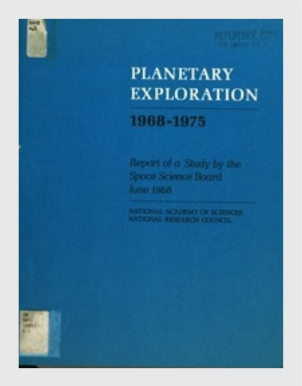

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PLANETARY EXPLORATION

1968 - 1975

Report of a Study by the Space Science Board

Washington, D C., June 1968

National Academy of Sciences - National Research Council

July 1968

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FOREWORD

This is the report of a study convened by the Space Science Board to consider planetary exploration in the period from 1968 to 1975. The group reappraised the recommendations of the Board's 1965 Woods Hole Study in the light of advances in scientific knowledge and changing demands upon the nation's resources.

The study was conducted during the week of June 10, 1968 under the chairmanship of Gordon J. F. MacDonald and involved 23 scientists representing the spectrum of scientific interests in planetary studies. The recommendations of the group were presented to NASA management on the afternoon of June 16 and were discussed and endorsed by the Space Science Board at its meeting on June 24 and 25.

The Space Science Board is grateful to those who participated in this study, to Bruce N. Gregory of the Space Science Board staff who ably directed the study, and to Miss Ann Wagoner, also of the staff, for her contributions to the report's publication. The Board acknowledges with appreciation the support of the National Aeronautics and Space Administration, which helped to make this study possible.

H. H. Hess, Chairman
Space Science Board

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SUMMARY OF PRINCIPAL RECOMMENDATIONS

1. We recommend that the planetary exploration program be presented, not in terms of a single goal, but rather in terms of the contribution that exploration can make to a broad range of scientific disciplines (page 3).

2. We recommend that a substantially increased fraction of the total NASA budget be devoted to unmanned planetary exploration (page 3).

3. (a) We recommend that duplicate missions for a particular opportunity be undertaken only when a clear gain in scientific information will result from such double launches (page 4).

(b) We recommend that NASA initiate now a program of Pioneer/IMP-class spinning spacecraft to orbit Venus and Mars at every opportunity and for exploratory missions to other targets (page 5).

(c) We recommend the following larger missions to Mars: A Mariner orbiter mission in 1971, and a Mariner-type orbiter and lander mission, based on a Titan-Centaur, in 1973 (page 5).

(d) We accord next priorities (in descending order) to a Mariner-class Venus-Mercury fly-by in 1973 or 1975, a multiple drop-sonde mission to Venus in 1975, and a major lander on Mars, perhaps in 1975 (page 6).

4. (a, b) Rather than attempt to define in detail payloads to be carried aboard high priority missions, we have selected several sample payloads (page 6).

(c) We recommend that with regard to Mars and Venus, NASA continually reassess, in the light of current knowledge of the planets, its program, methods, and mathematical model for meeting the internationally agreed objectives on planetary quarantine (page 11).

5. (a) We recommend strongly that NASA support radar astronomy as an integral part of its planetary program. In particular, we recommend that NASA fund the development and operation of a major new radar observatory to be used primarily for planetary investigation (page 12).

(b) We recommend that NASA planetary program planning be closely coordinated with Earth-orbital telescopes being designed for the 1970's and with the infrared aircraft telescopes now under construction (page 13).

(c) We recommend that the NASA program of ground-based optical planetary astronomy continue to receive strong support and that opportunities for planetary astronomical investigations be increased by:

(1) Construction of an intermediate sized optical telescope in the Southern Hemisphere
(2) Construction of an infrared telescope employing a very large collecting area and permitting interferometric measurements at a dry site
(3) Development of new infrared devices, including improved detectors and high resolution interferometers (page 14)

(d) We recommend that steps be taken to facilitate the analysis by qualified investigators of the data secured by the photographic planetary patrol (page 14).

Chapter 1

INTRODUCTION

In the summer of 1965, the Space Science Board undertook a comprehensive review of the scientific opportunities in lunar and planetary exploration.* The purpose of the study was to judge in a broad way the scientific priorities that should be assigned to investigations of the planets and to identify the principal scientific problems that could be elucidated by such exploration. Since 1965, much has happened. As the 1965 study was ending, Mariner 4 encountered Mars and shortly thereafter began to return historic photographs of the Martian surface. In 1967, Mariner 5 and the Soviet probe Venus 4 measured the hot, dense atmosphere of Venus and discovered that the solar wind interacts much more directly with the atmosphere of the planet than it does with Earth because of the lack of magnetism on Venus. Surveyors and Lunar Orbiters, together with their Soviet counterparts, have provided a wealth of detailed information concerning the surface and even the interior of the Moon. In addition, ground-based observations, particularly radar studies, have yielded valuable information on the solar system. Long held views with regard to the rate of rotation of Mercury and Venus have been shown to be incorrect and a puzzling set of phenomena involving the coupling of the spin of a planet to its orbital motion discovered. Radar has also given important new information about the major surface features on portions of Venus and Mars. Thus, the combined efforts of Earth-based and space-based observations have greatly increased our knowledge of the planets since 1965.

However, it was not only the rapid development in science that motivated the Space Science Board to conduct a reappraisal of the 1965 report with regard to planetary exploration. Since 1965, the budget of the National Aeronautics and Space Administration has been cut severely. The level of the present and probable future support that the nation is willing to provide for planetary exploration raises a number of difficult and important issues. What should be the nation's program for planetary exploration in a time of continued budgetary constraints? What portion of the limited resources made available to the space agency should be allocated to planetary studies? What information can be gained by further ground-based experimentation or observations from near-Earth satellites, and what information can only result from space vehicles at or in the vicinity of a planet? These are some of the questions that the present study attempts to assess.

The 1965 study examined both lunar and planetary investigations, clearly pointing out that the two are closely interrelated and that one cannot adequately plan a planetary program without comparable planning for lunar studies. While recognizing the validity of the earlier study's conclusion, the present study examines only opportunities for planetary exploration. Scientific investigation of the Moon involves even more complex issues than those in planetary investigations. Exploration of the Moon in part will be a manned venture, and in this venture scientific considerations are intertwined with questions of national prestige. In view of the uncertainties about the future of the manned lunar program, the present group did not consider in depth the development of a total lunar and planetary program.

The planning of a program of planetary exploration presents special and complex problems. Voyages to the planets are long and can be undertaken only on limited occasions. While favorable times to visit Mars and Venus occur about once every

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* Space Research Directions for the Future: Report of a Study by the Space Science Board, Woods Hole Mass., 1965; Part I - Planetary and Lunar Exploration; National Academy of Sciences - National Research Council Publication 1403, Washington, D. C., 1966.

19 to 26 months, opportunities to use unusual configurations of the planets to carry out more extensive exploration arise only once in a decade or in some cases only once in a century. For example, in 1970, 1973, and to a lesser degree in 1975, it will be possible to use the gravitational field of Venus to assist an Atlas-Centaur launched spacecraft in a flight to Mercury. Exploration of Mercury would not otherwise be possible without employing a very much larger booster. This opportunity will not be repeated until the 1980's. A once-in-a-century opportunity occurs in 1977-1978 when the planets will be so positioned that their gravitational fields can be exploited for a grand tour of the great planets, Jupiter, Saturn, Uranus, and Neptune -- again without the need for extraordinarily powerful boosters.

The rare opportunities for planetary voyages, the length of these voyages, and the long times required for preparing experiments all imply that planning for planetary exploration must take place years in advance of the actual missions. Thus, decisions arrived at this year and next will go far in determining the future character and scope of planetary programs. Further, it should be emphasized that the long times involved in planetary missions demand long term commitments on the part of the scientists participating in these investigations. Quite naturally, scientists are reluctant to make such commitments without assurance that this nation will indeed have a continuing program for planetary exploration. The nature of the planning process and the importance of involving the scientists in the program make it clear that the decision makers within both the Executive and Legislative branches of the government must understand the very special problems faced by NASA and the scientific community in the planning of a planetary program. For these reasons, Planetary Exploration 1968-1975 is addressed in part to the decision makers.

* * *

The report consists of seven chapters. Chapters 3 to 7 discuss the individual scientific disciplines relevant to planetary exploration -- Atmospheres; Surfaces; Dynamics and Interiors; Particles, Fields, and Interactions with the Solar Wind; Exobiology -- and suggest the experiments and programs best able to deal with their special questions. These chapters were prepared by small groups of participants in each discipline, making use of position papers prepared and circulated in advance of the study. Using these chapters as the basis for discussion, the study group as a whole agreed upon the priorities and recommendations set forth in Chapter 2.

Chapter 2

RECOMMENDATIONS

1. Goals of the Planetary Program

The 1965 study identified three goals for the nation's planetary program. The planetary program should be designed to provide for progress in our understanding of:

(1) The origin and evolution of the solar system
(2) The origin and evolution of life
(3) The dynamic processes that shape man's terrestrial environment

We believe that these goals remain valid and that all three should be recognized in the development of the national program.

In our view, the program of planetary exploration will contribute in major ways to the growth of science. Exploration of the planets is not an end in itself. In isolation, the discovery of new facts about the planets has little intrinsic interest. Scientific interest in the planets lies in the expectation that investigation of their atmospheres, surfaces, and interiors will contribute greatly not only to unraveling the complex history of the solar system and problems of how life originated and developed, but also to an understanding of the Earth and of the processes which today take place in the atmosphere, oceans, and deep interior.

Our view is that no single goal, such as the determination of whether life exists in other parts of the solar system, should be set for the planetary program. Rather, it should be emphasized that the scientific return from planetary exploration will flow into many areas of science and thereby strengthen them. Furthermore, there is every reason to expect that continued development of scientific understanding of the planets will lead to benefits for all mankind. For example, since the 1965 Woods Hole study, a series of investigations of the lower atmospheric dynamics of Mars has followed the preliminary determination by Mariner 4 of the character of Martian atmosphere. None of these studies is conclusive but nevertheless a remarkable relevance to terrestrial studies is revealed. Investigation of the Martian atmosphere has focused attention on a number of important aspects of the Earth's atmosphere that were not receiving the attention they deserved. On Mars, energy is transferred within the atmosphere through radiation. This mechanism is also important on Earth but to a great extent has been neglected. The interaction of the lower levels of the Martian atmosphere with the surface is of great importance in controlling the motions of the atmosphere. Greater understanding of these interactions on Earth is being gained as a result. In the case of the atmosphere of Mars, there is thus a direct and demonstrable connection between study of that atmosphere and a better understanding of our own. From this better understanding, we can perhaps expect advances in the techniques of long-range weather forecasting and eventually the development of effective methods for weather modification. The significance of the exploration of the Martian atmosphere to studies of the Earth is by no means unique: we can expect further advancement in the understanding of our own planet as a result of the study of the other planets.

Because of the rich contribution that planetary exploration can make to a broad range of scientific subjects, we recommend that the planetary exploration program be presented not in terms of a single goal but rather in terms of the contribution that that exploration can make to a broad range of scientific disciplines.

2. Level of Support for Planetary Exploration

The 1965 study recommended that an increasing fraction of the space program's resources be devoted to planetary exploration. The basis for that recommendation was the expected richness of the scientific return from such increased efforts. Since 1965, a number of groups external to NASA have made similar strong recommendations. In 1967, the President's Science Advisory Committee recommended that the exploration of planets and space astronomy should be primary objectives toward which the U. S. space program is oriented during the post-Apollo period.

In the coming fiscal year, planetary exploration will receive at most 2 percent of the total space budget. We believe that this amount is totally inadequate to take advantage of the opportunities available to us. The importance of planetary investigations is that these explorations provide information that cannot be secured by Earth-based studies. We cannot form an experimental planet with a light atmosphere composed largely of carbon dioxide to determine how its motions are influenced by radiation. We cannot easily reproduce the conditions that have existed on the surface of Mars over billions of years to determine whether life forms could develop. Instead, we can take the existing planets and attempt to secure information most relevant to some of the great scientific problems of our time. The uniqueness of planetary studies makes any comparison of the relevant benefit of expenditures devoted to these and Earth-based studies not only difficult but in many cases irrelevant. Study of the planets can provide understanding about major questions that could never be achieved solely by Earth-based investigations.

Today we have developed the technology to place scientifically meaningful payloads near or on the planets and have developed the instrumentation with the long lifetimes required to carry out the complex planetary missions. We believe that we must take advantage of these developments. While not competent to argue the complicated questions of national prestige, we certainly believe that we cannot abandon a broad area of space activities to our competitors. Therefore, we recommend that a substantially increased fraction of the total NASA budget be devoted to planetary exploration.

3. Priorities in Planetary Exploration

At present, the planetary program is limited by resources rather than by technology, lack of competent scientists, or important ideas. The real fact of resource limitation makes it essential to examine the question of priorities with great care. In this examination, we have been strongly guided by the concept that planetary exploration will strengthen broad areas of science, as has been emphasized above.

(a) Redundancy

In the past and in current planning, it has been customary to build considerable redundancy into a particular mission. This redundancy takes two forms. It is usual to plan for two spacecraft to accomplish a given mission. In addition, backup spacecraft for use on the ground are also manufactured. Several of the original reasons for such redundancy have vanished. Technological advances have made failures infrequent, whether at launch or during the mission. The many space endeavors have reduced the prestige of a single space shot and the public no longer requires success at every opportunity. Planetary exploration is no longer a primitive and risky art but rather a highly developed technology. In view of these developments, and with the great need to conserve resources, we recommend that duplicate missions for a particular opportunity be undertaken only when a clear gain in scientific information will result from such double launches.

We do not find persuasive the argument that an additional launch costs only 20 or 30 percent more than the single launch. For the expensive planetary missions that have been proposed, this add-on is sufficiently large to support significant other missions. However, in view of the funding already committed to the 1969 Mariner Mars fly-by missions and the clear gain of covering different portions of the planet, we recommend that NASA proceed with this part of its program.

(b) Small Spacecraft for Planetary Missions

Of central importance in the future of planetary exploration is the initiation of a series of relatively small and inexpensive spacecraft to orbit Venus and Mars, primarily, and to make exploratory missions to Jupiter (as currently planned in the Pioneer F and G missions) and perhaps to Mercury and to comets. Such a broad and well conceived series of opportunities for space experiments, with flights to the nearer planets at each opportunity, will make it possible to reach and maintain a high level of scientific interest and participation in the planetary program. This is particularly vital during these times of uncertainty concerning support of the larger missions. A continuing program with numerous opportunities for planetary research using relatively inexpensive planetary orbiters makes it possible to:

(1) Involve a larger number of scientists, young researchers, and graduate students in planetary research

(2) Plan and progress systematically to more ingenious and important experiments as more information is obtained

(3) Respond quickly with a new payload in order to take advantage of new findings

(4) Give groups of experimenters the opportunity to plan for the use of a total integrated payload

(5) Conduct experiments that support or complement larger missions (U. S. and Soviet) or prove out a concept that may have high interest but also high risk

(6) Carry out planetary and interplanetary particles-and-fields experiments on the same missions rather than instrumenting separate spacecraft for the two types of measurements; this will also reduce the load on the tracking facilities of the Deep Space Network

The merits of small planetary orbiters have been discussed for several years and are recognized in particular in the Space Science Board's 1967 "Report of a Study on Explorations in Space with Sub-Voyager Systems." The success of small, spin-stabilized spacecraft has been well demonstrated in the IMP, Pioneer, and other programs. Pioneer A has now continued to operate in deep space for 2.5 years. The technical feasibility of placing such a spacecraft in orbit around an extraterrestrial body was proved when IMP-6 (Explorer 35) was placed in orbit around the Moon in July 1967. Experience from these programs can also be used to estimate costs with a high degree of reliability.

There are both advantages and disadvantages to a spinning spacecraft as compared with a Mariner-class stabilized spacecraft, but recent technical advances in electronically phased antenna arrays, for example, can be used to simplify antenna designs for high gain communications with Earth.

We recommend that NASA initiate now a program of Pioneer/IMP-class, spinning spacecraft for orbiting Venus and Mars at each opportunity, and for exploratory missions to other targets. We endorse the proposed diversion of the existing Pioneer E spacecraft to orbital studies of Venus in 1970 and we recommend that NASA openly solicit scientific experiments for this mission. We strongly support the Pioneer/IMP-class missions for investigation of the near-Jupiter environment in 1972 and 1973. The possibility of taking advantage of the 1973 Venus-Mercury opportunity with a Pioneer-class mission should be carefully examined.

(c) Study of Mars

The Space Science Board and its various panels have on frequent occasions emphasized the great importance of the investigation of Mars for the purpose of detecting possible biological activity. The purely biological studies are of high risk but clearly the discovery of life on Mars would rank as one of the great events of this or any other century. Because of the great importance of such biological investigation we recommend the following for Mars:

Mariner orbiter mission in 1971

Mariner-type orbiter and lander mission based on a Titan-Centaur in 1973

The 1971 Mars orbiter is essential both for the significant information it will gain and for the support of subsequent landing missions. This orbiter will provide the first detailed maps of Mars and its sensors will be able to seek out persistent clouds as well as thermal and color anomalies indicative of water sources. This and other information will be of great value in selecting favorable sites for a lander, in the interpretation of data which a landed capsule will obtain, and in providing the first data on seasonal variations such as the intriguing wave of darkening. Therefore, the 1971 Mars orbiter should be accorded an especially high priority in the planetary program for it will make unique contributions to the search for extraterrestrial life.

We believe that the program outlined above -- small planetary spacecraft and larger Mars missions -- is a minimal program. It is our view that such a program has greater priority both in terms of expected purely scientific returns and in long term benefits to society than other space ventures such as the qualifying of man for planetary voyages (see Section 7).

(d) Other Programs

Our emphasis on obtaining broad knowledge of the solar system leads us to give the next priority to a Mariner-class Venus-Mercury fly-by in 1973 or 1975. We note that the favorable position of the planets that permits this mission will not recur until the 1980's.

Our incomplete knowledge of Venus coupled with the importance of understanding its atmosphere makes a multiple drop-sonde mission in 1975 a very high priority mission.

Finally, we note the importance of a major lander for Mars, perhaps in 1975, which could take advantage of the opportunities opened up by the earlier investigations.

4. Payloads

We have not attempted to structure a detailed program of experiments for each of the high priority missions discussed above. Instead we have examined in some detail possible sample payloads for the small planetary orbiters and for the Mars lander. In the case of the former we wish to demonstrate clearly that it is possible to carry out significant planetary observations with small spacecraft. In the case of the lander, this mission is of such importance that it is essential that detailed investigations of the instrumentation for that payload be undertaken at the earliest possible opportunity.

(a) Payloads for Small Planetary Orbiters of Venus and Mars

The scientific payload for spinning spacecraft of the Pioneer/IMP class placed in orbit around Venus and Mars is on the order of 40 pounds. With such a payload there are a number of exciting possibilities for fundamental studies of the atmospheres and surfaces of these planets as well as of the solar wind interaction regions and the interplanetary medium. With an ongoing program that includes flights to both planets at every opportunity, it is possible to plan both integrated payloads that incorporate a number of related experimental packages of relatively small size and payloads consisting of a single, large instrument for imaging in, for example, the visual, infrared, radio, or active radar bands. There has been some tendency to think of the small, spinning spacecraft as useful only for particles-and-fields experiments, but similar spacecraft in Earth orbit have demonstrated visual imaging capabilities of high quality using the spin of the spacecraft itself to scan the field of view.

A high priority is placed on obtaining visual and infrared images of the Venus atmosphere near the tops of the clouds to help deduce dynamical and physical characteristics of this massive atmosphere. Both remote and in situ measurements of atmospheric constituents are possible for Venus and Mars, with local measurements being emphasized during the time when the orbit decays into the atmosphere.

An early flight to Venus might be used to test the feasibility of obtaining radar images of the surface, using the S-band radio system, with an added instrument to sample wide-band characteristics of radio signals reflected from the surface in the bistatic mode. This could lead to the possibility of obtaining high resolution images of the surface of Venus using the total capacity of a spacecraft of the Pioneer/IMP class in a later flight. The bistatic radar echoes will also be of interest in establishing electrical and roughness properties of the regions on and near the surface. Recent results from the lunar-orbiting Explorer 35 suggest a variable thickness of the rubble layer (the regolith), thickest in the highlands and thinnest in the maria, and having anomalies of extreme thinness apparently correlating with infrared hot-spots.

A high priority is also placed on obtaining synoptic particles-and-fields data in the solar wind interaction region around Mars. Recent results from Explorer 35 and data from the Mariner 5 fly-by of Venus lead us to believe that such a synoptic study of Mars will be of great interest and importance in establishing fundamental properties of planetary bodies and their environment in the solar wind.

The radio links needed for communications have a high potential for scientific use, particularly if they incorporate range and range-rate capability and compatibility with the addition of a second frequency. Pressure and temperature profiles of the atmospheres of Mars and Venus obtained by radio occultation in the Mariner 4 and 5 flights provided our first accurate measurements of these two markedly different samples of the types of atmospheres that can evolve from planetary bodies. In fact, occultation data in conjunction with radar studies from Earth, radio tracking of Mariner 5, and the results of the Soviet Venus 4 probe, have established that atmospheric parameters near the mean surface of Venus at low to medium latitudes and at all times of the day are quite different than was first thought on the basis of the direct Soviet measurements alone. Approximate pressure and temperature values are 100 atmospheres and 700° K, respectively, rather than the 20 atmospheres and 550° K values suggested by the Soviet experiments.

Radio occultation measurements also provided initial profiles of the ionospheres of both planets. The peculiar, sharp, interface between the daytime ionosphere of Venus and the interplanetary medium, together with magnetic and plasma effects noted by Mariner 5, suggest that Venus and perhaps Mars represent a different class of planet, as compared with Earth, in terms of the ways a planetary body may interact with the solar wind. These differences could be of fundamental importance to eventual understanding of the different manner in which the atmospheres of Venus and Earth have evolved to have markedly different characteristics even though the two planets are very similar in size, mass, and distance from Sun. We assume that they probably formed from the same source of matter at about the same time.

The radio measurements will take on added meaning with the small orbiters, as compared with previous fly-bys. The atmospheric and ionospheric measurements would become synoptic, covering large areas of the planets and extending over the lifetime of the spacecraft. Accurate tracking will be used to establish the mass and gravitational moments of the planets. Venus is of particular interest in this regard because of its strange dependence on Earth for locking its rotational period. Accurate tracking at two coherent radio frequencies will also provide data on the solar corona and interplanetary medium, and will be used to improve planetary ephemerides. These same measurements, conducted when the planets are in regions around the opposite side of Sun, will yield information important to fundamental studies of general relativity.

Small Planetary Orbiters: Sample Payloads

Venus 1970, 1972, 1973, 1975. The next opportunity to place a payload in the vicinity of Venus (1970) could only involve existing spacecraft and scientific instruments. Subsequent sample payloads are designed on the assumption that no large Venus-orbiting missions will be conducted during the period under consideration. Detailed payload choices will depend upon previous results and characteristics of the proposed Venus-Mercury fly-by (1973-75) and the Venus drop-sonde mission (1975). In each case it is assumed that the radio tracking and data acquisition system includes range as well as range-rate capability, and provision is made for incorporating a second radio frequency for dispersive measurements. Thus, based on the high precision radio capabilities, occultation measurements of the neutral and ionized portions of the atmosphere will be conducted, as will experiments on celestial mechanics, relativity theory, the interplanetary medium. and the solar corona.

Sample payloads are:

1972

(1) Spin-scan camera (ATS) and tape recorder
(2) IR radiometer, spin scan
(3) UV at 3600 A, spin scan
(4) UV filter photometer
(5) Modules tapped off S-band system for initial bistatic radar studies of the surface (uplink)
(6) Particles-and-fields instruments, as many as can be incorporated, for study of the solar wind interaction region.

1973-75

Depending upon previous results, the total spacecraft may be devoted to bistatic radar imaging of the surface, optical and infrared imaging of the cloud tops, microwave emission (temperature) imaging, or to an integrated set of experiments to study the region of interaction between the upper atmosphere and the solar wind, including measurements of the interplanetary medium at 0.7 AU. In case there is no Mariner-class mission for multiple (three) atmospheric probes, it is important, and may be feasible, to design a small spacecraft for atmospheric entry. An orbiter may include a mass spectrometer to measure atmospheric constituents, particularly during orbit decay.

Mars 1971, 1973, 1975. We assume the radio measurements described above for Venus. If the small planetary orbiter is flown at the same opportunity as a Mariner-class orbiter, possible experiments for the solar wind interaction region, the upper atmosphere, and the interplanetary medium at 1.5 AU from the Sun include:

(1) Magnetometer
(2) Plasma probe
(3) Trapped particle detector
(4) Cosmic ray detector
(5) Micrometeorite counter

For the second and third missions, upgraded and new instruments will be considered.

Should the small spacecraft be the only orbiter, its payload should be chosen from:

(1) Spin-scan camera
(2) IR radiometer
(3) UV photometer
(4) Yellow-filter spin-scan camera
(5) Bistatic radar for surface studies

Particles-and-fields experiments are important, but probably of secondary priority in this case.

(b) Payload for Direct Biological Investigation of Mars (Table 1)

The central purpose of the lander mission is to determine the near-surface environment of the planet and, if possible, whether biological activity is present. The environmental investigations are of direct importance to the life detection experiment but also provide information of interest in other disciplines. We regard the mass spectrometer experiments and the gas chromatograph of particular interest both because of the unique capabilities in this kind of instrumentation that exist in this country and the value of these experiments in interpreting biological observations.

Instrumentation of Recommended Payload

(1) Imaging. This is considered to be a life-seeking experiment on a gross scale. A resolution of a few centimeters at 10 m will suffice for a first lander, although higher resolution is desirable if it can be attained. A high resolution view is needed of the sampling site, preferably while the sampler is being operated.

(2) Sampler. The experiments which need a soil sample (Items 3, 4, 7, and 12) require a total sample weight not exceeding one gram.

(3) Pyrolysis: Gas Chromatograph/Mass Spectrometer. This is a dual purpose instrument. The first use is for atmospheric analysis. A complete analysis of the atmosphere including trace constituents is needed, through a diurnal cycle if possible, to detect thermodynamic unstability (e.g., CH_4 and O_2 present). The analysis of the atmosphere should include helium and the argon and neon isotopes since the relative proportions of these elements could yield information on processes that affected the primitive atmosphere and on the possibility of secondary outgassing, solar wind accretion and solar wind sweeping. The gas chromatograph of this instrument is not the same as that in Item 6 since a column suitable for the separation of organic compounds does not perform satisfactorily with the fixed gases.

The second use of this instrument is for organic analysis. The information to be gained will indicate the presence of organic compounds, permit identification of the classes of compounds present and, in the case of compounds present in high relative amounts, will allow identification of individual compounds. Other valuable evidence suggestive of life is the detection of organic nitrogen compounds since these have not been found in meteorites in significant amounts.

Both the atmospheric analysis and the organic analysis are considered to be life-seeking experiments.

(4) Direct Biology. Experiments in this class involve the detection of growth, photosynthesis, or metabolism in the soil sample. Depending upon the number of controls, the number of different chambers, the number of media, etc., the detectors will range from simple, low-weight (1-lb) instruments to complex, higher-weight (10-lb) instruments. They require at least 3 days' operating time.

TABLE 1

Suggested 1973 Mars Landed Payload, In Order of Priority

Objective	Priority	Functional Range	Est. Weight	Data Bits
Imaging	1	1 gross scan upon landing, 1 high resolution of sampler	4 lb	10^7
Soil Sampler	1	Up to 1 g	2 lb	10^2
Pyrolysis (gas chromatograph/mass spectrometer)	1	10 to 140 amu Dynamic range, 10^6	16 lb	10^5
Direct Biology	1	Growth/Metabolism	10 lb*	10^3
H_2O Detector	1	Sensitivity, 10^{-5} mb	1 lb	10^3
Gas chromatograph	2	Atmosphere: H_2, He, CO, N_2, N-oxides, CO_2 HCN, O_2, NH_3, CH_4, C_2H_2	4 lb	$2x10^3$
Differential thermal analysis	2	10 mg at 0.01% H_2O	1 lb	$2x10^3$
Penetrating subsurface H_2O probe	2	-	3 lb	10^3
Soil temperature	3	$\pm1^{\circ}$C	0	10^3
Air temperature	3	$\pm1^{\circ}$C	0	10^3
Air pressure	3	±0.1 mb, range to 30 mb	1 lb	10^3
Element analysis	3	X-ray fluorescence X-ray diffraction	20 lb	10^4
Neutron probe	3	Permafrost(?)	3 lb	10^3
Air velocity	4	Range up to 200 km hr^{-1}	2 lb	10^3

*A 1-lb life detection experiment may be feasible.

(5) <u>Water Detector</u>. A knowledge of the amount of water present at the landing site is crucial since water is considered essential for biological processes. This instrument measures water vapor. One additional water detecting experiment (Item 8) will determine the presence of a permafrost layer while a second additional experiment (Item 7) will analyze for water in the soil sample.

(6) <u>Gas Chromatograph</u>. This is a "gap-filling" instrument which analyzes the atmosphere for those gases that are difficult to analyze with the mass spectrometer or which lie outside its mass range. These gases are H_2, He, CO, and N_2. Since the column would separate other possible atmospheric gases, the capability of the instrument can easily be extended to include the other gases listed in the table.

(7) <u>Differential Thermal Analysis</u> (see Item 5). This instrument, which includes a water vapor sensor at the output of the DTA unit, will allow positive identification of water in the soil sample, including liquid water, ice, water of crystallization, and hydroxyl water.

(8) <u>Penetrating Subsurface Water Probe</u> (see Item 5). This is a self-contained and self-powered probe which penetrates the soil and gives indication of a permafrost layer. Physically, it is part of the neutron probe (Item 13)

(9) <u>Soil Temperature</u>. A knowledge of the temperature cycle of the soil at the landing site is useful in describing the environment.

(10) (11) <u>Air Temperature and Pressure</u>. Measurement of air pressure and temperature during at least one diurnal cycle is desired to determine whether liquid water can exist at the landing site.

(12) <u>Element Analysis</u>. This analysis will yield environmental data as well as information concerning planetary differentiation.

(13) <u>Neutron Probe</u> (see Item 8).

(14) <u>Air Velocity</u>. This measurement will help to determine whether aeolian erosion may be occurring at the landing site. It will also contribute to the description of the biological environment.

(c) <u>Sterilization of Probes for Venus and Mars</u>

Direct impact probes are the most reliable and economical devices for exploration of the lower atmosphere of Venus. The development of this type of mission for the U. S. space program may have been inhibited in the past by the need to meet international sterilization requirements. It now seems likely that these sterilization requirements can be satisfactorily fulfilled. Probes which detach from the main bus and which can impact in regions where surface temperature is uncertain can be heat-sterilized.

Uniform heat sterilization of all components of the bus should not be necessary. For the probes, an equatorial impact point can be chosen where the surface temperature will exceed 700°K. Clean construction, heat sterilization where possible, surface sterilization of all components, and the surface ablation during entry should then be sufficient to reduce the probability of contamination to acceptable limits.

As information about Mars continues to become available a similar reassessment of sterilization requirements for this planet will also be desirable.

We <u>recommend</u> that with regard to Mars and Venus, NASA continually reassess, in the light of current knowledge of the planets, its program, methods, and mathematical model for meeting the internationally agreed objectives for planetary quarantine.

5. Ground-Based and Near-Earth Observations of the Planets

In the years since the 1965 study, ground-based observations have produced a wealth of new information on the planets. We expect that the returns from investments in such studies will continue to be high. We urge that NASA pursue an active ground-based planetary program.

(a) Radar Studies of the Planets

Since 1961, considerable strides have been made in our knowledge of the nature of the terrestrial planets, much of which has come from ground-based observation. Earth-based radar observations of Mercury, Venus, and Mars have been especially fruitful. In this Section we review briefly the major contributions made using this technique and outline extensions of the work that are possible with more sensitive radar systems. In view of the large number of remarkable discoveries already obtained by radar and its high potential for further advances, we advocate its support as part of the NASA planetary program. In particular we have in mind greater use of the Deep Space Network and improvement of its capability for radar observations, the upgrading of other radar facilities, and the construction of new ones.

We believe that planetary exploration can best be carried out with a proper mix of space-probe and Earth-based observations. Those data of importance that can be obtained less expensively with, say, ground-based radar equipment ought to be so obtained. Such a cost-effective approach is especially important in a period of stringent budgetary limitations.

Past Accomplishments. The study of planetary dynamics has benefitted enormously from the extreme precision -- several parts in 10^9 -- of radar measurements. The value of the astronomical unit, in particular, has been refined by almost five orders of magnitude. Perhaps most startling have been the discoveries of the spin-orbit resonances of Mercury and of Venus. Further, these results from Earth-based radar allow sensible inferences to be made about the existence of a fluid core in Venus.

Radar refinements of the mass and radius of Mercury have established its density as equal to the Earth's within 1 to 2 percent, and provide important constraints on models of Mercury's internal composition. The accurate determination by radar of the radius of Venus, coupled with the Mariner 5 results, demonstrate that the Venus 4 probe did not transmit from the surface and that the surface temperature and pressure are closer to 700°K and 100 atmospheres than to 550°K and 20 atmospheres, the Soviet values. The absorption by the Venus atmosphere of high frequency radar waves observed from Earth-based measurements also favors strongly the former set of surface conditions. Clearly these radar results are of enormous importance to future investigations of the lower atmosphere and surface of Venus.

Radar has also played an important part in studies of planetary topography, surface roughness, porosity, and composition. Notable discoveries include large altitude variations (12 km) on the surface along 21°N latitude on Mars, and the near-circularity (variations < 2 km) of the equatorial region of Venus. The only maps of the Venus surface, albeit at present with very poor resolution (100 to 200 km), have been produced using Earth-based radar and show a large number of features. It has been found that the surface of Venus backscatters radio waves about twice as efficiently as either Mercury, Mars, or the Moon, indicating a more compact surface or one made of a material with a higher dielectric constant. Surface slopes on the scale of the radar wavelength have been shown to be smaller on Venus, further accentuating the differences. Similar comparisons among the Moon, Mercury, and Mars are possible from the present measurements.

Future Potential. The potential of radar for planetary exploration has not been fully exploited. New facilities, with improvements in sensitivity of nearly three orders of magnitude over existing radar systems, can be built for relatively modest capital expenditures. With one such facility, detailed radar maps of Mercury, Venus, and Mars can be prepared, the Venus map having a resolution of 1 to 2 km which is comparable to that achievable in Earth-based optical photographs of the Moon. (A radar interferometer can be used to resolve the twofold ambiguity present in a map made with only a single radar receiver. The size of the second receiver is not critical.)

Radar studies of the Galilean satellites of Jupiter will be possible, giving information on their rotation rates, surface properties, and densities. These measurements will also permit enormous improvements in the knowledge of the orbits of the satellites and of the orbit and gravitational field of Jupiter. Radar echoes from Jupiter may not in themselves be detectable because of its very deep atmosphere. However, the upper regions of the Jovian atmosphere and magnetosphere can be studied by ground-based radar using signals reflected from the Galilean satellites near occultation with the planet. The largest satellite of Saturn, Titan, will also be accessible to radar study as will the two tiny moons of Mars. The inner planets themselves will be easily detectable at all points in their orbits, allowing important extensions of existing studies. For example, the Martian topography can be determined over the entire latitude and longitude region spanned by the subradar point. The important values of the fractional differences in the equatorial moments of inertia of Mercury and Venus -- vital to an understanding of their spin-orbit couplings and their interiors -- can be determined from radar measurements of the physical libration of these planets. Several asteriods and perhaps some comets can also be brought under surveillance by such a new radar system. In all cases, the knowledge gained from the radar investigations should aid significantly in arriving at the best choice of instruments for more detailed exploration with space probes.

In view of the past accomplishments and future potential of the radar technique, we recommend strongly that NASA support radar astronomy as an integral part of its planetary exploration program. We further recommend that NASA fund the development and operation of a major new radar observatory required for the investigations discussed above.

This observatory should provide an increase in sensitivity of about 10^3 over existing systems, and such an increase seems readily obtainable for a capital expenditure of the order of $30 millions.

(b) Planetary Observations from Near-Earth Orbit

The NASA planetary program should take account of the Earth-orbital telescopes that are being planned for the mid-1970's. These all-reflecting telescopes having apertures of about 1 m, are being designed for diffraction-limited performance at $\lambda = 5000$ A and will therefore have a limiting resolution of 0.1 arc sec. High resolution imagery can be obtained particularly in the wavelengths from 2000 to 8000 Å and long term observations of planetary surface and atmospheric detail on the planets will be possible.

For the mid-1970's it appears that fly-bys and orbiters are practical for only Venus, Mars, and Jupiter. In all these cases, a fly-by or orbiter is capable of at least 100 times more resolution than an Earth-orbital telescope, but over only a rather small fraction of the planet's surface at a given time. Therefore, the Earth-orbital telescopes should complement these detailed photographs by planet-wide photographs taken at appropriate intervals. High resolution photography of planets from the Earth's surface at periods of exceptionally good seeing should also be vigorously pursued.

The largest contribution of Earth-orbital telescopes to planetary imagery would appear to be imagery of the major planets. With the exception of Jupiter, fly-bys of these planets cannot be expected until at least 1980. Furthermore, the relatively long exposures required for these objects, particularly Uranus and Neptune, make it more difficult to obtain ground-based photographs during intervals of good seeing.

Many other solar system objects are photographically resolvable with a 40-inch diffraction-limited telescope. These include Pluto, about ten of the largest asteriods, Titan, and the Galilean satellites of Jupiter. The latter have angular diameters of about one arc sec and should exhibit surface features on photographs having a resolution of 0.1 arc sec.

Ultraviolet spectroscopy from Earth-orbiting telescopes should also be used to complement spectrographic data obtained by planetary missions. In the next few years, three Orbiting Astronomical Observatories (OAO's) are scheduled to carry a variety of instrumentation for wide-band photometry and spectroscopy. Although primarily designed for stellar observations, each of these OAO's can give valuable supporting information on the planets.

Attention should be directed toward the feasibility of specially modifying the Small Astronomical Satellite (SAS) for optical planetary observations.

High resolution (0.1 to 0.3 Å) ultraviolet spectroscopy of the planets, utilizing the high spatial resolution of a diffraction-limited 1-m telescope would be extremely valuable. Instrumentation now being designed should allow such spectra to be obtained in the mid-70's.

At present, there are no active plans to perform infrared spectroscopy of the planets from Earth orbit. However, NASA is equipping a Convair-990 jet aircraft with a 36-inch telescope, stabilized to about one arc sec, and to be flown at about 40,000 feet. There appears to be very little water vapor about this level with the result that the infrared spectrum is nearly completely transparent below 25 μ and partially transparent above 25 μ. This new facility, scheduled to become operational in 1970 or 1971, should be vigorously utilized for obtaining ir spectra of the planets at moderate spatial resolution (1 to 2 arc sec).

In light of these considerations, we recommend that the NASA planetary program planning be closely coordinated with the Earth-orbital telescopes being designed for the mid-1970's and with the infrared aircraft telescopes now under construction.

(c) Ground-Based Optical Planetary Astronomy

We commend NASA for its program of ground-based optical planetary astronomy, and for the construction of new telescopes being undertaken to further this program. The effectiveness of the program would be greatly increased by the addition of a Southern Hemisphere observatory: A number of planets will be in the southern sky three to ten years hence with the result that high resoltuion spectroscopy from northern observatories will be hampered by the large masses of air through which observations must be made. Moreover the highly successful technique of interferometric spectroscopy should be extended to the fainter planets and comets. We recommend that this program continue to receive strong support and that its capabilities for planetary astronomical investigations be increased by:

(1) Construction of an intermediate-sized optical telescope in the Southern Hemisphere

(2) Construction of an infrared telescope employing a very large collecting area and permitting interferometric measurements at a dry site

(3) Development of new infrared devices including improved detectors and high resolution interferometers

(d) Photographic Planetary Patrols

We commend the implementation of a worldwide photographic planetary patrol, but we are concerned that the data obtained will not be sufficiently evaluated. Therefore, we recommend that steps be taken to facilitate the analysis by qualified investigators of these data for such things as presence and motions of clouds on Mars, Venus, Jupiter, and other planets, the seasonal wave of darkening on Mars, and unusual phenomena.

6. Interaction of the Planetary Program with the Scientific Community

At present, only a few groups in the country are directly involved with the scientific portion of the planetary program. There is, however, widespread interest; we believe that the program could be greatly strengthened by increased involvement. We have recommended certain steps -- institution of the small planetary orbiter program -- to help. Further initiatives will be valuable.

(a) Principal Investigators or Teams

The methods for selection of individual scientists to participate directly in past planetary missions have been criticized. This is due to *de facto* procedures which precluded open announcement of flight opportunities or evaluation of proposals of experiments from *all* interested and qualified scientists, and led to the present situation in which only a very few individuals have access to planetary spacecraft as platforms for their experiments. An alternative method, the formation of scientific teams whose members have been chosen in several different ways, has had similar consequences, i.e., eliminating the participation of well-qualified and interested investigators.

Panel's Views. We recognize that certain types of spacecraft experiments are sufficiently complex or costly so that no one individual scientist is able or interested in utilizing the full capabilities of the experiment. Visual imaging experiments are an example of this class. By contrast, *in situ* measurements of the planetary environment by ion spectrometer or magnetometer represent another class of experiments in which an individual scientist can and indeed is interested in both developing the instrument and analyzing its results.

We *recommend* that NASA openly solicit participation in all future planetary missions by announcing flight opportunities with adequate time for response from the scientific community. The assignment of experimental responsibility to a principal investigator or to a team of scientists cannot be made under a single policy that does not recognize the unique aspects that a given experiment may have from the point of view of different disciplines. Thus, at present, we *recommend* that responsibility be assigned to teams of investigators for imagery-type experiments and to principal investigators in all others. Because of the wide applicability of images to many disciplines, dissemination of the preliminary imaging information should be as widespread and prompt as possible. We *recommend* that these data be released immediately through the NASA Goddard Space Flight Center National Space Science Data Center. We recognize that the imaging team should have the exclusive opportunity and responsibility to carry out the complex procedures, dependent on knowledge of the camera system, required to yield photometrically and geometrically correct scans. Similarly, for those experiments that demand complex treatment of the returned data before interpretations can be made, we *recommend* that the present well-tested principal investigator system, which is based on an exclusive-use period, be continued.

(b) Summer Institute on Planetary Exploration

It is essential, in planning missions and supporting specific experiments for the future, that the participation of the scientific community be broadened. Only a small number of scientists have had an opportunity to participate directly in the planetary exploration program and the experience they have gained is not generally familiar to the planetary science community. New ideas from young scientists must be included as must concepts from more established scientists not yet involved in the planning processes of NASA.

We *recommend* that NASA develop a summer institute program expressly designed to introduce interested scientists and engineers to the science, technology, and administration of the planetary program. Participants would include graduate students, junior and senior university faculty, NASA and industry scientists and engineers. The purpose of the institute would be to: (1) Summarize the present state of knowledge of the planets and the kinds of scientific studies that can be made from either spacecraft or the Earth's surface; (2) Review the technology and operations of past, present, and future spacecraft and their support constraints; and (3) Discuss the mechanisms of NASA financial support of instrument development and missions studies.

It is essential for a vigorous, viable program of planetary exploration that the future participation of individual scientists be encouraged. Such a summer institute will complement the existing post-doctoral research programs that NASA supports through the National Academy of Sciences - National Research Council and which should be maintained.

7. Manned Planetary Exploration

During the past few years, there have been numerous discussions of the possibility of manned planetary exploration. For example, in 1966, the NASA Office of Manned Space Flight prepared a detailed study for a manned round-trip mission to the immediate vicinity of Mars to collect information about the planet and, perhaps, to return a small sample of Martian surface material. It was suggested that such a mission could be flown as early as 1975. This study and other similar proposals supported the point of view that the next major space goal should be the placing of man on a planet, presumably Mars since the other planets appear to be even more inhospitable than the Moon.

In order to maintain the option open of eventual manned exploration of the planets, several groups, including the Space Science and Technology Panels of President's Science Advisory Committee have recommended that NASA undertake a variety of programs. These include biomedical programs exposing man to space conditions for long periods (100 to 200 days) in Earth orbit to determine whether he is qualified to undertake planetary missions (these missions involve round trips of about 700 days). Such biomedical qualification requires the development of special vehicles since neither the present Manned Orbiting Laboratory nor Apollo could easily be adapted for long term missions; needless to say these programs involve substantial funding.

Panel's Views. We were unable to identify a need in planetary exploration, in the forseeable future, for the unique abilities of man. For example, in the proposal for a manned fly-by of Mars, man is not utilized in a unique role. In the face of a limited space budget, we favor reallocation to the unmanned exploration of the planets those resources directed to efforts preparatory to a manned planetary program. The rapid development of technology suggests that full automated systems of substantial complexity will be available for planetary exploration and that this technology should be capable of answering the major scientific questions that we can now pose about the planets.

While at some time in the future it may be in the national interest to undertake manned missions to the planets, we do not believe man is essential for scientific planetary investigation at this stage. Therefore we recommend that those resources presently intended for support of manned planetary programs be reallocated to programs for instrumented investigation of the planets. The scientific investigations recommended in the 1965 study and in this report apply directly to the proper planning of any eventual manned program, but they should be viewed in terms of their contribution to the major scientific goals of the NASA program rather than in support of manned exploration of the planets.

8. New Opportunities for United States-Soviet Cooperation in Planetary Exploration

During the early years of the space program, President Kennedy sought cooperation in space with the Soviet Union. He believed that the development of areas of common interest could gradually be expanded as time went by, thus establishing a habit of cooperation between the two countries. Some of the early proposals for cooperation involved joint physical implementation in space ventures. For example, President Kennedy in a speech before the United Nations General Assembly in 1963 suggested that the Soviet Union and the United States join in sending to the Moon representatives of a number of nations. The preliminary attempts at bilateral cooperation resulted in limited agreements for sharing meteorological data, examining possible joint efforts in obtaining magnetic data, and in using communication systems.

Looking ahead to the near future, it is clear that there will be numerous opportunities for new and substantive collaboration with the Soviet Union in planetary exploration. Pressures placed on the programs of both nations by limited resources may add impetus to efforts for cooperation in space activities. Cooperation between them and eventually with other nations may take the form of planning for, rather than the actual joint physical implementation of, space experiments. Joint planning would permit the maximum use of the special talents of each of the countries involved, while at the same time providing prestige returns to each. Cooperative planning has the additional advantage of not necessarily involving detailed hardware considerations; as a result, questions of security, in the narrow military sense, are not as relevant as they would be in joint implementation of space flights.

For example, the Soviets recently penetrated the atmosphere of Venus with a probe. Although this experiment was not entirely unexpected, U. S. planning for the exploration of Venus had not taken into account possible Soviet results. The experiments aboard Mariner 5, which flew by Venus shortly after the entry of the Soviet probe, in part confirmed by indirect radio techniques what the Soviets had obtained by in situ observation. Apparently the Soviet experiments were not carried through to completion and radar observations carried out in the United States were needed fully to interpret the Soviet results. We do not know to what extent Soviet planning for their Venus probe recognized possible U. S. experiments, even though U. S. plans had been published several years earlier.

Duplication of effort may have been valuable during the early stages of space exploration because of the high probability of failure. The great reliability of the present systems resulting from continued experimentation and advances in technology has eliminated the need for repetitive experiments. In the same sense, the rapid development of a broad area of space activities dilutes the prestige value to any nation of a particular space success. There is no longer the great prestige advantage that accompanied a scientific discovery made in advance of competitors now that such discoveries have become more commonplace. This is so even with respect to the exploration of planets, in which considerations of celestial mechanics permit visits from Earth to a particular planet at intervals spaced as far apart as one or two years. Only if one nation were completely to dominate an area of exploration, such as planetary studies, would the present balance of space-related prestige be upset.

The above considerations and the positive values to be derived from enlarging areas of contact suggest that in the future cooperation in planning for the exploration of the planets will be rewarding to both the Soviet Union and the United States. Planetary exploration may be the earliest and most suitable candidate for such combined planning efforts. Journeys to the planets are expensive, they require long lead times because of limited opportunities for making the journey and great sophistication in instrumentation if the instruments are to survive the lengthy voyage. Furthermore, planetary investigations have no relevance to national security, nor has any nation as yet made a national goal of palnetary exploration.

Implementation of such joint planning would, of course, require agreement between the two governments. Despite this need, it would appear best to proceed first through the mechanism of informal but coordinated contacts between American and Soviet scientists. We know little about how the Soviet space program is planned. Informal discussions with Soviet counterparts might provide a basis for such an information exchange. On the basis of such early discussions, it should be possible to draw up a specific proposal which then could be presented to the governments for agreement.

We <u>recommend</u> a coordinated effort involving representatives of NASA, the Department of State, and the National Academy of Sciences with the purpose of informally contacting knowledgeable Soviet scientists in regard to the possibility of joint planning of planetary exploration. Such early discussions should provide the basis for more formal intergovernmental agreements.

Chapter 3

PLANETARY ATMOSPHERES

Introduction

The 1965 Woods Hole Study identified three principal objectives in the exploration of the solar system. The investigation of planetary atmospheres can contribute to these objectives; the relation of research on the inner planets to advances in terrestrial meteorology is particularly direct.

Understanding of Man's Environment. Since the 1965 Study a number of theoretical investigations have been made of the lower atmosphere dynamics of Mars and Venus, and other studies are known to be under way. None of them is definitive, or even nearly so, but a remarkable relevance to terrestrial studies is already clear. Investigation of the Martian atmosphere focuses attention on a number of important aspects of the Earth's atmosphere that were not receiving the attention they deserved. The role of radiative transfer is greatly exaggerated on Mars but is also important on Earth. The lower boundary layer is essential for long-term weather forecasting. No theoretical model atmosphere that includes the boundary layer exists for Earth, but one is being developed for Mars. The low level diurnal jet has been treated as a minor feature of the Earth, but it is a planetary-scale phenomenon on Mars and is receiving increased attention. Diurnal tides also are of much greater importance on Mars. In the case of Venus, two questions are central. First, the circulation must closely resemble a Hadley cell, a type of circulation generally neglected on Earth, but of importance in the tropics. Models of Venus may be the first to give a modern numerical treatment. Second, we do not know how the Venus clouds couple to the atmospheric dynamics nor how a complete cloud cover can form. Why do the terrestrial oceans not evaporate and produce a hot, cloud-covered planet like Venus? We may discover that such a configuration was possible in previous geological eras.

As we learn more about the structure of the atmospheres of Mars and Venus, we find that our understanding of the Earth's atmosphere is increased. Since atmospheric theories must now explain three planetary atmospheres, the new theories are more likely to be correct. There are examples in both meteorology and in upper atmosphere physics. The electron density in the ionospheres of Mars and Venus has now been measured. It has been surprising that these ionospheres do not have F2 regions as the Earth does. The physical processes that produce this high, dense ionization region on the Earth are being re-examined to understand not only why such a region does not form on Mars and Venus, but why it does occur on the Earth. The understanding of the thermal structure of the upper atmosphere is another such problem. Mars, Venus, and the Earth provide three atmospheric examples. Two of the planets have carbon dioxide atmospheres, the other an oxygen-nitrogen atmosphere. Venus rotates very slowly while Mars rotates with nearly the period of Earth. Now that exospheric temperatures of these planets are being measured, the theory being developed to explain all three will increase our understanding of the heating and cooling processes in the Earth's atmosphere.

If Mercury has a few millibars' pressure of CO_2, its atmosphere may also be of meteorological interest. It is more likely, however, to be of most relevance to planetary aeronomy. The outer planets, and Jupiter in particular, are less similar to the Earth, but still of some importance. Stone's studies of symmetric instabilities extend the range of previous work on baroclinic stability and lead to a much better fundamental understanding of these phenomena.

The Origin of Life. The determination of the composition of a planetary atmosphere will show if the basic ingredients necessary for life, as we understand it, are present in the planetary environment. Compounds containing the atoms of carbon, oxygen, hydrogen, and nitrogen, and water in particular, are of central interest.

History and Origin of the Solar System. The atmosphere of an inner planet comprises only a minute part of the whole planet. Nevertheless it is an indication of the recent surface history of the planet -- the first stage in understanding its planetology.

The origin of the atmospheres can be better understood by comparing their composition. The inner planets appear to have lost their original atmospheres. The Earth, we know, has obtained its present atmosphere from the outgassing of its interior. The amount of helium, argon, and nitrogen in planetary atmospheres such as Mars and Venus are indicators of the amount of degassing that has occurred. While Venus and Earth are about the same size and the same distance from the Sun and were probably formed from the same source at about the same time, their atmospheres have evolved quite differently. A major problem is the lack of water in the atmosphere of Venus to correspond with Earth's oceans.

The outer planets, notably Jupiter, appear to have their original atmospheres. Following these ideas, the Jovian atmosphere is a current-day example of what the material that formed the solar system was like. The composition and structure of the atmosphere of Jupiter may serve as criteria for understanding theories of the evolution of the solar system. There may be no sudden change of phase on Jupiter or Saturn and we may be able to understand a great deal about the bulk of the planet from a study of its atmosphere. Since these planets are in a quite different stage of evolution, and since they form the major part of the planetary system, they are vital to our understanding of the solar system.

Measurements Required

Venus

The physics, chemistry, and dynamics of the lower atmosphere should be given high priority in investigations of Venus. The physics of the upper atmosphere is a desirable concomitant. Earth-based observations are still of value, but since the bulk of the Venus atmosphere is beneath the cloud tops, the atmospheric drop-probe will be the most important tool for investigating the lower atmosphere in the next decades.

The following measurements are needed to test and develop existing theories: temperature-pressure profiles at subsolar and antisolar points and at the poles, with measurement of the point of impact; the vertical flux of solar radiation down to the surface; composition, number-density and size distribution of cloud particles; winds, turbulence and cloud dynamics; and detailed composition of the atmosphere, with particular reference to condensible constituents. Molecules such as nitrogen, oxygen, and ozone can be measured by ultraviolet spectroscopy. The temperature of the upper atmosphere and its variations can be determined from the ultraviolet radiation emitted by hydrogen atoms. The electron density in the ionosphere should be measured as a function of location about the planet by radio occultation techniques.

A mass-spectrometer capability should be developed to measure the concentrations and isotopic compositions of the noble gases of Venus. Inventories of He^4 and Ar^{40} in in the terrestrial atmosphere have been valuable indicators of the outgassing of radioactive rocks. These inventories will be similarly important for the other terrestrial planets. The kind and amount of the remaining rare gas isotopes in a planetary atmosphere are functions of the original entrapment of those isotopes within the planet and of its subsequent outgassing history. As such, these determinations can set useful boundary conditions on theories of the planet's origin. Differences, if they exist, in isotopic composition between planetary matter and terrestrial matter are probably most easily detectable in the rare gases; and all the gases from neon through xenon can probably be included in the search because background interference in mass spectrometer measurements tends to fall off more rapidly with increasing atomic mass than does the abundance of the rare gases. In the case of xenon, special anomalies in isotopic composition due to extinct radioactive I^{127} and Pu^{244} have been prominent in the meteorites and may be visible in Venus. If so, it may be possible to infer when Venus was formed.

Mars

The problems of the Martian atmosphere differ greatly from those of Venus; similarly, research vehicles and the types of measurement employed to resolve them will differ. Meteorological and aeronomical studies of the Martian atmosphere are concerned with the free atmosphere, above the surface boundary layer. This boundary layer will exist even over a perfectly smooth and uniform surface, will probably be from 10-m to 1-km thick, and will differ greatly from the overlying free atmosphere in its thermal and dynamical processes. A major difficulty in studying the boundary layer is that no one site can be considered truly representative. Except for compositional determinations, those atmospheric measurements made by an early Martian lander will be designed to support biological or surface studies rather than for atmospheric studies.

Drop-probes are of secondary value to study the Martian free atmosphere; their main objective would be to make a detailed chemical analysis of the lower atmosphere. The most valuable vehicle is an orbiter. With the orbiter, radio occultation measurements can map the lower atmosphere temperature profile and the ionosphere. Airglow and resonance fluorescence experiments will give composition information. While the major constituent of the Martian atmosphere, carbon dioxide, has been identified, it is not known whether molecular nitrogen, molecular oxygen, or ozone are present and if so, in what amount. The diurnal temperature changes in the upper atmosphere should be measured. A CO_2-band infrared instrument could give crude temperature profiles. A radiometer could map ground temperatures and compare them with topography. The same radiometer would give temperatures of clouds and the nightside.

Winds, an important feature of the Martian atmosphere, are probably best measured indirectly by means of visual observations of clouds. Dust-devils and other local phenomena will probably also be visible if the resolution is sufficiently high. A floating balloon-sonde is not out of the question, but should probably be deferred until the results of cloud motion studies are known.

Again, a mass-spectrometer capability is needed to measure the concentrations and isotopic compositions of the noble gases of Mars, for the same reasons as for Venus. It is likely that useful information can be obtained from orbiters. At one extreme, helium measurements should be possible in a spacecraft orbiting the planet at large distances. At the other extreme, a complete rare gas assay should be feasible from an orbiter of collapsing orbital radius, and certainly from a lander.

Jupiter

Jupiter, the nearest and most thoroughly studied of the major planets, presents interesting and important physical problems to planetary astronomers. Jupiter apparently possesses an internal source of energy, and is thus of interest to stellar astronomers. Space probes to Jupiter are becoming technically feasible and they seem certain to be as fruitful as those already sent to Venus and Mars.

One of the most important questions is the composition of Jupiter's atmosphere. A detailed knowledge of the composition will provide constraints on theories of the origin of the solar system. A better knowledge of atmospheric abundances will also permit improved models of the interior. It seems fairly certain that H_2 and He make up the bulk of the atmosphere, but the H_2/He ratio is quite uncertain. Although the evidence suggests that the abundance (by number) of He is comparable with H_2, we cannot exclude a solar composition (H_2/He 3) or a predominantly helium atmosphere $\left(\frac{H_2}{He} < \frac{1}{3}\right)$. Ground-based observations in the near infrared may provide the H_2/He ratio if the dominant source of opacity in this wavelength region is due to collision-induced transitions in H_2. In this case, the spectral distribution of the emitted radiation will depend on whether the H_2 transitions are induced by He or self-induced. Although the results of these observations are not likely to be highly accurate, they are of great interest.

With such limited information attainable from Earth, it seems fully justified to commit a significant portion of the instrumentation aboard a Jupiter space probe to determining the composition of the atmosphere. Radio occultation experiments, which have proved very successful on Venus and Mars, would yield an accurate value for the scale height. From this the mean molecular weight can be determined if the temperature at the occultation level is known.

The radio occultation experiment is probably the only observation relating to atmospheric composition that is practical for the initial fly-bys. However, observations with a more advanced mission of the absorption of ultraviolet solar radiation by Jupiter's atmosphere may yield further results. In particular, the flux of the 584 Å HeI chromospheric emission line at Jupiter is of the order of 10^8 photons/cm^2 sec. It seems feasible to measure the helium abundance from the absorption of this line as the planet occults the Sun.

Another important question to which a Jupiter space probe can contribute uniquely is the planet's energy balance. It seems plausible that the total energy emitted by the illuminated hemisphere could be measured quite well by the planned Convair-990 telescope. The major uncertainty in Jupiter's bolometric albedo, however, arises from the lack of knowledge of the phase function, since only a 12° variation in phase angle is observable from the Earth. This uncertainty can and should be removed by visual photometric measurements, ideally in several colors, during the Jupiter mission. Nevertheless, the total energy emitted will not be known precisely until bolometric measurements are made of the dark hemisphere. Therefore, the spacecraft should also make sufficient measurements in the far infrared to establish the difference, if any, between the amount of radiation emitted from the light and dark hemispheres.

Much information on Jupiter's atmosphere can be obtained from the absorption lines and bands found in the near infrared spectrum. However, the analysis of these lines and bands is greatly complicated by the fact that they are almost certainly formed in the clouds. A major question about Jupiter thus concerns its cloud structure. Is it a single layer or is it composed of several layers as is often the case in the Earth's atmosphere? Are the cloud tops highly irregular? Definitive answers will probably require atmospheric drop-probes on more advanced Jupiter missions. One important measurement for these probes is cloud particle density as a function of depth. Closely related are measurements of the upward and downward flux in visible wavelengths as a function of depth, indicative of the depth to which solar radiation penetrates.

Direct measurements of pressure and temperature by a probe into Jupiter's atmosphere would provide a direct test of atmospheric models and would require relatively simple instrumentation. Measurements with a mass spectrometer would provide an independent check on the atmospheric abundances, especially important in the case of the rare gases.

In addition to the observations suggested above, measurements of interplanetary magnetic fields and plasma and of solar and galactic cosmic rays to distances of 5 AU (further in case of a swing-by) would be of enormous importance to cosmic ray physicists. These measurements would decisively test theories of the solar modulation of galactic cosmic rays and provide a better knowledge of the low energy end of the cosmic ray spectrum. Extremely valuable measurements can also be made on Jupiter's immense radiation belts. These measurements include the energy spectra of the trapped particles and the strength of the magnetic field.

In summary, because of its many diverse phenomena, Jupiter is probably the most interesting planet from a physical point of view. It is now technically feasible to send space probes to the vicinity of Jupiter. We therefore recommend that Jupiter missions be given high priority, and that two exploratory probes in the Pioneer class be launched in 1972 or 1973.

Mercury

Since it is doubtful that Mercury has any atmosphere, the planet is at present of minor interest for atmospheric investigations.

Ground-Based and Near-Earth Studies of Planetary Atmospheres

Observations from the surface of the Earth and from near-Earth orbit produce valuable information on the nearer planets, and for many years to come they will be our primary source of information on the more remote planets. Such observations are less expensive than planetary missions. Because our knowledge of the more remote planets is very limited at present, every avenue of study from the vicinity of the Earth should be explored before we commit expensive probes to these objects.

We note with satisfaction the assistance to ground-based planetary astronomy that NASA has been providing since the Woods Hole Study. An intensive ground-based program is obviously a sensible means to assure, at relatively low cost, that optimal use is made of deep-space exploratory missions.

In the same sense, we urge that more attention be directed to the use of astronomical telescopes in Earth orbit for planetary observations. To date, the only space observations of planets are a few low resolution, ultraviolet spectra of the bright planets obtained from sounding rockets (which provide about 5 minutes' observing time above the atmosphere).

(a) Earth-Based Observations. Important planetary observations have recently been made by conventional telescopes. One of the foremost are the excellent spectra of Venus, Mars, and Jupiter obtained by P. and J. Connes. The spectra were obtained with a resolution of approximately 0.1 cm^{-1} but they covered only a limited part of the available infrared spectrum. This type of observation should be extended throughout the infrared and should specifically include the regions where molecular vibration, bending, and rotation are revealed. A resolution of at least 0.1 cm^{-1} appears to be a reasonable goal. At especially dry locations (total water vapor content of the atmosphere less than 2 mm) a sizable portion of this region can be reached from the ground.

NASA is to be commended for sponsoring the construction of three large telescopes (the 105-inch telescope at the University of Texas, and the 88-inch telescope at the University of Hawaii, and the 61-inch telescope at the University of Arizona), and for up-dating for planetary use the facilities of two other large telescopes (the 60-inch at Mt. Wilson and the 82-inch McDonald telescope).

Optical astronomy can furnish data that allow, or facilitate, interpretation of atmospheric data obtained from space missions and that guide the formulation of these missions. With respect to the structure of planetary atmospheres, ground-based measurements yield information on the following: (1) Composition of lower atmospheres, (2) Pressures and temperatures, (3) Wind velocities if clouds can be photographed and followed synoptically, (4) Properties of particulate matter in clouds, and (5) Variations of minor atmospheric constituents (particularly water vapor) with season or other parameters.

Another ground-based program supported by NASA is a planetary patrol to be carried out at 6 locations around the world for continuous diurnal photographic monitoring of the planets when they are favorably situated. This program goes far toward implementing item (3) above. We note, however, that immense quantities of data will be produced, and must be interpreted if they are to be of use. We commend NASA for supporting the planetary patrol and urge that data analysis be given particular emphasis.

To be fully effective, ground-based observations should be made before, during, and following encounter of a specific planet by a space mission. Conditions found during encounter can thus be related to the large body of information acquired in the long history of telescopic examination. This procedure is important not only for photographic observations, but also for spectroscopic observations of variable minor constituents.

(b) Planetary Observations from Near-Earth Orbit. The NASA planetary program should take account of the Earth-orbital telescopes that are being planned for the mid-1970's. These all-reflecting telescopes, having apertures of about 1 m are being designed for diffraction-limited performance at λ=5000 Å and will therefore have a limiting resolution of 0.1 arc sec. High resolution imagery will be parctical in the wavelength interval from 2000 to 8000 Å. Imagery below 2000 Å is possible, but the relatively long exposure times required makes compensation for blurring due to planetary rotation and motion increasingly difficult. Imagery above 8000 Å can also be performed, but special imaging sensors will be required.

Long term observations of surface and atmospheric detail on the planets will be possible. The following table indicates the limits of resolution ℓ and its ratio to the diameter D of a planet at a favorable apparition:

Planet*	D (km)	ℓ (km)	D/ℓ
Venus	12×10^3	50	240
Mars	6×10^3	35	170
Jupiter	14×10^4	300	470
Saturn	12×10^4	650	180
Uranus	5×10^4	1200	40
Neptune	5×10^4	2000	25

For the mid-1970's, it appears that fly-bys and orbiters are practical for only the first three planets in the above table. In all these cases, a fly-by or orbiter is capable of at least 100 times more resolution than an Earth-orbital telescope, but over only a rather small fraction of the planet's surface at a given time. Therefore, the Earth-orbital telescopes should complement these detailed photographs by planet-wide photographs taken at appropriate intervals. High resolution photography of planets from the Earth's surface at periods of exceptionally good seeing should also be vigorously pursued.

The largest contribution of Earth-orbital telescopes to planetary imagery would appear to be imagery of the major planets. With the exception of Jupiter, fly-bys of these planets cannot be expected until at least 1980. Furthermore, the relatively long exposures required for these objects, particularly Uranus and Neptune, increases the difficulty of obtaining ground-based photographs during intervals of good seeing.

Many other solar system objects are photographically resolvable with a 40-inch diffraction-limited telescope. These include Pluto, about ten of the largest asteroids,

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*The planet Mercury is excluded because of the difficulties in arriving at a thermal design for an Earth-orbital telescope pointing within 20° of the Sun. These difficulties are unlikely to be solved in the next few years.

Titan, and the Galilean satellites of Jupiter. The latter have angular diameters of about one arc sec and should exhibit surface features on photographs having a resolution of 0.1 arc sec.

Ultraviolet spectroscopy from Earth-orbiting telescopes should also be used to complement spectrographic data obtained by planetary missions. In the next few years, three Orbiting Astronomical Observatories (OAO's) are scheduled to carry a variety of instrumentation for wide-band photometry and spectroscopy. Although primarily designed for stellar observations, each of these OAO's can give valuable supporting information on the planets.

Attention should be directed toward the feasibility of specially modifying the Small Astronomical Satellite (SAS) for optical planetary observations.

High resolution, 0.1 to 0.3 Å, ultraviolet spectroscopy of the planets, utilizing the high spatial resolution of about 1 arc sec and a spectral resolution of about 1 Å can be obtained by a 1-meter orbital telescope for all but the most distant planets. Instrumentation now being designed should allow such spectra to be obtained in the mid-1970's.

At present there are no active plans to perform infrared spectroscopy of the planets from Earth orbit. However, NASA is planning to equip a Convair-990 jet aircraft with a 36-inch ir telescope, stabilized to about one arc sec, and to be flown at about 40,000 feet. There appears to be very little water vapor above this level with the result that the infrared spectrum is nearly completely transparent below 25 μ and partially transparent above 25 μ. This new facility, scheduled to become operational in 1970 or 1971, should be vigorously utilized for obtaining ir spectra of the planets at moderate spatial resolution (1 to 2 arc sec).

In light of these considerations, we recommend that the NASA planetary program planning be closely coordinated with the Earth-orbital telescopes being designed for the mid-1970's and with the infrared aircraft telescopes now under construction.

Mission Priorities for Atmospheric Research

The following attempts to order priorities for atmospheric research. It represents an assessment at one moment of time, and priorities should be reevaluated as results accrue from successive missions.

Small Planetary Orbiters

Small planetary orbiters of the Pioneer/IMP class should be a part of the planetary exploration program, whatever the level of funding allocated to it. While their value for planetary atmospheric research is not equal in every case to that of larger probes, the prospect of a relatively low-cost, ongoing, flexible program is extremely attractive. Instruments should include plasma probes and magnetometers for solar plasma measurements; aeronomy instrumentation in certain cases for use during orbit decay into planetary atmospheres; radio occultation; imaging in the visible and infrared for meteorological studies, if the payload permits; microwave radiometry for Venus orbiters.

Direct Impact Probes on Venus

From the standpoint of planetary atmospheres, the study of the lower atmosphere of Venus is the most important single area in which rapid advance is possible at the present time. Critical measurements include physical and dynamical properties of the cloud-cover, thermal and dynamical characteristics at a number of locations above the planet's surface, the radiation environment, and the chemical composition of the atmosphere. Such measurements can be made only from probes that penetrate to the planet's surface. Since the probe must pass through the upper atmosphere, valuable aeronomical measurements can also be made during this phase of the mission.

The most economical, convenient, and reliable mission employs a direct impact trajectory. Owing to the planet's very high surface temperatures, the sterilization problem can probably be overcome. A very elaborate experiment can be carried out using multiple probes on a Mariner-class mission, and it is possible that useful parts of the experiment can be accomplished using smaller vehicles.

We recommend that *in situ* exploration of the lower atmosphere and cloud layer of Venus be one of the principal objectives of the space program of the 1970's.

Mariner Orbiters to Mars

We recognize the value of the planned 1971 Mars orbiter mission. Visual and infrared imaging are powerful tools for meteorological investigations. It is possible that wind velocities can be obtained from measurements of dust-devils, clouds, etc. Vertical temperature profiles can be measured from the 15-μ CO_2-band emission. High resolution infrared spectroscopy may be used to map concentrations of polyatomic gases (including water vapor). Synoptic measurements of atmospheric pressure and temperature profiles, and ionospheric profiles, can be obtained from radio occultation. Plasma and airglow measurements can also be made.

It is not clear to what extent these measurements can be made from small planetary orbiters or to what extent Mariner-class vehicles are required. The possibilities must be examined in order to plan missions after 1971.

Jupiter - Small Fly-by

Exploration of Jupiter must have a modest beginning. We recommend that this start be made during the early '70's. A small fly-by could perform a valuable occultation experiment, giving data on both the ionosphere and the neutral atmosphere. Measurements of plasmas and magnetic fields would be a prime objective of the flight.

Mars Drop-Sonde

Depending on the results of measurements from orbiters it may prove important to obtain *in situ* data on electron density and the ion species in the ionosphere, the composition of the neutral atmosphere, and on atmospheric structure with particular reference to the presence of a turbopause. A small, hard-impact probe could perform these tasks.

Mars Landers

Because a surface station would be situated in a complex thermal and mechanical boundary layer the value of a lander for atmospheric studies is not great. We anticipate, however, that such missions will take place in the 1970's and would wish to make use of them for accurate atmospheric analyses, particularly of the noble gases. To do so will require special instrumentation because the abundant gases should be gettered away before measurements are made. If weight is available on the lander during descent, the drop-sonde mission described above can be performed as well.

Mercury Fly-by

Atmospheric investigations of Mercury should not command a high priority in the near future. Fly-by missions to the planet may take place for other reasons, however. If so, radio-occultation and flourescence measurements would be of interest.

Chapter 4

PLANETARY SURFACES

Introduction

Exploration of our solar system constitutes an intellectual endeavor in which the search for extraterrestrial life is only one of many fascinating elements. Increased understanding of the origin and evolution of the solar system can be best accomplished by visiting a number of other planets. In particular, it is on the surfaces of these bodies that we must look for a record of their history. Here we search for evidence of present or former life, and also study materials formed in diverse environments and over a long period of time.

The interiors of most planets are largely inaccessible and, in any event, their present state tends to reflect only comparatively recent history. Planetary atmospheres are more readily studied but, here too, we examine the end product of a whole sequence of earlier processes. Only on a planet's surface do we find a written record of events that can lead us backward in time over a considerable fraction of the duration of the solar system.

In the rocky surface of each of the minor planets is written a long record of planetary change through time. The early events of this record may be overprinted by later events, or even obliterated. Yet a planetary surface invariably preserves a clear sequence of many major events that have shaped its present form. At a particular spot the last signature on the surface rock may be that of any one of dozens of different processes: the lava spewed forth from a hot interior; the accumulation of sediments and fossils on the floor of a former sea; the deposits left by a vanished glacier; the craters formed by infall of objects from outer space.

Because planetary surfaces have been subjected to a great variety of modifying processes of both internal and external origin, we expect considerable local variation. Some processes operated only in the distant past; others are still active. These processes can be recognized only when the surface is explored on the scale to which the processes succeed in modifying it. For example, large craters formed either by meteorite impact or by volcanic activity on the Moon or Mars are readily recognized in photographs having relatively poor resolution. Here recognition depends upon the distinctive and relatively simple shape of the feature. Events on the surface of the Moon can be recognized as being of internal origin in photographs with resolution of 1 km or better. In the absence of other information we may take this figure of 1 km as the upper limit of resolution needed to detect past volcanic activity on Mercury or Mars. Other processes require even better resolution. For example, Orbiter photographs of the Moon with resolution of about 10 m show transported boulders with distinctive skid and skip marks.

Visual photographs may be supplemented by infrared images or, in the case of Venus, by radio emission plots which depict the thermal properties of the surface. This type of remote sensing also tends to distinguish between compact material (e.g., rock) and fine debris (e.g., sand) and may also provide evidence of recent volcanic activity. Imaging by radar seems the chief means by which the surface of Venus can be studied in view of its high surface temperature and extensive cloud cover. Nonspecular radar tends to single out structure on the surface having comparable size to the radar wavelength and can penetrate thin coverings of dust to yield the pattern of the underlying rock. Specular radar echoes, while not forming images of high resolution, would reveal larger scale structure and would provide a measure of the dielectric constant and layering, such as might be produced by subsurface water.

From the recognition of physical processes revealed by large scale pictures of adequate resolution, one may proceed to the study of the particular by means of pictures with finer resolution, and eventually to local exploration by means of a landed vehicle. Photographic reconnaissance of representative portions of the surface is critically important for selection of best sites for landing missions. Without this background two dangers exist: first, that the lander may be directed to an unfavorable site, and second, that the lander may return important information which is largely uninterpretable out of context. We single out Mars orbital imagery as the highest priority mission that is now possible in the area of planetary surfaces.

It is also important to attempt at least an exploratory, preliminary examination of all the terrestrial planets. In practical terms, this means that we place considerable value upon a photographic reconnaissance of Mercury and radar examination of Venus.

In the sections that follow, we outline what is currently known about the surfaces of Mars, Venus, Mercury, and the moons of Jupiter. We discuss also the questions we believe must be answered next and the strategy for arriving at the answers. Our evaluation is predicated on assumptions concerning the overall balance that the space program will have during the next seven years. Thus, we suppose that opportunities to study the surface of Mars will far outnumber those for Venus and Mercury.

Mars

Both because of its proximity to Earth and its physical behavior Mars holds a favored position in plans for planetary exploration. In addition to providing a possible habitat for life, Mars has surface features and an atmosphere that can be easily and profitably studied. Our present knowledge of the Martian surface is at a critically favorable point. On one hand, we know that there are certain enigmatic but distinctive markings: dark and light regions, bright polar caps, "canals," and craters. On the other hand, our present knowledge is so fragmentary that it is impossible to test many of the models proposed to explain these features. A closer look at the planet by a Mariner-type orbital mission is certain to provide much of the information needed to guide further exploration.

Present knowledge of the Martian surface comes from study of large scale variations discernible with Earth-based telescopes and radar, and from Mariner 4 photographs of a small part of the surface (less than one percent). Earth-based observations show dark areas of irregular shape covering about one-third of the planet and featureless brighter areas covering the remainder. Polar caps, perhaps composed of ice frozen from either H_2O or CO_2, appear seasonally.

No agreement exists concerning topographic relations between bright and dark areas. Certain radar observations suggest that the dark areas are relatively high, but other observations indicate they are low. Mariner 4 photographs show a surface covered with craters having diameters from the resolution limit of 4 km to at least 120 km. The spectral and photometric properties of both the bright and dark areas are consistent with a surface composed primarily of silicates. The size-frequency distribution of particulate materials is unknown, but radar returns suggest a highly porous surface material.

Two "waves of darkening" start alternately from the two polar caps at half-yearly intervals, cross the equator, and fade at about 22° latitude in the opposite hemisphere from which they began. One importance of these waves of darkening lies in their interest as a possible clue to the presence of life on Mars. This interpretation is challenged by other models involving inorganic changes affecting either the surface or the lower atmosphere. For example, gases seasonally generated by melting or subliming ice caps may systematically move light absorbent hazes towards the equatorial regions, causing clearing and color change. Alternately, subsurface seasonal changes in temperature may lead to melting of permafrost, thus changing the color and albedo of the soil. From present data no clear choice can be reached between various models.

Scientific Priorities for Mars

(a) Orbiters and Fly-bys. Mariner fly-by and orbital missions provide an ideal opportunity for a reconnaissance survey of the Martian surface by means of visual photographs and adjunct infrared-thermal and ultraviolet imagery. The orbiting spacecraft that fly during the 1971 opportunity will be able to image the southern hemisphere during the time of maximum change in contrast. Utilization of two spacecraft will permit photography of two sets of critical phenomena. One spacecraft can monitor the area, recording its image at high Sun angles. These observations will allow determination of albedo difference and color contrast -- important for detection both of organic and inorganic processes. The second spacecraft could be launched at the appropriate time to obtain images at low Sun angles. These pictures contain the topographic detail that makes possible the interpretation of processes and their sequence in time.

Study of the images obtained during the 1969 fly-by missions will assist in mission design; that is, in choosing the Sun angles that will make the orbital missions most effective. Also, the results of the fly-by will influence camera design for subsequent missions. The importance of these considerations is indicated by the fact that only 5 of the 26 photographs taken during the Mariner 4 fly-by are high quality, appropriately exposed images.

Study of the images of large portions of Mars obtained by the two orbital missions will allow classification of those features of internal and external origin, and comparison with similar features on Moon and Earth. The crust of Mars can be subdivided into regional geological and environmental provinces. Predictions concerning the favorability and unfavorability of those provinces as hosts for biological activity can be made. This study, then, assists the later direct search for life.

A specific solution of the wave of darkening phenomenon may be indicated by topographic differentiation of light and dark areas, by textural variations between light and dark areas, or by secular changes in the boundary between light and dark areas.

(b) Lander. It is reasonable to expect that much of the experimental payload on the first Mars lander will be devoted to experiments either investigating the ability of the surface environment to support life or seeking to detect life directly. Some of these experiments are also important in characterizing the inorganic nature of the surface. For example, a photographic experiment, of critical importance in detecting large living forms, is also a first requirement for a detailed inorganic reconnaissance of the surface. It appears likely that the lander will be directed to regions thought to be large sedimentary basins -- a favorable kind of locality for the exobiological experiments. If, indeed, the region is underlaid by sediments a single panoramic photograph may confirm and amplify this presumption by showing stratification and other large scale sedimentary features. The panoramic photography should be followed by a limited and selective program of detailed photography to a lower limit of 1 mm coupled with 0.01-mm resolution microscopic photography of the sample. Rounded, frosted grains may bear witness to wind transport. Euhedral shapes may reflect a primary crystallization history involving either igneous processes or crystallization from low temperature media on the surface.

The microscopic experiments will bear greatest dividends if coupled with a direct chemical-mineralogic experiment making use of a combined x-ray fluorescence and diffraction unit.

So that the chemical analyses just mentioned will have optimal relevance, it is desirable to direct the lander toward an area representative of a large part of the Martian surface. Even then it is unreasonable to expect that the analyses will reveal "the" chemistry and mineralogy of the entire Martian crust. But it is reasonable to expect that a careful reading of the results will indicate the major processes and products of crystallization and sedimentation on and perhaps below the surface.

Venus

Study of the surface of Venus may be more difficult and less rewarding than that of Mars. Recent evidence indicates surface temperatures between 600 and 700°K, and atmospheric pressure as high as 100 atmospheres. The combination of these properties renders it exceedingly difficult to devise instruments capable of operating on the surface for more than a short time. The dense atmosphere may prevent most imaging systems from observing the surface unless they are equipped with their own sources of illumination. From orbit one must contend both with the effects of the dense atmosphere and with extensive cloud cover.

Despite these drawbacks the nature of the surface of Venus stimulates our scientific curiosity. What would happen to the surface of the Earth if it were raised to 700°K and blanketed by an atmosphere one hundred times as dense? Why should Venus, which is a near-twin of the Earth in size, mass, and location within the solar system have evolved so differently? Is Venus at a more primitive stage of evolution than the Earth?

Ground-based radio and radar studies are the source of virtually all our knowledge of the surface of Venus. Radar ranging experiments when combined with orbital analysis yield a value for the radius of approximately 6050 km. The planet rotates in a retrograde manner with a period of 243.1 Earth days, with the result that the length of the solar day on Venus is about 117 Earth days and the Sun rises in the West and sets in the East.

The 243.1-day rotation period implies capture of Venus' rotation by the Earth. Thus a stick placed on Venus to point to the Earth when Venus is closest would do so again at each succeeding inferior conjunction (i.e., at intervals of 19 months). This means that, as seen from Earth, Venus executes precisely four axial rotations between close approaches to our planet.

The dynamical properties have something to say concerning the interior of Venus and seem to point to the existence of a liquid core. Radar distance measurements are capable of yielding information concerning the variation of surface height along a region close to the equator of Venus. The present measurements suggest that the surface of Venus is level to $\approx \pm 2$ km over horizontal scales of the order of 100 km. This is in contrast to the Earth and Mars where elevation differences of ≈ 15 km are found between the tops of mountains and nearby valley (or ocean) floors.

Mercury, the Moon, and Mars have similar radar reflectivity, but Venus reflects about twice as well. Thus the surface of Venus is more compact than that of the Moon, if the two are made of similar material. Comparison of the radar backscattering properties of Venus and the Moon implies that Venus has a smoother and more gently undulating surface. Largely as a result of erosional processes, the surface of the Moon is smooth on a centimeter scale. One might therefore conclude that there is an extremely effective erosional mechanism on Venus.

From the foregoing one might expect that the surface of Venus is extremely monotonous. Yet radar studies have defined about ten regions that are remarkably rough compared with their environs. These regions are several hundred kilometers across, close to the present level of resolution. There appears to be a clustering of these regions close to the equator, though observational selection cannot be ruled out as the cause. In the case of at least one feature the shape seems to resemble that of a large circular crater, suggesting an impact origin or possibly a major internal convective process.

Radar maps of Venus made from Earth suffer from the inability to discriminate between the hemispheres unless multi-antenna systems are employed. Subject to this ambiguity, a small part of the disk of Venus has been mapped with a resolution of 50 km or less. By improving the capability of terrestrial radar systems, this limit might be reduced to a few kilometers, although still subject to the above ambiguity, which can be removed by an interferometer system.

Scientific Priorities for Venus

A step-by-step approach to the examination of the surface of Venus, as advocated for Mars, may not be possible within the present fiscal constraints. This suggests the advisability of making use of partial opportunities provided by other scientific missions. To be specific, visual imaging of Venus to detect discontinuities in the cloud cover seems a reasonable requirement for a Venus-Mercury fly-by, though it might not be accorded highest priority in a mission devoted entirely to Venus. Similarly, chemical analysis of the Venus surface at the termination of the descent phase of an atmospheric probe would be tremendously interesting even if not preceded by adequate reconnaissance missions.

For study of the Venus surface, we accord highest priority to an orbital mapping mission. Some form of radar imaging on a scale of about 1-km resolution, together with temperature measurements (albeit on a much coarser scale of about 100 km) may represent the two most worthwhile endeavors. The temperature mapping must be conducted at a wavelength that can completely penetrate the atmosphere with little or no absorption. This means a wavelength greater than 10 cm; the 12.6-cm communications wavelength would seem an excellent choice. The communications antenna might be oriented toward the surface near periapsis. If the satellite is at an altitude of 1000 to 2000 km and the antenna diameter is about 2 m the required resolution could be achieved. The precession of periapsis would permit determination of temperature as a function of latitude and insolation. Large local temperature variations might indicate the existence of internal convection cells or volcanic areas. These, in turn, may be related to the rough regions. A finding of temperature variation with latitude would discredit that model which depends upon internal heat production, and would also provide a valuable guide in the construction of models for the atmospheric circulation system for the planet. The radiometer developed for this measurement may be capable of a relative accuracy (day-to-day) of $\approx$ 1 percent though the absolute accuracy may be poorer (e.g., 10 percent).

Surface structure can be recognized by radar imaging. We propose that radar maps with resolution of about 1 km be prepared for parts of the surface. Perhaps this can be accomplished by using a bistatic radar system (e.g., illumination of Venus from the Earth). Relatively low weight requirements favor this experiment. However, it must be recognized that the feasibility of this technique remains to be demonstrated. An alternate spacecraft experiment would be a coherent side-looking radar. Unfortunately, it would probably require a large part of the total payload of a Mariner-class vehicle.

Fly-by or probe data can provide some information on the Venus surface. The distribution of temperatures detectable by multiple probes will give indications of the extent to which the planet's high temperature is a result of internal or of external heating. The atmospheric composition data may indicate the degree of degassing of its interior by past volcanic activity.

Mercury

Mercury is the smallest and most dense of the terrestrial planets. It is also the closest to the Sun. Albedo and radar reflection suggest a surface resembling the lunar maria. Only indistinct surface markings are visible from Earth.

The great range in temperature between subsolar and midnight positions, the large solar radiation flux, and the probable lack of an atmosphere must all influence the nature of the surface in a major way. Yet the remoteness of Mercury from Earth has served to lessen its attractiveness to those interested in planetary exploration. Recently, however, an upsurge in interest has been sparked by the realization that in 1973 or 1975 a spacecraft launched by a modest-sized vehicle can take advantage of the Venus gravitational field to gain acceleration and thereby enter a trajectory that will send it past Mercury.

Venus-Mercury Fly-by

The Venus swing-by mission proposed for 1973 provides the first opportunity to examine Mercury. As a prime objective, we recommend photographing the planet with a resolution of about 2 km. Similar photography of Venus during the fly-by portion of the orbit may reveal cloud patterns indicative of the atmospheric circulation system. Additional experiments for the Mercury encounter should include a magnetometer to determine if Mercury has a magnetic field and some form of emission line photometer to determine if Mercury has an atmosphere. If the trajectory permits occultation, a second test of the existence of an atmosphere can be achieved from the S-band radio links.

If imagery of Mercury at 2-km resolution is indeed obtainable from a Pioneer-class Venus-Mercury fly-by, it becomes a most significant experiment from the viewpoint of planetary surfaces. The Sun may have a profound effect on its nearest neighbor so that an unusual balance of internal and external activity is evidenced by surface topography. The best way to underscore the scientific value of a limited imagery mission for Mercury is to recall the major changes in our thinking about Mars produced by the 4-km resolution Mariner 4 pictures.

The short lead time, the low cost of a Pioneer mission, and the value to our national prestige of a planetary first are strong arguments supporting the basic scientific value of the mission.

Jupiter and Its Moons

The possibility of a "Grand Tour" in 1977-78 past the four large, low-density outer planets -- using their gravitational fields for assistance -- is an exciting prospect for it would permit the first complete reconnaissance of the major members of the solar system. The tour would provide much needed information on the outer planets which could be compared with our growing knowledge of the inner, more dense, terrestrial planets.

The spacecraft would pass some satellites of the outer planets, objects of considerable interest in their own right. Jupiter, the most massive planet, has three satellites that are larger than our Moon and one that is larger than Mercury. They might be considered semiplanetary bodies whose properties bear on the general problem of the origin of the solar system. The surface features of these satellites provide clues to the major events in their history. In all probability they will be cratered by meteorite impacts, but they may also provide some hints of the events associated with their separation from the gaseous material of their parent body, the extent of their internal activity, and the evolution of their own atmospheres.

The Grand Tour will not have as its main focus an examination of the satellites, but insofar as trajectory and equipment limitations permit, any data gathered on satellite surface features will be of substantial interest.

Comets and Asteroids

Comets and asteroids may be the oldest members of the solar system; that is, they may represent those bodies least changed by subsequent events since their creation. It is possible that the surfaces of the Moon, Mars, Mercury, and Venus contain material not much older than the oldest on Earth (3.5 billion years). In this event, our efforts to study the origin and evolution of the solar system will be circumscribed unless we can examine cometary and asteroidal material. At the present time the immense difficulty of studying a comet or asteroid at close hand makes this an unprofitable endeavor. The situation may change during the course of the next decade, however; in that event, the question should be reopened.

Principal Recommendations

1. Orbital Mapping of Mars

The acquisition of orbital images of a significant portion of the Martian surface is among the highest immediate priorities of the planetary exploration program. The wide range of information to be derived from orbital images has bearing on almost all other aspects of Martian exploration. The success ratio of the Lunar Orbiter missions and the extent to which their data have modified both our view of the Moon's basic processes and the detail of much of our present lunar planning speaks eloquently for the need of similar data for Mars. We strongly recommend Mariner-class orbital mapping missions of Mars in 1971 as justified by the large scientific return, by the early need for planning data, and on grounds that 1971 is an optimal year for orbiting of Mars.

2. Mariner Mapping of Venus

By 1975 it is hoped that a Mariner-class mission to orbit Venus can be carried out. The prime objectives of this mission are mapping the thermal emission of the surface and radar imaging. The former of these objectives will require a radiometer operating at a wavelength of 10 cm, a low altitude periapsis, and a high inclination orbit. A resolution of about 100 km is desired. Radar imaging could be carried out bistatically or by using side-looking radar, and should strive for a resolution of 1 km or less. By 1975 a substantial part of the surface may have been mapped at a resolution of about 2 km with Earth-based instruments.

3. Fly-by Mapping of Mercury

If images of Mercury with 2-km resolution are indeed obtainable from a Pioneer-class Venus-Mercury fly-by this becomes a most significant experiment from the viewpoint of planetary surfaces and is recommended.

The following table indicates those flights and instrument packages desirable for a meaningful planetary surface exploration program. Instruments are intentionally not described in strict engineering terms but rather in terms of desirable results. The results depend both on instrument design and flight configuration.

TABLE 2

Desirable Missions for Planetary Surface Studies

Planet	Flight	Mode	Year	No.	Payload
Mars	Mariner	Fly-by	1969	2	Visual imaging system producing several 100-m resolution photographs. IR radiometer designed to produce thermal maps of the surface.
Mars	Mariner	Orbiter	1971	2	Visual imaging system producing several 100-m (0.2-km) resolution photographs of a large part of the southern hemisphere. One flight to obtain black-and-white images with maximum morphologic detail taken at low Sun angles from a high inclination orbit; the second, same resolution at several band passes that will produce color images taken at very high Sun angles from low inclination orbit. System should be designed to have a lifetime of a year. IR radiometer designed to produce thermal maps of the surface.
Mars	Small Planetary Orbiter	Orbiter	1971	2	A back-up system for the 1971 Mariner orbiter. Visual imaging system with ≈1-km resolution.
Mars	Titan-Centaur	Orbiter-Lander	1973	1	Orbiter: Visual imaging system similar to that carried by the 1971 orbiter. IR radiometer designed to produce thermal maps of the surface. Lander: Panoramic photographic system and high resolution microscopic photographic system (0.01-mm res.); x-ray fluorescence and diffraction; alpha backscatter.
Jupiter	Pioneer	Fly-by	1972 or 1973	1	Photography of planet and satellites at ≈10-km resolution.
Venus-Mercury	Pioneer	Fly-by	1973	1	Visual photography of part of Mercury with 2-km resolution and of Venus at ≈1-km resolution.
Venus	Mariner	Orbiter	1975	1	Radar imaging of the surface with 1-km resolution effected either by bistatic or side-looking systems. Thermal mapping of the surface in the 10-cm wave length range.

Chapter 5

PLANETARY DYNAMICS AND INTERIORS

Introduction

This chapter discusses the orbital and rotational dynamics of the planets and their satellites (with the exception of the Earth-Moon system), asteroids and comets, and the composition and physical state of the interiors of these bodies. Of the three fundamental scientific questions underlying solar system exploration, these matters pertain most strongly to that of the origin of the solar system: the locations and densities of the planets and other bodies provide the severest boundary conditions on the formation of the planetary system. They also pertain to an understanding of the terrestrial environment in that models purporting to explain the mechanical and thermal history of the Earth must explain the history of the other terrestrial planets as well.

Experiments that contribute to an understanding of planetary dynamics and interiors include: determination of orbits and rotations of natural bodies and of the orbits of artificial bodies; measurement of radii and topographic variations by radar and optical means; seismometry; surface chemistry by techniques such as alpha scattering and x-ray diffraction; and infrared and radio emission observations and magnetometry insofar as they indicate internal conductivities, temperatures, and density. Geological information obtained from pictures of surfaces is directly relevant to the problem of the evolution of the interiors.

Spacecraft missions of interest to the study of planetary dynamics and interiors that appear practically feasible in the mid-1970's (listed in order of distance from the Sun) are:

Mercury fly-by
Venus orbiter
Mars orbiter
Mars lander
Asteroid probe
Short-period-comet probe
Jupiter fly-by

The plan of this chapter is to discuss each of the bodies to which these experiments pertain and then to take up the question of priorities.

Mercury

From the point of view of planetary dynamics, Mercury is perhaps the most important object in the solar system. Being closest to the Sun, it is the most sensitive detector of departures from the laws proposed to account for planetary orbital motions. Its spin is also unusual, being coupled to its orbital motion in a three-halves resonance state. In view of its unusually high density, the interior of Mercury is also of special interest.

A space-probe fly-by of Mercury could provide important information on both its dynamics and its interior. From photographs we may obtain the precise orientation of Mercury. Combined with similar pictures from later fly-bys, the vital knowledge of the direction of Mercury's spin axis and the fractional difference in its equatorial moments of inertia can be determined. Search for magnetic field strengths and determination of the electromagnetic radiation from Mercury's surface, as well as photographs of the surface, will provide important data on its interior structure. The radius, mass, and hence, density, and the orbit can also be refined from the fly-by data. An orbiter is required to determine the detailed gravitational field of Mercury and a lander to study the interior by monitoring seismic activity.

Venus

Venus is the planet most similar to Earth in size, density, and distance from the Sun. However, it differs significantly in having a much more massive atmosphere composed mainly of carbon dioxide; a much higher surface temperature (700°K); a much slower, retrograde rotation (period, 243.1 Earth days); and no moon or oceans. The rotation period is within 0.1 day of a spin-orbit coupling to the Earth, which constrains the viscous decay time for a reasonable probability of capture into the coupling, and which constrains the moment-of-inertia difference, (B-A)/C, for stability. Of relevance to the planet's interior, Mariner 5 and Venus 4 did not detect a planetary magnetic field and ground-based radar has not been able to observe topographic height variations on a resolution scale of a few kilometers.

The closeness of the mean density of Venus to that of the Earth (about 96 percent of the Earth's) compels us to presume, in the absence of evidence to the contrary, that the bulk chemical composition of the two planets is roughly the same. This similarity of composition would extend to the presence of the radioactive elements uranium, krypton, and thorium, which are believed to be the principal heat sources in the Earth. Hence Venus furnishes a valuable test of theories, both of the Earth's interior and of the origin of terrestrial planets: any acceptable theory must explain why Earth and Venus are different.

Simple extrapolations from the Earth to Venus inevitably result in contradictions: a higher surface temperature indicates a hotter, weaker, and more rapidly creeping interior, which is consistent with the viscous decay time but inconsistent with the strength implied by the spin-orbit coupling. However, higher temperatures may imply not only lower viscosity but also greater dynamic imbalances --convection cells, for example --which can sustain departures from hydrostatic equilibrium. Yet if the interior were so active, we should also expect a far more irregular surface than is indicated by radar. We should also expect Venus to have a core and hence a magnetic field --unless the presence of a moon is necessary to provide precessional torques as a driving mechanism. The absence of a moon is the biggest problem of all in explaining the differences in the origin of the Earth and Venus.

Plainly, more data about Venus are needed. It is also plain that chemical, seismic, or other measurements on the surface will be difficult if not impossible in the next decade. However, a Venus orbiter would yield valuable information. The deduction that (B-A)/C exceeds 10^{-4} can be tested by measuring variations in the gravitational field, which are a function of the level of dynamic imbalance. A more sensitive magnetometer measurement could lower the upper limit for an intrinsic magnetic field; such a limit would be relevant to discussion of the origin of magnetic fields driven by the action of precessional torques. Attempts to derive the planetary heat flow would be frustrated by the atmosphere.

A measurement that appears feasible with larger, more sensitive ground-based radars is a determination of the physical libration of Venus, which is a measure of (B-A)/C. The latter, in conjunction with orbiter measurements of $(B-A)/MR^2$, would yield the moment of inertia. This important parameter would be very difficult to determine by any other method. Determination of these gravitational and rotational irregularities would not only contribute greatly to the question of the origin of the terrestrial planets, but would also stimulate better ideas about mechanical and thermal models of planetary interiors, which in turn would contribute to a better understanding of the Earth's interior.

Mars

With respect to its interior and dynamical properties, Mars is of no greater intrinsic interest than Venus or Mercury, perhaps less. However, most of the appropriate measurements are much more easily made for Mars; we address ourselves to the determination of the dynamics and interior of Mars with special cognizance of this fact and of the unique biological and gelogical interest in the planet. In rough order of priority, the principal questions are:

(a) What is the internal mass distribution of Mars?

(b) Does Martian surface physiography provide evidence, in addition to the pattern of impact craters detected by Mariner 4 photography, of present or past internal activity? (Belts of mountain building or major volcanism of the sort found on Earth would be seen without difficulty with higher resolution imagery.)

(c) What is the present level of tectonic activity in Mars? (This activity would be reflected in the seismicity.)

(d) What is the heat flow at the surface?

(e) Does Mars possess even a feeble magnetic dipole moment? (Mariner 4 set an upper limit of about 3×10^{-4} of the Earth's dipole moment.)

Mars Orbiters

The answers to most of the above questions can be obtained by spacecraft orbiting Mars at rather high inclination. Observation of the spacecraft as they are occulted by the planet will give us the true geometric flattening of Mars, and thus resolve the present discrepancy between dynamical and optical values, which are 0.005 and 0.010 to 0.015 respectively. From these size and shape measurements, a better value of the mean density than the present 4.0 ± 0.15 can be derived. Sustained observation of the spacecraft's orbit will yield the differences in moments of inertia of the planet and these data, coupled with values for density and flattening, provide significant information on the internal mass distribution.

The physiographic information will be provided by a program of high resolution television photography, such as is commonly included in orbiter planning. A more sensitive magnetometer in a magnetically clean spacecraft is technically feasible; a feeble dipole moment detected by this instrument might be taken to indicate the presence of a small fluid core in Mars.

Planetary heat flow is a difficult question, one that is unlikely to be answered by an early generation of Mars landers because of the weight and complexity of equipment needed to implant heat sensors beneath the surface. A crude measurement from orbiters may be possible. It will depend on surface mapping both by infrared radiometry and by radio emission in the cm-wavelength range. From these maps we might hope to establish values of the thermal gradients at various points on the planet. In general, this problem is difficult to approach and any method must be subject to close scrutiny.

Mars Landers

The most important single instrument for study of the interior is a seismometer landed on the surface. Analysis of seismic signals permits a good first-order determination of (a) the compressional wave velocity versus depth, (b) the density versus depth, and (c) the existence and locations of earthquakes. The first two are of direct interest in the study of planetary interiors and lead to certain inferences about the composition of the interior. Their determination depends heavily on the existence of earthquake-like disturbances. The third permits a few fairly firm inferences about how the internal dynamics of Mars compare with the Earth.

The seismicity of Mars is a most informative measure of the planet's internal regime. Questions of interest here are: Is Mars highly seismic, aseismic, or slightly so? If it is seismic, do the epicenters cluster along narrow structural belts? The relevance of these questions stems from recent discoveries about the relation of the Earth's seismicity to its large-scale internal motions. It has been found that major "thin" (50 to 100 km) shells of the Earth's surface, with continental dimensions, are moving relative to each other at rates of about 2 to 20 cm/yr. These motions are apparently driven by heat generated in the mantle and produce nearly all the Earth's seismicity, which occurs along the boundaries between shells. Information on the seismicity of Mars can indicate the extent to which it shows the same tectonic style. This is of direct relevance to the question of the history of Mars, the history of its volatiles, and the amount of heat generated in its interior.

Conditions required for meaningful results from a seismic experiment on Mars are: (a) some 10 to 20 earthquakes from various parts of Mars during the recording interval, (b) a three-axis seismometer with a bandwidth from 0 to 1 Hz, and (c) ability to resolve signals in the presence of wind-generated noise, and to isolate the instrument from the direct vibration caused by wind.

Whether these conditions can be met, we can state on the basis of current knowledge:

(a) One can only make educated guesses about the frequency of seismic events on Mars. We estimate that a recording period of 3 months is appropriate for 10 to 20 events. Useful, but incomplete internal models can be obtained from fewer signals, and the total absence of seismicity over this period would, as indicated above, be significant.

(b) The weight of the three-axis system is a matter of some concern. While existing systems for lunar and Earth studies weigh 25 pounds or more, substantially lighter instruments can be constructed through effective utilization of modern methods of position sensing and signal treatment. The requirement of a three-axis, as opposed to a single axis, instrument is made to provide maximum information from the few recorded events anticipated. It would be far more difficult to unravel the structure of the planet from z-axis data alone, and the results would tend to be ambiguous.

(c) The ability to resolve signals in the presence of wind-generated noise is an outstanding unsolved question. Theoretical and model study ought to be able to provide fairly good estimates of the noise produced by ground-coupling and direct instrument coupling from the wind. We may hope that orbital photography in an early Mars mission and numerical-theoretical work on the atmosphere will provide closer estimates of wind velocities near the surface.

Jupiter

Jupiter is obviously of great importance because of its huge mass and central location in the planetary system. A fly-by to Jupiter could be used to refine knowledge of its orbit and, perhaps, of its gravitational field, and could obtain information on its magnetosphere that might shed light on the nature of the planet's interior. However, ground-based radar observations of the Galilean satellites can provide more accurate orbital information and better determinations of Jupiter's gravitational field. The fly-by, on the other hand, can provide important data on Jupiter's heat balance and on the chemical composition of its atmosphere. Theoretical and laboratory studies based on such data offer great hope for improving our knowledge of Jupiter's interior.

Comets and Asteroids

While comets and asteroids are relatively low on priority lists, they are nevertheless extremely interesting objects which potentially could provide significant insight into questions of solar system evolution.

Next to eclipses of the Sun, comets are the celestial phenomena that have most aroused human curiosity since the earliest days of primitive man. We know relatively little about their structure and composition. Possibly they could give us information on the chemistry of objects from the outer fringes of the solar system, and hence indicate whether a chemical differentiation of the heavier elements occurred outward from the Sun.

A probe passing through the coma of a comet and on into the tail might sample ions, neutral particles, electrons, and dust, using a mass spectrometer, an ion spectrometer, and various other sensing devices to measure composition. Interesting problems also exist relating to interactions between the solar wind and ions streaming away from comets. Most important would be an analysis of a cometary nucleus, but that is beyond our present capabilities.

Asteroids are of particular interest because of their possible relation to meteorites, on which a vast amount of research has been performed in the last few years. However, feasible experiments to achieve some such correlation are lacking. Probably the only practicable experiment would be one that determined mass from tracking data, and volume by imaging: from these the mass density could be calculated.

In the case of both comets and asteroids the real need is for returned samples. To accomplish this seems beyond the realm of possibility in this decade.

Related Investigations

A number of scientific disciplines and techniques contribute to the solution of problems of planetary dynamics and interiors; only some of them are carried on within the framework of the space program. The optimal program of study would be one that attained the best balance of support among them. Some of the related areas of endeavor are:

Celestial Mechanics. The main dynamical facts that must be explained by a satisfactory theory of the origin of the solar system, such as the distribution of angular momentum and mass, and various near-commensurabilities among orbits, have been known for some time. However, appreciable progress has been stimulated in recent years by new measurements such as the radar determination of the rotation of Mercury. These new data point to the various orbit-orbit and spin-orbit couplings as consequences of tidal friction effects. Some of the other near-commensurabilities may be the consequence of other types of energy transfer at an earlier stage in the history of the solar system. In recent years a better understanding has developed of hydromagnetic clouds and other phenomena, so that it is reasonable to expect improved dynamical models of solar system formation.

Cosmochemistry. The dynamical model of solar system formation must provide the pressure and temperature environments necessary to account for the chemical differences between the Sun, the Earth's crust, the meteorites, and (soon to be learned) the Moon. These chemical restrictions are continually being refined.

Geophysics. Most of our ideas about terrestrial planetary interiors are derived from studies of the Earth. Recent developments such as the evidence of sea-floor spreading have led to new notions about rheology and thermal conditions in the mantle, which in turn apply to the interiors of other planets.

Stellar Evolution. A dominant effect in the origin of the planets may have been the over-luminous stage of formation of the Sun as it contracted onto the main sequence. The T Tauri stars appear to be stars in the final stage of contraction. Hence a satisfactory theory of solar system origin will explain many of the observed properties of T Tauri stars: their mass loss, enhanced radiation, nebulosity, etc; conversely, attainment of a better theory will be assisted by better observations of these stars.

Planetary Astronomy. There is still much to be accomplished by ground-based astronomical techniques. Those that contribute most importantly to the study of planetary dynamics and interiors are radar measurements (which yield orbit and spin data as well as surface properties, including variations in topographic heights) and ir and thermal emission observations (which offer a possibility of obtaining the rate of heat flow from planetary interiors).

Conclusions and Recommendations

The object of highest priority, balancing intrinsic importance and feasibility, is Venus. Venus is the planet most like the Earth, yet it has significant differences which, in the first place, must be explained by any theory of solar system origin, and in the second place, make Venus a test body for theories of the mechanical and thermal regime of the Earth. Because of its excessive surface temperature and thick atmosphere, however, the only useful measurements that appear feasible are determinations of the variations of the gravitational field from satellite orbit perturbations. Such determinations, preferably using orbiters of differing inclination, should yield significant boundary conditions on the mechanical and thermal state of the interior. The only space vehicle instrument required is the tracking transponder, so the three Venus orbiter missions proposed (see Table 3) can be relatively simple Pioneers and small orbiters.

The exploration of Mars is of second priority, since it is of somewhat smaller size and should have a lower level of internal activity. Variations of Mars' gravitational field can also be determined by orbiter perturbations. However, the greatest improvement in knowledge of Mars' interior will come from a seismometer placed on its surface. A three-axis seismometer can be made to weigh as little as 5 pounds; aside from determining seismicity, it might improve knowledge of the variation of density with depth. The surface chemistry of Mars should also be pertinent to considerations of the interior. Eventually it is hoped that some estimate of the heat flow may be obtainable from a combination of ir and radio-wavelength radiometry in orbit.

Mercury is of particular interest as the end member of the sequence of terrestrial planets; some estimate of its internal activity may be obtainable from Mariner-type surface photography. Although there is little prospect that Mercury has its own magnetic field, a measurement thereof should be attempted on a fly-by. Certainly a look should be taken at Mercury, if only to suggest the most appropriate questions to ask.

Jupiter is of the greatest importance of all, relative to the question of the origin of the solar system. However, it is of low priority in the framework of this report because of the great difficulty of obtaining significant new information concerning its interior with the small spacecraft available in the time period under consideration. In addition to providing magnetic field measurements and heat balance information, a Jupiter probe could also improve our knowledge of Jupiter's orbit and mass. It should also carry a micrometeorite detector so we can learn something about the density of small particles in the asteroid belt.

Comets and asteroids are of interest because they may contain material that has been relatively undisturbed since the origin of the solar system. The structure of comets may also indicate the manner in which material originally condensed in the solar nebula. With the simple probes available in the early and mid-1970's, however, it is difficult to find a useful measurement to make, other than perhaps to derive the mass density of an asteroid.

Of the related investigative techniques, it is most strongly urged that support be given to the construction and operation of a more powerful radar system to study the variations in planetary rotations, orbits, and topography. Only in this manner can we hope, for example, to obtain the moments of inertia of Venus and Mercury. The topographic variations of any planet are needed for any effective interpretation of its gravitational field.

Table 3 summarizes the relevance to interior/dynamical questions of various planetary missions suggested for the early 1970's. Most of these experiments will yield constraints or boundary conditions which are necessary but not sufficient to understand the actual state and history of the planetary bodies. Not until landed spacecraft permit the placement of seismometers can more direct information be obtained.

TABLE 3

OPTIMAL MISSIONS FOR PLANETARY DYNAMICS AND INTERIORS

Year	Body	Type Mission	Capabilities of Interest to Dynamics & Interiors
1969	Mars	Fly-by	Surface photography IR radiometry
1970	Venus	Orbiter	Tracking transponder (3 months in orbit)
1971	Mars	Orbiter	Photography IR radiometry Tracking, 3 mos. in orbit
1972	Venus	Orbiter	Tracking, 3 mos. in orbit; different inclination from '70
1972	Jupiter	Fly-by (2)	Magnetometer Micrometeorite detector Tracking
1973	Mars	Orbiter	Tracking, 3 mos. in orbit; different inclination from '71
1973	Mercury (via Venus)	Fly-by	Surface photography & photometry Magnetometer IR radiometer
1975	Venus	Orbiter	Tracking, 3 mos. in orbit; different inclination from '70 and '72.
1975	Mars	Orbiter-lander	Lander: 3-axis seismometer Alpha-scattering or x-ray diffraction-fluorescence Orbiter: Tracking, 3 mos. in orbit; different inclination from '73 IR and 10-cm radiometry

Chapter 6

PARTICLES, FIELDS, AND PLANETARY INTERACTIONS WITH THE SOLAR WIND

Introduction

A study of the near-space environment of the planets requires investigations *in situ* of planetary magnetic fields, ionospheres, exospheres, radiation belts, and interactions with the interplanetary medium. Such studies contribute both directly and indirectly to satisfying the three main objectives for the exploration of the solar system identified by the 1965 Woods Hole Study.

Most of the volume of space and a substantial fraction of the mass of the universe is composed of a magnetized, collisionless plasma containing electrons, protons, and ionized atoms of heavier elements. A dramatic example of this plasma is given by the Earth's radiation belts. The state of plasma, the nature of shock waves and of the instabilities occurring within it, and the large-scale processes that accelerate and produce energetic charged particles can very rarely be studied in the laboratory. The space program has presented scientists with unique opportunities for performing *in situ* measurements of plasma within the solar system; our increasing knowledge of basic plasma and high energy particle physics is useful in both astrophysics and in laboratory plasma studies such as controlled fusion. It also can directly answer questions fundamental to an understanding of the origin and evolution of our solar system.

The origin of the solar system by condensation from a solar nebula requires understanding the basic magnetohydromagnetic processes of the interaction of ionized gases and magnetic fields. Studies of the near-space environment of the planets provide us with an understanding and knowledge of these basic processes. Theoretical extrapolation to the past can then be accomplished with a degree of certainty heretofore impossible.

Recent Developments

Significant progress has been made in this general field since 1965 by the United States with the small Earth- and lunar-orbiting IMP satellites, the heliocentric Pioneer space probes and the Mariner 5 Venus fly-by in 1967. Considerable advances have been made in studying the solar wind -- natural plasma from the Sun -- which represents the evaporation of the solar atmosphere supersonically into interplanetary space. The interaction of this plasma with the Earth's magnetic field generates a detached bow shock wave as the solar wind flow is deflected around the Earth's magnetic field, forming an extended magnetic tail of the Earth quite similar to cometary tails. Acceleration of charged particles appears to take place in the vicinity of the bow shock wave and also in the neutral sheet imbedded in the Earth's magnetic tail. Both processes are of direct interest to plasma physics and controlled thermonuclear reactions in the laboratory.

The studies from Lunar-Explorer 35 (IMP 6) reveal the direct impact of solar plasma on the Moon and the very rapid diffusion of the imbedded interplanetary magnetic field through the lunar body. It indicates that the outer layers of the Moon are at relatively low temperatures and that the composition and internal temperatures cannot correspond to an origin similar to chondritic meteorites.

Mariner 5 data indicate that Venus has no appreciable magnetic field although its ionosphere leads to an interaction similar to the Earth's whereby a collisionless bow shock wave develops. These results were also obtained by the Soviet Venus probe which performed plasma and magnetic field measurements to within 200 km of the planetary surface.

In summary, these studies reveal the existence of two additional classes of solar wind interaction with the planets and suggest a new and powerful method for indirectly studying the planetary interior by observing the solar wind interaction with those planets that do not possess an appreciable intrinsic magnetic field or an ionosphere.

Relation to Prime Objectives

The study of the history and origin of the solar system is not a short term project but one that demands successive increments of understanding as progress toward the ultimate goal is achieved. A knowledge of the present state of the solar system is an important element of this understanding. As additional information on the near-space environment of the planets is obtained, a better understanding of the significance of differences in their environment will result. The sister planets Venus and Earth are now known to be radically different: although Venus has approximately the same size and average density as Earth, it has no magnetic field nor radiation belts.

An understanding of the origin of planetary atmospheres requires an evaluation of the long term effects of the contributions, both positive and negative, of the solar wind to the atmosphere. The nature of ancient planetary atmospheres requires estimates of the strength of the solar wind interaction with the planets.

As knowledge is gained concerning the near-space environment of the planets, it will be possible to evaluate the merits of specific theoretical models of the Earth's environment. One example is the strong support that the negative results obtained on a Venus magnetic field gives to theories that require rapid planetary rotation to produce planetary magnetic fields. The existence of the Earth's field prevents the solar wind from interacting directly with the terrestrial atmosphere. Solar disturbances do affect the atmosphere and ionosphere indirectly, in ways that affect basic day-to-day living on Earth, such as in communications and perhaps in long term weather cycles. The response of the Earth's atmosphere to charged particles from the Sun has been studied by observing the varying atmospheric effects on satellite motions.

The contributions that the study of near-space environments can make to an understanding of the origin and nature of life are at best indirect. The existence of a planetary magnetic field partially shields the surface from bombardment by biologically harmful radiation. More important, it facilitates the formation and retention of a planetary atmosphere which is indispensible to life and which also plays a larger role in blocking harmful radiation.

The long term effects of exposure to low level ionizing radiation are not well known. However, the establishment of quantitative values for cosmic ray background flux and for periodic outbursts of solar flare particles will provide estimates for comparison with dose levels to which present-day forms of life are exposed. Indeed, one of the results of satellite studies of the near-Earth environment has been to secure data on the radiation hazards of manned space flight. At the same time, spacecraft have provided quantitative information on the levels of radiation directly incident on those planets possessing no magnetic field and only a small atmosphere.

Measurements Required and Scientific Priorities

Detailed measurements of the near-space environment of the planets do not require large and expensive three-axis oriented spacecraft of the Mariner class or larger. It is possible to conduct highly accurate and precise measurements of magnetic fields, plasmas, energetic particle fluxes, and subsidiary measurements of the exospheric plasma in the planetary environment by ion and mass spectrometers from small and relatively inexpensive spacecraft. This was pointed out by the Space

Science Board in a report issued in 1967.* The present study strongly endorses the concept of using small spacecraft in the planetary exploration program as discussed in detail in that report.

Lack of Near-Space Environment Studies in Present Program

The present NASA program for planetary exploration does not include any direct, *in situ* measurements of the near-space environment. In fact, the Mariner-class spacecraft that have flown thus far have not provided sufficiently clean magnetic backgrounds for accurate studies of the interplanetary magnetic field or planetary magnetic fields. As the exploration of the solar system progresses and exploratory studies are replaced by refined measurements, it will be essential that magnetic field measurements of the planetary environments be performed on spacecraft that are magnetically clean along the lines of the IMP's, anchored IMP's (i.e., planetary or lunar orbiters), or Pioneers.

Such measurements can be best performed from spacecraft placed into moderately eccentric orbits and for which the possibility of decaying periapsis through the use of onboard propulsion will permit radial profiling of the exospheric and upper atmospheric composition. An important requirement of these orbital studies is that they extend for periods of approximately half a planetary year so that seasonal variations can be effectively studied and a synoptic monitoring both of the environment and its dynamic response to solar disturbances be carried out.

Anchored Monitoring Platforms

Thus far, measurements of the interplanetary medium have been made on space probes from the orbits of Venus to Mars, but not beyond. The extension of these measurements to the outer regions of the solar system beyond Jupiter and closer to the Sun than the planet Mercury will contribute significantly to our understanding of the present-day solar system and our development of appropriate theoretical models. Simultaneous observations from anchored monitoring platforms will permit detailed studies of the propagation of solar disturbances into interplanetary space and the response of the planetary environment, the ionospheres and atmospheres, to such phenomena.

Adequate provision for availability and scheduling of ground-based antenna facilities in support of the monitoring programs must be considered in any long range plans for systematic study of the solar system.

Planetary Priorities

The planet having the highest priority by far in terms of studying its near-space environment is Jupiter. In addition to its radio emission, which demonstrates the existence of a large planetary magnetic field and a huge radiation belt, the study of its near-space environment is significant with respect to a study of the depth of penetration of the solar wind into deep space. Jupiter at 5 AU may be near the boundary separating our own solar-dominated environment from that dominated by the galaxy. An early exploratory probe to Jupiter would provide a definitive study of the radial gradients of the physical properties of the interplanetary medium as well as establishing the quantitative nature of the Jovian environment.

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*"Report of a Study on Explorations in Space with Sub-Voyager Systems," Space Science Board, NAS-NRC, Washington, D C., 1967

Measurements beyond 5 AU present practical difficulties with respect to power sources, and radioisotope thermonuclear generators may be necessary to furnish sufficient thrust and effective communications.

As previously discussed, the Moon, Venus, and Earth represent a suite of near-space environments that becomes increasingly complex as interaction with the interplanetary environment progresses from minimum to maximum. Although Venus has been identified as an intermediate case between Moon and Earth with respect to its near-space environment, no synoptic studies have yet been performed. Thus, a small spacecraft to monitor and study synoptically the Venus environment, to define more precisely the effects of the solar wind sweeping on the high atmosphere and to investigate the formation of the ionosphere, are of high priority in the study of near-space environments.

A distant fly-by of Mars by the Mariner 4 spacecraft has established only a conservative upper limit to the presence of a planetary magnetic field. While it is expected that the solar wind interaction with this planet and its magnetic field is more analogous to Venus than to Earth or Moon no positive conclusions can be drawn without definitive measurements <u>in situ</u> of the magnetic field, plasma, and charged particle environments. The higher rotation rate of Mars, once every 24 hours, may induce the formation of an intrinsic planetary field. The absence of nonthermal emission from Mars, however, does not encourage this point of view.

Little is known about the possible magnetic field of Mercury. The planet's high density and probable lack of an ionosphere suggest that a study of solar wind interaction with the planet can provide information concerning the electrical conductivity and thermal regime of the interior, as in the case of the Moon.

In summary, balancing scientific interest and technical feasibility, the following ordering in priority of the planets is suggested:

1. Jupiter
2. Mars
3. Venus
4. Mercury

We strongly endorse present plans for Pioneer F and G fly-bys of Jupiter in 1973 and 1974. A series of small planetary orbiters as recommended in this report (see Chapter 2) will provide the necessary measurements of the Mars and Venus near-space environments in the early 1970's at relatively low cost. Finally, a Mercury-Venus swing-by mission in the mid-1970's will present an opportunity for detailed studies of Mercury's environment.

Chapter 7

EXOBIOLOGY

The biological examination of a planet other than the Earth was considered at length in Biology and the Exploration of Mars.* The scientific justifications, the proposed general strategies for various kinds of explorations, and the detailed review of many subordinate aspects of this general topic presented in this volume constitute a useful background for current planning of planetary exploration.

What is Life?

Life is not a thing in itself; rather it refers to a state of chemical complexity. Its physical basis occurs in discrete units called living organisms. A living organism is a coherent, heterogeneous, and thermodynamically improbable assemblage of molecules that is far out of equilibrium with its immediate surroundings and exhibits self-coordinated internal functions. It interacts with its environment as an open system through which a continual flow of energy and materials must occur, channelled by a highly specific catalysis that enables this living system to be self-maintaining and self-replicating. Essential to the catalyzed energy flow and to the replicative process are large molecules of especially high information content, the structural requirements for which can be met, we believe, only by a chemistry based on carbon. The chemical and energetic changes that constitute the metabolism of the organism can occur only in association with a polar solvent; of the solvents, liquid water appears to be the only reasonable possibility. The requirements that the chemistry be based on carbon and that the solvent be water establish a limited temperature range, dependent on solute content and atmospheric pressure, within which the essentially aqueous living system can continue to function.

We thus assume a carbon-water biochemistry. The relative cosmic abundance of the pertinent elements, the spontaneous formation of organic compounds, the ability of carbon to combine with many other elements and especially with itself to form large compounds stable enough for continuity and labile enough for metabolism, and the anomalous behavior of water (with temperature) speak for this assumption. Exotic biochemistries based, say, on silicon are quite unlikely.

How to Search

The search for life should be preceded by an identification of environments where conditions are compatible with the existence of living systems. Thus, compounds of carbon, some water (not necessarily oceans), and a temperature range that permits water to exist in liquid form at least part of the time are absolute requirements. Additionally, the ultimate energy source needed to maintain a highly evolved system of living organisms is radiant energy, and the form in which carbon enters the open system is most likely to be carbon dioxide. Therefore a search for life could be rewarding where light can penetrate and where there is diffusional access to CO_2. Exploring a planetary surface with respect to these environmental variables will tell us whether life is possible there and in what specific regions it is most likely to be found. Such an examination of the environment is not, however, a search for life itself.

To demonstrate in fact that extraterrestrial life exists, we must either

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*Biology and the Exploration of Mars: Report of a study held under the auspices of the Space Science Board, 1964-1965, C. S. Pittendrigh, W. Vishniac, and J. P. T. Pearman, eds., NAS-NRC Pub. 1296, Washington, D C., 1966.

(a) identify organisms by imaging methods (by recognizing an unmistakably biologically derived morphology), (b) identify environmental factors that point incontestably to the influence of life processes (certain isotope fractionations, major departures from chemical equilibrium, consumption or production of CO_2 or other specific substances) or (c) observe an increase of biomass or a catalytic activity for which a nonbiological explanation would be considered most unlikely. Any or all of these things can be accomplished upon the planetary surface or on a returned sample here on the Earth.

Consequences of Discovering Extraterrestrial Life or of Negative Results

The discovery that life exists on another planet would yield unique information on the origin of life anywhere. If the extraterrestrial life were fundamentally different in its chemical organization from life on the Earth, we should have to conclude that it originated independently of terrestrial life. This would imply strongly that life is a common phenomenon in the universe. If the extraterrestrial biota bore a close chemical resemblance to terrestrial life, we would entertain the possibility that life originated on one planet and was transferred to the other. This would suggest that the origin of life is an unlikely event, and that transferral of living matter through interplanetary space is more probable than is now generally thought to be the case.

The discovery of life on another planet would have consequences far beyond its immediate scientific implications. Such a discovery would be one of the momentous events of human history. Its effects on man's view of himself, of nature, and of the universe would be profound and far-reaching.

Failure to find life on a planet whose environment was compatible with life would imply that the origin of life is an unusual event, not a predictable outcome of geochemical processes. This information, too, would be of scientific value for it would demand a reappraisal of the widely held assumption that life inevitably originates in any hospitable environment.

Strategy and Tactics

Our choice of planetary search targets is limited. Besides Earth, only Mars appears to be suitable for life as we know it. Mars provides an environment of light, water (albeit in severely limiting amounts), and CO_2. The temperature range is compatible with terrestrial life. The Venus surface is too hot, and the idea of a floating biota at great altitude, while not to be rejected out of hand, requires special and complicated assumptions. Too little is known about Jupiter, and the outer planets are too cold.

In common with his colleagues in other disciplines, the biologist searching for life is interested in atmospheric composition, soil structure and composition, water economy, meteorological data, temperatures, and other physical parameters such as radiation flux. The biologist is primarily interested in data pertaining to the vicinity of the surface, i.e., the microclimate immediately above, at, or just below, the surface. In atmospheric analyses the gases of interest to him include, besides the major component, CO_2, the small amounts, if any, of H_2S, NO, NH_3, HCN, CH_4, CO, N_2, O_2, and volatile organic compounds. He is also interested in the noble gases, He, Ne, A, which can tell something of the atmospheric history of the planet. Soil analysis to him means in particular a search for organic compounds and determination of water content. He searches for departures from predictable equilibria: both in chemical compositions, which are thermodynamically unlikely unless some continuous chemical activity (possibly biological) regenerates them, or in isotopic distributions which suggest a continuous fractionation.

One exclusively biological experiment is the attempt to observe "active biochemistry," be it growth or a catalytic activity that is unlikely to be of nonbiological origin.

The measurement of temperature is bound to be of some use, but its biological significance (with respect to determining the presence or absence of life) is low. Hence this is a low risk - low yield measurement. The attempt to determine active biochemistry is risky, but an affirmative observation would have a very high yield indeed.

The strategy of a biological mission should therefore be to carry out a variety of observations among which risk and yield are properly balanced. The determination of atmospheric composition, for example, carries with it little risk because we are bound to obtain useful information and are independent of the mechanically difficult problem of obtaining a solid sample. At the same time, it provides us with significant yield since the information is of biological relevance (more so if carried out over several diurnal cycles), and can even be indicative of life if it shows the atmosphere to be far from an equilibrium mixture.

Conclusive Negative Results

It is easy to describe the kinds of evidence that would demonstrate that Mars is inhabited, but is it possible to prove the negative? What observations would convince biologists that Mars is a dead planet? The following, taken together, would, we believe, constitute such proof for most biologists:

(a) Demonstration that the observed seasonal changes (wave of darkening) on the planet result from nonbiological causes

(b) Finding that Mars has negligible amounts of water

(c) Finding that the Martian atmosphere is essentially in chemical equilibrium (or, more correctly, does not depart significantly from the steady state expected from interaction of the atmosphere with solar radiation)

(d) Demonstration that Martian soil at a number of different sites contains organic matter no different than that expected from meteoritic infall

(e) Failure to find evidences of the existence of liquid water on the planet in the past. Such evidence could be obtained by photoimaging from an orbiter and by chemical analysis of the soil (specifically, with reference to the presence or absence of clay minerals or other hydrated material)

(f) Negative results in life-seeking experiments

Timing

An early biological mission to Mars is desirable because of:

(a) Scientific and philosophical significance. The discovery of life on another planet would, as noted above, be one of the momentous events of human history, with profound and far-reaching implications. It is within our capacity, for a relatively small expenditure of money and effort, to reap a tremendous harvest.

(b) The problem of possible contanimation. Many biologists consider Mars to be a suitable environment for the multiplication of certain types of terrestrial microorganisms. This opinion is not unanimous, but so long as the issue remains unsettled, it will be prudent to carry out biological studies on Mars at the earliest opportunity. Thus the planet can be examined before any rocket-borne organisms can have altered the Martian ecology, and the question of whether terrestrial microorganisms can infect Mars will be resolved. If the answer is negative, spacecraft sterilization thereafter will be unnecessary.

A Program for 1969-1973

The 1969 Mariner-Mars fly-by spacecraft are, at this writing, well on their way to completion. These spacecraft, if successful, will greatly enlarge our knowledge of Mars. They will acquire new photographs of the Martian surface at higher resolutions than any yet obtained, and of the entire planet at lower resolutions. They will make radiometric measurements of the surface temperature and spectrometric analyses of the atmosphere from the neighborhood of the planet. Our attitudes toward Mars as a possible habitat for life will be strongly influenced by the results of this mission. It is equally true, however, that the Mariner '69 fly-bys will not settle the question of life on Mars and that further study of the planet will be needed.

The Mars 1971 orbiter program recommended elsewhere in this report will observe the important wave-of-darkening phenomenon, whose existence first led astronomers to suggest that Mars is an inhabited planet. It will map most of the planet photographically, and it will seek locales especially favorable for life by examining the surface and atmosphere for evidences of higher-than-average water content. These results will strongly influence the choice of a landing site for the Mars 1973 lander.

The 1973 Titan/Centaur-launched Mars orbiter/lander which is recommended in this report will be the first U. S. planetary entry-and-lander mission. A suggested landed payload for biological investigation of the planet is described in Chapter 2. The entry-lander capsule will be accompanied by an orbiter whose primary function will be to support the capsule as a relay link. There is a strong possibility that a soft landing by means of retro-descent would contaminate the atmosphere and surface, and thereby invalidate the scientific experiments. A "hard" or "rough" landing is therefore far the preferable for this mission unless it can be demonstrated that the retro-descent will not, in fact, disturb the area to be sampled nor interfere with the atmospheric analyses to be obtained during entry.

Second-Generation Lander

After the biologically significant missions recommended by this study (1969 Mars fly-by, 1971 Mars orbiter, and 1973 Mars orbiter/lander) have been completed, what will remain to be done? If we assume that after we have digested the results of these missions Mars will command enhanced biological interest, then NASA should be prepared to optimize its exobiological effort and doubtless will be justified in raising priorities for experimental work on the Martian surface. The next generation lander/orbiter, which we recommend should be planned for 1975, will build on earlier mission results and therefore will be able to carry out more sophisticated experiments. Surface exploration by means of a roving vehicle carrying a complement of scientific instruments -- a mobile laboratory for biochemical and related studies -- would be very desirable, allowing us to take advantage of a potentially great opportunity to study an exotic biota in detail. Instruments on the vehicle might include "wet chemical" apparatus not recommended for the 1973 lander payload. NASA should carry out studies to determine whether a Titan/Centaur vehicle would provide the payload capacity to accomplish a suitably ambitious mission at the 1975 opportunity. Alternatively, if Saturn-class vehicles are made available to the planetary program for a 1975 mission, it would provide an opportunity to introduce a mission which will effectively utilize this capacity into the planning for Martian exploration.

In conclusion, we emphatically support one of NASA's major goals: to increase our understanding of the origin and nature of life. We recognize that this is achievable mainly through survivable Martian lander missions when properly supported by fly-by, orbiter, and entry science. In terms of the particular missions recommended in this Study, we note that the 1969 fly-bys followed by the 1971 orbiter and the subsequent 1973 orbiter/lander are rational steps that could place us in position, for the first time, to make close-up observations of the kind that could establish whether living organisms are present or absent on Mars.