacecraft observations of the inner planets have provided a steady flow of information from which our present state of knowledge is derived and the areas of our ignorance are recognized. In the wake of our persistent efforts to understand the solar system stands a remarkable record of achievement. This can be readily seen by comparing recent textbooks in astronomy with those of a decade ago. The earlier speculation and modest level of knowledge about the planets is replaced by substantial information that clearly shows the rich diversity of the planets in the inner solar system.

Prior to Mariner 10 in 1974-1975, astronomical and radar observation of Mercury determined the radius, mass, density, and rotation rate. Some of the data obtained suggested that its surface might possibly be similar to that of the moon. Mariner 10 was the first reconnaissance mission to Mercury and provided the first close-up view of that planet; it demonstrated some similarities with the moon, showing a well-preserved bombardment history followed by emplacement of extensive smooth plains, and also showed that Mercury has a strong magnetic field that is probably intrinsic. However, the interior structure of Mercury is thought to be grossly different from that of the moon. While the Mariner 10 multiple passes were unable to view the dark side of the planet, sufficient information has been obtained to conclude that subsequent investigations will require encounters of longer duration.

Venus has been visited by three U.S. flyby missions: Mariner 2 (1965), Mariner 5 (1967), and Mariner 10 (1974). These missions provided the first close images of Venus and exposed the general patterns of atmospheric dynamics in the dense global cloud cover. Data on the upper-atmosphere composition, the ionosphere, and an accurate estimate of the mass of the planet were obtained. Earth-based U.S. radar observations have provided most of the limited information on the surface of Venus. In 1961-1962 these observations determined the retrograde rotation period and the mean equatorial radius. In recent years, radar maps of small portions of Venus have revealed many large topographic features with resolutions approaching 5 km. The Soviet Union has sent missions to Venus at nearly every opportunity since 1960. Five Soviet Venera probes into the hot, dense atmosphere have measured the pressure-temperature profile and sampled the atmospheric composition. Venera 8 survived sufficiently long enough to obtain limited chemical data. In 1975, the Soviet Union placed two large highly insulated soft landers, Venera 9 and 10, on the surface. Photographs of the near vicinity of the landing site were successfully retrieved, and measurements of the atmospheric composition were attempted and some surface properties determined in this hostile environment. Other investigations of Venus by the United States will be carried out in 1978 by the Pioneer-Venus mission, which will conduct longer-duration studies of the atmosphere and ionosphere from orbit coupled with multiple atmospheric probes and should provide the first precise measurements of the composition of the Venusian atmosphere and some basic observations on its atmospheric dynamics. Subsequent investigations of Venus should build upon these major advances and recognize the complementary capabilities of the U.S. and U.S.S.R. spacecraft and instrumentation.

The gross character of Mars including polar caps and seasonal changes has been long known from earth-based telescopic observations. The major scientific advances were obtained from spacecraft investigation of Mars, which began with the Mariner 4 flyby in 1964 and continued with Mariner 6 (1969), Mariner 7 (1969), and the orbiter Mariner 9 (1971) up to the completion of the nominal and extended mission phases of the Viking landers and orbiters (1978). With the flow of analytical information and with improved resolution, for both imaging and spectral observations, Mars provided a series of startling surprises from each successive mission and has led to some new basic conclusions and new questions. Most of what we now know of Mars has come from the Mariner 9 and Viking missions, which have shown the planet as extremely diverse in its character, having undergone bombardment like the other terrestrial planets but with distinctive earthlike volcanism and clear and surprising evidence of large-scale fluvial erosion. The precise atmospheric composition and structure, which were the subject of speculation for many years, are now basically known. The absence or low level of hydrocarbons in Martian surface soil in at least two locations is established, and preliminary characteristics of the soil chemistry are identified. Efforts by the Soviet Union to explore Mars with the missions Mars 4, 5, 6, and 7 in 1973 were unsuccessful.

The enormous scientific returns from the Apollo missions have advanced lunar studies from the reconnaissance levels achieved by Ranger and Surveyor missions into the intensive exploration phase. The availability of lunar samples has moved some elements of investigation into the in-depth study phase. Early manned missions, however, have bypassed some fundamental exploratory-stage objectives. At present, there is only sparse information on the nature of the distinctive back side of the moon and the lunar poles. U.S.S.R. interest in lunar exploration is continuing, as evidenced by the most recent successful completion (Luna 24) in its series of unmanned sample-return missions.

In keeping with the terms of the Space Act, the United States made the results of space missions available to all interested investigators in any nation and has encouraged and invited other nations to participate in the planetary program at a level appropriate to their national interest and resources. The level and scientific content of U.S. cooperative programs has grown steadily over the past decade to the point where major non-U.S. experiments to be launched in U.S. spacecraft and complete spacecraft to be developed and built outside the United States are being seriously discussed for deep-space investigations. Cooperation with the Soviet Union has been limited; however, it has provided for the exchange of lunar samples and the release of mission results over a relatively long period of time.

PRESENT STATE OF KNOWLEDGE

The planetary bodies of the inner solar system are Mercury, Venus, earth and the moon, Mars and its satellites Phobos and Deimos, and numerous asteroids. These bodies orbit at distances ranging from about 0.4 AU to about 4 AU from the sun, with orbital periods ranging from about one-quarter year to about eight years. Mercury—the planet closest to the sun—resides so deeply within the pull of the sun's gravity that departures from the Newtonian theory of gravitation are apparent in its motion. Of the four planets, only earth possesses a major moon of planetary dimensions. The moons of Mars are small bodies; Venus and Mercury have no moons orbiting them.

In a broad sense, the compositions of the five major bodies of the inner solar system—the four planets plus earth's moon—are similar in that they are composed largely of rocky and metallic substances. However, systematic differences in composition are reflected in variations in the mean densities of the planets. The densities range from the 5.5 g cm^{-3} density of Mercury to the 3.9 g cm^{-3} density of Mars. Broadly speaking, the densities of the inner planets decline as a function of distance from the sun. But the reversed positions of the earth and Venus in the sequence and the low density of earth's moon and the very low density of the Martian satellite Phobos stand as a provocation to our simplest accountings for this sequential arrangement.

Meteorites, many of which may originate in the asteroid belt, appear to be relics of the very earliest accretion processes in the solar system and show the effects of early planetary differentiation stages. The results of early planetary differentiation and volcanism are still evident on the moon's surface, which is older than most of the rocks known on earth. The moon has preserved some rocks formed during its early melting near 4.5 aeons, and giant impact basins were subject to frequent volcanic flooding between 3.9 aeons and the last significant flows at about 3.0 aeons. The most ancient rocks on the earth are about 3.6 aeons, and most of the surface of the earth is covered with lavas only 0.1 aeon old. A record of the meteor bombardment history of the earth-moon system is preserved on the surface of the moon, which shows that an intense late bombardment took place around 3.9 aeons, which made many of the large impact basins and which may have triggered the subsequent volcanism. The intense late bombardment history of the moon has been used as a reference. It is inferred that all the planets of the inner solar system underwent a similar late bombardment, the evidence of which is still well preserved on Mercury. A more limited record is visible on Mars, where erosion and atmospheric and tectonic processes have played a major role in shaping the surface. Some record may possibly be present on Venus as indicated by the limited observational data.

From extensive chemical and mineralogical data, earth and its large satellite moon are known to have distinct chemical compositions, and it appears that they cannot have been produced by the simple fission of a single parent body. Each of the volatile elements appears to have been largely lost during the moon's formation. This absence of volatiles on the moon may possibly have taken place during formation of the earth-moon system from a common source. There are no substantial data on the chemical compositions of the other terrestrial planets. However, the atmospheres of earth, Venus, and Mars are now known sufficiently to permit a comparison.

The widest–and, from an anthropocentric viewpoint, the most apparent–range of variation among the inner planets occurs in the characters of their atmospheres. Mercury and the moon are airless bodies; each is devoid of any stable atmosphere. Venus's thick, hot, CO_2 atmosphere is nearly 100 times denser than earth's; Mars's tenuous CO_2 atmosphere has only 1/100 the density of earth's atmosphere. Venus and earth have similar abundances of N_2 with respect to the planetary masses, while Mars appears to be highly depleted in N_2. Venus is covered by a dense blanket of clouds, which may be composed, in part, of sulfuric acid. The motions of these clouds indicate the presence of global-scale winds possibly driven by a diurnal cycle of insolation that is much longer than earth's. Mars is known to have episodes of high-velocity winds, which raise dust storms of sufficient size to shroud large parts of the planet's surface. The waxing and waning of Mars's carbon dioxide polar caps indicate that the atmosphere cycles CO_2 between high and low latitudes on a seasonal basis. The "permanent" Martian polar caps are now known to be composed of a layer of H_2O ice, which appears to be a feature of long duration. Among the planets, earth is conspicuous for the copious quantities of free water on its surface and in its atmosphere. Neither of the other two "air"-carrying planets shows any significant abundance of free water, although water is present at the Martian polar caps, and its vapor is an essential component of Mars's atmospheric chemistry. There is good reason to believe that water must have been a substantial component in the original composition of Mars and Venus. Mars is subject to severe dust storms, and wind is a dominant mechanism of erosion and deposition on that planet. However, Mars carries remarkable evidence of ancient large-scale flooding and possible glaciation. The history of this Martian fluvial activity is one of the major puzzles posed by recent solar-system exploration. It is highly plausible that significant amounts of volatiles on Mars may still be trapped as a frozen layer at substantial depths below the present Martian surface. The episodic, or at least temporary, nature of the fluvial activity on Mars, as well as the evidence for active glaciation, raises a fundamental question as to the long-term stability of the atmospheric-climatic conditions on the terrestrial planets, including earth.

So far as we know, only earth's surface and atmosphere offer an environment sufficiently benign to have allowed the development of life forms. In this way earth stands remarkably apart. The surface and atmosphere of Venus are apparently much too hot to permit the survival of living organisms. Mars has commonly been thought to be the next most likely location for the development of life in the solar system. However, the absence of detectable quantities of organic materials and the ambiguous results of attempts to detect metabolic processes suggest that living organisms are absent from the surface of Mars. This conclusion is consistent with the apparent fact that the Martian surface environment is hostile to organic molecules. The recent discovery that Mars may have had large quantities of flowing water on its surface suggests that the Martian environment may not have always been hostile to the development of life and raises the possibility that evidence for previously existing organisms may be preserved in the diverse Martian terrains. The surfaces and atmospheres of Venus, earth, and Mars have evolved along widely divergent paths. The Committee is profoundly impressed that the magnitude of the separation between these different paths seems large in comparison with the differences in initial conditions of formation as currently understood. Elucidation of this phenomenon is an important challenge facing our attempt to grasp the past evolution and present behavior and probable future development of our environment and must require extended study by a multidisciplinary approach in the broadest sense.

The earth is, of course, the most completely studied of the inner planets. What we know about earth stands as a benchmark against which to measure our knowledge of the other terrestrial planets. Major advances in our understanding of the processes shaping earth and its atmosphere have been made in the last 20 years. Many of these advances have been made almost concurrently with advances in our understanding of other planets. Some of these discoveries came from viewing the earth from the perspective of our own exploratory spacecraft. In some cases, discoveries on other planets have led to the definition and solution of basic problems of earth's processes. We now know that earth's surface is in a state of ongoing dynamic evolution, with crustal material continually being created and destroyed. The construction of mountain chains, and chains of volcanoes, is the result of tectonic activity, the overturning motions of earth's mantle and the sliding and colliding of global-scale plates of lithospheric material. From a comparison with the other planets, we deduce that these phenomena may be connected with the presence of a persistent water ocean on the earth. The two airless planets–the moon and Mercury–exhibit no evidence of continuing dynamic tectonic activity or of ancient mountain chains, their surfaces being the nearly changeless remnants of ancient volcanism and meteor bombardment and their interiors the relics of early planetary differentiation. Beneath its tenuous atmosphere, the surface of Mars shows evidence for continual evolution but at a rate far below that of earth's. Moon, Mercury, and probably Mars have large topographic depressions produced by major impacts that were early flooded with sheets of lava. Mars has enormous shield volcanoes and possibly some large-scale flooding by sheets of lava as occurs on earth, but it does not exhibit the topographic contrast between ocean basins and continents and the mountain chains as found on earth. However, there is clear evidence for the presence of abundant water and volatiles on Mars and for widely distributed sediments. It is thus possible that Mars was once covered by large areas of water-laid sediments and may represent an arrested stage of planetary evolution that precedes the full development of oceanic and continental crustal segments as found on earth. Venus's surface, veiled by its dense atmosphere and shielded from most meteor bombardment, remains in large part an enigma. However, low-resolution radar images suggest the presence of large-scale crustal features, and apparently fresh fragments of rocks cover the surface. The nature of chemical weathering on the surface of Venus is as yet completely unknown.

The benign conditions at the earth's surface support life forms that contributed in a major manner to the chemistry of the surface and the atmosphere. The metabolic activity of living organisms is an intrinsic part of the cycle of erosion and deposition on the earth and in the generation and transport of certain chemical compounds. This interaction between the biosphere and the macroscopic inorganic environment of the earth must be manifest in differences between the earth and the atmospheric composition and weathering processes of the other planets that are yet to be explored.

Earth is known to be differentiated compositionally and configured in four major layers. A small solid core at the center, surrounded by a thick liquid metallic layer, fills earth to about half of its radius. Most of the rest consists of the plastically flowing rock of the mantle, which is overlain by a thin veneer of crustal and erosional debris. From seismic refraction results, the moon is also known to have a distinct crust and mantle. For Mars there is indirect evidence for a dense core, from the low moment of inertia, and for a low-density crust, from the isostatic compensation of surface topography. By analogy, and theoretical reasoning, we tend to think of the other planets as similarly differentiated and configured. But no direct and definitive evidence is in hand for any of the other terrestrial bodies.

The convective overturning motion of earth's fluid core interacts with earth's rotation to produce the strongest planetary magnetic field in the inner solar system. Earth's magnetic influence extends through a volume of space many times the size of the planet itself and forms an umbrella shielding earth from the flowing interplanetary plasma. Of the other inner planets only Mercury has a comparable magnetosphere, though even it is vastly reduced in scale, being hardly larger than the planet itself. Venus, Mars, and the moon show tantalizing hints of magnetic fields. In the case of the moon, the magnetic fields are the result of residual permanent magnetism in the rocks, left by a magnetizing source of unknown origin early in the history of the solar system. Strong local variability is found in the lunar magnetic field, which appears to be correlated with certain craters and is not yet understood. The existence of magnetic fields on Venus and Mars is still a subject of speculation that cannot be resolved with the existing observations. The extended magnetic fields surrounding earth and Mercury are in a state of restless agitation, which results in the annihilation of magnetic energy and its conversion to heat and the kinetic energy of energetic particles. These processes are believed to be similar, on a small scale, to explosive phenomena that occur much more spectacularly on the sun and in exotic astronomical objects.

The comparisons that we have been able to make among the planets, and the conclusions that we have been able to draw about their gross similarities and differences, are largely the result of spacecraft studies carried on over the last decade and a half. These researches have focused our attention on a new generation of pressing and fundamental questions, which challenge our understanding of the origin and evolution of the planets. This challenge in conjunction with our present and potential technical skills in space exploration places us in a position to carry forward new exploratory and intensive investigations required to address these important issues.

STRATEGY FOR STUDY OF INNER SOLAR SYSTEM: 1977-1987

As a statement of basic policy, the Space Science Board has defined the primary goals for investigation of the solar system. The primary goals 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.

In implementing the means for achieving these goals, the Board recognized that the levels of investigation could be divided into three categories: reconnaissance, exploration, and intensive study. It was recognized that the sequences of investigations should in general follow the order of reconnaissance, exploration, and intensive study. As an initial consideration for developing the present strategy statement, the Committee made a general assessment of the present level of solar-system investigation. It is clear that we are in a transition period in the levels of investigation of different planetary bodies. However, in general as a result of the successful reconnaissance program carried out by the United States, *the Committee recommends that there should be a shift in emphasis toward systematic exploration of selected planets. The Committee also believes that some level of reconnaissance should be continued in accessible regions where our ignorance is greatest and where the opportunity of new discovery is large.* These considerations with regard to the outer solar system led COMPLEX to recommend in its 1975 report that in-depth exploratory investigations of Jupiter and its satellites and reconnaissance of Uranus be the major objectives of an outer-solar-system strategy for the period 1975-1985; and further, that advanced planning should be carried out adequate to initiate the exploration of Saturn and its satellites subsequent to the return of some basic data from the reconnaissance mission. On the basis of the review of the present knowledge of the inner solar system, the Committee considers that the reconnaissance mode of study is now completed and that a new generation of studies should begin. The gross characteristics of the major bodies of the inner solar system are now known to a sufficient degree to justify the development of exploratory objectives that seek the systematic discovery and understanding of the processes, history, and evolution of the planets of the inner solar system on both the global and local scales. Investigations of the minor bodies in the inner solar system and of comets, however, remain at the early reconnaissance level. COMPLEX considers these objects to be of major scientific importance, and specific recommendations for the minor bodies and comets will be addressed in a separate report.

From intensive review of the state of knowledge of the terrestrial planets and in light of the discoveries recently made, it appears that further basic information on the inner solar system will lead to a major increase in understanding of planetary formation and evolution. Such advances are in accord with the primary goals outlined by the SSB and *the Committee concludes that exploration of the inner solar system will be an undertaking of major scientific importance well beyond the next decade.*

Within the framework of a constrained budget, we have developed a conservatively paced strategy for planetary exploration in the inner solar system for the next decade in which we have identified limited goals of the highest importance that should be achieved within this time period and that can only be obtained with planetary encounters by spacecraft. In consideration of these constraints, *we have identified selected planets as the principal targets for investigation over the next decade in order adequately to fulfill the primary scientific goals enunciated by the Board and guarantee scientific advances in these areas rather than attempt to bring forward to a uniform level of exploration all the objects in the inner solar system.* In this consideration, both the scientific potential and technical and propulsion capabilities were used as a guide in determining which planets should be of highest interest. *On a time scale of two decades it is our view that the general level of exploration for all the planets of the inner solar system should be brought into balance.*

In determining objectives on an intermediate time scale, accessibility is a primary initial consideration. From a launch vehicle viewpoint, Mars and its satellites, Venus, and the moon are all accessible, and exploratory studies are feasible with the present launch capability or the capabilities now under development. By contrast, exploratory objectives for Mercury are contingent on a commitment for and development of an adequate low-thrust propulsion capability.

Observations of the surface morphology and the broad-scale properties of a planet have consistently proven to be a source of important discoveries and a sound conceptual basis for all other aspects of exploration. As a broad exploration objective, *the Committee has concluded that observation and measurement of the morphologic, physical, and chemical character of Mars, Venus, Mercury, and the moon on a global scale are of high general scientific importance and are basic to all planetological studies.*

In comparing the planets of the inner solar system, it is apparent that all of them show evidence of chemical and physical differentiation. This differentiation has produced layering in the solid or molten interiors and in the case of earth, Venus, and Mars has also produced substantial atmospheres. Our knowledge of atmosphere-free planets is based on studies of the moon and Mercury. It is inferred that the moon and possibly Mercury never had sufficient volatiles to produce an atmosphere. The atmosphere-free planets appear to have undergone magmatism and volcanism over the first 30 percent of solar-system history and have subsequently been dormant, their surfaces modified mainly by meteor bombardment.

In contrast, the planets with atmospheres have had a more dynamic history, which involves vigorous interaction between the solid planet and its atmosphere. For the earth, there is a major effect due to the contributions of living systems. As pointed out by the Committee on Planetary Biology and Chemical Evolution, there also is a growing realization "that life itself may modify a planet's surface and atmosphere to optimize conditions for its existence." Assessment of the observations gathered from the Viking mission indicates that there is no positive evidence for the presence of life on Mars at the present time. The contrast between the earth as the only known oasis in the solar system and the other atmosphere-rich planets, which have bulk characteristics similar to the earth, provides a strong motivation for comparative study.

Recent discoveries on Mars have exposed a host of existing phenomena that are worthy of scientific study. The Martian surface has shown itself to be richly diverse and is ripe for intensive exploration. Problems with our current concepts and gaps in our knowledge have identified a well-defined set of objectives. The nature of Martian surface chemistry and the interaction between the atmosphere and surface have produced a unique morphology that has some similarities to earthlike processes and that must constitute an appealing objective for exploratory studies. The basis for the Martian climatic cycle is now ready for study and analysis. Venus has a substantial atmosphere that exhibits important dynamical and meteorological phenomena; the surface of Venus is shrouded so that its topography is only vaguely known. However, the bulk properties of Venus and Mars exhibit similarities to those of the earth. These similarities suggest that we can observe three natural experiments in planetary evolution. One experiment produced the earth with abundant free water in its atmosphere and oceans. The interaction of this water with the earth's surface and interior has played a vital role in tectonic processes and environments and in the evolution and sustenance of life. The second experiment produced Venus with almost complete outgassing of its volatiles to the atmosphere and with virtually a total lack or total loss of water from the planet. The third experiment evolved Mars, which has lost a large part of its atmosphere or which never released sufficient volatiles to produce a substantial atmosphere.

These distinctive states of three terrestrial planets with atmospheres, with apparently similar gross properties and a common origin, constitute a unique opportunity to study and understand critical evolutionary stages that have produced such different end states, only one of which appears to be hospitable to life. It is conceivable that Mars may today represent an earlier evolutionary state of the present earth when the atmosphere was being formed and that Venus may represent a state of the earth if its surface temperature were to rise very significantly.

Based on the preceding arguments and the rich diversity of phenomena manifested by terrestrial planets with atmospheres, we conclude that major advances in planetology can be achieved by the intensive exploration of these bodies. *COMPLEX recommends that the triad of terrestrial planets, earth, Mars, and Venus, should receive the major focus in exploration of the inner solar system for the next decade. The ultimate goal in this exploration is to understand the present state and evolution of terrestrial planets with atmospheres. The comparative planetology of these bodies is a key to the understanding of the formation of the earth, its atmosphere and oceans, and the physical and chemical conditions that lead to the origin and evolution of life.*

The atmosphere-free terrestrial planets, Mercury and the moon, are complementary bodies of high scientific interest. They represent the accumulation of volatile-poor matter or material that lacked volatiles during early planet formation. The evolution of these bodies exhibits the earlier stages of planetary history unaltered by atmospheric interaction and is a record of the major episodes of bombardment. Indeed, the successes in lunar exploration have yielded major advances in our knowledge of the solar system. At present we lack information of the lunar surface chemistry on a global scale and of the structure of the interior. The chemical composition of the back-side lunar highlands is poorly known, and there is a good possibility that the large, unfilled basins in this region may have exposed samples from deep within the lunar crust. The proximity of the moon and the relatively smaller level of investment required to underwrite investigations to achieve these objectives clearly indicate that *the moon must remain an important object of exploration, which should receive strong consideration during the decade.*

Mercury, with a lunarlike surface, is inferred to have a metallic core a few hundred kilometers beneath the surface and has an intrinsic magnetic field. The evolution of this planet must be most strongly governed by core formation from what is believed to be highly refractory materials. The first stage of reconnaissance has revealed a variety of interesting features that suggest that major processes have occurred on Mercury that are worthy of investigation.

Advances in the exploration of Mercury, however, are predicated on observations and measurements from a circular orbit of the planet, which the present U.S. launch capability cannot currently provide. *Steps should be made to prepare for the investigation of Mercury after definition of an adequate propulsion capability and in advance of availability of the system.*

Although COMPLEX has developed the strategies for the outer and inner planets in succession, they must be considered as a unified and fully integrated strategy for the period 1975-1987. It must be recognized that during this period the investigation of Jupiter and its rocky satellites constitutes the scientific link connecting the outer planets to the inner planets.

The solar system is the larger extension of our environment and a larger arena into which human technology and civilization will ultimately spread. Our near-term objectives are to develop a better understanding of this environment, and its implications for earth, through spacecraft exploration. Subsequent to the exploration recommended in this report there may be endeavors involving active experiments in planetary atmospheres that may be carried out. On a longer time scale it is appropriate to envisage that there will be eventual larger-scale operations in space and utilization of extraterrestrial materials. We must be sensitive to the longer-term requirements of such possible ventures and attendant scientific enterprises in developing near-term objectives.

During the development of the outer- and inner-planet strategy, the Committee was fully cognizant of the major commitment toward development of the Space Shuttle and that the present budget will likely continue to be constrained over the next several years. *Subsequent to the development of an adequate transportation system, it is the view of COMPLEX that more substantial resources should be diverted toward the accomplishment of the major objectives of planetary exploration.*

SAMPLE RETURN AND *IN SITU* INVESTIGATIONS IN STRATEGY DEVELOPMENT

In the case of the inner solar system, where the reconnaissance stage appears to be complete and where in several cases extensive exploration has been carried out, it is appropriate to examine various approaches to implementing the intensive study stage of investigation of extraterrestrial bodies.

There are two main categories to complex scientific questions that are characteristic of this stage of investigation. In one case they can be answered only, or particularly suitably, on the extraterrestrial body itself. Extreme examples are the studies of the dynamics and structure of planetary atmospheres and interiors. In the second category are questions the answers to which are less dependent on the locale of the investigation, but more on the sophistication of the techniques that can be brought to bear. These questions can always be addressed most effectively in earth laboratories using samples brought back from the particular solar-system body. The appreciation that, in the intensive stage of planetary investigation, earth-based examination of returned samples can play an important role has led COMPLEX to re-examine the scientific advantages of this approach, not only in the case of Mars and the moon but in the sequence of studies of all the inner planets. These considerations may well also be applicable, on a longer time scale, to other solar-system bodies.

In making this examination we are fortunate in having available the history of two programs that illustrate the potentialities of intensive laboratory investigations of small samples of extraterrestrial material. The study of meteorites has a hundred-year history; that of lunar samples made available by the Apollo and Luna missions is only nine years old but even more applicable. In both cases, fundamental questions about the history of the planets and the solar system have been attacked using techniques that were developed specifically for the purpose of advancing knowledge in these areas and that represent major and continuing advances in our analytical capabilities. The actual experience with these programs is the basis of many of the conclusions presented here. Many of the techniques and approaches developed by the Apollo program have proved very useful for studies of the earth.

It is also worth noting that the examination of meteorites and lunar samples has had, and can be expected to continue to have, a continuity that is in marked contrast to the episodic nature of space mission opportunities. Meteorites will continue to fall on the earth. Further, the Soviet Union may continue their successful program of returning lunar samples by automated means and we hope will continue to make these samples available to the international scientific community. Thus, numerous laboratories exist in this country and throughout the world that continue to develop and extend the types of results that can be obtained with even very limited samples of extraterrestrial material.

Advantages of Investigations of Returned Samples in a Terrestrial Laboratory

Based on the experience with meteorites and returned lunar samples, examples can be cited of the advantages of having samples in a terrestrial laboratory. These advantages pertain both to the quality and nature of the scientific questions that can be addressed and the methodological and technical approaches that can be utilized in comparison with what can be achieved with a well-instrumented spacecraft.

Among the terrestrial laboratory techniques currently available to attack important scientific questions about a solar system body are the following:

1. Various isotopic chronological techniques (e.g., Rb-Sr, K-Ar, Sm-Nd, U-Th-Pb) that can provide the ages of the important stages in the history of a planet.
2. Petrological and petrochemical examinations that by microscopic examination establish the basic nature of the processes that produce and decompose rocks and illuminate the basic mechanisms of planetary evolution.
3. Analysis of trace elements on the submicrogram to picogram level that provides information not only on the bulk composition of a body but also on the early solar-system processes that were involved in planet formation and in the later processes of planetary differentiation and the chemical characteristics of the planet that might relate to the appearances of life.
4. Microscopic examinations of materials with a wide range of instrumental techniques to study the morphology and chemistry and that can probably provide the most sensitive detectors of fossil life forms.
5. Precise isotopic analyses, which are assuming an important role in our thinking about the early stages of the solar nebula and the segregation of the planets.
6. Determination of rare volatile elements that give information on the evolution of the atmosphere of the extraterrestrial body.
7. Study of the reactivity of rocks and soils from a planet with the planet's atmosphere under simulated and controlled laboratory conditions in order to understand both the present and the past state of the planet with a sensitivity and control unattainable under ambient planetary conditions.

Aside from these specific examples for which there is direct experience to demonstrate their pertinence for future planetary samples, there are basic methodological advantages that result from having a sample of an extraterrestrial body brought to an earth laboratory. Among these are the following:

1. Many fundamental experiments are too complicated and interactive in nature to be carried out on a spacecraft.

2. The experimental apparatus that is flown in a deep-space mission invariably approaches obsolescence by the time the experiment is carried out because of the long time required for preparation and flight.

3. Experiments must be fully planned prior to a mission, and complex interactive experiments that mutually rely on new results cannot in most cases be carried out.

4. The possibility to follow up on results with appropriate new experiments that were not even envisaged before the first result became available.

5. The availability of portions of the samples, properly preserved in a curatorial facility, for new experiments by succeeding generations of scientists, which result from the development of new techniques or the evolution of new scientific ideas.

6. The ability to establish firmly important conclusions as a result of using different techniques and different approaches after a given scientific question is identified even when restricted to using minimal amounts of materials.

Advantages of *In Situ* Sample Investigation

The introduction has called attention to major classes of problems that can be addressed only, or most effectively, on an extraterrestrial body itself. Global-type studies of large-scale morphology, magnetic fields, and interaction with the space environment are examples of these, as are investigations of the interior structure of the body, of the atmospheric structure and circulation, and of the presence of important transient species. Also in the case of sample investigations, there are the following unique advantages of *in situ* analyses:

1. There are many questions that relate to processes in a local environment that are essentially not reproducible in a laboratory. Examples are the extreme vacuum of the moon, the radiation environment of the moon or Mars, and complex unstable molecular species in the upper atmospheres and at the surface of the planets.

2. One of the greatest advantages of *in situ* measurements is their ability, depending on the mission mode, to obtain a first-order characterization of the surface of a planet at a number of different points. This obviously becomes more important for planets whose surfaces show considerable diversity.

3. Missions involving *in situ* examinations have the possibilities of interactive, follow-on capability. If an *in situ* examination of a specific sample indicates an important result, the possibility exists of selecting another

sample at the site either to verify the result or to decide what further experiments to undertake on locally available samples.

4. Many phenomena are time-dependent and need to be monitored over long periods of time. The interaction of surface materials with the atmosphere, involving, for example, the exchange of volatiles at the interface, are expected to be "weather"-dependent.

In situ measurements can never hope to duplicate the range or precision of earth-based laboratories. Nonetheless, even at their current level of development such investigations are capable of measurements at the accuracy necessary to achieve certain important scientific goals. This has been amply demonstrated by the early Surveyor missions and by the more recent Viking missions. However, we again emphasize that a wide class of measurements (such as atmospheric structure) must be determined by *in situ* investigations and that the experimental techniques for these must be developed to an adequate degree of accuracy.

It is imperative to realize that *in situ* measurements are capable of considerable improvement. A comparison of the unmanned missions to the moon (Surveyor, 1967) and Mars (Viking, 1977) illustrates the state and rate of advance of *in situ* studies, just as the examination of lunar and meteoritic samples illustrates the potential of returned-sample investigations. The automated missions have progressed from the primarily passive observational modes of operation of Surveyor to the more advanced manipulative operations of Viking (choosing, picking up samples, and sorting them) and to the carrying out of preprogrammed experimentation (heating samples, reacting them with different reagents, analyzing the products). In addition, Viking has been able to demonstrate an interactive capability—the possibility to modify its operations in response to early results—that is a preview of future possibilities.

It is clear that, with proper support, the experimental techniques and instruments, the manipulative capabilities, and the interactive potential of *in situ* investigations can be significantly improved over those demonstrated by Viking. Already, chemical analytical capabilities for such missions are available that are more accurate and at least an order of magnitude more sensitive than used on Viking. Advanced chemical and analytical experiments are being used in the large probe in the Pioneer-Venus mission. As we discuss in detail in a separate section of this report, the extent to which more accurate and sensitive analytical instruments can be realized depends on the support given to the development of new generations of instruments.

These are examples of the advantages of sample investigation on site. Clearly a rational program of intensive investigation of extraterrestrial bodies should keep in mind both *in situ* and for returned sample studies.

Policy Considerations for Sample Return

In view of the foregoing considerations and as part of the present, decade-long strategy recommendations for exploration of the terrestrial bodies, COMPLEX has undertaken to develop a set of principles for placing extraterrestrial sample return into its proper relation to the other components of planetary exploration. The Committee considers it useful to make explicit the factors that it recommends be taken into account in assigning relative priority, and place in a sequence, to sample return as an exploration technique.

In the past, the SSB has recognized the importance of sample return, specifically for the moon and for Mars. With particular reference to Mars, the SSB recommended earlier (*Opportunities and Choices in Space Science, 1974*, National Academy of Sciences, Washington, D.C., 1978) that sample return "be adopted as a long-term goal." COMPLEX has extensively reviewed the major scientific goals of solar-system exploration and the possible mission techniques and has concluded that, *in order to carry out an adequate program of exploration of the solar system, there will be a need to return several times to some planets with different spacecraft carrying different experiments, over a period of two decades.* In this context, no single mission mode should be considered an end or terminal goal in its own right. Rather, we view exploration of a planet as an organic program of investigations including both *in situ* studies and studies on returned samples. Efforts must be expended to ensure that maximum advantage is taken of the interactive and mutually supportive natures of the various program components.

In general, sample return from any extraterrestrial body cannot be considered as a viable mission option *ab initio.* A minimal base of information is necessary to guide the choice of a sample to define and address substantive and important scientific questions in a definitive way and to ensure the acquisition of a sufficient variety of materials, and with adequate documentation. This base of information is also necessary to guide the development of sample-gathering techniques adapted specifically to the environment of the sample object and to allow planning for sample-handling protocols that protect the scientific integrity of the returned material. Precursory missions must be carried out that provide this base of information while at the same time they carry out basic and independent *in situ* studies.

In keeping with the desired, mutually interactive design of *in situ* and returned sample studies, and in view of previously mentioned requirements, COMPLEX considers that sample return becomes a viable exploration option, competitive with other mission modes, when the following general criteria are met: (1) sufficient information is available to ensure the acquisition of a scientifically interesting set of samples and provide for their integrity; (2) best judgment indicates that, within the constraint of mission options realistically available, information obtained from additional precursory missions is not likely to impact on the scientific criteria for site and sample selection in an essential and realistic way—this assessment must be made with due regard for the diversity of physical environments that may be expected to exist on any particular body; and (3) that the further course of planetary exploration is as likely to be guided by returned sample investigations as by further *in situ* missions. Once these criteria are met, the choice of a precise sequential arrangement for sample return missions and *in situ* investigations should depend on scientific priorities and technical capabilities and the impact on institutional resources and other priorities.

Conclusion, Summary, and Recommendations

The advantages of examination in an earth laboratory of properly selected samples from an extraterrestrial body are so great that sample return should be considered one of the basic modes of intensive study of a planetary body. It cannot and should not replace *in situ* measurements—even in the intensive stage. Many measurements will have to continue to be made on or near the planet itself. In addition, the results of both types of intensive studies can be expected to raise new questions about the planetary body requiring probably both a new generation of *in situ* measurements and additional returned samples.

As a result of this examination, COMPLEX *recommends the following:*

1. *Sample return from solar-system bodies should be considered a mission technique within the framework of a continuing program of scientific exploration and not a terminal, long-term goal.*
2. *Studies should be initiated to develop the special technology for such sample returns, with the moon, Mars, and Venus being examples of prime candidates for such sample return.*
3. In situ *investigations should continue to be a major mission mode for planetary exploration.*
4. *Efforts should be undertaken (as described in Chapter 6) to identify and develop the more advanced scientific instrumentation and manipulative techniques needed for the scientific objectives of future* in situ *planetary investigations.*

In a more general context, we wish to emphasize that the control and operation of sophisticated interactive experiments for any prolonged period using spacecraft systems is a most taxing and costly endeavor. It is the balance between the quality of the scientific advances, the cost of a sample return and analysis program, and the cost and complexity of prolonged spacecraft systems operation and data analysis that should guide the specific programmatic decisions.

THE ROLE OF GLOBAL MAPS

While assessing the progress of inner-solar-system investigations the Committee has been impressed by the important contribution that global maps of surface topography and morphology have made toward our qualitative picture of the behavior and evolution of planetary objects. The first planetary view of the earth was obtained in 1968 from the Apollo 8 mission. Prior to 1966, the only planet besides earth for which there existed good resolution images was for one half of the moon. In 1959, the first views of the back side of the moon were obtained by Luna 3, and in 1966-1968 the first good resolution images of the whole moon were obtained by Lunar Orbiters 1 through 5 and exposed the remarkable fact that lava flows only covered the earth-facing side of our synchronously rotating sister satellite. At present, reasonable resolution images only exist for the earth, the moon, Mars, and one half of Mercury. The first good resolution images of the disk of Mercury were by the Mariner 10 spacecraft and exhibited for the first time a lunarlike surface subject to intensive meteor bombardment and flooded by sheets of lava. Good-resolution global images of Mars were first obtained by Mariner 9 and Viking and revealed a surface of bizarre land forms, which proved or hinted at major planetary processes. Enormous volcanoes and canyons were seen, fluvial erosional and depositional land forms and glacial-like valleys were observed, which suggest the importance of liquid and frozen volatiles sometime in Martian history. These observations have had a major impact on our conception of the terrestrial planets.

A global map or image of the surface of a planet at good resolution is considered to be a major scientific contribution and is basic to any advance in the understanding of the terrestrial planets. The existence of volcanoes would show that internal heat sources that caused planetary melting have been active; major fractures would be indicative of stresses due to heating or cooling; the existence of mountain chains would be indicative of truly dynamic tectonism so far found only on earth; large-scale impact craters would indicate heavy late-stage bombardment by meteorites, and intermediate-sized craters would show that the planetary atmosphere evolved after most of the meteor bombardment. The possible occurrence of large-scale erosional features gives important information about the history of climatic and atmospheric conditions. Such maps have raised new and profound questions, which play a major part in directing our approach to continued solar-system exploration and in our view of the family of terrestrial planets. *We do not wish to imply that global images at increasingly high resolutions are the means for continuing planetary exploration, but rather that an adequate resolution global image provides a fundamental framework for carrying out the specific analytic experiments on planetary bodies. The quantitative, analytic, and observational experiments must undoubtedly be the major means of scientific advance in an era of intensive exploration.*

OBJECTIVES FOR MARS

The objectives for continuing scientific study of Mars in the period 1977-1987 were reviewed by COMPLEX after the completion of a substantial portion of the Viking mission. The Committee's recommendations reflect the knowledge obtained through the Viking experiments and observations. In defining the scientific objectives for Mars, COMPLEX has particularly kept in mind the substantial commitment of resources that its recommendations may entail. We believe that the weight of our recommendations and the extent of the national resource that they claim are compatible with the scientific significance of the program and the fact that vigorous exploration of the solar system and the increase in knowledge engendered by it have historical significance. Until now the United States has occupied the leading position in Martian exploration. We believe that it would not be responsible to turn away from this vigorous effort and to relinquish this role.

The planet Mars displays a host of geological and atmospheric processes and shows a continuing history of surface activity of diverse sorts and on many scales. Fundamental questions about the constitution and history of Mars and about the nature of these planetary processes will remain after all experimental results from the Viking mission have been analyzed. Mars is a key member of the triad earth-Mars-Venus and is closely linked to the earth by virtue of the volcanic, erosional, and climatic phenomena that it is known to exhibit. The study of Mars is an essential basis for our understanding of the evolution of the earth and the inner solar system. To accomplish the scientific objectives, *we recommend that intensive study of Mars by spacecraft be achieved within the period 1977-1987.*

Two important precepts have guided the Committee's definition of primary objectives for future exploration of Mars. First is the need to carry out intensive studies of the chemical and isotopic composition and physical state of Martian material to determine the major surface-forming processes and their time scales and the past and present biological potential of the Martian environment. Second is the need to achieve a broad-based and balanced planetological characterization in order that meaningful comparisons can be drawn between Mars and the other members of the triad earth-Mars-Venus. It has been clearly demonstrated in studies of the earth, the moon, and meteorites that intensive study of local areas has yielded the key information to comprehend the nature and scale of planetary evolution. This information in conjunction with that from broader morphological areas and studies at different locations will provide the fundamental knowledge for understanding the basic chemical, physical, and biological evolution of the planet.

Primary Objectives

In summary, the primary objectives in order of scientific priority for the continued exploration of Mars are (1) the intensive study of local areas (a) to establish the chemical, mineralogical, and petrological character of different components of the surface material, representative of the known diversity of the planet; (b) to establish the nature and chronology of the major surface forming processes; (c) to determine the distribution, abundance, and sources and sinks of volatile materials, including an assessment of the biological potential of the Martian environment, now and during past epochs; (d) to establish the interaction of the surface material with the atmosphere and its radiation environment; (2) to explore the structure and general circulation of the Martian atmosphere; (3) to explore the structure and dynamics of Mars's interior; (4) to establish the nature of the Martian magnetic field and the character of the upper atmosphere and its interaction with the solar wind; (5) to establish the global chemical and physical characteristics of the Martian surface.

These objectives are multiply connected. For example, definition of the volatile inventory should pay proper attention to gas exchange between the planet and the solar wind.

In the following we will briefly expand on the substance of these recommended objectives and outline the recommended strategy for accomplishing them.

CHARACTER OF SURFACE MATERIALS AND PROCESSES

The establishment of the chemical, mineralogical, and petrological character of the various components of the Martian surface material should include (in approximate order):

1. Gross chemical analysis (all principal chemical elements with a sensitivity of 0.1 percent by atom and an accuracy of at least 0.5 atom percent for the major constituents).

2. Identification of the principal mineral phases present (i.e., those making up at least 90 percent of the material in soils and rocks).

3. Establish a classification of rocks (igneous, sedimentary, and metamorphic) and fines that define Martian petrogenetic processes.

4. State of oxidation, particularly of the fine material and rock surfaces.

5. Content of volatiles or volatile producing species (H_2O, SO_3, CO_2, NO_2).

6. Determination of the selected minor and trace element contents.

(a) Primordial radionuclides: K with a sensitivity of at least 0.05 percent; U and Th with a sensitivity of at least 1 ppm.

(b) Selected minor and trace elements (e.g., C, N, F, P, S, Cl, Ti, Ni, As, rare earth elements, Bi, Cu, Rb, Sr).

7. Measurement of physical properties (magnetic, and, in the case of fines, density and size distribution, and rheological properties).

The establishment of the nature and chronology of the major surface forming processes should include determination of:

1. Cosmic-ray exposure ages of soil and rock materials for both long and short time scales.

2. Crystallization ages of igneous rocks, recrystallization ages of metamorphic rocks, and depositional ages of sedimentary rocks.

The distribution and abundance of the volatiles H_2O and CO_2 in the Martian regolith should be determined to a depth of 2 m with an accuracy of 10 percent of the concentration and a sensitivity of detection of 0.1 percent. The surface temperature and temperature gradient should be measured.

Evidence for the existence of life in the past or any information relative to the conditions under which it might evolve, are required to assess the biological potential of Mars. Among the measurements of the Martian surface material that address this objective are the following: (a) a complete chemical analysis including all the principal chemical elements (those present in amounts greater than 0.5 percent by atom) as well as those of special biological significance (C, N, Na, P, S, Cl) with a sensitivity of at least 100 ppm; (b) a determination of the oxidation state of the sample and of the pH of water in equilibrium with it; (c) the quantitative determination of the amounts of water in different forms (mineral, hydrate, absorbed, or free) as a function of depth; (d) the determination of the water-soluble constituents of the sample; (e) the determination of the major anions and cations present if the sample is exposed to water at various pH from 5 to 9; (f) The determination of the amounts of reduced carbon present with a sensitivity of

10 ppb; (g) the identification of the major mineral phases present; (h) the extensive search for possible fossil forms in Martian soils and rocks.

It is obvious that many of these measurements have pertinence to other than the biology-oriented objectives of Martian exploration.

Among the measurements that address the role of the environment of the Martian samples and their ability to support life are (a) establishment of the radiation environment at the surface of Mars, including electrons above 1 MeV and photons above 10 eV and (b) determination of the amounts of minor constituents of the atmosphere (e.g., CH_4) that may reflect the existence of conditions someplace on Mars more favorable to the development of life than were found by Viking.

Establishment of the interaction of the surface material with the atmosphere and its radiation environment should include the following investigations in addition to the specific analyses of surface material given above:

1. Reactivity of fine material with the constituents of the atmosphere (e.g., solubility in water, absorptive properties for CO_2, H_2O, CO, or O_2).
2. Noble-gas contents and isotopic composition of atmosphere and soil to a precision of better than 0.5 percent for all major isotopes.
3. Determine the composition of the Martian atmosphere at the surface over an annual cycle.
4. Precise determination of oxygen, nitrogen, carbon, and hydrogen isotope ratios in selected components of Martian surface material and atmosphere.

STRUCTURE AND GENERAL CIRCULATION OF THE MARTIAN ATMOSPHERE

The circulation of the atmosphere of Mars provides the closest analogy to that of the earth in the solar system, and it therefore serves as an ideal test site for dynamical and climatic theories developed for the earth. Mechanical and thermal effects of topography on circulation, baroclinic instability, forcing and propagation of tides, generation of dust storms, and long-term climatic variations represent specific topics relevant to both the earth and Mars. Neither the Viking landers nor Mariner orbiters have provided adequate information to define the global circulation pattern. Atmospheric temperature measurements with a resolution of roughly 5 degrees in latitude, 30 degrees in longitude, and 5 km in altitude between the surface and at least 30 km are needed. This goal could be achieved using a downward viewing infrared sounder in a low-altitude, circular, polar orbit. Much more detailed knowledge of atmospheric waves, including tides and Rossby waves, of the hydrological cycle, of regional meteorology, of the role of dust in the general circulation, and of winds above the boundary layer, is also needed. These problems could be addressed using about four ground-based stations with lifetimes exceeding one Martian year and spaced between high latitudes and tropical regions. These stations should be sited to provide at least one triangular network with roughly 1000 km sides, and each station should measure pressure, temperature, relative humidity, atmospheric opacity, and wind velocity. The benefits to be derived from simultaneous measurements from the orbiter and the ground station network should be determined and assessed.

STRUCTURE AND DYNAMICS OF INTERIOR

Determination of the internal structure of Mars, including the thickness of a crust and the existence and size of a core, and measurement of the location, size, and temporal dependence of Martian seismic events is an objective of the highest importance. The level of Martian seismicity, however, has not been established by the Viking seismology experiment. The possibility cannot be excluded on the basis of currently available data that the seismicity level may be substantially below the upper bound set by the Viking 2 seismometer results and/or that the absorption characteristics of the Martian interior may be comparable with or enhanced over those of the earth's mantle. In such an eventuality, the number of seismic signals recordable on the Martian surface from distances of greater than 1000 km may be very few.

In spite of this uncertainty, which has been recognized in assessing the relative priority of the determination of internal structure and dynamics as a major scientific objective for Mars exploration, we regard the likelihood of detectable natural seismic events as sufficiently high to recommend that a passive seismic network be established on the Martian surface. Such a network should consist of at least three stations with broadband sensors, each with a sensitivity at least 100 times improved over the Viking seismometers, spaced approximately 1000 km apart and operating simultaneously for a period of at least one year.

Accurate determination of the moment of inertia of Mars, a valuable constraint on internal structure, requires measurement of the Martian precessional constant. This measurement can be made from the long-term tracking of one or more landed transmitters, an experiment that may also yield information on the existence of a Martian Chandler wobble and on other polar motions.

Combined mapping of gravity and topography will allow global extrapolation of locally derived seismic structure and will address the question of Martian isostasy as a function of space and time. Some high-resolution gravity coverage is currently being obtained from the Viking orbiters, but topography at comparable resolution is lacking. Topographic mapping of Mars using earth-based radar should be continued.

MAGNETIC FIELD AND INTERACTION WITH THE SOLAR WIND

Determination of the character of the Martian magnetic field and elucidation of the nature of the planet's interaction with the solar wind and the structure and dynamics of the upper atmosphere are essential objectives of continuing Mars study. After completion of the Pioneer-Venus mission, Mars will be the only planet in the inner solar system out to Jupiter for which the first-order properties of the magnetosphere and solar-wind interaction region are essentially unexplored. For Mars, this area has been neglected programatically and should be given serious attention in future Mars exploration as it can be readily attacked using existing instrumentation.

Measurements sufficient to separate an internal, global-scale magnetic field from the solar-wind-induced field and to establish the presence of an internal field having a surface intensity approaching 10^{-5} G should be carried out. Confident separation of internal global and regional fields from the induced external components would be facilitated by simultaneous measurements of both the plasma and the magnetic field as well as measurements in the free-streaming solar wind.

The interaction between the solar wind and Mars's upper atmosphere presents a host of problems that are fundamental to our understanding of both Mars and of planetary atmospheres in general. Among the major issues are the physical processes that produce mass exchange between the atmosphere and the solar-wind flow and the atmospheric mass-loss (or -gain) rates that result; these escape processes are essential to our understanding of the evolution of Mars's atmosphere.

Characterization of the Mars solar-wind interaction will require establishing the distribution of neutral atmospheric constituents, as well as the ionized plasma and charged-particle distributions from both the solar wind and the atmosphere separately. These should be carried out both in the dayside interaction region, near an altitude of 300 km, and in the nightside, downstream magnetosphere, or wake region ranging to several Mars radii. In addition, the fluxes of energetic particles that may be accelerated by the Mars solar-wind interaction should be established.

GLOBAL CHARACTERISTICS OF SURFACE

The global chemical characteristics of the planet should be established with at least 50 percent coverage of the planet at a resolution of 500 to 1000 km.

Potassium, thorium, and uranium should be determined to a sensitivity comparable with the levels in Apollo 11 basalts, and the following elements with an accuracy of 10 percent at the indicated concentrations: Fe, 1%; Ti, 0.5%; Si, 5.0%; O, 5.0%; Mg, 4.0%; H, 1.0%. The measurement of Al, Ca, Na, Mn, and Ni would be highly desirable.

STRATEGY FOR MARS

The diversity of the Martian surface, as well as the wide range of environmental conditions and our ignorance of some of the key processes active on the Martian surface, compel us to the view that the scientific objectives will best be met by exploring broad areas that exhibit the effects of distinctive processes that have influenced Martian evolution and by the intensive study of an intelligently selected suite of Martian samples returned to earth. The selection of returned materials should be based on our understanding of the global and local diversity of Martian terrains and environments. *Thus, the global and* in situ *studies of the planet and the return of Martian material are complementary components of an overall program of investigation; each of the components is separately necessary.* The strategy for carrying out these aspects of Mars study must be structured to take advantage of their interactive and mutually supportive natures. *We note, however, that although a specific sequence of investigations, namely global and* in situ *studies followed by sample return is desirable, it is not necessary.*

To understand the current and past processes operating both at and near the surface of Mars, it is essential to explore the diversity of Martian terrains that are apparent on both global and local scales. *We therefore recommend that detailed exploration, on both global and local scales, of the diverse environments of Mars for purposes of understanding surface, near-surface, and atmospheric processes is a worthy goal in its own right and should be accomplished within the next decade.* To this end, intensive local investigations in selected areas of 10 to 100 km in extent should be carried out, and, in addition, measurements at single points of extreme planetary environments should if possible be exploited. These local investigations should explore terrain and sample diversity with a wide range of chemical, mineralogical, and physical techniques. Both the analytical techniques and the manipulative skills of the experimental devices should be much advanced from those used on Viking, but without attempting to duplicate an earth laboratory. Several science objectives requiring global-scale investigations can be accomplished with orbiters. Geochemical and geophysical mapping and atmospheric temperature soundings should if possible be carried out over the entire planet with spatial resolution compatible with science objectives. We emphasize that geochemical and geophysical mapping experiments must provide results that are clearly interpretable in terms of fundamental planetary characteristics and processes. In addition, temperature sounding should cover the full diurnal and seasonal cycles. Investigation of Mars's magnetic field and atmospheric interaction with the solar wind requires both dayside and nightside measurements. These measurements should cover the direct interaction region at and below about 300 km altitude, the free-streaming solar wind, and the downstream nightside magnetosphere or wake ranging to several Martian radii.

The Space Science Board (see *Opportunities and Choices in Space Science, 1974*, National Academy of Sciences, Washington, D.C., 1975, p. 19) has previously recommended for Mars that the "long-term objectives of exobiology and surface chemistry investigation are best served by the return of an unsterilized surface sample to earth" and further recommended that Mars sample return be adopted as a long-term goal. The Committee has thoroughly reconsidered this matter and concluded that understanding of the basic physical-chemical mechanisms that govern the surface of Mars can only be obtained by sophisticated and interactive analytical investigations. The return of Martian surface and subsurface samples to earth laboratories will allow the full range of the most sophisticated analytical techniques to be applied for the study of chronology, elemental and isotopic chemistry, mineralogy, and petrology and for the search for current and fossil life. In addition, such samples will be available to future scientists for study with improved techniques or with wholly new concepts compared with those available at the time the sample return mission was designed. *We therefore reaffirm our view that the return of unsterilized surface and subsurface samples to earth is a major technique for the exploration of Mars.* Samples of distinctive materials, including rocks and fines, should be selected from an area of at least 2 m^2, based on visual inspection and major elemental analyses at the landing site. Materials should be selected that reflect the diversity of the local environment and the processes of broader planetary evolution. Samples should be returned to earth in a manner that preserves their integrity and that is free from terrestrial contamination.

Relationship of Science Objectives and Mission Techniques

In outlining the goals and scientific objectives for Martian exploration, we have referred to the use of particular mission techniques. It is the Committee's view that the scientific objectives and scientific priorities should govern the planning and design of subsequent missions with due regard for the constraints imposed by the technical capabilities and institutional resources. The Committee considers that each future Martian expedition should yield a major increase in scientific knowledge. However, the

Committee is also concerned that the exciting diversity of phenomena and terrains observed on Mars may evoke mission plans that are overly complicated and demanding. We do not consider that a single mission can provide an omnibus for addressing all the scientific questions. It is evident that the intensive exploration of Mars will require several missions over the next two decades and that these missions should be complementary or supplementary in nature.

We further consider that in formulating mission plans, certain specific primary objectives should be selected as a guide for the overall mission objectives. The final mission plan may comprise some selected high-priority primary objectives and some secondary objectives.

The problem of mission operations and management of scientific instruments will have to receive very serious attention for any of the mission techniques that have been discussed in this section, as indicated in the earlier section on Mission Operations.

For the purposes of illustration we may consider two possible mission scenarios, one emphasizing intensive *in situ* exploration and the other sample return. For the intensive *in situ* exploration, we consider substantial mobility necessary to carry out broad exploration of a selected area 10 to 100 km in extent with the general area selection based on existing information. A robot manipulability would be required to repeatedly obtain samples of rocks and soils at the surface and soils from below the surface. The experiments should provide high-quality macroscopic and microscopic imaging, precise data on all major chemical elements including water, detection and identification of hydrocarbons (organic compounds), determination of the mineral phases present, and measurement of the atmospheric composition and pressure. These experiments must be carried out repeatedly during the traverse across the Martian surface.

An alternative *in situ* scenario might be to utilize a few landers without true mobility but with manipulative skills and accessibility within a radius of about 1 to 2 m near the craft. The analytical capabilities are considered to be of substantial quality possibly equal to that envisaged for the long-range roving laboratory. The effective decrease in mobility would be offset by the use of multiple lander stationary laboratories, which would be deployed in both typical areas and in the extreme environments including the polar area.

For a sample-return mission, we consider that some mobility in a selected area of a few meters may be necessary with the general site selection based on existing information. A robot manipulability sufficient to obtain and package samples of a few rocks and soils from the surface and soils from below the surface is necessary. Basic imaging and chemical analytical capabilities are necessary to select some diverse materials of reasonable quality for return to earth. Extensive orbital science, possibly with gamma-ray mapping, multispectral imaging, and atmospheric sounding may be accomplished as a supportive function for both mission scenarios but need not necessarily yield full global coverage. These scenarios illustrate that major advances in Martian exploration can be achieved within a circumscribed framework.

Changed Role of Life-Seeking Experiments

The primary scientific objective of the Viking missions was to seek evidence relating to the existence of life on Mars. For this reason Viking carried three experiments that sought to detect active metabolic processes in Martian soil samples. Other experiments sought to establish whether organic compounds produced by present or prior life forms were detectable in the regolith. In addition, the television images could be used to observe any large forms that might be suggestive or indicative of living creatures. Unambiguous positive results from these experiments would have constituted a finding of truly major scientific importance. For this reason any plans for future Martian exploration were strongly dependent on the Viking findings.

The SSB charged the Committee on Planetary Biology and Chemical Evolution (CPBCE) with evaluating the biological implications of the Viking experiments and also requested that committee to develop recommendations for any subsequent biological investigations for Mars. Close liaison between COMPLEX and CPBCE was maintained through the past several years. Independent experts from the general scientific community with knowledge of biology and biochemistry were brought in to aid in addressing this important issue. The evaluation and recommendations of CPBCE were published in the report *Post-Viking Biological Investigations of Mars* (National Academy of Sciences, Washington, D.C., 1977).

In assessing the presence of reduced carbon and organics in the Martian regolith, the CPBCE reported as follows: "With the possible exception of data from the pyrolytic release experiment, there continues to be no evidence for the existence of carbon reduced below the state of CO and no clear evidence of any form of carbon save in the atmosphere and in the winter polar caps. No organic compounds other than traces attributable to terrestrial contaminants, have been detected in regolith samples analyzed by the GCMS. If volatizable organic compounds were present in the samples, they were either present in concentrations below the parts per billion range (the detection limit of the instrument) or they were totally restricted to substances like methane with molecular weights of less than 18, which are undetectable or detectable only at reduced sensitivities." With regard to the active biology experiments that committee has stated that "All three experiments have yielded signals that clearly indicate chemical activity. What is less clear is the interpretation of the signals. Some aspects of the data are consistent with those expected from biological activity comparable to that observed on Earth, but other aspects are inconsistent." They also point out "that abiogenic explanations of the results are conceivable ... and that conditions at the regolith–atmosphere interface on Mars are vastly different from those at the soil–atmosphere interface on Earth. This vast and incompletely characterized difference makes it inordinately difficult to conclude that experimental results, which are unambiguously ascribable to biological activity on Earth, are unambiguously ascribable to biological activity on Mars." The CPBCE report makes clear that those aspects of the exobiological experiment results that appear to be inconsistent with metabolic processes far outweigh those aspects that are consistent. Despite current and possibly future inability to reach rigorous interpretation of the Viking active biology experiments, that committee concluded that "the evidence at hand is sufficiently persuasive to require that that strategy be predicated on the assumption that the positive signals from Mars are not biological in origin."

The CPBCE report distinguished between biologically relevant experiments that should be conducted remotely on the surface of Mars in an ensuing mission or missions and those that should be conducted on samples returned to earth. For the former, it recommended analyses on samples of those characteristics that would constitute items of paramount importance to present or past biology and to organic chemical evolution, namely, the presence of reduced carbon, and the isotopic state of carbon, the amount and state of water, the presence of water-soluble electrolytes, and the existence of nonequilibrium gas compositions. It recommended that specific "life-seeking" metabolic-type experiments *not* be conducted remotely on the Martian surface, but that they only be conducted on unsterilized samples returned to earth.

With regard to the role of life-seeking experiments in the future exploration of Mars, COMPLEX is in accord with the general views expressed by CPBCE. Based on the goal of understanding how the appearance of life in the solar system is related to the chemical history of the solar system, COMPLEX has formulated a strategy for future Mars exploration on the following premises:

1. Characterization of the chemical composition and physical state of materials on the surface and below the surface and the interaction of these materials with the atmosphere and sunlight are of basic importance to understanding the biologic potential of the planet.
2. The abundance and distribution of carbon compounds and water (including liquid H_2O) in different materials is of significance.
3. The direct search for the study of chemical effects that relate to

metabolic activity in Martian materials and the intensive search for possible Martian fossils should be carried out on unsterilized materials returned to earth without contaminating them with terrestrial materials.

4. Substantial attention and sensitivity toward the biologic potential of the Martian environment should be associated with the *in situ* chemical and physical characterization of Mars without directing specific efforts toward active life-seeking experiments.

OBJECTIVES FOR VENUS

For the purpose of formulating a strategy for Venus extending to 1987, we have assumed the successful completion of the Pioneer Venus mission. This exploratory mission is scheduled to be launched in 1978 and is aimed at elucidating the basic structure, dynamics, and chemical composition of the atmosphere and the character of the ionosphere, magnetic field, and solar-wind interaction. We recognize that the detailed results of the Pioneer Venus investigation will have an impact on planning for future Venus studies. Possible Soviet missions are presumed to include landers and atmospheric sounders including balloons. The results of all these exploratory missions and those earth-based observations that may be carried out have been anticipated, and we have directed the thrust of our Venus strategy for the next decade toward major scientific objectives that are complementary and will remain applicable independently of the results of these investigations.

When the Pioneer Venus results are combined with data already obtained from Mariners 2, 5, and 10 and from the U.S.S.R. Venera series of probes and with observations made from earth, the following status of our knowledge of Venus is probable by 1980. A major feature of Venus is its dense atmosphere, and in many ways this will be the best studied feature of the planet by 1980. More than a dozen probes will provide a corresponding number of snapshots yielding vertical profiles of atmospheric parameters such as wind velocity, temperature, pressure, density, and the composition of major constituents. The chemical composition of the lower atmosphere should be well established for all major species and some trace compounds. Local values of cloud opacity and of solar-energy deposition will be known. The upper atmosphere, ionosphere, and magnetosphere will have been surveyed extensively, but only general information on atmospheric circulation will be available. Approximately half of the surface of Venus will be mapped by radar at a resolution of better than 20 km; 10 percent will be mapped at a resolution of 4 km. Much of the remainder (except for the polar regions, where no data will be available) will be coarsely surveyed at a resolution of between 20 and 50 km. Surface topography on a lateral scale of 10 to 100 km will be available to 100-m vertical accuracy between latitudes of -50 to +75 degrees at all longitudes. The topography along limited suborbital tracks and the surface dielectric characteristics will be determined by surface radar mapping experiments on Pioneer Venus. Crude (order of 200-km) resolution maps of the distribution of the surface emission temperatures may have been obtained using the Very Large Array (VLA) radio telescope. A few high-resolution photographs of very localized surface regions will be available from short-lived U.S.S.R. landers, together with corresponding analyses of near-surface concentrations of the natural gamma-ray emitters (potassium, uranium, and thorium).

In assessing the anticipated advances in understanding the state of the planet Venus, we note several key areas that will not be addressed by Pioneer Venus. The gas-phase composition of the upper atmosphere between the cloud tops and the homopause, where important photochemical reactions take place, will not be substantially studied. We will have no direct determination of cloud composition or absorbing components in the clouds. There will not be sufficient information to describe the global atmosphere circulation below the visible cloud layer. There will be no global map of the morphology and topography on a fine enough scale to reveal the basic characteristics of the Venusian surface and the major processes that have shaped that planet and to guide subsequent exploration. We will have no knowledge of the major elemental and mineralogical composition of the rocks and weathered material to be found on the surface nor of the origin of those rocks. The times of formation of the Venusian crust will be unknown, and any inferences as to overall planetary history will be highly speculative. The possible existence of a planetary magnetic field may be settled, but the whole interior structure of Venus will remain a mystery. It is clear that our knowledge of Venus will not approach in detail that which we have already obtained from Mars in certain areas. In order that meaningful comparison between earth, Mars, and Venus can be made, *the Committee, therefore, recommends that there be continued exploration of Venus by spacecraft during the next decade.*

In consideration of the major focus for studying the planets earth, Mars, and Venus, we have recognized limited objectives that will provide a common framework for a comparative planetological study that should yield advances in basic knowledge. The important contribution of a good quality global map toward obtaining a qualitative picture of the major processes and the evolution of planetary surfaces has been discussed previously. The acquisition of a map of Venus's surface has been inhibited by the dense layer of clouds, which is only weakly translucent to visible radiation and which blankets the planet. Ultraviolet images of the planetary disk have successfully revealed cloud patterns, and observations in the visible region at near ground level have yielded excellent images of the surface in limited areas. Low-resolution radar images have been obtained of much of the Venusian surface with the use of earth-based radar antennas. The limited earth-based data suggest that radar may be able to yield high-resolution images of good quality of the Venusian surface if used in close proximity to the planet. A global image of the surface of Venus with sufficient fidelity to allow the recognition and interpretation of major planetary features is of the highest importance and will allow the direct comparison of surfaces of earth, Mars, and Venus.

Knowledge of the composition, nature, and age of the rocks and weathered material of the Venusian surface is recognized to be of prime importance because of the constraints that this information places on the composition and evolution of the planet. Such knowledge is available for the earth, and it is anticipated that appropriate analyses of Martian material will also be obtained. The surface-atmosphere interface on Venus is of special interest because at the high temperatures and pressures prevalent at the surface, chemical reactions between minerals and atmospheric gases could be rapid. The rates of gas-solid reactions in the absence of water are not known, and the presence of angular rocks on the Venusian surface may indicate very slow reaction rates or very recently formed rocks. It is important to know if the exposed surface contains those particular minerals expected if thermochemical equilibrium exists between the surface and atmospheric CO_2, CO, HCl, HF, H_2O, and other gases. It is also important to know if gases such as S_2, SO_2, and SO_3, which have been formed as a result of disequilibration by ultraviolet light in the upper atmosphere, will react sufficiently rapidly at or near the surface to maintain the lower atmosphere and surface region in thermochemical equilibrium, Finally, in order to determine the history of water in the atmosphere it is also very important to know the oxidation state of the surface, particularly the Fe^{3+} to Fe^{2+} ratio.

For both Venus and Mars, CO_2 is the major constituent of the atmosphere, and catalytic processes dominate paths for chemical recombination. Chlorine is important in the Venusian upper atmosphere in a manner analogous to that in the earth's stratosphere. Venus's visible clouds are composed of partially hydrated sulfuric acid. This smog chemistry dominates on a global scale for Venus and controls ultimately the thermodynamic state of the lower atmosphere. According to present ideas, sulfur is transported upward as COS or H_2S. These compounds decompose photolytically near the visible cloud tops. Sulfur is oxidized by O_2 formed photochemically from CO_2 at higher altitudes. Light and dark areas of the clouds are observed at ultraviolet wavetions in the relative abundance of elemental sulfur and H_2SO_4. Since sulfate aerosols appear to be an important contributor to the infrared opacity of the atmosphere, then it is possible that sulfur chemistry plays a role in maintaining the surface temperature at 750 K on Venus. Measurements from Pioneer

Venus and the Venera missions should provide a more quantitative data base, but they may be expected to raise further questions, which demand intensive study in the area of atmospheric chemistry.

As a result of these considerations, we have identified the following scientific objectives for Venusian exploration:

Primary Objectives

The primary objectives of the exploration of Venus during the period 1977–1987, beyond the Pioneer Venus mission, in order of importance, are (1) to obtain a global map of the topography and morphology of its surface at sufficient resolution to allow identification of the gross processes that have shaped the surface, (2) to determine the major chemical and mineralogical composition of the surface material, (3) to determine the concentrations of photochemically active gases in the 65–135 km altitude region, and (4) to investigate the physical and chemical interactions of the surface with the atmosphere and the composition and formation of atmospheric aerosols.

A map of the surface of Venus should be obtained with a horizontal resolution of at least 1 km (corresponding to the wavelength at which the modulation transfer function of the imaging system is at 50 percent of its peak value). As a key to interpreting these images and to aid in understanding the possible processes that have governed the development of the Venusian surface, we *further recommend that some images be taken of a limited number of selected regions at a substantially higher resolution than indicated above.* Such a task poses substantial technical problems. Based on the available radar results and considering the increased resolution that should be possible with an orbiting spacecraft in close proximity to Venus, there is reason to believe that a high-resolution map may be obtainable. The expectation that radar images will be susceptible to adequate interpretation for the purposes of clearly recognizing surface morphology and identifying surface processes must be properly tested. This approach if reliable could yield a high-quality global image of the type desirable. A topographic map of the surface should be obtained along with the image map to aid in the interpretation of landforms, regional slopes, and large-scale figures. The highly technical nature of the undertaking is one for which the United States is uniquely well prepared through its development of imaging radar systems such as SEASAT for earth applications.

Determination of the major element composition of surface material should be carried out for at least the elements C, N, O, S, Si, Mg, Ca, Fe, Na, K, and Cl, assuming them to be major species, to a level of precision of 3 percent or better. The oxidation state of Fe should be determined. Other chemical elements that might be diagnostic in elucidating the atmosphere-soil-rock equilibrium should be identified and included if possible in the chemical analysis. The nature of the mineral phases whose abundance is greater than 10 percent should be determined by various selective procedures with due consideration to possible phases peculiar to the environment of Venus. The possible choices in analytical measurements that could in principle be used will be restricted by the environment at the surface, which will place serious constraints on total time for data acquisition and the survivability. *The Committee has considered some possibilities and concludes that a serious study effort of the technology of instruments operating at Venusian surface temperatures and pressures must be undertaken before any particular approach is recommended. This substantial effort should be initiated in this area at the earliest possible time.*

Studies of the photochemistry of the upper atmosphere of Venus have important analogies with the photochemistry of the earth's stratosphere, in particular, through the relevance of HCl, Cl, ClO, OH, COS, and H_2SO_4 to both planets. The detailed composition of the primary photochemical region between altitudes of 65 and 135 km will not be explored by the Pioneer Venus mission. Methods of decelerating an entry probe to enable *in situ* measurements of photochemically active gases, for example with a mass spectrometer, as well as other possible techniques, should be considered and investigated. Simple methods for quantitative chemical analysis of both volatile and nonvolatile atmospheric particulates during the descent phase of a lander should be explored.

Secondary Objectives

It is clear that seismic data, either passive or active, would be of immense value in studying the interior of Venus and would form a primary objective if it appeared feasible to obtain. Unfortunately, however, our experience with the earth, the moon, and Mars suggests that surface measurements extending over several months' duration are required to obtain useful data; such durations in the surface environment of Venus appear not likely to be feasible with the levels of technology forecast through 1980. Nevertheless, *COMPLEX believes that acquisition of seismic data from Venus should be maintained as a visible and highly desirable goal.*

It is considered of importance to obtain measurements of the gravitational field and of those local surface properties that could substantially aid in interpreting the orbital images.

As recommended elsewhere in this report, we view return of a surface sample as a normal evolutionary step in the intensive study of Venus. *We* *recommend that preliminary studies be undertaken to determine the technical feasibility of returning samples of materials from the surface of Venus in order to ascertain the significance of this technique in developing an effective strategy for Venusian exploration over a time period of 20 years.* Additional secondary objectives in the intensive study of Venus are exploration of the general circulation of the atmosphere and exploration of the three-dimensional character of the Venusian magnetic field and solar-wind interaction.

Strategy and U.S.–U.S.S.R. Cooperation

In reviewing the present level of exploration activity aimed at Venus and in constructing a strategy to carry this activity forward through the next decade it became apparent that Venus has been, and continues to be, an object of high scientific interest and of effort exerted by both the United States and the Soviet Union. Venus has been visited by a number of flyby vehicles, the United States has embarked on an in-depth study of its atmosphere, and the Soviet Union is engaged in a continuing sequence of studies using landed vehicles on the Venusian surface and possibly in atmospheric study using balloons. It is also apparent that in carrying out their respective programs of scientific investigation the two nations have developed skills and competence that are highly complementary. *It is the view of this Committee that continued scientific exploration of Venus offers an ideal arena for cooperation between the Soviet Union and the United States. We recommend that cooperative efforts be undertaken according to the precepts set forth earlier in this report.*

COMPLEX considers that a reasonable first step in establishing a cooperative working relationship would be the bilateral exchange and joint, in-depth discussion of topographical and morphological information, gathered by U.S. mapping efforts, and information gathered by Soviet surface measurements. Initiation of an early cooperation in this direction has several scientific and operational advantages. First, it will allow the selection of landing sites to benefit from knowledge of surface properties so that missions may be addressed to specific and important scientific questions. Second, the mutually cooperative interpretation of data, independently gathered by each nation's efforts, will further the quality of inference drawn from these data. Finally, this minimal level of cooperation addresses mutual problems at the interpretive and planning levels but makes essentially no direct impact on the independent operation of the two exploration programs; therefore it can be put into effect immediately. A program should be immediately established to exchange and discuss scientific data in order to enhance the quality of interpretation and to facilitate the more rational planning of future missions.

In order to fulfill the primary objectives of this strategy it will be necessary to undertake a series of investigations on the surface of Venus. The Soviet Union has a demonstrated capability for spacecraft operation on the surface of the planet for intervals of the order of, or longer than, an hour. The United States has demonstrated a very high level of capability in the design and construction of remote analytical experiments as well as in the construction of electronics and telemetry to support such experiments. It is rational to bring these capabilities together. Such a course would allow investigations to be carried on at a depth and quality that is beyond the foreseeable means of either nation alone and that provides economies to both sides.

The Committee recognizes several possible approaches to cooperative exploration of Venus. Such cooperation can involve the mutual development of single pieces of equipment or the independent development of complementary experimental equipment. The Committee considers that either is an acceptable approach and considers that a rationally constructed program of cooperation could employ elements of both approaches. It is logical and expedient that joint exploration be entered into in a progressive fashion, with increasing levels of cooperation evolving as experience is gained in collaborative efforts. *Thus, we recommend that a first U.S.-U.S.S.R. collaboration proceed through the independent development of complementary experiments to be flown on a single spacecraft and plan for an early collaborative launch.* This approach will facilitate the earliest possible initiation of a joint program and thus will provide an early opportunity to address the problems that arise. Discussions should be undertaken between the Soviet Union and the United States to plan for such a launch and to address the specific contributions to be made by each nation toward the design, construction, and execution of the mission, taking into account the special skills of each side, and that concrete consideration be given, at an early stage in the discussions, to problems of spacecraft configuration and the compatibility of power and data-transmission systems.

The Committee sees no fundamental barriers standing in the way of close U.S.-U.S.S.R. cooperation in the continued exploration of Venus and anticipates that a vigorous effort exerted by both sides will bring it about. However, we recognize the possibility that unforeseen difficulties, or lack of enthusiasm, could abort an effective and continuing cooperation. Against this possible eventuality, we wish to reiterate our view that *studies of Venus will make a crucial contribution to our understanding of planets in general and, in particular, to our understanding of the behavior and evolution of terrestrial environments. As a result, studies of Venus must be pursued vigorously regardless of the fate of attempts to make it a cooperative endeavor.*

ROLE OF THE EARTH

To an alien observer, the earth would appear remarkable relative to her closest neighbors, primarily as a result of two features: the ubiquity of water on the planet and the obvious presence of complex living organisms. Water totally dominates the volatile inventory at the surface, being about 280 times the mass of the atmosphere and 3 times the mass of CO_2 present as sedimentary carbonates. Surface temperatures enable most of the water to be in the liquid phase, filling vast ocean basins covering some 70 percent of the earth. Water condensed as liquid droplets and ice crystals forms clouds that cover some 50 percent of the earth. Water is also an essential fluid to almost all life forms on the planet. The living organisms themselves range from the simplest prokaryotic cells to highly evolved intelligent animals. They have invaded almost every ecological niche on the planet's surface, being found in situations as diverse as the deep-ocean floors and the polar ice caps. Photosynthesizing plant organisms occupy significant portions of the dry land area and the ocean surface waters and are the dominant sources of the molecular oxygen that comprises about 20 percent of the atmosphere and is consumed in the respiration of both plants and animals. This oxygen also gives rise to an atmospheric layer of ozone, which screens the biosphere from ultraviolet radiation and which itself is regulated by minor products of the biosphere, including N_2O.

In addition to these two essentially unique features of water and life, the earth presents many other distinctive features that may be compared to those of Venus and Mars. The topographic surface of the solid earth displays several broad characteristics that can be attributed to surface and internal processes. Most of the area is at an elevation either near sea level or about 4 km below sea level. The first level corresponds to the continents, and the second to the ocean basins. The transitions between the two regimes are relatively sharp, and the areas of markedly higher or lower elevation are relatively limited in extent. The highest elevations occur in mountain chains, elongated features with widths of about 500 km and lengths of up to 10,000 km. The greatest depths occur in the oceanic trenches, elongated features with dimensions comparable with those of the mountain chains. Distinctive topographic features within all the earth's major oceans are the midocean ridges, which have characteristic widths of a few thousand kilometers and rise about 3 km above the abyssal plains. None of these three features (mountain chains, trenches, or ridges) is known to occur at similar magnitude or scale on Mars, Venus, or the moon or any of the terrestrial planets or satellites. The smaller-scale morphology of the land surface has been shaped in recognizable ways by the actions of water, ice, wind, and chemical weathering and by life.

The terrestrial crust that occupies the ocean basins is only ~6 km in thickness, relatively uniform in chemical composition, and consists of very young materials of ages less than about 0.2×10^9 years. This young age reflects rapid recycling, which has destroyed almost completely all record of older oceanic crust within the present ocean basins. In contrast, continental crust is about 35 km in thickness, has a variable and distinctive chemical composition, and consists of materials ranging in age from 0 to 3.8×10^9 years. The oldest known materials on earth were formed somewhat after the last major bombardment of the earth-moon system. These constituents of diverse age preserve a record, although often biased and distorted, of the history of physical chemical processes, the growth and modification of the continents, and of the evolution of life on earth. The observations indicate that the continents have grown by addition of new material from underlying mantle sources, accretion of island arc volcanic systems to the continental margins, and interaction with ocean basins.

Continents, mountain chains, trenches, and ridges—all are thought to be related by the dynamical processes described by the theory of plate tectonics. According to this theory, the outer shell or lithosphere of the earth is divided up into approximately a dozen nearly rigid plates. These plates have thickness ranging from perhaps a few tens to a few hundreds of kilometers, extend horizontally for several thousand kilometers, and are in constant relative motion. The plates move over an underlying layer of little strength—the asthenosphere. Where two plates converge, the boundary is typically marked by a trench, an associated island arc, and large destructive earthquakes, as one plate subducts beneath the other and is pulled by its own weight into the asthenosphere. The midocean ridge marks the line of separation between plates that are moving apart. New oceanic crust is formed by upwelling magma at the ridge to fill the gap between the separating plates. Old oceanic crust is consumed at the trenches at a comparable overall rate. The continents are carried along by the plates in which they are embedded, so the relative positions of the continents change over times of millions of years. When continents collide, or when a continent is carried to a trench, the surface rocks are overthrust, folded, and raised to form a mountain range, it is thought because continental lithosphere is too buoyant to be subducted. There is no evidence for the occurrence of plate tectonics on planets other than the earth.

The earth's internal structure is divided into three major compositional zones: the crust, mantle, and core. The crust consists of basic to acidic igneous intrusive and extrusive rocks and their sedimentary and metamorphic derivatives; it is heterogeneous in thickness, in composition, and in age. The mantle is ultrabasic in composition and is the source for magma for most surface volcanism. The central core is mostly iron and nickel, with some admixed lighter elements. The outer core is molten and is thought to be in convective motion, a process closely related to the generation of the earth's magnetic field. An inner core is rigid.

Earth shares with Venus and Mars the feature of an atmosphere that is generally believed to have been outgassed from its interior. The principal outgassed components on earth are H_2O and CO_2, the former condensing at the oceans and the latter, in contrast to Venus, entering the solid phase as carbonate minerals as a result of reactions between dissolved carbon dioxide and magnesium and calcium ions weathered from surface rocks. With water largely contained in the ocean and carbon dioxide largely contained in the solid phase in rocks, we are left with an atmosphere composed mainly of nitrogen and oxygen. The surface pressure is 1 bar, and the average surface temperature is 288 K. There are diurnal and seasonal changes in atmospheric temperature and circulation that are intermediate in amplitude between those of Venus and those of Mars. Midlatitude traveling cyclones and anticyclones developing as a result of baroclinic instabilities in westerly flows are an obvious feature of the circulations on earth and Mars but not on Venus. There is clear evidence in the geological record for climatic extremes or "ice ages" in the earth's past during which the water-ice polar caps extended into the midlatitude land areas.

Minor gaseous constituents of the atmosphere include Ar, H_2O, CO_2, Ne, He, CH_4, Kr, H_2, N_2O, CO, Xe, O_3, H_2S, SO_2, HNO_3, and HCl. Of these, CH_4, N_2O, CO, O_3, H_2S, and HNO_3 appear to be direct or indirect products of living organisms. Nitrogen and carbon dioxide are also participants in complex cycles involving both biological and geochemical reactions. Like Venus, the earth has a stratospheric layer of sulfuric acid particles formed by photochemical reactions, but the optical depth of this layer is several orders of magnitude less than its Venusian counterpart. The photochemistry of the earth's upper atmosphere, in particular, chemical reactions involving H_2O, HCl, and O_3, appears to have many important analogies to the photochemistry of the Martian and Venusian upper atmospheres.

Our knowledge of the earth as a planet is naturally far superior to that which we possess for any other planet. However, it is important to note that, despite an extensive library of facts and theories, there are a number of fundamental questions about the earth that have not yet been answered. By their nature, the answers to these questions must be derived from a comparative study of the triad. Our overall objective for the earth is thus to resolve the following fundamental questions:

1. Why the earth alone possesses vast water oceans, and why the earth's atmosphere has a markedly different mass composition and evolutionary sequence than either Venus or Mars;

2. Why the earth and Venus, with nearly identical mass and diameter, and why Mars with smaller diameter and bulk density, each had markedly different thermal and tectonic histories;

3. What are the rates and mechanisms of transport of materials from the deep interior of the planets to their exteriors, and what are the chemical and physical processes of exchange of matter between the interior and the crusts of the planets and their atmospheres;

4. Why the earth, Venus, and Mars possess very different internal magnetic fields;

5. Why particular conditions on earth have led to the evolution of living organisms but apparently not on Venus or Mars;

6. How and why the atmospheric circulation and the long-term climatic variations on earth differ from those of her nearest neighbors;

7. How man and his culture will influence and modify the biosphere and the physical and chemical composition of the earth on a global scale.

The first-order questions are far from independent, and we have not ranked them by order of priority. Indeed, the mutual interactions between them, as expanded upon below, constitute an essential aspect of the study of earth as a planet.

To understand the presence of oceans, we must continue to build a picture of the way in which the oceans and atmosphere evolved over geologic time. However, a complete understanding cannot be derived only from considerations of the earth itself and its history because the volatile inventory of the earth is heavily dependent on the chemical and physical conditions that led to its accretion in the primitive solar nebula. For example, to understand the presence of H_2O, we might hypothesize that Venus, earth, and Mars received successively larger amounts of hydrous minerals such as serpentine and tremolite during their accretion because of their successively lower formation temperatures. Thus, subsequent outgassing would be highly deficient in water on Venus, but plentiful on earth and Mars. Different surface temperatures and different outgassing rates can then be invoked to explain the presence of permanent oceans on earth but not on Mars. The test of such an hypothesis clearly demands a particular degree of knowledge about planets other than earth in order to understand a fundamental property of the earth itself.

Earth and Venus have similar mass, radius, and bulk density and are inferred to be broadly similar in overall composition. Yet the evolutionary paths of these two sister planets appear to have diverged widely. The earth underwent early core-mantle differentiation, an event that released a large amount of gravitational energy as heat and may have been responsible for the apparent lack of a permanent crust on the earth prior to 3.7 billion years ago.

Plate tectonics has dominated both the surface geology and the heat transfer in the upper mantle for the rather recent geological past and possibly for the last 3 billion years, resulting in repeated recycling of most of the earth's crust and some of the atmosphere. While the evidence on the internal evolution of Venus is at best sketchy, there are glimpses in the earth-based radar images of the Venus surface of large impact basins evocative of cratered regions on the moon, Mars, and Mercury and suggestive of surface units older than 3.7 billion years that have seen neither the recycling nor the surface erosion characteristic of the earth. Mars, smaller and lower in uncompressed density than the earth and Venus, appears to have had a different history still. Mars also differentiated a core and a crust, but the crust is relatively thicker than on the earth and shows no evidence for present or past plate tectonics. The surface displays a remarkable hemispherical asymmetry, with ancient and elevated terrain in the south and relatively younger and generally less elevated terrain, of primarily volcanic origin, in the north. The extensional tectonics on Mars, less well developed than on the earth, can be explained by global thermal expansion and point to cooler initial temperatures for Mars than for the earth. Is the earth unique in having plate tectonics dominate a large fraction of its evolution? The answer is of profound importance in understanding the mechanism of plate dynamics on the earth, including the roles of early heat sources and of the volatile content of the mantle. Differences in the compositions and in the early histories of earth, Venus, and Mars are, as with the volatile inventories, linked closely to the conditions in the primitive solar nebula during planetesimal formation and planetary accretion and to the different distances of the three planets from the sun. Thus understanding the nature of these differences is a major step in the direction of understanding the origin of the solar system.

The earth is still outgassing into the atmosphere its primitive gases trapped in the interior and is circulating in a vigorous fashion volcanic and sedimentary materials over its exterior. Further, the earth's crust is being cyclically produced and destroyed from melting in the interior. The basic question is to understand these processes on earth and other planets and whether they are typical of terrestrial planets with substantial water content and internal heat sources.

The earth displays a large internal magnetic field, whose source resides within the earth's central metallic core. The origin of the field is most commonly attributed to a convective dynamo in the core's fluid outer regions. The earth's magnetic field generally acts as a shield against cosmic rays. Venus and Mars possess fields smaller by at least three orders of magnitude than that of the earth, yet both bodies are expected to have highly conducting cores. If large portions of the cores in Venus and Mars are fluid, why are the internal magnetic-field strengths so much smaller than on the earth? The answer should yield a more general theory for the origin of planetary magnetism.

The best evidence to date indicates that life evolved on the earth probably within the first billion years of its existence. Is liquid water the essential ingredient on earth that allowed simple abiogenic organic molecules to generate the complex proteins and nucleic acids that form the basic elements of living organisms? Have tectonic processes enabled essential nutrients to be recycled from seafloor sediments back to the biosphere and thus sustained the long-term evolution of life on earth? What are the special conditions on earth that have allowed essentially continuous shielding of delicate organisms from destructive ultraviolet radiation? In what way does life, once it exists, respond to, influence, or manipulate its inanimate environment? These are central questions about the earth the answers to which must also incorporate the fact that life has apparently not evolved on Venus or Mars.

The development of a better understanding of the factors governing climatic trends over time scales of thousands of years or longer has recently become a topic of considerable interest and importance. In fact, we know surprisingly little of the detailed history of climatic change on earth and even less about the mechanisms that underlie it. The central problem in contemporary climate theory is not in enumerating possible external climatic influences, which is relatively easy to do, but in assessing the way the atmosphere-hydrosphere-biosphere system, with all its possible degrees of freedom to interact, responds to these external influences. Thus, the study of feedback cycles that can amplify or damp initial perturbations assumes considerable importance. The external influences and feedbacks involved in the development of the ice ages on earth, in the generation of ice caps and dust storms on Mars, and in the evolution of the massive CO_2 atmosphere and sulfuric acid cloud layer on Venus represent comparable climatic phenomena that can and should be studied in parallel. There are central questions about the climatic regime in which we live today that do not have adequate answers. Can relatively small perturbations of the atmosphere, such as increasing the atmospheric concentrations of the minor species CO_2 and N_2O, lead to changes in climate? What is the role of dust in affecting climatic conditions? Will the responses that we develop to these questions be able to explain the contemporary circulations and surface climates on our nearest neighbors?

An event of unusual importance took place during the Pleistocene ice ages, the most recent of the earth's extreme period of climatic instability. It was the origin of man, the maker of tools, and the concurrent rapid evolution of involved cultures. Since man and his culture are potent forces of planetary modification, a much clearer understanding is needed of how intelligence evolved and how the evolution of cultures has been guided by the peculiar environmental conditions of the last few million years.

We seek to understand the earth, its present workings, and its past, not only because it is our own planet and therefore unique, but also because such understanding clarifies for us what has happened or might be happening on other planets in our solar system and on planets in systems still unknown. In addition, without an understanding of the present and the past of the home of mankind we cannot, with confidence, predict how our actions will impact the future.

OBJECTIVES FOR MERCURY

In many ways Mercury is unique among the planets. It is the planet closest to the sun, has the highest mean uncompressed density of all the planets and, by inference, the highest fraction of metallic iron-nickel. Mercury serves as a boundary condition to many cosmochemical theories, since it may be the most refractory of all the planets. Thus it is an important end member in testing our ideas about the origin and early history of the inner solar system, about the generation of planetary magnetic fields, and about the processes of planetary differentiation, volcanism, and tectonism.

In addition to its intrinsic scientific interest, Mercury provides the best probe in the solar system for very sensitive tests of gravitational phenomena. Both the strong local effects of the sun's gravitational field and cosmological effects such as a possible change in the constant of gravitation with time can be tested by accurate tracking of an orbiter and/or lander. Mercury's proximity to the sun also makes it a useful base for solar measurements.

Current State of Knowledge

Mercury has been studied from earth using the full range of the available electromagnetic spectrum, and a reconnaissance level study of the planet has been accomplished by three Mariner 10 flybys. The planet appears to be in a stable spin-orbit resonance. Earth-based optical, thermal, and radar data indicate that Mercury is covered with a relatively thick layer similar in physical properties to the lunar regolith. Surface temperatures range from a maximum of 600 K to a minimum of less than 100 K at the equator and vary with latitude. Mariner 10 imaging of about half of the planet at 1- to 10-km resolution revealed large basins, heavily cratered terrain, and smooth plains, having similarities to certain morphologic units on the moon. Some surface features, such as scarps, are the result of tectonic activity with possible implications for global contraction. Mercury possesses an exceedingly tenuous atmosphere much like that of the moon; it has a well-developed magnetosphere, which accelerates particles, and a sizable magnetic field of internal origin. The magnetic intensity at the surface of the planet is several hundred gammas, and the field seems to be dipolar with its axis aligned roughly with the rotation axis.

Data on the chemistry of Mercury are at best scanty. From its uncompressed density, about two thirds of the planet by mass may be iron-nickel. From whole-disk spectral reflectance measurements, Mercury on the average appears similar to mature lunar soils with low FeO content, a result consistent with Mariner 10 multispectral imaging.

But for the indication from J_2 that Mercury is not in isostatic equilibrium, information on the planet's internal structure is nonexistent. There is no direct information on whether Mercury has a core or a crust.

Scientific Objectives

The primary planetary objectives in the exploration of Mercury for the period 1977-1987 are to determine the chemical composition of the planet's surface on both a global and regional scale, to determine the structure and state of the planet's interior, and to extend the coverage and improve the resolution of orbital imaging.

SURFACE CHEMISTRY

Determination of the major chemical components of Mercury's surface and the regional variation in chemistry on a scale of better than 200 km is a task of the highest priority. The chemical analysis must consist of at least two of the following measurements: (a) determination of the bulk elemental composition of at least one sample of Mercury's surface to an accuracy of 0.5 atomic percent for all the principal elements; (b) determination over at least half of the planetary surface of the absolute values, to at least 10 percent accuracy, of five or more independent abundance ratios involving the elements C, O, Na, Mg, S, Al, Si, Ca, Ti, Ni, and Fe; (c) determination over at least half of the planetary surface of the absolute abundances, to at least 10 percent accuracy, of the radiogenic elements K, Th, and U.

The bulk chemical analysis of Mercury's surface will provide a critical test of current cosmochemical theories, which predict that the planet should be enriched in refractories and severely depleted in volatiles with respect to the other inner planets. Geochemical mapping of the surface will address the question of whether the Mercurian smooth plains are basaltic, distinct from the uplands, and of probable volcanic origin. Geochemical mapping of unfilled basins offers the potential for detecting vertical variations of crustal chemistry to a depth of several kilometers below the surface. The unique possibility exists that samples of the planet's deep interior lie exposed near these basins.

INTERNAL STRUCTURE AND STATE

Determining the extent and state of a dense central core in Mercury is another objective of top priority. Two types of experiments that can satisfy this goal, in increasing order of difficulty, are geodetic and seismic.

The polar moment of inertia C/MR^2 for Mercury can distinguish between an undifferentiated planet and full core-mantle differentiation if it can be determined to an accuracy of at least 0.02, or about 5 percent, and as a useful test of internal structure models if it can be determined to within a smaller uncertainty.

Seismic sounding can determine the depth of structural boundaries in the planet, can ascertain whether a central core is solid or liquid, and can provide valuable constraints on interior chemistry and mineralogy. If natural seismic sources are used, at least three seismometers separated by at least 1000 km must operate simultaneously on the planet's surface to assure event location. A potential problem is that the level of quake activity is unknown and the number of large meteorite impacts during the course of an experiment is not likely to be large.

Landing a single three-component seismometer near one of Mercury's poles would be an economical first step in the seismic exploration of the planet's structure. At a minimum, such an experiment would yield a measure of seismic activity. If more than one kind of seismic wave can be identified on event records, then some information on the spatial distribution of seismicity and on planetary structure would also be obtainable.

IMAGING

Further orbital imaging of Mercury should be extended to include the rest of the planet not photographed by Mariner 10. Contiguous coverage with a resolution of several hundred meters will address the possible occurrence of volcanism and the tectonic and impact histories of Mercury's surface. The resolution for at least some of the imaging should be 100 m or better to enable the study of surface morphological processes. Earth-based radar imaging of Mercury's surface should be conducted as a first step in acquiring the global map.

Secondary planetary objectives of Mercury explorations are (1) further exploration of Mercury's magnetosphere and internal magnetic field, (2) measuring global heat flow, and (3) conducting gravity and topographic surveys of the planet.

MAGNETIC FIELD AND MAGNETOSPHERE

Exploration of Mercury's magnetic field and the magnetosphere can be taken together as a single objective. An important goal is mapping the average magnetic-field and charged-particle fluxes through the magnetosphere. The temporal variations of the magnetic-field and charged-particle fluxes should be measured throughout the magnetosphere.

HEAT FLOW

Measurement of planetary heat flow would be an important adjunct to chemical analysis because it allows extrapolation of surface concentrations of U, Th, and K to the planetary interior. An accuracy of at least 20 percent in the determination of heat flow and a resolution of 500 to 1000 km are desirable to test cosmochemical predictions for heat-source abundances.

GRAVITY

Doppler tracking of an orbiting spacecraft to map gravity anomalies (e.g., mascons) is a straightforward experiment that should be a part of any Mercury mission. Simultaneous altimetry mapping will permit more powerful interpretation of gravity anomalies and an evaluation of regional isostasy. Topographic mapping of the planet by earth-based radar should be also encouraged.

Conclusion

A number of these primary and secondary objectives require that a single spacecraft to conduct these investigations be able to achieve a circular orbit of the planet. At present, the U.S. capability is limited to ballistic-type launches, and the opportunities for such launches to insert an appropriately instrumented payload into a circular orbit of Mercury are precluded by one or more of the following constraints: small injected payload masses, infrequent launch windows, long flight times because of the need to fly past Venus to acquire gravity assists, and the present capability to achieve only highly elliptical orbits, which would seriously degrade the resolution and coverage of surface chemistry, surface imagery, and heat-flow experiments. Low-thrust propulsion systems offer the potential to inject large payloads into Mercury orbit at frequent opportunities without strong restrictions on orbit configuration and to reduce the flight time to approximately half of those of ballistic launches. However, since there is some uncertainty as to when during the next decade a new propulsion capability will be available, undertaking the next investigations of Mercury must remain indeterminate.

Should a low-thrust system become available and open the possibility of further Mercury investigations within the period 1977–1987, *we recommend that the planet then be included as an element in the sequence later in the decade and that the objectives as stated be adopted as the guideline for mission planning to implement the strategy, but with the proviso that initiating such a mission does not inhibit or detrimentally affect the primary emphasis on the triad earth–Mars–Venus.*

Important scientific objectives not directly related to planetary science that should be elements of Mercury investigations are (1) testing both local and cosmological predictions of general relativity by precision spacecraft tracking and (2) conducting important solar-physics measurements, which require long-term close proximity to the sun.

OBJECTIVES FOR THE MOON

As a result of the knowledge gained from the Apollo program, the moon has provided a major impetus to our understanding of the inner solar system. The lunar surface has preserved a record of events dating back almost to solar-system formation. Isotopic and stratigraphic dating of impacts on the moon has provided a foundation for a surface chronology for all the terrestrial planets. Fundamental processes in the early history of the terrestrial planets, including large-scale differentiation, crustal formation, and internal volcanic activity, are now better known because of the unique collection of well-documented lunar samples and of the large body of pertinent geologic, chemical, and physical data. The Apollo sites provide ground truth to a host of remote geochemical and mineralogical sensing tools that can be applied to other planetary objects. By virtue of its close proximity to the earth, the moon cannot be completely separated from any class of objects in which the earth is included, particularly as the moon may preserve the earliest record of planetary evolution within the triad earth–Mars–Venus. The moon serves as a focus for comparative planetological studies since it is an exceptionally volatile-poor body, which evolved completely in the absence of a major atmosphere or water. It has largely completed its planetological evolution and thus provides clues to the evolution of planets in general.

State of Understanding

From the rich variety of measurements and observations conducted on the lunar surface, from remote sensing on earth and from lunar orbit, and in terrestrial laboratories on the diverse suite of lunar rock and soil samples from six Apollo and three Luna landing sites, a picture of the moon as a complex planetary-size object has emerged. It has a thick low-density crust and has undergone extensive internal differentiation. Only an upper bound may be placed on the size of a possible small dense core. The moon had an extended history of igneous activity, including filling of the mare basins by basalt flows over at least a 500-million-year time span and is now tectonically very quiet. The surface records a period of intense meteorite bombardment ending about 4 billion years ago. Lunar rocks and large segments of the lunar crust possess remanent magnetization, but there is currently no discernible global dipole magnetic field. Highland and mare rock types are well characterized by returned sample studies and by limited geochemical mapping by the Apollo subsatellites.

The moon is deficient in many volatile elements compared with the earth and shows no evidence for containing water at any time in its history. Seismicity on the moon is low by terrestrial standards, but there are numerous deep and occasional shallow moonquakes, many of which appear to be generated by tidal stresses. Heat flow has been measured at two sites, and the values are about one third to one fourth of the mean earth heat flow. The moon's figure is not one of hydrostatic equilibrium; the thick lunar lithosphere is capable of supporting the nonisostatic stresses associated with the mascons in circular maria.

There is a nearside-farside asymmetry in the occurrence of maria and in the apparent crustal thickness. There are important regional-scale differences in the ages and chemistry of mare surfaces and in the major and trace-element chemistry of highland areas.

Thus while many questions about the moon have been answered, many more have been raised. Global coverage of surface chemistry and mineralogy, of heat flow, and of gravitational and magnetic fields is incomplete. The deep structure of the moon and the detailed structure of the crust (e.g., mare basin geometry) are known only poorly at best. The origin of large magnetic fields at the lunar surface 3 billion to 4 billion years ago is unknown. The assessment of the global importance of returned rock suites and the identification of all major unsampled rock types remain to be accomplished.

The Apollo program provided a rich dividend of scientific data. The optimum return from that endeavor can only be realized by a complete global exploration of the moon.

Scientific Objectives

The primary scientific objectives for exploration of the moon by spacecraft in the period 1977-1987, in order of importance, are (1) to determine the chemistry of the lunar surface on both a global and regional scale; (2) to determine the surface heat flow on both a global and a regional scale; and (3) to determine the nature of any central metallic core in the moon.

GLOBAL CHEMISTRY OF THE LUNAR SURFACE

Global geochemical and mineralogical mapping of the moon with a lateral resolution of 100 km, comparable to the dimensions of major geologic features, can provide valuable insight into the evolution of the moon. Chemical mapping should include (a) determination of the absolute abundances of the elements O, Si, K, Fe, Th, and U with sufficient sensitivity to determine their concentrations in a surface of Apollo 11 mare materials to an accuracy of 10 percent of their value and (b) determination of the elemental ratios Al/Si and Mg/Si to an accuracy of 10 percent. Additional elements for which accurate abundance measurements are highly desirable include Ca, Ti, and the rare-earth elements. The lateral variation of highland chemistry bears on the earliest history of the moon, including planetary differentiation, crustal formation, and nature of the nearside–farside asymmetry and possible highland volcanism. Variations in mare chemistry bear on the evolution of basalt source regions with space and time. Chemical measurements near large unfilled basins offer the possibility of extending chemical information to depth within the crust.

Further, reliable isotopic age determination, precise petrochemical analysis, and petrographic study of highland material from the far side of the moon are considered keys in tying our information about the surface chemistry of the near side to the far side through the more general study of the global chemistry. Determining this relationship may require sample return from the far side of the moon.

GLOBAL HEAT FLOW

The heat flow on the moon should be mapped at a horizontal resolution at least comparable with that of chemistry and should be measured to an accuracy of at least 20 percent. Determination of the mean heat flow of the moon would have a vital impact on our current thinking about the thermal evolution and present thermal state of the moon and the bulk abundances and distribution with variation of heat flow. Its correlation with surface abundances of Th, U, and K and with geological features would provide additional important constraints on the magmatic evolution of the moon.

STRUCTURE OF THE DEEP INTERIOR

Evidence on the presence of a central metallic core in the moon has been equivocal to date. A definitive determination of the existence and size, or of the absence, of a lunar core would help considerably in resolving theories of the paleomagnetic field on the moon and would strongly constrain the history of lunar differentiation and thermal evolution. A low-frequency electromagnetic sounding experiment, involving simultaneous magnetic-field measurements with magnetometers on the surface and in near lunar space, has the greatest likelihood of deciding the question of a lunar core. Magnetometer accuracies of better than 0.1 gamma (10^{-6} Oe) and some monitoring of the plasma environment of the magnetometers are required.

Additional secondary objectives of global exploration of the moon are (1) to map magnetic-field anomalies in the vicinity of the lunar surface and relating the anomalies to geological structure, (2) to measure gravity and altimetry for studies of isostasy and global asymmetry of crustal structure, and (3) to search for possible volatiles frozen into cold traps at the lunar poles.

U.S.-U.S.S.R. Cooperation

Both the United States and the Soviet Union have active programs of lunar exploration. Lunar samples from Apollo and Luna missions have been exchanged for a variety of scientific studies.

The technological accomplishments and capabilities of the lunar programs of the two nations are different and complementary. The Soviet Union has pioneered in the development of unmanned landers for return of rock and soil samples from the moon and is likely to continue its program of unmanned lunar exploration. The United States, with the Apollo program ended and with no plans for developing an unmanned sample return capability for the moon, has concentrated on developing instruments for conducting remote geochemical and geophysical measurements from lunar orbit and has already developed refined methods for the analysis of returned samples.

The moon thus is a strong candidate target for a cooperative U.S.-U.S.S.R. planetary exploration venture. Coordinated missions that would take maximum advantage of the complementary technological skills of the two space programs are a Soviet sample-return mission to the lunar farside and a U.S. geochemical-geophysical lunar orbiter that identifies interesting geochemical provinces as possible landing sites and subsequently serves as a relay for communication with the lander. *We recommend that formal negotiations to explore the possibility of such coordinated lunar missions be initiated.*

FIELDS AND PARTICLES FOR INNER SOLAR SYSTEM

It is now widely recognized that many objects in the universe generate internal magnetic fields. It is likely that the basic principles governing magnetic-field generation are similar for a large class of such objects.

However, only for solar-system bodies can direct measurements be carried out against which we can test our theoretical ideas.

Apart from the remanent magnetic fields carried by permanently magnetized rocks, planetary magnetic fields are thought to be generated by the motions of electrically conducting fluids in the planets' interiors. Thus knowledge of any planet's magnetic field provides an important clue as to the planet's thermal and physical state. Conversely, in order to interpret unambiguously the origin of a planet's magnetic field—particularly a weak field—it is important to have independent information about the planet's thermal and physical state. At present we have only a tentative knowledge of magnetic fields of the inner planets other than earth. *We recommend that the determination of the strength and character of the internal magnetic fields (both global fields and, where possible, small-scale remanent fields) be an important goal in the exploration of planetary bodies.*

The interplanetary medium and extended planetary magnetospheres constitute the only large-scale, astrophysical plasmas that are accessible to direct investigation. Knowledge gained through the study of solar-wind and magnetospheric plasmas is essential to the sound development of our ideas about the behavior of many diverse natural objects. Thus, in common with other areas of solar-system exploration, exploration of solar-system plasmas is important for the particular details learned about our near environment and equally important for the contribution that it makes to our understanding of the general processes that shape the universe.

Direct exploration and study of the interplanetary medium, its particles and electromagnetic fields, in the inner solar system has been carried on for about 15 years. A great deal is known about the near-earth interplanetary medium, but numerous important and fundamental problems remain to be elucidated. Also, but for Pioneer 11 and a few inferences based on radio scintillations and comet-tail studies, regions farther than a few degrees from the solar equatorial plane are totally unexplored.

The properties of the sun and interplanetary medium are known to be time-dependent. Both the modulation of galactic cosmic rays and the intensity of geomagnetic activity vary with a period equal to that of the solar cycle. Cosmic-ray modulation is thought to reflect certain average properties of the solar wind and its embedded magnetic field. Up to now neither the magnitude of the modulation nor its time dependence is well understood in terms of the measured average properties of the interplanetary medium. Resolution of this mystery will require extensive and long-term measurements of the solar wind, its fluctuations, and its three-dimensional structure, along with appropriate measurements of energetic particle fluxes. Other questions include detailed transport properties of astrophysical plasmas, the propagation and dissipation of waves in the collision-free, but turbulent, interplanetary medium, the occurrence and nature of magnetic-field-line reconnection, particle acceleration, and the detailed way in which the interplanetary medium originates as an emission from the sun.

Planetary probes in the inner solar system spend up to several years in transit and in orbit and may traverse as much as 1 or 2 AU in radial distance from the sun. A Mercury orbiter will reside for a long period of time within 0.4 AU of the sun, thus facilitating some otherwise difficult solar measurements. In the case of a comet encounter, the probe may penetrate space to a moderate distance from the solar equatorial plane and to high solar latitudes. Thus planetary probes make excellent vehicles from which to conduct limited studies of particles and fields in the inner solar system. *We recommend that both cruise and orbiting phases of planetary missions be utilized for the conduct of important interplanetary and solar measurements when such an approach offers a significant increment over other available techniques and with due regard for a balanced approach to the first-order goals of such missions.*

The interactions of the solar wind with various planetary objects provide the opportunity to explore a number of major problems of long-standing interest. Of the four major planets in the inner solar system, two have extended magnetospheres and two have atmospheres that interact more or less directly with the solar wind. Comparative study of these planets will facilitate disentangling the fundamental principles that govern such interactions. Among the physical processes that will be addressed are the dynamics of magnetic-field reconnection, charged-particle acceleration, and mass exchange between the solar wind and planetary atmospheres. *The Committee regards an understanding of the fundamental processes governing the solar wind's interaction with planets as a major goal of solar-system exploration and recommends that a global characterization be obtained of each planet's interaction with the solar wind.*

MINOR BODIES INVESTIGATIONS

COMPLEX regards the study of the minor bodies as central in our attempts to elucidate the conditions that led to the origin and evolution of the solar system, and, consequently, the *in situ* study of these objects is an essential component in a long-term strategy for planetary exploration. For the purpose of this report, *comets* and *asteroids* constitute the minor bodies. For completeness we also include a discussion of *meteorites* in this section. While not planetary objects that lend themselves to *in situ* study by spacecraft, meteorites are technically small or minor bodies. Their place of origin and relation to asteroids and comets is not known completely, their physical impact on the surface of terrestrial planets is readily apparent, and their role in the solar system and particularly in the evolution of planets is of no less importance than that of the asteroids or comets.

Comets

It is commonly thought that comets are among the most primitive remnants of solar-system condensation and accretion processes and that they represent the results of these processes in the outer part of the proto planetary nebula. According to several schools of thought, cometary-type objects are one of the fundamental building blocks of the larger planetary bodies. As a result of the small sizes of cometary nuclei, internal evolutionary processes are thought to be unlikely. Comets spend the greatest part of their lives in cold storage in the so-called "Oort cloud" at large distances from the sun, where they are thought to undergo few, if any, collisions. Altogether it is commonly believed that the material of comet nuclei remains nearly unaltered since the earliest history of the solar system, when planetary bodies were first accumulated. Elucidation of the structure and composition of comets is an essential step toward understanding the early processes of solar-system condensation and accretion.

Comets interact strongly with the solar wind. At present, our understanding of this interaction is in a rudimentary state. Detailed study of the gas dynamics and plasma physics in the vicinity of comets will contribute substantially to our comprehension of the behavior of astrophysical plasmas. It will also help to fix our ideas about the mechanism of interaction between the solar wind and neutral atmospheres in general.

Comets have been the subject of extensive observations aimed at studying their orbits, spectral characteristics morphology, and temporal behavior. Although there is evidently considerable variation in the overall properties of comets, no acceptable categorization has been achieved. The canonical picture that has emerged is that a comet consists of a nuclear agglomeration of icy material with a large amount of embedded dust and rocky substances. When a comet approaches within several astronomical units of the sun, the influx of solar radiations causes volatile gases to sublime from the surface of the nucleus. The outflow of gas carries along particles of dust and ice. That inhomogeneity is a major characteristic of the nucleus, as is indicated by the asymmetrical and episodic nature of enhanced gas emissions. As the gas molecules leave the vicinity of the nucleus, they become dissociated and ionized and take part in a plasma-dynamical interaction with the solar-wind flow and are eventually swept along with the wind to form an ion tail extending into a nearly straight line in the antisolar direction. There is at present no quantitative understanding of this process. In addition, the dust grains are also accelerated away from the nuclear region by the pressure of sunlight, eventually to form a separate dust tail. A characteristic mass-loss rate for well-developed comets is thought to be as large as tens of tons per second.

The major structural units of a comet are the nucleus, which may range from about a tenth to several kilometers in radius and which may never have been observed directly in an active comet; the multicomponent coma, consisting of the visible gas and dust with a radius of some 10^5 km, the solar-wind interaction region extending to a radius of about 10^6 km; the hydrogen coma extending to 10^7 km; and the tail region with a longitudinal scale of some 10^7 km. Although comets are frequently accompanied by swarms of meteoroid fragments, recovered meteorites have not been firmly identified with such objects. It is not known if any meteorites are fragments of comet nuclei.

The major outstanding questions surrounding comets are the place and nature of their formation, the subsequent evolution of their orbits, and the extent of their contribution to the major solar-system bodies, including their role in cratering processes, their genetic relationship, if any, to asteroidal objects, meteoroids, and meteorites, and the nature of their interaction with the solar wind. The composition and structure of the nucleus, along with its possible remanent magnetization, can be expected to make a uniquely valuable contribution to our picture of the early solar system.

We regard the nucleus, the coma, and the solar-wind interaction region as the primary targets for *in situ* investigation because the knowledge to be gained there has the broadest identifiable significance. While the actual region of space defined by this target will vary from comet to comet, in terms of the canonical picture described above the primary target consists of a region of some 10^6 km surrounding the nucleus. There is also significant information to be gained from the more distant reaches of the tail, and we regard exploration of the tail as a secondary target.

Important and useful measurements of the nucleus include its size, mass, shape, rate of rotation, phase function, structural features, chemical composition, surface temperature, spectral character, albedo, and reflectance polarization properties.

The large amount of material flowing out from the nucleus provides the opportunity to gather and analyze both gaseous and particulate components. Useful measurements will include distribution and abundances of both ionized and neutral molecular species, as well as the size and space distribution of dust and particles. Effort should be made to obtain key isotopic abundances. For example, a measurement of the $^{12}C/^{13}C$ ratio to an accuracy of 10 percent may help to distinguish between an early solar-system origin of comets and a more recent origin in the interstellar medium. Techniques should be refined for chemical analysis of the dust and particles.

The structure of the magnetic field, the distribution and flow of solar-wind plasma and ionized gases, measurements of plasma turbulence, electric fields, and energetic particles throughout the targeted exploration region are necessary to define the nature of the solar-wind interaction.

Asteroids

The relevance of asteroids to our ideas about solar-system cosmogenesis is analogous to that of comets. Asteroids are also among the relatively primitive bodies of the solar system. Unlike comets, asteroids are thought to still remain close to their place of formation. Also unlike comets, the surfaces of asteroids are directly accessible to remote astronomical measurements. Considerable effort has been expended in spectral-photometric comparisons of asteroid surfaces with meteorite materials. The results have been a number of tentative identifications between several classes of meteorites and several classes of asteroids. Since meteorites are directly available for intensive laboratory analysis, the validity of this identification is of considerable interest. A sound knowledge of the parent bodies of meteorites is necessary in order that the full significance of meteorite studies can be realized. In several respects the identification between asteroids and meteorite samples is incomplete. It is important to realize that meteorite samples are selected in at least two ways. Their orbital kinematics must be consistent with earth impact, and they must be physically capable of surviving penetration to earth's surface. The latter constraint on physical strengths is particularly important since a knowledge of the full range of physical strengths of planetesimal material is necessary to our understanding of accretion and fragmentation processes.

There is evidence that some asteroids, and the parent bodies of some meteorites, have undergone some metamorphosis since their formation. The range of evolutionary changes appears to be substantial. On the one hand C-type asteroids and chondritic meteorites are thought to have undergone little change since their accretion. On the other hand, metallic objects, for example, are thought to have gone through a thorough differentiation. Knowledge of the heat sources that have driven asteroidal differentiation and an understanding of the reason for such diverse amounts of differentiation are important in building our picture of early solar-system processes. Asteroids have undergone substantial fragmentation; deep cross sections of large asteroidal bodies are likely to have been uncovered by catastrophic collisions. Meteorites possess substantial remanent magnetization, suggesting early magnetic fields as intense as 1 G. Comparison with the intrinsic magnetization of candidate parent bodies offers one possible means of discrimination. Beyond that, knowledge of the distribution of permanently magnetized bodies in the solar system is important to our understanding of the origin of the magnetizing fields and may have a bearing on dynamical processes in the proto planetary nebula.

The number of remote astronomical techniques that may be brought to bear on asteroidal science is becoming exhausted. While these techniques are being applied to an increasing number of objects, continued growth in our knowledge of early solar-system bodies will eventually require direct exploration. Spacecraft measurements will permit accurate determinations of masses, sizes and shapes, resolution of structural features and chemical and mineralogical inhomogeneities, determination of chemical composition, and remanent magnetization. Possible techniques for probing the depth of an asteroidal regolith should be explored. Because of the diversity of asteroid characteristics, a reasonable strategy for eventual exploration must necessarily make provision for studying a representative set of objects. It is a minimum requirement that the level of science return from any asteroid mission be substantially greater than that necessary to test definitively the tentative identifications between asteroid and meteorite classes.

Meteorites

Until the advent of space missions, meteorites were the only extraterrestrial objects accessible for intensive laboratory investigations. Results from these studies played an important role in the planning for remote analysis of the lunar surface by the Surveyor missions and for the in-depth analysis of lunar samples returned by the Apollo missions and the Luna missions. While the source of meteorites is not well known, they appear to form a series of connecting links between the larger planetary bodies. Meteorites still represent the most diverse samples of extraterrestrial planetary matter currently accessible for laboratory studies and thus have continued to be a unique ingredient in our thinking about the nature and history of the planets and the formation of the solar system. The availability of samples from all over the world for such investigations, including new falls and major discoveries of meteorites in the unexplored Antarctic by Japanese and American expeditions, has invigorated these investigations. The search for meteorites, their preservation in special collections, and their distribution to the scientific community is a tribute to the cooperation of museum curators and scientists irrespective of nationality.

Experimental and theoretical work on meteorites is continuing to yield new and exciting results of the highest scientific importance relating to the nature of the planets and their evolution. Recent discoveries of the chemical, isotopic, and petrologic nature of certain meteorites are strongly affecting our views about the conditions in the early stages of the solar system and the processes of planetary evolution.

COMPLEX strongly recommends continued intensive studies on meteorites as a parallel effort to the study of lunar samples, to earth-based observations of the solar system, and to space missions in the study of the planetary system. These studies should be continued using advanced techniques. In addition, the development of new techniques should be encouraged, not only because of their contribution to meteoritic studies but also because of their role in understanding lunar processes, in defining new experiments for space missions, and the potential of maximizing the scientific results from the study of samples returned from terrestrial planets.

Conclusions

Exploration of these minor bodies with spacecraft poses new challenges and, in the case of comets, a number of centrally important measurements appear to be unique to this endeavor. *In order to develop properly the scientific objectives and the relative place that direct exploration of these objects should have in the overall strategy, as separate bodies and as a class of objects, COMPLEX recommends that a special study be carried out during the next year.* The study results will provide a basis and guideline for evaluating candidate missions proposed for small body exploration.

In the foregoing discussion, COMPLEX has defined the areas of primary scientific interest for the *in situ* exploration of comets and asteroids. To support the study, COMPLEX will require a substantial amount of information, which must include quantitative assessments, in detail, of the measurements in this area that can be accomplished and comparison of the quality of these measurements with the quality required to have a substantial impact on our present state of knowledge. This information needs to be developed in a comparative way, taking into account the efficacy of improved ground-based and near-earth orbit observations and the variety of mission modes available to an encounter. NASA has already undertaken a study of experiments for a possible comet mission, and we expect that a continuing assessment of possible comet experiments will be undertaken by the Agency.

COMPLEX calls attention to its 1975 recommendation "*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 investigation can be properly assessed in the framework of the current strategy.*" The prompt implementation of this recommendation and its expansion to include asteroids is essential to provide the required information to undertake the study of minor bodies.

5
Relationship of Inner- and Outer-Planet Strategies

Although the Committee has developed separately the strategies for inner- and outer-solar-system exploration, the separation was one of schedule convenience, and it is essential not to lose sight of the broader context into which these two studies fit: characterization of the current physical state and evolutionary history of the solar system as a whole and the chemical and the physical processes that have controlled its evolution.

As elaborated in this report, the principal focus for an inner-planet exploration strategy is the intercomparison of Venus, earth, and Mars, to establish the reasons for their diverse evolution. The opportunity to compare objects with differing compositions, size, and internal energy is our strongest test of models for inner-planet evolution. The satellites of the outer planets represent a continuation of this spectrum of terrestrial bodies and extend the range of compositions and initial energies beyond the values characteristic of the inner planets. Thus the outer-planet satellites represent one extreme among the terrestrial bodies in our solar system; examination of these bodies in the context of models for inner-solar-system evolution will profoundly broaden the basis of our understanding, ultimately, of the nature and history of the earth.

The 12 large solid bodies in the solar system, whose diameters are a few thousand kilometers or larger, include the four terrestrial planets, the moon, Pluto, and six of the satellites of the outer planets. These bodies span an enormous range of heliocentric distance and conditions of formation. Current models for the condensation and accretion of the material of these bodies generally agree that the temperature of formation of Mercury was above

1200 K, that of Mars near 400 K, that of the Galilean satellites of Jupiter near 100 K, and that of Pluto near 30 K. All available evidence suggests an enormous range in composition for these bodies, with consequent wide variations in their thermal histories, melting and differentiation sequences, and present internal structure.

Their vast compositional difference, however, should not obscure the fact that these bodies all share a near-simultaneous origin from a single reservoir of source material. Thus information on the origin, evolution, composition, and structure of any of these bodies when investigated as a system, has a profound impact on our understanding of the individual components.

Jupiter, Saturn, and Uranus are all known to possess regular satellite systems characterized by coplanar close satellites in very low-inclination, low-eccentricity orbits. This phenomenon has long been interpreted as evidence that these objects accreted in their present orbits and may sample primitive materials accreted at these large heliocentric radii. The concept of these systems accreting in orbit around the giant planets closely resembles our theories of origin of the planets of the inner solar system from nebula. The analog is reinforced by the gradient in composition radial to Jupiter shown by the densities of the Galilean satellites. Their densities range from 3.5 for Io, the closest, to 1.5 for Callisto, the farthest from Jupiter. Such densities suggest compositions ranging from silicate chemistries characteristic of the terrestrial planets to 50 percent mixtures of ices and rocks reflecting condensation temperatures ranging from several hundred kelvins down to 100 K. The outer satellites, Ganymede and Callisto, with their uncompressed densities of 1.4 $g\text{-}cm^{-3}$, are clearly largely made of ice-forming substances, notably "H_2O" and possibly NH_3. Bulk compositions capable of matching the observed densities have large enough abundances of rocky material so that the radionuclides therein will provide a large internal source of heat. It can be shown that, irrespective of the detailed assumptions regarding original temperatures and the internal distribution of radionuclides, extensive melting of the icy component will occur in less than a billion years for bodies as large as Ganymede and Callisto. Present-day thermal steady-state models for these bodies are characterized by their ice crusts, deep convective mantles of water or aqueous NH_3 solution, and a core of silicate–sulfide oxide minerals containing the heat-producing elements. Whether due to soluble salts or to dissolved ammonia, the electrical conductivity of the aqueous mantle will surely be high. The electromagnetic consequences of the presence of a convective, conducting fluid are of obvious interest and importance. Convection will, of course, have a strong influence on the behavior of a thin icy crust. It is entirely possible that the phenomena of crustal rifting and drift of crustal plates are as important on these bodies as they are on earth.

We have described a few of the fundamental ways in which information on the bulk composition of the Galilean satellites will be of great significance in the comparative study of planetary origins. Certain selected examples of processes in common, such as melting and differentiation, crustal tectonics, mantle convection, magnetic-field generation, photolysis, atmospheric and hydrospheric production and loss, and interaction with planetary magnetospheres, are all naturally raised even by the meager evidence available at present. For comparative studies, the limited number of terrestrial planets has been a severe hindrance. Extending the comparison to include the Jovian and Saturnian systems doubles the number of bodies, makes available two additional primary satellite systems, and extends the range of all relevant parameters governing their formation and evolution. Because of the close proximity of the Galilean satellites to one another and because of their orbital commensurabilities, a single spacecraft can subject all four bodies and their primary to comparative studies with the same set of experiments.

The opportunity to commence systematic comparison of the inner and outer planets has been outlined in the respective recommended strategies. When implemented, the strategies will, at a minimum, connect the exploratory study of the triad (Mars–earth–Venus) to the Galilean satellites and further to the first reconnaissance view at Uranus and its satellites. It is anticipated that subsequent strategies will specify more extensive exploratory missions and thereby extend the comparative framework beyond the orbit of Jupiter.

6
Scientific Instrumentation Development

COMPLEX is seriously concerned that adequate Supporting Research and Technology (SRT) funds be made available for scientific research and development in NASA centers, universities, and industry and that these funds be utilized in a fashion that is consistent with maintaining a vigorous scientific capability in both experimental and theoretical areas of space sciences. Such activities must be sustained and in consonance with the long-term mission objectives of the space-science program. The Committee calls attention to the fact that there is a serious deficiency in the efforts and the level of funding directed toward the conception and development of new scientific instrument capabilities, which are of general applicability in planetary exploration within the framework of the strategy recommended by the Space Science Board. The intermittent funding in key areas of scientific research and in the conception and breadboard development of instruments has had a debilitating effect on the overall scientific capability and the proper execution of a sound program of planetary exploration. This effect will increase, since, as program elements move from reconnaissance to exploration to intensive study phases, new concepts and innovations will be required. This issue was also addressed in the 1975 report.

Implementation of the strategies and objectives recommended in this report and in the 1975 report require prompt efforts in developing certain instruments and techniques. As representative examples we list the following:

1. Continued high-priority development of imaging sensors for use on flybys and orbiters. Of particular interest is demonstration of imager performance from surface penetrator afterbodies and other hard landers.

2. Development of other penetrator-hard lander science packages, particularly those to conduct seismic, inorganic geochemical, hydrocarbon content, total water, and temperature-gradient-heat-flow investigations.

3. Development methods to determine definitively *in situ* the mineral phases present in planetary rocks and soils.

4. Development of new organic and inorganic geochemical techniques for use on soft landers-rovers. Study of techniques to acquire and prepare samples of planetary surfaces and to implant instruments from roving vehicles.

5. Continued development of x-ray and gamma-ray instruments for orbital missions.

6. Study of possible techniques to obtain basic chemical and physical information during high-velocity and very-low-velocity encounters with comets.

7. Study of potential techniques to study the surface of Venus and Mercury, especially efforts to develop high-temperature electronics and sensors.

8. Study of techniques to determine atmosphere-surface interactions and chemical reactivity on Mars and Venus.

9. Development of visible-near-infrared spectral reflectance instruments to study ice and rocky surfaces.

10. Evaluation of new techniques to study atmospheric compounds and particulates.

11. Development of second-generation instruments for the Space Telescope and for Shuttle sortie use such as 1-50 μm ir high-resolution spectrometers, visible-near-ir high-resolution spectrometers, and visible-near-ir low-resolution spectrometers.

12. Study of techniques for Mars sample return, which retain the biological, chemical, and physical integrity of the sample. Prompt initiation of a study of a sample receiving laboratory for the handling, processing, and analysis of returned planetary samples.

13. Development of techniques to determine *in situ* the oxidation state of iron in soils and rocks.

The Committee is aware that significant expertise in instrument development and measurement techniques exists throughout government agencies, in industry, and in the universities, which may be applicable to the national space-science program. *We recommend that a mechanism be established by NASA to bring this knowledge to bear on planetary problems more effectively. Instrumentation studies, organized by NASA, which focus on specific instrument requirements and on present and future problem areas, would be an effective first effort.*

These recommendations clearly require a NASA management decision to begin the development of new instrumentation *before* flight projects are approved. We strongly recommend that immediate action be taken in this matter. If it is not possible to solve this problem, there will be no means of ensuring adequate, defensible flight programs for the immediate future. A formal Advanced Instrumentation Program (AIP) within NASA, funding appropriate investigators at the NASA Centers, within industry, and at universities, seems essential to direct the timely long-term planning required to provide adequate instrumentation to achieve mission science objectives. Should the Agency decide to implement this suggestion by establishing AIP as a program line item, we request that an annual review of its progress and achievements be provided to the SSB and its committees as a reference resource to guide the yearly evaluation of the overall space-science program and the assessment of mission planning to fulfill the strategy.

7
Postmission Data Analysis and Supporting Research

In the 1975 Report on Space Sciences the Space Science Board recommended that "future missions contain as part of the original mission cost sufficient funds for data reduction and analysis beyond the flight mission stage in order to ensure that the scientific return is reasonably exploited." NASA has taken some steps in this direction with the post-Apollo Lunar Program and the post-Viking study of Mars. We strongly endorse this approach and believe that only by such extended studies will it be possible to produce mature scientific product. This postmission activity should properly collate and assess the data obtained during the flight mission and integrate these results with a broader conceptual framework of planetary sciences.

The interrelationships between results from individual spaceflight missions must be recognized, assimilated, and applied by the general scientific community. The extension of studies beyond the flight mission and the inclusion of a scientific constituency extending beyond the flight experimenters will greatly aid in connecting the links between the planets and between missions and in exploiting interdisciplinary studies of comparative planetary meteorology, geology, physics, and chemistry, which must be established and maintained.

Comparative planetology is a new type of interdisciplinary activity, which requires the participation of individuals from many different fields ranging from cosmic-ray physics to a generalized form of geology. It is generally recognized that modern comparative planetology is a direct outgrowth of the space program. As a consequence, there exist no other sponsoring agencies that can reasonably bring together and monitor the new and diverse activities that comprise modern planetology. *We therefore recommend that NASA establish a vigorous and ongoing program of data analysis and synthesis, which is designed to foster interdisciplinary and comparative planetological research.* Such a program should include both theoretical and experimental research concerning both present and past states of the solar system. In making this recommendation we believe that comparative planetology should be formally recognized as an activity within NASA in order to provide sound follow-on to the successful planetary missions. It is not our intent to recommend indefinite postmission data analysis or studies related to any particular mission. It is the intent, however, of our recommendation that sufficient postmission studies for a particular mission be carried out to guarantee an adequate level of data analysis and interpretation subsequent to which the efforts should be directed toward integrating the overall scientific activity within the broader framework of planetology. To assist in this effort, *we further recommend that NASA arrange for each mission science team to provide within a reasonable time frame a comprehensive report of its experiments and experimental results along with extended interpretations of these observations.* This will give the general scientific community access to the fundamental data base in a collated form. The document on each planetary mission, which includes the summary of the data and summarizes what was learned and how this advance in knowledge affected or changed our previous state of knowledge, will be of great importance in a wider scientific community.

8
Evaluation of Proposed Programs

LUNAR POLAR ORBITER

We reaffirm the policy expressed earlier (*Opportunities and Choices for Space Science, 1974; Report on Space Science, 1975*) that a lunar polar orbiter to conduct global geochemical and geophysical studies of the moon is an important element of inner-solar-system exploration and will substantially enrich the scientific return from all comparative planetology programs, as well as from the Apollo program. The mission proposed to conduct high-resolution mapping of geochemistry and mineralogy will address questions of the mode and scale of lunar differentiation that are primary to an understanding of formation and early history of the crusts on all the terrestrial planets. Measurement of global heat flow by microwave radiometry would provide a vital measure of bulk radioactivity and a major boundary condition to interior thermal structure. High-resolution magnetic anomaly mapping and the detailed study of farside and polar gravity and topography will yield valuable clues to crustal evolution processes and to the origin of lunar magnetism. Further, the possibility exists that new results will confirm a central metallic core or lead to the discovery of water ice in polar cold traps.

The Committee wishes to note that mission studies and planning in preparation for a proposed new start for a Lunar Polar Orbiter mission should be cognizant of the relative position of lunar exploration in the ten-year strategy recommended herein. Further, the possibilities for coordination and cooperation in U.S. and U.S.S.R. lunar-exploration objectives should be fully explored and taken into account in mission studies and planning.

INTERDISCIPLINARY SCIENTIST STATEMENT

In considering the role of the scientific community for the purposes of studying and analyzing data from a deep-space mission, it is the view of COMPLEX that during the postflight phase "members of a more general scientific community as well as the experiment team should be involved in the study and data analysis" (*Report on Space Science 1975*, National Academy of Sciences, Washington, D.C., 1976).

Extending this point of view, COMPLEX has discussed the desirability of a broader participation by interdisciplinary scientists *during* a mission. As a result of extensive discussions, *COMPLEX unanimously agrees that every proposed planetary mission should follow the lead of Atmosphere Explorer and Pioneer Venus in having interdisciplinary scientists appointed to the mission Science Steering Group.* These people should have the status of Principal Investigators and an equal vote or a voice on the Science Steering Group. Their most obvious role during and after the mission is to organize cooperative investigations using data from more than one experiment. They can have, however, a different and perhaps even more valuable part during the planning and early operational phases. Here they can assist project scientists to maintain balance among the experimenters and group of experiments and ensure that the scientific return is optimized. They should bring a broad scientific view to the mission and see that priorities are maintained in their proper order. In the case of choosing alternate mission profiles, the interdisciplinary scientists can aid the scientific decision-making process without a personal commitment to the use of a particular instrument. The presence of interdisciplinary scientists on a steering group should greatly aid the project scientist in keeping the major mission objectives in proper perspective.

We concur with the procedure used on Atmosphere Explorer and Pioneer Venus to select interdisciplinary scientists on the basis of their response to an announcement of flight opportunity (AFO) and endorse it as standard practice in the issuance of AFO's for all future missions.

9

Status of Existing Programs

JUPITER ORBITER PROBE

The Jupiter Orbiter Probe (JOP) mission (now called Galileo) is regarded as an essential component of the near-term strategy consistent with our principal 1975 recommendations for exploration of the outer solar system. The *in situ* measurements to obtain vertical structure, composition, thermal state, and cloud distribution in the atmosphere of a major planet are a crucial component of the highest priority in the scientific goals of outer-planet exploration. Experimental approaches that focus on the geological, geochemical, and geophysical characteristics of the major satellites and on detailed surveys of the Jovian particle-field environment constitute the other primary goals of the mission. It is vitally important, now that the mission has been approved, that the instrumentation and mission profile be carefully designed and selected to focus on the major scientific objectives.

Progress in preparation for this mission is in general satisfactory; however, there are two areas of concern. It is the first planetary mission that will involve the new transportation system, the Shuttle Interim Upper Stage (IUS). The consequence of uncertainties relative to the schedule of any such new system are made more serious in this case because of the narrowness of the optimum launch window, the possibility that weight limitation may make unrealizable potential pre-entry science on the Jupiter Probe, the need for a Mars flyby for trajectory assist at the 1981-1982 opportunity, and the unavailability of an adequate alternative launch system.

The Committee is aware that the estimated maximum capability of the Shuttle/IUS system and the availability of the new spin-despin orbiter spacecraft makes launch windows to Jupiter accessible at approximately 13-month intervals. The Committee recognizes the added degree of flexibility that this new capability provides to mission planning; however, we must point out that the frequency of opportunity to combine an orbiter and atmospheric probe, the instrument designated to achieve the highest priority goal for outer planet exploration, is approximately eight years, the next available launch window being late 1981-early 1982.

It is disappointing that apparently the instrumentation needed to fulfill adequately the second most important objective of the mission–the investigation of the chemical and physical state of the Jovian satellites–is not adequately identified nor available. To some extent, this may be a reflection of the difficulty in carrying out the remote chemical analyses, particularly in the radiation environment of Jupiter. However, it is our view that new and innovative approaches might well address this type of problem. It is a case in point of the funding deficiency in Supporting Research and Technology to support development of instruments of wide applicability and of the need for long-range planning in developing adequate instrumentation for science objectives. In spite of this, the JOP mission promises to fulfill successfully the primary objective of establishing the chemical composition and many properties of the largest planet of the solar system.

VIKING-EXTENDED MISSION

The scientific return from the highly successful Viking Nominal Missions can be augmented significantly by a vigorous, long-range program of Extended Mission studies. *COMPLEX recommends that the meteorology and imaging experiments on both landers and the seismometer on Lander 2 be continuously monitored and that the ranging to the Viking landers be continued for as long as possible.* The radio-science measurements offer the potential for determination of the precessional constant and other important parameters of the solid-body motion of Mars.

Nominal Mission results prove that the quality and resolution of the Viking Orbiter imagery is far superior to that obtained by Mariner 9 in 1971-1972. Unfortunately, the Nominal Mission coverage is limited and needs to be extended to cover as much of the planet as possible. Such imagery is not only of high scientific value but is a practical prerequisite for planning future missions involving Mars rovers. *COMPLEX recommends that NASA maintain adequate ground support (personnel, tracking, computers, and image processing facilities) for the lifetime of the orbiters to ensure a maximum return of imaging data.*

The Committee is encouraged by the implementation of the Viking Guest Investigation Program and *recommends that special emphasis be given to laboratory studies aimed at deciphering the data obtained by Lander biology experiments.*

In February 1977, Viking Orbiter 1 made a series of close passages to Phobos, the inner satellite of Mars, obtaining a measurement of the satellite's mass and mean density. Preliminary analysis indicates a low density consistent with that of the more primitive carbonaceous chondrites, which have low albedos and gray colors closely similar to those of the surface of Phobos.

In view of the far-reaching implications of this result, *COMPLEX recommends that full advantage be taken of opportunities during the Viking Extended Mission to carry out a series of close encounters with the outer satellite, Deimos, in order to determine its mass and mean density.*

PIONEER VENUS

The Committee has updated its review of the preparation for this mission. The only area of current concern involves the orbiter infrared radiometer experiment, which has encountered some development delays. This important experiment, however, is being closely followed by the Pioneer Venus project office to ensure that it will meet the flight schedule.

The success of the U.S.S.R. Venera 9 and 10 missions in 1975 provoked comparisons between the Venera results and the science objectives of Pioneer Venus. On first comparison of these missions, they appear similar, each consisting of an atmospheric entry probe to make measurements to the surface of the planet and an orbiter carrying additional instruments. On closer examination, however, major differences become apparent. In contrast to Pioneer Venus, Venera emphasized surface science from landers ahead of atmospheric science, and its orbiters made no direct measurements of the Venus upper atmosphere. Further, there was no radar mapper on the Venera orbiters. A major feature of the Pioneer Venus mission's experimental payload is the capability to deploy four atmospheric probes, which comprises an instrument array for atmospheric measurements, simultaneously into the Venusian's atmosphere. This experiment typifies a pervasive emphasis on correlated measurements of related phenomena. Pioneer Venus probes will concentrate on detailed measuring of atmospheric composition, cloud properties, and radiation fields.

The Venera emphasis on surface science is illustrated by the fact that many of the atmospheric instruments on the lander-probe were turned off at 34-km altitude. The principal new facts reported on the atmosphere from

Venera measurements are the location of the cloud base at 49 km and a direct determination of surface winds to be 0.5 m/sec. Evidently, serious difficulties have arisen in the interpretation of the Venera mass-spectrometer data on the Venus atmosphere; no results have been reported to date.

Venera orbiter experiments made measurements of the solar wind and the atmosphere from orbital altitudes. The Pioneer orbiter measurements of the atmosphere will be made much closer to the planet, at one point dipping into the upper atmosphere. This low orbit allows use of a full complement of aeronomy instruments and a detailed exploration of the interaction of the solar wind and the upper atmosphere.

A comparison of Venera results and Pioneer Venus objectives has clearly shown that the relative differences in emphasis on science and its related technical competence are not redundant or excessively overlapping. On the contrary, they point out that there are major opportunities for U.S. and U.S.S.R. coordination and cooperation in missions and significant benefits to be derived from information and data exchange from missions carried out independently.

VOYAGER

The Committee has reviewed the technical progress and scientific goals of the Voyager (formerly called Mariner Jupiter Saturn) mission and reaffirms the 1975 conclusion of the SSB that this program is a priority mission of highest scientific merit in exploration of the outer solar system. It represents the most comprehensive reconnaissance mission launched to the outer planets and is the forerunner for embarking on the in-depth exploration of Jupiter and Saturn and their satellites. The science investigations include a broad range of high-quality experiments providing first-order information on the nature of the surfaces, global character, and intercomparison of the satellites; the dynamics, thermal character, and composition of the atmospheres of the major planets and the Saturnian satellite, Titan; characterization of the particles and magnetic-field environments; and valuable information on Saturn's rings.

The Committee has reviewed the potential for retargeting one of the Voyager spacecraft to encounter Uranus in the mid-1980's. The Committee is disappointed that the redesign and testing of an infrared spectrometer (MIRIS) for installation aboard one of the Voyager spacecraft could not be completed prior to launch date. This instrument would have provided the higher sensitivity and broader spectral coverage desired for a Uranus reconnaissance mission. The scientific goals of such a mission, including satellite and atmospheric imaging, exploration of the magnetic field and particle populations, thermal balance of the atmosphere, and characterization of the gravitational harmonics and upper-atmosphere species would be of tremendous importance in advancing our knowledge of the outer solar system. The recent discovery of rings around Uranus by ground-based and aircraft observations of a stellar occultation highlight our ignorance of Uranus and the potential for important basic discoveries. We recommend that every reasonable effort be made to allow retargeting of one of the Voyager spacecraft to Uranus. Uranus and Neptune constitute a special class of bodies in the solar system, differing from Jupiter and Saturn in that they probably contain a large percentage of ice and rock. Characterization of one of the icy-rocky major planets and its satellites is a first-order goal of the current strategy for outer-solar-system exploration.

PIONEER 11

For its 1975 report, the Committee reviewed the inflight status of Pioneer 11 and concluded that most of the spacecraft experiments were functioning well. The mission is currently deep into its extended mission phase, and the Committee is pleased to note that the spacecraft status remains excellent, having well exceeded its design and performance requirements and nominal lifetime expectancies.

The value of the Pioneer 11 scientific role in exploring the outer solar system led COMPLEX to recommend in 1975 "*the coverage of Pioneer 11 be restored to a sufficient level by mid-1979 in order to optimize the science return at the Saturn encounter.*" In addition to the mission objectives for Saturn, Pioneer 11 will also play a vital role as a precursor for the Voyager 1 and 2 spacecraft. The Committee is informed by the Agency of their decision to target Pioneer 11, for its September 1979 encounter, to cross the Saturnian ring plane at the same radius as will Voyager 1 and 2. This decision will provide valuable information on the ring plane environment when Voyager 1 and 2 arrive in 1980 and 1981 and will be the primary basis for a decision to direct Voyager 2 on to a Uranus flyby. For this now enhanced task, we urge that every reasonable opportunity be afforded Pioneer 11 to achieve its mission goals for Saturn encounter, with due consideration of status of performance of the Voyager spacecraft.

ROLE OF LUNAR SAMPLES AND DATA IN PLANETARY SCIENCE

The lunar samples and the data from *in situ* geophysical and geochemical experiments returned by the U.S. and Soviet missions to the moon represent a unique and invaluable scientific resource bearing on the early history and evolution not only of the moon but also of the solar system. While meteorite studies also contribute to our understanding of the early solar system, the lunar samples are uniquely important because they can be related to specific geological features on a terrestrial planetary body with a clearly definable yet primitive history.

Lunar-sample studies have provided us with a detailed picture of the early bombardment and magmatic differentiation, which have shaped the major surface features of the moon and, by implication, other terrestrial planets. In addition, sample studies continue to provide information on the current processes that are modifying the lunar regolith. ALSEP and Apollo subsatellite geophysical and geochemical experiments have provided key data on the nature of lunar crust and mantle and on the interaction of the moon with the particles and fields of the solar wind.

The policy of the United States in making data and portions of samples widely available has internationalized lunar science. There remains a vast wealth of information and new knowledge to be derived from the Apollo lunar samples. The continuing return of lunar material from Soviet missions promises to extend our knowledge to additional unexplored regions of the moon. These samples are becoming available to scientists through a highly successful cooperative agreement between the United States and the Soviet Union. In order to exploit fully the scientific information available from the Luna and Apollo samples, and to maintain or expand the present level of cooperation with the Soviet Union, it is essential that a vigorous lunar studies program be maintained.

Detailed lunar-sample investigations are also providing the impetus to develop new techniques to study extraterrestrial materials. This effort not only further advances our frontiers of knowledge but also provides guidance for the efficient scientific study of future planetary-sample returns. Similarly, as the synthesis of the abundant geophysical and geochemical data from lunar missions continues to refine our ideas on lunar structure, tectonics, and evolution, so it also aids in the planning of future missions to other terrestrial planets. *We view lunar sample research and lunar data analysis and synthesis as essential elements of planetary studies and therefore recommend that these elements continue to receive substantial support as an integral and unique element of the exploration of the inner solar system.*

In the 1976 SSB report, the need for a pristine and safe storage and processing facility for lunar samples was emphasized. NASA has initiated construction of a new and upgraded main curatorial facility for the Apollo lunar samples and has moved a substantial fraction of the lunar collection to a clean and secure storage area at a separate site. We fully endorse these moves to assure adequate preservation of this unique scientific treasure.