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# NATIONAL ACADEMY OF SCIENCES
# NATIONAL RESEARCH COUNCIL
OF THE UNITED STATES OF AMERICA

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SPACE SCIENCE BOARD

21 February 1965

Memorandum

To: Exobiology Study Participants

From: J. P. T. Pearman, Executive Director

Subject: Summary Chapter - Final Review

The Summary, previously circulated, has been revised by the Steering Committee on the basis of comments received. The revised text (but without preface, title page and table of contents) is enclosed for your final review.

Please advise me of any major points you find unacceptable at your earliest convenience. If we do not hear from you by February 26, we shall assume concurrence. Please do not hesitate to telephone or wire collect, if necessary.

I trust you will excuse this note of urgency - we wish to make the report available with the least additional delay.

## I. Introduction

Until recent years the origin of life and its possible occurrence elsewhere in the universe have been matters for speculation only. The rapid growth of molecular biology since 1940 has, to be sure, changed the discussion of life's origins into far more precise and explicit terms than were possible earlier; and the subject entered a new, experimental, phase in the 1950's with successful abiogenic synthesis of important biochemical substances in conditions simulating the presumptive environment of the primitive earth. But the real transformation which the subject has undergone stems from the spectacular growth of space technology in the last decade. The possibility of life's origin and occurrence on planets other than ours is no longer limited to idle speculation: it has entered the realm of the testable, of science in the strict sense. Given the rockets now available and especially those available by 1969 it has become fully realistic to consider plans for the biological exploration of Mars.

The Study which this report seeks to interpret was convened in June, 1964, by the Space Science Board of the National Academy of Sciences to examine this possibility. The working group comprised 36 people representing a broad spectrum of scientific interests: evolutionary biology, genetics, microbiology, biochemistry and molecular biology, animal physiology, soil chemistry, organic chemistry, planetary astronomy, geochemistry, and theoretical physics. The membership included some with considerable prior involvement in problems of space exploration and others with none. Advice was also sought outside the group of immediate participants on the potentialities of selected analytical methods for the experimental study of extraterrestrial life and its environment. More than 30 individuals contributed in this fashion written assessments of techniques in which they were particularly well versed.

Our task was to examine the scientific foundations and merits of the proposal to undertake a biological exploration of Mars. What were the potential scientific yields? How valuable, if attained, would they be? What, in fact, is the possibility of life occurring on Mars? And of our detecting it with the available and foreseeable technology? What could be achieved by further astronomical work from earth? by Martian fly-by missions? by Martian orbiters? and Martian landers? What payloads would we recommend for planetary missions? What timing and overall strategy would we recommend for Martian exploration were we to consider it worthwhile at all?

In brief the overall purpose was to recommend to the Government through the Academy's Space Science Board, whether or not a biological exploration of Mars should be included in the nation's space program over the next few decades; and, further, to outline what that program, if any, should be.

We emphasize that our conclusions were reached on strictly scientific grounds: that we recognize a much wider array of considerations bear on any ultimate decision to undertake Martian exploration. As a body we were not charged with nor did we attempt the broad over-view that entails these other considerations. We predicated our discussion on the continued vigor of a national space program. We did not, for instance, address ourselves to the question of whether the very large cost of developing the Saturn boosters could be justified on scientific grounds. Nor should we have; the development of the Saturn boosters is already firmly committed for other reasons. The questions we faced were whether the application of such boosters to the biological exploration of the solar system - of Mars in particular - can answer well-framed and important scientific questions; and what priority these questions merit within the space program.

## II. The Origin and Nature of Life

The modern, naturalistic view of life's origin and evolution dates from the foundations of modern biology a century ago. Implicit in the evolutionary treatment of life is the proposition that the first appearance of organisms was only a chapter in the natural history of the planet as a whole. Oparin later made this notion explicit in his view that the origin of life was a fully natural, perhaps inevitable, step in the ontogeny of the Earth. Systems capable of self-replication and controlled energy transfer - living organisms - had their origin in the sequence of chemical changes that were part of the planet's early history.

The tractability of this great inductive step to further discussion has been enhanced by the progress of terrestrial cellular biology and biochemistry over the last few decades. What has emerged from that progress is a unified picture of life at the subcellular and chemical levels, underlying the unity at higher levels which so largely influenced Darwin. Not only is there a common pattern to the structure of cellular organelles - membranes, mitochondria, nuclear apparatus, etc. - but a still more surprising unity is found in its molecular constituents. Everywhere on Earth the essential catalytic functions are discharged by proteins, energy transfer effected by ATP, and the synthesis of proteins today controlled by an elaborate nucleic acid system. The same enzymatic cofactors are found in organism after organism; particular metabolic pathways recur from cell to cell; and everywhere the fundamental functions of information storage and replication are assigned to the nucleic acids.

To a significant extent the discussion of life's origin must concern the origin of those molecular types that are crucial in cellular organization: the origin of nucleic acids, of proteins, of carbohydrates, and so on.

In the 1950's a series of experiments was initiated in which the synthesis of biologically important compounds was accomplished by application of energy to presumptive primitive environments. The list includes: amino acids and their polymers; carbohydrates and fatty acids; purines and pyrimidines; nucleotides, including adenosine triphosphate (ATP), and oligonucleotides - every major category of molecular sub-unit of which the cell is built.

The credibility of the naturalistic, evolutionary view of life's origin as an exploitation of previous chemical evolution on a sterile Earth is greatly heightened by these results: the great chemical complexity of its molecular constituents does not, in last analysis, require the intervention of the cell itself.

The general tenet that life involves no qualitative novelty - no élan vital - goes hand in hand with the more explicit proposition that it is the molecular organization, as such, of living things that alone distinguishes them from the non-living. The central issue in discussing origins now concerns not so much the prior evolution of complexity in molecular constituents as the attainment of their organization into a system that is alive. It is here we lack any sure guides - save one - on the contingency involved; on how improbable it all was. That one lead comes from the great and well-known advances of molecular genetics in the past ten years.

The essence of organization in one sense is its improbability, its dependence on specification or information. And the most characteristic feature of living organizations - organisms - is their capacity to store and replicate the evolving information on which their existence depends. The high point of our biochemical advance has been identification of the molecular basis of these defining characteristics. It is astonishing how much we have recently learned about the manner in which the information underlying life's organization is encoded in molecular structure; that we understand how that molecular structure is replicated; and further that simple polynucleotides have been synthesized in cell-free systems.

It remains unclear, of course, what precise sequence of events exploited the opportunities afforded by the purely chemical evolution of the earth's surface and atmosphere. But at some point in the unknown sequence a community of molecules would have been fully recognizable to us as a living as against a non-living thing: it would have been bounded from its environment by a membrane, capable of controlled energy expenditure in fabricating more of itself and endowed with the capacity to store and replicate information.

We cannot fully know the precise course of the earth's early chemical evolution, and the degree of contingency involved in the subsequent transition to a living organization of molecules; and for these reasons we cannot fully assess just how probable or improbable life's origin was at the outset of our own planet's evolution. Nor can we estimate to what extent the emerging picture of a single chemical basis to life on Earth reflects a physical necessity for living organization as against a mixture of physical sufficiency and historical accident. Is it so that the catalysis essential to chemical organization can be effected only by proteins containing the 20 amino acids we encounter in cells; and that the nucleic acids are the only polymers, for physical reasons, that can carry molecular information satisfactorily? Or are these and other empirical generalizations about life on earth, such as optical activity, merely a reflection of the historical contingency that gave such molecules first access to living organization, thus preempting the field; precluding realization of other physically sufficient molecular foundations for life.

To the extent we cannot answer these questions we lack a true theoretical biology as against an elaborate natural history of life on this planet. We cannot prejudge the likelihood of life's appearance on Earth; therefore we cannot confidently take the great inductive step when we are told by astronomers that there may be $10^{20}$ planetary systems elsewhere in the universe with histories comparable to our own. One thing is clear - for life to be unique to our planet the probability of its origin must be almost unimaginably low. If, on the other hand, the probability is at all reasonable, life must be abundant in the $10^{20}$ planetary systems that fill the sky.

What is at stake in this uncertainty is nothing less than knowledge of our place in nature. It is the major reason why the sudden opportunity to explore a neighboring planet for life is so immensely important.

We emphasize that the act of discovery itself would have this great scientific, and for that matter philosophical, impact. But it is also important that discovery would, in another way, be only the beginning. The existence and accessibility of Martian life would mark the beginning of a true general biology of which the terrestrial is a special case. We would have a unique opportunity to shed new light on the meaning of that astonishing molecular similarity in all terrestrial organisms. Is it there as a physically necessary basis for life? Or is it - physically sufficient but not necessary - an historical accident in the sense that in another instance of planetary evolution a different basic chemical complexity could equally well have emerged and preempted the local opportunity for life?

## III. The Possibility of Life on Mars

No thoughtful person will disagree with our assertion on the scientific importance of life elsewhere in the solar system. It is however another matter to conclude that search for it should proceed at once. The exploration will be costly in money and other resources. To undertake it we need some assurance it is not folly from the outset.

Interest immediately focuses on Mars. The nearest and most earth-like of the planets in the solar system are Mars and Venus,but the surface of Venus has been tentatively excluded as a possible abode of life, because of the probably high surface temperatures. The Martian year is long (687 days) but the length of its day is curiously similar to that of Earth, a fact that to considerable degree ameliorates an otherwise very severe environment.

Mars has retained an atmosphere, although it is thin; current estimates of pressure range from 10 to 80 millibars at the surface. Its only certainly identified constituents are carbon dioxide (accounting for 5-30% of the total) and water vapor present in very much smaller amounts ($2 \times 10^{-3}$ gm $cm^{-2}$). Oxygen has been searched for and not detected; the sensitivity of such measurements implies a partial pressure not greater than 0.25 millibars. Nitrogen and argon are believed to constitute the bulk of the remainder. The flux of ultraviolet radiation at the Martian surface may be high, but this is not yet certain. However, some models of the composition of the atmosphere allow for effective shielding.

The surface temperatures overlap the range on Earth: at some latitudes and seasons they have a daily high of +30°C with a diurnal range of about 100° C.

There are two white polar caps whose composition has been the subject of some controversy. The evidence now is clear that they are ice, in the form of hoar frost. They undergo a seasonal waxing and waning which is probably accompanied by an atmospheric transfer of water vapor from one hemisphere to another.

Our knowledge of what lies between the polar caps is limited to the distinction between the so-called "dark" and "bright" areas and their seasonal changes. The latter, usually considered "deserts", are an orange-ochre or buff color. The former are much less vividly colored. It is likely that early descriptions of the dark areas as green result from an optical illusion due to contrast with the orange "bright" areas.

Biological interest nevertheless continues to center on the "dark" areas. In several respects they exhibit the kind of seasonal change one would expect were they due to the presence of organisms absent in the "bright" (desert) areas. In spring the recession of the ice cap is accompanied by development of a dark collar at its border, and as the spring advances a wave of darkening proceeds towards the equator and, in fact, overshoots it 20° into the opposite hemisphere.

Polarimetric studies suggest that much of the Martian surface may be covered with small sub-millimeter sized particles. The curve on which this inference is based shows a seasonal displacement in the dark areas, but not in the bright. Infrared absorption features have been at ributed to the dark areas, suggesting abundant H-C bonds there - more recent analysis throws great doubt on his interpretation, leaving us with no definite information, one way or the other about the existence and distribution of organic matter.

Needless to say, none of these inferences about the Martian dark areas <u>demands</u> the presence of organisms for their explanation.

Indeed, the question is whether the Martian environment could support life at all; and further, whether the history would have permitted the indigenous origin of life. These are clearly different questions. Our answer to the first question is that we find no compelling evidence that Mars could not support life even of a kind chemically similar to our own. Were oxygen present to the small limiting extent current measurements allow, a fully aerobic respiration would be possible. But even its total absence would not of itself preclude life. One of our more rewarding exercises has been the challenge to construct a Martian ecology assuming the most adverse conditions indicated by present knowledge: it posed no insuperable problem. Some terrestrial organisms have already been shown to survive freeze-thaw cycles of +30° to -70°C. Others are known to cope with extremely low humidities and derive their water supply metabolically. There are many conceivable ways of coping with a strong flux of ultraviolet (and even of exploiting it as an energy source). The history of our own planet provides plenty of evidence that, once attained, living organization is capable of evolving adjustments to very extreme environments. And, finally, we are reminded that the evidence we have on Martian conditions is very coarse-grained, a sort of average that takes account of almost no local variations dependent on topography. Within the range of conditions represented by our present numerical estimates it is likely that there exist, perhaps abundantly - as on Earth - places where the extremes of temperature, aridity, and adverse irradiation are markedly ameliorated. Even the presence of water in the liquid phase is perhaps not unlikely, if only transiently, by season, in the subsoil.

A measure of our judgment that niches in the contemporary Martian environment could support life of a sort comparable to that of Earth is provided by our overriding concern with the danger of inadvertently contaminating Mars with terrestrial organisms. We shall return to this problem later.

The other question - whether life in fact <u>is</u> there - depends on our judgment of how probable its <u>origin</u> on Mars has been. This is precisely the question we cannot answer even for Earth and the principal reason for <u>considering</u> exploration in the first place.

Given all the evidence presently available we believe it entirely reasonable that Mars is inhabited with living organisms and that life independently originated there. However, it should be clearly recognized that our conclusion that the biological exploration of Mars will be a rewarding venture does not depend on the hypothesis of Martian life. The scientific questions which ought not to be prejudged are:

a. Is terrestrial life unique? The discovery of Martian life would provide an unequivocal answer.

b. What is the geochemical (and geophysical) history of an Earth-like planet undisturbed by living organisms? If we discover that Mars is sterile we may find answers to this alternative and highly significant question.

## IV. The Scientific Aims of Martian Exploration

We approach the prospect of Martian exploration as evolutionary biologists. The origin of organisms was a chapter in the natural history of the Earth's surface. The hypothesis to be tested is a generalization from that single case: the origin of living organization is a probable event in the evolution of all planetary crusts that resemble ours. We thus conceive the over-all mission as a systematic study of the evolution of the Martian surface and atmosphere: has that evolution included, in some niches of the planet, chemical systems with the kind of organization we would recognize as "living"?

Our aims in summary form are:

(1) The determination of the physical and chemical conditions of the Martian surface as a potential environment for life,
(2) the determination whether or not life is or has been present on Mars,
(3) the characteristics of that life, if present, and
(4) investigation of the pattern of chemical evolution without life.

This formulation emphasizes that as biologists we have as much interest as the planetary astronomers in a thorough study of the meteorology, geochemistry, geophysics, and topography of Mars. Whatever the outcome of a direct search for life, its full meaning will escape us unless the findings can be related to the prevailing environment.

## V. Avoiding the Contamination of Mars

Before proceeding to the more programmatic aspects of the undertaking, we are concerned to single out the task of spacecraft sterilization from the many and diverse problems that Martian exploration will entail. We believe that many of our non-biologist colleagues have still not fully grasped either the magnitude or the fundamental importance of this issue.

Contamination of the Martian surface with terrestrial microbes could irreversibly destroy a truly unique opportunity for mankind to pursue a study of extraterrestrial life. Other future uses of Mars are not evident to us now; whatever they are, they may be clumsily destroyed by premature and uninformed mistakes in our program. We are eager to press Martian exploration as expeditiously as the technology and other factors permit. However, our present sure knowledge of Mars is very slim and so our recommendation to proceed is subject to one rigorous qualification: that no viable terrestrial microorganism reach the Martian surface until we can make a confident assessment of the consequences.

In operational context this means that the probability of a single viable organism reaching the Martian surface be made small enough to meet scientifically acceptable standards. These standards, already established provisionally* should be continually reexamined in the light of all new information. Moreover, every effort should be made to ensure the continued acceptance by other launching nations of the recommended confidence levels for protection of Mars against contamination. The technical problems precipitated by this demand include the control of trajectories to

* Report of COSPAR Seventh Meeting, Florence, Italy, May 1964, Resolution 26.

an accuracy sufficient to prevent the accidental impact of unsterilized payloads, the development of sterilizable spacecraft components for vehicles intended for landing, the development of procedures which will prevent the introduction of microorganisms and the means for establishing the reliability of the entire program. Since we have not yet succeeded in sterilizing a space vehicle, the problem must be considered unsolved.

An energetic program for the development of sterilization procedures of space vehicles and their components must be implemented immediately if we are to take advantage of the opportunities which will arise between 1969 and 1973. We must guard not only against accidental neglect of necessary safeguards but also against placing ephemeral considerations of prestige above enduring scientific significance and utilitarian value of our exploration of space.

## VI. Avenues of Approach to the Exploration of Mars

For convenience, we distinguish four categories of work that can contribute to attaining our goals: (a) laboratory work needed to develop techniques for planetary investigations and the knowledge needed to interpret their findings; (b) Earth-bound astronomical studies of Mars; (c) the use of spacecraft for the remote investigation of Mars; and (d) a direct study of the Martian surface by landing missions.

### (a) Laboratory work

The consideration of the evolution of life on Mars raises many problems which can be studied in Earth-based laboratories. Such studies are, in fact, essential to provide the background against which the results of planetary missions must be interpreted. The work includes the chemical analysis of meteorites, especially with respect to their content of organic compounds, and the extension of studies on the spontaneous formation of organic molecules and their aggregation into larger units. These investigations may reveal to us the mechanism by which not only the materials essential for living organisms were first formed, but also the origin of reactions and mechanisms that lead to the formation of organized structures and their self-perpetuation. Other possibly interesting lines of effort include alternatives to the carbon-water system of biochemistry and simulations of Martian and other planetary environments. While some of these simulated environments may allow terrestrial microorganisms or enzyme systems to function, others may be more conducive to the activity of reaction systems based on alternative biochemistries.

It will become clear later that considerable work remains to be done in defining schemes for life detection and in developing the instrumentation to exploit them.

### (b) Earth-bound Astronomical Studies of Mars

The observation of Mars from terrestrial observatories enjoys the advantages of economy, absence of weight and size limitations, and high data rate. It is however limited by the terrestrial atmosphere in attainable resolution and spectral range and further constrained by daylight and weather. Nevertheless, much valuable work could be conducted at a cost which is low compared to that of space programs if the nation's large instruments were made available during prime seeing time for the observation of Mars. The use of 120" and 200" optical telescopes and of the largest radio telescopes and interferometers could rapidly extend our knowledge of Mars. We support the recommendations of another committee of the National Academy of Sciences* on the

* Ground-Based Astronomy, A Ten-Year Program, National Academy of Sciences Publication No. 1234, 1964.

development of ground based astronomical facilities. For such facilities to play a significant role in the planning of 1969-73 Mars missions work on this program must be begun early.

(c) The use of Spacecraft for Remote Observation of Mars

Some of the observational limitations imposed by the terrestrial environment can be overcome by balloon-borne observatories but since they are severely restricted in size and observation time their usefulness is limited; it is also restricted by absorption in the Earth's atmosphere. The projected Earth-orbiting astronomical observatory (OAO) overcomes some of these limitations and we believe the observation of Mars particular in the ultraviolet should be included in the plans for its use.

It is, however, from Martian fly-by missions and, in particular, from Martian orbiters that the remote observation of that planet is best undertaken. We hope to obtain our first closeup information on the Martian surface from the video scan to be carried out by Mariner IV and gain additional knowledge of atmospheric density by observation of the telemetry signals during occultation of the spacecraft.

Fly-by missions are, however, severely limited in the time available for observation; they provide at best a fleeting glimpse of the planet.

Martian orbiters will be technically possible for the opportunities of 1969 and thereafter. They offer an unparalleled opportunity to scrutinize the planet at comparatively short range. Potential orbiter payloads have been examined by another group and compositions of such payloads have been suggested for a range of instrument weights up to 200 lbs. (which is within the capability of the Saturn IB-Centaur).For example, a modest payload which any of several vehicles could place in orbit might include instruments for (1) infrared and television mapping; (2) microwave radiometry; (3) infrared spectrometry; and (4) optical polarimetry. These sensors would yield information on temperatures, surface and atmospheric composition, topography, certain characteristics of surface structure, etc. and, most important of all, permit a sustained scrutiny through a full cycle of seasonal change and over a major fraction of the Martian surface.

(d) Martian landing missions: ABL's small and large

While it is conceivable that the findings of a Martian orbiter could establish the presence of life on the planet, we are in any case convinced that landing missions are essential for adequate Martian exploration. The definition of lander payloads is a complex and demanding task which we have only begun to explore.

Their design is to some extent dependent on our knowledge of the structure of the Martian atmosphere. The size of the payload that can be deposited depends, for instance, on whether the use of a parachute is feasible or whether the density of the atmosphere is so low as to require the use of retrorockets - this is especially critical for small payloads. In this connection, we note the possibility that the density profile of the Martian atmosphere will be determined by astronomical means, or by Mariner IV, with sufficient precision for the purpose of designing a landing system. A more direct method for studying the Martian atmosphere involves the use of non-survivable atmospheric entry probes that could transmit information on atmospheric density structure and composition. Such probes could be launched from either fly-bys or orbiters. Since their design is not dependent on atmospheric density, these are useful devices for obtaining advance information if needed, for the survivable landing of an instrument package. The view has also been presented that a small surviving capsule would have even more value, in that it might determine not only the density profile of the atmosphere, but also its composition at the surface, wind velocity and other data that would enhance the probability of success of a large lander.

However, if we had a complete knowledge of these prerequisites for a successful survivable lander, our principal design difficulty would remain: it concerns the problem of life-detection. What minimal set of assays will permit us to detect Martian life if it does exist? A debate on this question for the past several years has yielded a variety of competing approaches. Each of these is directed to some manifestation of life according to the cues of terrestrial biology. Needless to say, visual reconnaissance, from microscope to telescope is one of the most attractive of these for it offers the expectation that many recognizable hints of life would immediately attract our attention. However, we can easily imagine circumstances in which this type of observation would be inconclusive. Many other suggested procedures seek to identify, at the outset, the more fundamental biochemical structures and processes that we would, in any case, explore in depth. No one of these analyses, however, whether photosynthesis or respiration, DNA or proteins, growth, enzymes or metabolism, or, in a figurative sense, fleas or elephants, can be sure of finding its target and reliably reporting on it under all circumstances, nor would any single approach satisfy all the particular interests that motivate different investigators in their search

We cannot recount here all our deliberations on the life detection problem. We have sought the most generalized criteria; among these is net optical activity, which is almost surely the result of steric restrictions imposed by an historical accident in the origin of life. Another is the presence in assays of exponential features which can only be ascribed to growth and reproduction. And we have reconciled ourselves to the fact that early missions should assume an Earth-like carbon-water type of biochemistry as the most likely basis to any Martian life. On that assumption enzymes that should be widespread can be sought; growth could hopefully be provided for by generalized media.

The fact remains, and dominates any attempt to define landers for detecting life, that no single criterion is fully satisfactory, especially in the interpretation of some negative results. To achieve the previously stated aims of Martian exploration we must employ as mixed a strategy as possible.

Discussion throughout our study has returned repeatedly to the conclusions that we would not be convinced by negative answers from single "life-detectors", that given the hazards of any chemical or metabolic assay we should ensure some direct visual inspection by television, and that the lander program must ultimately involve an Automated Biological Laboratory (ABL). The ABL concept is not fully defined: it involves provision for the multiplicity and diversity of chemical analytical techniques and biological assays that our aims call for; it involves, too, the idea of an on-board computer by means of which a variety of programmed assay sequences can be initiated contingently on the results of prior steps; it also involves the idea of a sustained discourse between the computer and investigators on earth. It is, in short, an ambitious concept. But our preliminary scrutiny of the ABL idea suggests that, though ambitious, it is realizable with the current technology.

In the long run we believe that manned expeditions and the return of Martian samples to the Earth will be part of the exploration of the planet. Neither of these is imminent, but some of our readers will be as surprised as we were to discover that manned Martian missions will probably be feasible in the 1980's. Certainly neither the return of samples nor the sending of men to Mars will be scientifically justifiable until unmanned landings have prepared the way.

VII. The Timing and Overall Strategy of Exploration

All of us would in principle prefer a gradualistic approach to the ultimate goals of landing a large ABL on Mars and, eventually, of returning samples for study here. It is clear on all grounds - of economy, and scientific prudence - that we should exhaust the possibilities of further progress using Earth-based observations and non-landing missions to Mars.

For instance, a strong majority of the Working Group believes a successful orbiter program should precede a landing. The orbiter promises an immense extension of our knowledge of the atmosphere (its density and chemical composition) and surface of Mars. Its capability for sustaining seasonal observation and extensive topographic mapping will permit a thorough re-evaluation of the several Martian features that have been considered suggestive of life. And it will permit a far better informed selection of landing site for the ultimate ABL missions. It has the further merit of effecting this substantial step forward with minimum risk of contaminating the surface.

Constraints to proceeding in a completely unhurried, stepwise fashion arise from several sources, however. They are a combination of celestial mechanics and the operational realities of space research. Any space experiment takes years of preparation and budgetary commitment; the preliminaries to actual flight involves years of experimental design, spacecraft development, and the coordination of effort among large numbers of people in a wide range of disciplines. The scientific investigator no longer has the total freedom he usually enjoys to make tentative starts, to explore hunches without full commitment, to stop and follow another course. He is further plagued by the prospect of investing years of work only to encounter a mission failure or cancellation in which it is all lost - at least until a new opportunity arises perhaps years hence. He may chafe under these circumstances but he must accept them if he wishes to proceed at all. The goal of a Martian lander will be by far the most complex and difficult spacecraft we will have built, involve an even wider range of disciplines and instrumentation to be coordinated. And it will be, for these reasons, the most costly and time-consuming to develop. A Martian orbiter is itself a much larger undertaking than any spacecraft so far flown. The point is that we are confronted with the necessity of near-commitment many years ahead of flight time; and the opportunities for flights to Mars are by no means always at hand. The orbits of Earth and Mars are such that these opportunities are now limited to brief windows which recur about every second year but undergo a further approximately 17 year cycle of favorableness. Our attempt to develop a systematic and gradualistic program is thus constrained to some extent by the fact that while favorable opportunities occur in the 1969-73 period they will not return before 1984-5.*

We have concluded that the 1969-1973 opportunities can be and should be exploited

* For these reasons an alternative strategy has been discussed: it would allow the early use of landing probes, always providing that reliable decontamination systems will have been developed and authenticated. A minority opinion holds that small landers may provide environmental information useful in the design of other spacecraft and may succeed more readily than orbiters. According to this view the way should be left open to their use - even though the results obtained may well be less comprehensive

for a substantial program of planetary missions. By that time the Saturn booster system will be available, and a four to five year lead time is evidently adequate for the development of initial spacecraft.

The more detailed planning of planetary missions for 1969-73 is for the most part outside the scope of this Study's competence and commission: the decisions concerned involve engineering and many other elements with which we did not cope.

## VIII. Conclusions and Recommendations

### (1) The Biological Exploration of Mars Recommended.

The biological exploration of Mars is a scientific undertaking of the greatest validity and significance. Its realization will be a milestone in the history of human achievement. Its importance and the consequences for biology justify the highest priority among all objectives in space science -- indeed in the space program as a whole.

### (2) The Scientific Aims of the Exploration

We approach the prospect of Martian exploration not only as biologists but as scientists interested in evolutionary processes over the broadest range. Living systems have emerged as a chapter in the natural history of the Earth's surface. We wish to test the hypothesis that the origin of life is a probable event in the evolution of all planetary environments whose histories resemble ours.

We thus conceive the over-all mission as a systematic study of the evolution of the Martian surface and atmosphere: has that evolution included, in some niches of the planet, chemical systems with the degree of complexity, organization and capacity for evolution we would recognize as "living"? Our specific aims are:

(a) The determination of the physical and chemical conditions of the Martian surface as a potential environment for life,
(b) the determination whether or not life is or has been present on Mars,
(c) the characteristics of that life, if present, and
(d) investigation of the pattern of chemical evolution, in the absence of life.

### (3) An Immediate Start to Exploit the 1969-1973 Opportunities

A major effort should be initiated immediately to exploit the particularly favorable opportunities of 1969 through 1973.

We are here concurring with the Space Science Board's views that planetary exploration should be the major aim of the nation's space science efforts in the 1970's and 1980's; and, further, that the biological exploration of Mars be the primary focus of the program.

### (4) Avoiding the Contamination of Mars: a Major Mission Constraint

Before proceeding to other aspects of the undertaking, we are concerned to single out from the many and diverse problems that Martian exploration will entail, the task of prevention of contamination.

Contamination of the Martian surface with terrestrial microbes could irrevocably destroy a truly unique opportunity for mankind to pursue a study of extraterrestrial life. Thus, while we are eager to press Martian exploration as expeditiously as the technology and other factors permit, we insist that our recommendation to proceed is subject to one rigorous qualification: that no viable terrestrial microorganisms reach the Martian surface until we can make a confident assessment of the consequences.

(5) Programmatic Recommendations

5.1 Every opportunity for remote observation of Mars by Earth-bound or balloon and satellite-borne instruments should be exploited. A vigorous program here can yield a very substantial increase in our knowledge of Mars before the major program of planetary missions begins in 1969.

5.2 It has become evident that an adequate program for Martian exploration cannot be achieved without using scientific payloads substantially larger than those currently employed in our unmanned space research program. Although predominantly engineering considerations may incline to early use of smaller payloads, we see very substantial advantages in the use, from the outset, of the new generation of large boosters which are expected to become operational toward the end of the present decade. These advantages include: the possibility of avoiding spacecraft obsolescence due to a change in booster; the potential for growth in the versatility of scientific payloads and the relief of pressure on the engineer to design spacecraft to the limit of booster capacity.

5.3 We deliberately omit an explicit recommendation in favor of any fly-by missions additional to those already executed or planned for the 1964 and 1966 opportunities. They yield at best a fleeting glimpse of the planet, and unless they are already so large that they could as well have been orbiters, the array of sensors they carry is small. Given the booster power adequate to deliver it, an orbiter is overwhelmingly preferable. It may well be, however, that strictly engineering considerations will demand some preliminary flights in 1969 and if these are undertaken their exploitation as fly-bys could yield worthwhile information.

5.4 Every effort should be made to achieve a large orbiting mission by 1971 at the latest. This mission should precede the first lander. (A dissenting minority view supports the simultaneous use of small landing probes.) By "large" we mean a scientific payload that would include instrumentation for: (a) infrared and television mapping; (b) microwave radiometry; (c) infrared spectrometry; and (d) optical polarimetry. The success of this mission will depend on the availability of a large booster and a substantial improvement in currently available communications facilities.

5.5 The first landing mission should be scheduled no later than 1973 and by 1971 if possible.

We have not yet outlined what the contents of a large lander should be in terms as specific as those used to describe the orbiter. The central point on which all agree is that the mission ultimately demands a large lander, which we have come to call an ABL (Automated Biological Laboratory). What is unclear at present is how fast such a large lander can be designed and developed from biological and engineering viewpoints. It is, however, clear that the development, both as to conceptual design and engineering, will go through several generations. It is hoped that the first generation of an ABL could be used for the 1971 opportunity.

The lander we are recommending for 1971 is something short of what is ultimately possible and necessary but could have a sufficiently diverse array of instrumentation to answer some of the scientific questions we have posed.

5.6 The task of designing an ABL should be initiated immediately as a continuing project. The contents of landers in 1971 and 1973 will be products of this continuing undertaking.

5.7 The problems associated with the biological exploration of Mars are diverse and the task of implementation raises challenges in many respects wholly novel. Orbiter and lander missions alike will involve many different experimenters. The evolution of an optimum scientific payload will require a continuing dialogue among all potential investigators and the engineers responsible for implementing their scientific goals. The undertaking we are recommending cannot proceed without some provision for organizing and sustaining that dialogue on a continuing basis. As the program develops other devices may become more appropriate but at the outset we believe a standing committee of the Space Science Board will be a useful provision. It should be charged with: (1) a continuing surveillance of progress from a scientific viewpoint; and (2) the responsibility of giving advice on request to the National Aeronautics and Space Administration.